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SUBCOMMITTEE ON ADVANCED REACTORS

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
SUBCOMMITTEE ON ADVANCE REACTORS

Rodeway Inn
Room 215
Chicago, Illinois

Wednesday, July 15, 1981

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

BEFORE:

M. CARBON, Subcommittee Chairman
J. MARK
M. BENDER

ACRS CONSULTANTS:

R. Avery
G. Golden
J. Hartung
W. Lipinski
S. Siegel

LS:1

MR. CARBON: No statements are required, Frank. We will move right on.

MR. GAVIGAN: Our first speaker will be Lou Baker from the NCR, who will describe what we do in 3.2 and why we do it.

MR. BAKER: My name is Louis Baker, and I will be discussing our LOA 3.2 activities, debris accommodation.

The overall objective is to demonstrate a low probability of containment failure from core debris-related threats.

Our work breakdown structure considers this in three categories: accommodation within the primary system boundary or within the reactor vessel; within the inner cells, the reactor cavity; and within the containment as a whole.

Our major program task -- let me first say, our discussion will be divided into three parts. I will talk about the results of a task force which we have recently concluded on core debris program planning.

Then Dean Pedersen from Argonne will discuss the ANL work on particulate debris bed coolability, and molten debris interactions.

Then Louis Muhlestein from HEDL will talk about the sodium-concrete interactions work at HEDL.

The other key activities are debris retention design options or engineering analysis at GE; hydrogen detection and control at HEDL; gas release from concrete at HEDL.

And we have three code activities which I will mention in a minute.

The overall strategy in the debris accommodation area is to demonstrate inherent debris accommodation capabilities, and in addition to that, provide a wide range of design options to enhance debris accommodation capabilities.

And our approach. as I mentioned, one was to convene a task force this year on debris accommodation.

In general, to develop understanding of inherent in-vessel retention processes, and evaluate the potential for engineer's in-vessel retention features.

The task force determined that a very important aspect of debris accommodation is what we call debris carryover. This is the debris which might be carried out of the reactor vessel into the piping. So this is an important aspect of our work.

We are also investigating the inherent limitations of the sodium-concrete interaction. We are

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evaluating design options to enhance ex-vessel debris accommodation. Our major code development efforts are the containment analysis code system CONACS development at HEDL.

This is a code system which is designed to be a comprehensive method for containment analysis.

A complementary effort is a development of concrete structural road at Argonne, to establish the combined thermal and thermal stress behavior of concrete structures which might become overheated in the core debris process.

Also, Don Ferguson mentioned yesterday our spray fire code work at AI.

MR. MARK: The concrete structural code, is there nothing covering that same point in the light water reactor business?

MR. BAKER: I don't know.

MR. MARK: The concrete structure and the temperature is not really a new phenomenon.

MR. BAKER: No. But there is really not an adequate treatment which includes the effects of cracking and the gradual heating of concrete.

Most of the work I think have been directed toward response to explosions or very rapid pressure rises.

But the question of whether, when or how the concrete structures will respond to gradual heating in a post-accident situation is not well established.

MR. MARK: Well, then I would guess if they don't have such a thing, when you get yours, it would be of as much interest there as here.

MR. BAKER: Right.

MR. MARK: In fact, for some time there will be more such reactors.

MR. BAKER: I would think there would be.

I would like to say a little bit then about the results of our core debris accommodation task force.

We essentially did three things. First of all, we examined the potential design options, the inherent retention capability, but what could we do to enhance this.

Secondly, we looked at each of these situations to establish what potential safety issues there are; that is, what parts of each analysis might become a matter of discussion at regulatory hearings.

And thirdly, from that, from the issues, what tasks, what R and D task do we need to do?

First of all, then, the design options. We considered in-vessel debris retention to be of high priority. And in addition to that, we considered a core retention plate, or without defining it in any detail, the general concept of adding a steel structure within the vessel to assist in retaining the debris.

As I mentioned, the retention of debris carryover, what is the fate of debris which may be carried through the piping into components.

And then in ex-vessel retention, we considered the three options to be extremely important.

First of all, the separation of released water, water which might be released from the concrete, separating that from sodium.

None of these, except with the possible exception of the actively cooled ex-vessel core catcher, these were not intended to be complete solutions to the core debris problem.

MR. CARBON: Excuse me. What does high priority mean? Promises, or that it needs urgent work? What is priority and what is high?

MR. BAKER: It means that it would be most likely to be used by a designer. Our goal was to provide the maximum flexibility for the designer without trying to tell him what he should do, but rather investigate

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the possibilities.

But at the same time, we didn't want to base a research program on very unlikely methods.

MR. CARBON: Okay.

MR. BAKER: The concept of a delay bed, this is the addition of high melting ceramic material to the reactor cavity. This will delay the release of water and gases from the concrete, and eventually delay the penetration, the penetration of the core debris in the concrete.

Cavity pool cooling by boil-off is looking at the advantages of letting the sodium in the cavity boil off, thereby delaying penetration into the cavity.

Less promising, but nevertheless worthy of looking at, was cavity cooling by the DRACS. In this the direct reactor auxiliary cooling system is in the reactor vessel.

It is conceivable one could make the cavity small enough that the DRACS would still be submerged, even after reactor vessel failure, and achieve cooling that way.

Of course, actively cooled ex-vessel core catchers were considered, and a range of alternative options were discussed.

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MR. LIPINSKI: How does that ex-vessel core catcher differ from the engineer's retention, core retention plate?

MR. BAKER: This is within the reactor. This is outside.

MR. LIPINSKI: One is definitely cooled. On the other there is no word cooled, so is there any effort to engineer any type -- is this just a big steel plate, or is it ceramic?

MR. BAKER: It would not be ceramic because we felt we would not want to put foreign materials into the reactor vessel for fear of causing an accident.

MR. LIPINSKI: What is it, a big steel plate?

MR. BAKER: It would be -- it is not defined. It would be a set of trays. Just in general, putting something near the bottom of the vessel to increase the area for core debris.

MR. BENDER: When you are talking about delay, or time, what objectives do you have in mind? How much delay?

MR. BAKER: Well, again, we are not specifying a particular situation. It would be rather, than, just to, again, this would depend on an overall design, and we did not create an overall design.

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We are saying we are interested in the principle of putting non water-releasing material in the cavity to delay the water release from concrete, and also the penetration into the concrete.

MR. BENDER: I am trying to get some feeling for the importance of this capability. And I would assume for it to be of value it has to be associated with either time to get the fission products out of the way, or to provide arrangements for some future cooling capability to be brought into place.

And it would have an influence on how useful such devices might be. When you make a judgment something has a high value, you have to say why.

MR. BAKER: I think the intent of it is to delay the generation of pressures within the containment or hydrogen in the containment, which would require you to vent at an early time, or in the absence of venting, expect building failure of some kind.

MR. GAVIGAN: Also allows evacuation within a reasonable length of time, if you want to put it right here.

MR. BENDER: That is one way of dealing with it. I wasn't really sure what you were driving for.

MR. GAVIGAN: These are R & D options we make available to design. We make them available, and now they are going to approach the safety problems.

MR. BAKER: The next step was to develop potential safety issues from the considerations of the design options.

We have divided these into in-vessel debris carryover and ex-vessel.

I won't go through all these in detail, but an example, debris ejection is associated with how does the core debris leave the core region.

We concluded here that in general we will have to consider that core debris may be ejected upward or downward, or both, so that there is no R & D there.

We have to consider all possible options.

In-place coolability is particularly associated with the blanket coolability.

In other words, we don't necessarily have to assume that all of the fuel melts out of the core region, or is ejected from the core region at time zero. There may be a considerable quantity which remains in place permanently or for a consierable period of time.

The next question is: Is the fragmentation of the material ejected from the core region

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complete? Can we consider particulate beds only, or do we have to be concerned with dense masses of fuel or molten pieces?

And is the process of fragmentation of the fuel cooled interaction source sufficient to damage important structures.

An important consideration is the particulate form from fragmentation distributed uniformly over the cross-section, or is there concern over piling up?

Of course, the question of coolability of the debris, debris which is ejected downward, or we have to establish whether it flows through the core support structure or whether it freezes in the plugs of the core support structure, and if so, how does it remelt?

The structural integrity of the core support structure, as well as any in-vessel retention plate, both from the thermal effects of debris penetration, as well as the structural, the pressure force effect of the ACDA itself.

We may wish to evaluate the coolability of dense masses of debris, that is molten layers or solid frozen layers.

We have to establish that the sodium

11 natural convection and flow is able to carry the heat away from these debris concentration areas to the cooling system as a whole.

It showed that there is an available heat sink. This is to establish in-vessel retention.

We need to know where the decay heat is distributed. Does it remain with the fuel, partly dissolved in the sodium and partly in the steel.

We have to be concerned with the question of possible recriticality during motion within the vessel; penetration by hot jets of any retention structure.

And we have to be concerned with falling masses.

I won't go through all of these, but I think it is important to mention the debris carryover.

The accident initiators, we have to look at the range of accidents which leads to the post-accident situation, particularly to establish what sodium flow rates we might have to determine the conditions under which the debris is ejected, in order to be able to calculate then the debris carryover.

If we assume that particulate forms in the upper plenum, we establish other limiting flow rates.

We need methods of predicting the fate of the debris; how much is carried over, where does it settle out in the primary system.

Debris coolability within the primary circuit is very similar to particulate coolability in the vessel, but here we are in a more constrained situation in piping or parts of components.

Should the debris become uncoolable in the primary system, we would then have to consider failure of the primary system and debris coming into the primary heat transferer system. We would examine then the liner integrity, possibly the penetration of core debris into concrete.

In general, our feeling is that debris carryover is limited, because in the loss of flow accident, the flow rate is well down before ejection from the core region would be expected.

And in the transient overpower accident there is relatively small amounts ejected upward before the nuclear reaction is turned off by the loss of fuel, such that we are optimistic about this.

But nevertheless, we have to look at all these steps, and even the possibilities of a small amount coming out into the PHTS cells may not be an unacceptable result.

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Ex-vessel, of course, I think these are pretty well known. We have to establish the behavior of the cavity material, coolability in the cavity, possible penetrations of part or all of the cavity material, the structural integrity.

This refers to the sidewalls, the sidewalls should be up here.

Falling masses protection for the cavity system, heat transport, heat sink availability, sodium concrete reactions, gas release from concrete, and an important aspect is hydrogen monitoring and control.

These issues then were coalesced to a series of R & D tasks.

I have indicated here which tasks are in progress.

The first three are rather general. Containment response analysis, we haven't done a great deal of this this year. We have instead concentrated on the development of methods.

The CONACS program at HEDL is being supported actively by ANL, GE and AI.

The models, specific models to be used in the overall CONACS problem are being recommended and developed at ANL, GE, ANL and HEDL.

HEDL will then put it together to the overall code.

In-vessel analysis is going on at GE. They are looking particularly at core debris distribution, the structural integrity of the core support structure. And they are looking at possible in-vessel retention devices.

The remainder of these, except for physical properties, our last one, are more or less in order of priority as we developed them in the task force.

In other words, the most important, other than the general analysis methods, development, is distribution and carryover.

We don't have anything going right now, but we expect to initiate work next year, which will use the available mockups of FFTF or CRBR, to inject simulated core debris into a water loop, examine it, where it goes and where it tends to concentrate, and how it would tend to spread across the reactor vessel.

Later on we may, we may have to do some real material experiments, probably with thermal generated core debris.

MR. BENDER: To what degree are these studies related to specific configurations?

Are they all general?

MR. BAKER: I think they are pretty well general.

MR. PETERS: The distribution and carryover --

MR. CARBON: Would you identify yourself?

MR. PETERS: Dean Peters.

The distribution and carryover is going to be very design dependent, so you would have to -- they will have to take and consider in the performance of that task, the various design concepts.

There is a significant difference in design between FFTF and CRBR, and CEF, in terms of their -- in terms of the effect that it will have on the design and on distribution and carryover.

MR. BENDER: That would be a volume in geometry.

MR. PETERS: Excuse me?

MR. BENDER: They are a volume in geometry.

MR. BAKER: Our main effort is to establish the general, the generic behavior.

And when we do have a new reactor design, presumably either the project or the base program would investigate that more specifically.

For example, in FFTF, you leave the

reactor vessel and you immediately go to upflowing pipe. And this helps to avoid excessive carryover.

Only the finest particles can make it up the riser.

But in general we would like to know how things might behave in a variety of cases. For example, in cavity performance, we would investigate the general behavior of core debris, and as it interacts with either concrete or cavity material that might be used.

Eventually, if a cavity is designed that we feel needs to be looked at, we would use that specific design.

I think that has been done. Dr. Muhlestein's group has looked at specific liner features and contact with sodium in the past for CRBR. I think it is a combination.

Now, we have identified in the area of, the need in the area of cavity performance for a larger core melt facility.

One of our tasks next fiscal year will be to examine the methods used at ANL and at AI with relatively small scale electrically heated facilities which Pedersen will mention, and to see if they are amenable to scale up. And also to study in more

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detail just what we do need.

We will also be looking at the CND core melt facility for the potential to meet this need.

MR. BENDER: I am still not clear on the parameters that have to be investigated.

I understand temperatures being a parameter, but the heat removal and distribution is a function of the way in which the material lays in the cavity.

And I am not sure that I understand how you do the experiments without having some specific arrangement in mind.

It seems to me it might be important to know that.

MR. BAKER: I think we have general arrangements in mind. In other words, one would -- the most promising cavity material other than pure concrete would be generally considered to be magnesia bricks. So we tend to study these, this system.

On the other hand, a designer might choose to choose an alumina liner, or some other combination.

When that selection is made, then we will tend to study that system.

I mentioned a concrete core development

at Argonne, and eventually we feel in a few years we will need some large scale thermal structural experiments on concrete in order to verify the codes.

We will be hearing about sodium concrete, core debris and dense mass coolability.

These, I might mention, are natural convections and heat transfer needs in the core debris area. We expect to be met by, as part of the LOA-2 programs mentioned yesterday by Emil Gluckler and Joe Mills, that both the tests in the water and in sodium would be extended to include typical core debris configurations where there is a distributed heat source corresponding to debris locations within the vessel.

I think I will, in the interest of time, leave it here. These are relatively lower priority needs which would be addressed later in the program.

We would not expect to start anything for a few years in these areas.

MR. FERGUSON: I would like to just add something. Don Ferguson from Argonne.

With regard to this core debris, what you are viewing here is the safety program, the base program trying to take the offensive in this area, in the following sense:

Always in the past, and in the past at least in my time, FFTF and later on in Clinch River, at some point in the project they realized that they had a problem with respect to core debris accommodation following severe accidents.

The safety-based program was always told, "Okay. Here is our design. Solve our problem."

And a series of stories were put together to try to justify whatever was required to meet their needs.

In the case of FFTF, showing successful in-vessel retention so that one did not have to worry about special containment features for core debris accommodation.

In the case of CRBR, essentially showing that by adding a vent filter system you would meet at least this twenty-four hour requirement for no venting and filtering that NCR placed on Clinch River in the case of the May 6th letter, May 6, 1976, letter.

During the FFTF days, the great bulk of the effort within the base program was aimed at providing the requisite technology for supporting this in-vessel storing.

Along Clinch River, given the same

twenty-foot diameter vessel, much larger core, it was noticed at a glance that in-vessel storage would never be successful.

So suddenly all the attention in the base program was focused on supporting this core in the floor concept that evolved at Clinch River.

CDS has essentially punted by saying we don't know what requirements NCR might place on us; and secondly, given a set of requirements, we really don't know what we can do to meet those in terms of well-defined demonstrated technology.

So at the present time the CDS project is taking a very much wait-and-see attitude in this arca.

The words are that leaving imbedments in the reactor cavity, for example, to be able to support the cavity liner material, for example.

What we in the base program are trying to do is to look across the whole broad picture and say what, what the totality of the range of options that a designer could possibly choose and in each case, what technology would be require to determine whether or not that option could be -- it is viable or not viable.

And so what we would add up here is the full range of options we considered to attempt to prioritize those in terms of giving consideration to things like both cost and probability of success of the technology development efforts.

And then we are embarking upon a program to investigate these areas, so hopefully a few years down the road we would be in the position to tell a designer, "The following design option will give you twenty-four hours, or thirty-six hours, or forty-eight hours. or in the case of some actively cooled concept, perhaps indefinite delay before you would have to worry about containment failure."

So that is the attitude. That is the perspective that we have in terms of this program that we are embarking on at the present time.

It is an attempt to take the offensive and put the base program in a position to tell the designers what range of options they could possibly choose, as opposed to the other way around, which is taking whatever the designers have evolved and trying to construct a story to meet their needs.

MR. BENDER: I like that idea, but just to pursue a thought that came to me earlier, it

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seems to me you would have to be presenting something like the heat transfer rate per unit of area, or per unit of volume has to the sum amount in order for the designer to know whether you have something that he can apply, is that coming out of these experiments?

MR. FERGUSON: Yes. For example, in the case of in-vessel retention with a flat plate, a core much like what the British are talking about for CDFR, or concepts of a phrase, like the French are talking about for Superphoenix, we would hope to be able to be able to establish what kind of thickness of bed as a function of time could be cooled without fuel melting and bed dryout. So that then the designer could say, "Okay. I have a given fuel loading in my core. I have a given diameter vessel. Will this concept be a viable one strictly from the standpoint of ability to cool a debris bed of a certain thickness?"

Of course, one also has to address the question, would the debris bed level to the point where I can concern myself with a uniform thickness and so on.

Going beyond that, if you wanted to have a thicker bed, are there concepts that would work, such as where the British are talking about where they have pipes that carry the sodium from below the pipe to above the pipe to enhance the natural circulation flow around the plate.

So yes, the idea would be to develop for the designer some design guidelines that tell him how far he can go with a given concept.

MR. BENDER: Now, the other aspect is a question of what the age of the melt is.

MR. FERGUSON: Excuse me?

MR. BENDER: The age of the melt is the other parameter that has to be taken into account.

At what time in the life of the core does the melt reach this bed, because that would, that to some degree determines how much heat is being evolved.

MR. FERGUSON: We certainly would be trying to deal with the worst case situation, which is an end-of-life core with a maximum decay heat loading.

MR. BENDER: I am thinking about how many hours or days it takes for the melt to reach the melt.

MR. FERGUSON: I think in the case of an LMFBR, liquid metal fast-breeder reactor, the debris retention plate in the matter of at most a few minutes, and it reached the cavity, and it didn't stop it there, at most ten minutes to fifteen minutes, something like that, it is effectively instantaneous.

You are still in the area of two to three percent full power in terms of total decay heat loading.

MR. BENDER: There is no heat capacity system?

MR. FERGUSON: That's right, for that purpose.

MR. LIPINSKI: I have a question. What kind of specifics is put on this in terms of the retention for what period of time, one year, ten years, fifty years, because I am just wondering if this is kind of like TMI-2 happens now in R & D programs, and try to solve the problem, because here we have the retention device that is being put into place, the material being collected, and then what happens into the future.

MR. BAKER: There is a designer option. It depends on his strategy.

We have not assumed any particular one here. We have considered the whole range.

He may wish to retain containment integrity only for twenty-four hours, as was suggested in the CRBR case, and then allow overpressuring and release it by venting.

MR. LIPINSKI: That is --

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MR. BAKER: Or he may wish to choose a permanent containment situation where there is never any relief.

MR. LIPINSKI: Can I rely on that for fifty years?

MR. FERGUSON: There is absolutely no work going on in the safety program in the area of accident recovery.

NCR in the past hasn't concerned themselves with accident recovery. They may well concern themselves now.

Our program is not concerned with that. It says in the requirement thirty-six hours. We will meet thirty-six hours.

Beyond that we will try to tell you what kind of releases come from containment as a consequence of the vent filter or overpressurization or cracking of the containment or whatever.

MR. LIPINSKI: It is not accident recovery, it is accident continuation, because this accident continues indefinitely into the future.

MR. GAVIGAN: What we did, I mentioned earlier, we are guided by two criteria.

One is the dose that occurs from the accident, or the normal risk curve that you are trying

26 to achieve. Given that, if you are able to allocate that down the system, we developed the R & D strategy that allows the designer to meet these criteria in a number of ways.

And from what we have seen so far, you meet those criteria.

Then considerations beyond that, that is out to fifty years or so, are not health criteria any more. They are psychological, and they are property damage.

So from our viewpoint we can't set a psychological criterion, because it is difficult to do research. And we don't do property damage research.

So we are left with either dose or risk.

Based on dose or risk, how far into time do you carry on to meet the criterion?

MR. GAVIGAN: That depends on the project approach. If the project decides where this particular site, where they have this reactor, and as such evacuate the population in blankety-blank time, one could keep the containment for twenty-four hours or forty-eight hours, and that there sets a requirement on what kind of R & D he selects, what has already been done, and what design features he puts in the building.

So he puts the time requirement on, and we try to provide all the information so he has the option of meeting the time.

MR. LIPINSKI: Are evacuation and containment figures of twenty-four hours satisfactory, even though the containment fails and the entire area is contaminated?

MR. GAVIGAN: I think that is true.

MR. BENDER: It is not the kind of thing which we could present in regulatory context.

There has to be a walkaway position which says the reactor system can be left in a state of permanence, if I can call five years or ten years permanence.

I don't know how long it is going to be, until something can be done to correct the situation.

And it is a matter of how the public is convinced that given an accident, it can be, the system can be put into some kind of passive condition that is acceptable, like TMI-2.

I think Walters is right. You would have to be able to show that when the accident is over and the people have evacuated, that there is a subsequent set of actions that can be done to make the situation, at least that status quo, stay where it is without jeopardizing the area any further.

MR. GAVIGAN: The particular plant, certainly they are all valid considerations. The plant we are designing for or thinking, is a government plant on a government site.

We are talking about a site in Idaho or Hansard or some place. Even though we have to consider those things in the long run, they may not be specific considerations for this reactor.

Of course, you can't say, therefore, we will melt down the plant and walk away.

You have to consider how you are going to recover. But recovery is by mandate, not part of our program. We have to be told that before.

MR. BENDER: I am not thinking about recovery in the sense of trying to reclaim the reactor system, but the ability to leave it in a passive condition. That is the thing that I think needs to be dealt with.

And I am not sure that you can afford to say that once you evacuated and got in all the people out of there, that that is a sufficient answer.

MR. GAVIGAN: I agree. That is one of the options.

The project has a number of options,

different kinds of containment approaches, different kinds of auxiliary cooling systems you can bring off.

People talk about bringing sodium tank cars in and pumping it in and cooling the system. There is a lot of things you can think of.

What bothers me about thinking these things, we sit here, we started on LOA-1 and LOA-2 presented yesterday.

We try and get the message across that we could have designed such that these accidents shouldn't have to be considered.

When we get out into this R & D program out in LOA-4, we now treat them as real things. In fact, we try to generate real criteria one would have to meet, because we act as if the accident is really going to happen.

It is a dichotomy in the thinking we have.

MR. BENDER: You are misinterpreting what I am saying. I am not trying to provide a solution. I am trying to argue that the philosophy associated with how to deal with the situation has at least been thought along further than the first few days, and I don't get that feeling right now.

MR. GAVIGAN: It has --

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MR. BENDER: I don't think it -- feel it really would.

MR. GAVIGAN: We are not talking about the project. We are talking about the R & D program where some of those other options I haven't mentioned are relaly project decisions.

For the R & D program we developed a capability for some of those options, not all of them. For example, the evacuation thing doesn't impact our R & D program much. It is something someone else may choose.

MR. LIPINSKI: In the debris accommodation, ex-vessel, is there a specific that that thing has a function for a certain period of time; days, months, years.

MR. GAVIGAN: No.

MR. LIPINSKI: That seems to be missing.

MR. GAVIGAN: That's right.

MR. LIPINSKI: Because in twenty-four hours you will guarantee it will work. Tell me what happens after twenty-four with respect to debris retention.

MR. GAVIGAN: One of the reasons, one of the charters the subcommittee has or will have is development of R & D criteria.

If you look at the existing criteria, there is nothing there on whether one would involve a core catch, how long it should operate.

You don't have it, we don't have it. And it is because you will have the uncertainty of this business that you try to understand what the problems are.

MR. LIPINSKI: Getting back to your earlier comment for looking at the government site, on one of your drafts you have associated with commercialization, [illegible] meeting safety criteria.

MR. GAVIGAN: Right.

MR. LIPINSKI: Your task on the government site could be a lot easier by bypassing a question that you could not bypass if you were going to commercialize.

MR. GOLDEN: The counter argument to that is that you might learn enough to the operation, the building and operation of that plant on the government site so that you would have an easier time in the licensing, and not have to put in as many levels of descents.

MR. LIPINSKI: I only agree with that if you are going to guarantee a core meltdown and give the experimental data.

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MR. CARBON: Let me suggest we better move on.

MR. MARK: Could I ask one question about what is here?

The debris that is accumulated and which you are studying, where it is in the tray or can be cooled, is hot.

I don't notice here a program to find out what fraction of the fission fragments actually stay with the hot broken-up core material.

All the volatiles will migrate at some rate or another. It is alleged they will take some of the source material with them.

MR. BAKER: Yes. We are concerned with that. We called in the KE distribution, task 17.

MR. MARK: I see. That includes the fact that the iodines will have left in one minute and so forth?

MR. BAKER: Right.

MR. GOLDEN: With the ions in the sodium.

MR. MARK: Where will they be?

MR. GOLDEN: In the sodium.

MR. GAVIGAN: The next presenter is Dean Pedersen from Argonne National Laboratory.

MR. PEDERSEN: I want to talk about two

technology areas. One is the coolability of particulate debris beds, which is work being performed at Argonne.

And the second area is the penetration of molten core debris into substrates, substrates being steel, concrete, MgO, anything of that.

And this work on molten penetration is being performed out at AI and at Argonne.

In the area of debris bed coolability, the debris is going to settle out in the ex-vessel cavity. It is going to settle on structure in-vessel, and there is going to be significant quantities of sodium overlying it.

Our program has been considering cases where the only mode of heat transfer or removal that the generation is upwards into the sodium pool that is above.

In this case, the heat removal is by liquid down below into the bed, and vapor flow back out of the bed.

Once you get into a boiling condition, it is possible to have a bed that is cooled by conduction, but with reasonable size bed depth that you expect in the ex-vessel cavity, and reasonable times, fairly early times of entry, you will be boiling in the debris bed relatively soon.

In the cooling of debris there is basically two regions here. A deep bed regime is when the bed becomes fairly thick, greater than approximately ten centimeters.

And a shallow bed regime where the bed is a lot shallower. The vapor channels here penetrate all the way to the bottom of the bed, and you get a significant increase in coolability.

If you look at the coolability versus height, this is Q versus H, H meaning the depth of the bed, you will find that in a deep bed regime out here, that the heat flux that you can remove, the most heat flux you can remove without exceeding the saturation temperature of sodium is almost independent of bed depth.

Where we get down into the shallow bed regime, where the vapor channels are penetrating to the bottom, then the heat flux that you can remove is a strong function of the bed depth.

And most of the time the bed depth you are talking about in the ex-vessel cavity will be in the shallow bed regime.

To give you some perspective as to what has been done in this field, Argonne, to support FFTF, performed experiments which direct joule heating of debris beds, and where they were heating, they wound up heating the sodium instead of the fuel, but measured the coolability limits of debris bed as a function of depth to support FFTF.

And this work was done in 1974.

They did experiment not only with sodium and UO_2, but they locked at the effect, the addition of stainless steel.

The major results of that work was that they did identify the deep and shallow bed regimes.

They only know the effect of subcooling of the overlying sodium. That is a point I will get back to a little bit later.

And they also observed, if you have this stainless steel lying on top of the bed, that you get some reduction in the overlying coolability of the bed.

In 1976 UCLA proposed a method of induction heating of particulate beds. You can't do this induction heating with simulate fluids.

We directly heat two beds itself. If you have a bed of steel particles you can inductively heat the bed and wind up with fairly uniform heat generation in the bed, and perform volume heated experiments on debris bed coolability with simulate materials.

And this work has been used a lot at Argonne. It has been used in Germany also as methods of studying some of the details of debris bed coolability.

Sandia in 1980, I have got 1978 to '80, has been performing some in pile experiments on debris bed coolability in their D series experiments.

And these in pile allow direct heating of the UO_2 particulate in sodium. Their results are in basic agreement with the Argonne data of 1974.

However, there is a couple inconsistencies. One is they observed a coolability reduction. You have large subcooling of the overlying sodium.

I am going to get back to that issue later. But we disagree as to the cause of that reduction.

They also indicated the significant margin beyond dryout, and dryout here is where the sodium boiling removes the sodium, the liquid sodium from a section of the bed, and the temperatures rise above the saturation temperature.

So you can have a portion of the bottom of the bed here that could be devoid of liquid sodium, and yet you could be in a coolable

37 state, because now, these thicknesses aren't terribly great, but the upward heat transfer by conduction into this region can be removed in a steady state situation.

Since that time, we have also been performing in the last couple three years, we have been looking at debris bed coolability, trying to develop a set of data base, an experimental data base with simulate fluids and uniform size particles so that we can develop models in the future to handle the many cases that we are going to run into.

In the same sense here, in historical perspective on some of the modeling work, Gabor and ANL, actually Lubaker, took the data that was performed, the experiments that were performed on direct electric heating, and just correlated that data with us where the least squares fit.

But that was just in FFTF, that correlation, those maximum heat loads were used in FFTF.

DHIR and Catton at UCLA performed the first debris bed modeling in which we assumed that the dominant resistance to coolability of the bed was the liquid flow. Where you are getting the liquid back in performed a dominant resistance.

This here has proven to be wrong in the sense that the dominant resistance is really in the vapor phase instead of in the liquid phase.

Hardee and Nilson at Sandia treated, used a model like this where they treated the downward liquid flow covering many particles, with a corresponding upward vapor flow covering many particles.

And they again, they were able to successfully develop a coolability model.

Jones and Epstein, using a similar type model, but instead of assuming that the liquid flow is covering many particles, they assumed that the liquid flow is on the surface of the particles, and the vapor flow is up between them.

This in essence, and the limit here, in both of these models, is when the upward vapor flow is preventing the downward liquid flow. You reach a flooding limit. The upward vapor prevents any more liquid from coming in.

Lipinski at Sandia has also developed a flooding model. And also John Gabor at Argonne has developed a bubble model where he assumes that the vapor is rising as bubbles in the liquid.

So we have a lot of models.

Now, a lot of models isn't our problem.

Our problem is none of the models agree very well.

Most of these models can be non-dimensionalized in terms of dimensional heat flux, which is a function of this property group here, which is the property group being Q, meaning the maximum heat flux, the vapor viscosity, one minus the void fraction in the bed.

The FG, or H of VL, the latent heat of vaporization, density difference and diameter squared, again, vapor factor, and again, porosity.

As a function of the Kinematic viscosity ratio of the liquid to the vapor.

You can see here Hardee-Nilson predicts the highest heat flux. The Jones-Epstein model, which is this liquid film contiguous flow model, predicts the lowest heat flux.

Lipinski tends to fall pretty much in the center.

I should say, for what you see on here, the experimental data from water, methanol, acetone, isopropanol or freon, for bottom heated experiments.

And for the bottom heated experiments, they seem to agree best with the Jones-Epstein model.

When we go to volume heated experiments,

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it does -- we seem to see a factor of two increase in the coolability limits.

So in that sense, Lipinski's model seems to be one agreeing fairly well with lots of the data. His probably is the dominant model at this time.

In the area of debris bed coolability, the outstanding questions are, we do have a large number of models, and all these models have been developed for uniform sized particles. There is very little work on particle size distribution.

So we don't have a real good -- in terms of, in terms of uniform sized particles, I think we do have a good data base, and we can compare the various models with that. And the Lipinski model probably agrees best.

But again, they tend to agree to be develcped for the deep bed zone instead of for the shallow bed zone.

The deep bed is the zone where the coolability limit becomes independent of depth, and in most of our situations we are going to be worried about, or are in the shallow bed region where the heat flux is a function of depth.

The other thing that they really

can't handle, for nonuniform particle distribution is one of their limitations.

The other limitation is stratification.

MR. BENDER: Excuse me. The point about shallow bed depth and its applicability to these models didn't come out very clearly to me.

Are you saying we don't have a model that is suitable for shallow beds?

MR. PEDERSEN: Yes, I am.

MR. BENDER: Can you say something about, or maybe you are coming to it, let me ask, are you going to discuss how you get at shallow beds?

MR. PEDERSEN: How we get a shallow bed model?

MR. BENDER: Yes.

MR. PEDERSEN: No, I wasn't going to discuss that issue.

MR. BENDER: Is what you are telling me academic?

MR. PEDERSEN: I think it is possible to develop a shallow bed model. It is just at this time one of the problems we don't have a significant, sufficient data base with uniform sized particles to develop a shallow bed model.

I think we are developing that right now. And that will be in place in the near future, that data base. Once you have the data base then you can start looking at models.

MR. BENDER: So you need more experiments?

MR. PEDERSEN: We need more experiments. There was already some theoretical work which was performed at Argonne that we think will be the basis for a shallow bed model, in that we are now able to predict the depth of these channels.

And we think we know how, or have some idea as to how to predict the lumber density of the channels.

With those two facts, then we feel it is possible to develop a shallow bed model. But that work is yet to be done.

MR. HARTUNG: Could I ask this question: On the shallow bed is it necessary to have a shallow bed model?

What I am wondering is the heat flux is greater.

MR. PEDERSEN: That's right.

MR. HARTUNG: Is the region in which we will be able to cool the deep bed, so that by inference, you cool a deep bed you can always cool

43 a shallow bed?

MR. PEDERSEN: We are almost always in the shallow bed, and we would like the benefit associated with the shallow bed.

MR. BENDER: I didn't understand that term. Let me try it again.

The benefit associated with the shallow bed is what? Why do you need the benefit? That is what I am trying to get at.

MR. PEDERSEN: What I was proposing was that if this heat flux here, the deep bed heat flux is sufficient to remove any potential generation, or any potential, in any accident you could run into, if this is greater, it is sufficient, the deep bed heat flux, for any sort of accident that you could run into, then why worry about developing a shallow bed model.

But the answer is, what we need to take benefit of this additional heat removal associated with being in the shallow bed region.

MR. BENDER: Because if we don't, what happens?

MR. PEDERSEN: Because if we don't we are going to dry the bed out and move into the next region of coolability of the debris.

MR. BENDER: And you don't know how much enhancement you need, I take it, at this stage of the game?

MR. PEDERSEN: We do know how much enhancement we need.

Are you going to answer that question?

MEMBER OF THE AUDIENCE: I was going to just add, we need to do that because that is going to be very important for our in-vessel change in strategy, because the state we are at, you are saying, the confidence saying, the whole core vessel, because you are saying it is going to dry out and it is based on a conservative limit.

If we can demonstrate with models, I think we are going to make some progress.

MR. PEDERSEN: It is one of those things where you want everything you can get.

MR. CARBON: Move along.

MR. PEDERSEN: Another area which we intend, we are going to need to look at, is the area of debris bed level in the ex-vessel cavity, or in the in-vessel.

We have, at this time, no assurance that we can have in essence a uniform bed depth throughout, as the material enters the ex-vessel cavity, whether it will settle into a uniform bed depth or whether it will pile up in a corner.

So what you want to be able to show is that that bed will level with time, and it will level before you dry out any portions of the bed.

There is, we do know the beds will level. What we don't know is the rate of leveling. So work will be done in that area.

In addition, another area where there is questions is that of bed fixation. If you just wait until the next viewgraph I will talk about that fixation a little bit.

The final area, in the final area of debris bed coolability, this has to do with post dryout behavior, is the post dryout region a stable region?

What happens to the steel that is in the debris bed when it melts? How is it going to move around?

And how do you in essence wind up making the transition in a debris bed which has exceeded the dryout limit, into the next state of actually a molten pool.

On the area of bed fixation, Sandia observed in their experiments a reduction in the coolability of the debris bed when the overlying sodium was subcooled.

Now, this is countered to the previous experiments at Argonne which were performed with joule heat.

They, Sandia explains this, and I agree with the explanation. We just don't agree with the cause.

What happens with overlying sodium subcooled, you wind up with a conjunction zone here at the top of the bed.

Now, what they -- the way they feel what happens is you have a subcooled zone at the top, you have a boiling zone down here.

Now, they say that all the vapor that is generated in this region comes up and condenses on this surface, and must be transferred by conduction through this surface. So you wind up with series heat removal.

Now, we feel that if this is the way bed fixation can cause a reduction in the heat transfer, if however you could show even with a subcooled layer up here that a vapor channel would penetrate that subcooled zone, then the energy which is simulated down here in the bottom, in the boiling zone, can go up through the vapor channel, and you in essence wind up with parallel heat transfer.

So the question is, is it possible in the area of bed fixation, is it possible to develop a layer -- the top that is sufficiently strong to prevent the vapor channels from penetrating.

Now, Sandia feels in their experiments that subcooling of the bed was the only -- was a sufficient requirement to do that.

We have, the Argonne experiments, we have performed experiments with simulative fluids, with water, and saw no reduction in coolability with subcooling.

With real materials, though, there is a couple of other effects that can potentially come in.

One is, we feel the UO_2 that was used in the Sandia experiments was UO_2 plus X. It had been sieved and ground, or ground and sieved -- I guess you have to grind it before you sieve -- a couple years previous to the experiment, and stored in plastic bottles.

Now, UO_2 will oxidize to UO_2 plus X. We don't believe that the X is very high in these experiments. We believe it is probably not more than 01 to 04.

But that, when you have UO_2 plus X, and sodium, you could have a chemical reaction between the UO_2 plus X and sodium, which produces a sodium urinate, if the temperatures are high enough, or a sodium oxide for low temperatures.

And what we are feeling is that if this were two particles here, that this, the chemical reaction products are sufficient to provide the cohesive force to hold the bed together.

MR. CARBON: Excuse me, Dr. Pedersen. We are running way over time on this, and we really would like to stay fairly close to our schedule.

MR. PEDERSEN: I will move faster.

MR. CARBON: Maybe we could just skip some of the details and hit the high points.

MR. PEDERSEN: The other potential problem is that UO_2 plus X can cause a reduction in the wetability.

And if you can't get the sodium to wet, to get into the bed, you can't cool it either.

What we at Argonne, we intend to look at these effects with a facility here which is

-- we are going to do real material coolability experiments with sodium, UO_2 and bottom heat.

I won't go any farther into that other than that is a facility that we have.

Here is a little viewgraph of the actual design of that facility. We are going to be heating it from the bottom.

You have a debris bed here that we are going to heat in sodium. We can have either subcooled sodium or we can have saturated sodium.

We can also look at the effect of concrete reaction products.

We want to be assured that concrete reaction products coming into the bed or settling into the bed from the top will not cause bed fixation.

In addition to the area of debris bed coolability at Argonne, we are also looking at the area of debris penetration in the substrates, particularly molten debris penetration.

And what we are interested in is penetration rates of molten debris into the substrate, whether it is UO_2 or MgO, you are interested in the gas release rates as it penetrates.

The various substrates you are interested in can be characterized as basically three

50 different types; miscible, non-gas releasing, which would be MgO, Al_2O_3, or alumina, et cetera;

Miscible gas-releasing, which is concrete;

And immiscible non gas-releasing, which is steel.

And one of the experimental requirements for these is you have got to have continued decay heating because are interested in long-term penetration.

You can't just perform bump experiments to address long-term penetration.

This next viewgraph, which I won't cover other than to say is real material experiments are very difficult to interpret. And so you need to have, to back it up with a set of simulate material experiments we can visualize what is going on.

And so that is what this slide discusses, is the various experiment that are going on in the area of simulate fluids to look at, to help develop models for molten debris penetration.

In the area of UO_2-concrete, some of the outstanding areas are still the gas release during penetration, long-term penetration rates, possibility of stress levels in concrete.

We want to be sure that as this

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material penetrates, we are not cracking the concrete, and in essence activating the penetration.

Present data only for limestone concrete, which is of the CRBR type.

MR. BENDER: Let me interrupt for just a minute to get it in context.

What we are interested in, I think, is how long it takes to get through this delay bed, or whatever it is.

MR. PEDERSEN: Right.

MR. BENDER: Right.

I mean, I am interested in the matter of the order of difference we are talking about. Are there variations in penetration of time, a factor of two, ten or a hundred? What is it we are dealing with here?

MR. PEDERSEN: Penetration of UO_2 into --

MR. BENDER: Into the delay bed or whatever it is you have got there, the MgO mixture.

MR. PEDERSEN: There have been no experiments performed up until just a couple weeks ago which addressed the question of long-term penetration of UO_2 into MgO.

MR. BENDER: That still leaves me, there has been some analysis.

MR. PEDERSEN: There have been analyses, yes.

MR. BENDER: In doing experiments, normally you do it because the analysis had a spread that was so big that you couldn't give -- live with it. and so I am asking now --

MR. PEDERSEN: -- what is that spread?

MR. BENDER: What is the time span we are seeing? Are they trying to condense the time or increase the time by doing experiments that show it is longer than we have to assume?

MR. PEDERSEN: Some of the questions are really with respect to what is going on in the bed.

We didn't, with respect to the MgO, one of the questions you want to ask, will the MgO, being lighter than UO_2, you want to ask will bricks float out?

And that is where you need fairly big experiments to address that question because if your bricks can float out of the delay bed, you can in essence enhance the penetration considerably.

The actual mechanism of penetration is also a little bit uncertain, in that UO_2 and MgO forms a utectic, and there have been very little experiments or analysis directed at actual penetration

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of one material into another with that form of utectic mixture.

And the analysis in essence ignores those issues so far. So I don't really know if I can answer your question as to what is the uncertainty in terms of the penetration. That is a good question.

This shows the results of the experiment performed at Argonne a couple -- about a year ago, which looked at the penetration of UO_2 into concrete that was direct electrically heated on a continuous basis.

The apparatus here basically consists of, as we all see is half of it, Brass "U's" that are insulated from one another. So the two sides are in essence high resistance path.

We have an electrode here, which in this case is graphite.

Another electrode here on the other face of this apparatus, and we are able then, by direct electrically heating of the UO_2 to generate molten UO_2 that can interact with concrete.

With this experiment, it was carried on for approximately one hour, we saw a penetration in that time of about five centimeters.

Now, what w characteristic is we

generated this, this layer down here which is a mixture of UO_2 and concrete. UO_2 and concrete are miscible in one another and generate a low melting point utectic almost at the concrete melting point.

We saw loss of gas release of the top surface, and we saw some throwing of that, of the material off of this surface, up, and actually generated a mound of molten material off the top.

Now, these experiments have only been done on limestone concrete, and they have only been done for limited times, limited being an hour or so.

But these experiments done will form the basis for developing of an adequate penetration code, being Q, you can calculate the penetration of materials, molten core materials into concrete.

We have performed an experiment just a couple weeks ago which in essence repeated this in a slightly different apparatus.

Both Argonne and AI have been looking at the penetration of molten UO_2 into MgO.

The penetration, you might say, "Well, you guys penetrated into concrete. You ought to be able to do it into MgO."

Well, the problem is, the MgO has a

55 much higher melting point, utectic temperature is like 2200 degrees C as opposed to concrete, which has a utectic melting point with UO_2 of around 1300 degrees C.

So you are talking about almost another 900 degrees C.

So both Argonne and AI have been looking at programs to develop methods to experimentally assess both UO_2 presentation into MgO.

Argonne has been using in essentially the same apparatus that was used for the concrete, with the only real -- this was an experiment which was a preliminary experiment performed approximately a year ago with a ten by ten centimeter cell, with thirty-three centimeters of Harklase brick. Harklase is a manufacturer of MgO brick, very high density, about ninety-nine percent dense.

In the experiment we generated a molten UO_2 pool, but we were only able to penetrate approximately a half-centimeter into the MgO.

At that time, the radial heat loss to these bases, which are insulated, was matching the generation that they were putting into the molten UO_2.

We have just last week repeated this

experiment with a twenty by twenty centimeter cell, and we were able to achieve penetrations in the order of three or four, three centimeters.

So we believe that now we are capable of looking at longer term penetrations.

In this experiment, the UO_2 is direct electrically heated by dual resistance heating from one electrode to the other electrode.

AI has been attempting to develop an induction heating method. What they have is a low frequency induction thermos, a hundred kilowatts, about 3600 hertz, in which they would like to direct, directly inductively heat the UO_2.

UO_2 at room temperature has a very low resistance, but as your temperature gets up near the melting point, above, actually somewhere above 1500 to 2000 C, its resistance drops fairly fast.

And the goal is in essence to transfer and directly inductively heat the UO_2 itself.

Now, they have run two experiments with stainless steel. They used stainless steel layers.

The goal of the experiments is to hear the stainless steel hot enough to transfer the

57 temperature to the UO_2. When the UO_2 gets hot enough, you should be able to transfer from heating the stainless to inductively heating the UO_2.

Now those, the first two experiments were not successful. They are going to now take and put a tungsten layer here.

What happens with the stainless, it melts and runs away. So you now have no way of heating the UO_2 at all.

By switching to a tungsten layer, the tungsten is not going to melt until actually above the UO_2 melting point. And hopefully you can generate an open UO_2 tool and directly heat that.

Their go for their experiment is sometime in the next month to perform that experiment.

That was all I had to say. Any questions?

MR. GAVIGAN: The next speaker is Lou Muhlestein from HEDL.

MR. MUHLESTEIN: What I am going to try to do is summarize some tests and some results which have been completed in terms of sodium-concrete reaction. I want to take a few minutes and realize, I guess we all do, in the postulated core meltdown incident we have core debris that we talked about,

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you have sodium. They are separated from the concrete by a steel liner.

But you try to break the problem up into small enough pieces that you can analyze, and this is the thing we are trying to do.

But we have to be careful not to research the problem to death for its own sake, but put it in total perspective.

So let me remind us that the issue of LOA-3 is to demonstrate even if we have a core meltdown we can maintain the containment integrity.

And there are two things that may violate containment integrity; overpressurization or hydrogen buildup.

And as you break this total big problem into smaller problems and look at them, the fact is that sodium-concrete reactions do provide a source term for both energy and hydrogen generation.

And so what we are attempting to do is provide a basic understanding of these reactions, and developing also the analytical tools that we can predict the rate and quantity of hydrogen and energy which is released under these accident scenarios.

I want to show you a matrix of a series of tests that have been performed at the

Hanford Engineering laboratory. And what I want to do is break them down and talk about specifics.

So there is a matrix, you can look at it.

What I have done on the next slide is to break this up in terms of the variations of parameters that have been performed in these tests.

First of all, in terms of sodium, the cool temperature has ranged from five hundred to eight hundred sixty degrees Centigrade in that test; the amount of sodium, from one kilogram to approximately four hundred fifty kilograms; contact time, three to five hours.

The concrete, from very small to nearly three square meters of concrete; thickness, a couple of inches to two feet.

Type: Basalt, limestone and magnetite.

And the water content, normal water content or that which the water has been carefully griffined from the concrete prior to the test.

And then because of additives, we have looked at sodium hydroxide, pre or post-sodium spill.

I would like to in somewhat of a summary fashion treat each one of those issues.

First, let me talk about sodium pool

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temperature. The water released from heated concrete is dependent upon the driving temperature and the time. So the amount of sodium-water reaction you would expect to be dependent upon the sodium pool temperature and the time that that sodium pool temperature was being applied to the concrete.

We know the mechanical properties of concrete are also temperature dependent.

There is data that exists both on differential thermal analysis and large scale, which show a temperature threshold exists, below which sodium-concrete reactions will not occur at a very substantial rate.

Put that together, however experience is that the total chemical reactions do not appear to be strongly dependent upon the sodium pool temperature as long as you are about the threshold temperature. That is in that range from five hundred ten to eight hundred sixty, the reactions have been quite similar.

What I want to show you is just a table of some differential thermal analyses performed early in this program.

The thing of essence here is the fact that in the basalt concrete there appears to be an exothermic temperature around twenty-five hundred degrees Centigrade, with something like one hundred ninety-five calories per gram of energy released, or magnetite around five hundred again, and for -- or this is magnetite, and limestone is here.

But the point is it is around that temperature regime, and we realize the differential thermal analysis may be somewhat insensitive, but the point is that there is some temperature has to be reached before these reactions can occur at a very substantial rate.

In terms of the amount of sodium, shallow sodium pools tend to totally react until all the sodium is consumed.

However, there have been tests where deeper sodium pools have not totally reacted. So sodium pools may or may not react until the sodium is consumed.

The total sodium-concrete reaction penetration does not strongly depend on the sodium pool depth. This, I think, this counts an earlier sodium hypothesis that the sodium pool had to be saturated by sodium hydroxide before [illegible] reaction could occur.

And let me point out some data that

illustrates that issue. And I want to look at these two sets of curves.

Here we have the hydrogen accumulation is a function of time. The reaction penetration is a function of time. And the remaining sodium left is a function of time.

First of all, let's look at the hydrogen generation.

The depth of the sodium pool was 6.3 centimeters to about four and a half kilograms; 15.2 centimeters, about 10 kilograms; fifty pounds and a hundred pounds. Those conversions!

The point is that for the shallower pool the amount of hydrogen given off is certainly less. It increases as you go to the deeper pool.

Let's look at this maximum reaction penetration, which is the dotted curve here.

For the four and a half kilogram, all of the sodium reacted. It penetrated somewhere in the order of five centimeters total.

We increase it to about 10 kilograms. The reaction penetration, about five or six centimeters, but in fact while you start to see that there is some sodium remaining.

And the numbers here are the times at

which the reaction, certainly when the sodium is consumed the reaction terminates.

As you go on, we go into longer times. For example, you get down here to a hundred pounds of sodium, forty-five kilograms, you had nearly twenty percent of the sodium remaining. And you notice that the reaction penetration tends to level off. It does not continue at the same rate for an indefinite period of time.

Let me turn to sodium contact time. Again, the water release from heated concrete is dependent on the driving temperature and duration of that heat flux.

However, in tests which we performed independently, most of the free water is evolved in the early time frame. Therefore, the amount of sodium water reaction you would expect to be dependent upon time and temperature, with a majority of that reaction occurring early in time.

And what we have in fact observed is that most of the sodium-concrete reactions occurred in the early time frame, zero to two-hour time frame, with little noticeable reactions beyond that.

And let me illustrate that point by two pieces of data.

The first is a large basalt test, about a meter square, four hundred fifty kilograms of sodium.

And what we have plotted here is first of all the pool temperature is a function of time. And then the thermal couples in successive depth to the concrete.

What you see is within three hours, down to even fifteen centimeters, as you look at this test in the long-term, that is after a hundred hours, the reaction had not penetrated more beyond this temperature, or this point here.

So you see the reaction occurred in a very short time frame. And even though there was sodium left over to some forty, fifty hours, the penetration was fairly slow. So it occurred fairly rapidly.

MR. BENDER: Before you take that off, my recollection isn't very good, nor is my understanding, but it would seem to me that the Sandia tests had suggested that there wasn't any point at which the sodium stopped penetrating.

Did I misinterpret what they said?

MR. MUHLESTEIN: No. You have to be careful. The Sandia tests, many of them were sodium-limited, so

65 so there is a fixed amount of sodium. And the conclusion was it reacted until all the sodium was consumed.

The conclusion was therefore it would react indefinitely. And I will show you some results on large-scale tests.

All their tests, the majority of their reactions and reaction rates were in a very short time frame. But I don't believe personally one can conclude therefore that in longer times it would continue at that same rate.

They were sodium-limited and you have to be careful in drawing that conclusion.

Let me show you the results of the other test which was not sodium-limited.

This is a large limestone reaction test, which at test conclusion was over fifty percent of the sodium left over. So there was a large amount of sodium.

Again, you see down to even a depth of 7.6 centimeters, a reaction occurred in the early time frame, down to that point of time.

But on disassembly, the total reaction was not beyond 7.6 centimeters, and there was plenty of sodium left over.

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MR. BENDER: The explanation for this limit is what?

MR. MUHLESTEIN: I will get to that.

MR. HARTUNG: Let me ask a question before you leave this.

Is the reason, as you go deeper, the temperature doesn't go up very greatly? Is that possibly because it was cooled at the bottom in some way?

MR. MUHLESTEIN: The situation is where you have the hot sodium applied to the top, that is by thermal conduction heating the concrete below, it takes a while for thermal conduction to heat the sodium.

There is a surface reaction occurring, but water is driven deep from within the concrete.

We find that typically eighty percent of the water is going in the total test duration.

But you see these thermal couplings, and I didn't show you the curve, even at long terms, in this test, fifteen to twenty hours, they were not very hot.

MR. HARTUNG: So the delay is just a matter of conductivity? There is no cooling on the outside.

MR. MUHLESTEIN: There is no forced cooling on the other side, no forced cooling.

MR. HARTUNG: Well, I guess that is the point I was getting at. Even if you have just ambient temperature on the other side, I will think about it on my own, but it could be there is a substantial cooling effect occurring unless you have done something to mock up what, if you had, let's say, a very deep bed in a reactor, you would have no capability for cooling the other side, even ambient.

MR. MUHLESTEIN: But I still could expect, the heat has to be from the top down, and you start at ambient temperature.

And our thermal analysis model says it takes some time for that concrete to get hot to drive off this water. So I don't anticipate this to be atypical.

MR. MARK: The fact that the bottom thermal couplings don't get off the ground at all means there isn't much heat flow.

MR. MUHLESTEIN: It takes a while for the heat to conduct itself down to the concrete, and it would in a real situation, too.

Let me talk about concrete pipe and size.

It seems that the limestone test

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results were more consistent as we looked at the data, between the small and large scale tests. And in all of our limestone tests, even I will show you some data earlier where we had some deeper penetrations early, but the end result is that the chemical reactions were self-limiting.

For basalt, however, most of the small scale tests had limited reaction penetration, but it didn't continue forever.

But the reaction for larger scale tests were more extensive. There is more uncertainty in the sodium-concrete reaction with basalt than with either limestone or magnetite.

Let's look at water content. We have done a series of tests with dehydrated concrete, and dehydrated basalt concrete reacts extensively with sodium.

But as you would expect, the amount of energy and hydrogen is considerably reduced. The water is gone, the hydrogen is gone, and the energy is reduced.

Dehydrated magnetite concrete, on the other hand, does not react extensively with sodium. And although we have not completed the test, there is data to suggest that the limestone concrete, dehydrated

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concrete also does not react extensively with sodium. And that has to feed in our understanding of formulas and what is going on in the total problem.

And let me, to illustrate, go back to this curve, which now I want to concentrate on these two.

We have the hydrogen accumulation is a function of time, and the hydrogen accumulation and the reaction penetration function of times.

The first test, one and six were dehydrated with nearly eighty percent of the total water used in the concrete driven off before the test was started.

Where the four, SET four had the normal water content.

As I said earlier, the amount of hydrogen generated is greatly reduced because the water is gone.

If you look at the hydrogen generation here as a function of penetration, this is the dash line here is the dehydrated basalt concrete. Very little hydrogen, this curve. But the pentration was rapid and extensive. Merely a foot of concrete in a very short time frame reactied right through it.

Whereas the amount of hydrogen given

off in the concrete which had the full water content had relatively little penetration.

That is a surprising result for us. We were looking at the effect of water on the reaction period, we have had to revise our thinking just a little bit. And I will try to wrap that up as we continue.

There have been a number of tests looking at additives.

The question is what about sodium hydroxide. Sodium hydroxide alone does not react extensively with basalt or limestone concrete. There is a little erosion, but nothing extensive.

Sodium hydroxide has been added before or after or during a sodium spill, and it does not drastically alter the reaction in terms of how much reaction occurs.

However, sodium hydroxide, which is formed from the sodium water reaction, is an important mechanism in the total concrete reaction.

You see, we tried to look at it a piece at a time. It didn't give us the results we anticipated.

But it is important in the total understanding of what we think is happening.

Let me talk about some observations

that we have seen from a large number of tests. It has been very striking that in those situations where you have deep, rapid reaction penetrations, they are characterized by a very voluminous friable type of reaction product.

On the other hand, for those situations where they were characterized by a limited reaction, the reaction products were very glasslike and hard.

Now, that has to be fed back into our understanding.

The extent of the chemical reaction penetration is reflected by the hydrogen generation. And I will illustrate that by some data.

The point is we don't know which is the cause and which is the effect; hydrogen released because of rapid penetration, or rapid penetration occurring because of hydrogen release. That is a fact we have got to contend with.

We do note, I believe, that the sodium-water reaction is dominant. Water deep within the concrete is thermally released. In the test up to eighty percent of the content of the water is released. Water migration is to the hot surface by pressure-driven flow, so there is not much going off the cold surface. And it dominates the hydrogen and energy release.

I will just point out, there have been a number of tests, a question, what happens if you have a liner but the liner is faulted, will the sodium proceed through the fault.

The first thing is the fact that the insulated material does thermally insulate the concrete in the early time frame. That is important from the amount of water griffined off. And hence the energy and the hydrogen generated, there is a reduced rate of water released and hydrogen released.

Obviously if you thermally insulate the concrete, depending on the material, it has, it may inhibit the sodium migration to the surface.

If it is salicious kinds of material like basalt, it will probably, the tests seem to indicate it does react much like that basalt concrete. But in that early time frame, which is important, they will tend to insulate the concrete, reducing the water content and energy.

And I have to remind us that is the thing we are concerned about.

Let me show you some examples. This is one of our large scale basalt tests. What we have plotted here is the hydrogen generation, generation rate, the penetration and penetration rate, and as functions of time.

First of all, the red curve is the measured hydrogen generation rate as a function of time. That measured, we integrated that to find the hydrogen generation, and that is this curve right here. That is an experiment. That is a determined parameter.

They then measured the reaction penetration into the concrete, the circled data here. And we integrated that to get a penetration rate.

And the thing that is important, again, is that the pentration rate envelops the hydrogen release rate. And it occurs in the early time frame, and with a little given off beyond that.

Again, I said I don't know which is the cause and which is the effect of that, but it is important to realize that on those time frames we are having large quantities of hydrogen evolve from the sodium-water reactions is the same period of time you are getting the rapid pentration into the concrete.

MR. BENDER: I don't know if I understand what you are telling us, so I am just tryingto ask a couple of questions.

MR. MUHLESTEIN: I an not sure I have the answer, but I know what the data says.

MR. BENDER: You are measuring the reaction of sodium with free water primarily.

MR. MUHLESTEIN: With free water which is driven from the concrete, so it is actually sodium onto the concrete. But there is water given off, and that is what is giving the hydrogen.

MR. BENDER: Is the combined water also reacting?

There is cement.

MR. MUHLESTEIN: That's right. You have what is of all evaporable water, which is maybe fifty percent of the water, that is certainly being driven off quickly.

But also because of the heat load to the concrete, we also get some of the bound water given off. That is nearly eighty percent of the total, the evaporable water, plus the bound water.

MR. BENDER: What is dominating the hydrogen generation?

MR. MUHLESTEIN: Total water.

MR. BENDER: Obviously the total water. Is most of it doing something?

MR. MUHLESTEIN: Again, the free water comes off at a lower temperature. The bound water comes off as a function of temperature and time.

In concrete particularly, in the order of fifty percent is what we call evaporable, and the other fifty percent is what we call bound water.

MR. BENDER: You are trying to show a curve which has a time element, and I can see most of the reaction is free water in that two hours, and the rest of the water tied up in the concrete isn't going to react for a while. And I don't know what you are telling me, so I am asking.

MR. MUHLESTEIN: The dominant early time is evaporable water because that comes off quickly and faster.

However, as part of this is also bound water in the distances not very deep within the concrete.

Now, if you remember the temperature curve, deep within the concrete it doesn't get very hot. Therefore, we don't have the bound water. We have the evaporable water. But in the top surface we have got the evaporable and the bound water, so I can't say which is more dominant, but obviously the evaporable water comes off at lower temperatures, faster.

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So in the early time frame it has to be a dominant factor. Eventually you get it all.

You have to consider the heat load to the concrete and the total water content. I can't isolate it to just the evaporable versus the bond.

MR. BENDER: Go ahead.

MR. MUHLESTEIN: They both contribute.

What I want to point out is another large limestone test.

MR. BENDER: I think you have to understand that to some degree, in order to make some argument about what the hydrogen influence is.

MR. MUHLESTEIN: I guess I should point out, we do have a model, and basically the first water to come off is the evaporable water. But later in time the bound water comes off as well.

And you have to look at that as a function of the heat load to the concrete, because some places both is coming off, and other places just evaporable water.

What I have here is another large limestone test in which we have the hydrogen generation as a function of time. And the hydrogen generation rate, it is very low, not very much, eighty gram moles per hour.

77 But again it peaked in the early time frame.

I am going to contrast that with another large-scale limestone test very similar to the first, in which we had the early time frame, a very rapid hydrogen generation rate, the order of maybe three hundred fifty gram moles per hour, again, in a very short time frame.

But those two tests at [illegible] looked very much the same. This had a more rapid penetration rate than the other one, as again evidenced by the hydrogen generation. But the total results are very similar.

And I need to point out, too, that the total hydrogen generation, also are very similar for the two tests. How they came out were different, but the total end results were very identical.

I want to spend just a moment on current understanding. Certainly sodium-water reaction produces the sodium hydroxide and hydrogen.

Both small and large tests indicated a reaction threshold around five hundred degrees Centigrade.

Most of the sodium-concrete reaction tests do not continue. Like I say, because of some sodium-limited tests, that assumption has been drawn. But we have tests in which both all reactants have been available after, we can feel that the sodium-concrete reactions consider almost a protective reaction product layer is formed, reaction product, sodium hydroxide, sodium carbonate, which are more effective in forming a reaction product layer than solid reaction products.

And this is typical, when we get that glasslike, very hard reaction product layer, that is typical of a viscous liquid product layer, which then prohibited further sodium from coming in contact with further solid materials.

Obviously the water vapor continues to migrate through it and react with the sodium. And that is why the sodium-water reaction, I think, is dominant.

So from this we conclude that most sodium-concrete reaction tests have shown that the chemical reactions are self-limiting. Most of them occur in the early time frame.

I think I can generalize that to all of the tests we have seen. The large reaction rate has occurred in the early time frame. We have to be careful and not extrapolate for those tests which were sodium-limited.

But under many conditions we have seen tests in which a protective reaction product layer has formed which separates the sodium from unreacted concrete and terminates the reaction.

Some tests have been sodium-limited. Some may have been water-rate limited. But when we overcome those two things, in the long time frame, lots of water, lots of sodium, we find things happen in the early time frame. But they do not continue at that same rate. I think that is important point.

Also, limestone concrete is more resistant to reaction with sodium than is basalt.

I want to show you just one other slide which is not part of your handout. As I said, sort of the committee, we are trying to find the hydrogen and energy generated. This is one test in which we had enough data that we could actually calculate the energy release as a function of time for basalt.

We have to make the assumption, first of all, we can calculate the hydrogen, the energy release from sodium-water reaction. That is somewhat easy to do, based on the hydrogen generation rate that considers all the water.

One has to assume that the silitous reaction is mostly coming from the silicate.

Now, the concrete is very complicated mixtures of minerals. But as you look at those minerals, the majority is silicate, though it is not in free silicate. It is tied up in a number of compounds.

But if you consider that as energy from the solid reactions is dominant, you get this curve. You add the two together, you get curve one.

What we attempted, take the differential thermal analysis which looks at reaction products, and we multiply. That is far too many joules per amount of material.

And we knew at the N state how much material had been reacted, and so we calculate at an energy, and that is the value there at the long time at forty hours it was this number.

We know how much water was driven from the concrete. Therefore we know how much water reacted, and that gives us an energy number there. You add the two together.

And I think the agreement is fortuitous. But what it tells us in terms of these calculations, taking the energy from the hydrogen reaction and the energy from the differential thermal analysis and the amount of material consumed gives us a fairly good pound of total energy which is produced.

It is the kind of thing, yes, we are in the ball park, and yes, we can calculate. And with that I will end.

MR. MARK: When you refer to limestone concrete, is it the same this week as it was last year in its detailed chemical analysis?

MR. MUHLESTEIN: Let me refer specifically to the Clinch River limestone mix. That is the thing we know more about.

Over the last several years the mix specifications have changed, but they have changed slightly.

MR. MARK: I am wondering about the chemical composition.

MR. MUHLESTEIN: Long-term chemical composition?

MR. MARK: One of them had a certain amount of silicates. Does the next batch have the same number, or is it dug from a different corner of the gravel pit?

MR. MUHLESTEIN: Again, the silicates would be the basalt. For the limestone it is more dolemite, and there is a number of other aggregates and combinations.

I think in the long term the chemical nature of the concrete is not changing.

There is some change in chemistry as the aging time progression in terms of aging, water changes, but I don't think that is a major effect. At least we have, for example, in the test series we looked at these reactions with concrete cured just twenty-eight days, and some cured over three years. And we don't see any major difference. The chemistry analysis we have don't show large differences.

But I have to point out in Petrographic analyses by very skilled people in concrete, it is more than a guess as to what is really there.

MR. BENDER: When you say limestone concrete, are you saying that the mortar mixture is ground limestone?

MR. MUHLESTEIN: I am saying that's the fine aggregate and coarse aggregate is dolemite or limestone rather than a basaltic kind of mixture.

The cement paste is very much the same. Earlier we thought because the cement paste was dominant in the water --

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MR. BENDER: When you say basalt concrete, fine aggregate and coarse aggregate is all a form of rock broken up to some degree?

MR. MUHLESTEIN: That's correct.

MR. CARBON: Thank you.

(Short break in the proceedings.)

MR. GAVIGAN: The next speaker is Lou to talk about LOA-4.

MR. BAKER: I would like to introduce the LOA-4 program. LOA-4 refers to attenuate radiological consequences.

Our top level objective is to demonstrate a high probability of obtaining a large attenuation of consequences from a radioactive release inside containment by inherent mechanisms and by engineered systems.

Our second level divides this into engineered attenuation and inherent attenuation.

Louis Muhlestein will be back in a minute to describe our program on air cleaning systems, which is our total engineered attenuation program.

Then Ravnesh Amar will be up to describe our inherent attenuation program, which includes aerosol behavior program at HEDL, high density studies at AI.

Our studies of leak plugging by aerosols, sparging source term development at GE, and our review of environmental attenuation models at Oak Ridge.

Our strategy in this area is one to develop efficient, reliable vent/filter systems such that they are available.

The technology base is available if designers wish to use this approach.

And also to demonstrate dose reduction by natural processes.

The approach to this is to, as I mentioned, support the containment air cleaning program at HEDL; to look at aerosol behavior experiments to demonstrate the rapid oglomeration/fallout of sodium and fuel aerosols.

We have initiated a program called ABCOVE, of aerosol code comparisons and experiments, large scale experiments in the containment systems test facility at HEDL.

The analysis program will predict the results of planned experiments as the key feature of the program.

We have also developed a -- we also

wish to establish a sparging source term, that is the availability of radiological species from the reactor cavity by bubbling, either by sodium boiling or by the bubbling of gases releases through concrete through the fuel-sodium mixture in the cavity.

And also, to demonstrate the self-plugging tendency of containment penetrations, and to establish improved environmental attenuation models.

With that I will turn it over to Louis again.

MR. LIPINSKI: Question: What is the self-plugging material? Are you talking about sodium systems?

MR. BAKER: It has been shown that sodium aerosol has a marked tendency to plug small holes. And this appears to be worthwhile exploiting, with regard to showing that leaks and containments may very well be explained.

MR. MARK: How do you exploit it? Do you drill little holes in the containment?

MR. BAKER: It is said --

MR. MARK: Never mind.

MR. LIPINSKI: The words containment presentation are not the words, but if you -- lines, but if you develop cracks, that is the way you are using the term containment penetration.

MR. BAKER: Yes. It is independent.

MR. LIPINSKI: If the pipes are open you will get the material through the pipe. Then it is a function of pipe size.

MR. GAVIGAN: And concentrate of sodium, that is they are very rapid, one-inch, two-inch, three-inch, four-inch, isn't that right, ram?

MR. AMAR: In fact, that is one of the concerns.

MR. MUHLESTEIN: Yes. With high aerosol concentrations, thirty to a hundred grams per cubic meter, we would get, if you have flow through four-inch duct it will plug very rapidly.

MR. BAKER: This should say leaks rather than penetration.

MR. CARBON: Thank you, Lou.

This is a good time for a break.

(Short break in the proceedings.)

MR. MUHLESTEIN: I have two boundary conditions. I am supposed to talk slower for this young lady, and I am supposed to speed up to keep on schedule.

MR. BENDER: How about skipping some?

MR. MARK: Maybe if the transcript contained only every other word it would be almost as valuable.

MR. MUHLESTEIN: What we are going to do is talk about engineering attenuation.

As Lou pointed out, the issue here is to be able to attenuate the release of radioactive aerosols within engineered systems.

There are two phases: one, internal systems; the other, exhaust systems.

For internal systems, we call those that act within containment to reduce the source term.

They can be categorized in terms of direct acting features, that which will enhance the already natural phenomena of oglomeration, settling and played out of the radioactive aerosols so they do not become part of the source term; or recirculating systems, which internally would circulate the radioactive particles through internal filtration device, and feed them back into containment.

For exhaust systems, we are talking about the collection of radioactive aerosols as exhausted from containment from a controlled manner to environment. That is the key issue, in a controlled manner.

Two things are necessary. One, if you choose to vent to prevent overpressurization of

88 containment, or if you choose to purge to control the hydrogen concentration in containment, and we will talk about both active and passive systems.

Our aerosol, or excuse me, our containment air-cleaning program, shown pictorially, is to develop and improve test containment systems.

You will hear Ravnesh Amar talk about the aerosol oglomeration program.

These tests are performed in the HEDL systems facility, which is in essence a large vessel, twenty-five feet in diameter, sixty-seven feet in a rural height, which is approaching on the right scale, the size of large containment as far as vertical height is concerned.

We have capability of handling either sodium sprays or sodium pool situations to create the containment systems.

I want to point out first that a path which has not been completed was to evaluate commercially available air-cleaning systems. That was first done for a postulated head reliance accident.

So we looked at the available air-cleaning devices and chose those which had most promise of being effective.

We then evaluated the same systems for a vent purge mode, following a melt-through accident, primarily in the Clinch River kind of an accident situation.

We then repeated that evaluation for the postulated SFTS conditions, and like I say, those have been completed.

The kinds of systems that we looked at I will divide in terms of active systems, prefilters along with HEPA combination, two or three stage aqueous scrubber.

For the passive systems, the sand and gravel bed, and a new system which I will talk about which we call our submerged gravel scrubber.

And I want you to realize never did we feel that the HEPA filters would adequately contain the amount of airfalls one would expect in a melt-through kind of accident, but things we had to look at in terms of smaller accident situations where HEPA systems may be used.

In that regard, HEPA filters, as you know, have a high removal efficiency. However, their mass loading capacity is variable, and it depends on the aerosol moisture content and the composition.

It is low in relation to the amount

90 of term that one anticipates would be available.

There is a reliability question with the corrosive sodium hydroxide aerosols in long-term, and it would require large filter bags, as I said, to achieve the mass loadings that are anticipated under accident conditions.

And so this was not a serious consideration. But it was something we wanted to look at for completeness.

Basically, due to suggestions made by ACRF in relation to FFTF, they continually ask about sand and gravel bed filters.

They have some attractive characteristics in that they are passive, high reliability. They withstand temperatures in a corrosive environment.

Their particle-removing efficiency is high for a broad range of air velocities.

But the fact is they operate at low velocity, meaning they have to be big in general. They have low mass loading capacity, again, requires a large surface area.

And because of this they tend to be costly.

Evaluation, the conclusions of our evaluations is that the loading capacity per unit area is too small for the anticipated melt-through accidents, such that the relative size and cost would be prohibitive.

We did look at them, however, did some experimental work and we found that we could optimize the loading capacity by changing the aggregate distribution and the superficial gas velocity by a factor of about two.

Therefore, we say that they are not recommended for situations which require high mass loading cases.

MR. BENDER: Before you get off that subject, the term prohibitive is hard to deal with in this discussion.

Are we talking about the cost being some fraction of capital investment plan, or how do you decide prohibitiveness in this case?

MR. MUHLESTEIN: Let me just say for shortness of time that we costed out a system, for example, for FFTF for Clinch River, and it looks like for what we were going to getout of it, that was just too expensive to put on. There are better ways to go.

MR. BENDER: That is not, since we are dealing with an issue here that as a matter of fact is about as important for water-cooled reactor stores as it is for LMFBR, it is important to know the absolute terms, what a system like that might cost.

MR. MUHLESTEIN: Okay.

MR. BENDER: How you cost them.

MR. MUHLESTEIN: I won't tell you what it is going to cost, but I will tell you how big it is going to be.

A large breeder reactor, for the sand and gravel bed, would have to be two football fields in size.

Now, people have taken those numbers and have run them through engineering analyses. I don't have the numbers, but that is the size.

MR. BENDER: And is that because of the existence of the sodium oxide particles?

MR. MUHLESTEIN: That is because of the large mass loadings, sodium hydroxide aerosols.

Now, with light water reactors there is another problem in terms of magnitude and rate.

MR. CARBON: That was for a thousand megowatt size plant.

MR. MUHLESTEIN: I think that in essence, or

93 Clinch River.

MR. CARBON: When you said a couple football fields, what is the volume?

MR. MARK: The depth?

MR. MUHLESTEIN: I believe we are talking about something like 4.6 feet high and two football fields.

MR. MARK: And there is a football field at Madison. You can go look at it.

MR. MUHLESTEIN: The point is from superficial gas velocity you can back out and get the size, roughly, as the two football parks, four to six feet high.

MR. BENDER: There is a question of gas distribution.

MR. MUHLESTEIN: That's right. And how do you feed that to get uniform distribution.

As I said, there are more viable alternatives. Three-stage aquaeous scrubber systems, shown in the photograph, where we have a quench tank, the red chamber there, then followed by a Venturi scrubber, and a high efficiency fibrous scrubber to cool incoming gas to acceptable levels.

Most of the material was collected in a Venturi scrubber, but you have a wide range of particle size, and the high efficiency fiber scrubber is intended to take out the small particle size of that distribution.

Shown here we injected sodium iodide, we missed in to look at the materials.

The other system we looked at was a two-stage system depicted here, in which we removed, now, the quench tank, and just used a Venturi scrubber followed by the high efficiency fibrous scrubber.

And I will show you some results.

MR. LIPINSKI: Relative to the two football fields, how many millions of gallons does that system need?

MR. MUHLESTEIN: Small. These units you saw are now CFM units, and the tanks are what, about four or five feet in diameter.

For FFTF system, which is being installed, I don't remember the capacity. It is the order, or not very much more than this area, much smaller.

MR. LIPINSKI: The total volume of water, because you are showing draining that system on a continuous basis.

MR. MUHLESTEIN: We have it on a drain

because we want to collect the material so we absolutely know what went in and what went out.

The FFTF concept is one total system. I don't remember the volume, but it is not that big. It is acceptable.

MR. BENDER: It is a recirculating system. You just pump the water out.

MR. MUHLESTEIN: You have a big tank and pump it. You are circulating. You have to worry about the concentrated sodium hydroxide that is in that, and from that we can calculate how big it has to be.

Let me show you some tests. Tests 1 through 4 were with a three-component system. 5 through 6 were two-component system.

We looked at dry sodium peroxide, we had sodium oxide, sodium carbonates. Again, added water to the sodium hydroxide. We tried to vary the aerosol composition through a wide range of anticipated numbers.

The aerosol concentration, this is in grams of sodium per standard cubic meter, and so normally speaking, that is the order of fifty grams per cubic meter in total airfall concentration.

We try to be specific, and particle

sizes and the amount of actual material which is removed.

Let me then quickly show you, for removal of efficiencies for the three-stage system, loose, you collect some in the duct, the quench tank for the first fifty-one percent. The majority of the material is caught in the Venturi scrubber. As I said, the fibrous scrubber is for the small particle size material.

The point is, this unit, three scrubber system or two scrubber system, is over 99.9 percent. We started looking at ninety-five percent. We would be happy, in fact, it is over 99.9 percent efficient.

We also looked at the very small particles that would be formed from the sodium iodide. That is very much different than the big agglomerates of the sodium oxides and hydroxides. So we took sodium iodide. But again, the total system is ninety-nine percent efficient.

You see the high efficiency fibrous scrubber doing its job, picking up.

MR. BENDER: What is the nature of the fiber?

MR. MUHLESTEIN. It is a packed polypropylene fiber.

It is a commercially available unit from Monsanto.

The conclusions are that the sodium removal average, as I said, larger than 99.9 percent. With the iodie it was again larger than 99 percent.

It is independent of the sodium aerosol composition.

The two systems, two or three-stage systems, are very comparable. However, the two component system is more sensitive to the flow conditions, the water flow conditions with the air, so you have to be a little bit more careful in the design of that one.

We looked at some ducts, too.

MR. MARK: If you had a lot of fission fragments in your aerosol, would that fiber scrubber get into trouble with a heat source there?

MR. MUHLESTEIN: We don't think so.

The reason I say we don't think so, we are looking at that. We have to separate effects tested to tell us, but we think the majority of material is going to be collected in that system in the Venturi scrubber anyway.

Obviously if you have large particles

in the small fiber, it would be very dangerous, plugs it up very rapidly.

But all of the things, most of the material, even the big stuff and the fission fragments we think would be caught in the Venturi scrubber, although we have a separate test.

MR. BENDER: That is such a loose term. If we were to try to define fission fragments, what would we be saying?

MR. MUHLESTEIN: The thing we are looking at is nonsoluble kinds of materials like plutonium oxide, uranium oxide. I think we are more interested in the nonsoluble materials.

Soluble materials I don't think we have any difficulty.

MR. BENDER: It is really broken-up fuel particles.

MR. MUHLESTEIN: Yes. Let me turn to the submerged gravel scrubber. I will show you a picture.

In essence, as passive system in the venting case, again, if you had to purge, there is no system that is passive there. It is a continuously washed gravel bed in that the density gradient of water flow or water slug flow provides a pumping mechanism so that you continuously wash the gravel bed.

And therefore you have large direction column. In essence, take the main features of an aqueous unit, the good features of a sand and gravel bed, merge them together and come up with a passive unit with a high-volume capacity, and that is what we did.

We add on to that a fiber demister pad, and the water then entrained from the pool continually washes that fiber demister pad, and you also have a positive pressure seal from the water pool.

Let me show you the engineering.

The engineering model of that system is shown in the figure here. In essence you have the gravel bed which is immersed in the pool of water, the incoming gas stream comes into the bottom, then back up through the gravel bed.

The density difference, I said, continually washes the gravel bed. The gas again goes out through the fibrous cylinder, and this entrained air is enough to keep that fiber bed moist.

So again, as far as a vent system is concerned, it is passive.

This shows one demister pad, and that shows pictorially, too.

Let me show you very quickly --

MR. BENDER: Does water carryover have an effect on the filter?

MR. MUHLESTEIN: The one up above?

MR. BENDER: The polymer.

MR. MUHLESTEIN: The packed polypropylene, it has to be continually washed. But there is no chemical reactions. We have looked at long-term chemical reactions.

MR. BENDER: Just a matter of whether the carryover, the mist itself.

MR. MUHLESTEIN: If it is too much mist, too much water entrained.

MR. BENDER: Yes.

MR. MUHLESTEIN: We have not seen any bad effects from this. We did several experiments trying to look at those kinds of things,and as I recall, we didn't get too much.

MR. GOLDEN: If you did, you could just put it in a cyclon stage and get rid of it.

MR. MUHLESTEIN: There are ways around it. We have looked at a variety of kinds of aerosol conditions, various sizes in the tests.

Again, there is the engineering model demonstrating feasibility, and one of the first questions we had to address, is there enough water

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pumping as a function of the superficial gas velocity to continuously wash the bed.

This is the base case. We say yes, there is enough pumping. You can wash the bed.

What about the pressure drop, because obviously you want -- you have got the positive pressure seal, you don't want more than is necessary. So we look at the pressure drop as a function of the gas velocity. Actually it turns around and goes back up, so there is optimum value.

There is more data than this now, but it turns out the removal efficiency is not very dependent upon the superficial gas velocity. Within some bands it is all about the same number, and there is obviously a removal efficiency as a function of bed height, and we strive to maximize that.

Let me show you some test results for a number of parameters. Again, you notice for just the submerged gravel scrubber by itself, the removal efficiency is the order of ninety-nine percent.

For a variety of the things like aerosol type, bed height, packing size, so forth.

We add the fiber demister and we get up to larger than 99.9 percent.

This one was with just a demister pad,

102 not the packed polypropylene fibers, but a normal demister pad.

And it did not collect the small particle size. We really need to go back to packed polypropylene fiber.

In summary, we have done the development, eight aerosol tests, seventy-five hours of total operation. The aerosol removal efficiency was independent of the chemical form.

Aerosol, there was adequate gas cooling for the inlet gas temperatures up to one hundred twenty-five.

In the big test we have gone up to higher temperatures and shown there was adequate cooling in the gas stream.

Let me move really to the final thing. We have gone to a solvent CFM units, which is shown pictorially here as the large red tank.

The inlet gases coming right there, down through the bottom of the fiber, or through the gravel bed, bubbles up to the gravel be, out through two demister pads and out through this exit.

We left these two components out. We didn't want to tear them out until they were through with them. And we have tested this large unit, and its removal efficiency is larger than 99.9 percent effective with the demister pads being effective for the very small particle size.

MR. BENDER: Excuse me. I don't understand that system as well as I should. The gravel is submerged in water to some depth?

MR. MUHLESTEIN: That's right.

MR. BENDER: And the idea is that you minimize the pressure drop across the gravel, but keep enough water there so at least you have to bubble through some water.

MR. MUHLESTEIN: That's correct We looked at the bed height, height of water above it, a variety of parameters to see how this thing operates.

It turns out there is a minimum bed height below which you certainly don't get the collection efficiency.

But the ideal, you see, the collection method is by the compaction through the voids in the gravel. So it is like a gravel bed.

But the nice thing, it is washed out. The size of this tank depends on the solubility of the aerosols in the water. So you can again size that tank.

For example, this tank here is about

six feet in diameter, and that is a thousand CFM unit.

You could either go ten times that for ten thousand CFM unit, or put ten of them in.

The point is we have drastically reduced it from our two football field size down to something that is manageable in a plant.

MR. LIPINSKI: What is the equilibrium operating temperature in terms of the gas in, gas out?

MR. MUHLESTEIN: I am going to have to say I don't remember.

MR. LIPINSKI: Do you have a design target for highest gas temperature of operating?

MR. MUHLESTEIN: One of the tests we are going to do is heat up the inlet gas by propane burner to the odor of about twelve hundred degrees Fahrenheit, and then we will measure the outcoming gas.

For the gas, we don't get very hot temperatures in large volume, sodium burning, you get ordinarily on the order of three hundred degrees Fahrenheit, and the gas coming out here is one hundred, one hundred degrees Fahrenheit.

But the point is I don't have a number. We think this is sufficient cooling. That

105 test is coming up in a couple weeks. We will actually heat the incoming gas, get it very hot and look at the cooling.

MR. LIPINSKI: You will have water evaporation, constant replenishment.

MR. MUHLESTEIN: Within some level you would have to have a makeup system, that's correct.

And that leads me to our conclusion that air cleaning system with high removal efficiency have been developed which can remove mass loading anticipated in postulated accident conditions.

We have systems that will work. The submerged gravel scrubber with the high efficiency fiber scrubber, we think is the best option for filter/vented containments because of its specificity and size.

MR. BENDER: What kind of gas velocities go with that submerged gravel filter?

MR. MUHLESTEIN: I wish I remembered all those things.

MR. BENDER: I don't want to know exactly. Roughly?

MR. MUHLESTEIN: What are we talking about? Something like four-tenths of a meter per second.

MR. BENDER: Thank you.

MR. MARK: Is that for your thousand CFM?

MR. MUHLESTEIN: That was for the smaller unit.

MR. MARK: But you said the thousand CFM thing had a diameter of approximately six feet.

MR. MUHLESTEIN: That is the tank is about six feet. The gravel bed is about four feet, as I recall.

MR. MARK: You can get the velocity.

MR. BENDER: I have to know the void fraction.

MR. MUHLESTEIN: I am sorry. I don't have that number. We have it in the data bank, but I don't have it upstairs.

As the magnitude, it is not supersonic. It is acceptable. It is not going to pose a problem to engineers to build that.

We are going to build a ten thousand CFM unit to use, and also demonstrate you don't need that.

MR. BENDER: I think gas distribution in that system will be an issue, and we need to know better.

MR. MUHLESTEIN: It is. Now, what we have done is we have taken a pie-shaped section of it on a rather large, we have numbers, about how big, how the diameter has to be before you get good distribution.

As I recall, we could go out to something like four feet and still get good gas distribution.

If you go much beyond that, what you would envision in this is a tank with several of these units in it, sized so you do have good gas distribution.

MR. GAVIGAN: The next speaker is Ravnesh Amar from the TMC.

MR. AMAR: The overall objective in the inherent evaluation program is to demonstrate that natural processes substantially limit the radiological consequences.

As Lou indicated before, the significant activities in this program are the study of sparging phenomena, the aerosol behavior studies, plugging of leakage paths, and radiological consequence assessment studies.

In order to determine the radiological consequences, one needs to evaluate what the source term is, and the release of solid particles from boiling or bubbling sodium pools can be as significant as, say, the gaseous species. Very little work has been done to date on the release of solid particles.

And so, the objective is to provide

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the data base for characterizing the transport of release of solid particles from either the boiling pools or the bubbling pool which simulates, which can simulate the sodium-concrete reactions.

It is intended to achieve this, and existing sodium loop at General Electric, and what we intend to do is inject Argon gas at the bottom of a sodium pool, and you could vary the size of these holes to simulate either the boiling phenomena or the boiling of gas.

The test parameters that have been identified as being potentially important for this phenomena, gas flow rate, that is as it can affect the number of bubbles reaching the sodium pool surface; the gas bubble size; the pool height. Pool height can affect the length of time the bubbles are in contact with the contaminated sodium.

Pool temperature; cover gas pressure; sodium impurity level.

These can affect the dynamics.

Bubble as it breaks away from the surface; particle size and the particle concentration.

Now, the proposed experiment shall be designed to be capable of --

MR. BENDER: Could you clarify the source

109 of the bubbles that arebeing simulated?

MR. AMAR: We will inject Argon gas.

MR. BENDER: That is what you are using to simulate?

MR. AMAR: It could be the gas coming out of the sodium-concrete reactions.

MR. BENDER: So it might be hydrogen. What else?

MR. AMAR: Carbon dioxide.

MR. BENDER: Maybe some steam?

MR. AMAR: The proposed experiment shall be designed to be evaluating all of the parameters shown, unless it can be convincingly demonstrated by analysis or water simulation studies that are in progress that the particular parameter is not important.

There has been only one attempt, one analyzing attempt, to model the sparging phenomena, which is the expression for the decontamination factor, which is the ratio of the concentrations in the pool to that of in the atmosphere above it.

And the proposed experiment shall either validate this model or maybe indicate improvements are required.

The next item is the aerosol behavior studies. The nonvolatiles released in an ACDA soon condense to form aerosols. And associated with these nonvolatiles can be a release of sodium that burns and adds to the aerosols.

Agglomeration and fallout of these aerosols can potentially result in reductions of several hours of magnitude in terms of release.

So this, the objective of these studies then is to demonstrate that natural phenomena such as agglomeration and settling significantly reduce aerosol concentration.

The accomplishments to date, this mitigation potential, led to significant activity in the model development and the code development in the last decade.

But in the last year or two, we have made some significant improvements in the model that basically has been used for FFTF and CRBR which is the HAA code.

And the basic improvement has been that we have incorporated a size dependent -- size dependence of the particle density, as to the various rate processes that have been shown to be important for the aerosol behavior studies.

And these have been incorporated into the HAA code.

In addition to that, the atmosphere has been generalized in the sense that now we can vary the volume and the atmospheric properties, pressure and temperature as a function of time.

Also, advanced computer technology has been utilized to revise the code structure.

Obviously, for these models to be acceptable, you need to validate these experiments. And experiments are in progress at containment systems test facilities at HEDL and at facilities at AI.

MR. CARBON: Excuse me. The aerosols here are plutonium aerosols?

MR. AMAR: Well, we haven't done any experiments with plutonium.

MR. CARBON: What is your objective here?

MR. AMAR: The modeling has been mostly done with sodium aerosols or UO_2 aerosols.

MR. CARBON: But this work is aimed at your source term from fission products and plutonium products.

MR. AMAR: Right.

VOICE FROM THE AUDIENCE: In combination with the sodium oxide as well.

MR. BENDER: Are you covering the whole

spectrum of an accident, starting out with the premise that the initial evolution is a large volume of hydrogen or steam and hydrogen, and that it changes from one as time goes by to some different mixture of gases, in order to see all the ways in which the aerosols are moved out of the system?

MR. AMAR: I would, I am tempted to say yes, we would.

MR. BENDER: Would you postulate?

MR. AMAR: The scope of the modeling and the experimental efforts so far have been narrower than what you are saying.

MR. BENDER: It seems to me that all the conditions ought to be postulated, and in some way the experiment should bracket the conditions.

And presumably you could do them all in this kind of test. But I am not sure that we know what they are yet.

MR. MUHLESTEIN: May I answer a question? Lou Muhlestein from HEDL.

As far as verifying the models, we will in fact look at the full spectrum, including the sodium aerosols.

We look at the situation where you

have lots of oxygen in containment to where you go to a very low oxygen content.

We ran a test a couple weeks ago with one percent oxygen, with sodium vapor as an aerosol.

We try to bracket that in the experiments and see how well this will treat the cases.

MR. AMAR: Now, aerosols produced in an ACDA are typically assumed to leak from the containment building, as would a gas as one-tenth of one percent volume per day.

Experiments on leakage of these aerosols, through capillaries, have indicated that these leak paths tend to plug up. So plugging of flow paths by deposits of aerosols can play a major role in reducing these radiological consequences.

So the purpose of activities in this program is to demonstrate that actual aerosol leakage will be significantly less than currently estimated.

Two efforts in this area in the recent past, a preliminary study was completed to characterize the leakage paths in a typical LMPR containment shell. About one hundred seventy different penetrations have been identified.

They have been categorized into several major categories; the failure modes identified; and design modifications suggested.

The second effort is the modeling effort, and very modest modeling efforts have produced a very encouraging result, that the mass that is transported past a plug can be described by this expression, before the plug, before this growing plug plugs up and prevents any further leakage through the path.

MR. BENDER: You are investigating circular holes?

MR. AMAR: That's the assumption at this time, yes.

We are not only assuming -- we are assuming there is one single plug. That may be a gross assumption, but I think it does reflect that a growing plug has a tendency to intercept more of the aerosol that goes past it.

MR. BENDER: I guess I would have to say that that is unlikely that that is going to be the form of the leak, if you have one.

And I wonder whether that is a valid basis.

MR. AMAR: I said modest efforts have produced a fairly encouraging result.

MR. MUHLESTEIN: May I also make a comment, that a straight through hole is the worst course. Where you have a tortuous path, turbulence is established around the corner, and that tends to plug up first anyway.

MR. BENDER: My only --

MR. MUHLESTEIN: The path is going to be very tortuous, and one can envisualize that is going to plug up quicker than a straight hole because of the turbulence around the corners.

MR. AMAR: That is going to be true in the case of concrete buildings where you have penetrations. Then your path may not be tortuous.

Well, just to indicate a success of this model, I would say, you know, it is encouraging. We have some data, and it correlates pretty well.

MR. MUHLESTEIN: May I add also, we have some six to ten-inch diameter ducts. We don't understand why it follows that same general, even the ten-inch diameter ducts seem to follow that curve as well.

MR. AMAR: True.

So the future plans in both aerosol and leakage studies are to continue updating the HAA code, continue comparison with experiments to verify the new models incorporated into this code, perform high density aerosol experiments, determine need for additional leakage and plugging tests, and lastly, an important effort here called ABCOVE means Aerosol Behavior Code Verification Program, being coordinated at HEDL.

It is an additional effort to investigate several aerosol codes, and specify the range of validity of each code.

And the program would be conducted in two phases. In Phase 1 we will have several bench mark cases, and those would be run by the six different aerosol codes being investigated.

And the purpose is to judge the adequacy of different aerosol codes.

The last activity --

MR. MARK: Could you put a time scale on that?

MR. AMAR: The ABCOVE program?

MR. MARK: Is it ayear or two years?

MR. MUHLESTEIN: It is like a two and a half to three-year effort.

MR. AMAR: Two and a half to three-year effort. And NRC is expected to participate in that effort.

In the consequence assessment area,

a new code called the Cracome code, which stands for Calculation of Reactor Accidents with effects of contamination and evacuation code was completed. What this does is merges the more attractive elements of the NRC Cracome code and Comrade X code with the addition of some new features.

Some of the new features are the Kaselman boil-off. It gives you the flexibility of putting in the release of fission products, which is different from the practice that was in the pool. And most other codes don't do that.

And then it has an improved evacuation model in the sense that it allowed you evacuation in the crosswind direction, which other codes don't do. Plus it allows you to put in a delay time and a warning time for evacuation.

So the overall dose that it calculates is a function of the initial position, the evacuation of the velocity, and the angle of evacuation, which is a new feature.

MR. MARK: Do I understand from that that the new code allows people to move in directions other than --

MR. AMAR: Than the radiant direction.

MR. MARK: Instead of staying right with

the cloud they can get away from it. Cracome is marvelous in that respect.

MR. AMAR: There is no concept, it is just radiant.

MR. MARK: It is in the direction the crowd goes, too.

MR. AMAR: But this gives you an additional flexibility, cross-ventilation.

MR. MARK: Does this code exist?

MR. AMAR: It is operational. It exists.

MR. MARK: Where?

MR. AMAR: HEDL.

MR. MARK: You are keeping it there. You have to go to HEDL to use it?

MR. AMAR: It is available.

MR. GAVIGAN: Is Cracome in the Argonne Center yet, but it may state that it may be sent there.

MR. MUHLESTEIN: I think the answer to that question is yes. I am not totally involved with it, but I remember, I think it is available. People are using it. And I don't know why it couldn't be.

MR. AMAR: That is my understanding. Very interesting.

Going back to some of the other things

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we are looking at in the consequence assessment, a program to perform an in-depth evaluation of all the existing models and data base is underway at Oak Ridge. And one of the objectives of this program is to identify any additional needs relevant to breeder safety.

So far they have identified needs in five different areas. They are rather generic, but the hope is that eventually when the study is completed we will know the different and the consequent assessment area, the differences for calculating consequences with the breeder situation.

Another particle in cell model code is being developed at Oak Ridge. This would take care of the closing distances.

The closing distances also would be useful for the breeder situation in the sense that this code is capable of taking care of the chemical reaction that goes on when you have sodium vapor release and the reaction is there.

Part of the same project is they are trying to quantify the imprecision in the dose estimates for various situations.

I guess that finishes my talk.

MR. GAVIGAN: Don?

MR. FERGUSON: In the interest of moving the meeting along, I am going to use only two or three of the new graph I have here.

The last major part of our program to be discussed here today is the part we label R&D integration, and the focus of that effort is to see that the technology that is developed throughout the four LOA areas, LOA-1 through 4, is properly integrated and presented to plant designers in such a way that they can understand the totality of implications for their safety considerations in the plant design.

We have three second level products, the integrated analyses, project support, and research and development management, the role performed by the technology management center.

In the integrated analyses area, we do work on generally in three areas: methodology and data base development; integrated analysis analysis and risk assessment.

The major tasks that we presently have in place include the maintenance and assistance of users of the large codes that have been developed at Argonne and Hanford and Los Alamos.

We have risk analysis methodology,

risk allocation and risk analysis being performed by General Electric and Atomics International.

We have the physical property data base development work, the actual measurements going on at Argonne and HEDL and other places outside of DOE and Oak Ridge National Laboratory, and actually operating the data base, which is called SACRED.

The reliability data base, called CREDO, that Oak Ridge National Laboratories also maintains, and then sensitivity studies and methodology development going on at Argonne and Hanford.

Mark Temme is going to be talking about the risk analysis methodology and risk allocation methodology development work following my brief introduction.

Project support then focuses on design and licensing support, and dissemination of the safety technology for support of licensing.

Several major program tasks here, actually all of the industrial contractors, are participating in this effort. It is being funded at Westinghouse.

This is the development of low-risk design guidelines in support of the standards under ANS 5411, to say the major industrial contractors are working along with NRC people in the formulation of that standard, and analytical work in support of that has been going on at Westinghouse.

A number of contractors, Argonne, Hanford, General Electric, Westinghouse, Atomics International, Low Alamos, have done analyses in support of the large development plant project.

Clinch River tasks, the entire program is meant to be supportive of Clinch River.

There are particular tasks that are identified by Clinch River as being specifically important at Argonne and Hanford, going on.

And then at HEDL we are fabricating special high enrichment fuel pins for insertion in special vehicles in the fast flux test facility for radiation, and these fuel pins will then be available for testing in TREAT sometime in the post-TREAT upgrade era.

They are special pins in the sense that we have a variety of designs, and they are also high enrichment pins which enable us to get better coupling factors in test reactors, if you will, a larger difference between the fission density and the test fuel and the fission density in the reactor in the test reactor itself.

MR. MARK: Those things we are just referring to would have to wait for the TREAT upgrade?

MR. FERGUSON: They would not necessarily have to wait for TREAT upgrade, but it turns out that they will be available at about the same time the TREAT upgrade becomes operational.

MR. MARK: That was nearly three years from now, two and a half?

MR. FERGUSON: That's right. The first, we hope to have the first of the assemblies of the high enriched pins, there are a total of ninety-one pins in each subassembly, the fabrication completed by the end of fiscal '82.

And they, the first of those would be inserted in FFTF sometime in late '82, for several cycles of radiation.

In order to irradiate these, there is a special double ducted vehicle that was built, and between the two ducts are two rows of boron carbide-filled pins, and that pulls the flux down enough that you can keep the linear heat rating and these high enrichment pins within tolerable levels.

And there is also special facing to control the flow of sodium, so it is an expensive undertaking.

The enrichment level in these pins goes up to essentially about ninety percent fissile material in there, but using enriched uranium in place of the depleted uranium that would normally go along with the plutonium, so they are very high enriched pins.

MR. CARBON: What kind of safety experiments?

MR. FERGUSON: All of the tests that will be carried out in the advanced treatment, the large, for use in TREAT upgrading, will require these enriched pins in order to get adequate coupling factor in order to get a high enough fission in those pins in order to be prototypic of starting in steady state conditions and going from there and doing accident conditions.

With that, I am going to turn it over to Mark Temme, who is going to discuss then the work going on at General Electric in the area of risk allocation and risk assessment methodology development.

MR. MARK: You mentioned that some of the work is directly thought of as supporting licensing activities. Do you have an idea of when it would be possible, as you see it, to get a construction permit for CRBR?

MR. FERGUSON: You have made a rather large jump there. There have been a variety of estimates made by various parties.

I think the Clinch River planning called for the granting of a limited work authorization, from fourteen months after the start of interaction. And that allowed for a start-up time for NCR, et cetera.

That is probably at the extremely optimistic end. NRC estimates twenty-four months from the time they formally start.

MR. MARK: When is this mysterious starting date?

MR. GAVIGAN: It hasn't come. We haven't -- the funding bill for CRBR presently is that the Senate has fully supported two hundred fifty-four million, and the House, I think, supported two hundred twenty-four million, $30 million reduction.

After the conference committee meeting we will have the money for FY '82.

Meanwhile we are doing some preliminary interactions with regular trying to get them to line up the personnel on the regulatory side and trying to help them with R&D.

MR. MARK: If that bill stays somewhere in the rank you have described and doesn't just get cut out altogether, then this start of the interaction would perhaps be --

MR. GAVIGAN: -- next month or so.

MR. MARK: Or the start of fiscal '82?

MR. GAVIGAN: Prior to that, as soon as possible. July, perhaps. We will write a letter that the NRC shall turn on the process.

Meanwhile we are working behind the scenes to line up the people.

MR. MARK: What do you think it is? This is the month of July.

MR. GAVIGAN: You are right. July or August.

MR. MARK: Okay. Thank you.

MR. TEMME: As Don mentioned, I am going to be talking about work both recent and ongoing work at GE in the area of risk analysis methods development and some of the applications thereof, and also risk allocation methods that have been developed both at Atomics International and GE, in somewhat separate programs.

The risk assessment methods program objective, the principal one have been to develop a technical basis for performing credible risk assessments of breeder reactors. That has been our objective since we first started this work in 1974.

Additional objectives have come along. We have not a very large level of effort right now applied to the establishment of line of assurance goals, but we are doing a little bit of work on it.

And we are doing work in support of the LVP project by doing risk assessment of the conceptual design as it evolves. And I am not going to go into that because we covered that in a previous presentation to this committee just a couple of months ago.

Since we are methods development program, one might ask what needs to be developed.

And out of our own experience, the attempts at applications of risk assessment techniques as well as the experience expressed in the Louis Committee Report, there are some needs to improve risk assessments in certain areas.

Scrutability is a popular term since Dr. Louis issued his report. That means principally to us, to define this in clear, straight-forward mathematical terms, how you get from the input to the output.

And it wasn't all that totally clear in some of the earlier applications. That probably is the only one of these technical challenges which has something like a nice, neat closed mathematical solution.

The development of methodology to address these other issues, it appears to me, is primarily a matter of trying things, seeing what seems to work, and getting agreement and consensus among the users that yes, it works, and they want to use it. And ultimately getting the same kind of agreement and consensus between the users and the reviewers.

I would be talking more about what we are doing in this area.

Completeness, of course, is another issue that Dr. Louis and others have mentioned. The statement has been made, we are not so much worried about what you have in the risk assessment as what you have left out of it.

Within our framework of doing things, the completeness issue probably, the initiating event completeness and radiological release category completeness, questions are at least to me the most bothersome.

The way we do our risk calculations implies, at least, that we have collectively exhausted a set of accident initiating events, and collectively exhausted the set of radiological release categories.

I don't know of any formal proof of that. I think the things that we do, somewhat systematically to give ourselves assurance that we are achieving that objective, are things like the use of deductive logic; that is, to lay out what you want to see at the end of the problem and then try to ask yourself how many ways can I get there.

Specifically, this would start by dividing up the range of possible consequences into discreet intervals and work your way back.

That, of course, is a principal of Foultrey logic and it does help you achieve completeness, perhaps, a little better than the inductive logic, which is related to failure modes and effects analysis.

And I am not trying to say that that should not be used, by the way. But, on the other hand, one can get quite involved in looking at all the possible ways a given accident scenario can go without considering how many of those ways contribute significantly to the final result of the risk analysis.

So when we are looking for completeness, it seems to me that principally the use of deductive logic is your best bet for a solution.

At the same time, I want to add here that we don't -- we aren't attempting, at least in our approach to risk analysis, to quantify the things we have never thought of. It just doesn't make sense to us.

So whatever we do in the way of risk assessment, we will always have a constraint on it. It can only include those events and the consequences from those events that we are able to conceive of.

Lack of data is brought up as a problem with risk assessment. To me it is kind of a subordinate issue.

I think the real question is what is the right way to use the data that we have. One of the questions that we always have, if we are able to use the data that we have correctly, is do we need more of it.

But I think in principle, one of the major reasons for doing risk assessment is that that is a way of dealing with the lack of data.

We have uncertainty, and that leads us into doing risk assessment.

We also would like to find ways in which people can estimate probabilities in a consistent fashion. And that certainly is a difficulty when we find people using the same information and producing probability estimates that are orders of magnitudes apart.

Again, I think there is a certain amount of systematic approach that we can take to this. But ultimately I think it is a matter of defining the rules in a way that is acceptable to both the performers of the risk assessment and the reviewers of the risk assessment.

Estimation of uncertainty really goes together with probability estimation in my view, because it has the same problems. And there I mean specifically rather than inputting to a risk model single point values of the risk probability, we should be putting in distributions representing uncertainties on failure rates and so forth. So we approach both of these problems at the same time.

Then, in the risk models themselves, which are at least summarized normally in the form of event trees, we have to deal with uncertainties about accident progression. Do we have enough understanding of basic phenomena to get all the branches in the event tree.

People at least raise questions, particularly regarding the sequence of events following a severe core damage accident or a core melt.

I don't really think the problem is as undual as some people suggest, but at the same time I thnk we need a significantly better structure for defining the possible courses of events and assigning probabilities to them.

And the only way to get it that I can conceive of is to get deeply involved in the problem and see how you can solve it. I don't know of a mathematical solution.

System interactions and common cause failures are somewhat related. The issue there is one of dependencies of failure probabilities. For example, the probability of shut-down system failure for the same system may be quite different depending on whether the initiating event is a small reactive accident or a one g earthquake.

And as you work your way through an event tree logic, all of the subsequent event possibilities really are in principle dependent upon what went before.

Modeling this system interaction, it is not so terribly hard to realize where they are, but it is a practical problem. It seems to me, because it can swamp you in enormous detail to get them all into the risk model.

So the key here I think is to find a way of using practical judgment so that you model the ones that are important and you don't model the ones that aren't important.

And that comes principally with experience.

The quantification of common cause failure probabilities has been a very bothersome issue since Wash 1400, and still is to my knowledge.

In my last discusssion with the NRC, or at least some of the NRC people who work on them things, I ask them what are you doing about it now that the square root law is no longer in favor.

And they said, well, we don't really know what to do about it.

The only thing I think we can do about it is to try to use systematic approaches to identify dependencies, because that is what it is all about. And the assignment of the failure probabilities corresponding to those dependencies may turn out in a lot of cases to be fairly subjective, in which case the best thing to do, I think, is to evaluate the sensitivity of the conclusion to the subjective inputs.

Quantification of the effects of human

factors, I think, if we do this, we will be able to include both positive and negative impacts on risk, due to having operators present.

And some of the more simplistic studies that we have been doing, for example, we make an assumption that if decay heat removal is lost, it never can become restored, which is a very unreasonably conservative assumption.

If we were to try to factor into the picture the likelihood that operators will find some way to restore a heat sink at some time, we could improve the risk.

There are also, of course, negative effects on risk that one ought to take into account. We hope to get a little bit into this during our fiscal year '82 work.

We have done nothing specific about it in the program so far.

Now, we got into the risk allocation applications because we felt that if we are going to be calculating risk, that the calculation should be something more than just after-the-fact windowdressing; that in fact we should be finding some way of using that information to influence what we do in the design.

So, this structure describes the, at least in block diagram form, the logic that GE uses in its risk allocation.

And I will show you a summary of the AI approach, which is a little bit different.

Basically, we treat the risk allocation problem as a constrained optimization problem. We are searching for a design configuration within the choices that we have available, which meets a given risk constraint at minimum cost.

And one can argue at great length about what should be included in cost for this purpose. I will mention what we include, or have included in our application so far.

Cost model has to identify for each of the safety-related design options, an associated cost, and an associated reliability, because these are the reliabilities, particularly are the kind of numbers that go into the event tree model.

We have, in our only applicationof this technique so far, included in cost only two elements. First, a kind of development cost, and plant capital cost.

And the former of those would also include cost of reliability demonstration. The logic

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that we have applied to limiting the cost to those two was simply we were focusing on CPS, which is a one-of-a-kind developmental plant. And it was our assumption that what we would want to minimize was not necessarily the overall cost of electrical generation from that plant, but the cost of getting it built and on-line.

Now, one can put different costs into this process, depending on what seems important at the time. Certainly, if we were trying to design a family of commercially -- plants we intended to make commercial, we would want to be optimizing the total cost of electrical generation.

The risk model that we use is an event tree model, and it is very similar to the one that we use in our risk assessment. We try to keep it a little simple and cut down on the number of events and event tree branches by sticking with those which are dominant.

But we find we are able to use a fairly large model, say, fifteen or so initiating events, without getting into any severe problems with respect to the calculation costs.

The risk criteria, which expresses the safety limit that we have been using for this, for

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these applications, is the Wash 1400 results for a single light water reactor plant, which expressed the risks of acute facilities, latent facilities and property damage.

And the rationale for that is simply that we are trying to achieve with this plant comparable risk to a typical LWR.

It is certainly not a presumption that the risk that is expressed by Wash 1400 has some formal acceptance, or that it should. It is just that we are trying to equal it with our design.

We have a computerized algorithm which takes all of these inputs, and since we are dealing with a discreet number of possibilities, we are able to use, rather than a continuous optimization scheme, a branch and bound technique, which assures us that we have not found a false optimum.

We are in principle, at least, we order from beginning, from highest cost to lowest cost, all of the design options which meet the risk constraint. And we take that with the lowest cost.

In practice, because of the nature of the relationship between cost and risk, we are using -- we can save a lot of computer time, we don't really have to specifically calculate the costs and the risks

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of all the options.

Now, this is one form in which we display the results of our allocation analysis. It is not as complicated as it looks at first glance.

For the problems we have been running, we have with each design option or each category of design option, we have put in only two alternatives.

It might be a high or low reliability, or to include or not include the particular design feature.

So the ones and twos represent low-cost options is number one, and two is high-cost option.

And we found this particular sensitivity analysis to be helpful to us in gaining insights on what is going to reduce risk.

All we have done here is simply reduce the risk limit criteria from its true value, which is the Wash 1400 curves, by different factors. So the particular answer that is the optimum configuration that just meets the Wash 1400 criteria for this case, happened to be one in which we were able to accept the low-cost option for all of these design questions, and the higher cost options for just these two.

Now, if we, due to a risk limit which is ninety percent of the original one, we find that we don't use these two any more, but we introduce this one, this one, the particular choice in this column happened to be the choice between a reinforced concrete containment building and a coolable steel building.

The key there was that the second one was coolable. And therefore, there is a way of preventing containment failure if you have a core melt.

And the picture that this conveys is that over a wide range of variations of the risk limit itself, this comes up as having significant potential to reduce risk.

MR. LIPINSKI: What is the difference between option three and seven? There is no digits changing in that table.

MR. TEMME: The differences between options three through seven, what this says is if you go to a risk limit that is ninety percent of the Wash 1400 curve, in fact, you can go clear down to one that is fifty percent of it, and this design option will suffice in meeting that risk constraint. And it is still the cheapest way to do it.

MR. LIPINSKI: What do I interpret from your table, because each horizontal line is the same for three, four, five, six. And I see a reduction taking place in the right column.

MR. TEMME: The reduction is not of the calculated risk, but of the risk limit. That is the criteria.

MR. GAVIGAN: That is the goal.

MR. TEMME: That is the goal.

MR. LIPINSKI: What does your table tell me? I only see one and two and three. I go to four and I see the same entries horizontally.

MR. TEMME: What that says is that you can reduce the risk limit, the criterion, by a factor of two, and still meet it with this particular design configuration at optimum cost.

So it tells you something about the robustness of the choice.

MR. LIPINSKI: I am with you. It is not until you get to eight that you have to pick another option.

MR. TEMME: Then you have to begin adding more. If you go down far enough you will have all of them in and you still won't be able to meet the risk in.

MR. LIPINSKI: I notice you didn't carry that to where everything was a two.

MR. TEMME: No, we didn't. There was no --

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MR. LIPINSKI: You have only got a factor of ten from top to bottom, from 1 to .1.

MR. GAVIGAN. The next step then is zero.

MR. LIPINSKI: The question is do you pick up another ten?

MR. TEMME: Use the same increment.

There are other ways in which we have tried to display the results of this, because to really get information of this kind of analysis, obviously there is a lot of subjective inputs to this, and we felt that the useful thing is to try to see how sensitive the conclusions you get from it are to the things you are very uncertain to it.

We have done a lot of systemic sensitivity analyses. And there is one of them which seems informative to me.

Now, I am going to go to risk allocation approach that Atomics International has developed and applied, and summarize that.

It is a little bit different from ours in some ways, although in general it is an optimization scheme with a constraint.

Candidate plants are selected via a screening procedure. A candidate plant here is one, one per mutation, or one combination of all of the

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possible combinations of different safety design features that exist.

And I think they have typically had several hundred of these possible combinations, if I remember correctly.

They use some screening procedure, for example, if you have two choices, both of which produce the same risk, but one of them is less costly than the other, then you can throw out one of them and leave it out of the rest of the analysis.

They calculate risk and cost for all candidate plants, and their measure of the cost is energy generation costs rather than just the plant capital cost or development cost.

There is also a transformation of risk into a dollar unit which they are using, so they can actually calculate a cost benefit ratio, whereas we keep risk in units of consequence.

And then as we do, they perform sensitivity analysis to see how the conclusions from it stand up under the uncertainties.

There also is a formal alternative analysis in which we input probability distributions on cost and reliability and see how that affects the decisions.

And then they are choosing roughly the minimum generation cost within the safety objective.

The risk model, I won't try to go through all the details of this, but just to give you a concept of the level of detail and the way the risk model is used, this is essentially the risk model that is used in the AI allocation approach, although there is a lot of analysis behind arriving at these probability numbers.

Severe core damage can occur with a failure to shut down, or failure to remove decay heat. So these are the respective frequencies of these two events.

And then you have an accident that either progresses through generic failure of containment, or through the melt-through kinds of scenarios.

And this is the model that relates risk to a particular design configuration, since each design configuration has specific probabilities for these branches.

Now, the result of the AI risk allocation, this kind of plot where there is energy generation cost and it is actually a relative cost compared to a reference, differential cost from a reference, and this is risk expressed in relative dollar units, actually, percent of reference energy costs.

The result looks like this. The result is actually a whole series of points, each one corresponding to a particular configuration.

The envelope of those, the lower envelope looks like this, and has the characteristics of going back up.

If you go far enough out on the risk scale, because one of the elements of cost is the cost of making repairs after the accident. So you find that if you are allowing enough accidents to happen, your risk is going up and your cost is going up as well.

So you can see,you can get different looking results here, depending on what you choose as an element of cost in the allocation process.

This line represents the strict risk limit. And another criterion used by AI is a cost benefit criterion. That is, hey want the slope of the cost-to-risk curve to be less than -- it is greater than ten. That is the point right here.

Now, these particular goals came from the AI risk allocation, and were input to the CDS conceptual design studies, just to give you a feeling for the kinds of numbers that derive from it.

And since the results of the GE risk allocation come at a slightly different level of detail, we wanted to compare our risk allocation results with AI risk allocation results at one point.

So we tried to summarize ours in the same framework. And this, this actually is the result of the GE risk allocation. They are not vastly different from those coming out of the AI one, with the exception of the probability of melt-through failure containment.

Again, this is not a calculation of the performance of the plant. This is a calculation that gives you an objective for the design of the plant.

If each of these is achieved, you would be able to meet the risk goal, and in principle, at a minimum cost.

I am going to shift gears at this point to talk about some of the risk analysis methods development work that we are involved in at CE, and particularly a thing we have called the single plant risk model.

This relates to the development need under the heading of scrutability that I mentioned earlier, or at least I view it in those terms.

The principal idea here is to try to

write down a mathematical statement which can allow someone to pick up a risk report and understand what you have done with the inputs to get the output.

At the same time, having to do that causes you to -- forces you to think about what assumption you have made in order to get the mathematical statement.

So we have found it somewhat enlightening to try to do this because it makes us realize at least a couple of the important assumptions that have been made in doing risk analyses.

We got started on this principally because we found a need to derive a logical relationship between initiating event frequencies and probabilities, and that is simply because the information that we normally deal with about the occurrence of initiating events is information about their average rate of occurrence over some period of time.

But the thing that we want to calculate in a risk calculation is a probability, so one has to ask himself how do you transform from frequency to probability. And that turned out to be a less simple question than it might appear at first, although there is a variety of ways of making such a transformation. Each of them implies a certain set of assumptions.

Also, we found it convenient, and not altogether unrealistic to make this assumption. This is the equivalent of the assumption that sometimes is made in reliability evaluations of no replacement. This simply says if an event occurs that has off-site consequences, that at least for the purpose of this risk model, operation of that plant is assumed to terminate.

Now, of course, we hope that that is not true in reality. And there are ways of dealing with it in a risk model to represent reality.

For example, one could view the restart of a badly damaged plant as a new decision requiring a new risk analysis. There are other ways to do it, too.

The model also allows you to consistently treat uncertainties in the input and propagate them clearl through the analysis so we can actually consider risk analysis in the risk curve, and in keeping with the fact that this is part of LOA integration, I had to put up something that had integrals in it.

This is what we mean when we refer to the single plant risk model.

Specifically, the calculation that we

are trying to do is the probability that the consequence of operating the plant exceeds some value for a discreet set of values. If we plot all of these we get the complimentary cumulative distribution of consequence.

There is a conditional probability, conditioned on operation of the plant for a given time, and L represents that time.

Normally, the time that we use is one year, but it also could be the plant lifetime. for example, or some other period of time.

You have the formal equation is simply this. This term is the probability of exceeding a given level of consequence for a given period of operation, and for a given vector, which represents the long run relative frequency of consequences in each interval. And that has to be developed further.

I will go into that a little bit. The integration is an integration over all the uncertainties and all the inputs.

Well, what I can do is not go into all of the details of the rest of these equations. My principal reason for showing them in the first place was so that people would have a concept of what we mean when we talk about a single plant model.

I want to point out that numerically it is not all that different from what you have seen before. In fact, this term right here is very close to a mathematical expression of the risk model that was used in Wash-1400.

If you expand this exponential in a tailor series, and the exponent is small, which is turns it is, you get precisely what Wash-1400 did.

So we have a way of relating this nicely expressed mathematical model to the one we used in Wash-1400, and elsewhere.

Of course, we also have expanded it to specifically include the uncertainties.

And one thing I wanted to point out, that in order to get that point, we made an assumption which is implicit in Wash-1400; that is, initiating events for model, improbability of occurrence in initiating events is models by a Poisson probability method. That is what we are doing currently, although we have not investigated the appropriateness of that model, nor have we yet looked into what will ch[illegible] if we change the model.

I will skip this one on uncertainty and go a little bit on results.

This is a typical input into this model. Instead of having a single number called probability of failure to remove decay heat, we have a distribution which represents our uncertainty about that number.

And this is also an initiating event, dependent probability, these two, this one would apply to initiating events which tend to not damage the decay heat removal function.

The other two have some potential for doing that. These are dominated largely by seismic events. In fact, this one by a very large seismic event.

And finally, there is a result. Now, the actual answer that is the result of that first integral equation is this curve labeled mean.

The other curves are percentiles in the uncertainty distribution, or more strictly, they are conditional probability distributions conditioned on the corresponding percentiles and the inputs.

Now, since we are a methods development program, and the kind of methodology that we are developing isn't one that you tie up all nice in a computer code, we always have this nagging question of how do we know when we are through, because the

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major part of the development is done by gaining the experience in trying to find out what works well.

That caused us at one point to focus on writing a procedural manual. We thought that if we are able to do that in a way that we think is intelligent, and people find a way to use it, then we ought to say we are through.

We took a stab at that a few years ago and got some review and comments on it. And we weren't totally satisfied with it for various reasons.

We have not gotten back to reissuing that as yet, or trying to do it again.

Meanwhile, there is a very large national effort to write something similar for application to light water reactors, specifically to the NREP studies. And so it seems logical to us to first interface with that effort in some way as a reviewer of their product, and a provider of suggestions and so forth.

And if at the end of that there still seems to be a need for us to do more in this area to tie up our effort, we will get back to our procedures manual.

Now, I also wanted to mention, this really, the point of these comments probably should

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have been on a different viewgraph, not under the heading of finishing the development, but the main point I want to make here is we came to the conclusion fairly early that it was hard to know when uor methodology was adequate without knowing what it is to be used for.

You can endlessly perfect the way you do a calculation, if you don't have in mind what kind of decision someone is going to make with the numbers.

We also conclude from that, then, that the need, the methods development needs are a little different in each of these major areas of application. Design safety management is essentially addressed by the risk allocation type of analysis.

And those are not results that you have to prove in a licensing environment. They are to guide the designer into a product that he then has to prove in a licensing environment.

He can assume a lot of approximations. R&D management is more or less the same.

So the point is we need to define the ultimate decisions that are to be made with the calculations in order to know when the calculations are good enough.

Now, at the same time, I find myself

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recently telling people who are working on safety goals that you can't define a safety goal in the absence of a definition for how you do the calculations.

So what I am trying to say is the two must go together very closely.

Now, I had two charts here at the end which make reference to the work that we have done on quantification of line of assurance goals. And we are doing, currently, a very small amount of work just to demonstrate how one might go back to that, to see if it is at all credible and worthwhile.

We will be discussing what we find from that with the TMC, and if they feel that some systemic way of quantifying the line of assurance goals is of value to them in making their decisions, we may do more work on it.

That is all I have.

MR. BENDER: One question. Excuse me.

MR. MARK: Go ahead.

MR. BENDER: I don't have any real problem with the approach you are suggesting. As a matter of fact, it looks to me like this kind of risk allocation is about the only way to play the game.

But the information requirements to the

risk assessment still seem to be very vague. There is a lot of subjective judgment in the assignment of risk.

And I don't see very much of that in this discussion.

How do we decide what information is needed to do the risk analysis?

MR. TEMME: Probably one reason you don't see it in there is I don't think I have a real good answer to that.

I have directions, I think, we should explore. I think in general you have to carry the details of your system reliability models and your models for core disruptive accident progression and so forth, to a level which would allow you to input information from SASCOV results, or SIMER, or structural analysis results, and there will be subjective judgments made. I don't think that can be avoided.

The only thing that you can do is try to make them at a level at which it is feasible to get agreement on them.

But really I do think this is a process that will, if it works at all, will require agreement on subjective judgments at some level. I

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don't think that can be avoided.

And if we really can't handle that, we aren't going to use PRLA, I believe. I think, I think we can become accustomed to dealing with that, and derive areas of agreement so that we don't have to argue endlessly about every single probability number and every risk assessment. It may take a while.

MR. BENDER: Let me take a couple areas and then I will stop.

One is the question of how much R&D is necessary in order to establish a risk basis, and where do you put the R&D at?

MR. TEMME: That we probably can treat a little bit more informally. And in fact, that is really essentially what we are trying to do with this whole angle quantification exercise.

MR. BENDER: The other question is who is authorized to make subjective judgments?

MR. TEMME: I think we all are.

MR. MARK: Who is excluded?

MR. BENDER: That is not a minor issue.

MR. TEMME: No, it is not.

MR. BENDER: The difficulty is nobody has been excluded up to now, and somehow or other you have to have a hierarchy of authority in deciding what risks to take.

MR. TEMME: Yes. This is the kind of concern that has led some people to talk about such a thing as a science court or a risk verification panel. And both of those concepts bother me a great deal, but I do agree that it is an unresolved question.

MR. MARK: I am glad you asked your question first. You will be able to answer my two.

On the slide you didn't show, there is a nice acronym, WBS. I am not familiar with it.

MR. TEMME: Work Breakdown Structure, and that refers to level one, level two and level three, which you have seen earlier in a number of viewgraphs.

MR. MARK: And another question, I presume from what you said Bernaro and Rosem at NRC are familiar with the work you have described here.

MR. TEMME: They probably are not as familiar with it today as you are. They know, they have been in this business, and I know Frank has had numerous discussions with them over the years of what we are doing.

But we haven't had any sort of formal presentation like this.

MR. MARK: You know who they are.

MR. TEMME: Indeed. I know them both pretty well. And I talk to them. But we haven't gone through any presentation of this work.

MR. GAVIGAN: They know our work and they get our reports.

MR. TEMME: That is true.

MR. MARK: Maybe even I get the reports, but that doesn't close any loop, necessarily.

MR. BENDER: I have one on my desk, about this thick, from GE.

MR. CARBON: Any other questions of Dr. Temme?

I have a general question, Frank. Do you have anything in your program on the effects of different environments on instrumentation?

MR. GAVIGAN: No. We do have, of course, as I mentioned, a division that does components and instrumentation development for normal operation.

That group does a little bit of work on environment, but not on access environments. There is work now starting, I think the CRBR project office is letting a number of RFP's down throughout the country to develop performance data in environments for a number of their components.

MR. GOLDEN: Frank, tangentially you would get into this NNMI area if you get into censored allegation.

MR. GAVIGAN: Right. But we don't have any work of that type now.

MR. LIPINSKI: That doesn't fall into MMI. It falls in NUREG 197, which says you will have adequate instrumentation.

MR. GOLDEN: All I am saying is censor validationis well recognized prior to the MMIF that you people talk about, in the NMFBR, NMF plant.

MR. LIPINSKI: Not in the same sense, having an accident and then verify is the censor working. You are looking at it more from an operational standpoint, whereas the accident case is special.

MR. CARBON: A second, general question. I think we found in the LWR area earlier we treated things too much independently and didn't pay enough attention to systems integration, systems interaction and so on.

Is there anything in your program that is oriented in that aspect, that concept, that these are systems that function together and have to worry about how one system interacts with another?

MR. GAVIGAN: I think the risk study does that for us. It is a total system study, because it does consider systems' interactions and how one failure affects the other, and for normal operation.

However, we have systems' performance codes, you have heard about those, all in demo code 4 CRBR, it talks about normal performance, and special codes developed for FFTF are YONIS, with some special in-core modifications to help give system performance, but not plant performance.

Overall the plant is considered in the risk analysis.

MR. CARBON: Any other questions anyone would like to raise?

MR. BENDER: Let me comment for a minute on what Frank just said.

In order to use this risk approach that has been described here, you have to have, for systems' interaction purpose, you have to have a lot of knowledge about explicit hardware.

As a matter of fact,you have to have the discreet system defined.

MR. GAVIGAN: Right.

MR. BENDER: So that means you have to start with equipment that exists. Is that being done or has it been done?

MR. GAVIGAN: Demo is a word that was coined for the Clinch River plant for those components, and YONIS, and it is core modification of COBRA is developed specifically for FFTF, its geometries, its pressure drops, its flows, its dimensions in core.

MR. LIPINSKI: Frank, you are not talking about the same thing. Those codes do analyze the systems.

The interactions we are talking about take a balance of plant air system that is connected to the instruments, and somewhere there is a connection with a water system that uses that air in a mixing valve. And somebody shoved the water up the air line and knocks out all the instruments.

Now, that is two systems that are interacting because somebody put a common valve in that that lets the water into the air system.

MR. GAVIGAN: That has been changed. The ACDF study, by having the overall study performed by GE and AI, and Stone and Webster did a balance of risk study on interaction between balance of plant and reactor system itself, as well as effect of fires, flood and other items in the DOP.

And it is a special method F & W used for their light water reactor plants, also. So I can't say specifically that one issue you mentioned is done.

But we have looked at it.

MR. LIPINSKI: It is those type of interactions that are put into these plants, if anything works, everything is fine. But if they go sour and you see an interaction you are not expecting where a water line will knock out your whole instrument air system.

MR. BENDER: I am not so impractical as to assume that they found everything. But just the idea that they have gone through the process I think has some merit.

MR. GAVIGAN: It always has merit. It is interesting, when we first started, FME, with FTTF Clinch River four years ago, sort of forced the contractor into doing it, when they started doing it, it is amazing the things they found in simple analysis weren't quantitative, but were qualitative.

When it was presented to the designer he made a change simply to make material in design. It is difficult to quantify, but we know the designer feels much better about the capability of that system to perform.

There were a lot of changes like that in Clinch River.

MR. CARBON: Well, let us thank you, Frank,

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and your colleagues. You have given us a wealth of very worthwhile material, and I think it is going to be very useful to us.

This will end the formal portion of the meeting. This afternoon the key members and consultants will continue to meet to have discussion with future plans.

This is open. No one needs to stay.

On the other hand, if anyone wishes to, they are certainly invited. We don't need to keep transcripts this afternoon.

Unless there is anything else to bring up, we will adjourn this portion of the meeting.

(WHICH WERE ALL THE PROCEEDINGS HELD THIS DAY.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

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in the matter of: ACRS-SUBCOMMITTEE ON ADVANCED REACTORS

Date of Proceeding: July 15, 1981

Docket Number: ______________________

Place of Proceeding: Chicago, Illinois

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

Leslie Sherman

Official Reporter (Typed)

Leslie Sherman

Official Reporter (Signature)