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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTIVE SAFEGUARDS

SUBCOMMITTEE ON CLASS 9 ACCIDENTS

Sheraton O'Hare Motor Hotel
6810 N. Manheim Road
Chicago, Illinois

Friday, May 9, 1980

The Subcommittee on Class 9 Accidents of the Advisory Committee on Reactive Safeguards was convened, pursuant to notice, at 8:30 a.m.

Present:

MR. WILLIAM KERR, Chairman
MR. GARY QUITTSCHRIEBER, Member
MR. PAUL SHEWMON, Member
MR. I. CATTON, Consultant
MR. R. SEALE, Consultant
MR. J. LEE, Consultant

C O N T E N T S

P R O C E E D I N G S 8:30 a.m.

CHAIRMAN KERR: The meeting will come to order. This is a meeting of the Advisory Committee on Reactive Safeguards, the Subcommittee on Class 9 Accidents. My name is William Kerr. I'm Chairman of the subcommittee. Paul Shewmon, another member of the subcommittee, is to my left, and the designated federal employee, Mr. Gary Quittschrieber, is to my left. Consultants are Mr. Catton, Mr. Seale and Mr. Lee.

This is the first meeting of this specific subcommittee that was appointed by the Committee to study the question of how to deal with the Class 9 accident, and I expect, although I don't think we'll get heavily involved in that today, that one of these responsibilities is that of deciding what a Class 9 accident is.

The meeting is being conducted in accordance with provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act, and all other national and international protocols that apply. Rules for participation in today's meeting have been announced as part of the notice of the meeting published in the Federal Register of April 24, 1980. A transcript of the meeting is being kept and will be available, as stated in the Register notice.

We request that each speaker identify himself, and since there seems to be some shortage of microphones, you may have to shout.

We have under consideration today as a principal item the research program of NRC, which has been designed to deal with part of the Class 9 accident situation. It appears that one might logically devise the consideration into at least three categories. Category 1 might be an immediate problem facing the Commission with respect to the Zion and Indian Point. Category 2 perhaps can be described as near-term construction permit plants or at least plants in early stages of construction. And category 3, future designs.

In each of these cases there are at least two questions to be answered. First, what is to be done? Second, what process is to be used in deciding what is to be done? For example, there is a lot of discussion, both oral and written, about the use of risk assessment in the decision-making process within the Commission. It seems unlikely that quantitative risk assessment is going to be an important feature of the decision-making process. Or at least it seems unlikely to me in these early decisions because I doubt if it will be well enough developed that one can use it as anything other than some minimum amount of guidance.

The two questions of what is to be done in the process to be used in making the decision are, and these of course are not entirely separable. Deciding what is to be done, certainly w. will be influenced to some extent by what is doable, but it seems to me that both of these responsibilities need to be spoken to in a research program, and the Subcommittee and ultimately the ACRS itself, in its reports on the research program, need to examine whether in our view the planned NRC program speaks to both of these questions.

In addition, I think the Committee must seek to determine if the resources available to the Commission are adequate to the task and if appropriate priority is being given to the work. I would hope that subcommittee members and consultants as well as the members of the NRC staff will attempt to deal with these issues during today's meeting and in a subsequent meeting currently scheduled for the 2nd of July in Los Angeles.

We will proceed with the meeting and I call upon Mr. James Meyer of the Nuclear Regulatory Commission staff. Mr. Meyer?

MR. MEYER: Because a considerable portion of my presentation will be directed to the meetings of the last two days in downtown Chicago, I prepared my presentation in my hotel room last night and I apologize for not having copies of my viewgraphs for you. I will have them typed up on Monday and send them down for distribution then.

I'm Jim Meyer of the NRR staff. Also here this morning from NRR is John Olshinski, who is the coordinator for the Zion-Indian Point program. My responsibilities lie in consideration of mitigation features presently under consideration for Zion and Indian Point.

I'd like to start out by very briefly reviewing the Zion and Indian Point program. Several of the first view-graphs you've seen before at the March 7th Full Committee meeting, but I thought it appropriate to very quickly display them to put the Zion-Indian Point progress into perspective.

The first viewgraph I decided to present to indicate the key points as to why the staff is considering Zion and Indian Point as opposed to other nuclear power plants presently in operation. The Indian Point and Zion sites are believed to present a disproportionately high contribution to the total societal risk from reactor accidents. This is based on the fact that for both sites, the cumulative population is considerably above that for average sites. It's considerably above that as presented as a standard or guideline in Red Guide 4.7.

The staff has asked the Indian Point and Zion licensees to determine what additional measures and/or design changes can and should be implemented that will further reduce the probability of a severe reactor accident and will reduce the consequences of such an accident by either reducing the amount of radioactive releases and/or by delaying any radioactive releases which would provide additional time for evacuation near the sites.

MR. SHEWMON: What does cumulative mean? Is that over time, over space, over what?

MR. MEYER: THat's cumulative over space.

MR. SHEWMON: At some instant in time.

MR. MEYER: At some instant in time.

MR. SHEWMON: Okay, I understand that these plants had -- it's not the first time the NRC has realized they're in high population density areas and there were certain features that went into these plants when they were built to cope with that.

Will you get into the basis for this conclusion considering those special features that were put in when they were built later in the presentation, or is this taken as given?

MR. MEYER: All that's reflected here is the question of the population density.

MR. SHEWMON: No, the second one says they do constitute a disproportionate risk, independent of the --

MR. MEYER: The yardstick for the societal risk judgment comes from the fact that they're in high population densities.

MR. SHEWMON: Okay.

MR. MEYER: Your question regarding certain features that were incorporated because they are in high population density areas is being factored in and I or John Olshinski will describe that later.

MR. SHEWMON: Good. Thank you.

MR. MEYER: A key element in the Zion-Indian Point action is the investigation of mitigation features for core degradation and core melt accidents. The purpose of this study is to determine how immediate and practical technical fixes can be implemented in the Zion 1 and 2 and Indian Point 2 and 3 units that will assure a real and significant reduction in societal and individual risk due to severe accidents, including core melt.

The approach is to pursue actively those design features that contribute favorably toward the mitigation of the consequences of a severe accident.

For this particular program, we have split up the goals into two parts. The first goal is rather limited and pragmatic, the one that the past two days of meetings have addressed in part. Its namely to answer the following question: will the proposed features significant mitigate the consequences of severe core melt accidents? This question can be answered with the assumption of core melt and then proceeding with the capability of mitigation features.

For that set of severe accidents that bound the expected dominant s quences for Zion-Indian Point, what do the practical mitigation features look like and what do they achieve in terms of attenuation and delay of release?

The more global goal and ultimate goal is the second one, which involves an assessment of the probability of accident sequences, as well as their consequences -- namely to answer the question, will the installation of any or all mitigation features make Zion or Indian Point significantly safer? A determination of dominant risk contributors, specific to Zion and

Indian Point, and the accomodation of those sequences by mitigation features is key. In order to do this, there is a comprehensive program at NRC which includes the interim reliability evaluation program, IREP, March-Corral analysis that you'll be hearing more about later on today, CRAC analysis, which is the WASH-1400 consequence analysis that determines for example early fatalities and latent cancers for a given accident. These will all be performed for Zion and Indian Point.

In addition, the question of ultimate containment failure will be addressed in order to address this final question.

The severe accident mitigation features that are under consideration are indicated by the first three bullets on this viewgraph. The primary one presently is filtered-vented containment systems. Also under consideration are core retention devices and hydrogen control methods for Zion and Indian Point.

A feature of the study that is something of an aside is the question of steam explosion evaluation. You'll perhaps recall in WASH-1400 that there was a contribution to the total risk for the PWR plant from a steam explosion causing containment failure by the production of a vessel head missile.

The evaluation is -- the program is taking a very close look at this problem, but other than providing for missile shields, there is no mitigation feature program that's directed to the steam explosion problem.

Rounding out the severe accident studies under consideration is the accident risk calculations.

MR. SHEWMON: I guess I'd like to know what you consider core melt when you assume a core melt, and that's not a -- okay. The other is I'm distressed by the fact that I don't see anything on here associated with cooling the core. It's my heartfelt conviction that after we've got the thing dry on the floor, you know to build a catcher down there to fondle this damned thing and let it radiate everything uncooled just doesn't seem like a very right approach to me, and I don't see anything in here about cooling it or guaranteeing sources of water to back up -- you assumed a core melt and I'd like to know what you mean you assume there, and then the other thing is this steam explosion, which to anybody who knows anything about the toughness of hot steel sounded like a desperation bounding approach anyway.

So if you're giving it up, I'm delighted that reason still lives occasionally, because I couldn't see how it could work anyway.

But let's go back to the core catcher -- sorry, to the core melt. Could you tell me what those words mean?

MR. MEYER: I think you asked three questions. What is meant by core melt, the question of water cooling --

MR. SHEWMON: And the steam explosion if you're only talking about will we have completely brittle bolts as hot as it's going to be in a core melt and will they do nothing to keep the head from flying through the containment I think is not a very good question to begin with. So if you're not asking it anymore, I think that's an improvement.

MR. MEYER: No, the question is being asked, and you'll hear much more about t c in the presentations later on today --

MR. SHEWMON: Okay, let's go back to the core melt.

MR. MEYER: The program starts from the assumption of a degraded core, a degraded core that could possibly lead to core melt or that has the possible potential for in-place coolability. But the assumption is that you had a considerably degraded core condition such that you either have a coolable debris bed in the location of the original core, or you don't have a coolable bed and the core starts to melt and then proceeds with the accident sequence.

MR. SHEWMON: What did WASH 1400 mean when they went to a core melt? Can you tell me what the sequence was there, or is that going to be another part of the presentation?

MR. MEYER: I don't think that there was a formal presentation on that subject, but Dr. Ray DiSalvo is here from NRC and he perhaps could say a few words about that.

DR. DI SALVO: Dr. Shewmon, in WASH 1400, if any core temperature was calculated to exceed 2200 F., the core would melt.

MR. SHEWMON: And melt means everything molten through the pressure vessel and down on the pad or what?

DR. DI SALVO: Well, then you calculate the subsequent sequence of events. Eventually that's what happens.

MR. SHEWMON: Okay, so they don't really consider --

DR. DI SALVO: You don't get credit for mitigation once the systems were presumed to have failed badly enough to give you the core melt.

MR. SHEWMON: So once you got the 2200 degree F. cladding, it was all on the floor?

DR. DI SALVO: Eventually.

MR. SHEWMON: Now is that also what you're assuming when you say we assume a core melt will occur in these systems?

MR. MEYER: In terms of the in-vessel assumption, yes, but in two minutes I'll be getting to I think what your concern is, namely supplies of water in the reactor cavity.

MR. SHEWMON: And it may be in the pressure vessel. Okay, in the cavity. Go ahead.

MR. MEYER: Well, there's potential for cooling this degree, both in-vessel and ex-vessel, and both these aspects are being considered in the program.

MR. SHEWMON: Fine, thank you.

MR. MEYER: I'd like to briefly review where we are now in our Zion and Indian Point program. We've issued an action plan, copies of which have been provided to the ACRS. I believe the date was March 17.

You'll recall in the Full Committee presentation on March 7 that there are two other elements to the Zion-Indian Point action in addition to the study of mitigation features. The two other elements are interim operational actions and generic and plant-specific actions. If you are interested in an update status of those particular components of the Zion-INdian Point action plan, John Olshinski will be glad to fill you in on that. If you wish rather to go directly to the mitigation feature study, we can continue with my presentation.

CHAIRMAN KERR: Give us some advice. Have there been significant developments so that we ought to be brought up to date, or is the plan about where it was when the Committee heard about it?

MR. OLSHINSKI: On the orders on the interim actions, as you're aware, they were broken down into immediate, 30 day, 60 day and 90 day, 120 effective items, and basically they proceeded on schedule. Some of the major items of those categories I just mentioned, that's stationing of an additional SRO and control room to the plant, two SRO's in the control room. In-plant walkthroughs and certain emergency procedures and simulator training on those procedures, in addition to the normal training. New containment fleet test requirements prior to starting up in cold shutdown conditions.

CHAIRMAN KERR: John, what do you mean by new?

MR. OLSHINSKI: A different -- what we're doing is now any time they cut down the coal shutdown conditions prior to starting back up, they have to verify containment integrity through a leak test. That was not a requirement before. A requirement that's been put into place on event V-testing, which any time basically they get down into the range of these plants in which the low pressure check valve or the RHR system could come off their seat, they're required to do a gross leak check of the test valve prior to restart. So this is an attack in the event of V failure scenario and WASH-1400.

A new diesel generator testing schedule has been imposed, which is essentially the same schedule which is currently required for new plants, which is a diesel generator testing scheme, which is based ona failure rate, your previous failure rate on your diesel. It's not a set schedule.

MR. SHEWMON: Are those things, the ones where you bring them on and have to go to full power in one minute or something, or these -- whether they'll last a long time?

MR. OLSHINSKI: It's a standard Red Guide requirement on the testing. The only thing that has changed is the --

MR. SHEWMON: That doesn't answer my question because I'm not sure what that means. Now I have heard of tests in which you talk about how long it has to be. I've heard of ones where they wind them up too damned fast for any good machine, and if you're increasing the frequency to that, I suspect -- you just don't know?

MR. OLSHINSKI: No, I don't know.

MR. SHEWMON: Well, I hope you're doing more good than harm I guess is my concern, and I hope you look at that part. Go ahead.

MR. OLSHINSKI: The Red Guide has been in place for diesel generator testing for some time and it was put together by a series of experts. The only thing that has been changed as a result of this is the frequency of testing, and it is a requirement that's been in place for a number of years for new construction plants and it's based on reliability considerations as far as the reliability of the diesel.

Talking to the person that developed that Red Guide, he indicated that as far as your concern is as far as wearout, I have many concerns. I've operated diesels for many years myself. As far as wearout considerations and things like that, in fact the testing would not be farmful.

CHAIRMAN KERR: You'll not let that statement allay your concern, Mr. Shewmon?

MR. OLSHINSKI: But I don't know where it cuts off, whether you have to take it up to load every time or not. I can't answer that.

MR. SHEWMON: It's really how fast it has to come up to load, which is distressing, I think.

CHAIRMAN KERR: John, I got the impression that you said that in addition to the standard testing Red Guide scheme that you were enforcing for these plants a new proposed Red Guide which in effect has a penalty clause so that when you start getting failures you have to test more frequently.

MR. OLSHINSKI: Yes, sir.

CHAIRMAN KERR: And that has not been looked at by anybody in any great detail except the NRC staff. And it has not been demonstrated at all that it will increase reliability. Indeed, I don't think it's good enough to do that.

MR. OLSHINSKI: I didn't mean to apply that. It is based on that scheme. It is based on an optimum test interval --

CHAIRMAN KERR: Based on the assumption that if you make the tests often enough, people will repair things, and that's about the basis ˉr it.

MR. OLSHINSKI: We have a slight disagreement on that.

Additional requirements are the stationing of NSSS vendor representative on-site, at each of the sites. That has been done. The establishment of an on-site safety review group. That's been formed and it's a group that is on-site but reports to off-site management and reviews a number of items in safety areas and they go look on their own at areas of concern, separate from the plant staff. And requirements on notification of NRC at lower emergency action levels than were required before. Those are some of the major items that have taken place as far as 30 and 60 day requirements.

MR. SHEWMON: I don't know where it comes and I can't it out from here, and I don't even know the words to use I guess because maybe it's not operational, but the licensees have put together a package which I assume was gone over in the last couple of days about special features in these plants and what they thought they'd do, and I'd be interested in what you think of that analysis. Are we going to hear anything about that today? Or if I say no, let's go on to research, am I foregoing it all when I say that, or what?

CHAIRMAN KERR: Do you understand the question, Mr. Meyer?

MR. MEYER: I believe I do. We went over a number of those features that you're referring to in the last two days' meetings. It was raised as a concern by the licensees that there has been less than complete communication in this area, and so we are pursuing arranging a meeting in the next month or so to make sure we understand that aspect very clearly.

MR. SHEWMON: And so you aren't prepared to talk about it now but you will be at our next meeting or what?

MR. MEYER: We will be prepared to talk about it at the July 2 meeting, yes.

MR. SHEWMON: Okay, I would like to see it on the agenda, then.

MR. OLSHINSKI: Could I add something, toc? In the accident mitigation features study, we're looking at the dominant sequences and there will be a consideration of how much risk any of these features will buy you. Clearly the plant-specific design consideration for Indian Point and Zion have to be taken into account when you make those determinations, and that's why we're getting together, to make sure we have a plant-specific design when we're looking at the dominant accident sequences or any evaluation of a mitigation features.

MR. SHEWMON: Yes, but you have had in your hands for some time the industry report on what they think resulted from the differences you imposed on them when they built these plants, or they agreed to, and I'd like of like --

MR. OLSHINSKI: Yes, sir, and that requires something like 10 to the minus 12 reliability and --

MR. SHEWMON: Good. What I'm interested in is hearing about it and you're telling me you don't know anything about it yet but you will by July, and I guess I wonder why --

CHAIRMAN KERR: I think what he said was that they would be prepared to talk about it in July.

MR. SHEWMON: Well, he did. He said -- I'm not sure what he said.

CHAIRMAN KERR: I think if that is not part of your scheduled presentation, you probably shouldn't spend too much tim on it but we do want to hear about it certainly.

MR. SHEWMON: I guess so.

MR. MEYER: Well, let me make two very brief points. We have proceeded with our analysis and assessment work on Zion and Indian Point based on plant-specific information that has been provided us by the utilities, so we are working with what we feel is the best information we have available.

MR. SHEWMON: That's comforting. It wasn't clear before.

MR. MEYER: And the data that we haven't addressed yet and we realize is important is the probability of various sequences due to unique features and how that folds into a risk analysis and an understanding of the contributions of these various sequences to the total risk from the plant. That is to be added to the program at a later date.

MR. SHEWMON: Okay, thank you.

MR. MEYER: The status of the third element of the Zion-Indian Point action, namely the mitigation features portion, in a sense is the subject of today's meeting, and you'll be hearing much more about that in terms of the research programs in place to address the various issues. Although the programs look beyond Zion and Indian Point, the first two reactors addressed are Zion and Indian Point units.

There are various NRR activities too that complement the research activities in studying the mitigation features. These were covered in the March 7 Full Committee meeting presentation. Basically they are proceeding on course and I won't go into those areas unless you have any questions regarding the NRR activities and participation.

CHAIRMAN KERR: In the course of your presentation, will we get some idea of the extent to which your program results from NRR requests and needs in connection with the Indian Point and Zion program?

MR. KELBER: Are you addressing that to me?

CHAIRMAN KERR: Well, whoever is appropriate to answer.

MR. MEYER There recently was a letter written from Mr. Ross to Mr. Murley in research that in commenting on the research program spelled out needs that we have and areas where we feel priorities should be placed. And I can go over that letter with you.

CHAIRMAN KERR: I think that's an answer to a question that maybe I should ask, but it wasn't the one I asked. But indeed I was going to try to determine if in describing the research program, which I think you said deals with mitigation problems as they affect Zion and Indian Point, does much of this or does some fraction of it, and you can answer this as you go as far as I'm concerned, result from requests of NRR or is it part of an on-going program which just happens to fortuitously answer questions that NRR will have?

MR. MEYER: I would say to a considerable extent the research program is directed to responding to needs peculiar to the Zion and Indian Point program.

CHAIRMAN KERR: It would just be helpful to me as you go along to give me some guideposts.

MR. MEYER: Sure. I think that if I were to summarize the emphasis that we would like to see coming out of the research program, it would be an emphasis on real systems, on taking on some very real problems that I will be listing in a few minutes.

We need answers to questions in the time frame of six, seven, eight months. And we feel that many of these programs properly directed can result in if not definitive answers, substantial help in making decisions regarding Zion and Indian Point.

In the suggested outline that you gave to us, you had an item called Problems, and I'd like to list a few at this time before I go into what conclusions we came to regarding the meetings over the last two days.

One key aspect to making a determination whether mitigation features contribute to the health and safety of the public by reducing risk is an understanding of the probability of the accident sequences involved. The IREP program that I referred to earlier is considered a key element in the process of that determination.

Presently, as I understand it, the IREP program is looking at Crystal River. We had hoped that they would have been started on the Zion and Indian Point reactors. However, it does not look like they'll be starting for some time, and we see that as a delay problem in terms of making the evaluation of risk reduction from the mitigation features.

CHAIRMAN KERR: Can you be a little more specific about who we and they are?

MR. MEYER: The Interim Reliability Evaluation Program is an NRC-RES probabilistic analysis staff program that is --

CHAIRMAN KERR: So "they" is sort of RES --

MR. MEYER: THey would be RES in this instance, and --

CHAIRMAN KERR: And who is "we"?

MR. MEYER: And we would be NRR and NRC in general in terms of folding in this probablistic analysis into our overall assessment.

CHAIRMAN KERR: Now it occurs to me from what you have said,and I will listen for what you will say further, that indeed quantitative estimate of these accident sequences is fairly essential to the decision-making process.

MR. MEYER: It's an important element.

CHAIRMAN KERR: I think that's what you're telling me, and that you see a delay in getting the information you need if the IREP program is not implemented on Indian POint, for example. Am I understanding your message?

MR. MEYER: It will certainly make the tests more difficult if we do not have the information in the report.

CHAIRMAN KERR: Is make it more difficult a euphemism for impossible? I'm not trying to put words in your mouth. I just want to know how you're going to make the decisions. Are you depending heavily on quantitative risk assessment?

MR. MEYER: WE would depend heavily on it. We may be in a position where decisions will have to be made independent of the results of that particular study. That's why I'm expressing it as a potential problem.

MR. SHEWMON: You're saying you will make the decisions whether you have the best basis for it or not, and the uncertainty of those decisions will be larger if indeed -- the uncertainty in the basis will be larger if you don't have it.

MR. MEYER: The uncertainty in the basis, from a risk perspective, will be greater if we don't have this information. That's correct.

MR. SHEWMON: Sounds like they will make the decisions whether they have a basis for it or not. The schedule is there.

MR. OLSHINSKI: If I may interject something, I think the uncertainty in the decisions has been indicated. It's going to be more without having that evaluation. However, up to now there's been no decision that I know of in the Commission in which a policy has been set forth on the use of probabilistic risk assessment in licensing actions, so it's not clear to me that there's a policy as to whether that -- your total decision will be based on risk evaluation or you'll use it as additional information. Clearly at least we intend to use it as additional information and perhaps make a deterministic decision, but the uncertainty will increase if you don't have that information.

CHAIRMAN KERR: Well, your comment I think is relevant to what I was trying to raise as an issue in my introductory statement. I would certainly expect that the Commission at some point would have to specify on what basis it's making decisions. I would think that it would depend heavily on recommendations from the NRC staff. It is therefore interesting to me to learn what the staff is doing to recommend to the Commission, in terms of those things that are needed to make the decision and what sort of decision-making process, in view of the staff, is an appropriate one. And I would hope that we could have some information on that as the meeting goes on.

MR. MEYER: We certainly view it as a very valuable tool in our better understanding of the potential for reducing risk from these several mitigation features. I don't think there's any question about that.

CHAIRMAN KERR: But at some point, Mr. Meyer, somebody's going to have to make a decision. I think either we use it or we don't. I see all sorts of publications by a whole spectrum of people who pay lip service to the value of quantitative risk assessment. I'm not trying to decide for or against it, but at some point the staff is going to have to recommend to the Commission a way of making this decision, and it's that you perhaps don't -- or are not prepared to make the recommendation today. I would be surprised if you were. But if this is a near-term obligation fairly soon it seems to me the staff is going to have to decide on what it's going to recommend to the Commission.

MR. MEYER: I agree. I had listed here as another problem area the whole role of risk analysis in our assessment. I agree with you that it is important. These decisions should be made by NRC. I agree.

MR. SHEWMON: I would submit that the only reason we're here today or at least the main reason is because of risk assessment and that is that WASH 1400 said that core melt is the largest contributor risk, and I leave out the word quantitative because we can argue, and the Commission hasn't gotten into whether it should be 10 to the minus 6 per year or 10 to the minus something else, but risk assessment is certainly there, or why we're here, I think.

MR. MEYER: I think we're here because of that and we're here also because of what we learned from Three Mile Island.

A perhaps minor problem but one that is worth at least mentioning very briefly is that NRR has had very serious travel restrictions imposed on us and I understand that research has too, and in terms of holding meetings, in terms of communicating to the ACRS as well as to the utilities, this may end up further -- causing further problems in the next six to nine months.

I'd like now to move on to what's been accomplished in the past two days in terms of the first two of the planned five technology meetings. I presented this viewgraph at the March 7 Full Committee meeting, an outline of how we at that time planned to have five technology meetings with the objective to obtain relevant information and expert opinion on a number of technical areas pertaining to designing, selecting and evaluating the effectiveness of severe accident mitigating features. The meetings Wednesday and Thursday basically followed the topics indicated on this viewgraph 1 and 2. The description of these meetings were provided to the ACRS in the form of a letter to Mr. Peoples of Commonwealth Edison dated April 10th.

The idea behind the structure that you see here on the viewgraph is to first have an understanding of what are the -- what we feel are the controlling and dominant accident sequences for Zion and Indian Point, walking through these accident sequences to gain a better understanding of the containment loadings that result as a consequence of these accident sequences.

The meetings were set up to be technology exchange meetings where we would hear the utilities' point of view on these accident sequences and then they can hear our work to date in analyzing these sequences.

The next meeting is planned two weeks from last Tuesday in Bethesda, Maryland. It will take on the question of hydrogen dynamics and hydrogen control measures. One of the results I'll get to in a few minutes of the past two meetings is the mutual feeling that the role of hydrogen will be key in assessing the consequences of these core melt accidents.

The fourth meeting will address the two other mitigation features under consideration, namely the filtered-vented containment and core retention systems. The fifth meeting will be directed to answering the question, how strong in fact are the Zion and Indian Point containments, the structural response to dynamic and static loadings, the modes of failure, and the location of failure.

CHAIRMAN KERR: Mr. Meyer, the parenthetical expression would lead me to believe that this was aimed at Zion-Indian Point. Is that true for example of item 1, which is very specific, or was it somewhat more general?

MR. MEYER: This is very specific to Zion and Indian Point. I think that unless there are further questions on this viewgraph, my next one indicates the scope of the last two days' meetings.

CHAIRMAN KERR: If some of you in the back are having difficulty seeing, there are seats nearer the front and there is no charge for front pews. No extra contribution is required, so feel free to move up.

MR. MEYER: Very briefly, and this is a viewgraph that I presented to open the two-day set of meetings, they were technology exchange meetings. They were not licensing meetings in the sense of determining policy, approach or position.

CHAIRMAN KERR: This reminds me a little bit of the disclaimer I see in a number of vendors' reports which say nothing in this report is to be construed as having any validity.

(Laughter.)

MR. KELBER: No warranty.

CHAIRMAN KERR: So I assume that if one is not using it to determine policy approach or position, it really doesn't have much value.

MR. MEYER: The intent of the meeting was to gain an understanding of the utilities' position regarding the matters at hand and we felt it important to divorce that from pleas for certain licensing approaches from the utilities, or for that matter getting into a lot of the licensing-related problems that can consume an awful lot of time. It's a very handy separation.

The second point is that these meetings, as well as the three meetings coming up, are limited to a consideration of consequences of core melt. They will not consider probabilities of core melt for the reasons I referred to earlier, namely that we would like to have in place the results of the IREP study before we folded that aspect into the overall risk evaluation.

MR. SHEWMON: Now the consequences of cladding, getting to 2200 degrees after the consequences of the melt being gone down, going to the map?

MR. MEYER: The assumption, the starting assumption is that you have the degraded core. That is an assumed.

MR. SHEWMON: Okay, it's in the pressure vessel and it's not cooling adequately and that's where you start your consequences.

MR. MEYER: THat's correct. And again, the remaining three meetings will be limited to the consideration of consequences.

The third point is that the analysis and assessment should emphasize realistic best estimate accident progression paths where we have the luxury of doing so. There are a number of phenomenological areas where there's considerable unknowns and uncertainties, and those must be handled as such. But we emphasized wanting realistic assessments as opposed to bounding or if you will conservative analyses.

The fourth point was that the discussion should be oriented to Zion and Indian Point plants. The meetings are directed specifically to Zion and Indian Point.

And the fifth point, that the discussions will proceed under the assumption that the participants had read the reports provided. The ACRS has been also sent these reports. They consist of a summary report written by RES contractors and two rather large reports. I don't have the new reg number.

MR. SILBERBERG: 1610.

MR. MEYER: They are advanced copies of a new reg report that will be published in a few months, but the ACRS has been furnished with these reports.

CHAIRMAN KERR: I know I received in the mail day before yesterday something about so thick, chapters 1 and 2.

MR. SILBERBERG: That's the right thickness.

MR. SHEWMON: Unfortunately, that's not a unique way to sort through your mail.

MR. LEE: One question. In your opinion, we understand enough about the core melt sequences and consequences that could justify somehow emphasizing the Indian Point and Zion plants, even under the assumption that we have a degraded core to start with?

MR. MEYER: Are you saying do we have enough understanding of the generic phenomenology to proceed with specifics?

MR. LEE: Yes.

MR. MEYER: I think as we proceed through the day, that question will be answered for you. I have one viewgraph that highlights problems that we're not going to solve in six months that we're just going to have to live with for the time being. There are areas, though, that I feel can be addressed and to a certain extent matters resolved.

But rather than answer that question now, I'd like to proceed and I think you'll see that as we go through the day's agenda.

MR. LEE: But I presume your answer is yes at this point?

MR. MEYER: To certain elements, yes, and to certain elements, no.

MR. LEE: Okay.

MR. MEYER: The research contractors did considerable amount of work over a short period of time to put those documents together and I think that their effort is -- their effort is appreciated and I think it should be recognized.

The only utility document that we had to work with was the one that Professor Shewmon referred to earlier, that we received several months ago. It was basically a transcript of their presentation before the staff of February 20, together with viewgraphs. We anticipate in the future that we will have more substantive documentation from which to judge their point of view.

I'd like to move on then to some general highlights, from my point of view anyway, general highlights of the meetings. I apologize for my handwriting.

One normally makes some complimentary remark at the beginning of a summary of any meeting, but I mean the first comment to be more than that. I really feel that the meeting was a good exchange of technical information. That may seem inconsistent with my second comment, but it isn't meant to be.

CHAIRMAN KERR: It sure does seem inconsistent.

MR. MEYER: The information flow basically was in one direction, that is from the NRC to the licensees. The format that developed was for NRC staff and contractors to make presentations based on the two inches of documentation that were prepared, and the licensees were given ample opportunity to direct questions to those particular analyses. It was more difficult to work in the other direction, to have a better understanding of how they viewed some of these sequences and some of the phenomenological problems associated with them.

One area where this was not the case was the area where Dr. Robert Henry of Fauske Associates, I believe is the correct consultant firm name, made presentations on two specific areas -- steam explosions and debris bed coolability from the utility's point of view. And I think there was a very good exchange of information in these two particular areas.

So I do want to emphasize that the net flow was basically from NRC to the licensees and we hope that in future meetings there will be a more balanced set of presentations so that we can leave with a better understanding of the licensees' point of view.

I've taken the liberty of trying to express what I felt was the major licensees' concern, namely that NRC is not considering appropriate sequences, taking credit for the Zion and Indian Point specific mitigation features. This was a problem that we discussed a few minutes ago, and we are planning to address that problem, as I mentioned earlier, by having a somewhat more limited meeting with key utility people so that we can gain a better appreciation of what they feel are in-place features unique to their plants.

CHAIRMAN KERR: It's not clear to me -- well, I could read those, that last point as being two independent points. Do you -- would you explain what they thought the proper sequences could be, or do they only want you to go through sequences -- are you saying they only want to go through sequences where they have mitigation places in place or what?

MR. MEYER: Well, I can give you two examples, one that we did address and one that we didn't. They feel that for Zion and Indian Point, it is virtually impossible to not have copious amounts of water in the reactor cavity when the core melts through the vessel, and that we had not taken proper account of that.

One of the several agreements that came out of the meeting, in my opinion, is that we all understand now that unless there s an intentional design change, there will be substantial amounts of water in the reactor cavity for the sequences that we've considered.

CHAIRMAN KERR: Up to that point somebody had sodium in there, right?

MR. MEYER: Right, exactly. We've always managed to keep sodium out of the reactor cavity also.

Another example is for the accident sequence that has loss of all AC power, including emergency AC power. They felt we did not take proper credit for certain steam-driven mitigation features that would be independent of the loss of power. ANd for example, containment sprays that would under certain circumstances considerably mitigate the accident by reducing the containment pressure.

MR. SEALE: Could I ask a question? Are you saying that there are mitigating features of which you were unaware, or are there mitigating features whose effectiveness you were not prepared to accept, let's say, to the same extent that the utilities would claim?

MR. MEYER: It's more the latter. The consideration of the trangient with loss of all AC power is a trangient to test the capability of the containment without engineer safety features being present. For the WASH 1400 PWR, the loss of AC power meant that you did not have sprays. I believe I'm stating it correctly that they say the loss of AC power does not eliminate that possibility.

DR. DI SALVO: I think that was Zion.

MR. MEYER: WAs it Zion? So we are in the process now of fine-tuning that and making sure that we incorporate those features if we feel they're appropriate, and the codes can do that.

MR. SEALE: I would think that it would be interesting to find out, as you go through your reconciliation of these differences, whether or not there are things in that plant, those two plants, of a mitigating nature that you weren't aware of.

MR. MEYER: We certainly intend to do that.

MR. SEALE: And I think this subcommittee might like to know about that.

MR. OLSHINSKI: If I could say one thing, what we were trying to arrive at here were the dominant accident sequences which are going to be looked at closely through the study and any features that might affect that. A question that had come up, another example for instance that had come up before is how we treat this melt once it goes out and for instance the number of structural plates below the core before it gets to the vessel can impact what your supposed scenario is once you get the melt through, and questions like that, as far as design differences go.

MR. MEYER: It's an important point too that several scenarios were selected in order to challenge the containment to see what the containment failure modes would be, and those scenarios by definition are the most severe to have an appreciation of what one is up against in terms of challenges to the containment.

There were several areas of --

CHAIRMAN KERR: Mr. Meyer, before you change, mitigation, in the case of both you and the licensee, this discussion, refers to mitigation under the assumption that core melt has occurred and one is mitigating the subsequent performance and consequences?

MR. MEYER: That's correct.

These are basically my opinions as of last night, but I think that they're shared by a number of - in fact one of them came from another member of the NRC staff, but they're certainly not ironclad agreement areas.

THe first one I referred to earlier is that a dry cavity is unlikely for the sequences under consideration --

CHAIRMAN KERR: Did somebody think of a way in which one could have a core melt with a dry cavity?

MR. MEYER: The consideration of a dry cavity is an important one to look at to compare the effect of having water in the cavity, so we have something to compare it to. There certainly would be -- there would be ways to keep the reactor cavity dry if it was felt in the best interest of the reactor safety.

MR. SHEWMON: Well, relatively dry. You're going to have to boil off a fair amount of water before you get rid of just what's in the containment, the ECCS and whatever your system was on that.

MR. KELBER: Could I comment on that?

CHAIRMAN KERR: If you're going to explain that later, we'll --

MR. KELBER: No, I'm not going to go into that much detail, but we found early in the investigation that the question regarding the presence of ECCS water and other water in the cavity is really a detailed function of the specific plant geometry.

MR. SHEWMON: No, I meant in the pressure vessel.

MR. KELBER: There are sequences where you can essentially boil off the water in the pressure vessel. Whether you want to consider those or not is another question. But the question of how much of the water that escapes the pressure vessel and gets down into the cavity is a function of the detailed geometry of the plant.

MR. MEYER: THere are ways of introducing water into the vessel directly, and we will be considering those, too.

MR. SHEWMON: It seems to me a basic question which we'll get into in the research is do you advise the utility and design your plant so that they keep adding water to it at all times, under any conditions?

MR. KELBER: Th t's a real qeustion that we have to face.

MR. MEYER: I might add too that it's not necessarily of benefit to have water in the cavity. There is considerable benefit of course in terms of debris bed coolability. I don't think there's too much question about that. But if you have the right amount of water in the reactor cavity, you can add to your containment loading to added pressure.

MR. SHEWMON: Or maybe the wrong amoung.

MR. MEYER: You could have the wrong amount.

Another area of agreement, which was perhaps just a way of expressing conservation of energy, but that prolonged debris bed cooling requires water flow and heat sink for the decay heat levels under conside ation.

Another area where I think it was a refreshing basic agreement was in the debris bed phenomenology. There is still some major questions regarding debris bed cooling, but it's more oriented to the initial conditions, what the initial conditions look like in terms of the molten fuel, molten steel constituents, and other temperature initial conditions that make the whole issue of coolability still an open question.

But the phenomenology of debris bed cooling itself, once you've established a debris bed, I think was felt to be well in hand.

CHAIRMAN KERR: I guess I don't understand that statement. Would you run through it again for me please?

MR. MEYER: If one were to propose a well defined debris bed with specified size of particles, water content, temperatures, decay heat, volumetric heat sources, then as I understand the sense of agreement, there was an understanding of the capability of the debris bed for cooling, dry-out and potential for further meltdown. But we don't know how one gets into the debris bed configuration well enough to say too much more about --

CHAIRMAN KERR: Yes, I thought that's what you said and in a sense it almost seems tantamount to saying if we could define a debris bed that was coolable, we would then know it was coolable, and I couldn't believe you were telling me that, so I was looking for something else. But that's sort of what you were telling me, that if you could put together a nice debris bed that was coolable, then you'd know how it behaved.

MR. OLSHINSKI: Basically, some of the basic questions of whether it's coolable or not are you know, particle sizes and the depth and area of the bed, and I guess what Jim was trying to convey is that providing there's an agreement on the range of particle sizes involved and the general shape or depth of the bed, there's agreement on both sides, yes, particular arrangement is coolable or no, that isn't. But as to the --

CHAIRMAN KERR: That's based on the fact that somebody has a code that purports to calculate the behavior of debris beds?

MR. MEYER: Perhaps Dr. Coates could amplify that.

MR. COATES: Richard Coates, Sandia Labs. Our tools to assess the coolability of a given configuration are in much better shape than our tools to predict what configuration we're going to have to cool. In other words, the models that we have developed, we're in pretty much agreement with the utilities as to the --

CHAIRMAN KERR: Okay, maybe I should reask my question. My question was, is the assumption that you can understand this based on the fact that somebody has a workable code that he believes?

MR. COATES: Workable models that we feel to be reasonably adequate.

CHAIRMAN KERR: It wasn't meant to be a critical question. I was just trying to understand on what --

MR. COATES: Yes, it's based on models that have been developed on the basis of experimental programs.

MR. SHEWMON: Is the uncertainty in that as things go finer or as you pack things with void fractions smaller than something or other, then they aren't coolable; if they get bigger and you've got larger void fractions then they are coolable? Is the threshold or the boundary of that simple?

MR. COATES: Well, it's not, I don't think you can give a very simplistic answer to the question. I think that the phenomenology associated with evaluating the effect of the voids, the depths of the bed and the particles, is reasonably well in hand and I think that there is agreement between the utilities and the NRC in those tools and the various effects.

In terms of looking at particular geometries, there appears to be a range of uncertainty as to the smallest particle size allowed before you have a coolable bed, but it's a very narrow range.

MR. SHEWMON: Now the reason I bring up my question is that in some of the stuff I was reading last night there were comments about void fractions of 55 percent or 49 percent or something, and that's very nice if you all agreed to -- in fact it's not even nice if you agreed to take one size particle. If you happen to end up with a range of particle sizes, 50 percent really is a pretty unwell packed arrangement. And that was why I asked my question of to what extent do the models or is the limit by how well they pack and by the particle size? We can discuss it again later. My impression is that you could have answered my question yes. Since you chose not to, we can go into detail --

MR. COATES: We will cover those considerations this afternoon.

MR. SHEWMON: Okay. My impression is that in laboratories things tend to be one-sized particles more than they might be in reality.

CHAIRMAN KERR: Mr.Kelber?

MR. KELBER: Just one more comment. In the light water reactor system that we're considering, there are a number of meltable species, and that does complicate the question of what is the composition of the debris bed, beyond the type of thing that you --

MR. SHEWMON: Independent of geometry?

MR. KELBER: Independent of geometry. And that does complicate the problem beyond what you normally consider in the laboratory tests.

MR. MEYER: I have two -- actually one more viewgraph that highlights important nearterm work, and that will complete my presentation.

The first item under important nearterm work, and by nearterm I mean work where NRR would like to see some definitive answers over the period of four to six months, one is a better understanding of the pressure spike. I could go into what is meant by the pressure spike. Will that be covered at all in the presentations this afternoon?

MR. KELBER: We hadn't planned on it.

MR. MEYER: Just very briefly, I'll give you an indication of what that pressure spike is. This is a March code analysis of the containment pressure history for the TMLB', which is the trangient with loss of all AC power. The spike that I'm referring to is this rather abrupt pressure rise that poses problems both in terms of continuing failure and also in terms of the design of a feature to mitigate that particular spike.

Much of the meeting over the past two days addressed how fast that pressure surge actually rises, and we feel that this is an important nearterm problem that needs resolution.

MR. SHEWMON: Ivan, is this the code where when the core melts it all melts together and goes down in these small particles you were speaking about?

MR. CATTON: Once the vessel is penetrated, all that is melted is placed in the water, is small particles with a heat transfer coefficient uniformly mixed, and that gives a very rapid generation of steam and results in that pressure spike, which I think is unrealistic.

MR. SHEWMON: Is there any possibility in the near short time schedule of getting at the -- you know, whether we're fighting a code or whether we're fighting the real problem?

MR. MEYER: I think there'so agreement that over a six-month period several things can be done to answer the question of whether this is a real spike or not. Right now it's a March predicted pressure spike based on some of the assumptions that I've referred to.

CHAIRMAN KERR: In your research, has your request -- understand what you want, a better understanding of pressure spike?

MR. MEYER: I believe they have a very clear understanding of the problems and the urgency that an answer is needed.

CHAIRMAN KERR: And this is likely to be an analytical program, I presume, since you can't do much about a new experiment in six months, or can you? Is it decided your approach -- I don't need to know what the approach is, but --

MR. MEYER: It will be analysis followed up -- we're studying whether it can be followed up with experimentation right now.

MR. KELBER: Let me comment a little bit. Getting just a little bit ahead of this story, the response we made to the Zion-Indian Point problem was an ad hoc response. We marshalled resources, redirected efforts in a short-term study. It's clear to us that there are some open questions that still have to be resolved.

I take it as my job to point these out to my management and they have a very difficult job of apportioning resources but I believe they are resolved to try to do their best to answer some of these questions. This particular one of fighting the code, I do not believe will be a big job.

The other point I would make is that we are not pretending, in this effort, which is a very high priority effort on formal transmission of needs, we're working very closely with NRR. We have excellent communication. The Commission has taken, as you may know, a very constructive view of the user need situation, and we're proceeding to work with them as best we can to get these issues resolved in a timely basis.

But we may have a resource problem. That's one of the problems that's affecting IREP. Either people resources or money resources.

MR. SHEWMON: This is only a steam spike we're talking about here, nothing to do with hydrogen burn?

MR. KELBER: There are some spikes with hydrogen.

MR. MEYER: There are hydrogen spikes, but this is addressed to how March models the steam pressure surge.

MR. SHEWMON: Your first item is only the March steam pressure surge?

MR. MEYER: That's correct.

CHAIRMAN KERR: Okay, I interpret the combination of responses that I have gotten to say that NRR knows what is wanted; RES understands what is wanted. There is some question about the resources being available to get the results in time.

MR. MEYER: Another nearterm effort that we feel would be important would be a better understanding of the sensitivity of certain assumptions in the March code 2, the final results -- namely, the pressure and temperature loading of the containment, and we intend to proceed with this activity.

CHAIRMAN KERR: What does that have to do with?

MR. MEYER: The March code calculates pressure histories like I just showed you --

CHAIRMAN KERR: And you want to find its sensitivity to certain kinds of assumptions?

MR. MEYER: Certain kinds of assumptions. For example, there are three different kinds of models, two different kinds of user input options for describing the initial core melt into the lower dome of the vessel. We feel that it is important to understand the sensitivity of the final output to these various user input options, and there are a number of other ones that we'll be --

CHAIRMAN KERR: Okay, how do you purport to get that understanding? Just by making a lot of computer runs?

MR. MEYER: There's a formal activity under way, as I understand, at Battelle Columbus, where the code was written. We have the capability at Brookhaven National Lab to run March, and they will be pursuing this, and we also can run March in-house within NRR.

CHAIRMAN KERR: Who is telling Brookhaven or whoever is going to run the sensitivity analyses what is needed? You?

MR. MEYER: I almost said advanced reactor branch. The reactor systems branch will be directing that effort at Brookhaven.

CHAIRMAN KERR: Even before the reorganization I didn't know exactly where all the branches fit.

(Laughter.)

CHAIRMAN KERR: This is the reactor systems branch of what?

MR. MEYER: Now you're going to test my knowledge. The division I believe is called systems integration, under Denwood Ross.

CHAIRMAN.KERR: Okay, it's not research?

MR. MEYER: No, it's the NRR office.

CHAIRMAN KERR: Okay, you're sort of communicating with Ross and he's talking to research and to Brookhaven at the same time?

MR. MEYER: No. The NRR has a technical assistance contract directly with Brookhaven, so we can work directly with Brookhaven, not through RES. We also have the code running in-house and have had --

CHAIRMAN KERR: In effect, are you going to tell Brookhaven, these are the kinds of things we need, or are you going to Brookhaven and say we think we need to understanding more than we understand about the sensitivity of this code, tell us what to do?

MR. MEYER: Two members of the Brookhaven staff were at the meeting and I would envision it as sitting down together and both directing and getting their suggestions as to how to go about it.

CHAIRMAN KERR: Thank you.

MR. MEYER: The third point needs a word of introduction. IF there is a steam explosion in the vessel that does not fill the vessel, one has to determine how that impacts in a positive or negative way the resultant loading of the containment due to slow pressurization. There are possible positive effects through scattering of the debris and maintaining some coolability for some time. There are negative, possible negative effects of high pressures threatening the steam generator tubes. These are aspects that haven't been looked at very carefully and will be folded into the evaluation.

CHAIRMAN KERR: And again we're talking about a six to eight month time schedule?

MR. MEYER: Yes.

CHAIRMAN KERR: And who's going to do this? Or maybe I should ask, do you know who's going to do it or have some idea who you're going to ask to do it?

MR. MEYER: Well, in my closing comments I was going to remark about the role of the utilities and the role of NRC and how research all fits into that. I put these together last night and I really haven't -- I certainly haven't made recommendations to Dr. Kelber or to the utilities regarding how to approach this. I think that it's something that's important and that after taking a look at what our resources are we'll have to assign it accordingly.

CHAIRMAN KERR: What is meant by "what are containment integrity implications"? Does that mean whether it will retain integrity or not or what happens if it does? I'm just not quite sure what you have in mind.

MR. MEYER: Perhaps I should have said what is the containment loading history subsequent to a steam explosion that does not fail the vessel.

MR. LEE: Could we perhaps have another look at your viewgraph showing the -- what's a little bit curious to me is certainly in addition to the pressure spike that you're talking about, we do have a sustained pressure, long-term pressure level there that could indeed result in failure of containment.

MR. MEYER: That's correct.

MR. LEE: It's not just the pressure spike but overall pressure transient that has to be looked at altogether, in my opinion.

MR. MEYER: That's true. It's importent to consider the whole history. However, the spike presents a unique problem because if the March predicted spike is in fact real, then the mitigating feature will have considerably different characteristics than it would with a very low pressure -- with a very slow pressure rise. In particular, for example, a very large penetration of the containment.

MR. LEE: But suppose we somehow rounded off the spike. Would we be home free or would we still have problems? It looks like we still have problems.

MR. MEYER: Yes, there is a problem, again making a determination of when in fact the containment fails is one consideration, which is part of our program, and then making a determination of appropriate design criteria, for example for a filtered vent.

MR. SHEWMON: I think that what you design your filter for in a vented containment, for example, would --

MR. LEE: Would be considerably subject to where the spike occurs?

MR. MEYER: The design bases and the design criteria would be based in part on how this pressure history looks in containment.

MR. CATTON: That particular spike there I think required a vented containment with the capacity of 10 million cubic feet per minute, which is kind of ridiculous.

CHAIRMAN KERR: Now if Mr. CAtton's evaluation of the code performance or modeling is valid, or I should say if I understand it, it seems to me you can almost eliminate apriori the sharpness of that spike. Now what do you do?

MR. MEYER: Well, I don't think -- it's not quite that clear, I think, that it can be eliminated. But if you could --

CHAIRMAN KERR: I say if Mr. Catton's evaluation, which says that things have to break up into little pieces and they all fall in the water at the same time, in order to get that spike.

MR. MEYER: Well, this is just one sequence among many. If it was determined to be the dominant sequence and our best estimate of the --

CHAIRMAN KERR: No, I'm saying that the spike was mentioned predominantly on your slide. I don't know whether it was deliberate or not but that's the way it came out. Now we can do a little hand waving and eliminate the spike, let's say.

MR. MEYER: I don't think it's that straightforward quite yet.

MR. SHEWMON: Are you questioning the powers of the chairman?

(Laughter.)

MR. OLSHINSKI: Can I interject something? The real question is the rate of the pressure rise.

CHAIRMAN KERR: Well, the rate and the height.

MR. OLSHINSKI: And the height but the rate is, you know on this initial impact when you fail the vessel, that rate is likely to be the driving item. I don't know if this is the correct sequence in which that rate is the highest, but it's likely to be the driving item on the design of a filtered vented containment as far as sizing goes. So if we agree that the spike is not that sharp, we're going to have to come to some agreement on what rate we're really talking about, an agreeable rate that --

CHAIRMAN KERR: I'm thinking about a six to eight-month time sequence, and you don't have time for much more than hand waving in six to eight months, and I just wonder if we shouldn't just do the hand waving here and save all that agony.

MR. KELBER: I'd like to comment on this because I've given considerable thought to what we can do technically. There are two parts to the problem, as Ivan Catton has correctly pointed out. One is the heat transfer itself, and the heat transfer coefficient, and I will propose that we undertake in the very short term to try to get some better estimates of what the variability in that might be.

CHAIRMAN KERR: By looking at the literature?

MR. KELBER: By using what we now know, yes.

CHAIRMAN KERR: It's not meant to be a criticism but I don't see that we can do anything else.

MR. KELBER: It would be possible to do something but it wouldn't be sufficient to scope the problem experimentally.

CHAIRMAN KERR: Now this implies that the people who built the code haven't looked at the literature very carefully.

MR. KELBER: Not in this particular area for some while, and the correlation used is a rather standard one, but I would point out that the time predicted for the production of this pressure surge is actually fairly long, ranging certainly from quite a few seconds to quite a few minutes. So it's not clear that considerations of transient heat conduction, for example, are going to dominate. But we will be looking into these questions as best we can.

The next question is much more difficult, and I don't know that there's anything you're going to do in six months, and that is are there inherent mechanisms which say that it takes a long time, several tens of minutes, for this mass to fragment and form heat transfer area? Or, if it does in fact have say some 25,000 gallons of accumulated water dumped on it, it fragments within a few minutes.

I don't see any way out of staying with that assumption for the time being. We would like to do more on this but --

CHAIRMAN KERR: Which assumption?

MR. KELBER: Of the relatively rapid fragmentation, and relatively rapid is not within mill'seconds or even within seconds but within a few minutes.

CHAIRMAN KERR: You would stay with that because you feel it is the more conservative --

MR. KELBER: At the present time I see we have no course other than that.

CHAIRMAN KERR: Okay, and this is the point at which you depart from best estimates and go conservative?

MR. KELBER: Yes, sir. It's the only thing we can do at the present time.

CHAIRMAN KERR: No, it isn't the only thing you can do. If you're trying to make best estimates you make best estimates.

MR. KELBER: It is our best estimate at the present time because I do believe --

CHAIRMAN KERR: You just referred to it as conservative.

MR. KELBER: It's conservative; it's also the best estimate. The two agree at this time. I have no other estimate so it is my best one.

MR. SHEWMON: It's also your worst estimate.

MR. KELBER: That's correct.

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MR. MEYER: Shall I continue?

CHAIRMAN KERR: It seems to me the best estimate normally implies physically plausible.

MR. KELBER: It's physically plausible that --

CHAIRMAN KERR: It does not seem to me that this is physically plausible.

MR. KELBER: I have seen large masses of metal when quenched suddenly break into small fragments.

CHAIRMAN KERR: All the same size?

MR. KELBER: It doesn't have to be all the same size to get an estimate that is accurate for this work.

MR. SHEWMAN: I don't know whether we should try to solve your technical problem. They don't work too well. But on the other hand, it seems to me I would like to at least meditate for a half an hour one morning on what the geometry of that stream is likely to be as it comes out of the reactor. Where is it likely to fail, what is it likely to be inside. Is it going to come out as a dribble, as a jet, or as a big blob and could that value anything.

MR. KELBER: Yeah. We are going to discuss that. That is, I think, a key issue involved and we are going to discuss that this afternoon, I believe.

MR. MEYER: The impact of hydrogen on containment loadings is a very important area and we looked to the upcoming hydrogen meeting to give us a clear understanding of the utilities' position on this important issue as well as our sharing what we feel to be the various important aspects of the hydrogen problem, both in terms of the dynamics of hydrogen production, migration, burning or possible detonation, the loading of the containment and the mitigation features that have been recommended to considerably reduce the hazard associated with the hydrogen problem.

CHAIRMAN KERR: What new information do you expect to be able to obtain in six to eight months on this and who is going to do it for you?

MR. MEYER: I understand that there is considerable information along the lines that we are interested in from technology outside the nuclear industry as well as inside the nuclear industry and various experts are being invited, either representing the utilities or representing NRC to make presentations on what we feel are key subjects in this area.

CHAIRMAN KERR: Do you expect that to take care of your needs, or is that still an open question?

MR. MEYER: It will not answer all the questions, but I would anticipate it to be very beneficial in focusing on problems that hopefully in the near term could be resolved.

CHAIRMAN KERR: By what means? I am trying to find out whether this is something you are going to ask research to do, you are going to do it in-house. You refer to it as part of important near-term work.

Does that mean you need results before you make a recommendation?

MR. MEYER: We need results before we make a recommendation. That's correct.

There is a part of the research program in the hydrogen area that was reported in the Volume One that you received.

CHAIRMAN KERR: Does it bear directly on this?

MR. MEYER: Yes, it does.

CHAIRMAN KERR: And results are likely to be forthcoming in six to eight months from that work?

MR. MEYER: As far as that specific work is concerned, I'd have to --

CHAIRMAN KERR: The answer is you don't know at this point.

MR. MEYER: I don't know at this point. I think that a lot of the meeting will be so that we are asking the right questions, the key questions.

CHAIRMAN KERR: After the meeting you have to decide what to do next, but at this point you aren't sure what to do next, except you're sure you need information.

MR. MEYER: That's correct.

CHAIRMAN KERR: Do you know what information you need, or is it still an open -- you just know hydrogen is a problem and it could explode and this could be bad for a container?

MR. MEYER: There is a large body of data -- we do know an awful lot already about how hydrogen burn fronts, what that means in terms of temporal and spatial loading of containment. There are in place various hydrogen control methods. The technology of hydrogen control is quite well-known. It is a matter of assembling from diverse sources this information so that we can make judgments regarding the state of where we are and what we do next.

I think it's more pulling together the information than it is extremely long-term R&D type problems. I'm certainly hoping that that's the case.

MR. SHEWMAN: It's my understanding -- and this is another thing that came up in the last day or two -- is the fact that if your core sprays are on, the drop whips themselves have a salubrious -- well, beneficial effect. Tend to quench out the flame.

And that, possibly, is another part --

MR. KELBER: They have to be so much finer than the sprays.

CHAIRMAN KERR: I'm not trying to solve the problem here. I'm just asking, is there a plan in place that is likely to produce the information that you think you need.

MR. SHEWMAN: That's even nicer, because those are the ones that are going to stay around longer.

MR. MEYER: There is a place within research, within our technical assistance at NRR and the utilities have a program in place also.

CHAIRMAN KERR: Thank you.

MR. MEYER: The second problem is probably not resolvable, namely a bette understanding of core melt progression in-vessel, a problem that has been discussed already this morning.

I think we would have false expectations if we thought within six months we would have a handle on the progression to the extent where we could talk in terms of a dribbling in of molten core to the lower vessel dome.

CHAIRMAN KERR: I would agree. What is the significance of the statement as it appears here, that you do require more information before you make a recommendation, or that you would like to have more but you don't think you'll have it?

MR. MEYER: It's the latter, and I put it here to highlight that we may have to consider it as a variable in the sense that we would consider the containment loading based on a variety of proposed melt progression sequences, and then to, for example, a March analysis, see what the continuous impact is on the --

CHAIRMAN KERR: If you had a variety of core melt sequences, none of which you understand, I am not sure that you are better off than having one that you don't understand. The implication of several, in some cases, is that you can sort of decide on one that's better or two or three that are likely to cover all cases.

MR. MEYER: It would be directed, again, to asking the right questions. We, right now, don't know how sensitive for example it is to assume one plausible sequence over another within the vessel proper itself. It impacts significantly on my third important near-term area, of a better understanding of the vessel failure modes. In fact, several people have told me that you give me an answer to the first question and it is a straightforward matter to ascertain the vessel failure modes.

CHAIRMAN KERR: Both of these are perhaps in the same category, which is the category in which you are likely not to have much better understanding six or eight months from now?

I'm not trying to put words in your mouth. I'm trying to understand.

MR. MEYER: I'm pessimistic that we'll have a much better understanding in this area.

CHAIRMAN KERR: Do you have a course of action in mind assuming that your prediction is valid? Are you prepared to make recommendations under the circumstances that you have very little more information other than what you now have?

MR. MEYER: It's an uncomfortable position to be in, but -- it depends on what you're looking for. If you're going to be installing a filter-vented containment system, then you want to understand the maximum loading --

CHAIRMAN KERR: I'm looking at two possibilities. One is that you have to make a recommendation in eight months. That's possibility A. The second is that you will decide in eight months, we know so little that we shouldn't make a recommendation at this point. We need more information.

There are a lot of other possibilities; those are two.

Now, is possibility one the one you feel most likely to be faced with, that in eight months, or X months, or whatever, you will have to make a recommendation based on whatever information you have?

MR. OLSHINSKI: I think I'd like to address that.

I think one of the inputs as to whether we have to make that decision or not is we are going to have to look at the sensitivity of the sequence as far as mitigation features are concerned, and once we look at them, maybe we could answer that question.

CHAIRMAN KERR: But at this point, you don't have a specific deadline?

MR. OLSHINSKI: We have one we're going to try to shoot for, but if we find that our answer to our mitigation features is extremely sensitive on what we assume that core melt progression is, then we may --

CHAIRMAN KERR: How can you find that out in six or eight months?

MR. OLSHINSKI: We can assume different progression sequences and see where that leads to as far as containment loading.

Whether or not they happen to be right, we could start out with the total drop that we talked about before, immediate drop with total mixing. That gives you an almost --

CHAIRMAN KERR: Well isn't it almost certain that you can pick up some sequence to which that is going to be very sensitive, so you almost know the answer to that question now.

There certainly have to be sequences to which it is sensitive and others to which it isn't.

MR. OLSHINSKI: What we looked at at that slide was a TMLB' sequence. It doesn't have hydrogen burn into it. There's other sequences that may turn out to be dominant.

CHAIRMAN KERR: I'm suggesting, though, that right now you could probably sit down, after having looked at 1400 other things and probably pick out sequences to which the vessel behavior and containment behavior would be very sensitive.

MR. OLSHINSKI: I'm not talking about accident sequences. I'm talking about the core melt progression sequence. That is the sensitivity we're going to be looking at.

CHAIRMAN KERR: I hadn't considered that that was part of an accident, and it was in that sense that I --

MR. OLSHINSKI: Well, it is, but what I'm saying is that a particular accident sequence may become limiting because of a hydrogen burn, for instance, and reduce any sensitivity we might have when we look at this.

That's all I'm saying; we have to look at that.

CHAIRMAN KERR: Thank you.

MR. SHEWMAN: Would you tell me two core melt progressions? It seems to me that one could prove pretty conclusively that it wasn't going to all melt at once, given the power distribution which we know exists in that core.

Is that part of a progression, or what do the words mean?

MR. MEYER: Well, I could comment in the context on what is assumed in the March code where you are correct, it does not all melt at once, but the question becomes one of how cooler portions phased to plug up the core debris, what weakens first -- assume that we do have essentially a plugged up bottom portion.

MR. KELBER: Excuse me, but Dick Coates will be covering some of this in some detail.

MR. MEYER: Perhaps it would be better to have that presented by Dr. Coates.

CHAIRMAN KERR: Okay. Fine.

MR. MEYER: I have perhaps one final, but very important, comment, and that is to communicate to the subcommittee the urgency that the NRR considers in this whole Zion and Indian Point action.

We look to the research program in particular to, through proper priority rating of their programs, to come up with short-term answers to the best of their ability and to take on real systems type questions and design considerations.

We also look to the utilities to aggressively, and in an imaginative fashion, pursue these activities.

We think that the next three meetings will be good indications of whether, in fact, the utilities are taking this matter seriously and doing the amount of work that we feel is appropriate for them to do.

That ends my presentation.

CHAIRMAN KERR: Mr. Meyer, perhaps it is inherent in what you've been presenting, but help me a bit.

I have not heard anything about the sort of information you think, or the approach that you are going to recommend to the Commission, in arriving at criteria for making a decision.

For example, has the decision to install a core-containing device already been reached?

I'm not asking you to answer that question. Perhaps it has, perhaps it hasn't. If it hasn't, what criterion are you going to recommend, or what set of criteria are you going to recommend to the Commission to make the decision and, more important to this meeting, is the information that you need to make that recommendation now available or is research, or somebody, likely to need to collect information that you are going to need to make that recommendation to the Commission?

What I have heard so far, I think, is the technical information that you will need to have an understanding of the progression of accident sequences. I've heard less about the risk assessment, a little bit, and I assume this perhaps means maybe unconsciously that you're not going to be able to use risk assessment very importantly in the decision-making process.

Do you understand what I'm driving at? The decision-making process itself, ultimately the Commission has to make a decision, based certainly on recommendation of NRC staff but supported by, I assume, some recommendations among which the Commission can choose, or at least enough information so that the Commission will understand on what basis they're finally making a decision.

I haven't seen what you need of anything in order to make recommendations to the Commission on the decision-making process.

MR. MEDIEROS: Jim, I think the answer to Dr. Kerr's question --

CHAIRMAN KERR: Just a minute. We need a microphone. We know these words are important.

MR. MEDIEROS: My name is Manny Medieros from the Office of Standards and Development. I'm the next up after Jim and I think the answers to all your questions, Dr. Kerr, will come clearer after my presentation because that is where we hope to elicit the kind of information where we can then make recommendations to the Commission and answer the types of questions you are raising.

CHAIRMAN KERR: I guess I have to wait until after your presentation.

MR. MEYER: In the context of Zion and Indian Point, the rulemaking, unfortunately, will do little to guide us since it is scheduled to follow the Zion and Indian Point action. But in terms of the general questions of mitigation features --

CHAIRMAN KERR: I'm talking now about Zion and Indian Point, which strikes me as very important. I mean, somebody has got to make the decision at some point, and I assume it will be the Commission, and they will make it based upon your recommendations.

MR. MEYER: I could sketch out the --

CHAIRMAN KERR: I'm not asking for the details of the process. What I'm trying to ask is, do you have in hand the information that you're going to need to tell the Commission how to reach a decision? If you don't, do you have a program which will permit you to give the information?

MR. MEYER: The Zion and Indian Point plan is directed to obtaining what we feel is the information needed to make the recommendation to the Commission.

John, do you wish to comment further?

MR. OSHINSKI: Let me answer that question.

The IREP is one item of that Zion-Indian Point action plan which we felt we would like to have that information prior to making a recommendation. Now, whether that will become --

CHAIRMAN KERR: But you've already told me that it is very unlikely that you will have that, I think -- did I misunderstand?

MR. OSHINSKI: Well, we know that slip. Now, originally that was scheduled to be completed in August. We know it's at least two months later than that and maybe six months later than that. We don't have any better handle on it than that.

CHAIRMAN KERR: Well, it strikes me that if you really need that information in order to make your recommendation to the Commission, that is a fairly high-priority program.

MR. OSHINSKI: It's very high priority. We're not sure that that is going to be critical to us at this point in time. I definitely feel that it is very high priority that we have it, if we can have it.

Whether we can make the decision without it or not --

CHAIRMAN KERR: It's a high priority program that's not very critical. Is that it?

MR. OSHINSKY: There are high priority -- I'm not going to give you an answer on whether we are going to make it on the risk assessment with or without --

CHAIRMAN KERR: Unfortunately for maybe the both of us, we have some responsibility to try to tell the Commission, and the Congress, if we think the research program is appropriate.

Now, it strikes me in this case that the research program and your other assets ought to be geared to trying to provide information that you will need to make a recommendation to the Commission.

All I am trying to find out is whether you think that's the case.

MR. MEYER: Perhaps I could take a try at it, using your example of a core retention device.

You can answer certain questions regarding a core retention device or you can get some answers to questions for the other mitigating features without resorting to the IREP study.

For example, we intend in reasonably short-term to have an understanding of whether a core retention device would buy you anything in regard to penetration of the basemat. We also are trying to ascertain how much benefit that core retention device would have in terms of considerably reducing the containment loading because you wouldn't have the generation of gases from concrete core interaction.

If you can answer the question, if you install it, are you more or less guaranteed for bounding accidents that it will help you, then you have come a long way in a decision-making process concerning it.

There is the other very important aspect, is it practically to backfit it into the Zion and Indian Point plant, and that's being addressed, too.

MR. OLSHINSKI: Let me get back again to your more general question.

CHAIRMAN KERR: I promise I won't interrupt.

MR. OLSHINSKY: At the beginning of the process, we felt that the IREP program would be a very important element in helping us evaluate the decision. We still feel that way.

I'm not trying to be evasive. What I can't tell you is, if we don't have the IREP, when we get more information, I don't know whether we will feel confident enough to make a decision with that information without IREP or not. I can't tell you that.

CHAIRMAN KERR: I find, for example, that we're being told that risk at these two sites, at this point, is deemed to be overly high, and that therefore it needs to be reduced.

Now, I haven't heard anybody comment on how far it needs to be reduced or how one will judge that an appropriate risk reduction has been achieved. I don't know, for example, whether an appropriate risk reduction is a reduction that would make it equal to the average risk at sites, whatever that means -- I'm not sure that I know at all what that means -- or whether it should be maybe one-tenth of the risk of the average site.

It seems to me that these kind of things have to be considered by you when you finally make your recommendation to the Commission. We have concluded that the risk is too high -- I assume you have -- we think it ought to be reduced so much, or we just want to reduce it as low as is reasonably achievable.

But there has to be some measure, some criterion, some set of guideposts. Otherwise, neither you nor the licensee will know where you are going, I think.

MR. OLSHINSKI: I don't disagree with those statements. That is a true statement. We have to have some measures.

The only question in my mind, we will have some reliability analyses, and that will be from both us, the staff, and from the licensee. The question in my mind is to whether it is going to become necessary to have a full-blown IREP program completed before we make our decisions. I can't answer that at this point.

CHAIRMAN KERR: If you're going to make a decision by X months from now, where X maybe has some flexibility, it seems to me you have to have a fallback position. You can say, I need IREP, but if I don't have it, I have to make a decision anyway, and here is the basis on which I'm going to make it. I either have the information I need, or I had better start getting it -- because you don't have much time, it seems to me.

It's that that I'm looking for.

MR. OLSHINSKI: I think we're in the process of taking care of the fallback already in that what we're doing, as Jim said, one of the items that we've identified is to try to get more plant-specific details in the dominant accident sequences and we're going along those routes irrespective of IREP.

That's part of IREP program to do that, and we're going that on our own now as a fallback.

Whether we'll feel we'll need the full IREP program or not --

CHAIRMAN KERR: So you feel there is in place a fallback program which could be used if IREP does not produce results that you'd like to have.

MR. OLSHINSKI: Yes, sir. We're proceeding along those lines.

CHAIRMAN KERR: And that fallback program is pretty much within your division, or are these things that you're asking RAS or somebody else to do?

MR. OLSHINSKI: Yes, sir. We've got Dr. Falsalbo of PAS who's helping out on this program. We are using his input and we are using PAS as much as we can until they can get the IREP going, to get that fallback position.

But it's not, by any means, the full IREP study that we're doing and at this point we can't make the decision as to whether we'll need that full study or not. We're not close enough yet to answer that.

CHAIRMAN KERR: Mr. Kelber?

MR. KELBER: I'd like to add one important qualification to a statement that was made earlier, and that is the judgment that these plants represent a major portion of the societal risk from the existing plants is qualified by the assumption that the risk from these plants is similar from the risk of the model plant in WASH-1400, namely the Surry reactor.

It may be greater, or it may be less.

CHAIRMAN KERR: Mr. Kelber, you can make that statement, but other representatives of the NRC have made that statement without qualification.

MR. KELBER: No, I do not believe so.

CHAIRMAN KERR: I have heard it made without qualification.

MR. KELBER: Then it should have been so qualified.

CHAIRMAN KERR: I can't argue that point with you. I am simply telling you what I've heard and what is --

MR. KELBER: That's why IREP is so important, Bill.

CHAIRMAN KERR: I agree that it's important and it therefore concerns me to hear that it may not be available when the decision has to be made. It seems to me that one would want it available.

MR. OLSHINSKI: I would like to second Dr. Kelber's assessment that the statement on societal risks that was prepared and given to the Commissioners by NRR was basically based on a transposition of the WASH-1400 referended plant design.

CHAIRMAN KERR: I understand how the results were achieved, but they have not always been qualified when they were made in public.

MR. SHEWMAN: Like on the slide given up here, where population is more people equals more risk. Period.

MR. MEYER: Well, I tried to qualify that that that -- when you have twice as many people in a given area, you have twice the societal risk, everything else being held constant.

CHAIRMAN KERR: Does that complete your presentation, Mr. Meyers?

MR. MEYERS: That completes my presentation.

CHAIRMAN KERR: Thank you.

Any questions?

Our schedule calls for a coffee break after two more presentations but it also calls for a coffee break at 10:40. I'm going to rule that, since it is very close to 10:40, we will take a ten-minute recess.

(A brief recess was taken.)

CHAIRMAN KERR: Mr. Olshinski wanted to make a brief comment before we come to Mr. Medieros.

MR. OLSHINSKI: I just wanted to make an additional comment. There were some questions during the break about my statement of our back-up to IREP that I was asked by Dr. Kerr in case IREP didn't come through, and I just wanted to make it clear it's not our intent, and perhaps I said it wrong and didn't say it quite properly -- it's not our intent that we're going to do, that is, NRR, is going to do a risk evaluation as a back-up in case IREP doesn't come along.

What we intend to do, however, as part of that back-up is the licensees have done on many risk assessments at this point and are in the process of doing a more detailed evaluation, IREP, so to speak, and we will be examining theirs and modifying our sequences as necessary when we come to agreement as to whether their particular evaluation is proper or not.

So that will be our back-up program.

CHAIRMAN KERR: That seems perfectly clear to me.

Thank you.

Mr. Medieros?

MR. MEDIEROS: Good morning. My name is Manny Medieros from the Office of Standards to speak on the degraded cooling of advanced notice of rulemaking and I will hand out the copies of distribution of the viewgraphs that I will be using here today.

I've been allotted ten minutes and I've got ten slides and so I'll try to budget the time accordingly here.

(The materials discussed below follow:)

DEGRADED COOLING -- ADVANCE NOTICE OF RULEMAKING

TASK ORIGIN

- MAY 1979: OFFICE OF STANDARDS DEVELOPMENT (SD) GENERAL COMMITMENT TO THE COMMISSION TO UPGRADE AND IMPROVE THE REGULATIONS

- AUGUST 1979: SD DETAILED PLAN TO THE EXECUTIVE DIRECTOR FOR OPERATIONS ADDRESSING THE TASK

- MARCH 1980: THREE MILE ISLAND ACCIDENT ACTION PLAN ITEM II.B., "CONSIDERATION OF DEGRADED OR MELTED CORES IN SAFETY REVIEW"

- FUTURE DATE: INTEGRATE TASK INTO THE AGENCY OPERATING PLAN

THE PROBLEM

- DEGRADED COOLING AND RESULTANT CORE DAMAGE IS TREATED UNEVENLY IN THE REGULATIONS

- SAFETY ANALYSIS STOPS SHORT OF CLASS 9 ACCIDENTS

- THE CURRENTLY ANALYZED DESIGN BASIS ACCIDENTS DO NOT "ENVELOPE" PLAUSIBLE EVENTS SUFFICIENTLY

EXAMPLE OF RELATED PROBLEMS

- "SAFETY GRADE" IS BASED PRIMARILY ON WHETHER OR NOT THE FUNCTION IS CREDITED IN THE ANALYSIS OF A DESIGN BASIS EVENT (THUS A PRIMITIVE VIEW OF IMPORTANCE TO SAFETY)

ADVANCE NOTICE OF RULEMAKING (ANR)

- DEFINE AREA OF CONCERN

- EXPLAIN PROBLEM TO THE PUBLIC

- PROVIDE THE PUBLIC AN OPPORTUNITY TO ADVISE

- ELICIT ADVICE BY ASKING QUESTIONS

- USE PUBLIC RECOMMENDATIONS TO SHAPE A PROPOSED RULE

SOME EXISTING ADVICE

- NRC TMI-2 LESSONS LEARNED TASK FORCE FINAL REPORT
  - PAGES A-14 AND A-15
- KEMENY REPORT: THE ACCIDENT AT THREE MILE ISLAND
  - PAGES 72 AND 73
- ROGOVIN REPORT: THREE MILE ISLAND
  - "....WE HAVE COME FAR BEYOND THE POINT AT WHICH THE EXISTING, STYLIZED DESIGN BASIS ACCIDENT REVIEW APPROACH IS SUFFICIENT. THE PROCESS IS NOT GOOD ENOUGH TO PINPOINT MANY IMPORTANT DESIGN WEAKNESSES OR TO ADDRESS ALL THE RELEVANT DESIGN ISSUES. SOME IMPORTANT ACCIDENTS ARE OUTSIDE OR ARE NOT ADEQUATELY ASSESSED WITHIN THE 'DESIGN ENVELOPE'; KEY SYSTEMS ARE NOT 'SAFETY RELATED'; AND INTEGRATION OF HUMAN FACTORS INTO THE DESIGN REVIEW IS GROSSLY INADEQUATE."

ANR FEATURES

- o REQUIRE COHERENT CONSIDERATION OF CORE DAMAGE IN DESIGN AND REVIEW

- o ANALYZE A BROAD RANGE OF ACCIDENTS WITHIN AND OUTSIDE TRADITIONAL DESIGN BASIS ENVELOPES

- o RANGE FROM CLAD PERFORATION THROUGH CORE MELT

- o CONSIDER MULTIPLE FAILURES AND OPERATOR ERRORS

TYPICAL QUESTIONS

o SAFETY ANALYSIS

THE THREE MILE ISLAND ACCIDENT WAS TERMINATED AFTER THE CORE WAS DAMAGED SEVERELY BUT BEFORE SUBSTANTIAL MELTING OCCURRED, A CONDITION WELL BEYOND THE CURRENT DESIGN-BASIS-ACCIDENT EVENTS CONSIDERED IN THE SAFETY ANALYSIS. SHOULD THE NRC REQUIRE THAT EVENTS OF THIS TYPE BE CONSIDERED IN FUTURE SAFETY ANALYSES? IF NOT, WHY NOT, OR, IF SO, WHAT CRITERIA WOULD YOU IMPOSE TO JUDGE DESIGN ACCEPTABILITY?

AND

ALTHOUGH THE CONSEQUENCES OF CORE-MELT ACCIDENTS HAVE BEEN CONSIDERED TO SOME EXTENT IN ASSESSING NUCLEAR POWER PLANT SAFETY, SUCH AS IN REQUIREMENTS FOR SITING, EMERGENCY RESPONSE PLANS, AND CERTAIN ENGINEERED SAFETY FEATURES, EXPLICIT CONSIDERATION OF THE CAPABILITY OF CURRENT DESIGNS AND CASUALTY PROCEDURES TO COPE WITH CORE-MELT ACCIDENTS HAS NOT BEEN A PART OF SAFETY ANALYSIS SCRUTINY BY THE NRC. SHOULD CORE-MELT ACCIDENTS BE SPECIFICALLY EVALUATED IN SAFETY ANALYSIS REVIEWS, AND, IF SO, TO WHAT EXTENT, OR, IF NOT, WHY NOT?

TYPICAL QUESTIONS

- DESIGN IMPROVEMENTS

IF LOSS OF CORE COOLING AND RESULTANT CORE DAMAGE OCCUR IN A NUCLEAR POWER PLANT, THERE ARE CERTAIN PREDICTABLE CONSEQUENCES. CAN THESE CONSEQUENCES BE MITIGATED SUBSTANTIALLY, AND THE RISK OF SEVERE PUBLIC HEALTH DANGER THEREBY REDUCED SUBSTANTIALLY BY PRACTICAL DESIGN IMPROVEMENTS? IF NOT, WHY NOT, OR, IF SO, WHAT DESIGN IMPROVEMENTS CAN BE MADE AND AT WHAT ESTIMATED COST? HOW DO YOUR RECOMMENDATIONS IMPACT ON OTHER SAFETY CONSIDERATIONS?

- HYDROGEN CONTROL

HOW WOULD YOU VALUE REQUIREMENTS TO INCORPORATE SYSTEMS FOR CONTROLLING HYDROGEN COMBUSTION INTO CONTAINMENT DESIGN? DO YOU FAVOR METHODS OF CONTROL THAT SUPPRESS COMBUSTION OR DO YOU FAVOR CONTROLLED BURNING? IF YOU FAVOR SUPPRESSION OF COMBUSTION, WHAT TECHNIQUES WOULD YOU RECOMMEND AND SHOULD THEY VARY AS A FUNCTION OF THE DESIGN CAPABILITY OF CURRENT CONTAINMENTS? IF YOU FAVOR CONTROLLED BURNING, DO YOU RECOMMEND OPEN FLAMES, SPARK PLUGS, CATALYTIC COMBUSTORS, OR SOME OTHER MEANS? WHAT PERCENT OF A CORE'S ZIRCONIUM BEING OXIDIZED WOULD YOU DESIGN FOR? WOULD YOU RESPOND DIFFERENTLY FOR DIFFERENT REACTOR OR CONTAINMENT TYPES? IF SO, WHAT DIFFERENCES WOULD YOU RECOMMEND?

TYPICAL QUESTIONS

- MITIGATING FEATURES

RECOGNIZING THAT THERE CAN NEVER BE COMPLETE ASSURANCE THAT ONLY EVENTS ANALYZED AS DELINEATED IN A SAFETY ANALYSIS REPORT WILL OCCUR, WHAT ADDITIONAL ANALYSIS, PROCEDURES, OR DESIGN FEATURES WOULD YOU PROPOSE TO MITIGATE FUEL DAMAGE ACCIDENTS IN THE RANGE FROM EXTENSIVE CLAD PERFORATION WITHOUT OXIDATION, THROUGH A FEW PERCENT CLAD OXIDATION, THROUGH EXTENSIVE OXIDATION TO FULL CORE MELTDOWN? WOULD YOU RECOMMEND DIFFERENT AND PERHAPS OVERLAPPING DESIGN FEATURES DEPENDING ON THE SEVERITY OF CORE DAMAGE TO BE COPED WITH.

- CONTROLLED FILTERED VENTING

HOW WOULD YOU VALUE A NEW REQUIREMENT TO CONSTRUCT, AT EACH NUCLEAR REACTOR PLANT SITE, A NEW STRUCTURE FOR CONTROLLED FILTERED VENTING OF THE REACTOR CONTAINMENT STRUCTURE? WOULD YOU LIMIT THE FUNCTION OF SUCH A NEW STRUCTURE TO FILTERING PARTICULATES, ELEMENTAL IODINE, AND INORGANIC IODINE OR WOULD YOU EXTEND SUCH AN APPENDAGE TO INCLUDE ADSORPTION BED SYSTEMS USING CHARCOAL OR OTHER PROCESSES SO THAT ORGANIC IODINE AND NOBLE GASES COULD BE TRAPPED? WHAT QUANTITIES AND RELEASE RATES OF GASES AND PARTICULATES WOULD YOU DESIGN SUCH A STRUCTURE TO HANDLE AND AT WHAT REMOVAL EFFICIENCY AND COST? DO THE POTENTIAL REDUCTIONS IN RISK EXPECTED FROM SUCH AN APPENDAGE OFFSET POTENTIAL INCREASES IN RISK THAT MAY MATERIALIZE FROM INCIDENTS SUCH AS INADVERTENT OPERATION OR THE CONCENTRATION OF HYDROGEN IN THE FILTERING APPARATUS?

TYPICAL QUESTIONS

o CORE CATCHERS

HOW WOULD YOU VALUE A NEW REQUIREMENT TO INCORPORATE INTO CONTAINMENT DESIGN, A CORE RETENTION SYSTEM TO MITIGATE THE CONSEQUENCES OF CORE MELTDOWN BY, FOR EXAMPLE, INCREASING RESISTANCE TO MOLTEN CORE DEBRIS PENETRATION AND THEREBY SUBSTANTIALLY REDUCING GAS, VAPOR, AND AEROSOL GENERATION TO LESS THAN THAT WHICH OCCURS WHEN CORE DEBRIS IS ALLOWED TO INTERACT WITH CONCRETE? ASSUMING A CORE RETENTION SYSTEM IS REQUIRED, DO YOU FAVOR A DEVICE THAT DELAYS MELT-THROUGH OF THE CONTAINMENT BASEMAT, OR DO YOU FAVOR A DEVICE THAT PERMANENTLY RETAINS CORE DEBRIS WITHIN THE CONTAINMENT BUILDING? IF YOU FAVOR DELAY OF CORE MELT-THROUGH, DO YOU RECOMMEND REFRACTORY MATERIALS (SUCH AS MgO, ZrO_2) TO PROTECT THE CONTAINMENT CONCRETE BASEMAT, OR DO YOU RECOMMEND SOME OTHER MEANS? IF YOU FAVOR PERMANENT RETENTION OF CORE DEBRIS, DO YOU RECOMMEND USING REFRACTORY MATERIALS IN COMBINATION WITH COOLING SYSTEMS THAT RELY EITHER ON NATURAL CONVECTIVE COOLING OR FORCED PUMPING OF COOLANT AROUND THE EXTREMITIES OF THE REFRACTORY MATERIAL, OR DO YOU RECOMMEND SOME OTHER CONCEPT? WOULD YOU RESPOND DIFFERENTLY FOR DIFFERENT CONTAINMENT TYPES? IF SO, WHAT DIFFERENCES WOULD YOU RECOMMEND? HOW DO YOUR RECOMMENDATIONS IMPACT ON OTHER SAFETY CONSIDERATIONS?

TYPICAL QUESTIONS

o TRAINING AND PROCEDURES

HOW DO YOU VALUE ACTIONS SUCH AS REQUIRING MORE EXTENSIVE OPERATOR TRAINING, REQUIRING STRICT LITERAL COMPLIANCE WITH NEW AND IMPROVED DETAILED OPERATING PROCEDURES, AND EXPANDING CONTROL ROOM MINIMUM MANNING AS ALTERNATIVES OR SUPPLEMENTS TO DESIGN IMPROVEMENTS?

o DESIGN CRITERIA

WHAT DESIGN, QUALITY AND SEISMIC CRITERIA WOULD YOU RECOMMEND FOR ANY ADDITIONAL SYSTEMS TO PREVENT THE POTENTIAL BREECHING OF CONTAINMENT SUCH AS SYSTEMS FOR CONTROLLED FILTERED VENTING, HYDROGEN COMBUSTION CONTROL, AND CORE RETENTION MENTIONED IN PREVIOUS QUESTIONS? DO YOU FAVOR EVALUATING DESIGNS OF SUCH SYSTEMS ON A REALISTIC BASIS, AS OPPOSED TO THE CONSERVATIVE METHOD USED TO EVALUATE ENGINEERED SAFETY FEATURES? DO YOU FAVOR ESTABLISHING DESIGN CRITERIA FOR SUCH SYSTEMS THAT ARE EQUALLY STRINGENT, LESS STRINGENT, OR MORE STRINGENT THAN THOSE APPLIED TO ENGINEERED SAFETY FEATURES? PLEASE EXPLAIN YOUR RESPONSE IN TERMS OF CRITERIA YOU WOULD RECOMMEND, INCLUDING CONSIDERATION OF REDUNDANCY, DIVERSITY, TESTABILITY, INSPECTABILITY, AND STRUCTURAL DESIGN LIMITS (INCLUDING SEISMIC REQUIREMENTS).

MR. MEDIEROS: The first slide talks about the task origin. In May of 1979, Mr. Minot from the Office of Standards and Development made a general commitment to the Commission that we should upgrade our regulations and improve them to consider degraded cooling. And, later in August, he provided a detailed, 20-page plan from the Executive Director of Operations that would address this task.

And then, later on, as many of you know, the task action plan as a result of the Three Mile Island accident was developed about December and in March, in item II.B. -- in case you want to reference it to that task action plan -- in Item II.B., "Consideration of Degraded or Melted Coolers and Safety Review," was written into the plan as a high-priority item and today we are going to talk about Item II.B.8., which is the degraded cooling advance notice of rulemaking.

It's not proposed rulemaking; it's not effective rulemaking. It's an advance notice of rulemaking, and I'll explain what that means in a minute.

And, of course, in the future I expect that this will be integrated into the agency's plant.

Briefly stated, the problem is one that degraded cooling and resultant core damage is treated unevenly in the regulations. For example, if you go to Part 100 you will find that it assumes substantial melting, it assumes 100 percent release of the noble gases, 50 percent release of the halogents, 100 percent release of the particulates.

If you go to a different part of the regulations, part 5044, you'll see where we talk about 5 percent hydrogen -- this is the hydrogen control portions of the regulations. If you go to the ECCS portion of the regulations, which is 5046, you'll see something like 1 percent hydrogen and then some other criteria, like 2200 degrees Farenheit and 17 percent clad oxidation, and so forth.

But you can go through various other sections of the regulations and see an uneven treatment in the regulations of degraded cooling which, of course, results in degraded cores.

The second way, I think, to express the problem is that the safety analysis stopped short of Class 9 accidents, and that is well-documented and we feel that the safety analysis report, therefore, is inadequate, if it stops short of discussing Class 9 accidents.

The third way to categorize the problem would be that the currently analyzed design basis accidents do not envelope plausible events sufficiently. In particular, I am thinking in terms of multiple failures and operator errors and so forth.

And then I put at the bottom here an example of related problems because this spawns a whole series of other problems. For example, the safety grade idea is based on whether or not a function is credited analysis of a design basis event.

The advanced notice of rulemaking has five features.

First of all, we try to define to the public the area of concern and then we try to explain the problem to the public as best we can.

Then we try to provide the public an opportunity to advise the Commission and usually give them 45 or 50 days. We try to elicit this advice by asking questions and half of my slides here today will be typical questions that I will suggest that we ask.

That's the bulk of my presentation.

And then, of course, we use these public recommendations to shape a proposed rule, and, Dr. Kerr, this is what I had in mind earlier when I popped up inappropriately when you were interested particularly in Zion and Indian Point and I thought you were talking more generally and suggested that this was one of the means that we would try to answer some of the concerns that you had.

CHAIRMAN KERR: Mr. Medieros, in connection with your earlier statement, in your view does a Class 8 accident include core degradation?

MR. MEDIEROS: Core degradation? Onl· to the extent, I guess, that the ECCS criteria considers it and that the --

CHAIRMAN KERR: Well, you just pointed out that Part

100 specifies that the siting analysis considers significant melting of the core, and I believe it does. I would assume that that is associated with core degradation.

MR. MEDIEROS: That's right.

CHAIRMAN KERR: Hence, does Part 100 deal with Class 9 accidents in its present form, in your view?

MR. MEDIEROS: Yes, sir. Certainly.

Part 100, with that criteria, has to assume a Class 9 accident because it says substantially --

CHAIRMAN KERR: So we are already dealing with Class 9 accidents.

MR. MEDIEROS: We're already dealing with Class 9 accidents. My point is that we don't deal specifically with them in the safety analysis.

CHAIRMAN KERR: Okay. Thank you.

MR. MEDIEROS: The next slide is the fact that we already have some existing advice. It's not that we don't already receive much advice. The Commission has.

I show, for example, here that if you pick the NUREG 0585, "Lessons Learned: Task Force Final Report", you'll find on those pages -- and I don't want to take the time to recite that, but there are about ten brief questions that are in the form of advice as to what we should be asking and looking at.

Similarly, the Kemeny report, the accident on Three Mile Island, if you look on those pages, for example, you'll find recommendations in particular with attention to multiple failures and operator areas. And especially since it was short enough to put here, I plucked some appropriate advice from the Rogovin report which is quite pointed and says, "We've come far beyond the point at which existing design basis accident review approach is sufficient. The process is not good enough to pinpoint many important design weaknesses, or to address all relevant design issues.

"Some important accidents are outside, or not adequately addressed, within the design envelope and key systems are not safety related and integration of human factors in the design review is grossly inadequate."

So this sums up some of the thinking that is going into this advanced notice of rulemaking.

CHAIRMAN KERR: If you are expressing, and I assume you are, a Commission position here, it is a big surprising to me that you do not refer to an earlier report which also both explicitly and implicitly suggests that one look at multiple failures and probabilities of accidents that might involve core melt.

Certainly the Kemeny report and the Rogovin report are not the earliest advice.

MR. MEDIEROS: No, sir. I said some. I just picked three that were timely, that would fit on the slide. I suspect that we could put up three or four slides with all of the advice that we've gotten.

It was just an illustration of the fact that we already have some advice and this advice, for example, is some of the thinking that's going into the advance notice of rulemaking.

Now, the features of the advance notice of rulemaking -- and this is the last slide before I get to the typical question -- the features as they are presently envisioned -- and I might say at this point that we are in the process of writing this advance notice of rulemaking. It hasn't been presented to the Commission for review, for approval, for concurrence.

The Office of Standards has a draft paper working with NRR and the other offices to try to get agreement on a Commission position --

CHAIRMAN KERR: Remind me, what is an ANR?

MR. MEDIEROS: Advance Notice of Rulemaking, as differentiated from the next step which would be a proposed rule, which would set forth what exactly we had in mind and get comments on that; and then the effective rule being the third step.

The Advance Notice of Rulemaking, to go back a couple of slides, is more a kind of question thing.

CHAIRMAN KERR: That's enough.

MR. MEDIEROS: Yes, sir.

Okay.

The features that we would invision in this Advance Notice of Rulemaking again, we would require a coherent consideration of core damage and design and review; not to mean that all systems would be designed with the same degree of cooling, degraded cooling and core damage, because that might not make sense. But at least if you went through the systems and the various parts of the regulations, they would be coherent and you could explain why, one, maybe was less stringent than another on a consistent basis.

The second point about the Advance Notice of Rulemaking, the second feature, is that we would suggest analyzing a broad range of accidents within and outside the traditional design basis envelopes. We would try to express the idea, I guess, that the clad is really not a strong barrier, not only when you have degraded cooling and degraded cores does it let the fission products out, but it contributes to your accident by generating an explosive gas.

We've known all these things all along, but I think we would try to highlight this more in asking the questions and this would form an important basis for our rule -- the fact that the clad might not be as important or as strng a barrier as we've thought it to be. It contributes greatly to the accident.

We would try to do this analysis in a range from clad perforation, a very small kind of problem, right through core melting, because the problems differ at each end of the spectrum and in between, the problems you have to handle, and so you might have different design ideas to cope with the problems.

For example, at the perforation end, you might have a little bit of gap activity and a little bit of hydrogen because you will probably heat it up a bit. Then as you got a little bit further along, you'd have lots more hydrogen to worry about and more gap activity.

Then eventually you'd have a lot of hydrogen and then, of course, finally you'd not only have the gap activity but you'd get the radioactivity coming out of the fuel as well. So the problem will get worse as you get towards the core melt in, and different solutions might be indicated at various steps on the way, in terms of mitigation or in terms of prevention.

MR. LEE: Question.

Could you first comment a little bit on your first statement there that you'd like to read a coherent consideration of core damage with regard to the discussion we had earlier in the morning relating to the probablistic risk assessment, and so on?

MR. MEDIEROS: Well, I don't view, or envision, a terrible much use of probabalistic risk assessment in this work, frankly. I just don't feel that the tools are there these days to use it that much, although I recognize it is a very trendy subject and --

CHAIRMAN KERR: You quoted the Kemeny Commission as one of the reasons that you are doing this.

MR. MEDIEROS: Yes, sir.

CHAIRMAN KERR: It certainly recommended it strongly.

MR. MEDIEROS: It certainly did, and so did Rogovin and everybody else. I realize it is very trendy.

CHAIRMAN KERR: How do you decide which of the Kemeny Commission's advice to take and which to ignore?

MR. MEDIEROS: The ones that make more sense than the others I think we would take, and the ones that --

(Laughter)

MR. MEDIEROS: I don't think they all make sense, really.

CHAIRMAN KERR: You use what is called engineering judgment?

MR. MEDIEROS: Yes, sir. That's what I'm saying. Yes, sir.

CHAIRMAN KERR: One of our committee members just pointed out that the difficulty of using engineering judgment is that it requires both engineering and judgment.

(Laughter)

MR. MEDIEROS: Yes, sir, and we will try to apply them in liberal quantities.

Did I answer your question, or did I dodge it?

MR. LEE: Essentially you are suggesting some kind of bounding calculation?

MR. MEDIEROS: Well, perhaps. I think as we come to the questions here in a minute, this is a typical question and very specific. You will see some of the things I'm suggesting.

And then, if I haven't answered your question, please --

CHAIRMAN KERR: When the Commission goes into Advance Notice of Rulemaking, does it have in mind a rule that it is going to propose, or does it sort of go in blind and hope that wisdom will come out of this process?

MR. MEDIEROS: I hate to say it, but closer to the later, really. We try to have something in mind.

CHAIRMAN KERR: I was hoping you would tell me something different.

MR. MEDIEROS: No, sir. That's what we do.

We try to ask the questions as best we can to elicit as much good technical advice as we can so that then we can factor that into a proposed rule and we're not flying sort of by ourselves in the proposed rule. We get the best technical input that we can get.

CHAIRMAN KERR: Well, I don't know what your experience has been, but my experience has been that you get much better advice on something if what you propose is specific. If we just go in and say, hey, tell us what we should be doing, the advice you get is frequently not very meaningful.

MR. MEDIEROS: I think my next questions will not disappoint you in that regard. Can I beg off for just a couple more slides, and I think then make you will see that the kinds of questions that are being asked are specific enough that they might suggest an answer.

CHAIRMAN KERR: It also strikes me, though, that at some point in this process, the Commission ought to propose some answers ---not in final, but its view of the best way to approach this.

Is that part of the process?

MR. MEDIEROS: Yes, sir. It certainly is.

CHAIRMAN KERR: But it doesn't happen yet.

MR. MEDIEROS: You are going to see some of that in the questions already. They presuppose some answers.

CHAIRMAN KERR: Oh. So they aren't really questions?

MR. MEDIEROS: Well, they're a hybrid.

CHAIRMAN KERR: All right.

MR. MEDIEROS: One other thought that I think I should put on the table that is guiding a lot of this work -- it's not one that I subscribe to. I really don't believe it, but a lot of people I respect to and so it's important, and we've thought about it in this work, is that we've reached a point of diminishing returns in the area of accident prevention and that we ought to be concentrating our efforts in the area of mitigation. And that's a very popularly held view, and this paper will try to factor that in as well.

As I say, I don't necessarily agree with that, but that should come through in some of the questions that we ask here.

Okay. Let me get to the next slide, which is the first one with typical questions, and it will get more detailed as I go.

MR. CATTON: Who is the public that you're going to ask these questions of?

MR. MEDIEROS: The public is design agents, the utilities, the common, ordinary citizens -- anybody that cares to respond to the rulemaking and we will publish, in addition to the Federal Register Notice, we will publish this and send it to a long list of people we consider interested persons.

We will try to put this together from various Commission computer lists, and so forth.

The first typical -- I call it a typical question. We don't have an exact set of questions yet because we're still working on this in the Commission, but it would discuss the safety analysis, and I would put down to typical questions with regard to safety analysis.

For the people in the back of the room, real quickly: The Three Mile Island accident was terminated after the core was damaged severely but before substantial melting occurred, a condition well beyond the current design basis accident events considered in the safety analysis.

Should the NRC require that events of this type be considered in future safety analyses? If not, why not or if so, what criteria would you impose to judge design acceptability?

It kind of suggests an answer, I think, and some direction and some guidance, but not quite, not completely.

The second question, along the same lines of analysis: Although the consequences of core melt accident have been considered to some extent in assessing nuclear power plant safety, such as in requirements for siting, emergency response plans and certain engineered safety features, explicit consideration of the capability of current design and casualty procedures to cope with core melt accidents has not been a part of safety analysis scrutiny by the NRC.

And the question: Should core melt accidents be specifically evaluated in safety analysis reviews and if so, to what extent and if not, why not?

So that's a couple of typical questions on analysis.

The next typical questions would be on design improvements.

Here we say that if we have a loss of core cooling and resultant core damage in a nuclear power plant, there are certain predictable consequences. By that, I mean from the perforation that I showed you right on through the stages to core melt.

Can these consequences be mitigated substantially and the risk of their public health danger thereby reduced substantially by practical design improvements?

If not, why not; or if so, what design improvements can be made and at what estimated cost?

And here I have a question that I consider is particularly important. How do your recommendations impact on other safety considerations?

For example, somebody were to say in response to this that we should inert all PWR plants, my gosh, what would happen to maintainability, inspection. Or if you say well, we are going to have a new policy. We'll put some people in there with masks, then you create another hazard.

So there is all kinds of things that I think we must balance in this kind of work in terms of impact on present safety considerations. I think that's extremely important.

Then I think a typical question to the public would concern hydrogen and how I've stated it here: How would you value requirements to incorporate systems for controlling hydrogen combustion and containment designs? Do you favor methods that suppress combustion, or do you favor controlled burning?

If you favor suppression of combustion, what techniques would you recommend and should they vary as a function of the design capability of current containments.

If you favor control burning, do you recommend open flames, spark plugs, catalytic combustors or some other means?

And then, most important, what percent of the core's zirconium being oxidized would you design for? The 44 to 60 percent we saw in Three Mile Island or 30 percent, or 25 percent?

And then, would you respond differently for a different reactor or containment types and, if so, what differences would you recommend?

This ties in quite a bit with what Jim Meyers recommended at the earlier ACR's meeting on hydrogen control and a good many of these ideas come from that lecture.

CHAIRMAN KERR: It is also at this point that your earlier position on risk analysis puzzles me very much, because I don't see how one can answer a question like this without answering it in the context of a contributor to total power plant risk.

I mean, you muck around with hydrogen all you want to, but if it doesn't reduce the risk any, so what, and if it reduces the risk significantly, then it may be important.

So I don't see how the Commission and the people who respond to this and other questions have much of a context in which to make a response unless they give fairly careful attention to risk analysis.

MR. MEDIEROS: I think, in a relative sense, if you're looking at relative probabilities and relative changes and that sort of thing, I suspect the contribution to be made. But I really haven't heard of any two experts that could agree on any order of magnitude myself on a lot of these probabilistic risks.

CHAIRMAN KERR: I would say that if they come within an order of magnitude, that's very, very good.

MR. MEDIEROS: Maybe I should say within three orders of magnitude.

CHAIRMAN KERR: There are two people here that want to add to what you've said.

Yes, sir.

MR. MEDIEROS: I can use the help.

MR. MEYER: One of the questions that are included in the draft that I've seen, anyway, that I don't think are included in the sample that Manny has is the question of the whole role of risk analysis in this area to obtain opinions from the public and interested parties, how they view quantitative risk analysis in an absolute sense, a relative sense, so on and so forth.

So this opinion will be solicited from the public.

MR. MEDIEROS: Absolutely. I expect to put a question of that kind in. I don't personally feel that it's as important as one of the others and so I left it out.

CHAIRMAN KERR: That's going to be in the category of the advice you're going to ignore because you don't think it makes much sense.

MR. MEDIEROS: No, I'm not saying that. I'm saying in the short presentation today, I picked more important questions, I think, to put on the slide.

CHAIRMAN KERR: Mr. Kelber.

Will you yield to Mr. Kelber?

MR. MEDIEROS: Absolutely.

MR. KELBER: Regardless of other roles, I believe, Bill, that you pointed very directly to a key role of risk analysis and that is in evaluating the technical benefits and the technical costs -- I hate to use the term negative benefit -- the technical denigration to safety that might be put in by a given fix. And I see no tool available to do this in a quantitative, satisfactory way other than risk analysis.

CHAIRMAN KERR: Thank you, Mr. Kelber.

I hope that was helpful.

MR. MEDIEROS: I certainly will be accepting advice from inside the Commission as well as outside the Commission. His is as important as anybody's.

Okay. Let me go to the next question. The next typical question.

CHAIRMAN KERR: I'm not sure I'd agree with that last statement.

(Laughter)

MR. MEDIEROS: It makes him feel good anyway. He won't heckle me, maybe, if I say that.

Now, getting to the mitigating features, we say that -- and this, again, is just a typical question -- recognizing that there could never be complete assurance that only events analyzed as delineated in a safegy analysis report will occur, what additional analyses, procedures or design features would you propose to mitigate fuel damage accidents in the range from extensive clad perforation without oxidation, thus no hydrogen, through a few percent clad perforation, through extensive oxidation to full core meltdown?

Would you recommend different, and perhaps overlapping, design features depending on the severity of core damage to be coped with.

What I'm asking is basically, if you're talking about mitigation for just the lower class of accidents, then perhaps inerting would be appropriate. If you're thinking about mitigation for a more severe accident, then maybe some of the halons that Jim Meyers talked about in earlier presentations, you may want to consider those.

CHAIRMAN KERR: Mr. Medieros, this is a lot of fun, but we are far beyond schedule. It's partly my fault, for asking too many questions.

So let me urge that we speed things up if we can.

MR. MEDIEROS: I'll do it. I've got two more slides.

And control filtered -- in fact, if the people can read it in the back of the room and you've had a chance to read it, I won't reread it. They all don't have copies.

CHAIRMAN KERR: We have to assume that the people that sit in the back of the room can't read -- that's the reason they're sitting there --

(Laughter)

CHAIRMAN KERR: And if they want to read, they'll come to the front.

Just make a few comments.

MR. MEDIEROS: All right. On controlled filter venting, we have a question much like the others. I think the public needs to know that what we're talking about with this kind of a scheme, this kind of an idea, is letting the bird in the hand that you have out for presumably getting two in the bush.

Here you've got the radioactivity all captured in containment and you're purposefully going to let it go with this kind of an idea.

MS. SHINEFLUG: Excuse me. Could you please read it, as long as I am making a tape recording, and people not able to be here today would then have the benefit of knowing what the questions are?

MR. MEDIEROS: Dr. Kerr, what are your instructions?

CHAIRMAN KERR: People who are not here today can have a copy.

MR. SHEWMON: We might make the point that there is a transcript available for purchase five days after the meeting.

MS. SHINEFLUG: I cannot afford 8 cents a page.

CHAIRMAN KERR: Is this okay?

MS. SHINEFLUG: I'm not satisfied with it, no. I think that as long as he's read the controlled filtered venting, I'd appreciate it.

CHAIRMAN KERR: You're unwilling to accept the written material?

MS. SHINEFLUG: No, I did not say that. I said that I'd prefer that he'd read the questions into the tape recorder. If you will not rule accordingly, then I will accept this gentleman's copy of the written questions.

MR. MEDIEROS: I will proceed to the next slide, and the next slide is a typical question on core catchers, and I will leave it up long enough so that everyone can read it and it incorporates the kinds of thoughts, ideas, that were presented by Jim Meyer at the last ACRS meeting. It's the kind of knotty thing that we're going to have to grapple with if we're going to make any sense out of whetheror not the core catcher idea is a reasonable idea.

And then lastly I will put up typical questions concerning training and procedures. If you believe that there is still a good deal yet to be done in the area of prevention in addition to mitigation, then this type of question might appeal to you on the value of actions by requiring more training and strict literal compliance with operating procedures, and so forth, which, personally, I believe we've barely scratched the surface. I believe we've got a tremendous amount of work to do in this area and that could have a great effect on increasing the safety of our operations.

And then lastly, the last typical question I've put up concerns the design criteria that you might use for some of these mitigating features whether they be core catchers or control filtered venting or hydrogen control, and whether you use the realistic basis that was up on earlier slides that Jim Meyer presented, or whether you go to a more conservative method, and the reasons therefore.

And with that, I can conclude my presentation and bring you closer to your schedule.

CHAIRMAN KERR: There is one thing I didn't see on there that I would recommend you would include.

MR. MEDIERCS: Okay.

CHAIRMAN KERR: And that is a consideration of the risk of these accidents compared to other risks to which people are exposed. It may well be that most contributors will think that there should be different, but it seems to me that it would be helpful to have that input.

MR. MEDIEROS: I had that and a lot of other questions earlier and I felt that to give the proper response to this we're going to have to narrow and keep the scope very tight, and that was one of the questions I took out.

CHAIRMAN KERR: That's a fairly key question.

MR. MEDIEROS: I understand. There's probably a dozen more like it, but I accept your advice and I'm going to put that down, and maybe we'll reconsider that.

But I am trying to narrow the scope to a manageable one here, and I'm already being told that maybe it's a bit broader than it should be.

But thank you.

CHAIRMAN KERR: Other questions or comments?

Mr. Lee?

MR. LEE: Would you perhaps comment on the procedure involved in the use of the feedback you'd be getting from the public in response to these questions?

MR. MEDIEROS: Yes, sir.

Let's suppose that we could put this out tomorrow, and then let's start from there. Then there would be a 60-day period in which the public would have time to read all of this in the Federal Register or by a separate mailing, or some other way, find out about it and write a response. The responses would start dribbling in during that 60-day period, but more than likely they would all come in on the last day and so there would not be much you could do in that 60-day period.

Then you would start, depending on how man you had and how complicated they were and all that sort of thing, you would start for the next month or two trying to evaluate each technical response and then to form a position within the Office of Standards.

The Office of Standards would do that facet of it.

Then once the Office of Standards felt that it had properly reviewed all the public's response, understood what they had to say and made the proper conclusions, a paper would be prepared and circulated in the key offices in the Commission prior to making a recommendation to the Commission.

And so that's a many month job, as you can see, assuming we could start tomorrow. Now, we're not in a position to start tomorrow, but we're working that way.

MR. LEE: I guess what I'm more interested in was that the legal or procedural or schedular aspects of it, but how to classify the technical responses, and who makes the judgments on which ones they recommend, and things of that nature.

MR. MEDIEROS: Who in the Commission?

MR. LEE: No, who in the Office of Standards.

MR. MEDIEROS: In this particular case, it could be me, or it could be some more knowledgeable person, or several. It could be any number of people who are not identified today. I cannot identify them by name.

But engineers in the Office of Standards will do their work and then in concert with engineers in other key offices, would come to a proposed position on each of these points.

Does that answer your question?

MR. LEE: Yes.

MR. MEDIEROS: That's how we would do it.

CHAIRMAN KERR: Are there any other questions?

Mr. Catton?

MR. CATTON: I notice the agenda shifting over to Dr. Kelber. I'd like to ask Jim Meyer a question before we get off of this area.

This goes back to the --

CHAIRMAN KERR: Let me see if we've completed questioning of Mr. Medieros.

MR. CATTON: All right.

CHAIRMAN KERR: Have we?

Thank you, Mr. Medieros.

MR. MEDIEROS: Thank you.

CHAIRMAN KERR: Mr. Meyer, will you consent to answer questions?

MR. MEYER: Sure.

MR. CATTON: In the mini-WASH-1400 report where the utilities attempted to demonstrate how safe their plants were, I think the numbers that they came up with were that the plant was about 1000 times safer and that overall they were a factor of ten better than WASH 1400.

If that --

CHAIRMAN KERR: The plants to which you refer are --

MR. CATTON: Zion and Indian Point.

If indeed their study is correct and you were agree to it, what more could you require of them when they are already ten times better than Surry?

MR. MEYER: If it is the Commission's determination that in fact they are as safe in that risk perspective as they claim, my personal opinion would be that there -- well, that would certainly be a very important input to considering not doing anything in terms of further mitigation features.

CHAIRMAN KERR: I think that's a statesmanlike response.

MR. MEYER: There's the whole question of population density.

MR. CATTON: Well, they included the population density.

MR. MEYER: I'm aware of that.

MR. CATTON: And still came out with a factor of ten less risk than Surry.

I'm not sure that I believe all of those factors, but it was impressive.

MR. MEYER: Well, speaking for myself, if we felt that that was an appropriate analysis from a risk perspective, there is no reason to go ahead with any mitigation features.

CHAIRMAN KERR: Our next agenda item is Mr. Kelber.

MR. KELBER: I have copies of my testimony. My testimony is in three volumes today and it is all bound together.

I have been asked first to address the status of the Class 9 technology with regard to Zion and Indian Point study.

The basic work of assembling the best current estimate of threats to the containment and assessment of suggested mitigating features is completed with the issuance of reports which are NUREG CR1409 and NUREG CR1410 Volumes I and II. These, I believe, have been sent to you and if you haven't received them, blame it on the U.S. mail.

These are advance copies. The laboratories are printing the reports and they should be more widely available shortly.

The study leaves a number of questions open. I am preparing, together with Walt Murfin of Sandia Laboratories, a list of open topics focusing on the filtered vented containment system proposed specifically for Zion and Indian Point.

When that list of questions is complete, I will send you a copy via Mr. Quittschriber.

The study was organized according to logic imposed by WASH 1400. Therefore, the obvious assumption is that these plants are not fundamentally different from the Surry plant.

There are differences in detail, and these will be taken into account as IREP and related work proceeds, but the basic logic is sound.

My first viewgraph demonstrates the relationship between failure modes and the risks and consequences that was developed in WASH 1400.

Now, the direct bypass LOCA in the event of Type V that actually dominates the risk is not treated in this study. Instead, an early analysis was made of Indian Point and Zion by the probablistic analysis staff in cooperation with NR --

CHAIRMAN KERR: Charlie, maybe it should be obvious to me, but help me a bit. The aim of this study was not to try to determine whether one should require core catchers for containment.

MR. KELBER: No.

CHAIRMAN KERR: But rather to see what the state of the art was if one had to design them. Is that correct?

MR. KELBER: That is correct.

All we seek to do is give NRR the best technical support we can give and the best technical support available right now. I will discuss the future program later.

An earlier analysis was made of the events of Type V, because they were not within the scope of this study, and I believe that appropriate orders were issued and are being complied with to markedly reduce the likelihood of that failure mode.

We also did not review in our study failure isolation which is another mode illustrated on the graph.

The conclusions of this study should be carefully qualified. As the report indicates, we simply did not have time for the type of quality assurance that we normally like to apply and though we did our best we want to emphasize that there has to be more than the usual type of qualification.

I've made, for presentation purposes, a short statement of the conclusions of our study. The pressure spike that has been discussed earlier, or the hydrogen burn, is the most challenging condition from the point of view of designing an effective filtered vent containment system.

The venting strategies that show the promise of handling the widest range of accident conditions also appear to have a high potential for harmful system interaction. Filtered venting does show promise of great reduction in potential consequences -- by great reductions, I mean in excess of an order of magnitude.

Steam explosions are unlikely to threaten containment integrity. Hydrogen from the metal water reaction is unlikely to detonate but burning might threaten the containment integrity.

The penetration of concrete basemats cannot be ruled out, but three to four days is our best estimate for the time for penetration if it should occur, and penetration will be as a solid mass of material.

The coolability of large-sized debris particles at early times appears doubtful, either in- or ex-vessel, but small particle debris bits may be coolable, and at later times, coolability seems to be much more likely.

CHAIRMAN KERR: Now, are these statements in the context of any existing plant?

MR. KELBER: These are directed specifically at the Zion and Indian Point Plants and no other.

CHAIRMAN KERR: And no core catcher, but some consideration of how filtered vented containment might affect the actual sequence?

MR. KELBER: Yes, sir. Specifically for those plants, and no others. It may be applicable to others, but it is not specifically aimed at those.

We met our objectives of this study with somewhat less precision than we had anticipated in some respects, but nevertheless, with some useful guidance. As you will hear later today, the conclusions on steam explosions represent the synthesis of two groups' work. There is not complete agreement on the details, although the conclusion for this application is unaffected thereby.

But we expect that the current program at Sandia will yield conclusive data in the next two years and then this whole issue can be resolved.

The area of hydrogen control is still troublesome. Work there is just beginning and we in the utilities have to put more effort into this problem.

CHAIRMAN KERR: Is the conclusion that detonation is unlikely based on the assumption everyone has completed mixing?

MR. KELBER: Yes, sir.

I am not pleased, as an individual, with the current status of work on this problem. We made surprising progress, by the way, on structural response. Surprising because the problem is technically formidable.

There are two teams, one at Sandia and one at LASL, and they did a magnificent job. Basically, the response to static or quasi-static loads turns out to be determining and failure is predicted at about twice the rating of the containment.

With respect to the major objective of the report, that valuation of filtered vented containment systems as a means of mitigating failure by overpressure, we confirmed some expectations and developed some unexpected insights.

An FVCS -- Filtered Vented Containment System -- is feasible and will be effective in reducing predicted consequences of a Class 9 accident by an order of magnitude or more, and that confirms our expectations based on earlier studies.

On the other hand, an investigation of how the FVCS interacts with the rest of the containment during the accident sequence shows that interaction is complex, may carry with it some chance for added risk, and so an assessment of the net benefit from FBCS is incomplete.

Now, this is particularly important in the context of backfitting an existing plant. If we were to design a completely new plant, we might not have such a problem.

I don't have time to explore this topic in detail with you now -- nor, in fact, the required expertise -- but I suggest you may want to schedule a presentation at a mutually satisfactory time with the experts, particularly Al Benjamin and others at Sandia Laboratory, who have devoted a major effort to this task.

Now, we've learned some lessons from all of this.

MR. CATTON: Excuse me, Charlie.

MR. KELBER: Yes, sir.

MR. CATTON: Number 7, isn't that opposite of what was presented yesterday, coolability of large-sized debris particles at early times appears doubtful? I thought the large particles were more coolable than the small ones.

MR. KELBER: I think the answer to that is it depends. It depends on the time of formation and the heat source available.

MR. CATTON: Also, don't you have to consider that the fragment --

MR. KFLBER: I may have misquoted the report. That's why I said, this graph has to be read with some qualifications. But coolability of debris bed is not a simplistic topic and it is going to be treated by the experts in the area later this afternoon.

So why don't we defer until then. But I thought that was an accurate quotation, summary, from the report.

We did learn some lessons and these lessons have a wider range of applicability, and I'd like to dwell on them a little. Accident management is a new topic. It's been referred to in various aspects in your own report to Congress, in NUREG 0603, in Section 1.2 and Dr. Murley is formulating a new program that includes this topic, which I will comment on a little bit later.

I am sure you want to hear more about our suggested program in this area, as soon as Dr. Murley has these matters well in hand. In the particular application reviewed in the Zion and Indian Point study, we developed a large number of alternative strategies for managing the accident, once we presumed that we were inexorably headed for core melts.

That is a presumption which was questioned earlier by Professor Shewmon and I'm going to comment on that in my final presentation.

Most of these strategies are governed by the prediction that the interaction of the molten core with waters from the accumulators would produce so much steam, so rapidly, that the containment pressure would rise from near, or over, the faliure point, even with a significant amount of venting capability.

The strategies aimed at reducing the steam spike, or by circumventing it, by reducing containment pressure earlier, are spreading out the period of steam generation.

We cannot at this time rule out the production of such a steam spike, but there is a natural reluctance that was explored earlier to accept this prediction without better data.

Finally, the prediction of structural failure under static loads is still less precise and more dependent on personal interpretation of sophisticated computing results than we would like to see. We will be discussing these problems with the structural engineering research branch, who are very supportive of our efforts, and the obvious question is ought we do a more precisely directed containment testing program to study this problem in greater detail.

that concludes the first part of my presentation.

CHAIRMAN KERR: Are there questions on this first part?

MR. LEE: Yes.

Could you comment on the verification of the March code in your program plan at this moment?

MR. KELBER: I'm going to be taking up that in the next two presentations, and Dr. Mel Silverberg will be following my second presentation with a detailed discussion of the work directed specifically at fuel melt interactions.

So perhaps we could cover that then.

CHAIRMAN KERR: Do you have other questions?

If I understood your comment concerning filtered vented contingencies, I would assume that you would not, at this point, with the information available to you, feel very comfortable about making a decision as to whether one should be installed?

MR. KELBER: We have yet to come to an understanding of what is the way we arrive at a design basis for such a system if, in fact, we go ahead and decide to put one in.

CHAIRMAN KERR: Now, I would assume that this, at this point, is an open question at the Indian Point and Zion situations.

MR. KELBER: That is my understanding.

CHAIRMAN KERR: What does NRR have in mind? Is there a program that will provide you with the information that you think you need to make a decision within the time you need to make such a decision, or do you agree with Mr. Kelber's evaluation that at this point you don't have the information you need to make a decision?

Is my question clear? It was somewhat rambling.

MR. MEYER: Let me try it.

We presently do not have the information. We have been working closely with research, and I referred to one letter in particular that has recently been transmitted to research that outlines our concerns and our expectations regarding several of the program elements that are germain to the design Indian Point effort.

In particular, we are anxious to see the core melt interaction with concrete materials program accelerated. We are --

CHAIRMAN KERR: This is an experimental or an analytical program.

MR. MEYER: This is an experimental and analytical and Mel will be telling you more about that.

CHAIRMAN KERR: And it is your view that within six or eight months it can be producing the results you need to make your decisions?

MR. MEYER: I wish that Andy Marchese was here to answer that question.

CHAIRMAN KERR: I'll accept --

MR. MEYER: I'm not the appropriate person to answer that question.

CHAIRMAN KERR: But I think if you're going to need the information that you do not now have, in order to reach a decision, is there an alternate way of getting whatever information you will have to have to reach a decision?

MR. KELBER: Jim, may I interject a remark?

I think that we will have some early data that will be very useful in screening various strategies with respect to the employment of a filtered vented system.

There are some early analyses --

CHAIRMAN KERR: I don't know whether that means yes or no.

MR. KELBER: The answer is that we will not have complete data but we have data that will narrow the problem substantially.

MR. MEYER: We hope we have impressed on research the urgency and the importance of the Zion and Indian Point activity and like I said before, the addressing of real systems, practical conceptual designs, the type of thing that has been done in Al Benjamin's work at Sandia, these are the types of things that we are looking for in order for us to make decisions regarding design basis and design criteria.

CHAIRMAN KERR: Well, you see, if I were a pessimist, I could assume that one is moving towards a decision and it would be nice to have the information needed to make the decision, which is you're going to have to make the decision anyway. The problem with that -- I'm in favor of decisions, but the problem with that is that you might make the wrong one and it might be a very wrong one.

I'm groping for something that says to me, you won't have perfect data -- you never do -- and you won't have all the information you'd like to have -- you never do -- but you're confident that given some decision point you will have enough information that you can make a reasonably intelligent decision.

Now, do you think you have a schedule that will permit you to be comfortable with that?

MR. MEYER: I think that, if I understand some of the redirection that is taking place now in formulating the final research program correctly, that we should by our December milestone date, have enough information that we can at least answer the questions you implied a minute ago, namely are there flaws in these systems? Are there ways in which they interact with other systems to possibly present more problems than solutions and also have a very good feeling for, given what we feel are representative and scoping accident sequences, that these systems have a reasonable chance of working as designed.

I think that we're in a position to address those. The more comprehensive question of risk reduction is another matter.

CHAIRMAN KERR: In your view, you have the resources and the priorities that are appropriate to dealing wiht this question?

MR. MEYER: Well, no, I didn't say resources. I said my understanding of the redirection of the plan. I think Dr. Kelber will comment on resources.

CHAIRMAN KERR: I'm not trying to make you say things that you didn't say, but it seems to me if you say that you think results will be available that will permit you to answer the question, that implies that the resources and the schedule are such that, come December, you will get the results.

I'm not trying to put you through the third degree or anything. I'm just trying to understand what you're telling me.

MR. MEYER: There will have to be some reworking of priorities in terms of both NRR and Research, financial resources, in order to accomplish this. But perhaps I'm an optimist, but I would anticipate that type of reallocation taking place.

The Zion/Indian Point and the action plan has the highest priority of the three priority classes and I understand the Commission will accept these priorities.

CHAIRMAN KERR: Thank you.

Continue with your next presentation please, Mr. Kelber.

MR. KELBER: Obviously, the effort directed at Zion and Indian Point is just the tip of the iceberg. In addition to the near-term work we have now to plan a more systematic approach and I have been asked to comment on the needs for future Class 9 accident research.

The program that I am now in the process of formulating is related to the agency mission as follows.

First, the NRC rulemaking activities related to Class 9 accident evaluation and site evaluation require technical foundation as did the ECCS proceedings in 1973.

Class 9 accident research program aims at producing some key results while the rulemaking hearings are still underway.

I am not so optimistic to believe that those hearings will be over in a short period of time. I believe it is going to require a great deal of deliberation and that the hearings may take two to three years.

I have in mind formulating a program that lasts approximately four years, producing some key results within the first two years, but there are some pacing problems which I will discuss with you as we go on.

CHAIRMAN KERR: Just to give me some perspective, we've talked a lot about Indian Point and Zion, which I think is a sort of a first decision.

MR. KELBER: Right.

CHAIRMAN KERR: There is a later group of plant, some of which are beginning construction, with which one must deal. Is the assumption that the Indian Point/Zion results will permit one to deal with those plants or does research have some responsibility to get additional information perhaps before rulemaking to deal with those problems?

How does one see this schedule?

MR. KELBER: I do not know what the view of my management is on the scheduling problem, but I have expressed somewhat forcibly the position that we will not have the luxury of waiting indefinitely for answers to come forth, and that some of these problems are very near term.

I think there are a substantial number of staff at the NRR who feel that in the near-term, analagous problems will arise and we will have to handle them as they come.

CHAIRMAN KERR: But as far as your chronology is concerned, you are going from Indian Point/Zion to research that you feel will be related to rulemaking at the present time?

MR. KELBER: That is correct.

I do not believe that we can carry on effectively an ad hoc program. There are too many problems here which cut across the board.

CHAIRMAN KERR: Does that imply that in your thinking you believe that the results of the rulemaking must be in hand before one deals with plants beyond Indian Point and Zion? I'm not trying to ask now for Commission policy, but the basis for your plan.

MR. KELBER: I have given a lot of thought and have not formed a conclusion to that, because I'm not sure what the pluses and minuses are on all the sides. I simply do not know how to answer that question at this time.

I just know that we have to deal with these problems and that we have to deal with them expeditiously.

CHAIRMAN KERR: Mr. Oshinski?

MR. OSHINSKI: Thank you, sir.

I'd like to say something here. At NRR there is an evaluation taking place now and a paper being put together for the Commission in regard to whether selection of additional plants past Zion and Indian Point is appropriate, and, if so, what type of selection criteria might be considered and for what purpose, additional risk evaluations, or whatever is appropriate.

That is being put together now. It is my understanding that that would go to the Commissioners within the timeframe of the next couple of weeks.

CHAIRMAN KERR: Thank you.

MR. KELBER: The second item that relates this program to the agency mission comes back to management of accidents, including strategy for engineered safety featuring deployment or operator action. This is largely unexplored territory.

The Class 9 accident research program will provide the technological basis for comparative analysis of strategies --

CHAIRMAN KERR: I will not continue to interrupt indefinitely, but I get the impression that this is a program that is being done, not in direct response to NRR but in response to what you think some of the questions are that they would be asking if they had time right now.

MR. KELBER: That is correct, and we are going to meet with them. It is my mode of operation to set down my thoughts as well as I can, discuss them with my own management, discuss them with NRR in a collegial way, discuss them with you.

One of the reasons we are here today is to get your input. And there will be an agreed-upon plan in the not very far distant future.

CHAIRMAN KERR: Thank you.

MR. KELBER: I should say, by the way, we anticipate that there will be questions developed during the rulemaking that obviously may change the direction of work. None of this plan is going to be graven in stone.

The Class 9 action and the research program will provide the technological basis for comparative analysis of strategies similar to the analysis of the filter vented containment strategies now being carried out. The Class 9 accidents are generally characterized by multiple failures, including failures of engineered safety features.

It is highly likely, therefore, that operator intervention will be required to avoid containment failure.

From a risk perspective, this program of work forms the basis for assessing methods for preventing and mitigating the greatest source of risk to the public. Note that I am not addressing whether that risk is too high or too low. I'm simply saying that this chart which as the logo signifies, was prepared by Battelle, says that if all PWR's were like Surry, the risk to the public expressed in the rather peculiar unit, equivalent to iodine 131 curies per reactor year, is 540 from the Class 9 accidents and the next largest is the normal operational release of 1.7.

CHAIRMAN KERR: Is the significance of plural of the PWRs something or other, or is it just some arbitrary normalization?

MR. KELBER: An arbitrary normalization.

Now, this comparison is hardly exact for any plant. It may show some significant variation from plant to plant, but the potential for risk reduction by prevention and mitigation of Class 9 accidents is very clear and a factor of 100 reduction in the relative risk from Class 9 accidents which may be achievable is probably as far as one can reasonably go on a cost-benefit basis since that then would bring this number into the as low as reasonably achievable range.

The Zion/Indian Point study suggests that a factor of 10 reduction in relative risk is readily achievable, at least in new designs, and whether we can get another factor of 10 or not is still an open question. It is my personal judgment at this time that we can.

We don't suggest, in our program, whether this reduction is needed.

CHAIRMAN KERR: I don't understand the significance of those numbers.

MR. KELBER: This is simply a common basis for comparing the risks. This is similar to farmers unit of curies per reactor year.

CHAIRMAN KERR: I know, but for example, I assume the probability of the normal operational release at one.

MR. KELBER: That's correct, but it's a very small release.

CHAIRMAN KERR: Now, clearly the probability of the 540 is not one.

MR. KELBER: No, no. This is the product of probability times consequences. It is the expected value --

CHAIRMAN KERR: It says curies --

MR. KELBER: It is the expected value in curies per reactor year.

The risk is put into -

CHAIRMAN KERR: It seems to me that that implies that some accident with low probability is going to release just about all of the iodine, I presume.

MR. KELBER: As an example.

Multiply the frequency of such actions per reactor year times the release.

CHAIRMAN KERR: I understand the technique, but I don't understand the significance.

MR. KELBER: The significance is that this is a measure, properly normalized, of the relative contribution to risk from normal operation, from the Class 3 through 8 accidents, and from the Class 9 accidents.

The risk all resides, to all intents and purposes, with the Class 9 accidents.

We've heard this statement before. This is a numerical expression of what is meant by that. It is also a numerical expression of where the cost-benefit may possibly lie, how far you would be able to go.

Now, there is a temptation, nevertheless, to focus on the more probable, the less consequential events. Let me illustrate very briefly the comparison of the likelihood and consequences of the accidents in the Class 3 through 8 as compared with the Class 9 accidents.

The temptation is great to concentrate on the Class 3 through 8 accidents because they are so much more likely as a class. But the pay-off is very small, as we have illustrated on the preceding viewgraph.

Of course, we have to avoid some blanket judgments. Again, these are numbers based on a study of what is believed to be a representative plant, but it is known that there is wide variability from plant to plant.

An example is the study of valve reliability. An examination of WASH-1400 shows that the failure of a valve to reseat the event Q is reasonably likely in a plant like Surry but the sequences involving event Q do not contribute substantially to risk.

On the other hand, a brief IREP review of Crystal River apparently indicates that sequences involving such an event are predominant there. So the studies such as IREP are vital to determining the priority which one attaches to various problems of this sort. Simply because a problem is of low priority in one plant does not mean, in other words, that is of low priority in all plants, or conversely.

Now, the studies of component reliability do not, by theselves, assure accident prevention or mitigation and I want to address this in more general terms to set the context and this I do in my next viewgraph.

MR. LEE: Would you say the uncertainties associated with these estimated probable consequences would increase as you go into low probability events?

MR. KELBER: That may be a general statement, but I'm not sure that, as all general statements on risk analysis, I'm not sure that that has a great deal of meaning in it. It certainly is a generally true statement that we know much less about rare events than about likely events. But, on the other hand, we might not need to know very much about them.

MR. LEE: I don't understand why you say we may not need to know very much about it.

If the uncertainties for low probability events are four orders of magnitude larger -- I'm exaggerating, of course, perhaps -- it is much of a difference.

MR. KELBER: It is possible that there are events which are very rare whose likelihood we either overestimate or greatly underestimate and, to give you an example, a question is the existence of earthquakes that are so severe that they would do massive damage to the plant. Now, there is nothing you can do about mitigating the consequences of such an earthquake except not build the plant.

Now, it is going to be a very difficult job to be able to make a very precise determination. If, however, you know that it is less than some given bound, you may be all right in going ahead in ignorance. And that certainly is the basis on which are proceeding at the present time.

I would like to compare, at this time, the benefits and the ways we look at prevention and mitigation. This was brought up earlier.

I think we have to appoprtion resources between these two topics, and I want to point out that there is a substantial stake that the plant owners have in accident prevention. The experience of TMI-2 shows that quite apart from the questions of public health and safety, the owners have great incentives to avoid the enormous penalties associated with the cost of replacement power during a prolonged plant outage.

At least one utility has had favorable experience in bond rating as a result of a company funded and staffed program aimed at the prevention of serious accidents.

It is my recommendation that the NRC role in accident prevention -- and this is strictly a personal view -- be restricted relative to that of DOE and the utilities. The IREP, the human factors, ECCS and the related programs, the multiple failure and accident analysis, enable us to assess the solutions to discovered problems and to audit, in a detailed way, the work of the utilities.

I, and many of my colleagues join me in this, feel that the burden of developing detailed solutions and implementing them in this area should be borne by the industry. In this respect, the Committee may wish to assess in some detail what the industry groups are doing to prevent Class 9 accidents and, in so doing, I recommend the logic developed in WASH-1400 be used to screen those programs that cater to prejudices existing in the industry, or within the NRC, and those which purposefully address risk reduction.

Now, one common factor to both prevention and mitigation is the multiple failure accidents analysis. I've mentioned this briefly before.

In accord with the recommendation I.2.10 in NUREG 0603 and related recommendations you made in your most recent report to the Congress, Dr. Murley is organizing a program devoted to analyzing the system interactions and opportunities for operator intervention to reduce accident consequences.

I'd like to say that I understand that you are preparing to hear a brief report prepared by NSAC at a later date on operator actions which might be employed in response to a small brake accident. I think that is certainly a step in the right direction.

CHAIRMAN KERR: I'm trying to relate to what you are telling me to the rulemaking and what you're telling me now is sort of a philosophy and an approach to safety which would include Class 9 accidents.

MR. KELBER: Yes, sir.

CHAIRMAN KERR: But at some point it's going to become more specific?

MR. KELBER: Yes, sir.

CHAIRMAN KERR: I'm discussing how we apportion resources, and what I am trying to justify is a concentration within our program on mitigation knowing that there are tools in place outside the scope of our program which allow us to assess prevention.

In other words, what I'm saying is that our program is not all things to all men, that there is an important role for prevention. I think it is equal to mitigation. I don't think we should be doing it.

If you feel we should be doing it, we can tackle it.

CHAIRMAN KERR: Okay.

Now, you earlier also mentioned, if I understood you correctly, that there was some significant difference plant to plant in risk and I assume your evaluation was one in which the problem was also a corresponding significant difference in the probability of core melt. Or was that it?

MR. KELBER: I would imagine there would be.

CHAIRMAN KERR: Now, is it being thought about, in a longterm basis, even within RES as a recommendation to the staff, or within the staff, that one will attempt to establish some uniform goal for core melt probability or for consequences? Without necessarily determining how it should be achieved, but to say here is what we consider to be a reasonable goal for something or other.

MR. KELBER: I suspect that one may want to try a variety of techniques. One could establish a goal. One could establish a variety of partial standards, such as the likelihood of core melt, that you have to meet or exceed.

Bill Stratton, who used to be a member of your committee, has commented that perhaps we ought to have a trial period in which, along with a conventional licensing approach we have a parallel approach based on this.

I think this is a topic which --

CHAIRMAN KERR: I think you're telling me that you're telling me that there hasn't really been much thinking about this with RES but maybe some day somebody will give it some thought.

MR. KELBER: I think it has to be done.

CHAIRMAN KERR: Yes, sir?

MR. OLSHINSKI: In that regard, the generic proceedings the Commissioners have said they are going to hold -- I don't know the timeframe, but fairly soon -- on high population density site questions as to whether they should be trated differently than just overall deterministic ruling, I think will touch upon this because, in addition to looking at the population density they will also be looking at the risk question and clearly, part of that question is going to be what type of limit do we want to have on a class 9, of what probability?

So I think that proceeding --

CHAIRMAN KERR: Has the Commission asked somebody within the staff to begin some serious work on setting such goals?

MR. OLSHINSKI: Not yet, but they have recently gone on record as saying they were going to have generic proceedings in that regard for high population density plans.

CHAIRMAN KERR: I guess I don't know how one has generic proceedings and I probably wouldn't understand it if I did, but it seems to me to be helpful if the Commission went into this with at least some background homework. Now, who is going to do that?

MR. OLSHINSKI: As far as I know they have not asked the staff at this point.

CHAIRMAN KERR: When are the hearings to be held?

MR. OLSHINSKI: The timeframe has not been specified. The feeling I got during the discussions that they had on it was talking within the next six to eight months.

CHAIRMAN KERR: So they will have to ask somebody fairly soon.

MR. OLSHINSKI: Yes, sir.

MR. KELBER: I think there's a problem here and that is all of these proceedings, the siting evaluation, the degraded core rulemaking, emergency planning, all have a common thread. There is a logic which is supplied by the risk evaluation approach and the question you're asking is, is that going to be applied? And I don't know the answer to that.

I think that's a question you may want to take up with Dr. Budnitz.

MR. SHEWMON: While you're interrupting, let me bring up another point here. I can remember DOE people quoted as saying that they only did research on things which had to do with licensable events -- that is, not Class 9.

Have you any evidence that TMI-2 has changed that viewpoint over there, or that they are going in this direction at all with their program?

MR. KELBER: I would prefer that you ask them directly because we have to give them time to review their program just as we are doing it, and you know, we're just in the midst of recasting our program now.

I don't want to prejudge their views.

If you'd asked me that several months ago, I would have said no, but just as we're changing our views, they may be changing theirs.

MR. SHEWMON: As we get into this research program, we're going to put off until next year, Mr. Chairman, talking to DOE probably because we're not sure what we're doing ourselves yet and by that year maybe they'll know?

CHAIRMAN KERR: Is that a quesiton or a statement.

MR. SHEWMON: It is sort of like Medieros's questions that sort of implied a conclusion, I guess.

MR. OLSHINSKI: Dr. Kerr, there is a meeting of the improved safety system of the subcommittee on June 25th in which that subcommittee is going to renew the NRC work on improved safety as well as the DOE work on improved safety.

They will begin to discuss this exact issue.

CHAIRMAN KERR: That's a meeting --

MR. OLSHINSKI: In Washington on the 25th, June 25th.

CHAIRMAN KERR: June 25th.

And DOE will --

MR. OLSHINSKI: They've been asked to give a presentawtion.

CHAIRMAN KERR: To NRC?

MR. OLSHINSKI: No, to the ACRS subcommittee on improved safety systems.

CHAIRMAN KERR: Okay. I'm finally catching up to you. Thank you.

MR. KELBER: Let me finish with this viewgraph by saying that for the nearterm the primary goal that I propose is to develop means for assessing the utility of mitigating features with the point to reducing the risk to the public.

I believe this is clearly in line with the NRC mission of assuring public health and safety and I think it is appropriate for NRC to bear the major burden here.

Let me anticipate a little bit the type of Commission decisions which I believe will follow the rulemaking.

These decisions require technical support and we aim to supply that support. We aim to supply it by answering a series of questions which relate to how do you assess the threat to containment.

Answering these questions forms the logic for the program. The reason is that as a core melts, that is, as a Class 9 accident, the containment is a physical barrier between the fission products and the public. Public health and safety is assured by assuring containment integrity at least long enough for evacuation.

The Zion/Indian Point study indicates that it may be feasible to assure at least three to four days of warning time provided there are some adequate additional safety features. Those features may not necessarily be a filter vented containment system. They may be improved methods of maintaining power to emergency core coolers and things of that nature.

So I don't want to foreclose any possible answer.

Now, I want to call attention to two unusual items in this list. The first six items, can pressures in a primary system breach the secondary? Can a melted down core breach the pressure vessel? Can a hydrogen explosion breach the containment? Can a steam explosion breach the containment? Can a hot core melt the basemat? Can the containment slowly heat up and be over-pressurized?

All these have been faced with Zion and Indian Point.

A question that we are coming to realize is that we may need to maintain some vital functions which also bypass the containment in order to keep the containment cool or to attempt to keep the core cool in some location. The let-down line is an example. The main steam line is an example. There are other ines.

And the question is, can maintainment of these vital functions bypass the containment or threaten its integrity if they should fail.

There also is brought forcibly to our attention the question can failures in instrument and control compromise the safety systems, a very serious question. And this has led a number of people to put emphasis on passive devices for protection of containment.

I think there is some merit to that.

CHAIRMAN KERR: I guess I don't understand the context for number eight. It seems to me the answer is clearly yes.

MR. KELBER: Not if you have a passive device, for example, a natural convection containment cooler that does not require power.

CHAIRMAN KERR: As you ask the question there, can failure as an instrumentation of control compromise safety systems, the answer to that is clearly yes. So you are asking apparently some different question even more specific?

MR. KELBER: You're right. For purposes of brevity I did not go into the full exposition. But my point is that there may be significant value to passive features which do not require external sources of power either for their control or their continued operation.

CHAIRMAN KERR: Well now, I'm trying to fit this into the kinds of decisions that NRR is going to have to make, and then medium and longterm.

Many of these questions have been asked and answered in the Zion/Indian Point context --

MR. KELBER: For those plants.

CHAIRMAN KERR: Yes. But surely you are not just proposing sort of an incremental process which preturbs things a little?

MR. KELBER: I think we have to give the technical basis to be able to answer it for anything.

CHAIRMAN KERR: This is called the Class 9 accident research program logic.

MR. KELBER: That's right.

CHAIRMAN KERR: Now, into what program are these results expected to feed? Just the rulemaking hearing?

MR. KELBER: They will be important to rulemaking but I believe they will give NRR the technical basis for reviewing designs just as the work on ECCS gives them the technical basis for reviewing the vented inspection performance there.

CHAIRMAN KERR: Suppose somebody said to you ---maybe you already in fact have this in place -- but at what point would the results of this be usable in plant design? Because it strikes me --

MR. KELBER: Do you want a schedule of work?

CHAIRMAN KERR: No. I'm trying to get some idea of when this is going to have some influence on what one is doing with plants, because this is a research program. It will reduce some results. It says we do or do not need filtered containment. We do or do not need core catchers, and then one has to design those systems, which is another period and it strikes me that this particular program you're talking about is likely to have influence on plant design maybe ten years from now at the earliest.

Is that your feeling?

MR. KELBER: Okay. Let me give you some estimates of where I think things might happen first, and all this is based on getting the necessary resources, obviously.

I believe that on hydrogen loads, hydrogen control and containment response, we can settle that issue in a year. I believe there is substantial industry interest in that as well, and I believe that that can be taken care of within a year.

CHAIRMAN KERR: How can you take care of that until you have decided how much metal water reaction you are going to design for. Are you going to decide that within a year?

MR. KELBER: I think we're can decide --

CHAIRMAN KERR: Doesn't that require rulemaking?

MR. KELBER: It requires a rulemaking if that's the approach you're going to take.

CHAIRMAN KERR: I thought that was the approach that the Commission had proposed. Am I mistaken?

MR. OLSHINSKI: I think you're correct, yes.

MR. KELBER: You're correct in that, but what I am saying is that I believe we can give you a technical basis for control of hydrogen up to the maximum you can get.

CHAIRMAN KERR: So you aren't really asking, can a hydrogen explosion breach the containment, but how can we handle the thing so it won't?

MR. KELBER: That's correct.

CHAIRMAN KERR: And that's a research program?

MR. KELBER: That's correct.

Steam explosion, I believe that we're partly there. I think for the first time there is some real science being brought to bear on this question.

CHAIRMAN KERR: That has to have extensive experimental work associated with it, doesn't it?

MR. KELBER: Yes, sir, and I think there is an extensive program underway. The program manager and project manager are both here, as well as one of the key investigators.

CHAIRMAN KERR: And you expect results from that to be available?

MR. KELBER: I think there are some key results now and I think that issue will be closed in two years.

Can a hot core melt the basemat? That is, what problems arise when an uncooled core attacks the concrete, and I would say that the nature of the answer depends on what you want. If you want to know the answer to the type of question which was asked at TMI-2, we have that answer now.

If you want to know more precisely whether there are favored forms of concrete, whether you should use a core catcher of one sort of another, I think we are perhaps three to four years away.

MR. CATTON: Charlie, in looking through some of these programs, do you plan to do anything with materials other than MgO, like, for example, depleting the O_2?

MR. KELBER: Yes. I think we can classify it into four different types of work. Mel will talk about some of the nearterm plans. But basically I think we have to look at both refractory and sacrificial materials, we have to look at both active and passive systems.

CHAIRMAN KERR: There's one thing I don't see in here explicitly. Maybe it's there implicitly. Are questions, it seems to me, that might arise in the course of the NRC evaluating designs of core catchers and filtered vented containments.

MR. KELBER: I'm sorry, I don't follow you, Bill.

CHAIRMAN KERR: Well, I don't know what a core catcher is going to look at. It probably won't be made out of concrete, so the question is not can a hot core melt the basemat, it's how do I --

MR. KELBER: I may have misled you. When I say basemat here, I don't necessarily restrict that to being concrete. We will test various basemats.

CHAIRMAN KERR: But I don't see anything in there that would directly interact with designs of specific mitigating systems and the kind of information that the NRR staff, for example, might feel they will need --

MR. KELBER: Dr. Silberberg will discuss such problems. That's in the details of the programs.

But the impetus for doing that work is to answer this type of question.

CHAIRMAN KERR: I could understand that you would want to answer that type of question. It seems to me that you would also want -- not you, maybe, but if people are going to have to license these things, it would be likely they would need to ask questions that have to do with the details of design of some of the systems that are likely to be proposed.

MR. KELBER: Yes, and there is an element that Silberberg will be describing to you that addresses this question.

CHAIRMAN KERR: Just be patient and I'll get the answers?

MR. KELBER: Yes.

By the way, in that, I want to return to a very important lesson that we learned from Zion and Indian Point and that is you hae to consider the interaction of these systems with the rest of the plant. It is not a trivial matter.

Let me now turn to what I think is my last viewgraph in this part of the presentation which is what is the structure we intend to give to this program in terms of our management of work. This is very fluid and, in fact, I have reason to believe this may be somewhat out of date already, since it's about two or three days old.

But basically we look at a four part structure. The transition to the debris bed from the coolable core. And I want to discuss this later on this afternoon to, among other things, address a question that was raised by Paul Shewmon earlier and also to remind you of some comments I made in your meeting with the fuel behavior branch that this area is the interface with that branch.

Again, I will remind you that because there is an interface, we are not going to construct an in-pile loop in this program if there is a perfectly satisfactory in-pile loop somewhere else.

The resources simply aren't available to do that sort of duplication.

The integrated fuel melt program that Mel Silberberg will describe to you next is the bulk of the effort. It is roughly 50 percent of the program and represents first a systematic collection and coordination of all the efforts that we have going on now that addressed the problems that arise when molten fuel interacts with parts of the reactor system.

CHAIRMAN KERR: What is the significance of describing this as Class 9 accident research advance safety technology? Is the implication that this goes on beyond what you've been describing on the previous slide?

MR. KELBER: No. It was just a name that was used for purposes of organizing the budget categories. That name is no longer operative, to use a somewhat old-time Washington phrase.

I believe the correct name now is severe --

MR. SILBERBERG: Severe accident phenomena and mitigation research.

MR. KELBER: -- severe accident phenomena and mitigation research, but I haven't been in the office for a few days, and that may have changed. Sooner or later we'll fix on a name, but I believe that is what it's being called now.

The third element is containment response to accident loads and I include in this the topic which I have alluded to a number of times of systems interactions but the whole point is the containment is a system and its response has to be done as a part of a system analysis.

Finally, included within this budget category will be the work on LMFBRs. We are providing for a possible continuing program of work devoted to problems specific to LMFBRs, but most of the work in that area will be changed in emphasis to address problems in these first three categories.

Now, that concludes my presentation on the research needed in this area and later on I will address the strategy to perform the research and some realted problems.

CHAIRMAN KERR: Thank you.

Are there questions?

MR. KELBER: I will also, by the way, be giving Bob Curtis's presentation for him.

CHAIRMAN KERR: All right.

This appears to me to be an appropriate time for a lunch break. We will break for an hour and reconvene at 1:30.

(Whereupon, at 12:30 p.m. the meeting recessed, to reconvene at 1:30 p.m. this same day.)

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AFTERNOON SESSION

(1:35 p.m.)

CHAIRMAN KERR: We will reconvene to hear Mr. Silberberg give us an overview of the integrated core melt program.

Mr. Silberberg.

XXXX MR. SILBERBERG: Thank you, Mr. Chairman.

This afternoon I'd like to present just that, the overview of the integrated fuel melt research program, and that's about all one can really do this afternoon. Fortunately, two of the elements in the program will be discussed in some detail by speakers following.

The recent background for the motivation -- well, let me just say that this program is a major component of the Class 9 accident research program that was just introduced by Dr. Kelber. The recent motivation for this program, that portion that I'm going to be discussing today, is both before and after TMI as is, I am sure, very well known to the subcommittee; and this interest with ACRS dates back many years. And as we look to the next few years it is clear that the needs for the program will be driven by the details of the degraded core rulemaking work that was described by Mr. Medieros and the NRC decisions which will evolve.

You may recall that the initial basis for program planning reflected the RES response to new directions in research growing out of TMI-2. The formulation of an integrated fuel melt research program was one of several actions taken by

Dr. Murley. And as program manager, in October 1979 I developed a first draft plan with the assistance of individual project managers in LWRSR, PAS, and ARSR.

The draft plan was reviewed at a special research review panel meeting in February, and the major comments of the review reported in April in a letter from myself to management. These comments have provided some of the bases for a substantial revision to the plan which is currently in progress.

Following the February review meeting and the insights from the Zion/Indian Point studies that were going on at that time, it became clear that the fuel melt research program was logically part of a broader program on Class 9 accident research.

We expect a complete revision of the draft plan in about one to two weeks so that it can be factored into the Class 9 accident research program in preparation for the fiscal 82 budget call.

The purpose of my presentation today is to provide you with information about the technical objective, scope, logic, and key elements of the program as described in revision one of the plan.

CHAIRMAN KERR: What class was it in before it was put in Class 9?

MR. SILBERBERG: I think that the information being directed --

CHAIRMAN KERR: It was unclassified before then.

MR. SILBERBERG: Well, certainly unclassified, but in the case of the LMFBR components that were part of it were in that direction, you know, beyond the Zion basis. And I think previously in the case of the LWR part of the program it was in the context of WASH-1400 considerations.

Program emphasis will be placed on providing some of the technical bases for rulemaking, NRC decision options, regulatory criteria, and siting and safety reviews involving considerations of degraded core and core melt accidents.

The objections of the fuel melt research program should be viewed in the context of the following questions related to the challenge as to containment from Class 9 accidents listed in the next viewgraph. And some of these in the viewgraph after this are the ones that Dr. Kelber referred to.

Again, on the objectives of the program we're looking at a data base and models for assessing consequences, information that allows us to assess special features that one may wish to design to mitigate accident consequences, leading to the overall objective I have as the third item

CHAIRMAN KERR: Give me an example of a special feature that one might assess.

MR. SILBERBERG: Filtered-vented containment system, a core catcher.

CHAIRMAN KERR: The fuel melt program you can relate --

MR. SILBERBERG: Yes. I have that logic.

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CHAIRMAN KERR: Good.

MR. SILBERBERG: I have shown here four of the questions that Dr. Kelber had on his viewgraph this morning which is where this program, this portion of the program addresses in terms of breaching the vessel, and steam explosions, basemat penetration, and ways of -- considerations of mitigating that, and the question of slow heatup and overpressurization of containment, and how one might address that.

CHAIRMAN KERR: Help me understand the difference between his slide and yours. Yours is labeled "program logic."

MR. SILBERBERG: Yes. In effect I have just taken his slide just taken the four items, four of the eight items that he had.

CHAIRMAN KERR: Okay.

MR. SILBERBERG: Because those are the ones most pertinent to my discussion.

MR. SHEWMON: Now, let's see. I don't know whether this is mitigation or prevention. Anything before the molten core gets down breached and on the bottom is prevention and not mitigation, or does mitigation -- like spraying water, for example, that's not mitigation?

MR. SILBERBERG: That would be mitigation, and within the -- let's say within the reactor vessel, and this portion of the program that I'm addressing here does not refer -- does not address that.

MR. SHEWMON: And what does?

MR. SILBERBERG: That would be looked at it in terms of the accident management portions of the program that Dr. Kelber described, as well as how you might go through some of the -- in conjunction with the melting sequences within the core.

MR. SHEWMON: Now, what does "integrated" mean? Apparently most of the problem or much of it is not integrated into this. What is integrated into this?

MR. SILBERBERG: What is integrated are the items from the time that the reactor vessel is threatened and the debris or molten fuel melts through to the time when we're into the reactor cavity and then looking at mitigation features for adjusting that.

MR. SHEWMON: Now we're told that Coates is going to talk this afternoon about melting through a pressure vessel. That is not part of this program?

MR. SILBERBERG: The debris bed and how it might melt through the pressure vessel is part of this program, yes. And maybe I can help you on the next slide.

MR. SHEWMON: Maybe.

MR. SILBERBERG: The next two slides I'm going to show are -- the shaded areas that I have here on the first of two slides that take one through the type of analysis one would normally go through for let's say a March-Corral calculation, portions thereof. And what I'm looking at here in the integrated fuel melt program is starting at the point after one has loss geometry, goes through core disruption and has in effect debris at the bottom of the vessel now challenging the vessel. In other words, is that debris bed coolable at that point?

MR. SHEWMON: Now, is it dry always in your program?

MR. SILBERBERG: No, definitely not.

MR. SHEWMON: And so debris behavior is in the presence of water much of the time?

MR. SILBERBERG: Yes. And as we get to one of the elements which deals with debris bed coolability, one determines what those dryout limits are and the so-called extended dryout capabilities of the bed even after it has dried out.

As I proceed from the assumption of vessel failure, then I proceed to the bulk of the emphasis on the program. I might say fission product behavior release in transport from the time we start to keep track of fission product release, some of which was described to you I believe last week, Dr. Shewmon, we continue to keep track of the fission product release and accumulate it until we get to the point where in the cavity as we go through these interactions we then bring on other materials, mostly non-radioactive, that will help determine the course of the settling behavior of the fission products that have come out throughout the entire course of the sequence.

Mitigation features would also be a part, an element of our program that I will describe.

MR. SHEWMON: You'll describe that today.

MR. SILBERBERG: Yes. Well, just what the components of it are.

MR. LEE: In your program with the vented containment and so on, how far or how long would you like to be able to follow into the postulated accident, in terms of days or months or years?

MR. SILBERBERG: We would like to follow the entire course of the accident as long as we're either continuing to threaten the containment integrity over a longterm or continuing to provide a challenge to the performance of mitigation features that one may have.

Now, one will, of course, in terms of the phenomenon, the behavior of these interactions and some of the things that come up in the accident consequences, those will probably require, you know, extrapolation techniques and things like that in the phenomena.

But the interest is as long as the methods are needed to assess containment challenge.

Yes.

MR. SEALE: Going back to the other end of this thing, that is, the development of this molten whatever it is, early on there was a problem, I seem to recall from the Kemeny report, regarding the energy available from the oxidation of the zirconium and the possibility of a uranium-zirconium eutectic being formed in the cladding which would limit the amount of zirconium available for oxidation.

Is that kind of thing part of your program, or is that part of another program?

MR. KELBER: May I address that, please? That's the interface I was referring to earlier. In preparation of the research budget for fiscal year 82 we have agreed that the Fuel Behavior Branch will prepare budget proposals dealing with questions -- with work, which attempts to address the questions connected with how the fuel under these accident conditions reaches the core plate.

We will prepare proposals which deal with how the fuel moves from the core plate through the lower course of core structure and forms a debris bed.

MR. CATTON: Mel?

MR. SILBERBERG: Please.

MR. CATTON: Do you have this figure?

MR. SILBERBERG: Yes. I'm going to be getting to it.

MR. CATTON: Okay. I'll hold until you get to it. I thought with those other figures that I'd missed something.

MR. SILBERBERG: A capsule summary of the elements and scope, very briefly, of the integrated core melt program are shown in the next viewgraph. Later this afternoon you will hear more detailed representations of two of these elements -- fuel debris behavior and steam explosions. This afternoon I will describe only briefly the remaining elements so that you will get some idea of the work in progress and that which is planned.

Before going to some of the program elements, it would be useful to look at the overall logic of the fuel melt research program and its relationship to the Class 9 accident research program.

The logic I've shown -- by the way, the double-line items are items which refer to the previous presentation by Dr. Kelber -- transition debris bed, improved system codes, and dealing with containment response to accident modes.

The elements of the program that I've just shown you are basically here, and what we have in the logic is that at this point we're laying out the program. As we proceed here on the phenomenological aspects of the program described in these elements and improve our capabilities for assessment of systems analysis, we then go back into an uncertainty and sensitivity analysis to see if we've reached a point in terms of a quantitative goal that would allow us to address a priority in terms of how refined do we want to get our answers. And this would be, if you will, the tools of the program. This would be either for risk assessment or for design evaluations, as one might need, and the requirements for the two are somewhat different.

Now, I've shown here that -- and this goes back to what Jim Meyer was saying this morning and Dr. Kelber -- that an important element of the planning here at this stage of the game is the fact that we now have to go into conceptual design studies and evaluations of real systems to see if indeed one is going to make decisions on those systems. And I want to point out that for the moment even though it's shown in this logic, such decisions have not been made.

But as one goes through this part of the program, then depending on what is needed here and to meet, let's say, the requirements of rulemaking and NRR and licensing, we would then go back in and have to reassess the priorities of the program.

The problem now is getting into this sequence after we have -- we're taking a program that has ongoing work and trying to move it in this direction so that we can come up with and be useful in terms of the near-term and long-term applications.

MR. SHEWMON: What does "core melt modeling" mean?

MR. SILBERBERG: That would be taking pieces or modules, if you will -- they might even be codes in some cases -- that would be part of an overall integrated systems analysis code and providing, if you will, the needs here and having an interface at that point. So this work here and this work here would closely follow one another.

MR. SHEWMON: You haven't said any words that speak to my question yet. You'll have to try another way, I guess. Are you going to melt things up and worry about the chemistry of it, you worry about the fluid hydraulics of it or fluid dynamics or what?

MR. SILBERBERG: Excuse me. Yes. Primarily we're looking for the areas on that other chart, the accident sequence chart that I showed previously. We'd be looking at the thermal, chemical, if you will, thermodynamics that relate to the interactions of melt with other materials primarily, leading to load sources such as gases, vapors, aerosols and what have you.

MR. SHEWMON: Okay.

MR. CATTON: Does that mean that you would take the data from melt interaction, your second block, and feed it into core melt modeling so that you'd have something to stick in the code?

MR. SILBERBERG: Yes. Some of that --

MR. CATTON: So one is the experiment and one is the --

MR. SILBERBERG: In other words -- yes. Some of the phenomenological modeling obviously would be done as one works closely with the experiment. This exact interface here, between here, in terms of programmatically between the modeling and the codes are still under discussion, if you will, internally. Dr. Kelber may say something more about that.

MR. SHEWMON: Maybe a better way to ask the question is what do you expect these codes to be able to predict?

MR. SILBERBERG: Be able to predict the containment pressure, temperature, history, fission product behavior, aerosol behavior within containment.

MR. SHEWMON: So part of that might be to get a more realistic evolution of the melting process in March?

MR. SILBERBERG: Yes.

MR. SHEWMON: Okay.

CHAIRMAN KERR: As I look at the caption under figure 2, I see integrated fuel melt research program logic and relationship to Class 9 accident research program.

Now, which is the Class 9 accident research program and which is the fuel melt research?

MR. SILBERBERG: Excuse me. The ones that were in the double box, as I thought I had mentioned, Dr. Kerr --

CHAIRMAN KERR: Well, you probably did.

MR. SILBERBERG: -- Correspond to the same categories that Dr. Kelber had on his chart this morning here, here and here.

CHAIRMAN KERR: So what I see, the larger chart is the fuel melt research program?

MR. SILBERBERG: That's correct.

CHAIRMAN KERR: And what you're showing me in those boxes is what relationship it has to the Class 9 accident research program.

MR. SILBERBERG: Right. Now, in other words, information from this program flows to it.

CHAIRMAN KERR: Whose research program is the Class 9 accident research program? Is that RES's program or NRR's program?

MR. SILBERBERG: Yes. It's RES's program.

CHAIRMAN KERR: And it's being designed in some consultation with, but not necessarily at the request of NRR, is that --

MR. KELBER: Let me reply to that. Yes, it is an RES, projected RES response to a perceived need. Very shortly as we get our perceived needs listed more precisely, we will be discussing this with NRR and other interested parties. We will incorporate your review as well, and we will be going to review groups, and we will have a final plan, the program.

We're giving you a snapshot in time of our best guess as to what the program contains.

MR. CATTON: Mel, before you leave this, transition to debris bed, fuel debris, melt interaction, and steam explosions all look to me like they're experimental programs of some kind or another.

MR. SILBERBERG: Yes.

MR. CATTON: Now, in the face of the conclusion that was made yesterday by Pete Cybulkis that the bottom line being radiological release, the sensitivity of various aspects like core melt modeling, he felt that it was relatively insensitive to that.

It's my feeling that the fuel debris or debris coolability and dryout is fairly well in hand; it really doesn't need much more. Could you kind of put a figure of merit on each one of those, something that you would use in allocating your resources?

Where is the biggest gain with respect to radiological release in those four boxes?

MR. SILBERBERG: Well, I actually will discuss that very briefly, but yes, as we've noted in our planning, right now this box is -- these two here are currently the largest. But this is way under --

MR. CATTON: I understand mitigation features.

MR. SILBERBERG: Yes. I want to couple them.

MR. CATTON: I'm looking at the basic programs that you have that are here in support of your looking at some concept or whatever, and I see transition to debris bed, which I think is very important and that came out that we didn't know what the debris bed looked like, but if we did, we knew whether it was coolable or not. That kind of tells me that your fuel debris box there ought to be a low priority item.

Melt interaction, core melt modeling based on the results of the March-Corral predictions sort of indicated that gee, those are not all that important either, because it didn't change the source term.

Now, is that a result of the March-Corral code system?

MR. SILBERBERG: Yes.

MR. CATTON: And it's bad?

MR. SILBERBERG: To a large extent I might say that's kind of getting to my next item, but --

MR. CATTON: Well, I can wait.

MR. SILBERBERG: Well, it turns out Battelle-Columbus is doing this uncertainty analysis --

MR. CATTON: That's what I'm referring to is their uncertainty analysis.

MR. SILBERBERG: That's right. And when that report is out, we will have to assess it and --

MR. KELBER: Let me interject one remark.

MR. SILBERBERG: Go ahead.

MR. KELBER: There are two types of accuracy needed, and the uncertainty analysis addresses itself to the different classes of uncertainty. If you are doing risk analysis, order of magnitude uncertainty may well be acceptable. If you are doing a system evaluation to decide the relative benefit or lack of benefit of a mitigation feature, you may need substantially more accuracy.

I think that it is premature to apply the order of magnitude type of analysis to this type of work. I tend to agree with you that there are some things we know a great deal more about than others, but the fact that we know a great deal more about them may not mean -- does not necessarily imply to me that we should abandon them.

To take as an example the steam explosion work, I don't think we can leave that problem hanging just a little bit away from final resolution because we're pretty sure we know what the answer is. I think we have to close the loop and make sure that our estimate, our current estimate, is really in the right direction.

MR. CATTON: I guess I would submit that as long as it's not at the expense of something that's needed in the short-term.

MR. KELBER: Okay.

MR. CATTON: And that's what I was referring to, an allocation of resources, and I was trying to get at how you did it or how you're going to do it.

MR. SILBERBERG: Well, we're just starting to do that.

MR. KELBER: I think a lot of that is going to come out of the question of how we do a systems analysis of the filtered vent containment system. That's going to tell us a great deal about what really matters.

MR. SILBERBERG: Ivan, I think you made an excellent point, and I thought I tried to explain at the beginning that we're in the process of trying to make those decisions now for ongoing programs, sort of, you know, moving out and looking ahead to the budget process. And I think the types of --

MR. CATTON: I understand. It's just that I see a very heavy load about to be placed on you by NRR's requirements for the near-term Z/IP study. What is it, eight months or something? And it seems to me that if you wait to make these decisions very long, eight months is going to be gone.

MR. KELBER: Yes.

MR. SILBERBERG: Good point.

Well, let me say this very clearly, that the work I'm going to describe that's in this program, much of it comes well beyond eight months. I think we ought to be realistic about that.

MR. CATTON: Would you mention briefly what you have in mind for core retention devices? I don't see it spelled out anywhere in here?

MR. SILBERBERG: It's in the mitigation element, and I'll just say that our work started with some of the needs of a floating nuclear plant, and this now will be extended to consider land-based plants.

MR. CATTON: The work for FNP was mostly MgO, as indicated earlier, and I think that that's highly restrictive.

MR. SILBERBERG: Correct. And under our generic program and materials interactions element down the road here we do look at other candidate materials.

MR. CATTON: So how timely is this going to be, again with the thought that you've only got about eight months?

MR. SILBERBERG: Right now in the next eight months the only candidate material that we have the most information on is the MgO, again and at the direction of NRR in their most recent --

MR. CATTON: Well, you're doing a large number of Z/IP type studies. Are you going to do any analysis on other type materials? For example, SNR-300 has a 15 centimenter thick cooled core catcher depleted UO_2. The Japanese had some ideas. Are you going to try to bring any of this to bear for NRR, or are they going to do that themselves?

MR. SILBERBERG: No. If I get to the mitigation features element, we will be going into a conceptual design study --

MR. CATTON: I'll wait if you're going to talk about it.

MR. SILBERBERG: Okay. Now, we would like to make those --

MR. KELBER: Not in the next eight months.

MR. SILBERBERG: Probably not in the next eight months. I think we'll be maybe happy, you know, to get the work started in the next eight months, but let me get to that.

MR. LEE: I have a question.

MR. SILBERBERG: Yes.

MR. LEE: For the near-term again, the transition to debris bed and so on, what are the assumptions you make for your part of the program?

MR. SILBERBERG: We assume that we have a debris bed.

MR. LEE: Uniformly mixed?

MR. SILBERBERG: Uniformly mixed, sitting, if you will, down near the bottom of the vessel. And one now has to determine is it or isn't it coolable. But as we proceed on with the other parts of the program, one assumption is -- that's why we have the interaction of the concrete -- is that it's not.

But the research program will try to address what the limits of that coolability will be and the conditions.

MR. KELBER: Mel, excuse me. There is a request that you could consider mitigation systems now. Someone has to leave for a plane.

MR. SILBERBERG: Okay.

MR. KELBER: While Mel is shuffling through, I might say that the only information we have that covers the transition to the debris bed comes out the LMFBR program. The materials are different, the designs are somewhat different. Some of the considerations, however, are general enough that we can transfer those considerations. The technology certainly can be transferred.

But from the point of view of estimation of what the debris bed looks like, we have no hard data in this area. We have to make the best guess we can based on what we know from LMFBR experience.

We will have a key experiment in the LMFBR area run off this September, but it will not bear directly on this problem.

MR. LEE: So you have to consider something like transition phase and so on again?

MR. KELBER: Yes. Same problem, different reactor.

MR. SILBERBERG: In the mitigation features --

MR. CATTON: Excuse me. Are you referring to the transition phase in Clinch River?

MR. KELBER: Yes.

MR. LEE: I was.

MR. CATTON: Do you expect to have a boiled up core?

MR. KELBER: No. We don't expect to have it in Clinch River either.

MR. CATTON: I would agree with that.

MR. SILBERBERG: I might add that in the mitigation features element the NRC project managers are Ray DiSalvo from PAS who actually got the program started, and Tom Walker from ARSR who's been looking at core retention devices.

The program be it for vent filter containment, core retention, or alternate containment systems basically has the same logic which Ray has set up for the vent filter containment study. The work on vent filter containment is being done at Sandia, and the task leader for that is Alan Benjamin.

And the three aspects of the objectives on mitigation features are shown here, namely to propose functional safety design requirements in the features, assess their value and impact for implementation, and then as needed perform separate effects tests and analyses to confirm either the feasibility and/or performance of the mitigation features in terms of has one set the proper design criteria for them, can one test whether or not there is something missing, or perhaps some uncertainty on the Zion criteria that is not confirmed, if you will.

And the scope, very briefly, is to develop a set of general design concepts for a spectrum of accidents in containment designs and LWR, assess feasibility and effectiveness and impacts for each of the concepts, develop detailed designs for the most promising considering both backfit and new construction considerations. More detailed are the -- specifically will be work on the molten core retentions systems, vent filter containment, to determine practicality and feasibility and to identify key research needs.

Also mentioned this morning, the systems interaction studies using any one of these devices. Are they beneficial or is there a problem? In other words, do we gain less by putting them on?

MR. SHEWMON: The only place you consider cooling this thing or cooling anything is core retention, is that right? Primarily you're looking at containment?

MR. SILBERBERG: That's correct.

In other words, if we go way back to my objective chart, the bottom line was the threat, you know, the immediate threat to containment. But as I mentioned, the accident management work and work up above debris bed, those aspects, looking for, if you will, mitigation there, is an important part of the overall program and --

MR. SHEWMON: Yes, but I think you're going to end up talking out a hundred or ninety degrees out of phase with regard to the industrial people then, and they come back and say there are so many ways to cool this on the way down that that's the way we ought to worry about mitigation. And what I see here is you won't say -- are you doing anything that will allow NRR to speak to this any better, or do I miss something?

MR. SILBERBERG: That is, I believe, part of the accident management part of the program.

MR. DI SALVO: Well, I think most of those sequences in which there are so many ways to cool the debris at some point -- and there probably are places where you can intercede -- all rely to some extent on the availability of electric power.

MR. SHEWMON: They can find ways to do it with steam.

MR. DI SALVO: Well, you've still got to get water in the core. You can move it around. So, you know, we're looking at it, fine, if power's available, you do what you can to help mitigate. On the other hand, for those sequences where power is not available, you've going to have to look at features like containment venting.

MR. SHEWMON: Yes, I don't argue that. It's just I think it's myopic or distressingly narrow to worry only about those situations in which you have no water and do nothing to help get more water, because I feel reasonably strongly that you can contain the radioactivity best if you can keep it cool, not worry about how big a lasso you can run around it, or a big crucible that you back up to catch it with or something.

MR. DI SALVO: Are you talking about in-vessel or ex-vessel?

MR. SHEWMON: Any time if you can cool it you're better off than if you can contain it. It's nicer to contain it in the fuel than to go back to defense in depth.

MR. KELBER: I think there's no argument on this, and yes, we are --

MR. SHEWMON: Well, the argument is I don't see it here.

MR. KELBER: There's a technical question that I wanted to raise later on, because I think you're headed in the same direction; and that is, is there in fact a condition of the core -- let's start with a core that's a perfectly good and healthy core and undergoes an accident.

Is there a point in the condition of that core as it undergoes the accident that melting is inevitable no matter what we do? The current approach assumes that there is such a point. It is by no means clear to me that that is the case. And I must be honest with you and say that plans on how to address that are anything but definite, partly because we really don't know too much about the answer to the basic question.

I suspect that you are correct and that it might be very wise to consider the possibility of steam-drive pumps which could circulate significant amounts of water within a damaged primary coolant system.

But there are many variables, and I think you also have to look at the cases where that is not possible.

MR. SHEWMON: There are power outages and power outages, and I suspect we're talking about many hours or many days. It's easy to say gee, there was a power failure; let's not bring up the subject again. But on the other hand, if we're talking about days, then there are also ways to recover.

MR. KELBER: Yes.

MR. SHEWMON: If nothing better than making sure they install a stand pipe that's long enough so the local fire department can get on the other end.

MR. KELBER: Yes.

MR. SILBERBERG: Summarizing the accomplishments and plans of the work, conceptual design options have been for the vent filter systems for large, dry PWR containment, and this production potential has been indicated with some of the qualifications noted.

This work will be extended to other PWR and BWR designs, and the quantities will be specified later on.

We'll be deciding from the program as to what the needs are in terms of data, including models, related to uncertainties in the vent filter analysis.

In support of the core retention design features work for either FNP or LVP, large molten fuel interaction tests are planned in '81 and '82; and that is in addition to the type of work which I mentioned before to Dr. Catton, namely that the generic work on the interaction modes or mechanisms of erosion and so forth for these materials and some of the chemical effects would be done in separate effects under the materials interactions element.

MR. CATTON: I only see MgO up here. Is that because this is an old slide?

MR. SILBERBERG: No, because right now the emphasis has been placed on MgO in terms of the large testing. As the other work proceeds, the generic work proceeds then and the design evaluations proceed, I think one would look to test other materials in specific design.

MR. CATTON: Okay. So it's some distance downstream.

MR. SILBERBERG: Yes.

MR. SHEWMON: Does the SN-300 run water pipes through a concrete mat or how do they --

MR. CATTON: It's a steel plate, the bottom side of which is cooled. On top of the steel plate is 15 centimeters of depleted UO_2.

MR. SHEWMON: And UO_2 interacts differently with UO_2 than MgO does, is that right?

MR. CATTON: It's heavy, so the heat transfer is different. You don't penetrate it near as fast, and what you melt stays there.

MR. KELBER: In the same connection, one thing we haven't really given any attention to, but SuperPhenix is considering at least an internal core catcher which is cooled by natural convection in sodium, of course, both from above and below. And I must say that they're doing their design based on models we have to date.

So there are numbers of devices that are out there being looked at.

MR. SHEWMON: Glad somebody's doing it. Dried PWR means no ice or what?

MR. SILBERBERG: That's right. The non-ice condenser plants.

We will in fiscal 81 initiate conceptual design studies on alternate containment systems, and in fiscal 81 initiate conceptual design studies on core retention systems.

I might say that this item in particular was in the most recent -- was noted in the most recent recommendations from ACRS to Congress and to the Commission last year.

And I guess in any case here, identifying concept feasibility and, you know, testing needs that one would have to come up with for either feasibility or testing of design criteria and performance.

I might, as long as I'm out of order for the person who has to leave, if I could go way back to the end and then maybe just only touch upon those elements that aren't going to be discussed today --

CHAIRMAN KERR: How much more time is your presentation going to take, Mel?

MR. SILBERBERG: Oh, I'll see if I can finish it in ten minutes. Is that too long?

CHAIRMAN KERR: If it takes that long, let's do it.

MR. SILBERBERG: Okay. I just wanted to note here that on a schedular basis we are here now on the program where a technology device has been developed and is being developed under the various arms of the program that I have mentioned. And as we proceed in time -- and I'm not going to attest to the accuracy of this in terms of the rulemaking hearings and so forth -- but we would see the program as needed, depending on what decisions were being made out here for mitigation features, see the program moving from the technology stage to the question of looking at the needs of real systems, be it either analytical or whatever experimental work one might wish to do to confirm.

Now, I've made no allowance in here at this point or presumptions as to the contributions of others, namely the industry or DOE, who work in this area. This is something that one will have to certainly factor in as those decisions are made.

I just wanted to give you an idea of how that would head as one moves in that direction for any particular mitigation feature.

I just wanted to mention on program guidance that there is some work going on now at Battelle-Columbus on certainty analysis which will give us, start to give us some handle on guidance. And my understanding is this work will be available, the report on this will be available in May.

MR. SHEWMON: Who's the PI on that?

MR. SILBERBERG: The PI? It's under Dr. Denning at Battelle-Columbus.

And we would try to continue to make a budgetary allowance in '81 and '82 to keep that part of the -- that work going.

Very quickly, Dick Coates is going to describe work on fuel debris behavior, but this is work, as Dr. Kelber mentioned, that is an outgrowth of our studies of debris beds and sodium coolant. And basically in that work we're looking at the post-accident core debris, and there are some unique experiments done on where melt retention materials are interacted with core debris in the reactor.

A key aspect here that Dr. Coates is going to describe is getting a handle on the formation and characterization of a debris bed.

Briefly on plans there, it is our current intent, depending on budget, to move into the lightwater work, high pressure water cooling of debris and in-pile experiments in fiscal 82. Some planning in '81 and in '82, depending on the budget, we will --

MR. CATTON: You're going to do water debris dryout tests in-pile?

MR. SILBERBERG: Yes.

MR. CATTON: Well, what do you expect to learn that you don't already know?

MR. KELBER: Do we have the same extended dryout characteristics we have for sodium?

MR. SILBERBERG: Dick, I believe, is going to get into that.

MR. CATTON: Okay.

MR. SILBERBERG: I suspect you'll want to leave that. I think it fits there.

MR. CATTON: Sure.

MR. SILBERBERG: Melt interactions with structure, I mentioned this somewhat already, and primarily two things -- development of interaction models such as corcon, the large-scale scoping and model verification tests with concrete, and small-scale scoping and phenomenological interaction experiments. This will be not only for use in the concrete materials but refractory and sacrificial retention materials which I have mentioned previously in answer to Dr. Catton's question on looking at other materials.

We have already looked at, in some of the scoping tests, things like high alumina cement and borax in the case of the sacrificial --

CHAIRMAN KERR: What is the time scale of what we're looking at now?

MR. SILBERBERG: I'm going to do that on the next viewgraph.

This is where we are now. We have a large facility that's been completed and going into operation this month at Sandia with those capabilities. The plans, the first test of the facility with again an MgO test will be conducted with 200 kilograms of UO_2 this fiscal year, and we're taking a look at what additional testing requirements are needed to wrap up the concrete interaction work. And that should be available this year to give us final guidance in '81-'82.

Emphasis will be placed, as one of the conclusions from the Z/IP study was that the longterm interaction behavior of hot solid debris after the debris has frozen is an important -- an item of interest. And work on that, some work has already been done, and this work will continue.

CHAIRMAN KERR: What was the Zion/Indian Point work that indicated that that was interesting?

MR. SILBERBERG: Calculations made by both -- at Sandia by Walt Murfin and Dana Powers, there was a question as to whether or not the basemat could or couldn't melt through in times of the order of, let's say, three to four days is what they came out with. And the conclusion was that they could not, based on the uncertainties today in the data base, data technology, they could not conclude whether it would or wouldn't with any assurance.

MR. SHEWMON: Now, that's in a situation where there's no water in the plant or attainable ever for three days, is that right?

MR. SILBERBERG: That is correct. And I would like to note that this work with the larger scale MgO work or the work here, we will be looking at the effect of water cooling on the program in fiscal 81-82. Up to now we've done the dry tests and we will --

MR. SHEWMON: Murfin's calculation or whatever was what --

MR. SILBERBERG: Yes, that was -- Dana, would you like to -- Dana Powers.

MR. POWERS: Dana Powers, Sandia. The calculations were made both with and without water in contact with the top of the core. The results were fairly insensitive to that.

MR. SHEWMON: Once you had the bed there, putting water on top of it didn't influence how fast it went to the bottom, is that right?

MR. POWERS: It was fairly insensitive to that. It was not treated as a bed but rather as a molten pool, and with water it simply closed the crust over. We did not allow the water to go in.

MR. SHEWMON: Did you ever hear of thermal shock on ceramics?

MR. POWERS: The problem is these ceramics in the form are fairly ductile because of the amount of concrete incorporated.

CHAIRMAN KERR: If we're going to carry on a dialogue, we need mikes for both. The recorder is having problems.

MR. QUITTSCHREIBER: If she'd take her earplugs out, she could hear.

(Laughter.)

MR. POWERS: That answers the question.

MR. SILBERBERG: I would like to note that the NRC project managers on the material interactions work is Rick Sherry from RSR, Tom Walker from RSR, and at Sandia, Marshall Berman and Dana Powers.

I will skip steam explosion because that's going to be covered by Dr. Corradini this afternoon, and let me just go through the last element I wish to discuss, the radiological source term and just give you a quick flavor of what's in there.

We're looking at release and transport of fission products and aerosols over a spectrum of accident conditions. And I believe it was just noted that the sensitivity studies done to date by Battelle-Columbus people with the March-Corral indicate that the release as well as transport behavior of the radioactivity during the entire progression of the accident is an important factor in the consequences, and the uncertainties there give rise to an equal measure of concern about the consequences for dominant accident sequences.

Now, one really wishes to know this because of certainly those analyses, but also one needs to have an understanding of the radiation environment as well as the aerosol load that one will get, and how that -- in order to look at the effects on engineer safety features as well as mitigating features such as containment venting systems.

I want to note here that the NRC project managers in this area are Rick Sherry from LWRSR and myself, and in the field we have Tony Malinauskas from Oak Ridge, Tom Kress from Oak Ridge, and Marshall Berman and Dana Powers from Sandia.

The emphasis in the near-term will be on work in the melting fuel area in 1200 to 1800 degres C. for irradiated LWR fuel. The melting fuel work will proceed somewhat later.

The key aspect here is getting at the source of the non-radioactive aerosols which will, we believe, determine the behavior of aerosol transport in the containment. And the longer one maintains containment integrity for the larger accidents, then this mitigation feature which is taken into account in LMFBR accident analysis needs to be fully accounted for with that technology in LWR, because I believe there's a natural mitigation feature that we're probably not taking full credit for, and one needs to get at that.

There is a question of verification of fission product and aerosol transport models. Some of this work can start immediately in the NSPP at Oak Ridge, a reasonable scale, working with facilities equipped to handle the environmental conditions that one sees in LWR accidents. And in '81 evaluation will be made as to whether or not there's a need for a facility other than that, for a larger facility; and this is a decision one has to make.

Work is going on at Sandia on looking at the deposition characteristics of fission products in the primary system. Work on the trap-melt model for the primary system fission product transport will be extended to the containment. Right now it is handling the primary system deposition during, let's say, blowdown cases.

By and large I think that summarizes my -- completes my presentation, Mr. Chairman.

CHAIRMAN KERR: Questions? Now, as I interpret much of what you've been presenting, it is not tied to Zion/Indian Point-2 at all. It could be tied to some later plants, but there is not a direct tie at this point; rather, you are developing basic information which is likely to be useful but which has not yet been identified by anybody other than you.

MR. SILBERBERG: Excuse me, Doctor. Let me add that much of the technology that was used in the most recent Zion/Indian Point study came from the ongoing program.

CHAIRMAN KERR: No, I'm not trying to be critical. I'm trying to identify the way in which this program is being planned.

MR. SILBERBERG: You're correct. You're correct.

MR. KELBER: There is no plan that ties current research programs to the Zion/Indian Point schedule in this element, with the possible exception of some efforts in PAS. The way we did the work, this was an ad hoc program that was a short-term special effort. We could of course repeat it. I don't think that's the way to do things. But aside from those few things in PAS, there is nothing tied directly to the Zion/Indian Point schedule as of now. That can change at any minute.

CHAIRMAN KERR: What about the later plants and plants yet to be designed? The programs within NRR for answering those questions do not yet exist or has there not been communication between the two of you?

MR. KELBER: When we talk with NRR we are going to have to face how we schedule our work to jibe with their needs. It' going to be very difficult decision.

CHAIRMAN KERR: Are you going to talk with them tomorrow, next week?

MR. KELBER: Probably the end of the month or early June.

CHAIRMAN KERR: And that talk will have some influence on your FY 81 or FY 82 or what?

MR. KELBER: It sure better. It sure better, and start with '81.

MR. SILBERBERG: The sooner we start the process, Dr. Kerr, the quicker the results.

CHAIRMAN KERR: Thank you.

MR. SEALE: To put it another way, you have no communications of requirements from RSR with regard to the Class 9 accident problem in the generic sense?

MR. KELBER: Yes, we do.

MR. SILBERBERG: Yes.

MR. SEALE: Okay.

MR. SILBERBERG: In fact --

MR. KELBER: You meant NRR, did you not?

MR. SEALE: NRR, yes.

MR. SILBERBERG: NRR, yes. In fact, during -- as an outgrowth of the first draft plan, Dr. Seale, which was reviewed in February, it was commented on by NRR, and we have made or are making adjustments to the program.

MR. SEALE: Those are to be in here.

MR. SILBERBERG: That's correct.

CHAIRMAN KERR: The next presentation is R. Curtis being represented by Mr. Kelber.

MR. KELBER: In the interest of saving time, the presentation is really very short. I would like to just give you the handout and suggest that if we have time at the end of the day, we might discuss it, if that's your desire. It's a very quick read.

CHAIRMAN KERR: That sounds reasonable to me.

XXXX MR. KELBER: And it's reasonably well self-contained.

Basically the point that is being made is that there are a number of tools available in both the fast reactor and the lightwater reactor fields that we believe can be adopted to give a framework for model development in this area. But no one should pretend that we have, except for a few things such as debris beds, really good models of many of the processes that have to be discussed. We know how to fit them together. We don't have the building blocks.

CHAIRMAN KERR: Well, when one talks about accident analysis in the context in which Mr. Curtis would have talked about it, one is not talking, I gather, about risk analysis kind of --

MR. KELBER: No. We're talking about mechanistic analysis of accident sequences.

CHAIRMAN KERR: This might be with the objective of considering the possibility of making a class of Class 9 accidents a design basis.

MR. KELBER: That's correct. And the detailed evaluation of the behavior of mitigation systems, the evaluation of possible operator intervention and the evaluation of the utility of different types of instruments.

CHAIRMAN KERR: Now, I would assume that this kind of thing would be strongly or of considerable interest to the rulemaking people.

MR. KELBER: I would consider it so.

CHAIRMAN KERR: Does this sort of thing come in response to somebody who's responsible for rulemaking, or are you ahead of them?

MR. KELBER: I think we're ahead of them.

MR. CATTON: Charlie?

MR. KELBER: Sir.

MR. CATTON: This looks quite similar to the Kess system developed in Germany.

MR. KELBER: There are some obvious resemblances. The logic, of course, is that.

MR. CATTON: Some of the codes are kind of -- they use the Boil code, for example.

MR. KELBER: Yes.

MR. CATTON: And their system is already operational.

MR. KELBER: We will get what we can. We have some pretty good relationships with Karlzruf(?).

MR. CATTON: You're jumping ahead of me. What I was wondering is if you would compare it with March-Corral and compare it maybe with what you have in mind with respect to how good it is.

MR. KELBER: There are many elements that are similar, but let me point out where the problems are. The problems are where we don't have the data. In other words, where we have a basic model such as Boil, which is really a pretty sophisticated model, the logic tells you there isn't that much you can do except perhaps invent some new computational tricks.

The problem where we have difficulty is in tracing the formation of the debris bed. If someone were to tell us what the debris bed looks like, chances are we could do a pretty good job of analyzing its thermal behavior. Although I might say it's not straightforward because we may have layers of different meltable species; there may be very odd geometries in it. So let's not prejudge that issue.

MR. CATTON: I guess I'm confused now. My view of their system was it looked rather complete. There may be some --

MR. KELBER: Oh, March-Corral is complete. It takes you from --

MR. CATTON: I'm not referring to March-Corral.

MR. KELBER: So does Kess.

MR. CATTON: Well, the Kess system looked to me to be better than the Corral.

MR. KELBER: Well, that's quite possible.

MR. CATTON: And supposedly they're going to hold a workshop at the end of summer and going to give us Kess.

MR. KELBER: The project manager who is most familiar with this is sitting right behind you, and he can tell you some of the details; but let me make again one point. I do not believe that you can construct an accurate model out of whole cloth. Kess may be a better system, but it has got to have basic defects in it because we don't have data on the fuel pin melts down.

MR. CATTON: I guess what I'm really trying to drive at is are you going to start from scratch rather than to take over something like Kess?

MR. KELBER: No. I think that's the whole point of Curtis' contention. Now, he did not mention the foreign work for a variety of reasons. One of them was brevity. But Curtis' contention is that there is a large family of codes which can be used to analyze these problems.

There are no lightwater codes, Kess or no Kess, which analyze the meltdown of the pin into a debris bed; but there are LMFBR codes which can be adapted to do that task.

MR. CATTON: Well, I don't know that --

MR. KELBER: Now, March, for example, does it by an arbitrary model, but it is not a deterministic model.

MR. CATTON: Well, the Kess series models of that aspect of the core melting are more sophisticated, I think.

MR. KELBER: They be more sophisticated, but that doesn't mean that they're more correct.

CHAIRMAN KERR: Why don't we reserve further discussion until Mr. Curtis is here?

MR. KELBER: Rick Sherry is here and can discuss the status of Kess with you.

CHAIRMAN KERR: All right.

MR. CATTON: I'm aware of the status of Kess, and that really wasn't the question. I just wanted to find out if you were incorporating this into your thinking, was Kess any good, could you make use of it by maybe modifying some of the weak models in it rather than starting from zero?

MR. KELBER: That is precisely what I think is the thrust of Curtis' discussion, that we will use what's available.

CHAIRMAN KERR: Well, that couldn't be the thrust of his discussion if he didn't mention it.

MR. KELBER: I don't think he can mention every code that exists.

CHAIRMAN KERR: No, but if he's going to mention a code that he's going to make a good bit of use of --

MR. KELBER: We haven't actually seen Kess yet.

CHAIRMAN KERR: So he may or may not use it.

MR. KELBER: Well, I think that Rick is kind of chaffing at the bit to say a few words.

MR. CATTON: Well, not having seen it would have been a good answer at the beginning.

MR. KELBER: Well, Rick is somewhat familiar with it.

MR. SHERRY: We have an agreement with the Federal Republic of Germany to compare on a module by module basis the components of the March code and the Kess code to see which of the models are the better models and to incorporate these models, you know, to transfer these models between the various countries.

CHAIRMAN KERR: Who is "we?" Who is going to do the comparison?

MR. SHERRY: Which contractor? It will be an NRC contractor.

CHAIRMAN KERR: You don't know who's going to do it yet?

MR. SHERRY: I would guess it would probably be Battelle-Columbus.

CHAIRMAN KERR: Any other questions?

Let's see. Sandia. Fuel debris cooling study.

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MR. COATES: Well, I'd like to talk about cooling. I'm Richard Coates, Sandia Laboratories. And in order to talk about an assessment of coolability of any given system, you have to know what that system is, so I'd like to concentrate in this portion of the talk on the meltdown sequence, then the states of the core that could result at any point that intervention has taken place and water is reinjected into the system. And Ron Lipinski, also from Sandia, will then talk about cooling of that state.

So let me begin with the process of evolution to meltdown. The first step where our world begins is when we start to boiloff and void water from the fuel zone. Of course, many sequences can take you to this point. We won't worry about how we got there.

Boiloff from the core region. Then the fuel starts to heat up. Things that we're concerned with are the decay heat, the fission product loss -- as damage increases, we're probably going to lose some of our heat sources very early in the game. We have the zirc-water reaction which is producing heat. We have hydrogen generation. We have heat loss by various mechanisms, including gamma decay heat, radiation, conduction, convection, helium -- I'm sorry, not helium but hydrogen performing some cooling perhaps, clad damage, oxygen embrittlement, we have spallation perhaps. The clad is going to relocate after it's damaged. We need to know where it goes. And then we have the possibility of the formation of a fuel clad rubble, much as might have been hypothesized for Three Mile Island.

That's so of step one. That's the early behavior. Following clad damage we get into the area of fuel melt, and by fuel melt here I'm talking about the local melting of the fuel still pretty much in place. We have various low melting point eutectics to contend with, some perhaps significant ones with

1700 degrees Centigrade. And we have to start worrying about how the melt is going to move, whether it comes down in a candle fashion or whether it doesn't. There will be refreezing as it gets to lower regions that are cooled by water, and perhaps there will be remelt at lower elevations, and perhaps blockage to prevent the steam from coming up and cooling the melt.

I want to emphasize that these are just conceptual drawings.

Things that you do have, though, you do have crust formation obviously, you have sintered rubble regions, you have fractured fuel regions, all sorts of possible configurations.

The next major area is what we refer to as core melt, and by core melt we simply mean that we've got a relatively large involvement of the core; it's not just a local effect. And we have the possibility of a formation of the molten fuel giving the crust that we have up here. There is structural heatup to consider and melt-in of structural steel from above perhaps.

It's in this regime that we also start to worry about the effect on core barrel, because we may have significant heating of the core barrel up above the core, in the vicinity of the core; and we have to concern ourselves with whether it's going to be impacted.

Then you also worry about things like crust failure. If you break your crust, can the UO_2 or can the melt start to stream. I won't go into code applications or the code implications, but let me just move on.

The next step is how do you approach the lower plenum? There is an awful lot of structure that you have to go through, and my personal opinion is that it would be very difficult to have a coherent drop of this melt into the lower head.

MR. CATTON: Not even via the anulus?

MR. COATES: I'm sorry.

MR. CATTON: Not even via the anulus?

MR. COATES: I'm not convinced yet, Ivan.

MR. CATTON: I'll keep trying.

MR. COATES: Okay.

MR. SHEWMON: The anulus is an end-run, is that --

MR. CATTON: Yes.

MR. COATES: It's an end-run. My comeback to that is --

MR. SHEWMON: It doesn't solidify on the way.

MR. CATTON: It might. But the anulus is fairly -- you know, it's six to eight inches.

CHAIRMAN KERR: Go ahead. Don't pay any attention to him.

MR. COATES: All right.

(Laughter.)

MR. COATES: Does that go for the rest of this talk?

CHAIRMAN KERR: Just the next three minutes.

MR. COATES: Okay. At any rate, you do have to contend with the failure now of the below core structure, the melt-water interactions that can occur in addition to just freezing, quenching, perhaps steam explosions of some degree which could be beneficial in a sense if they could spread the debris, and debris bed formation.

I hate to leave the impression that debris beds are only those things that result when you get down here, because debris beds are any state of the core that you have where it's all jumbled up and you're worried about cooling. So debris bed terminology applies to other parts of the accident sequence also.

Vaporization of the remaining water, attack on the pressure vessel, and then breach and an exit of the core materials perhaps on to the basemat.

I won't go into the various modes that we have conceptualized for vessel failure, but rather I'd like to just concentrate on the cooling aspects.

Now, the phenomenological uncertainties that we have --

MR. SHEWMON: Do you really think vessel failures are more likely upside than down bottom?

MR. COATES: That's one scheme. I can't answer the question in terms --

MR. SHEWMON: Because that's almost --

MR. CATTON: Same thing, it's just a different place.

MR. SHEWMON: Different place. Go ahead.

MR. COATES: There are reasons to think that that is a plausible failure point.

Okay. Now, in all of that there are obviously uncertainties, and what I have here is just a partial list. Some of the thing that are early are the mode in which the clad fails and how it relocates.

Now, we have information on clad attack and so forth, but we have very little information on relocation.

Fission product redistribution -- this may not be a terribly significant effect -- it may be a 10 or 20 percent effect -- but we need to look at it.

The behavior of the early melts that are formed, the eutectics, how they move, their behavior, properties -- we really need at this stage of the game to look at the failure modes of the internal structures and blockage effects; you know, how permeable blockages are that could form here.

Then melt motion, streaming, recreasing, and coherency. I will mention one thing in connection with March. It has three types of models that you utilize in addressing the remaining part of the sequence. One, which is called Model C, allows the fuel to melt and stream down to the bottom of the vessel upon formation. Models A and B more or less keep the melt in place. It's quite significant in the effect on the sequence, so you really do need to know whether you can stream materials or not.

Late in the accident you worry about stress formation, its strength, and how much structural integrity it has to hold the melt back, and in remelt. Again, steam explosions come up, and again, they could be beneficial in spreading the debris. Melt-water interactions, degree of fragmentation, dispersal, and so forth. And now the question of coolability, vessel failure modes and timing.

MR. CATTON: Before you leave this, Dick --

MR. COATES: Yes.

MR. CATTON: Melt motion, streaming, refreezing, and coherency -- there is a German program that's addressing that question. Do you think it's going to give you sufficient answers?

MR. COATES: I do not know. I can't say that it will.

MR. CATTON: Okay.

MR. SHEWMON: Do you have a schedule for looking at it, or the ability to look at it?

MR. COATES: Not an in-place -- oh, do you mean to look at the German program?

No, we have not scheduled such a thing, but it certainly is a good thought.

MR. SHEWMON: I hope you do. I mean, last time they were out to your place there was sometimes a flavor of not invented here, and I'm sure that was just our misperception. But you could do things to come to a melt motion.

Is this after it comes -- is that streaming after it comes out of the vessel, or is this between layers inside or both?

MR. COATES: No. I have not gotten out of the vessel.

MR. SHEWMON: Okay. So the streaming is redistribution within the vessel, okay.

MR. COATES: I will point out in connection in our defense, we are working very closely with KFK people. Most of our involvement, our own personal involvement to date has been with the people in the advanced reactor side. They are doing some modeling in debris beds which Ron Lipinski will talk about. We're just now getting into this end, and we have not had a chance to look at those things.

MR. KELBER: There is in the lightwater area a long-standing agreement for exchange of information, and since we are uni[illegible]ing our melt program, the German people have got to face this. And we have had a very effective exchange, principally under the direction of Rick Sherry, and yes, we have access to that information and will follow up on that.

MR. COATES: Okay. Now, hopefully not to insult your intelligence, I will put up a very simple viewgraph; because now what we want to do is talk about intervention, some action on the part of an automatic system or the operator, restoration automatically of AC power. How do we look at this problem?

Well, we have to reintroduce water to the core. You form a debris bed, no matter where you are in the core, assuming that you have gone to some sort of local melt, perhaps not even. And then the question becomes is the debris coolable, and we've got the very simple binary system to address, yes or no. We might be able to terminate the accident at that point.

Now, the uncertainties that we have upon the reintroduction of the water to the damaged core, we can look at it in various areas, and I'd like to talk about what we call the early stage and then the late stage. And the early stage is when we have not had a great deal of core degradation. The later stage is where the core has become more involved.

We worry about -- well, it's simply a problem in the early stage, if you've got this hot material here, what happens when water comes through. Thermal stresses and so forth will probably break it up into smaller particles. We worry about the location of the debris, whether it's in place, whether we've formed a rubble heat, or whether it has settled into some parts of the core that aren't yet damaged. It could form blockages or what have you. We need to know what the geometry of the debris is, what particle size we have, and what particle size distribution, what the shape of the particles is, and the void fraction, i.e., the packing. And we worry about other water entry effects, heat transfer and hydrodynamic effects. How does the water come in? If we are getting water back into the system, does it just come up and plug the core, or are we directing something in on the core? That has a difference.

In the late behavior obviously you need to know the type and the extent of the melt, the size of the melt zone, how much sensible heat this melt zone has before the water comes in, how uniform and how coherent is it, the location, whether blockages are present, because blockages below the melt preclude forced cooling; effects of water entry, the steam explosion, quench, fragmentation, dispersal, and then again, the question finally, the debris bed formation, its cooling, and then you have to address the questions if you can't cool it, how does it remelt and what happens from that point on.

So that pretty well summarizes, I think, the first part of the cooling problem -- what do we have to cool?

Mr. Chairman, that's all I have.

CHAIRMAN KERR: Questions?

Well, I think that's a good summary, and I found it interesting. I have a couple questions.

How are you going to get the information to answer the questions? And second, what are you going to do with the information once you get it?

Maybe the answer is you're going to write a report, and you don't know what people are going to do with it.

MR. COATES: That's true. Well, I don't know how you would get at all of these questions. There are some, I think, key questions that you can approach. The questions of what happens to debris when water is reintroduced or to a damaged core when water is reintroduced. There are some experiments that are currently in place at Sandia where -- the steam explosion work -- where simply you might change the mode of water entry into the melt, either by spray or by slow immersion, to answer some of the questions of what results when water returns to the system.

There are other experiments where you might utilize the large melt capability or a thermite capability to look at some aspects of core slumping onto grid plates, and to whether or not you can indeed have this coherent drop. And you can obviously answer some of the streaming questions.

So I think that you can answer several key things in a reasonably short time. It would go into a report, and hopefully it would be used in a good way.

CHAIRMAN KERR: Well, in planning your approach to this list of questions, to some extent, of course, you try to get answers to the questions that you think are amenable to investigation.

MR. COATES: That's right.

CHAIRMAN KERR: But you also attach a priority to questions, the answers to which would be helpful to people who are doing licensing and answering specific questions. What method is going to be used to select among those, or is that something that you don't worry about, and somebody else makes that decision for you?

MR. COATES: Well, the latter is probably more correct at this time, since programs are being formulated and ideas are being formulated. But I really would like to field that question to Charlie.

CHAIRMAN KERR: Well, if the answer is that you don't really participate in that --

MR. KELBER: We know once we have formulated rather more precisely than we have now our technical objectives, we have to address this in a collegial fashion with NRR, with PAS, as well as the RSR staff and the contractors. It's not going to be a unilateral decision.

I'm pretty smart, Bill, but I do like to get some advice from other people.

CHAIRMAN KERR: What process are you going to use?

MR. KELBER: Review groups.

CHAIRMAN KERR: What is --

MR. KELBER: The research review group process.

CHAIRMAN KERR: This is an ad hoc --

MR. KELBER: Once we get a program plan that's in reasonably good shape, we will have to form a regular research review group to go over this.

MR. SHEWMON: When you get that in place I would very much like to see it, because my experience in other parts of the forest with research review groups is it's where the people with positions like yourself get together with the principal contractors and they sort of chew over who's going to do what next; and I don't call that a review.

MR. KELBER: Not really. It's a --

MR. SHEWMON: I may be better informed as to what I'm talking about than you are. We both hope that yours is more of a review.

MR. KELBER: Well, we try to hold fairly dispassionate and critical -- in the best sense of the word "critical" -- reviews, independent reviews. We try to bring in people who have no ax to grind in the issue but who have expertise to bear. And it's going to be hard on this one; it's not going to be an easy process.

But I don't know of a better way, and I'm open to suggestions, as a matter of fact, if you think there are better ways. But I certai ly don't think that we're going to -- that we can afford to go through a process of sitting down and cutting up a pie. The pie isn't that big.

MR. SHEWMON: Well, I wouldn't mind -- well, I would urge you to get some people who aren't your contractors.

MR. KELBER: Yes, yes.

MR. SHEWMON: And that is too much what happens in some other places.

MR. KELBER: We have in fact had good cooperation from DOE in this respect.

MR. SHEWMON: The other thing is you could get some people from EPRI or some place else even if they do, you think, have a cause to push just to see what they have to say.

MR. KELBER: We did have EPRI at the fuel melt review, for example, and the review of the Zion/Indian Point report. We had both DOE and EPRI representation, which was very useful.

MR. SHEWMON: Somebody ought to be worrying about the most large experiment of this type done yet and how we get into it, and that has to do with TMI-2 which ought to be part of the fuel melt.

Do we hear about that today?

MR. KELBER: No. I have a backup slide if you want to hear about it.

MR. SILBERBERG: Dr. Shewmon, with regard to fission product transport or release based upon TMI-2 data, that is part of the radiologic source term work, that portion of it, but there are others.

MR. KELBER: And you know that Dr. Johnston in his discussion of their work, or I think it was Pickelseimer actually, referred to the TMI-2 investigation. I'm coordinating that for the Office of Research, and our latest information is that we hope to get a peek at the core late in fiscal '81 and actually get in and get at it, get samples in fiscal '82.

MR. SHEWMON: Are they still waiting to decide whether they'll vent radioactive gas?

MR. KELBER: I don't know what decisions were made this week. I suspect so.

CHAIRMAN KERR: Thank you, Mr. Coates.

MR. COATES: Certainly.

CHAIRMAN KERR: Steam explosion studies. Oh, I'm sorry. Another half of this.

MR. COATES: There was a second half to this one.

CHAIRMAN KERR: There was.

MR. COATES: Yes. It's Ron Lipinski.

CHAIRMAN KERR: Let's have it.

MR. COATES: Okay. Fine.

CHAIRMAN KERR: You just talked about half of fuel debris. This is the other half.

MR. COATES: Yes.

MR. SHEWMON: We aren't to steam explosions yet, or we are?

MR. KELBER: The next one.

XXXX MR. LIPINSKI: Ron Lipinski, Sandia Labs.

The possibility of cooling debris is quite exciting because it lends the possibility of terminating the accident at that point, so it's worth looking into. I want to talk about what we presently know about debris cooling, and maybe get into whether or not it's worthwhile to do any more research on the subject.

There are three, getting very simplistic here, three items of concern in cooling debris. One, you have to have a heat sink, some place for the heat to go to. Two, you have to have water in order to convey the heat from the debris to the sink. And those two items are essentially engineering type items.

Now, in some accidents they don't exist, so perhaps you can make engineering changes to increase the probability they do exist.

But the third item is getting the heat from the debris to the water before it goes to the heat sink, and that's more of a phenomenological item, and that's what we'll be talking about mostly.

The state of the debris, as we have seen, can be anywhere along the accident progression. It could be a solid state of rubblized fuel, damaged pins with dripping clads and stuff. It's also possible it could be a molten pool.

Now, inasmuch as we have to have water in order to get the heat to the sink and the water has a tendency to fragment molten fuel, I'd say the more likely case is going to be particles. Now, we can't just dismiss molten fuels; we have to talk about them later. But for this talk I would like to talk mostly about particle beds, lumping into that term the idea of any sort of a solid array of torturous paths in the fuel.

The debris is going to be assumed to be on an impermeable plate as might be if it were in the bottom of the vessel or perhaps on the bottom of a reactor cavity.

MR. CATTON: Ron, if you did have a molten pool would the water be as ineffectual as claimed for the basemat?

MR. LIPINSKI: Our best estimates say that if you had a molten pool and the water stayed on top of the molten pool without fragmenting it, the limiting heat transfer would seem to be within the bed itself by internal conduction; and that's a question which has to be addressed.

MR. CATTON: So you answer is sort of a qualified yes.

MR. LIPINSKI: Sort of a qualified yes, yes.

MR. CATTON: Go ahead.

MR. LIPINSKI: And the final thing is that we're going to consider boiling of the water as a means of getting the heat out of the bed.

Now, the question is what do we know at the present stage.

MR. SHEWMON: Can you explain what that last -- I mean, that almost seems so obvious, it does insult my intelligence, so you must have something else in mind.

MR. LIPINSKI: Of boiling water?

MR. SHEWMON: Yes.

MR. LIPINSKI: Well, you could conceivably have pumps on and consider only -- well, inasmuch as we've had an impermeable support plate being part of the assumption, it's kind of hard to consider any other way. With very cold water I'm sure you might consider conduction. But that's insulting, you're right. Okay.

Just looking basically at water dropping down through a bed and vaporizing and going back upwards, the question is what are the considerations on cooling these particles. Well, one limit is if these particles are quite big, you might make them so large that they start to melt from the center. That's a very easy conduction calculation, and you can find out that particles have to be less than 10 inches to avoid that, and that's not too hard to visualize.

The other limit, though, that comes in is that you're assuming that the liquid can penetrate into the bed in this process. If the bed is quite deep -- and indeed, in-vessel beds might be six, possibly nine feet deep -- it could be that the water would all vaporize before it reached the bottom.

Now, if that were the case, looking on a macroscopic scale, you would have some region where you had boiling, sort of a wet debris, but at the bottom you'd have a dry region where heat would have to be removed by conduction.

Now, a dry particle bed is not a very high conductivity material, like 1 degree Kelvin. And easy calculations of conduction indicate that you'd have fuel melt with only like a three and a half inch or so dry region. And with a fuel melt you'd have high jumper curves(?) and attach the lower structure of the reactor cavity.

So you try to avoid dryout, and that's why we have been investigating dryout in our particle bed investigation.

So it's known that as you have smaller particles, it's harder for the liquid to flow through; it goes slower so it evaporates faster. So obviously whereas you have a limit that if the particles are too small, you achieve dryout. So you are now struck between two limits: one, you want your particles to be small enough so it doesn't melt inside the particle, but big enough so that the bed itself doesn't dryout. The principal problem is making the particles big enough, as indicated by some fragmentation studies.

MR. SHEWMON: If you've got liquid, you get these things called rayleigh cells or whatever you call them; you know, you can put dust on top of a flat pan of oil and it makes pretty pictures.

Is this well enough understood so that you can say there are similar two-dimensional or three-dimensional instabilities, or do you treat this in the total absence of this because you don't know any better, or because you know those are absent and these are --

MR. LIPINSKI: Normally I believe the rayleigh cells apply to sub-cooled, non-boiling single phase heat removal.

MR. SHEWMON: That's true, but I only bring it up as the only sign of a macroscopic instability I know of, and if I look at rice, that there the heat flow is again different, but some channels are where steam comes out, and other places are where water goes down or something. And there are various things going on at once, so they may go on in different places I guess is my main point. And to what extent is that phenomenon understood in a bed like this?

MR. LIPINSKI: Okay. The phenomenon of channeling is one example. Experimentally it appears to be limited to like a 5 to 10 centimeter type of a bed, and if we're talking between 1 meter for ex-vessel beds and 2 to 3, channeling is probably not significant.

The other part, the business of rayleigh cells, with boiling going on, I don't know. I have never seen any reports that indicate that that is what's going on, although there have been very few investigations that would look for such a thing.

MR. CATTON: My guess is that they probably do. Any time you have something that's buoyancy-driven, it tends to do that. But I'm not sure how effective that would be. The primary cooling, I think, is going to be due to just the phase change, and then the bubbles are going to get out. The liquid flow really won't contribute a whole lot.

MR. SHEWMON: I guess it's more a matter of where it flows to, but go ahead.

MR. CATTON: That's right.

MR. LIPINSKI: There have been quite a few experiments performed on dryout itself, starting off at UCLA several years ago and continuing with Dr. Kapman and his associates, and Argonne National Lab with Gabriel and Baker, Sandia Labs, of course. The atomic energy establishment at Winfurth in England has come up with some recent measurements, and they've also done some modeling indicating that capillary forces are important. Verlie over at KFK in Germany is doing some ex-core experiments with freon. There have been other experiments coming up from I believe it's Wisconsin with Abdul Kalik, also sponsored by KFK. and there have also been some down at Cornell with bottom heating as opposed to volume heating.

Now, the problem is that all of these experiments have done with beds typically around 10 centimeters high and particle diameters less than a millimeter, and now we're confronted with the question can we extrapolate this to a meter, to two or three meter high beds.

MR. SHEWMON: Why is it we --

MR. LIPINSKI: Oh, I'm sorry.

MR. SHEWMON: -- We wait to make everything uniform, and so if we're going to fragment any of it, we have to assume we fragment all of it. Is that why you end up with sort of a material balance that gets you to a two meter deep bed?

MR. LIPINSKI: Yes. No, it's not a material balance so much as maybe possibly a conservative assumption. We assume that all of the core is in the particle state in order to say that --

MR. SHEWMON: And that's even the outer rim which is most likely not going to melt under most any conditions you want to talk about.

MR. LIPINSKI: Right. For in-vessel cases you take -- this is kind of a two-sided picture here -- if you take all the UO_2 and an equal amount by volume of structure, it would probably come up to about this high if it were down at the bottom like this, or if it were sitting on top of a grid plate, it would come about so high, this length or this length being three meters.

MR. SHEWMON: Go ahead. It's a good place to start. I just --

MR. LIPINSKI: Okay.

MR. SHEWMON: We get locked into these models after a while because it's the only place we can look.

MR. LIPINSKI: Right. Well, even if you cut that in half, it would still be like one and a half meters which is significant. And it leads to the question of how can you possibly extrapolate from such --

MR. CATTON: I think more importantly if the edges are not part of the rubble bed, you have a wicking, so you can essentially feed water into the bottom of the bed and boil it up like with a bubble pump.

MR. LIPINSKI: Okay.

MR. CATTON: That would be a far more effective means of cooling than you have with the water having to percolate down through the top.

MR. LIPINSKI: Okay. Let me address --

MR. CATTON: I think that's what you were driving at, wasn't it?

MR. LIPINSKI: Good. I will address that question after we go through all the modeling here, because we have looked at that.

Well, it's pretty simple to establish a set of equations for conservation of mass, momentum and energy involving both the liquid phase going down and the vapor phase going up, using

1950, 1929 data for flow-through particle beds. And let me just display these equations.

There has been some progress since I last addressed the ACRS in December on such a model. If you look at the vapor and the liquid momentum equation, we have added a term for the possibility of turburlence when you have large particles. And this is an important effect in these large beds with the high heat fluxes that we're considering. And also as suggested by Shires and Stevens in Great Britain, we've added the capillary pressure for small particles. That's not important for this particular issue.

You can solve these equations in closed form and get a solution for dryout, and it's instructive to display the solution in the following manner. I apologize if the people at the back can't see. The bottom says "particle diameter" and it's on a log scale of one millimeter in the center. And the vertical axis is the dryout flux, the heat flux coming out of the top at the point of dryout.

Now, the present model for like a one meter deep bed is the green line that merges with the red, and for a 10 centimeter high bed it's similar. The important thing is that we see how this compares to previous models which we may have used for previous expectations or estimations of bed dryout.

Previous models had indicated that the heat flux would vary with the square of the particle diameter, hence you have a slope of two there. But the new model, because of the turbulence, suggests that it's the square root of the particle diameter that changes with the heat flux for larger-diameter particles.

Now, this has an important effect in that the heat fluxes we expect for the PWR accident cases are typically between two and four megawatts per meter squared, so there's a discretion between the two possible models here which might be on the order of five times or so.

So whether or not this model is true is of some concern to people making decisions on whether or not particle beds are coolable. Unfortunately, all of the available data at the moment is right in the region where all the models kind of cross; and that's because the models were derived after the data was gotten. So what we need is a few key data points out here at larger diameters to see which is true.

Now, if I were to make my best judgment, it would be this one here because it does include the possibility of turbulence, and the other models in the LMFBR simplification were ignoring turbulence because of the expected small diameter.

MR. SHEWMON: Two to four is right away after you put the rods in, or one hour, or one day or --

MR. LIPINSKI: Okay. If you take all the debris and kind of let it drop down without spreading it out much, you get two at about five hours and about four at about a half of an hour afterwards roughly, so that's in that span.

MR. CATTON: How did you get your coefficients for the turbulence?

MR. LIPINSKI: Okay. The relation between turbulence and diameter came from the Ergon equation which was based on a lot of experimental work for core melt.

MR. CATTON: Okay. That's enough.

MR. LIPINSKI: Okay. But the final coefficient for the dryout was done through an optimization procedure of minimizing the -- maximizing the heat under the different viscosity tradeoffs between liquid and vapor.

Now, I do want to point out one point, in that this model, although it is phenomenologically based and all that and has some experimental input from a coefficient, is not absolutely accurate, okay. I'm plotting the predicted dryout flux versus the measured dryout flux for 125 different data points for dryout and so forth. And you'd expect it all on a straight line for a perfect model, and as you can see, it doesn't; it's off by a factor of two.

The other models are even worse, and this indicates that either the model is not getting all the phenomena that are involved, as Dr. Shewmon suggested, or perhaps some of the early work, because it wasn't aware, may have had some scatter to it. So we now have two areas of uncertainty: one, whether the model itself works at large particle diameters; and two, whether or not the model has all the phenomena or perhaps just the data are just scattered.

MR. CATTON: I can speak for some of the data. It's data scatter.

MR. LIPINSKI: By factors of two?

MR. CATTON: Sure.

MR. LIPINSKI: So --

MR. CATTON: It's a tough kind of experiment.

MR. LIPINSKI: It certainly is, right. Some of the more recent data is falling closer in line. The stuff from Germany which was sent over to us is this and this, and that's kind of closer in line. You know, they're spending more time.

MR. CATTON: Freon behaves better than water.

CHAIRMAN KERR: That Hungarian data looks rather good.

MR. LIPINSKI: Pardon?

(Laughter.)

MR. LIPINSKI: Okay. So let's apply this knowledge now to a case, and actually let's go right to the ex-vessel case since that is the bottom line, and we're running short on time here.

Okay. I'm just going to list the assumptions that you have. All of UO_2 is in the particle bed, and you have equal amounts by volume, and you have a uniformly mixed bed, and the bed is sitting in the bottom of the cavity and extends out just below the vessel itself; it doesn't push down the cavity. And just as a test case. And the water is at saturation temperature. If it were sub-cooled, you'd maybe a 20 percent increase there.

The void fraction is .4 in this assumption here, which is what you kind of get when you actually take a distribution of particles, pour it into something, and you measure what the void fraction is. But it may be on the low side; I don't know.

And the problem is we're not too sure what the effective particle diameter is, so we're going to phrase our question, what diameter will give you dryout, and then we'll ask what diameter do we expect.

MR. CATTON: How deep was this bed?

MR. LIPINSKI: This bed turns out to be in the cavity about 1.2 meters high under these assumptions.

MR. SHEWMON: You're describing an experiment or calculations?

MR. LIPINSKI: I'm describing a calculation here.

MR. CATTON: If you spread the bed out through the total area --

MR. LIPINSKI: Then it drops down to .8 meters.

MR. CATTON: Okay.

MR. LIPINSKI: But you have to have a mechanism for doing that.

MR. CATTON: Boiling will do that for the smaller particles.

MR. LIPINSKI: Possibly vigorous boiling or explosions might do that. I'm not too sure whether just steady state boiling would do that.

MR. CATTON: For small beds there's some data from Argonne that seems to indicate that the boiling process, even though not violent, will spread it.

CHAIRMAN KERR: We will investigate that.

MR. LIPINSKI: That would be nice if it did, because we have a factor two that you get there.

This is the bottom line for this case. For the large break loca which happens like 20 minutes after the scram, you would need particle diameters between 2 and 5 millimeters as an average. And this is a pressure effect here. The containment is pressurized which kind of helps because your vapor is denser, it can get cleaned out quicker or easier. If you're at one atmosphere of 85 millimeters and like five hours afterwards your loss of AC power, it's like 1 to 2 millimeters.

Okay. So that's what we're after is those particle diameters. The question is what do we expect?

CHAIRMAN KERR: How seriously do you take those results?

MR. LIPINSKI: Well, we have at least a factor of two uncertainty from the scatter and the model itself, so there is a first basis. And it can go either way. So at least a factor of two.

MR. SHEWMON: You have to have a fair spread in the distribution of the junk that ends up down there under those conditions, too. I don't know what that does to you, but it --

MR. LIPINSKI: You're talking about distribution of particle size?

MR. SHEWMON: Yes.

MR. LIPINSKI: Right.

MR. SHEWMON: Or shape, or --

MR. LIPINSKI: Right.

MR. SHEWMON: -- Homogeneity or --

MR. LIPINSKI: The attempt to handle the particle size distribution is normally handled with this equation which was originated numbers of years back and reported by Barrow. You just kind of take the weight, and you inverse it, and you sum it, and you inverse that. And it seems to work fairly well. It was brought into the limelight by LaRigolero from France last year.

CHAIRMAN KERR: I'm sorry. What is the meaning of the statement that it seems to work very well? You mean if you put it in a computer --

MR. LIPINSKI: Empirically.

CHAIRMAN KERR: -- You get something out?

MR. LIPINSKI: I mean, if you do it experimentally for fluid flow through a porous material, it seems to work for the pressure drop, it turns out; and if you try to do a dryout experiment, there is evidence that it does come in there, too, from UCLA. So that's another slight uncertainty, too, is whether or not this is accurate, but at least we're getting some numbers.

The question now is what fragment size do we expect, and this, I think, is where the largest uncertainty exists. And this is kind of -- again, I apologize. The bottom says the "weight percent," that is, less than a given particle diameter D, and the vertical axis is the diameter.

What we're plotting here is the results from an experiment where 13 or 14 kilograms of alumina oxide, not UO_2, was dropped in the water and fragmentation occurred.

Now, out of 48 tests -- this was done at Sandia -- 37 of them kind of were explosive, and 11 of them were not explosive. The explosive ones kind of ended up with a smaller diameter, or this bunch of lines, and the non-explosive ended up with much larger. The average diameter, according to this definition, ended up to be like 200 microns, .2 millimeters for explosions. For non-explosions it ended up like two. So we have a factor of 10 difference between explosion and non-explosion. However, this is with only 13 kilograms.

The first indication of this, and I would have to say that this suggests that particle beds are not coolable if they involve the entire core, ex-vessel at, you know, like one atmosphere in the reactor building. And that sounds like bad news.

The only hope I can hand out at this time is that if you look at the progression of data, when very small one gram samples were dropped into water, hit with the trigger and fragmented, the average size was like 50 microns, way down here. With these larger melts like 13 kilograms, the average size was like 200; it's getting bigger.

Now, if you're trying to be optimistic, you say well, perhaps if you have large amounts of fuel -- and we're talking about total core here in order to make these heat fluxes as large as they are -- maybe you will have a larger average diameter. At present we can only say maybe because there's no experimental justification. So that's one possible hope.

The other possible hope, too, is that was very early data. A lot of the stuff has blown out the top, so this may shift one way or the other depending on --

MR. SHEWMON: If you only had half the core down there would it be any better?

MR. LIPINSKI: If you only had half the core, it would certainly be better; and the diameter varies at this point with the square of the amount of the core down there. So if you have half the core, you need one-fourth the size diameter, and that gets you down to one millimeter or less.

MR. SHEWMON: Nobody can assume that you're unconservative if you put the whole core down there, but it seems to me given the power distribution and any ideas on cooling, the chances of the center coming out and the rest staying or plating or sticking some place is approaching unity.

MR. LIPINSKI: Right. You know, the hard part is justifying it strictly.

Okay. So let me just get to a bottom line then at this point. And that is, what do we find from all this? We find that we're close, that at the present time we have to say that particle beds are not shown to be coolable. However, there is a possibility that they are, and it might be worth fighting for.

What we have to do to resolve this question, what is needed to resolve the question, the biggest item is the fragmentation size because that can vary by factors of ten. And I guess you'd have to go to large scale --

CHAIRMAN KERR: Suppose it did vary by a fraction of ten. Then what?

MR. LIPINSKI: Okay. If right now we had the particle size average as .2 millimeters, a factor of 10 gets us up to 2 millimeters, and 2 millimeters is right underneath the noise level of whether or not particle beds are coolable.

MR. CATTON: For the whole core.

MR. LIPINSKI: For the whole core, yes. That may be defendable, so that's what the question is. It's getting tantalizingly close, but we're not there yet at all, strictly.

The other item is high pressure. This applies to in-vessel cooling which I didn't show you the numbers for. It requires significantly larger particle diameters, but on the other hand, if you have high pressure the question is would you get explosive fragmentation. You might get, as we've seen when you have non-explosions you get bigger particles; you might get systematically bigger particles. So the fragmentation with large scale and possibly high pressure is one degree of freedom that has a potential payoff.

The bed structure itself, whether or not there are voids involved, sodium UO_2 experiments suggest that you do get up to like a 50 percent void fraction; but it's hard to justify for large-scale interactions.

Stratification unfortunately is a bad issue. If the beds fall through water and the heavy particles go down to the bottom with light on the top, you'll have a tendency to have poor heat removal capability because the small particles on top restrict the flow down and back up again.

If stratification does exist, we have to find out about it so that we're not incorrectly assuming the beds are coolable.

Large particles, the difference between the two models seems phemonenologically reasonable, but it hasn't been verified at all. And large bed heights, the difference between 10 centimeters and one meter, may have some unknown phenomena that will have to be looked into.

And again, high pressures, when you're talking in-core, in-vessel cooling, for example, loss of AC power, you might have high pressure which because of the denser vapor has the potential for payoff. And this has not been at all verified, the pressure effect on the model. So far it's just phenomenological guesses.

So that's basically where we stand.

CHAIRMAN KERR: Questions?

MR. LIPINSKI: Shall we take time to answer the one question about the wicking?

CHAIRMAN KERR: Do you want the question about wicking answered? No.

MR. LIPINSKI: It won't take more than two minutes.

I've looked at it. I've made the calculation. In the very deep bed limit the effect of wicking if the bubbles condense up here is that it is a very small effect. At high pressure maybe it's a 30 percent effect. At low pressure it's even less than that.

The problem is that the pressure gradient is established by the height of the bed where the bubbles are. The resistance is determined by the vapor volume which doesn't matter much whether the liquid comes from the top or the bottom because the liquid is so small compared to the vapor. That was unfortunate, but that's what the numbers say.

If the bubbles remain bubbles all the way to the top, then you have a higher effective hydrostatic head, and maybe you get a factor of two out of it, roughly.

MR. CATTON: There were some diameter assumptions in that, weren't there?

MR. LIPINSKI: Diameter of what?

MR. CATTON: Diameter of the debris bed distance from say the center of the bed to the edge where the downflow is occurring?

MR. LIPINSKI: No. It was assumed a basically one-dimensional model in which the liquid was able to get over here without any problem at all, and then it went up one dimension from that point.

Now, if you're talking about liquid coming in here and entering this way --

MR. CATTON: Well, that's what I was referring to.

MR. LIPINSKI: Okay. Oh, I'm sorry. That's a possibility that hasn't been investigated.

MR. CATTON: I was following up on Dr. Shewmon's observation that the edges of the core probably would not be destroyed.

MR. LIPINSKI: Okay. Side entry is something I haven't looked at.

MR. CATTON: So you come down through an intact fuel bundle, and all the way down you would be feeding into the debris bed.

If the flow resistance is that high, I'm not sure that would do any good either.

CHAIRMAN KERR: More questions or comments?

Thank you, sir.

I declare a ten-minute recess. We will reconvene at about 19 of.

(Brief recess.)

(Whereupon, the meeting began before the Reporter returned.)

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MR. CORRADINI: By doing this you generate a large amount of steam at high pressure, a.d the whole concern with steam explosions, although it's a very interesting academic pursuit, is in terms of the reactor safety implication -- can this steam do you harm in terms of expanding in any type of reactor system and doing you harm?

MR. SHEWMON: How do you know the particles break up?

MR. CORRADINI: How do I know? From the post-test examination of our experiments. When we run the experiment you can see the molten material come in at some size, break down prior to the explosion to some smaller size.

MR. SHEWMON: I see. These are molten; they're not solid.

MR. CORRADINI: Right. But after the explosion they quickly solidify, and then from post-tests you can compare the relative sizes. So after the explosion, as Ron mentioned in his viewgraph -- before the explosion you have sizes of the order of around a centimeter; after the explosion, the size of the particles, the fragmentation of when they've solidified is on the order of anywhere from 100 to 1,000 microns, so anywhere from a factor of 100 to a factor of 10 smaller, okay.

So in any case this is just a qualitative picture, and I just only want to reference you that the important thing in terms of reactor safety is how can this rapid production of steam cause you damage in the reactor. So I'd like to talk about it in the four areas; that is, fuel coolant mixing, triggering, propagation, and containment failure. And I'd like to dwell on containment failure probability and just try to rapidly move through the first three.

In terms of fuel coolant mixing, right now our current conclusions are that water will probably be in the lower plenum at the time when a large portion of the core is molten, okay. And from the experiments that we have done in many different geometries with essentially an intermediate scale, we find that using a simulant -- that is molten iron aluminum oxide -- we find that this mixing process when the molten material falls in and mixes with the water occurs quite rapidly, on the order of about 100 milliseconds, and you see the coarse mixture of approximately a size around 1 to 2 centimeters as an upper bound.

Now, the important uncertainties at this point, and I think most of the people, Dick, in particular, Dick Coates, in particular, from Sandia mentioned it before, is that given this fact, there are a lot of uncertainties in the core meltdown progression on how we go from state A to state B. Those being, for instance, how does the material move prior to fuel coolant contact; that is, how does the meltdown progress? What is the mode of lower core grid plate failure, the vessel geometry at the time of failure, the effect of scale on mixing, and the behavior of real reactor materials versus our simulant. And what I mean by real reactor materials is coriums A and E compared to the simulants.

Now, at this time our current research is trying to get a few things, and I'll simply mention them, and if you have questions, we can dwell on them a little bit more. But the current research is trying to begin some type of phenomenological modeling of the core meltdown process, analyze possible modes of grid plate failure, both structurally and internally and continue what we call the fully instrumented test series, FITS, in terms of the steam explosion experiment. And then we're trying in the longterm to go up to larger scales, perhaps as high as 100 kilograms of molten material, and introduce it to water, and develop mixing models from that.

Now, in the second area of triggering and propagation, I'd just like to take these in tandem.

MR. CATTON: Are you going to make some attempt to vary the ratio of the drop to the water?

MR. CORRADINI: I'm sorry.

MR. CATTON: Water mass to molten material mass, the ratio?

MR. CORRADINI: Yes, we'd like to do that. Right now our ratios are approximately running, approximately like 20 to 40. We'd like to decrease that down.

MR. CATTON: Twenty times as much mass of water?

MR. CORRADINI: Yes. It's a very water-rich situation; that is, you have a very large tank of water.

MR. CATTON: I understand.

MR. CORRADINI: Okay. So in explosion triggering our current conclusions are that right now we can trigger vigorous explosions both in small-scale -- what I mean by small is approximately a tenth of a gram to a few grams -- and intermediate scale, intermediate scale being approximately 1 to 20 kilograms of material.

These explosions are spontaneous, and we note that the explosion intensity, if you want to measure it by some figure of merit, the conversion ratio, and what we call that is the work of the explosion divided by the thermal energy content of the melt, is very dependent on the initial conditions. I've just listed these here.

And at this point we have simple models which can explain some of the physical macroscopic variables which we measure and observe during the experiment, but at this point they are simple models.

MR. SHEWMON: I guess I don't understand what you mean by spontaneous and triggered.

MR. CORRADINI: Okay. What I mean by that is, spontaneous is kind of a word to hide your ignorance. All experiments when you throw the molten material into the water and you get the explosion, you get some type of -- and this is the physical basis -- some type of local liquid, liquid contact between the hot and the cold material which then propagates the rapid heat transfer spatially and temporally through the rest of the material, okay.

Now, when I say by spontaneous I mean it happens, but we as experiments are too ignorant to know what caused it.

MR. SHEWMON: I have some familiarity with the use of the word in thermodynamics; let's leave that. But the question is do you separately trigger, or does it spontaneously trigger, does it self-trigger?

MR. CORRADINI: It does self-trigger.

MR. SHEWMON: Okay. Thank you.

MR. CORRADINI: What we try to do is artificially do it just for experimental purposes to time for data acquisition, if you see what I mean. In other words --

MR. SHEWMON: You say you do trigger, but it will trigger, and you're sure you get the same results whether you triggered it or it triggered itself.

MR. CORRADINI: Yes.

MR. SHEWMON: Okay.

MR. CORRADINI: In terms of work output, yes, okay.

Let's see. Now, in terms of the propagation, and what I mean by propagation is the rapid fragmentation spatially and temporally in time. The explosion conversion ratio, which I defined as the work output divided by the thermal energy content, is a function of scale. In small-scale experiments we see conversion ratios, anything from no explosion to 20 percent. And I should just mention the thermodynamic maximum of the conversion ratio -- that is, the isotropic work divided by the thermal energy content -- is approximately 30 percent.

In intermediate scale experiments, though, we see an order of magnitude reduction anywhere from 0 to 2 percent. And the rest here we just see that we do measure some of the macroscopic variables, the propagation velocity and very high peak pressures from the explosion.

MR. SHEWMON: Do you know why the intermediate scale tests give you less?

MR. CORRADINI: We have ideas, if you'd like to hear them.

MR. SHEWMON: I was wondering whether you understood the phenomena well enough to be able to scale it up to a 50 or 500 kilogram mass.

MR. CORRADINI: No, to be dead honest with you. We have ideas, but I wouldn't --

MR. SHEWMON: You know, drop two more orders of magnitude, and it'd be news.

MR. CORRADINI: I don't think it will. I think it's quite sensitive to -- well, in any case I think it will drop, but I don't know two orders of magnitude.

MR. SEALE: What ambient pressures are these done at?

MR. CORRADINI: These, the small-scale experiments were done -- well, all these experiments that I'm quoting here were done at ambient pressure, slightly above ambient pressure. We are in the process of doing high ambient pressure tests both at small and intermediate scale. I think you'll see something in the next viewgraph which will get, I think, to the question you're making.

Right now is the uncertainties in triggering propagation, and the major uncertainties are, if you remember the first viewgraph, if the initial conditions play a role in determining the conversion ratio. And I think what Dr. Seale was mentioning is one of the things that can reduce the conversion ratio of the interaction is the ambient pressure, as the other ones here that I've mentioned.

At this time we're in the process -- and I'll just skip down to the next viewgraph -- at this time we're in the process of doing experiments, both small and intermediate scale, by raising the ambient pressure. There are two theories as to why the ambient pressure reduces or eliminates the explosion, and what we'd like to do is experimentally with reactor materials or reactor simulants try to understand which of the two theories is correct.

And now in terms of containment failure, which is what I wanted to dwell on a little bit more today, if I could, what we have are, first of all, to talk about containment failure you have to now link the explosion, which is an event that occurs in many industries, okay, to the core meltdown scenario.

Here there are two possible modes of containment failure, the in-vessel and ex-vessel steam explosion. First of all, missile generation. This is what was assumed in WASH-1400; that is, the steam explosion created the reactor vessel head as a missile and projected it up to containment, generating a hole. So you have missile generation by the expansion of a fuel coolant mixture. And secondly, you may generate some type of leakage through containment penetrations caused by gross motion of the system or the components of the system or surrounding structure given the steam explosion event. That is, it doesn't fail containment directly but causes some type of gross motion.

Now, the conclusions that we're drawing here are only applicable to Zion and Indian Point and similar containments; that is, large PWR dry containments.

The interim conclusions are at this time that it is unlikely that large mass missiles will be generated. What we mean by large mass missiles is the missiles that were assumed of the same size range as in WASH-1400; that is, missiles of the order of the reactor vessel head. And the reason for this, we feel, is that we looked at various loading conditions, five different loading conditions inside the vessel, using somewhat conservative assumptions about structural -- fluid-structure interaction. And we came to the conclusion that with this fluid-structural analysis you find that vessel head failure first occurs at the top of the head rather than at the sides, which was assumed in WASH-1400. So because of the difference of location, instead of generating a large mass missile which would simply rip and be sent up, rather you generate a local failure which would cause the material to be ejected through the hole but not necessarily generate a large mass missile, okay.

Now, we can discuss this later if you have questions, but I just want to move on.

In terms of small mass missiles, our conclusion was that small mass missiles could be generated by the explosion. What we mean by that is missiles of the order of like control of our drive mechanism from the impact of the fuel-coolant mixture inside the reactor vessel head hitting the top of the head could throw or control our drive mechanism.

Now, slug impact could eject a small missile. In terms of bounding the velocity of the missile, the bounds were, as you see here, somewhere on the order of 40 to 400 meters a second. The missile, though, I should maintain has to penetrate not only the containment but before that must penetrate the missile shield which is always installed in PWR's above the reactor vessel.

In looking at this we found that low velocity missiles could not penetrate the missile shield, and as the velocity increases, the depth of penetration increases. However, the missile becomes destroyed as it tries to penetrate the full missile shield. So the conclusion we reached was that although small mass missiles are generated, they don't appear to threaten containment because they either do not penetrate or are destroyed in trying to go through the missile shield.

Now, in terms of current uncertainties in containment failure probability, what we've come to is that we'd like to show the reliability of our current analysis in terms of missile generation and damage potential, and then we'd like to look at different containment systems. Right now we just looked at the large, dry, pressurized water reactor containment. There is Mark I, Mark II, BWR's, and the ice condenser plants.

Secondly, although we've identified it, we haven't addressed it technically, is how does the gross motion caused by the steam explosion compare to other gross motions which are designed for in the plant, that is, seismic events and design basis accidents.

And third -- or I'm sorry -- and fourth, what I haven't dwelled upon very much is that in the analysis that was done, because it had to be done over a short time period, we could not take into account a lot of the mitigative effects that we know to be there in the reactor vessel or outside the reactor vessel; and some of these I just list here. That is, you know you're going to have some upper internal structure which is not going to be melted out, but rather will contribute to breaking up the fuel-coolant mixture before impact on the reactor vessel head and can mitigate the generation of missiles.

Secondly, you have the lower plenum of the vessel where most likely in-vessel you'll get the explosion seems to be the weak link, at least for static pressure loads; and you have the possibility of failing it before the explosion does any damage to the upper internals to cause a missile. And this may simply be a relief path, a very benign relief path for the explosion, and therefore even reduce further the possibility of missile generation and containment breach. And then finally you have obstructing objects in the containment.

Now, the one thing that did bother us is if you take, which has been done in the past for final safety analysis reports for missiles, if you take simple empirical correlations developed from weapons-related work and apply it here, you predict that the missiles may penetrate the missile shield.

And what we did in this analysis is rather than use the empirical correlations, which we feel to be conservative -- and what we mean by conservative is that they predict penetration more than you would actually get -- we used a code calculation based on CSQ, which is a two-dimensional hydro-code at Sandia, to look at missile penetration of the missile shield. And our conclusions were based on the code calculations.

The reason we feel that the code is somewhat reasonable is that right now at Sandia large-scale missile turbine tests are being performed for EPRI where a large missile is being thrown at full-scale at simply a reactor containment building wall, and the CSQ code is being used to calculate or try to predict the effects of the experiment, and it's doing a fairly decent job of that. So we think it's a good tool.

And then finally just to end off in terms of current research, right now we'd like to in the future evaluate the possibility of leaking and containment piping penetration through the gross motion both for in-vessel and ex-vessel explosions. We would like to refine the missile generation analysis and refine it in terms of the mitigation features, because we know in our analysis at this point we are on the conservative side, to some extent because we have neglected some of the mitigation effects.

And finally, in terms of being prudent for our technical base we'd like to go to some type of scaled fluid structure experiment to ensure that our analysis, because it is not at this point supported by experiments, is indeed accurate.

So I'll stop here, and if there are any other questions about specifics, we can go into it.

MR. SHEWMON: Fluid structure means whether the steam explosion would --

MR. CORRADINI: Actually eject a missile or how it would --

MR. SHEWMON: I hope your priorities run in that order.

MR. CORRADINI: I'm sorry?

MR. SHEWMON: I hope that since it's at the bottom of your list it's your lowest priority.

MR. CORRADINI: If you want to take it that way, that's fine, but that's the way we view it, yes.

Any other questions?

MR. CATTON: Yes. What's your view to the conclusions expressed yesterday that it's time to redirect our resources with respect to work in the steam explosion area?

MR. SHEWMON: Towards what?

MR. CATTON: Away from steam explosions. The feeling was that possibly enough work had been done.

MR. CORRADINI: I'm going to be diplomatic here. I'll give you my opinion and then I'll bow to anybody else that would like, in terms of programmatic.

I think at this point we have a feeling based on the analysis that steam explosions are unlikely to threaten containment. Now, "unlikely" is more of a qualified term, so I think more work is being done.

In the future we're going to direct more of our work towards -- in cooperation with the debris bed work. And I think -- it hasn't been mentioned, but it already been in the process -- we've done some enclosed steam explosion experiments in the FITS facility where we're in the process right now of doing debris analysis, so we can interact with Ron and Dick in terms of the average particle sizes you get with or without a steam explosion.

So we're going in that direction. I don't know how fast you would like to see us go versus how fast we would like to see this go, so there may be some disagreement about speed.

MR. SHEWMON: Steam explosion is not a matter of developing enough pressure totally to rupture the containment --

MR. CORRADINI: No, no.

MR. SHEWMON: It's only a matter of whether you can get a missile.

MR. CORRADINI: That's right. The reason that --

MR. SHEWMON: Okay.

MR. CORRADINI: -- The first one is physically impossible is simply because of the size of the explosion versus the size of containment.

MR. SHEWMON: I can't see really where it must have a very high priority for injuring the public, I guess.

CHAIRMAN KERR: Mr. Lee.

MR. LEE: Is it your understanding that the phenomenon of thermal explosion or vapor explosion or whatever you want to call it, rather than limiting ourselves to steam explosions -- for example, if you have a sodium and molten uranium dioxide system, is the magnitude of the fission release and so on different compared to the molten fuel system?

MR. CORRADINI: Based on experiments I think the answer would be yes. Where's Dick? I don't know where he went. But experiments have been done at Sandia under, I think, Dick's direction for sodium and molten UO_2 of stainless steel mixes. And the experimental evidence appears that they are much less energetic than the molten metal or molten fuel-water experiments.

MR. LEE: Did the model or some of the microscopic pictures that you have developed, would that apply to the sodium-molten fuel interaction?

MR. CORRADINI: In my opinion, no, not at all, because physically you're in a different temperature regime and a different fluid flow regime. So I'd be very hesitant to transfer the model directly.

MR. LEE: Would you have some comments on interpolating from the simulant missile situation to actual fuel steam situations?

MR. CORRADINI: What simulants are we speaking of now?

MR. LEE: I thought you were using some simulants.

MR. CORRADINI: That's right. The reason that I would put more faith in terms of these simulants versus what you just described in terms of fuel and sodium is that fuel and sodium, you're in a different temperature regime in terms of interface temperature, in terms of boiling point. Sodium is a much less volatile fluid than water given a constant temperature, mass and composition for the fuel, whereas here what we've changed in terms of a very quick and dirty change for the iron-aluminum oxide simulant is we have the same, essentially the same metallic compound which is molten iron, and we've changed the oxidic component from a higher melting point oxide, the UO-zirconium mixture, to the aluminum oxide.

Now, that does have some bearing, okay, but I don't think a drastic jump in what the fuel material is, because in small-scale experiments we have seen with corium A -- I'm sorry -- with corium E or with iron-alumina, the same character of the explosion -- that is, the same debris, same general pressure-producing behavior. So I don't think it's a bad simulant or a simulant that's very off the mark.

MR. LEE: Okay. I have a couple more questions perhaps. In one of your viewgraphs in the list of studies you'd like to perform you had listed trigger magnitude.

MR. CORRADINI: Yes.

MR. LEE: Can you comment on that? I thought in answer to Dr. Shewmon's question you mentioned that the triggered cases essentially give you the same results as spontaneous cases.

MR. CORRADINI: Yes.

MR. LEE: From which I sort of deduced that trigger magnitude did not play a role.

MR. CORRADINI: Yes, but let me just turn the question around, not to delay it a little bit. Then you have to ask yourself well, what was the spontaneous trigger? Okay. And since we don't know that, we can postulate that spontaneous trigger could have been any type of random event which causes a very large pressure pulse over a very short timespan. Since we can't characterize the spontaneous trigger, the more logical thing to do is go back and look at the artificially applied trigger over a variety of magnitudes to understand what had to be in the spontaneous trigger to give us the explosion

See what I'm saying?

MR. LEE: Yes.

MR. CORRADINI: Okay. So that's the reason we're going for the artificially triggered experiments and varying the trigger magnitude, so we understand what's needed.

MR. LEE: So they indeed show sensitivity. Do the results show sensitivity to the trigger magnitude?

MR. CORRADINI: Oh, yes. Oh, yes, yes. There are some preliminary experiments we're doing right now on small scale that show that if you reduce the trigger magnitude below some threshold and suppress the spontaneous interaction, that you cannot -- you will not get the interaction if your trigger is too small.

Now, how small is small? I would say less than 10 bars.

MR. LEE: So do I somehow or can I somehow conclude that we still don't understand the mechanism behind --

MR. CORRADINI: Behind triggering?

MR. LEE: -- Thermal explosions altogether too well?

MR. CORRADINI: I think you can say that quite definitely; but what I would emphasize is --

CHAIRMAN KERR: Are you sure that you understood his question?

MR. CORRADINI: No. Well, then, maybe I didn't.

MR. LEE: Well, I'd like to think that we don't fully understand the mechanisms behind the steam explosion or thermal explosion in general yet, because if you cannot somehow distinguish between triggered explosions and spontaneous explosions and so on.

MR. CORRADINI: You'll have to be a little more specific. What mechanisms? Let me see if I can break down --

MR. LEE: What triggers apparently how the thermal explosion takes place or is initiated, perhaps it's not well understood.

MR. CORRADINI: Okay.

MR. LEE: Can I do this?

MR. CORRADINI: I would say that's a fair conclusion. We don't know in many cases why the spontaneous explosion occurs in terms of the physical reason, the academically physical reason; that is, the mechanistic, what's happening on the microscale. But what I'm trying to get at is the macroscale. Given the fact you get the explosion and given you have some reasonable handle under the conversion ratio from the explosion, how does it macroscopically affect you in the reactor system, I think we have a pretty good handle, okay?

MR. LEE: Good.

MR. CORRADINI: I want to break apart the two because one of them is a long-term pursuit and one of them, I think, is a short-term pursuit.

MR. LEE: How much of a sensitivity do we have to the magnitude of the trigger? Is it order of magnitude type or a factor of two type of sensitivity? Could you perhaps comment further?

MR. CATTON: It's a threshold, isn't it?

MR. CORRADINI: It's a threshold.

MR. CATTON: The actual peak pressure from your interaction does change as you change the trigger providing it's large enough, is that right?

MR. CORRADINI: Say it again. I'm sorry.

MR. CATTON: The magnitude of the pressure pulse from the interaction does not change the triggering. It's a matter of meeting a threshold requirement.

MR. CORRADINI: That's right.

MR. LEE: I didn't understand it.

MR. CORRADINI: Okay. Physically what we're thinking of is film collapse and liquid-liquid contact between the two materials. What you need in the trigger to cause that, as Dr. Catton was saying, is a threshold type of thing, so we know what magnitudes don't do it, what magnitudes do do it, okay.

But in terms of changing the conversion ratio, experimentally, empirically we do not see a difference whether it's spontaneous or artificial.

CHAIRMAN KERR: Is it always the same within some data scatter independently of whether you trigger and with what magnitude you trigger?

MR. CORRADINI: Within some data scatter for the intermediate scale experiments you see a scatter or around a half a percent to two percent, and in terms of if you want to plot it versus various abscissa, melt mass, water mass --

CHAIRMAN KERR: But what about plotting it against trigger magnitude? No correlation?

MR. CORRADINI: No correlation.

CHAIRMAN KERR: Once you get above the threshold.

MR. CORRADINI: Once you get above the threshold. At this point that's what we see.

MR. LEE: One last question.

MR. CORRADINI: Sure.

MR. LEE: How is the energy affected by the presence of impurities?

MR. CORRADINI: What do you mean by impurities?

MR. LEE: Well, for example, in an actual reactor situation postulated with accident materials, some steel or iron or something mixed together with the fuel. Would that change the picture?

MR. CORRADINI: Oh, okay. You mean fuel composition? In fuel composition we see very little change if it's molten. The real physical question is if you've got a lot of solid debris in there. Then I would say the effect of solidification is quite apparent, that if you solidify you will reduce the --

MR. LEE: They are molten, too. The stainless steel or whatever is molten also with the uranium dioxide. Then it would not affect your result very much?

MR. CORRADINI: I'll let Rick --

MR. SHERRY: I think you ought to mention the effect of the metallic versus oxidic content of the melt in the production of the non-condensed --

MR. CORRADINI: I don't know how deeply everybody wants to know about this.

MR. CORRADINI: I think it bears on this question.

MR. CORRADINI: Okay. In terms of experiments we find that when you have a metallic melt, you generate non-condensable gases because the metallic melt is essentially oxidizing in the steam space, okay, and the non-condensable gases serve to protect you and make triggering more difficult. So in terms of that, a metallic phase melt is harder to trigger than an oxidic phase melt. Okay. That's one effect.

Another effect is solidification. If you're closer to solidification temperature when you're throwing the molten materials in, obviously if you're going to start solidifying some of the debris or some of the driving hot fluid, again your conversion ratio is going to go down. That's been empirically seen also, okay.

So for each different effect, as you term it impurities, whether it be near-solidification or more metallic, you get different effects.

CHAIRMAN KERR: Other questions?

MR. SEALE: Is there a summary available of sort of the best estimate results or model which you have?

MR. CORRADINI: Well, there's a series of reports. In terms of containment what we've done, and I think you get the impression the way I'm talking, is we try go separate what I will call the microscale, what's happening physically in the mechanistic level here, and the macroscale, how does it affect the reactor system.

And in terms of the reactor system we put out a Sandia report, 79-2002, and that talks about containment failure probability from the steam explosion. In terms of the microscale we've put out a Sandia report 2003, 79-2003, and more reports -- I mean, we could talk afterwards; I can give you a whole list of reports in terms of our experiments that are either in publication or have just been recently published.

CHAIRMAN KERR: Other questions?

Now, let's see. Sandia creates steam explosions, and LASL creates steam explosions, and I assume they're different. We're now going to hear about LASL steam explosions.

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MR. STEPHENSON: I'm Mike Stephenson from Los Alamos, and Dr. Kerr, for once we've created the same steam explosion.

What I'll talk about very briefly, and because of the time constraints I'll be as brief as I possibly can, are analyses that we performed for the Zion-Indian Point study. What we tried to do in these analyses was to take a computer model, specifically the Simmer code, that has been used in similar LMWBR analyses, benchmark that against a Sandia experiment, a particular one, and then use the code to extrapolate to reactor conditions, using the same heat transfer assumptions as gave us a reasonable analysis of the experiment.

The purpose of these calculations was to see if there are strong effects due to the interactive nature of the expansion process, and the hydrodynamics, the heat transfer and the hydrodynamics in that process.

The particular experiment that we analyzed was test number 43, performed by Larry Buxton at Sandia. The assumptions we used were very similar to those that we used in other analyses, in analyzing other experiments and other accident configurations, both with simulant fluids and in the accident case, for the LMFBR materials.

There are three primary pieces of the test data that we tried to match with the experiment analysis. Particular test -- I should add the test involved pouring molten termite-generated materials into an open tank of water, atmospheric pressure of the water more or less at room -- well, at 300 Kelvin or something of that sort.

The experiments exhibit behavior which shows the molten materials streaming down towards the bottom of the vessel in a film boiling mode, with rather large particle sizes, globules of fuel. I should add that the configurations that I'm talking about and the conclusions I'm making on these configurations were drawn from tests performed in a lucite tank, where one can see what's going on.

In our experimenta analysis we assumed that the explosion was trigged as the molten material touched the bottom of the vessel. That appears to be the case in most of the experiments in which explosions occurred.

To match the rapid pressure rise measured in the experiment on the vessel wall, we had to assume in our experiment analysis that the particles were fragmented to small size. The particular number that we used was 300 microns in diameter molten material particles. This is in reasonable agreement with those particle sizes observed after some of the tests.

We also assumed that there was a vapor chimney above the intermixed region, the primary intermixed region. The vapor chimney gives a very rapid pressure decay following the initial rapid spike caused in the interaction.

Our analysis matches the peak pressures, the rapid pressure rise, the rapid decay fairly well. The calculated efficiency, in the same terms that Mike Corradini was talking about, the kenetic energy of the system to the thermal energy of the molten material, the calculated values were about .5 percent. The measured value in this particular test was .43.

In performing reactor calculations for Zion specifically, we took the reactor geometry of the vessel; we used the same heat transfer assumptions, that is basically the 300 micron particle size; we took the same ratios of molten material to liquid water and steam in the primary interaction zone that gave a successful match -- that's a 50-25-25 percent ratio. We assumed in different cases that there was 10 percent or 20 percent of molten core materials, premixed with water steam.

In the reactor case, we assumed that as the molten core, part of the molten material, fell into the lower part of the vessel due to a sudden failure, the falling material mixed coarsely, much as observed in the experiment, and triggered, as it hit the bottom of the vessel. The remaining molten core materials were left in the original core position. We assumed 100 percent of the core being molten in these calculations.

I should add that in my opinion, the largest uncertainty in these calculations is given by these initial conditions, assuming the amount of core materials mixed. As I said, we have seen 10 percent or 20 percent in separate cases.

This simply shows the geometry used in the calculations, an RZ two dimensional calculation, including the reactor core region, the downcomer region, which allows water to flow up and even out an outlet pipe, so there is some compliance, inertial compliance in that direction included.

Now in this reactor case, the primary difference that we see from the experiment analysis is the large inertial tamping given by this molten core above the interaction zone. What we find is that the efficiencies in this case are larger than in the experiment analysis. The higher inertial tamping leads to a longer expansion time. In the experiment case with the vapor chimney, the expansion time is fairly short and there's an early venting out -- very early there is venting out that, although the liquid does close off that chimney during the expansion. But nevertheless, there is not much inertial tamping in the experiment case.

One of the points we were looking for is to see if the two-dimensional behavior of this molten slug affects the expansion dynamics and the loading dynamics. Our calculations show that it certainly does. This large slug of molten material tends to break up in a two-dimensional fashion as it moves towards the head.

Separate calculations performed with another code indicate the same kind of behavior, although the two-dimensional aspects are very dependent on the assumptions made on the initial geometry of the expansion zone.

In all cases we find that the loadings on the head are very much biased towards the center, towards the apex. This tends to decrease the likelihood of large missiles being generated, very strongly.

One point that I'd like to make strongly is that we feel that even though not analyzed in detail, the lower head is likely to fail during the expansion process, prior to any large impact delivered to the head, in cases giving large kinetic energy, so that the missile generation in the upward direction is even more unlikely.

These calculations are reported in the Zion-Indian Point study. I think most of you have had a preprint. That's all I have to say. If there are questions I'll be happy to try to answer them.

CHAIRMAN KERR: Question?

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MR. KELBER: I come now to the final part of my discussion of our program. You've heard some of the details of planning for the nearterm with regard to Class 9 accident research and you've heard of some specific activities that are going on.

I now want to describe the logic and some of the history that's led us to this position, a current concept of the strategy resolved in these issues, and the relationship of our work to certain recommendations that you have made, and finally to describe my understanding of how the budget will be handled.

The logic has been dictated by a perception, a circumstance, and a misconception. That signals the possibility of a change in approach, and I assure you of our readiness to listen carefully to your advice in completing the program formulation.

The perception is implicit in WASH 1400 and is referred to in the Rogovin report that at some point core damage is so severe that despite what efforts one makes, the core will proceed inexorably to meltdown. The problem with this concept is that at each end of the scale, the core is coolable for some set of coolant and power conditions. Certainly it's coolable when only a small amount of damage has been received, and even the TMI-2 core, which is evidently pretty badly damaged, is coolable.

We're fairly sure that a sufficient time has passed for decay heat to reduce. Even a large particle debris bed can be cooled, although as you have heard, we are much less certain about the debris bed forming earlier in the damage sequence.

So I find it difficult to identify conceptually the set of variables whose values being in a certain range indicate non-coolability. And I have a little cartoon here that illustrates my conceptual problem. I apologize for the artistry or the lack of it. The artist is a miserable fellow, poor wretch. Name's Kelber.

But basically the question is, do you proceed along with a core whose heat can be removed with reasonable facility so at some point damage accumulates and there's nothing more you can do? Or as damage accumulates does it simply become harder and harder to remove the heat but all of the heat, if you try hard enough, you can remove the heat? And that dictates a different strategy if you believe the one or the other.

For the present, we are proceeding with the assumption that after some degree of damage accumulation and some condition of decay heat, the core no longer can be cooled but begins to melt down into a series of blockages and debris. There's an operational value to this assumption because it allows us to draw a reasonably close distinction between the work under the aegis of the fuel behavior branch and the work that's described here. Clearly, there's overlap and an active interface.

Superficially the behavior in this regime is like that described in the transition phase in fast reactors, but we must remember that in the LMFBR case the assumption is made that there's been a loss of coolant flow.

The other neat point about this assumption that signifies another convenient dividing line, the debris bed, and with a high degree of confidence we view the issue of debris bed coolability as a technically separable problem.

Now, it is awfully tempting therefore to develop a program strategy based on such assumptions that lead to nice logical compartments for managing the work, but we ought to in some respects correspo: l to reality and we ought to have some checks on whether we're making a basic error.

The circumstance I referred to is that soon after the immediate response to TMI-2 was in hand, Dr.Murley realized that there was a need to integrate the existing fuel melt program and developed a plan to use all our resources effectively to resolve problems related to the treatment of fuel melt in the coming years. Dr. Murley designated Mel Silberberg as program manager to perform this task of integration and lead the forward planning and you've heard his progress report.

Soon afterwards work began on the TMI-2 action plan and integrated fuel melt program was incorporated as part of the research activities ascribed in the plan. I recommend the integrated fuel melt plan to you. A draft will be sent to you soon. Nevertheless, I'm constrained to point out that it is created on the basis of two assumptions -- one, that the technical problems can be separated at the debris bed stage. I just discussed that aspect. The second is that containment loadings in response are a function of the fuel melt process and there's no substantial interaction between containment systems and the fuel that modifies the fuel melt process.

For conventional, large, dry containments it appears that this is a correct statement except for the operation of sprays and the ECCS system. And that's a key question that was raised in the Zion-Indian Point study, and that is if it is power restoration at some time after core melt, should you provide for some way of diverting sprays and ECCS or how should they be handled? Do you repressurize the containment? The introduction of mitigating systems such as vents and core catchers may change this view. These reservations are a consequence of the circumstance that the integrated fuel melt plan is a forerunner of a more complete program plan. And that circumstance has led to a certain amount of confusion in people's minds.

Still, we have to get started and the fuel melt plan is a better basis than most. As is the case with most fuel melt work, the fulk of the research involves extrapolation to a difficult thermophysical regime. Simulation of these effects is often difficult and expensive.

The program under fuel melt forms the largest single chunk of Class 9 accident research and will get correspondingly the bulk of the attention. But the circumstances that the fuel melt plan was the only currently available basis for a more complete Class 9 accident program became evident during the detailed review of the fuel melt plan in Feburary by the research review group referred to earlier.

First, an important gap was evident in the area of the transition from a severely damaged core to a debris bed, and second, key technical interfaces with the fuel melt plan were identified and their role and importance within a broader framework for organization of the total program needed. During this same time period, lessons learned from the preliminary results of the Zion-Indian Point study reinforced our thinking regarding the need and structure of a broader Class 9 accident research program.

All this, I might say, was accompanied by some negotiations over the task action plan which I can only describe as byzantine. They'll ask me to take that out of my testimony, but I have to get it in. And that simply compounded some of the misconceptions.

Because these circumstances and the groundrules, timing and pace of the formulation of the task action plan led to a misconception, it has to be cleared up. That is that the integrated fuel melt program was the Class 9 accident program. The bulk of it, but it is not the same thing.

I hope I've clarified the situation.

CHAIRMAN KERR: I didn't know there was a situation to be clarified until you told me about it.

MR. KELBER: I think there was when we tried to organize this meeting.

CHAIRMAN KERR: And now you have me puzzled, but at the next meeting you can clarify it.

MR. KELBER: Okay, it's always good to have something left over for the next meeting.

Dr. Murley's leadership in the integration of the fuel melt program coupled with some of our earlier experiences in assessing the coolability of the TMI-2 core caused us to examine the question of ho- resources devoted to LMFBR safety might be effectively utilized. We quickly realized that a considerable spin-off of LMFBR safety technology could b- made to save both time and money in addressing Class 9 accident problems of LWR's. Note that it is technology and skilled people that are being spun off, and not the product itself. There are strong resemblences between many of the particular problems that arise, but we are under no illusion that an LWR is just an LMFBR with a different coolant.

The process of technology transfer or spin-off is illustrated in my next viewgraph, and this is hardly complete, but it is illustrative. There are ample opportunities for transfer and we intend to make the most of them. This point has been mentioned in Congressional testimony this year by the Commission and by Dr. Budnitz and as far as I know, it's had a favorable reception throughout.

We believe that many of the techniques and technology developed here, the skilled people that have done this work can technically address many of these questions and save us a considerable amount of time.

Finally, we're conscious of your views with respect to maintaining a program directed at LMFBR's. The budgetary pressures are such, however, that we will need to make most of dollars to double duty. To the extent that LMFBR safety technology can be transferred to LWR problems, the reverse is likely to be true as we-l. So we hope to be able to carry out a program that also attacks key problems unique to LMFBR's, such as the sodium-concrete interaction, and we've provided for that in our program logic; we await Congressional guidance as to the future of that program. We believe that we will receive guidance to continue work in this area.

With this strategy in mind, I want to recall to you our program organization, the four-part organization that I illustrated earlier. The second and fourth items, fuel melt plan and the remnants of the LMFBR program, are well planned and have received a fair amount of review within the Commission and within review groups. The presentation that was to have been made by Dr. Curtis earlier today exposes our thoughts with respect to the first and third, where we see a short-term but good-sized effort to attempt to take the various codes that are available, couple them with what model tests and what other tests are being done in this area to produce some good system analysis methods.

I've been asked to comment on how this work will be handled in the budget. Dr. Murley and Dr. Budnitz are engaged in rectifying the budget line items and it is my understanding that this program will appear as a major part of a line item. And I believe that line item will be called Severe

Accident Phenomenology and Mitigation. I expect that my management will be meeting with the Full Committee soon if they've not already done so to discuss the budget organization. I believe they have had preliminary discussions with the executive director for operations and the comptroller. I assume that they will meet with the Commission and with the ACRS.

There are significant interfaces that have been described and as the budget organization is made clear we will have to pinpoint these areas of interface and overlap. We're preparing to do that.

This program does not by itself respond fully to the recommendations for new directions in research listed in chapter 2 of your report, although it does address the topic of studies of courses of serious accidents and of molten core retention and steam explosions.

Taken together with the new work being initiated by the Fuel Behavior Branch, the IREP program and the work on multiple faults accident sequence analysis, now called severe accident sequence analysis, the aggregate response is very good. As budget guidance is developed the pace of work will be better defined, but the scope of work is as you recommended. We have no quarrel whatsoever with the recommendations in that regard.

Recent decisions to prepare for rulemaking, to include Class 9 accidents in evaluation of environmental impact, et cetera, all point to a consensus regarding the need for more knowledge about these accidents. Important decisions have to be made about the pace of the programs that develop this knowledge. Recently Dr. Ross of NRR commented on this matter in a memo to Dr. Murley. Let me quote it because I think it is a key statement.

"We view the timely and appropriate execution of a substantive research program in this area as important to the successful resolution of sfaety and licensing issues centering on severe accidents beyond the design basis as described in Section II.B of the TMI Action Plan, NUREG-0660. In particular, we consider this component of the RES program to play an important role in the rulemaking proceedings regarding consideration of degraded core/core melt accidents in safety reviews, especially in providing some of the technical bases on the issues that will be discussed/raised during the proceedings."

That's the end of the quotation. I might say that in that action plan, there is also anticipated approximately 100 man-years of effort by the industry in this same area.

Well, we share Dr. Ross's view. We are as one in this and we anticipate that you do, too. And that concludes my testimony on this matter.

CHAIRMAN KERR: Are there questions? Charley, I don't know whether you're the one to whom I should be addressing this question, but I gather that much of the present planning is aimed at supporting the Commission in the rulemaking proceedings?

MR. KELBER: Yes, sir.

CHAIRMAN KERR: Part of that --

MR. KELBER: That is the concept which I share with many others, but it's one which I think I have taken the lead on and developed.

CHAIRMAN KERR: Part of the proceeding will necessitate some understanding of the phenomena that will be discussed. At some point the Commission or some consortium of the Commission and others will need to put forth some criteria, if one follows the sort of thing that was done in say ECCS.

Who's going to be responsible for that effort? Will that be standards, NRR, RES?

MR. KELBER: I expect that NRR will be taking the lead, but that they will involve us heavily as their prime source of technical support, and I assume that standards will also be playing a role. I'm frankly not aware of all the intricacies by which criteria are developed.

CHAIRMAN KERR: It would not seem too early to begin thinking about what research would need to do to assist in --

MR. KELBER: I think that's correct. I think that we are behind the times and have to hurry a great deal to catch up.

CHAIRMAN KERR: Other questions?

The agenda shows a ten-minute closed session to discuss budget. Is that still appropriate?

MR. KELBER: I think it's still appropriate. You suggested that we might move it to the end. I'm agreeable to do that if you wish.

CHAIRMAN KERR: I do think that that would be the thing to do, and I'll therefore declare the non-Executive Session part of the meeting at an end. We now go into executive but open -- this it not a closed session. It's just an Executive Session. We've been advised among other things that we don't need to have it recorded.

(Whereupon, at 4:40 p.m. the meeting went into Executive Session.)

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This is to certify that the attached proceedings before the

NUCLEAR REGULATORY COMMISSION

in the matter of: Advisory Committee on Reactive Safeguards Subcommittee on Class 9 Accidents

Date of Proceeding: May 9, 1980

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.

Sharon Connelly

Official Reporter (Typed)

Sharon Connelly

Official Reporter (Signature)