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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

In the matter of:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Docket No.

SUBCOMMITTEE ON CLASS 9 ACCIDENTS

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NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON CLASS 9

ACCIDENTS

Monday, April 25, 1983
1717 H Street, N.W.
Washington, D.C.

The Subcommittee on Class 9 Accidents met at 8:30 a.m., pursuant to notice, William Kerr, the Subcommittee chairman, presiding.

PRESENT FOR THE ACRS:

W. KERR

C. SIESS

P. DAVIS, Consultant

J. LEE, Consultant

M. CORRADINI, Consultant

A. WANG, Designated Federal Employee

G. QUITTSCHREIBER, Designated Federal Employee

AUDIENCE PARTICIPANTS:

C. REED

T. BUHL

M. FONTANA

T. TYLER

E. FULLER

P R O C E E D I N G S

MR. KERR: The meeting will come to order

This is a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Class 9 Accidents.

My name is William Kerr. I am Subcommittee Chairman. Mr. Siess is also present as a member of the Subcommittee, and as consultants today we have Mr. Lee, Mr. Davis and Mr. Corradini.

We are here to listen to a description of the industry degraded core rulemaking program.

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

Gary Quittschreiber is the designated Federal employee. He is assisted by Alan Wang, an ACRS Staff engineer.

Rules for participation in today's meeting have been announced as part of the notice published in the Federal Register of Friday, April 8, 1983. A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. I ask that each speaker identify himself or herself and use the microphone.

We have received nowritten statements from members of the public, nor have we received requests for time to make oral statements.

During the course of this meeting or subsequent meetings or both, we hope to learn something of the objectives, the progress and the ultimate contributions of the IDCOR program.

There have been a number of meetings between IDCOR and members of the NRC Staff, at least one of which I attended early on, and some of the ACRS Staff has attended some of the meetings. I would hope that at some point we could address ourselves and get at least partial answers to some of the following questions.

First is sort of the obvious one, and that is what question or set of questions the IDCOR research was designed to answer. Second, how are you going to know when you have the answers? Third, who was it who posed the questions? Fourth, what is going to be done with the answers? That is, how is it expected that the answers you are trying to get will be used in whatever process, primarily by the industry, by the industry and the NRC or whatever?

Fifth, in what context is the work being done? For example, is the work being done with the assumption or philosophy that existing reactors are safe enough and that this research is principally confirmatory of that estimate, or is it based on some other assumption or set of assumptions? If the assumption is that existing reactors are safe enough, I would be interested to know how that conclusion was reached,

and hence, on what basis the research proceeds. We probably will raise other questions as this conversation and other conversations proceed, but I would at least be interested in answers to those questions.

I must say that we want to express now and I will, I am sure, later, our appreciation for your willingness to come here and tell us about this. I know it represents a considerable expenditure of effort on your part. We want to understand it insofar as we can because I think it is an important part of the whole effort to try to establish where we are and where we should be, and it is to everybody's interest to achieve a better understanding of the answers to those two questions.

I am told that Mr. Cordell Reed will open remarks, and I am delighted to welcome him to our meeting.

Mr. Reed.

MR. REED: Thank you, Mr. Kerr. We are pleased to be here today to explain our IDCOR program. I am Cordell Reed, Vice President of Commonwealth Edison, and Chairman of the IDCOR Steering Group. I am accompanied, to my left, by Mr. John Siegal, who is the Project Manager in the Atomic Industrial Forum Office, and to my immediate right is Dr. Anthony Buhl, Program Manager and Vice President of Technology for Energy Corporation, and to my far right is Dr. Mario Fontana, Program Director.

We also have additional members of the Steering Committee in the next row and members of the Tech Staff.

We will answer the specific questions that you raised, maybe at the end of our presentation. I think during the presentation that some of them will be answered, but we will be sure to go through each one of those in particular at the end to make sure that we have answered them.

My part of the program will be, at a very minimum -- I would like to just take you through very briefly the organization of the IDCOR group and then we will let Dr. Buhl and Dr. Fontana give you the technical status of the program.

(Slide)

The IDCOR program was started in December of 1980, really under the auspices of the Atomic Industrial Forum. The IDCOR program consists of a policy group which chaired by John Selby of Consumers Power. The policy group consists of all of the sponsors. There are some 58 sponsors of IDCOR, Industry Degraded Rulemaking Group. They consist of nearly all of the nuclear utilities, with the exception of one or two, the NSS vendors and the major architect engineers.

Each sponsor has one vote and we meet at least twice a year in the policy group. Under the policy group is the steering group, which I chair. The steering group consists of 12 members. They are all senior managers of

either utilities or architect engineers or NSS vendors. We meet probably on the frequency of once a month, oft-times more frequent than that. We give overview to the program manager for the day-by-day operations.

The major activity is carried out by the program managers, which is TEC, Technology for Energy Corporation. Tony Buhl is the program manager, and he is supported by a staff of managers within his organization.

In addition to the TEC staff, there is a legal adviser, which is George Edgar, whom some of you may know. We have a technical advisory group which is headed by Miles Leveritt of EPRI. The technical advisory group will call into being smaller groups which will review specific subjects. We will try to bring in experts from utilities and from consultants and sometimes national labs to go over a specific subject with TEC. They are kind of like a sounding board to make sure all of the technical facts are such that we feel we can stand behind them.

In addition, TECH has formed a senior consultants advisory group. It consists of Hans Spowski, Sol Levine, Norm Rasmussen, Bob Sill and Bill Stratton, and we received input from the senior consultants all through the program, and they certainly vectored us down to make sure that we don't make some of the same mistakes that were made on WASH-1400. It has been a very useful group and we have received

a great deal of input from them.

I just might tell you, when we went into this operation in December of 1930, our attitude was one of wanting to reach some sort of closure on the whole severe accident issue. We did not come into this whole process with any predetermined thoughts. As a matter of fact, back then most of us had measured the distance between the bottom of our reactors and the floor to see what size core catcher would fit under there. We had also considered pretty seriously containments vents, and what concerned us at that time -- we were talking about sizes of containment vents without understanding the scenarios or the phenomena that would get us there and at what time you would open them up.

So we started this whole project in order to try to gain the best technical resources we could to address the whole issue of severe accident and to just prepare the technical case.

Our original goal was to have that technical case completed by July 1st of this year, and we are pretty well on schedule. We anticipate having a final draft of our technical case in July of this year. Now, maybe it is best to have Tony and Mario to discuss the technical aspects of the program, and then we will resolve ourself as a panel to answer any questions you have, and you can ask us questions as we go along, also.

MR. KERR: Thank you, Dr. Reed.

MR. BUHL: Thank you and good morning.

We will come back at the end of the presentation to the specific questions you asked and try to answer those very directly.

At the outset I should say that the initial question was asked by the Advanced Notice for Rulemaking in October of 1980, and that is: To what extent, if any, should nuclear power plants be designed to withstand degraded core or molten core accidents?

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And to what extent, if any, should the regulations be changed to reflect this?

We are pleased to present this program to the Advisory Committee today. What I will do is give you a broad overview of the program, the outline of the program, tell you about our interactions with NRC and where we stand today so you have a sense of where the program is now two years later. Following that, Dr. Fontana will take each technical element of the program, walk you through task by task and give you some sense of what has been accomplished in those tasks and the insights we have gained from that work.

(Slide)

So that what I'm going to talk about today is part one.

(Slide)

We spent a lot of time early on in the program developing -- I think each of you have a copy of the program plan. This document underwent a great deal of review, and the basic logic of the program, the basic technical logic remains. We made a number of changes since this was produced some two years ago, but nonetheless, the basic program logic remains, as is shown in the large foldout sheets in this particular document. As I said, Dr. Fontana will go through those in some detail in a moment.

First of all, the objective was to develop a comprehensive, integrated, well-documented, technicall sound position on the technical issues related to degraded core accidents. The purpose for doing this was to have for the industry a technical basis for participation in whatever sort of decision-making process NRC decided upon for the severe accident issues.

At that time, that is, the time we developed the program plan, it was not all together clear -- there was an advance notice for rulemaking on the street. It appeared to the industry to be a rather prescriptive rulemaking in the sense that the ANR identified certain design features and gave some details along those lines.

(Slide)

The program we developed has some 45 technical

tasks, and they can be thought of broadly in four categories. In a moment you will see a logic chart, the same one that appears in the program plan, which follows this basic logic. Some people think the IDCOR program is a PRA program. It is not. PRA is used and we think it should be used, particularly in this area.

We examined the existing PRAs to establish the current risk status. I won't go through these tasks, since Dr. Fontana will do so in a moment. This represents about 25 percent of our program, to give you a sense of the effort. We have a second set of tasks which are focused exclusively on core damage prevention: what are the kinds of things the operators can do, what kind of equipment changes, design changes, modifications, whatever are available to prevent core damage. So we have a good bit of focus here.

we put a great deal of attention in this particular area, containment effectiveness, where we look at all the various phenomena which drive and challenge the containment. We look at the structural capability and we have developed a number of new tools, if you will. You will hear something about the MAAP code. That is M-A-A-P, Modular Accident Analysis Program, which we hope will become the industry standard to replace the MARCH code series which was developed several years ago and because of the nature of that code has been misused by a number of people. It's very easy

to misuse that code if one is not intimately familiar with all that goes in and all that comes out.

We have also developed other tools such as RETAIN in this particular set of tasks. We also look at things like equipment survivability. Here what we did was to take the list of various mitigation features and devices which have been suggested by many people over the last few years and evaluated the effectiveness of those devices.

For example, in the core catcher arena, we were fortunate to find the Germans had done an evaluation of some designs which gave us a good basis for starting there. Other examples: in the filtered vent evaluation, of course, the Swedes had the project Filterum, and we were able to work out arrangements with the Swedes to obtain all that data.

MR. KERR: Is that small "result box" there in the middle deliberately obscured?

(Laughter)

MR. BUHL: No. Perhaps as an afterthought -- you will see the logic, and all the little blocks are small. I just wanted to give some big picture sense of the program. There is a logic laid out which tracks the results of the various tasks into our technical positions. It is not deliberately obscured. I guess it is on this Vu-graph, though.

MR. CORRADINI: Pardone me. Just one question.

Within each of the areas I gather there is effort to look at the uncertainty of the prediction of the calculations.

MR. BUHL: Yes.

MR. CORRADINI: So you will speak to that later on also?

MR. BUHL: Mario will speak to uncertainty as we walk through. I should say in this particular block we did not do a new PRA. We simply captured what is available. And as you know, in some of the PRAs, uncertainty has been looked at in some detail; in others, it is lacking. So we make our statements on uncertainty in the PRA area based on what those PRAs contain. We will talk about that in the other areas, though, as we go through.

(Slide)

I would like to give you some sense of the way you have developed the plan in process for documenting these results. We have some 45 technical tasks, 23 contractors working on the program, so it is important to be able to document the results in a way the industry can understand, regulators can understand and the general technical community can understand.

We developed a three-tiered process for reporting the results of this program. Each of the various technical

tasks provides us technical reports. They are the usual kind and provide detailed technical information which will be used to support the final positions.

We have above that a technical summary, which is maybe a couple inches thick, which summarizes the results contained in this large stack of technical reports covering the entire program. The purpose here is to support the IDCOR positions on the issues and be able to index and provide traceability to the decision-makers.

We have written for people like the Commission and others a very thin document which states this is the issue, this is what we know about the issue, in a paragraph or so, and these are our recommendations; and those particular conclusions and recommendations will identify the issues and identify traceability back through the technical summary report back to the details.

This is a process we have in place to help us with our own accounting, to help us communicate with what we have learned to others.

(Slide)

All contracts have long since been in place. We have 23 contractors working on the report on the 45 tasks, and the total program entails about $10 million in resources.

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As Cordell Reed mentioned, the technical program

is tracking on schedule for completion this July. We have final technical reports for 14 reports. We have draft reports for 21 of the tasks and will talk about how we reviewed the work in a moment. We do have expert review groups set up in a large number of technical areas.

We have a close-out process for completing the technical program by July. We have developed two additions now to the final report, and as Cordell mentioned, that will be available in the July time period. That is the final technical summary. And the final draft of the summary and conclusions will also be available in July.

(Slide)

Now, to move away from the program, which Mario will come back to shortly, I would like to talk a little bit about NRC interactions and address, at least in part, some of the questions you raised at the outset.

Very early on, we recognized it was important not only to do this technical work but also to communicate it to others and to work particularly with the NRC in trying to find areas of consensus, highlight areas of disagreement, and work toward closure and resolution, because what the industry is really looking for is some permanent resolution in the regulatory arena for these issues. Otherwise, the licensing instability in this particular area will remain.

So back in May of last year, we had our first

management meeting with NRC. We had several technical meetings covering different areas. I won't go through the dates. We did have a fuel damage program review three days in July of 1982, and since then we have had a number of additional technical and management meetings with NRC to understand what the research people are doing, what NRR's is doing, and so they could understand what we are doing.

We have briefed the Commissioners on one occasion. We met with NRC management on March 2nd this year to talk about the future. We plan to complete our technical program in July, and the industry has decided that it would be wise to continue the IDCOR program for another year in a different mode, and that is what we call the closure phase of IDCOR. There we don't expect to be doing a great deal of technical work but a great deal of interaction with NRC with the national laboratories and other people, perhaps the ACRS, to explain the results we plan to put on the table in July, what they mean, and try to reach a consensus and resolution.

We missed by one day when we made up this Vu-graph. We didn't plan to brief the ACRS tomorrow. We have developed a plan for closure next year leading to NRC decisions on severe accident decisions on mid-1984. This is consistent with the policy statement the NRC has issued for comment. We have been following that and your interaction with the Commissioners and Staff with some interest.

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For each technical task we have identified all the milestones. We have expert review groups inplace. As Cordell mentioned, Miles Leverett of the PRA chairs our technical advisory group made up of people all across the industry, national laboratories, universities. These people perform our expert review of each technical report, we cycle it back through the contractors for comments and so forth. So we have a rather stringent and formal process in place.

We have identified along the way a number of information transfer points which may or may not relate to draft reports so that the task can go forward, and as I have already mentioned, all of this information is being summarized in the conclusions and recommendations report and in the technical summary report.

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I am going to spare you-all of this except for one Vu-graph to give you a sense of schedules. We do have some tasks still ongoing. Many tasks have been completed, as you can see. These numbers won't mean anything to you at this point. Mario will walk through these one by one in a few moments. But we have laid out a rather stringent close-out program to ensure completion in July, and I am going to skip all the others, which look like this.

I think with that -- we may want to come back after

Mario's presentation and talk a little bit about the objectives of the closure phase which we expect to enter in July after the completion of the technical program and give you some sense of the kinds of things we have suggested to the Staff. We have had meetings with Mr. Minogue and his associates talking about this closure phase, so we can come back if that is of interest to you. But I think now we should probably move on to the technical program and walk through the program piece by piece.

MR. KERR: There are some questions.

Mr. Lee.

MR. LEE: Could you perhaps comment in general on any differences, if you have any, between the approaches that your group is taking relative to NRC?

9:00 a.m. MR. BUHL: Versus whom?

MR. LEE: Any approaches between what you are taking and the NRC approach in general.

MR. BUHL: Well, I think as NRC has developed NUREG-0900, as I understand the decision-making process they have laid out, the logic is fairly consistent with our logic. That is, the way NRC is tracking is fairly consistent. I think there are a number of differences in terms of detail. I think some of the NRC programs, particularly in fuel behavior and some of the others, may be going into some of the areas much more detailed than we are. In other areas we may be looking at what I would call some of the practical questions in more detail, so it is kind of hard to give you a simple answer to that.

I think what you have to do is accept that the basic logic is about the same and then contrast what we are doing piece by piece with what NRC is doing piece by piece. In terms of the PRA, for example, we both have the same PRA set available to us. In terms of the biggest difference, I think it's in terms of the containment effectiveness work.

As I mentioned, we were not pleased with the state, particularly, for a lot of analysis of the existing computer codes, so we developed our own computer codes. We are working with NRC to do benchmarking of the MAAP, RETAIN code series with their MARCH series to assure that at least

we know how to calculate the same things, but I think there are major differences in that particular area.

In the mitigation area, I think our approach is not so much in the mitigation area to look for new, innovative ideas but rather to take a large set of innovative ideas which have been put forward over the past decade and evaluate those.

MR. LEE: I guess I can be a little more specific in my question, perhaps. From the way I understand it, NRC has embarked on research efforts for degraded core analyses and rulemaking, which may take a matter of a few years, if not several years from now in order to obtain meaningful results, while you are interested in closing out technical issues by sometime mid-next year. So you must have a different perspective on technical issues that seems to suggest you can close out on these technical issues while NRC management seems to think they need a lot more information than they have in hand.

MR. BUHL: I'm not sure that's true. I think what is true is the following. I think NRC management, at least the management we have talked to, believe they will have enough information available by mid-1984 for decision-making. I think this is an important point you make. That is, one always has to ask himself the question that Dr. Kerr listed: how much information do I have to have to make decisions? I

think there is a tendency on all of our parts to say we want to know everything about a particular issue. Take fission products, hydrogen or whatever. That is a dangerous approach because it says I can never make decisions, and there is basically no a priori criteria which says in any decision-making this is precisely how much information I have to have to make decisions. On the other hand, one has to be careful not to fall into the trap of saying I must know everything.

I believe NRC management agrees with us -- you will have to ask them -- but I believe they agree with us that by the mid-1984 time period, enough information will be on hand on the major issues. A lot of information is on hand already. A lot of the things we did in this program was to take existing information and integrate it into some logical, understandable series and put that together with new analysis and evaluations.

I think the critical issue is can we use our judgment to separate that set of information which is needed for decision-making from long-term confirmatory research needs. That is a critical issue and one that NRC has wrestled for years. It is not a new issue, but in this case at least my understanding NRC management believes there will be enough information for decision-making.

That is not to say that we or the NRC expect to answer all questions on all subjects. We will not. I presume

this committee also will have to come to grips with the idea of separating information into that set needed for decision-making, which we think we have done -- that has been the basis for our program -- and pull that apart and say, yes, we recognize there are long-term questions which require further confirmatory information, and if that information, two, three or four years down the road turns up a novelty which none of us anticipated, then of course you take the proper actions. But I think we have to be careful not to assume we cannot make decisions because we don't know everything.

MR. LEE: Let me follow up on another point, a general perspective perhaps I would like to get from you at this point, and that is in regard to your desire to develop another computer code system, MAAP or whatever you call it. Why do you feel that such a code is necessary in light of what I perceive as major computer development efforts that again NRC has embarked on? I think it includes a number of computer code systems.

What technical basis do you think would provide you with better methodology than NRC would be able to accomplish in, let's say, the next year or two that motivated you into another development effort here?

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MR. BUHL: It was not our desire to get into computer code development. We had no desire whatsoever to do that. We decided that we would do this and evaluate other codes as backup, because I was familiar, and many people even more so on our team, with the NRC codes. In fact, I'm the guy that caused MARCH to be named MARCH. You may not be aware that when the contractor first came and I was working for NRC and the first name of the code was COMICS and I argued that was not an appropriate name for such a serious code. In retrospect, I think I was wrong because that code has been misused rather broadly by a number of people.

(Laughter.)

The reason we felt it was important to develop a code is because MARCH was really a stringing together of the modules used in WASH-1400; it was not put together in the way one normally puts a computer code together to use present-day jargon, to be user-friendly, or to be useful to the intelligent user.

Rather, MARCH was trying to gather in such a way that it gives you whatever answers -- it gives you answers which are valid to the extent that you have detailed understanding of not only what you put in, but how each step in the computer code works.

MR. KERR: I don't want to interfere with the flow of your logic, but I get the impression from what you have

said so far that MARCH is the only code developed in which the NRC is going to be involved. Am I mistaken? I thought they also had underway a modern, much more elaborate and more extensive one.

MR. BUHL: I'm going to come to that in my logic. It follows beautifully in the next step. Our point was that the NRC was developed MELCOR and other codes off in the future. But to come back to the point of decision making, our feeling was if we were going to do all the containment-effective analysis we needed in the two-year time period we have, nothing beyond MARCH would be available, and that hasbeen the case. We did not want to wait the next two or three years for NRC to develop their own MELCOR series, and the new codes which they are developing for decision making. So we didn't have a desire to go into the computer code development business. We did it as a practical matter so we could have these analyses in a timely way.

Now, the last part of your question had to dowith why do we think we can do it better. I don't know if we thought we could do it better, necessarily; it was just a matter of timing.

MR. KERR: Mr. Corradini?

MR. CORRADINI: I guess I don't want to let go, I want to continue what John started. What interesed me is -- you'll have to educate me because I don't perceive the industry's

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view on this -- what is the rush on the closure on decision-making in the 1983 timeframe? Is it a matter of down-side risk or uncertainty for plants and further modifications, or what is the rush?

MR. BUHL: I would question the word "rush," when you've looked at it for three years. It may be rush on a long-term research timeframe or a decision-making timeframe that we're accustomed to, but --

MR. KERR: We will stipulated that "rush" is withdrawn.

MR. CORRADINI: I'm sorry for using that word.

MR. BUHL: With that I'll ask Cordell to respond.

MR. REED: That will be struck from the record.

The industry set goals for itself when it ventured off into this process, and as in retrospect we see that those goals were tight goals but reachable goals. Again, one of our greatest motives was to find out the answers and to reduce the uncertainty surrounding whether or not the reactors have to address the design basis beyond which they were designed in.

Also, we have a number of sponsors, and I think most of us are concerned with projects that are underway and never end. So what we've done is we've tried to keep ourselves to tight schedules and tight goals, and to make sure that this program is managed properly so we don't consider ourselves rushing. However, we certainly won't tolerate any just waiting

around.

MR. CORRADINI: I don't understand it. It's not that I'm critical of it; I'm still trying to understand. If it went to 1985 or 86, -- one of the things that you said struck me. You said we had to be very careful of that nasty habit of continuing to look at things until we know all of the details. On the other hand, one of the nasty habits that most people make in decision making is to rush to judgment because we're pressed into or because we have a need to make a decision when facts are extremely uncertain and remain so for years to come.

I guess I remain so. On the other side of the coin, we want to come to a decision and we want to come to it now. Just to finish of the issue -- and you mentioned in your speech or your talk that if some new novel thing comes out in confirmatory research then we'll have to deal with it. I guess I'm concerned with the overall picture of how the public perceives nuclear power, and how bad it will look after supposedly all these issues have been made clearly certain and clearly answered and clearly taken care of. And -- so again, I'll ask the questions in terms of motivation.

What is the down-side risk for the industry in making this decision two or three or four years from now in terms of plant operation, or is it plant modification?

MR. REED: No, I don't think we will envision there being any modifications required in the interim while we're

still searching for the answer. Again, I don't know if going through the technical task will completely answer your questions, but we have laid out all of the technical tasks and we have reviewed them, and we think that those tasks will give us the answer and we think we have the right scope around what we have to do. We just think that you can reach this answer in this timeframe.

Look at the technical task, and maybe that question would be a good question to talk about a little more once you see what we're trying to do.

MR. KERR: It is correct, isn't it, that this program began about 1980, and in 1983 seemed three years away. Whereas now, 1983 doesn't seem three years away. You're taking it into account in the questions you're asking?

MR. CORRADINI: I am, I guess, but I want to turn it around in another way. If, in July of 1983, the reports come out and you come up with a bottom line of what you perceive to be the progression of possible postulated accidents, and on the NRC, on the other hand, comes out with a progression which causes physical events to create and generate source terms that are far out of line with what you're doing -- or to put it another way, if I draw it on a curve you have one bound and NRC may come up with another bound -- isn't it conceivable that those bounds will be so widely disparate that it's not going to take just a year to obtain closure, as you speak of it?

MR. REED: That's a possibility, although the industry will stand behind their case. I can't speak to the NRC's case. We've made every effort to have interactions and sort of -- I'll show you mine if you'll show me yours -- maybe that shouldn't go on the record like that.

(Laughter.)

MR. KERR: I think it's too good to lose.

MR. REED: In the final analysis, we'll have to stand behind our case and our schedule. We're just hopeful, though, that during the next year that the NRC will be far enough along in order for us to converge on many of the issues.

MR. CORRADINI: Okay, I'm sorry I took up so much time. I have one short one. You mentioned before that your Technical Advisory Group, which reviews your technical summary and reports, was made up of people from industry, national labs and universities. I looked at it. Is it the 12 people in the back of the book?

MR. BUHL: Yes. You're looking at a two-year old report. That's the Technical Advisory Group we envisioned two years ago. Today, it has grown to 40 -- I'm guessing, somebody help me -- 40 or 50 people, on that order. Lots more people. Mario is going to run through just a dozen or so different expert review groups that we have, for example, and it's much broader than what you see there.

If it's interesting to you, we can provide you a list

of those.

MR. CORRADINI: When you speak of review, my ears perk up and I would like to know the method and procedures in reviewing these.

MR. BUHL: Okay, we can cover that.

MR. KERR: Other questions? Mr. Davis?

MR. DAVIS: I have a couple and I'll make them brief. On what basis do you envision the IDCOR program making a decision on whether mitigating devices are required? For example, are you going to use some modification of the safety goal, or some novel risk comparisons? What is your approach?

MR. BUHL: First of all, the safety goal is not -- let me start and walk up to that question. Developing the safety goal is not part of the IDCOR program. What we have done, when you look at our program logic, is that each step along the way, we come to conclusions about a particular subject area. We compare those conclusions with the safety goal to see where we stand with respect to safety goals. But in terms --

MR. KERR: Excuse me. When you say the safety goal, do you mean the qualitative goals or the quantitative guidelines?

MR. BUHL: Yes, that's correct. That's correct.

MR. KERR: Excuse me. Which is correct? That you compare it with qualitative goals or the quantitative design guides?

MR. BUHL: Yes, both of those are correct. We have

laid out the program such that the quantitative pieces come out structured so that we can make the comparison, and then we can also make quantitative arguments as necessary.

But to come now to specifics, I want to explain where we stand with respect to safety goals. What one does is take a specific design feature, and based on all the analysis we have done, one sticks that in the plant and calculates the effectiveness of that particular feature that you have to assess for this particular core catcher or filtered vent; what do I gain. And one looks at the risk reduction, or in some cases, one looks at the risk increase because certain design features tend to increase the risk and not reduce it, so you can't make the a priori assumption that just because, in fact, it -- it may not decrease it, it may increase it.

Then you have to ask yourself on a cost-effective basis, is this something that makes sense. And when you come to that, unfortunately, engineers and scientists like to deal with equations, and there's a great deal of methodology available for doing cost-benefit, all of which when peeled back, comes to judgment at some point.

MR. KERR: I was going to say that's sufficiently ambiguous, that I certainly could or could not understand it.

(Laughter.)

MR. REED: I was just clarifying with my colleague here on the IDCOR group, and I won't add anymore to the

confusion. We do not have a pre-determined cost-benefit standard by which we will or will not make modifications to the plant.

We are mostly looking for a sensitivity analysis, and we're going to look at the data. We do not have a criterion. We have an extensive set of groundrules for doing evaluations, but the passing criteria we do not have.

MR. KERR: That was question number 1.

MR. DAVIS: The second question -- in some of the literature that we were provided, the IDCOR group has expressed some criticism of the NRC's program. In particular, there was criticism that the various elements of the program were not coordinated or integrated, and there was some criticism also of the 0900 program.

Do you feel that these problems are going away, or is it your current position that there still are difficulties in the NRC's approach?

MR. BUHL: I think they're improving. The only thing -- I don't like to guess when people hit me with literature. But the only thing I can recall is our earliest review of 0900 in the draft form. At that time, we made a number of strong suggestions about content and structure. We felt it extremely important that the logic of 0900 was obvious or evident. I think the logic perhaps was more evident in the heads and minds of those who developed the document than

perhaps it was in the documentation. I think other elements of NRC made similar comments, and that document was, in my view, substantially improved over that time period.

MR. KERR: Are there other questions? Mr. Davis?

MR. DAVIS: One final one, and this follows up Dr. Lee's question. I appreciate your desire to try to close these issues when you feel you have enough information, but it seems like there are some areas where we are still exploring, and the NRC has a rather extensive program to try to obtain some exploratory information from which to make decisions.

I'll give you three examples. External events -- there's many unknowns in this area with respect to risk contributions and so forth. Containment penetration integrity is another area where a large program is just now getting started. And the last one is source terms.

Now I'm wondering how your closure is going to occur in light of the uncertainties in these areas. Do you feel confident that you have enough information in these areas to make good judgments at this time?

MR. BUHL: Yes, but let me take them in reverse order. The fission product business -- there is still research to be done, as you say, but I think at this point we have sufficient understanding to come to some decisions fairly quickly; certainly in the next year. And I'm sure there will be specific questions remaining. But on balance, one could

make permanent and intelligent decisions.

In the containment penetration area, first of all, you would show and would argue, I think, that from the driving forces you simply do not challenge a containment. I think with the kind of things we did in WASH-1400 and in earlier studies, we now feel much more comfortable with the containment response that we might have three years ago. But at the end of that road or at the end of that sequence, one always has to delete, and there will be research done there and there is research being done there.

So I think in all three of those, we can make decisions but that doesn't say that everything is known. I think there will be research certainly in the first two areas you mentioned, and I'm sure there will be some in the fission product area.

But I think it is a sense of confidence, as I said. I've spent a lot of years around --

MR. KERR: Excuse me, there was a third issue that he raised which you didn't mention -- external events.

MR. BUHL: I said I know that particularly in the earthquake outside the PRA, what one can do with external events is look at the accident sequences which are initiated by external events. There's a separate body of regulation dealing with sabotage, for example, so we have not, in the IDCOR program, folded in an evaluation of that program or of

sabotage. We are not doing seismology in the IDCOR program. There are separate efforts in that area.

What we have done is captured those accident sequences which can be initiated by external events. That is not to say that there is not and should not be work continuing in these other areas, but I think the nonetheless, one can still make fairly confident decisions in the next year in all three of these areas.

MR. KERR: Other questions? Mr. Siess?

MR. SIESS: I guess I didn't understand your answer about the containment. This is a special area of interest to me partly because I'm a structural engineer, but chiefly because if the containment works -- and I'm not talking about structurally; I'm talking about works as a containment -- you can do an awful lot of damage to the core without doing any damage to the people. And you answered that question about containment in terms of challenge to the containment, and I didn't really understand what you were saying.

What is the question there? What is the decision about containment that you need to get information on?

MR. BUHL: I think the set of questions you're now asking are going to be much better answered if we walk through these technical programs one by one. We could continue this dialogue if you like. It's going to take a while because I'm now starting to go through Mario's presentation. I think it

might be better if we hold that and take that as part of the technical grouping.

MR. KERR: Is that satisfactory?

MR. SIESS: Okay.

MR. KERR: Other questions? Let me go back to your earlier comment about the parallelism between your program and the NRC's to see if I understand. It had been my impression that the IDCOR program was counting primarily on existing information and that you didn't really have a very significant research program to try to develop new data on physical phenomena, for example. Am I correct?

MR. BUHL: It depends on how one makes that measurement. If you're talking about comparing our research program to the NRC's, it's rather small. But we have people like EPRI --

MR. KERR: I'm not trying to criticize; I'm trying to understand.

MR. BUHL: One of our general premises is early on we recognized we had some limitations, and, in fact, one of our early requirements was to utilize to the fullest extent possible existing information, and we have done that.

MR. KERR: My understanding of the NRC research program is that a significant amount of resources is being allocated to experimental research. I just wanted to make sure that you and I have the same perception of the program.

MR. BUHL: I think that's correct.

MR. KERR: It, therefore, seems to me you are not quite proceeding from the same premise. One group thinks that a great deal of experimental research is needed. You can talk about confirmatory research if you want to, but this is tax-payers' money on the one hand, and rate payers' money on the other. I mean, we're all paying for this.

And it seems to me the NRC's research program is significant enough, and it surely is not viewed by the NRC as a luxury. It's viewed as something that is needed. Whether you agree with that or not is moot at this point. But it seems to me there is a rather significant different in the approach. Am I missing something?

MR. BUHL: No. My point was the basic technical logic of the program. I think there are great similarities. Certainly, the NRC is running a big research program and we are not. A big experimental research program and we are not. In fact, our approach has been to utilize the great wealth of experimental data which already exists.

MR. KERR: This was my understanding.

MR. SIESS: Why isn't NRC willing to utilize that? It seems to me there must be a difference between IDCOR and NRC and their perception of how much information is needed in order to make a decision. NRC apparently thinks a great deal more information is needed than you do, unless, of course, the two of you are talking about different decisions.

Are you in agreement with NRC on what are the decisions that have to be made, or which information is required?

MR. BUHL: Well, I don't want to stand here and present answers for NRC. I would recommend you ask NRC.

MR. SIESS: We have, and we haven't gotten an answer. I thought I could get it from you.

(Laughter.)

MR. BUHL: Six or seven years ago I might have been answering for NRC, but no longer.

My perception, at least in talking to NRC, particularly the staff management, is that --

MR. SIESS: Would you clarify that as to whether it's NRR or Research?

MR. BUHL: Both. That they are committed to at least the attempt to assure that the information necessary for making the set of decisions which have to be made is available in the next year. There are long-term research programs, particularly experimental programs, which they perceive they need --

MR. KERR: What is the starting point for the next year?

MR. BUHL: Mid-1984. I think that's what they've said in their documentation, particularly in their SECY 82-1-B, which is out for comment.

I think there's not so much difference in the logic or in the need for the decisionmaking. I think NRC has focused very strongly, and perhaps should be in what I call confirmatory research. That is,beyond making the decisions, there are uncertainties which could cut both ways. That is, uncertainties which after long examination one could come back and say those decisions need to be modified in this particular area. There are certainly uncertainties which may come back and say our decisions were too stringent, and we need to release those decisions in some areas.

So I don't think it's in total conflict, but NRC has a longer-range program with more experimental research. I don't think that's necessarily in direct conflict with what we're saying.

I think the concept that's most difficult is one that says one can make decisions and can assess that this is the amount of information I need for those decisions, and one can agree that there are questions yet to be answered, there are uncertainties yet to be resolved beyond decisionmaking, and those uncertainties -- when one gets to that level -- often require a much longer time period for evaluation.

So I don't think one has to read this as total conflict.

MR. SIESS: You used the expression, "set of decisions that has to be made." Will we see later in your presentation a slide which lists what IDCOR thinks are those decisions that

have to be made?

MR. BUHL: I don't know if we have the slide that says the decisions that have to be made. I don't think we do. We have sent to the agency on two or three occasions a long list --

MR. SIESS: How do you develop a program to provide the information to make decisions if you don't know what the decisions are?

MR. BUHL: I challenge your presumption. You didn't let me finish the sentence. Rather than attacking my words, I'd like to finish the sentence.

The point I'm making is that we have sent to the NRC to Mr. Ross -- we can provide those letters to you; they're in the public record -- the decisions by issue which need to be made. Those are on the record; I just didn't bring this on viewgraphs.

MR. SIESS: I'd very much like to have those.

MR. BUHL: We'll certainly give them to you.

MR. SIESS: Are those the same decisions the NRC Staff is trying to make, or are these the ones you think they ought to try to make?

MR. BUHL: These are the ones we think they ought to make; otherwise, we wouldn't suggest them. But we do know, based on our meetings, specific kinds of decisions that they're talking about. There are certainly decisions that we think

should be made that perhaps they don't. So that kind of question is really the basis of the management meetings we're having with NRC; to struggle with these decisions and try to define them as precisely as we can so those decisions can be made. Some are obvious and evident; some are much more difficult.

MR. REED: Let me just add to that, there's one basic question that we have gone after, and that is, should the reactors being designed for severe accidents with design features beyond the current designs of the plants, and that's the basic decision.

In trying to reach that decision, we've tried to get a better understanding of the phenomena and the information that exists, and as we look at those and issues arose, we looked further. We hope we are going to have some bounding kinds of conclusions. We've done what we hope to be some realistic analysis, but in the case of source term we're not going after the greatest source term that appears to be out there; we're going to try to take advantage of one that we think is supportable with the data that exist.

Now we think in a couple of years there's going to be more information to show that what we have used is going to be conservative. So that's our approach.

MR. KERR: Well, Mr. Reed, let me follow up a bit bit in order to try to better understand. You mentioned that

one of the questions you were trying to answer, I think, -- the NRC was trying to answer -- is should analyses go beyond existing design basis accidents. The minute you mentioned source term, it seems to me you're going beyond design basis accidents, because the only source term currently used in the design basis accident is a completely unrealistic TID-1484; an realistic source term which is not done on the basis of accident analysis at all; it's arbitrary.

So if you're talking about the kind of source term that's in WASH-1400, that is not used currently in design basis accident analysis. So have you already concluded that the answer to your question is yes, we do have to take into account some accidents beyond the design basis accident, we have to take into account accidents serious enough that one has to look at what the source term is.

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MR. REED: We have looked at all sorts of core melt down analysis, and not only looked at the probability of those but we have looked at the consequences of those in order to see what source terms and what sort of challenges to the containment we would look at. I don't know if that answers the question, but we have --

MR. KERR: But you have not yet reached a decision as to which, if any, of these should become something like a design basis accident, in the sense that you design your plant or your training program or your operators or something

to deal with it. You have not yet reached that sort of decision?

MR. REED: We have not, but that's what we're going to evaluate, and the way the program is structured, you have all of these -- and again, I guess we're going to make Mario's speech before he ever gets up here; I sure hope it's good -- but all of the paths are now converging unto the point where we now evaluate and look at these things. And I might tell you, too, we have some reservations in giving all of the conclusions.

We have said to ourselves we're not going to put out draft reports; it's going to go through a full review process, our expert review groups, our senior consultants, our steering group, before we slip out these little bits of information. And we're going to brief our sponsors and we're going to let them know before they read it in the newspapers.

In our presentation today we're going to tell you as much as we can, but there will be some things that we've asked the program manager don't just conjecture.

MR. KERR: It's part of our job as an advisory committee to unscrew the unscrewable, and if we probe occasionally, it's for that purpose.

Mr. Buhl?

MR. BUHL: I think at this time I'd like to introduce Dr. Fontana, from whom we have promised much.

(Laughter.)

MR. FONTANA: My name is Mario Fontana. I'm responsible for pulling together the technical cast for IDCOR. I'm going to go through a presentation on what our technical program is, what the logic structure is and where we are to date.

We also have with Mr. Terry Tyloer, and Dr. Ed Fuller from EPRI assigned to the IDCOR program at TEC. I'd like to overview the program and give you some of the highlights but I should indicate that the program is not complete yet. We're in the process of some of the final calculations, so we don't have all the answers.

Als-, as Tony mentioned, we are setting up -- we're trying to set up a series of meetings with the NRC in which we can come to some kind of discussion and agreement on these issues. Those will go into some technical depth with NRC and its technical contractors, and I presume the ACRS would be invited to those meetings.

(Slide.)

MR. KERR: Those are not yet scheduled?

MR. FONATANA: No. Correct, they're not.

(Slide.)

The overall technical approach that we're trying to take here is, first, to try to develop some generic evaluation criteria, and what this really means is some kind of a measurement by which we know -- some kind of measurement to let you know if you've done enough. Now, what this really means is

safety goals. IDCOR has not developed any safety goals, but we have tried to structure the program and tried to structure as many of the answers as a problem that we've gotten in the program so that they could be compared to safety goals. It hasn't been entirely successful, though we've gone quite a ways in that direction.

We needed to select representative reference plants. Much of our work we tried to do as generically as possible, but there always comes the question of trying out what you've developed in some real situations in some real plants. To do this, we've picked four reference plants which I'll get into later, which we think are fairly representative of the plants out there.

MR. KERR: What is meant in this context of their being representative? Does that imply that one should be able to use the results as a rough indication of what other plants might do, or that you simply chose four plants because you had to be specific?

MR. FONTANA: I'm going to get into how I chose those four plants later. You may want to repeat the question then.

MR. KERR: Okay.

MR. FONTANA: We had two -- we wanted to identify dominant accident sequences which can result in degraded core as if let to proceed. The main point there is we didn't just want to assume that you had a degraded core or core on the floor

and what are you going to do about it. The situation by which you could get to that condition had to be traceable from a logical initiating event.

Then it was important to characterize the reactor behavior as realistically as possible. We're not trying to say that we allowed the calculation of reactor behavior right on the nose, but it's the best we could do in the timeframe that we had and within the data base we had. And in some areas, there may be some conservatism built into these, but we think that this -- we're getting a best effort analysis of reactor behavior, and this is as opposed to a conservative analysis every step of the way.

We wanted to assess the opportunity for reducing risk throughout these sequences, and what that really means is these sequences take time to proceed, and we wanted to look at things that could either prevent the sequence or terminate it in the time window in which the opportunity presented itself. And again, inasmuch as possible, we wanted to relate our results to safety goals.

(Slide.)

Now, as Tony showed, the program has 24 tasks and 45 subtasks, broken down into four general areas. The current risk status, core damage prevention, containment effectiveness and mitigation. In the current risk status we looked at existing PRAs. I should point out that IDCOR has not done much

in the way of PRAs or in the area of new information on probabilities. However, we have sorted the existing PRAs and tried to come up with some kind of assessment of the risk as perceived by reading the PRAs that existed before IDCOR.

MR. KERR: Excuse me. Tell me if this is the wrong place to answer this question and I'll reserve it if necessary. But I'm trying to understand how you used the existing PRAs.

For example, in your evaluation of vessel rupture as a candidate, did you modify your perception of these PRAs to take into account current concerns about pressurized thermal shock?

MR. FONTANA: Well, the pressurized thermal shock -- not so much. The reason is we essentially ignored PRAs when it comes to consequences. We summarized what the PRA said with respect to probabilities. With respect to pressurized thermal shock, we did not bring that back in for two reasons. One is that the area is being covered in other proceedings in depth. We're trying to stay up with it.

The other is that if you look at some of our sequences, we do end up in some places in sequences in which they envelope the situation you get into, if you get into pressurized thermal shock. So the answer to your question is yes and no. I think we covered some of the consequences of that, but we didn't change the probabilities of getting into some of these severe accidents because of that. It's covered in other areas

and also, there is a --

MR. KERR: The point I was making is it seems to me if it is a concern you have to decide that you think it either does or does not change the probability of vessel rupture. If it does not, you've made that decision and you go ahead. If it does change the probability of vessel rupture, then it seems to me one also has to look at the possible consequences and the product of the two. And to either, it seems to me, make an implicit decision that it doesn't matter or you ignore it -- or an explicit decision that it doesn't matter and you ignore it. I was just trying to get an idea of how you treated the existing PRAs in terms of newly-developing information.

MR. BUHL: Maybe I could help. As Mario said, -- your example is a good one -- we didn't go back and modify existing PRAs. What we did was to go into the bottom, to particularly, containment effectiveness, and ask ourselves for this particular sequence, do we have the driving forces, if you will, on containment covered. And in that context, the kind of questions you're asking were covered with our analysis, but we didn't go back and ask ourselves on Zion or Surry or some other plants, how does this particular new phenomenon, be it pressurized thermal shock or whatever, affect those probabilities. So we didn't modify existing PRAs for that.

We did try to cover those particular events under the

envelope of the analysis we do in the containment effectiveness block. I think you'll see that as we march through these one by one.

MR. KERR: Thank you.

MR. FONTANA: We had a task to look at ways of preventing core damage. We had a task on containment effectiveness which could be re-read as behavior of plants under servere accident conditions, and a cluster of tasks looking at some of the mitigation views that have been presented from time to time.

(Slide.)

Now, the 24 tasks are shown in here. We're going to go through some of them. The numbers that you see here are task numbers; they're not particularly important, but the logic of the tasks is. Now, these tasks are not arranged in chronological form or anything like that. They're arranged as work areas. Some work areas would feed others.

First, we had to develop groundrules which we have done, and those, of course, were made available. We went through the existing PRAs and selected the dominant sequences, what sequences appeared to be important ones, and I'll get back to that a little later. Then we selected the phenomenological sequences, the sequence of events. In other words, containment event trees, exactly as it proceeds with time. And tried to sort out which sequences and which phenomena appeared to be

important on the basis of existing PRA. Of course, we modified that as we got farther on into the program.

(Slide.)

Also, we thought it would be interesting to look again at the existing work and tried to identify, on the basis of pre-existing work, what the human error contribution to risk and core damage probability was, and what the contribution due to important equipment systems. And we find here that something like 50 percent of the sequences are affected by the human in one way or another. And in the equipment risk profile -- I'll get back to this -- we find there are some pieces of equipment that are important with respect to risk, and you already know what some of them area. They are equipment that provide water to the core and decay heat removal.

Then we tried to -- again, looking at the existing PRAs -- sum up a baseline risk; what is the risk presented by the plants that are out there today, on the basis of what was known at that time. And then, we are just barely completing right now a look at post-TMI changes, those changes that were required after the TMI accident, and tried to summarize what they were and the benefits of them.

(Slide.)

And then we reach a cluster of tasks which shows up several times in this logic chart. This is task 21, which is a risk reduction. This is the measurement of the change in

risk that has occurred because of what went on before. In other words, here we look at the post-TMI changes, and this task is to look at the post-TMI changes and see what change in risk, benefit or detriment was caused by implementing those changes.

At this point, it is logically feasible to compare the risk levels with the safety goals and go to a technical position, or if you learn something, go back and iterate to the identification down to the sequences. I'll get back to this.

MR. KERR: Will the reports finally identify any of the post-TMI 2 changes that increased risk?

MR. FONTANA: I really don't know what the report is going to say.

MR. KERR: I wouldn't be surprised if you didn't find some increased risk, since I think some of them were made without taking it into account explicitly.

(Slide.)

MR. REED: That's interesting. Let me ask you a question. In what area -- we're comparing post-TMI changes.

MR. KERR: I have not done the quantitative analysis. I'm simply saying that these decisions were made, in some cases without quantitative analysis being done, and I would therefore be surprised if it didn't indicate that risk was increased. And I wondered if you had identified any such cases.

I didn't expect you to tell me now necessarily, but I would hope that the report, when it finally comes out, does

identify them, and indeed, if --

MR. REED: Yes, that is one of the objectives.

MR. FONATANA: Our contractor is working on that one right now, and that's the reason I can't give you an answer.

We have a task which looked at core damage prevention methods, and I'll get back to some of the results of this. That has really not been completed.

Here we show two tasks which show two other times, and that is, an identification of safe, stable states; a state at which the reactor could be stabilized and the sequence prevented from getting any worse. The application of that safe, stable state in this region is trivial because that is the stabilization of the reactor in a condition for which it was designed and built.

Also, there's a task which shows up again -- operational aspects of accident management. That is an attempt to look at things that could be done operationally to prevent accidents or to keep them from getting worse. These that are related to core damage prevention are, of course, closely related to the normal operating procedures of the plant, so we didn't repeat that sort of thing, but it does show up later.

MR. KERR: I'm interested in the arrow that goes to the safe, stable states, because it seems to me it's extremely important. When we talked with the French, the ACRS some months ago, they described some things that they are studying

which might be procedures to be used in an extreme situation in which one would take rather extraordinary measures to try to effect cooling. Are you looking at that sort of thing, maybe bringing fire hoses to put water in somewhere or other?

MR. FONTANA: Yes. Both the procedures would be in here and the identification of ways of getting -- when we thought about this and worked all the way through it, it ended up being a workshop for people who are supposed to know what they are doing and who knew their plan, and it ended up being a workshop of ways of getting water into the core.

MR. KERR: So you are looking at it in some detail, at possible, rather extreme procedures?

MR. FONTANA: A lot of them are not particularly extreme.

MR. KERR: No, I mean you'd use them in extreme. situations. They're perhaps normal equipment or systems.

MR. SIESS: Are you doing that only for your four representative plants?

MR. FONTANA: That's part of it, because it's pretty plant-specific. We're doing it with the four reference plants and somewhat for three other plants that we call representative but not reference, which I'll get into in a minute.

More with respect to plant specificity -- I think as you get -- as the accident sequence gets more and more severe I think they become more and more generic. I think things you

can learn about the plant behavior in a severe state is pretty applicable to a broad range of plants out there. The more you go upstream in the sequences, the more you're into the nuts and bolts of it because there are more specifics. So this one can be fairly specific in talking about how do I route specific valves or specific pipes to get water to a specific place that maybe it originally wasn't designed to get to.

MR. SIESS: I know that's gone through. I'm looking at the SEP plants which are usually not looked at as far downstream as you're talking about.

(Slide.)

MR. FONTANA: We have recognized that when you're really talking about hands-on, nuts and bolts. Now we get into the area of behavior of the plants under severe accident conditions. And the first thing we do is try to gather what knowledge exists out there on the important phenomena. Now, as Tony mentioned, one of our groundrules was to try to use existing data to the maximum extent possible. In a two-year program we really did not have time to start any big experimental programs. We did feel that we could put together some new rules, which I'll discuss in a minute. And we did feel that we could put together at least what was known at that time on the important phenomenological issues that affect the plant behavior.

An important one, of course, is fission product

transport. This has several subtasks in it. One cluster of tasks pulled together by EPRI was release of fission products from fuel in the region just above the fuel before it really gets into the primary system. That gets into multi-component chemical equilibrium, the vaporization phenomena and the conditions which may lead to aerosol behavior in subsequent transport.

This task also looks at the chemical -- the inherent retention of chemical behavior under those conditions where you might have water available and that sort of thing.

Another task in here looked at the fission product transport from the region just near the fuel through the primary system and into the containment. And we pulled that together and developed a code that analyzes that behavior.

We also looked, as part of this task here, at the potential sneak paths; paths that could exist by things being left open and so on in a reactor system, other than a path to the environment that would exist as a normal outcome of an analysis of severe accident progression.

MR. CORRADINI: I wanted to ask here -- I asked earlier about uncertainties. Where do uncertainties fit into the physics here?

MR. FONTANA: I'm going to get into that. I'll get a little ahead of myself.

MR. CORRADINI: You don't have to. I'm sorry. If

it's coming later, then don't.

MR. FONTANA: If I try to dodge it, you'll ask it again.

MR. CORRADINI: Don't worry.

MR. FONTANA: There's a cluster of tasks on hydrogen generation and burn. Down here in the core debris behavior is some tasks looking at core heatup and circ oxidation, and the hydrogen transport,mixing, flammability limits, detonability limits and that sort of thing were done in this task up here.

This task, we were looking at steam over-pressure and here we looked at those conditions which could lead to steam explosions and also, to steam spikes. And here we looked at the phenomenon affecting core debris behavior, and that involved heatup of the core and zirc oxidation, and it also involved the development of some fairly development-complex heatup codes out at EPRI.

In a ddition, we looked here at the behavior of core debris as it might fall in the water. There's an important factor here; that is, the debris dispersal behavior. The size of debris you end up with, and we tried to come up with some conclusions on that and also, the coolability limits of core debris. Given you can get water to it, what are the limits in power generation at which you can still cool it?

So now we've pulled together -- these have free-standing reports that describe what we did. And in many

respects, we feel they are generic. We feel that some of these conclusions will hold up, regardless of what plant you're talking about.

MR. CORRADINI: If I could ask a question, and again, if it's not the right point, tell me to stop -- but you mentioned in the fission product transport area that you developed a computer model, then you mentioned under 14 that you developed a computer model. And I gather all of these things are being integrated into this MAAP code.

My question goes back actually to something that I think Professor Lee asked earlier, and that was -- again, I'm not going to quibble about the computer model as much as what prompted you to develop a different computer model, and then to suggest alterations to current models from a technical standpoint.

MR. FONTANA: I will get to that, but I'll answer some of it right now. These important phenomena we're trying to gather into our computer models in this task here I'll call integrated model development. This has three subtasks -- well, more than that now. The subtasks were, one, to look at the codes that exist out there now and try to see what phenomenon we felt were worth incorporating, what were the problems with the codes, could we use them, and that sort of thing.

The second subtask involved pulling these phenomena together into the code, and the third one was writing the code.

Now, you asked us why did we decide to write a code.

MR. CORRADINI: Let me get specific. This essentially assumes that any control volume is adiabatic, so there is no other transfer radially. And any flow-through is essentially just convective flow as the only heat transfer between those. I know that it's not necessarily hard, but again, not necessarily easy, to include in that radiation transfer both radially and axially, to include the effect of radiation transfer within the heat volume.

I know you mentioned the EPRI heat-up code. A lot of those physical effects are included in that, and my basic question is why not take the BOIL with separate equations and include those effects in it, rather than rewrite it in another computer program.

MR. FONTANA: We were considering that. We decided that what we really wanted -- first of all, what we really wanted was -- MARCH was not designed in the best way for alteration. It's not the best logical code in the world as far as computer logic goes. And we decided we wanted a simple executive code. In fact, we started off calling it the IDCOR executive analysis program, and people got it mixed up with psychiatry.

MR. KERR: The executives didn't want to be analyzed.

(Laughter.)

MR. FONTANA: It was monitored, and an executive

code -- and it had to be modular, it had to be built in such a way that you could pull the model out, put another one in. If you didn't like our debris model you could put another one in, and that sort of thing. So we looked at the alternatives and decided that we might as well start from scratch rather than try to modify an existing code that wasn't all that well-designed to start with. We could start from scratch.

MR. CORRADINI: It was just a matter of time and money. It would have taken less time and less money to essentially start again than to alter.

MR. FONATANA: That was our guess. And, of course, if you like BOIL, there's no reason in the world why you can't put BOIL and consider its compatibility and plug it in here in one of the subroutines.

MR. CORRADINI: What you just said sounds very much like the justification I've heard for developing MELCOR after MARCH.

So then one could, using the logic I understand, one could quite conceivably take the core heatup model that the laboratories are developing for MELCOR and plug it into MAAP, take in the model for the reactor cavity response and plug it into MAAP and see the differences.

MR. BUHL: We evaluated BOIL, and I think our people are very familiar with the whole MARCH series. And I wouldn't want to leave on the table that it was just simply a

cost-time-effectiveness study that led us here. That's not totally the case. We have no reason to attack or cause problems with BOIL, MARCH or anybody else therein associated.

However, what we wanted was a computer code based on the fundamentals which we felt could be used on a timely basis and could provide some long-term reasonable usage by the industry so that when we did this evaluation, as Mario said, we looked very hard in the first step of this to say can we use the code out there. It was our judgment that using the codes would have led to a great deal of confusion and slowed down our program, so we simply developed our own computer codes.

You're quite right, the logic that led us there is the same logic that led NRC, from my understanding, to go forward with the longer-term program called MELCOR.

MR. KERR: But the comment or question was, does it follow that you can readily compare MELCOR with MAAP almost segment by segment, by plugging one in and seeing what happens? If that's true,then I think it's very good.

MR. BUHL: I think it is, too, but it's very hard for us to judge because we haven't seen MELCOR.

MR. KERR: It would be nice if the people developing the two could keep that in mind.

MR. BUHL: As I mentioned in my comments, we have established a program at NRC to do some bechmarking, but primarily that's going to be done with the MARCH 2 kinds of

things as opposed to MELCOR. I don't think MELCOR is going to be availabile.

MR. CORRADINI: If I believe what they tell me, it's going to be available at the end of 1983. But I want to go a little further,though. I want to ask one other question, and that is, I sense there's a difference in philosophy, though, because to develop a computer code on the level of what MARCH is supposed to do in a modular structure -- I was looking at your timetable here -- in a matter of about a year and a half,is a phenomenal amount of development, and it strikes me that the philosophy of the way the computer models are put together must be intentionally different than what MARCH is. Intentionally different in the sense that MARCH has the unique or the bad aspect that given the fact that we have balanced mass, momentum and energy, assuming they do that, that you have the capability to put in fudge factors to partition the energy and movement of mass in a number of ways, and essentially, you can produce the answer that you may want to produce given the calculation and what you set out to do.

I think this is a totally different approach here, and that is, most of the physics that you think may be going on is hard-wired in to the computer program so that if the user gets on and tries to see what's going to happen, given the fact that he looks at the same PWR with the same ice condenser or the large dry, he's not going to see a whole heck

of a lot of difference in terms of accident response.

MR. FONTANA: You're correct in a sense in that it's more hard-wired than MARCH, and it was deliberately done that way. If you know what you're doing, you can get in and change the code, but we wanted to try to make it so that only people who know what they were doing could do that.

The code will allow you to put in -- it has a range of allowable parameters it will allow you to put in, and if you try to put in a number that the writers of the code felt was not physically feasible or rational, the code will kick out, it won't run. That was built in. You can do sensitivity analyses with it, you can do uncertainty analyses where you can vary within the bands of uncertainty what you don't know about the physical phenomena, but it won't let you go outside into a region; that is physically impossible.

Now, you can do sensitivity analyses and look at the effects of physically impossible things by specifically reworking the code to do so. I'm going to get back to this and why we laid into this.

Okay. What does MAAP really do? It calculates the core heatup, the zirc oxidation, the hydrogen generation. It sort of ignores what goes on inside the core when you get into a lot of melting, because that's a highly complicated area that we felt really wasn't worth all that much because we feel that the results are dominated by what happens after the molten debris falls into water.

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Now the question is how much can the containment withstand? We had a task called containment structural capability, and we pulled together about 20 experts in this area who had designed reactor containments and knew a lot about them and asked them in their judgment to come up with an assessment of what the ultimate capability of these containments are for the various kinds we were looking at. I will get back to that.

MR. SIESS: Excuse me. Before you leave that, I think it is obvious that if the containment failures structurally, it will leak. The converse is not true. Is there a box somewhere on here that talks about containment integrity or containment effectiveness? I have been looking at this diagram and trying to draw a line that says to the left of this line everything is still inside containment, and to the right of this line, things start getting out into the environment, and I cannot find a way to do that.

MR. FONTANA: I will get back to this a little bit, but I will get ahead of myself, and that is, the experts that we have pulled together, depending on the containment, seem to feel that the containments could stand most of the time close to four times design pressure.

Now, how did they arrive at this?

MR. SIESS: Without leaking? Without leaking?

MR. FONTANA: Well, let me get to that.

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MR. SIESS: I know all about the structural capacity of containments, but I'm concerned about their effectiveness as a containment of radioactive material.

MR. FONTANA: I'm aware of that. They felt that once it went to 2 percent strain, that was good enough as far as they were concerned because the strain would increase so much that that was enough.

With respect to leak-before-break, they had a problem. They felt fairly confident that an unanalyzed concrete containment would probably leak and reduce pressure. They had a problem, however, with steel containments. Everybody feels a containment is going to leak before it breaks, but they would not pin themselves down and say, yes, they could show categorically that the containment is going to leak before it breaks.

So there are several ways of looking at this. The energy and the steam generation rate and whatever else occurs in here during the accident is such that if you got the equivalent of a hole 4 inches in diameter, you could preclude further pressure buildup. Now, that gives you a problem on how do you handle the release.

One way of handling it is if you assume the thing leaks, it is going to leak to the point where you get this amount of release and further pressure buildup will not occur, and that is how you calculate the release. If you

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have to assume in some areas that the thing is going to pop, then you calculate a "puff" release.

Now, the key thing here is the time to failure. For most of our sequences it takes a long time to failure, and the question is what has happened to the fission products in this time period. We think that you are going to find a significant amount of those fission products have either dissolved or plated out and that the amount in the gas phase available for release is significantly less, or certainly less than what is in TID-1484.

There is a residual question here on re-entrainment in the event of a real massive failure.

MR. SIESS: Which of those boxes addresses the leak-before-break?

MR. FONTANA: They tried to address it here and could not really come to a --

MR. SIESS: Is it somewhere in that integrated model of containment analysis?

MR. FONTANA: We are trying to look at it. Also, we are having a consultant. We are trying to have a stress analysis done with a consultant taking a look at this, but we are not promising anything.

MR. SIESS: Incidentally, in passing, are there proceedings of that workshop you had on containment available?

MR. FONTANA: They are available within IDCOR.

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They will be printed pretty soon. Do you have it yet, John?

MR. SEGAL: I got them for printing about three days ago, so in another month or so, we will have them out.

MR. SIESS: I would appreciate getting one.

MR. FONTANA: These do go to the IDCOR sponsors first, and when we set up our NRC interaction meetings, you should be getting it pretty soon.

MR. KERR: I haven't heard you mention penetration failure? Is that implicit in this leak-before-break?

MR. FONTANA: Yes. Most of the experts looked at penetration and decided they were so well designed that something else would go first.

MR. SIESS: What about this isolation failure, the 66-inch butterfly valve that is halfway open? What box would that be in? What task would that be in?

MR. FONTANA: There was a task called 11.2 in which we looked at "sneak" paths. We looked at potential paths out of the containment. And that is not done yet.

Terry, do you have any light to shed on that?

MR. TYLER: This is Terry Tyler. In the review of that "sneak" path report, just the butterfly valve alone being opened may or may not create a potential problem if the pressure boundary of that system that butterfly valve is in remains intact. For the most part the results show that with the exception of indication in a number of the LERs

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already in existence today where paths have been opened, that there are no "sneak" paths that we can come up with other than those that have already been identified, and I think you will find that the integrity of that remaining barrier is essential as to whether the fission products or whatever escape to the atmosphere, and we don't think there will be enough driving force at this point in time for that to be a driving path.

MR. FONTANA: Here is where we took our tools and put them to work in this containment analysis. I am going to get back to this in a minute. We put together teams to do the analysis on each of our four reference plants, and I will get to that in a minute. They came up with the temperatures and pressures and fission product behavior and so on for each of the plants for the important sequences.

(Slide)

MR. LEE: Before you get off that general diagram, could you perhaps characterize your executive analysis package as the best estimate calculation package or more of a bounding calculation package?

MR. FONTANA: First of all, it is not a highly complex number-crunching code. It is based on phenomena, but it is based on the best judgment of the best people we could find that were available who had some knowledge of these phenomena. So I would have to say on the basis of the

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knowledge that exists today, that is is probably the best we can do, except there is a matter of judgment here. There are some areas which don't particularly affect the answers all that much, and in those areas it doesn't really matter if you are a little bit conservative or so on. But the risk is basically -- it's basically a risk code. It's not written like RELAP or some of the number-crunching codes.

MR. CORRADINI: I think all John wants to know is is it bounding in the sense that one looking -- I will give you a key example. When looking at steam explosions, do you say that they can occur and you look at thermodynamic maximums or you try to do the best estimate as to what the physics is?

MR. FONTANA: The bounding part of that is the debris model. The debris model is such if you show your debris sizes beyond a certain point in the surface area, that you can get enough energy into the steam to have a really big explosion or big steam spike. So the bounding part is the conclusions that were made with respect to the debris sizes that fall into the water fragmentation.

MR. CORRADINI: So it is bounded.

MR. FONTANA: With respect to steam explosions we are not saying there is not going to be a steam explosion. It appears that the results are saying that the steam explosions we are likely to get don't come close to

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challenging the structures of the cont inment.

MR. BUHL: To directly answer your question, the intent was to develop a best estimate code, not a bounding code. What we found along the way is that you can do a best estimate on a great deal, but you come to points along the way at which you say the data doesn't justify this, so you take a more conservative bounding step. And when we have done that, we have tried to identify those steps. And as Mario says, we think by and large those are not the steps that you could dictate to the decisions. But they will fall where they may. I think that is what you do with any code when one starts down this path.

Incidentally, the interesting thing is the NRC Staff was the first to warn us not to call it best estimate, but we have made bounding assumptions in certain places and point those out, so we are trying to do that.

MR. LEE: Could I just ask one other question? How large is the code and how large do you think it will turn out to be when it is all packaged and wrapped for user-friendly configurations?

MR. FONTANA: Do you have any idea of the size of this? He can answer it better than I can.

MR. FULLER: This is Ed Fuller. The code in a number of statements is very large. I don't know exactly the number, but probably in the order of 10,000 to 20,000 lines.

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However, it has been structured to be user-friendly, fast running, and actually take relatively storage. It has been developed on a FAX machine, for example. It will run faster than real time on the back so that when you go to the main frame machines it is much faster than real time.

MR. LEE: Thank you.

MR. FONTANA: John Roston. Where is John? How long does it take to make a run on TVA computers, about? I think it is on the order of a minute or so.

MR. ROSTEN: I think the only recollection I have is we ran 25 cases in one day. We found it to be a very user-friendly code.

MR. CORRADINI: Let me ask the next question I am going to ask anyway later on. I have got a 20,000 line code, and a lot of physical processes are hard-wired in and I want to go change it. How easy is it to go in there and find the physical process and change?

MR. FONTANA: Extremely easy because the way it is written, it's written -- the words and commands are written in a form that will bring to mind the phenomena you are talking about. In other words, it is as uncodified as possible. The statements for the parameters are close to what you are talking about, like "temp" for temperature, and things like that. For you it would be a snap.

We have a task here. Now we have calculated the

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temperatures and pressures and all that in the containment system. Now we have a task in which we looked at the survivability of equipment that is in there that you may depend on. This is not equipment qualification; this is an assessment of how we think the equipment may survive under the severe accident conditions, and I am going to get to some of those results.

Again, now that we have gone into severe accident configuration, are there safe, stable states? What can you do operationally? And here at this point, we do an atmospheric and liquid pathway dispersal calculation using the CRACK code. You see the sequences to identify a realistic sequence, do an analysis, get the transport, do the atmospheric and liquid pathway transport.

(Slide)

And then see what the risk change is as a consequence of doing these calculations. Well, obviously there is no true change in risk. The system is the same as it always was. But what we have here is the results of what we feel is a better analytical technique and better tools, so this change in risk task here takes a risk that came out of these calculations and then compares what we thought the risk used to be when reading the old PRAs.

This task has different nuances to it.

(Slide)

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Here we got into the area of --

MR. CORRADINI: Again I am going to interrupt you. Can you go back to the previous one?

(Slide)

I guess the thing I wrote down here, the thing that kind of struck me, if I think of the PRA in three parts, the probability of the accident sequence and the effects of the source term and the general public consequences -- you choose to take past PRAs in whatever state they are and use them for representative plants. Then at the tail end you choose to use the CRACK code to do the consequences, but then you choose not to use current models for physical processes during the meltdown.

MR. FONTANA: That's incorrect.

MR. CORRADINI: You said you used CRACK at the tail end.

MR. KERR: Give Mr. Fontana a chance.

MR. FONTANA: We use the existing PRAs to get the first cut at the probabilities of getting into some of the sequences. Then we clustered the sequences and we did maybe half a dozen important sequences that blanket almost all of the possibilities you would get into and use the best existing phenomenology there.

Then we leak to the environment through the CRACK analysis here to look at the update on the radiological doses,

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and then there is a task here which applies at several places of, okay, what is the risk level now that we have done that.

MR. CORRADINI: I guess it was my fault; I didn't make the question clear enough. In the first part you used existing PRAs. In the last part you used existing computer model to look at consequences. In the middle part, which appears to drive the risk, or maybe it doesn't drive the risk -- you tell me -- you choose to write a whole new set of models under a computer program. And I guess -- I will go back again. Is it later on now you address the uncertainties? Because if the middle part is what you have changed, then it strikes me that one ought to, as I'm changing it, look at the effect of my changes from a sensitivity or uncertainty standpoint.

MR. FONTANA: The sensitivity and uncertainty we are looking at are mostly uncertainties in the physical parameters, and sensitivities in various assumptions you might make. We have not gone back and changed the probabilities of consequences. That's an iteration that would be done if you iterated when you closed all the loops, but some of the results that come out of here could change some of these probabilities up here. We really haven't done that.

MR. BUHL: The answer to "why" is we wanted to get a much better handle on the phenomenology in containment

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effectiveness. I think your question is really why did we not rewrite CRACK. That is the only piece we didn't rewrite. The basic logic was to take the PRAs as they existed, understand those in the ways we have described, and then to do better the understanding of the containment's response to all these various challenges by better understanding the phenomenology and by developing our own system of codes, including fission product transport, and then to use CRACK or a version of CRACK which we felt was sufficient for the tail end purposes.

We didn't feel it was required to rewrite the CRACK code.

MR. CORRADINI: I understand that. I don't mean it as a criticism.

MR. BUHL: The uncertainty is in this big box called containment effectiveness.

MR. CORRADINI: That's my point. The uncertainty was there, and that is where you chose to do your major effort in terms of investigating current phenomena and then writing in an orderly fashion. Then again -- maybe I am getting ahead of your talk, but where does the uncertainty fit in? Because if that drives the risk, then any change there is going to drive the results. So if you come to the conclusion that current operating plants shouldn't be changed because of your perception of the risk and therefore the

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effects of the accident are nil compared to previous calculations, I will go to containment effectivess and say it's the MAAP code that's doing that.

So my next question is: Is it later on that you are addressing the uncertainties, or somehow did I miss where you addressed them in going through that logic?

MR. FONTANA: You may have a different --

MR. BUHL: Let me answer it. We addressed the uncertainties in this box because we knew the parametric evaluations and uncertainty evaluations all on the physical phenomena, so it is in that box of tasks that we really look at the uncertainties. And as we say, we think by better understanding the phenomena and the responses to those that are driving the response, we can narrow down the uncertainty.

We don't do anything -- again to repeat myself -- to modify previous PRAs. So it is in this area that we feel there is a great deal to be gained, and it is really not the MAAP code that does it, it is really our understanding of the phenomena that challenge containment which does it.

MR. FULLER: Can I add a little bit to Tony's comment? This is Ed Fuller again.

The specific way we are doing it is based on the phenomenological modeling that is in MAAP in our containment analysis tasks that the four teams are carrying out. They are including as some of their calculations specific

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variations on some of the best estimate parameters, doing the calculations on those changes within the bounds of physical realism, now, and determining what the effect on containment response is.

MR. CORRADINI: The reason I am asking all this is I keep thinking back to what John asked at the very beginning, which was the perception that NRC seems to look at experiments and need for more information, and it is a longer-range effort than here. And I guess as I look at that analysis or the analysis group under containment effectiveness, it appears that -- well, to be blunt about it, that the range of uncertainties that you may think that you would choose would be substantially narrower than the range of uncertainties that NRC may pick in terms of what the physical processes will do and what the source term will be. I don't hear those words, but I am sensing it because you go straight down that path.

MR. BUHL: That's an interesting conclusion since you haven't seen our results, and I guess I don't know in great detail what NRC's range of uncertainties are. I will be surprised if they are as broad as you suggest, but I think at this point we would just like to leave that on the table.

Your perception is that our range of uncertainties is much more narrow than NRC's. That is not my perception, but I think it's something we want to look at.

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MR. CORRADINI: The only reason I bring it up is to hear what the real facts are.

MR. KERR: Let me pursue that a little. NRC has said within the last three months, I believe, certainly the last six, that they do not have enough information to describe the behavior of containment. I can find that reference for you. It is either in their discussion of safety goals, which I believe is where it is, or it is in 82-1-B. It is in one of those two.

Now, you apparently believe you do have enough information to describe containment performance.

MR. FONTANA: Up to a point.

MR. KERR: I am not trying to be critical of anybody; I'm just trying to understand if we have the same perception. I can find the reference if the NRC Staff means what it says, and I think that they probably would feel the same way if they felt that way three months ago. You are telling me, I think, that you believe you can describe containment well enough to make a decision. NRC said they couldn't describe it well enough to use it in safety goals. They are not tbe same thing, I realize.

MR. BUHL: I would have to see what NRC said in context. I'm not familiar with that particular language.

MR. KERR: Well, I would urge that you get it.

MR. BUHL: I think we will. The only language I

recall from NRC really had to do with some discussion, and it was during the safety goal discussion period, I guess, about experimental data or understanding containment effectiveness, and clearly if one had to make a decision solely on the basis of large-scale or full-scale experimental measurements, that is probably the case.

So I guess we will just have to say we will find those references and understand what NRC meant by those rather than to try to guess.

MR. FONTANA: As already mentioned, it takes -- and I will get into this a little more as I get into these later Vu-graphs --

MR. KERR: You may be doing the NRC a big favor, because if you have the information which permits this to be described, I think you could save them a great deal of work.

MR. BUHL: We think we have the information and we certainly have the meeting set up with NRC or in the process of setting up meetings to exchange that information.

MR. FONTANA: When it comes to the point of reaching the ultimate capability of the containment, then there are some unknowns.

MR. KERR: There will always be some, sure.

MR. FONTANA: But the thing is a lot of our results show it takes a long time to get to that point. Then we get

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into this area on mitigation. There are various things that have been proposed. We looked at hydrogen burn control measures. We looked at alternative containment system. This means filtered vented containment, largely on the work the Swedes have done, and looked at core retention devices and rested largely on the work that Battelle-Frankfurt had done. We looked at something like 50 different devices and compare them against the required performance requirements.

Then we again took another look at equipment survivability.

(Slide)

And again, we identified the potential for stabilizing the situation. We looked at the operational aspects -- will do. These haven't been done yet for the mitigation features, the atmospheric liquid pathway calculation.

(Slide)

And at this point one can compare the risk change as a consequence of implementing the proposed mitigating features: that risk could go down or it go up.

(Slide)

Now, backing up, when we started, we had seven PRAs to look at, and these were the ones that there were. Surry is the reactor safety study, Oconee, Sequoyah was a RSSMAP. Zion was a full PRA. Peach Bottom was a reactor study. Grand Gulf was a RSSMAP and Limerick was a full PRA.

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MR. CORRADINI: Just going back to those seven, who performed each of the seven? Were they all the same people?

MR. FONTANA: No. They were separate. I don't know who did them all. Surry and Peach Bottom were Rasmussen and that bunch.

MR. KERR: We could get that information.

Mario, would this be a reasonable place to take a ten-minute break?

MR. FONTANA: It would be real reasonable.

(Laughter)

(Recess)

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MR. KERR: We are in session.

MR. FONTANA: As I said, we looked at seven existing PRAs, and some of the things that we found out, just things that now are fairly well known, are that dominant sequences involve small-break LOCAs with late loss of core makeup for PWRs. That doesn't mean that early loss of core makeup is not important, but just looking at the way these were, the probability of early core makeup is not very high. So when the contractor looked at the probabilities of various things happening, they found some of the more dominant ones involved the small break with late loss of core makeup and transients with late loss of containment heat removal or with early loss of heat removal.

We looked at sequences where human effects are involved with 50 to 70 percent of the core damage sequences, and around 50 percent of the total risk when you sum up the sequences and the contributions to risk.

MR. KERR: How far back does human effects go in this context? Is it operational human effects or does that include design errors?

MR. FONTANA: I don't think it goes back as far as design errors or even as far as maintenance.

MR. BUHL: We did not go back and assess design and say here is a better way to design that plant here.

MR. KERR: No, what I'm saying is is there an

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assessment of human error in the design, or when you talk about human error contributing 50 percent of the total risk, at what stage is the human error? Is it by operators, by maintenance people, by designers?

MR. BUHL: It's really operations, which includes testing and maintenance. All of those are built in.

MR. KERR: But not, for example, design errors made by humans.

MR. BUHL: No, I can't think of an example.

MR. FONTANA: What they did is follow the sequences and identify where a human effect is involved.

MR. KERR: But human effect is involved if you have got some design error in a piece of equipment which makes it malfunctional.

MR. FONTANA: I don't think that is the case here.

MR. KERR: Okay.

MR. FONTANA: The equipment systems of high risk importance are the core inventory makeup systems for PWRs and the power conversion system, the high pressure water injection system and residual heat removal systems for PWRs.

MR. DAVIS: I'm a little surprised that your dominant sequences don't include the inter-systems basic LOCA. Is this a core melt dominant or risk-dominant sequence you have up here?

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MR. FONTANA: Both.

MR. BUHL: That is in there. It doesn't have to be on the Vu-graph. This is just a very kind of top-level cut. Event V is in all our calculations. We are looking at those kind of bypass sequences to be sure we understand those when you come down to containment response.

MR. DAVIS: I'm just surprised it wasn't there.

MR. FONTANA: It's on the master list.

MR. LEE: Can I follow up a little bit on the question? How much of these conclusions that you have listed on a tentative basis you think could be affected by studies involving the models in MAAP or containment penetration failures or whatever subsequent analysis that have to come are source term, for example?

MR. FONTANA: Well, it's a little hard to answer. I don't know that I really can answer that on how these various sequences might change. We have got them all covered in the analysis. I'm not sure when one becomes more dominant than another one.

MR. LEE: It's not clear to me, at least. I thought you said these conclusions were reached based on the review of seven PRAs. Isn't that correct? Then I thought that these results could be affected by what you assume about containment failures -- what you assume about source term, certainly. So you have to go back.

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MR. BUHL: You have to go back, but what you generally find, of course, is that two things can happen if you think of a plastic consequence probability curve. You either find that whatever modification or whatever new insight you have either pushes the curve down, that is, reduces probability, or you may find some, for example, like the treatment of fission product source term and its transport, which may move that curve to the left.

In either case what you find is that you tend to take certain dominant sequences and push those down, and if you push them down far enough, there are other sequences on your list of whatever number you have that may become dominant.

MR. LEE: But there is a high degree of non-linearity in imposing time sequences and consequences that you have analyzed. I'm not sure by just reviewing these existing seven PRAs you can somehow make a judgment which are the dominant sequences to start off with.

MR. FONTANA: No, no. What we did -- what this slide shows is what those PRAs said the dominant sequences were.

MR. LEE: I am perhaps ahead of your process, but the ultimate question can we believe these conclusions, how much?

(Slide)

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MR. FONTANA: Well, ultimately this conclusion is interesting but not absolutely essential because when we do our analysis on the behavior of reactors in severe accidents, we do it in such a way that it blankets many, many sequences, and with the sort of existing PRAs, just show that the sequences were important, and after IDCOR I don't think it is really going to change that much except the consequences, I think, are going to be lower than we thought before.

MR. LEE: I am certainly interested in learning a little bit more in detail about how you arrived at some of the conclusions that you said you have reached, not the PRAs, but these conclusions regarding results from PRA reviews are not as material as you thought. I would like to learn how you arrived at that particular conclusion if you can help me later on.

MR. FONTANA: I'll try.

MR. LEE: Did I make myself clear?

MR. BUHL: Not to me. I'm sorry, I don't understand the question.

MR. LEE: You said the conclusions you have reached based on the PRAs are not as important for your overall risk analysis or for your overall decision process, and I would like to know how you have reached that conclusion.

MR. FONTANA: Okay. I think I am going to get to

5joy6

that.

MR. LEE: Okay.

MR. FONTANA: In our core damage prevention task, we looked at ways of improving core damage numbers. There is a tremendous volume of information out there in the utilities in all these plants, and almost all the procedures are involved in operating the plan properly, so we didn't repeat all of that, but we had our contractor look at some possible ways of things that affect core damage prevention. We found there were no quick fixes, no magic box that could be opened there.

With respect to initiating events, it looked like the best reduction factor was just experience with the plant. The plant maturity was the best single indicator of the number of sequences or number of events that potentially could lead to core damage. Much of this is plant specific, plant-specific reliability, and maintenance will reduce initiating events, and plant-specific examination of initiating events will improve performance.

We looked at major modification, appears to have less impact on core damage frequency than improving the use of the systems that are already there. When you consider the systems that are there, the ECCS systems, the redundant systems, that is not really surprising.

We also found that the operator is the most

5joy7

influential factor in determining the core damage frequency. That's the most important single thing.

MR. KERR: What does operator mean in that context? Do you mean the hands-on controller or do you mean the operating utility? What does operator mean?

MR. FONTANA: That's the maintenance and hands-on operation. I don't think it included utility management, but it probably should have.

MR. KERR: Well, unless it's there by implication, I don't see any indication of management attention, and the more I see of the accidents and near accidents that we have, the more I am convinced that there has to be a top-down dedication to reliable operation and safety in order to achieve it.

MR. FONTANA: I know that some of the utilities have done a significant amount of work in that area and have gone through a lot of drills.

MR. KERR: I'm not talking about drills; I'm talking about the attitude of top management toward safety and reliability and maintenance permeating the whole organization.

MR. BUHL: We couldn't agree with you more, Dr. Kerr.

MR. KERR: I don't see it up there.

MR. BUHL: Well, what you see up there, what one analyzes when he goes to specific plants -- and we are using

5joy8

the reference plants -- is the operations and set of procedures and process which is in place, and that is in place based on the top-down management tone, presumably. It is very difficult to go in --

MR. KERR: The number one item I see up there is plant maturity. I really don't think that has much to do with it. I think there are old plants which are in a lousy condition because the management is lousy and old plants that are in good condition because the management is good.

MR. FONTANA: All this says is if you just draw a plot of events versus age, the biggest thing that shows up is when the plant is four or five years old it is running pretty well.

MR. BUHL: I don't think that is disagreeing with you. If you look at the data out there, it says as plants get older, the events are less. I don't think that is in conflict with what you said. The management clearly sets the tone for all these operations, including testing and maintenance, and what we analyze, of course, is what is in place. It is very difficult to quantify what the manager said -- what one quantifies is what is there, both in terms of procedures, operations, testing and maintenance.

MR. KERR: I don't want to run this in the ground, but it seems to me that there is a tendency on the part of us who are in engineering to look at equipment and relook

5 joy9

at equipment and put on new gadgets and more gadgets, and I think we are missing an important ingredient. And if that really is an important ingredient, somebody needs to let this be known, that that is where risk reduction is most likely to take place.

MR. REED: Dr. Kerr, let me speak for management.

(Laughter)

I'm one of those guys. And it relates to the question that John Lee asked. I think in assessing the current PRAs what jumps out at you is that the operator and the maintenance have such a profound effect on risk and that the containment is so important that then it gets you back to the management. I mean the operators aren't making errors because they are dumb or they just don't care. I know when we track operator errors quite closely, we find that most of the reasons are related back to poor procedures, which management are responsible for and the lack of direction, which management is responsible for. I think that when we get into our writeup of accident prevention, a lot of that is going to focus on procedures and preventative maintenance and management; it is not Mario's -- it's not his direction to get into that aspect. He is really looking at the consequences of what this gets you into.

MR. KERR: Well, Mr. Reed, I'm an expert on management because I know almost nothing about it, so I can

5joy10

be objective in that evaluation. The thing that bothers me is the sort of thing that bothers me in professional basketball: when the team doesn't win, you fire the coach. In most cases what ought to happen is that the owner probably ought to be fired. When reactors don't operate correctly because of poor maintenance practice, one may decide to hire a new director of training. If you put the emphasis on poor maintenance, that is sort of the next step, when what may really be needed is that whoever is responsible for nuclear operations ought to be fired, or not fired, but his talents ought to be used in some other area and there ought to be somebody in there who will see that the whole operation is correct.

I'm not trying to tell people how to manage. I'm saying that if you are going to cure a problem, somebody has to identify the problem, and I'm not sure we are identifying it with the current approach to PRAs or risk studies or whatever.

MR. FONTANA: Of course, one thing, I think --

MR. REED: First I want the record to show that I don't agree we ought to fire the guy in charge of nuclear operations when something happens.

(Laughter)

MR. KERR: Your operation may be a very good one, you see, so you can take credit for it.

MR. REED: When you are dealing with 600 people at a station like a Dresden station, for instance, and we look at those things that we do poorly, again, the majority of the things we find are the lack of a procedure or not being clear enough. Very seldom is it just an operator that blatantly does the wrong thing, but there are many, many things that can go wrong in the plant. I'm sure you're not indicating that for each and every thing that goes wrong, that you ought to tie it to the vice president of nuclear operations.

I think we look in the overall. When we fail to learn from our previous mistakes, that is what makes me mad and that is what makes the chairman of the board mad. And Bill, I think what is going to come out of this is to highlight to our sponsors things like operator error as being important to risk, and then I think it is really up to the individual utilities to see how they deal with that.

We are going to have procedures for operators, but also it is going to be up to utility managements to see how to use this information in their own operations. At least our Zion PRA has taught us at Commonwealth Edison a great deal: that containment is the most important thing that is there, and we have made some training films for our operators and we are paying more attention to that containment.

MR. KERR: I have already said too much about

5joy12

management. That's it.

MR. REED: Okay. All I am saying is I don't think you should expect to see out of the bullets that Mario puts on these slides that aspect of it because that is not his thrust here.

MR. SIESS: You said containment is the most important thing, and that is certainly true as far as the health and safety of the public is concerned. But if I were a member of the Board of Directors of Commonwealth Edison, I think I would want to be pretty sure that I didn't have core damage even if the containment worked perfectly.

MR. REED: Every time we make a puff to the environment that is less than one percent of our tech specs, I have to deal with the mayor of the City of Zion, and that is important, too.

MR. SIESS: Even without that puff, if you have severe core damage, that is quite a blow to the corporation.

MR. REED: Indeed it is.

(Slide)

MR. FONTANA: Moving into --

MR. LEE: Can I interrupt you with one more question before you move to the next block of tasks and so on? Where does the management itself come into your overall picture?

MR. FONTANA: There is a Task 24.

5joy13

MR. LEE: So that belongs to the core damage prevention task; correct?

MR. FONTANA: There is core damage prevention in Task 24, accident management, that will work with each other very closely.

MR. LEE: I thought that belonged to the core damage prevention task, which is one of the four large tasks you have, which is what you summarized in the previous Vu-graph, as far as I could tell from this; right? Could you comment, then, a little bit on what you have concluded, if anything, about accident management?

MR. FONTANA: The core that you saw there was what was done so far on the prevention of core damage, and the major conclusion there was with respect to the core damage prevention, that the opportunities for improvement are involved with operational and human things and not adding more equipment.

As I said, that is not unusual because you have ECCS systems, redundant systems and that sort of thing. Now, you go beyond core damage, and the logic of the program is structured so that the accident management there is in Task 24, and that is not completed yet. What that task does is it looks at, first of all, ways of trying to quantify operator action so that you can have some dealing with the PRA. That's just part of it. Part of it is trying to come up with

5joy14

some measures on some probabilities of how the operator might respond under various challenging conditions. And the third one is identifying specific ways in which an operator could intervene in an accident sequence, and those tasks are not done yet.

MR. LEE: I am a little bit curious about your statement that perhaps you don't envision any additional equipment in accident prevention.

MR. FONTANA: No. What they looked at, they looked at the equipment that was there and they looked at improvements and this sort of thing, and they found that -- their conclusions were that the biggest improvement doesn't come from big, massive changes in hardware; it comes from two things: from operator improvement or there are some realignments of present plant systems that could be done.

MR. LEE: Perhaps I would like to come back to Mr. Reed's earlier statement emphasizing the importance of operating procedures, and I would like to really think that that is a very important aspect, and in this regard we are talking about types of accidents perhaps we have never encountered and we have never dealt with so far, and so I thought one of the emphases in this task should have been or could be in delineating and understanding perhaps what kind of procedures one could come up with for the operators to follow in case of unanticipated accidents.

5joy15

MR. FONTANA: That's one of the longer-term objectives. We will have first cut at it for some of the reference plants out of Task 24, but I can see this is something that would follow on.

MR. LEE: But it should be a very important and necessary integral part of the decision process you are trying to accomplish by mid-1984; is it not?

MR. FONTANA: I would think that identifying that as an important issue, as both Mr. Reed and Dr. Kerr said, top-to-bottom management attitude --

MR. LEE: One has to be able to say, okay, we have enough instrumentation to guide the operator in case of a severe accident. For example, I'm not talking about putting mitigating equipment. That belongs to a different task. But at least instrumentation and perhaps better training, different training, perhaps.

MR. FONTANA: I don't know for sure. I imagine this would be one of our recommendations. The containment analysis teams are looking at the behavior of these plants under accident conditions, and they will be looking at things like what instruments are there that might guide me.

MR. LEE: I have a slightly different perspective. I don't care about what kind of probabilistic analysis you do to guide the containment to survival or not. Regardless of that, from the operator's point of view you would like to

5joy16

protect your investment, certainly, and you would certainly like to come out with certain procedures that would guide operators.

MR. FONTANA: First of all, it is procedures that are there that will take you up to design basis conditions. We are identifying things that could be done when you get into severe accident areas. The reason I am qualifying is that we are only doing it for four reference plants, and there are a lot of other plants, and when you are talking about what an operator specifically can do, it may become plant specific.

MR. REED: Let me be a little more direct. I think Mario's hesitancy -- we have a great number of people with operation orientation. It's part of the review of what you can do in the case of an accident. But we are not generating the procedures. For instance, the symptom-based procedures that have been developed by the owners group is the most significant advance. It goes to what the French are doing, too. If you do something and the water level hasn't returned and you do something else and you do something else and you get to the point where you put Lake Michigan water into the reactor if the water level hasn't come up, what we will do is to generate from the phenomena time sequences in which you can act and see if the current procedures are sufficient to cover the situations. But that melding together of these

5joy17

procedures that exist now just coming out of the IDCOR program are the phenomena, the time lines in which you think you can do something at each stage. And they will be melded together.

But what Mario doesn't have before him now is that melding together of these two.

MR. LEE: But the melding process will take place sometime between the next year before your closure takes place?

MR. REED: It's taking place now, and the same people who have generated some of these emergency operating procedures for Three Mile Island are the people reviewing our sequences and seeing if any additional procedures will be needed. Again, that is one of the detriments of coming in at this stage. I'm glad we are here because your questions have really helped in maybe focusing on what we are going to turn out, but we are kind of at that point just before we put the most important pieces together.

MR. KERR: Again let me see if I understand. My perception is that the emergency procedures and the symptom-based procedures are designed to take care of situations up to the point of core damage but not after severe core damage. Am I correct?

MR. SEARS: I'm Fred Sears from Northeast Utilities, and I'm a member of the steering group, Dr. Kerr,

5joy18

and I will try to answer that.

We have procedures in place to deal not only with the normal operating phenomena but also with the events which would be beyond the design basis. Those procedures are in a continual state of update. We have been moving to the symptom-based procedures, and part of the management commitment that you talk about is the recognition on the part of management as to what are the important things we should be concentrating on.

We have learned to concentrate more on making sure the operator understands what goes on rather than simply having him become almost a robot and doing specific things. Part of that management commitment is learning to train for understanding, learning to have him emphasize to control the reactor, the recovery of the reactor, the cooling and the containing of it, so that when it goes into an event, whether it is a normal event or beyond the design basis or anything else, he knows those things to be paramount; and that is what the procedures are being modified to do so that they will help the operator to carry out that job so that we will lower the operator contribution to the particular risk.

What the PRA helps us to do is to understand where we should be putting emphasis. It helps management understand what is the thing that we ought to be pursuing more,

5joy19

what can we do to improve it? We don't rely on that totally, but it helps us to understand where we ought to be putting our efforts for improvement.

MR. KERR: I guess I didn't phrase my question very well. What I was trying to find out was whether the existing emergency procedures go beyond the initiation of core melt, for example. Do you tell an operator what to do and how to identify the 20 percent of the core melt -- let me just be ridiculous -- say, has occurred. It is my impression they don't go much beyond maybe some undercooling, but they don't take, perhaps, maybe the existence of a large amount of hydrogen in the containment into account.

My impression has been that they do not go much beyond what might be called a possible onset of core damage, but they didn't go on and say if half the core is melted, here is what you do.

MR. SEARS: We have procedures in place which allow for an assessment of the amount of core damage and for dealing with the cooling within containment, the pressure suppression, dealing with hydrogen and hydrogen recombiners. We have an organization set up for dealing with this type of an event which may exceed what procedures are designed to do, and we are trying to train the operators to maintain first and foremost in their mind those things necessary to deal with those and not be, if you will, restricted to that which

we had anticipated. So I believe we are trying to address that.

MR. SIESS: By "we," you mean Northeast Utilities?

MR. SEARS: I can speak specifically to Northeast Utilities and also for industry efforts such as IDCOR and many other activities, so it is with both groups in this.

MR. KERR: Thank you.

(Slide)

MR. FONTANA: In moving on, you remember the lower left-hand block which involved the behavior of the plant under severe accident conditions. We have already discussed a fair amount of this, and where we developed this integrated code for analysis of severe accident behavior. It is an attempt at best estimate phenomena, which is built on phenomenology It uses detailed plant design information straight out of the people who designed the plant and own it. It can handle system interactions or operator action. It can be run in a batch or interactive mode, and eventually it has capability for operator training as well as severe accident analysis.

It relates to what you were talking about, which is what are the parameters, the measurable parameters that would exist throughout an accident sequence. This would calculate what they ought to be and give an operator some guidance.

5joy2]

MR. LEE: Even at the risk of being a little bit facetious, do you really believe that code that you are developing could really help an operator fight a brush fire going on?

MR. FONTANA: No, not while it is going on. I think it could help him in training and running through drills and saying, okay, let's assume this happens, and let him see -- I don't think --

MR. LEE: But you said it is intended to be a best estimate code, but there is an ingredient of bounding conservatism inserted here and there; so with such a code you could train the operators realistically enough?

MR. FONTANA: Better than anything else that exists right now. Things will get better, I guess.

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We're also pulling together information on the source terms. We've developed a code called RETAIN for transportive activity through the primary system in containment, and fundamentally it's based on what is in the NRC TRAP-MELT code. It's faster than TRAP-MELT and I think it's coded a little better and written in such a way that it can handle both the primary and secondary containment systems.

MR. KERR: Do you think the NRC's code is likely to be called RELEASE?

(Laughter.)

MR. FONTANA: I don't think I'll answer that.

(Slide.)

As I said, we put together containment analysis teams to test these and do some analysis in some real plants. The four plants that we chose were Zion -- because that's a pressurized water reactor with a large containment, and the team members there are Commonwealth Edison who owns it, Westinghouse who designed it, Foske Associates who pulled together the models that we were working with, and Technology Energy Corporation, that's us. Sequoyah -- I should point out that the requirements for these reference plants were, one, they had to be representative of something that was out there. Secondly, it had to have a pre-existing PRA because we didn't want to go back and do work in getting these fault trees and probabilities and all that sort of thing; we wanted to pick

that up from where it was. Thirdly, it had to belong to a utility that was willing to cooperate, and these all met that criteria.

Sequoyah is the PWR with an ice condenser owned by TVA. Again, Westinghouse, Foske Associates and Technology Energy. Peach Bottom, which is a MARK-1 boiling water reactor with Philadelphia Electric, General Electric, Foske Associates and TEC. And Grand Gulf, which is a PWR MARK-3, and the team members there are NSS, Bechtel and TEC.

Now, these analyses are underway right now by the containment analysis teams.

(Slide.)

Most of them are almost completed, and the priorities are, first of all, what is the baseline behavior of the reactor under the half a dozen or so key accident sequences that we're analyzing. And these sequences cover the spectrum of things that you might get into such as small break LOCAs, large break LOCAs, transients, loss of heat removal, loss of power and that sort of thing, and various combinations of equipment available and unavailable.

Then we want to look at what some of the operator actions might be that could terminate or alter the accident. Then we want to look at uncertainty and sensitivity analyses and the difference between these two, at least in our dictionary.

MR. KERR: What is the difference in your dictionary?

MR. FONATANA: Uncertainty analysis, look at the uncertainty of events and things like physical parameters like my thermal conductivity might be between such-and-such, that sort of thing. That kind of uncertainty.

What we call sensitivity analysis is a grosser sort of difference, like, say, we don't really believe, for example -- let's say for purposes of argument that we don't believe there's going to be a steam explosion. And you say yes, I don't believe it but do it anyway, so we run a calculation and see what the consequences of that are.

So if I could -- like saying I don't really believe there's going to be a hydrogen detonation or a hydrogen burn, and say okay, make me calculate it anyway. This is what we call sensitivity analysis. Uncertainty analysis is the propagation within the area that you know and think to be within the physical range of --

MR. CORRADINI: So it's variation on physical quantity or correlations of data based on some stochiastic range where a sensitivity analysis is variations of models, correlations, et cetera, but based on physical understanding or lack of understanding?

MR. FONTANA: Yes. You might not want to model in, for example, pull it out, put it in. I might not believe it; you might. But that's how we defined the two. I'm not sure

if it's a universal definition.

MR. CORRADINI: I was just curious.

MR. FONTANA: And analyzing the effective mitigation features, --

(Slide.)

-- which comes down to really two; core catchers and filtered vented containments. Preliminary analysis of reference plants using a modular analysis program showed that we think that MAAP will be a useful tool for analysis of plant behavior throughout severe accident sequences. We put the experts to the test and they seemed to like what they saw with MAAP.

Some results that are coming out seem to show that containment failure -- if you get into a sequence where you lose heat removal, you lose power and this sort of thing, invariably, -- almost invariably, containment failure takes a long time to occur.

We're finding that it appears that the hydrogen that is generated during the accident is generating by the zirc oxidation that occurs while it's still up in the core region. Once the core melts and moves down into other areas, we think that further hydrogen generation is unlikely. It appears that if you get debris on the concrete base mat, you get some fairly rapid penetration to start with, but then it appears to slow down. And for almost all of these sequences, penetration of the concrete base mat is not an important factor. There are

other containment failures that would occur first.

(Slide.)

MR. CORRADINI: Are the models in MAAP substantially different from what I read for the ZION PRA in terms of the physical models, the physical processes?

MR. FONTANA: I think they are in places. I couldn't answer specifically right now.

MR. CORRADINI: Those conclusions don't look very different from the ZION PRA.

MR. FULLER: With respect to behavior within the core and the vessel, they're relatively similar. The models for debris coolability in core concrete attack in containment are, in some cases, fairly different.

MR. CORRADINI: Could you expand a little bit on that? Do you mean different in the sense of different physics or the hand calculations that have been made now and the computer calculations?

MR. FULLER: The core debris concrete attack model has been formulated based on energy balance considerations that although were intuitively known by the people who did the Zion PRA, they had not formulated them in a consistent way at that point. And what it comes down to is to look at heat balance effects between the debris as it generates its decay heat, and the way it simultaneously loses that heat upward from the convection of radiation and attacks debris downward --

attacks concrete downward. And the model explicitly accounts for the endothermic reactions that take place, which I don't believe were accounted for very well in the earlier Zion study.

MR. CORRADINI: If I could just rephrase that. Simply you're saying that the general thrust of the models ex-vessel is the same except now, because they're in a computer program form they are more consistent, and, therefore, an attempt is made to balance mass, momentum and energy or whatever so that some individual models may be the same but the overall structure now is more consistent. Is that the point?

MR.FULLER: It's true that the overall structure is more consistent because we have an integrated analysis tool which they didn't have available to them.

What I'm trying to say is that models have been developed that were not in existence at that point in time for purposes of doing these analyses now, which give -- you know, when coupled to debris coolability models and basic containment response, give a better understanding than previously on the integrated system behavior of the containment with respect to temperature and pressure generation and history.

MR. CORRADINI: The reason I'm asking -- since I don't have available what's in MAAP, my first jump is to go look in the Zion PRA and see all the models that are presented there in detail and try to estimate how different or similar the two are. That's the reason I'm asking.

(Slide.)

MR. FONTANA: They're not identical. Our phenomenological models are pretty much complete now. We developed the heatup code.

MR. CORRADINI: I'm sorry for interrupting again, but let me pursue that one logical step further. If you use MAAP now instead of what was used in the hand calculations and the Zion PRA, would you get same results or different results?

MR. REED: We're going to know the answer to that very soon. I'm the guy that has a keen interest in the questions that you're raising, and since we're one of the reference plants and we're doing Zion all on MAAP, we will soon have a direct comparison between that and our PRA.

I'm sure some of the smart guys in my company maybe know the answer now, but I don't know that answer.

MR.CORRADINI: Okay.

MR. FONTANA: The heatup codes are developed, and as you remember, these are the codes that calculate the heatup of the core when it's in the design configuration, and to the point of zirc oxidation. We developed a fission product release and transport model. The zirc oxidation models are incorporated into MAAP and the debris coolability models have been developed. The hydrogen generation distribution and combustion tasks are just about completed. Code comparisons were done. We looked at various codes for hydrogen distribution

and compared them with some tests that were done at Battelle and Frankfort and with HEDL, and it took the key models, the key phenomenological models, that were already incorporated into MAAP and RETAIN.

(Slide.)

Debris coolability studies indicate -- the first one is kind of obvious. The TMI-type fuel damage is coolable. There's a core sitting up there with damaged fuel that is coolable. You get a large amount of debris in a vessel head, then it probably is not coolable; it probably will go through.

The indication is that ex-vessel, the debris is coolable if water can be supplied to it.

MR. CORRADINI: Since I know about some experiments at Sandia, how does that third bullet jibe with some experiments I've recently seen by Dana Powers where they supplied water to the debris and it still continued to melt down and attack concrete?

MR. FONTANA: Well, I'd have to know what the debris configuration was.

MR. CORRADINI: I don't know the specifics.

MR. BUHL: Can you give us a date on that? Are these in the last month, or two or three years ago?

MR. CORRADINI: No, this was in May of 1982. I was at a meeting where Dana presented two experiments; one experiment where he had a bed of debris and he heated it up on

concrete, got it to go above melting, start melting down and injected water and essentially, the debris continued to stay in a molten state. About half of the debris bed stayed in a molten state and continued to attack the concrete, and the other half stayed coolable.

He started off with solid stainless steel bowls heated up with an induction furnace in a concrete crucible, and essentially raised the temperature. It started melting, he injected water, and some portion of the debris melted down and attacked the concrete. The other portion stayed coolable.

He had a bed in the second experiment of solid steel bowls with water in it in a concrete crucible, heated it up and dried out the bed, decreased the power -- I don't know the exact details; I don't think I should say that -- and even with the water thereafter going through dryout, was able to get the bed on the lower portion to attack the concrete. And again, an impermeable crust was formed.

MR. FONTANA: But he dried it out?

MR. CORRADINI: Yes.

MR. FONTANA: Well, it depends on the debris depth. I'm sure Bob Henry is aware of those experiments, and I really would have to ask him.

MR. CORRADINI: He was there at the meeting.

MR. FONTANA: -e developed this model. I can't answer that, but Bob could.

MR. CORRADINI: Are these portions of the MAAP computer program going to be -- I hate to use the word -- verified, validated, looked at in comparison to experiments which are out there that seem to go against the logic or some of the intuitive logic I see there?

MR. FONTANA: They'll be compared against all the experiments out there.

MR. CORRADINI: Under what timeframe? Not under the next year. That would be fairly difficult, I would expect.

MR. KERR: Bob Henry must already have compared them, since he's aware of the experiments.

MR. CORRADINI: He did? Where's that?

MR. BUHL: The problem we have here is we don't have all the people in the program in the room, and I think we'll have to maybe get you some answers if you'd like. But we are aware of those. We've had meetings with Sandia, as I mentioned in my presentation, over the last year. We're trying to think of the specifics of Dana Powers' work of two years ago.

I think we ought to get the people who know as part of our team.

MR. FONTANA: I want to pass that to Bob Henry because he's aware of that and he developed the model.

MR. CORRADINI: I only ask in the context of going further on. If you've got these models, what's the logic in relation to looking at them?

MR. FONTANA: The models reflect the experiments that were known at that time.

MR. CORRADINI: Okay.

MR. FONATANA: The question of post-meltthrough dispersal was confirmed by simulation tests that were done at Argonne. What we're talking about here is should the melt proceed through the bottom of the reactor vessel, would it just lay down in the reactor cavity or would it disperse. And experiments seemed to show that particularly where there's a path available in the containment, it would disperse. That has several ramifications.

One is, dispersal would spread out debris over an area that would result in a much thinner level of debris, and probably more coolable. And the other is that if you had a core catcher designed to sit in the bottom of the vessel, the debris wouldn't be there; it would be someplace.

So back in the early days, I know we didn't think too much about debris dispersal.

MR. SIESS: I don't quite understand that. Some reactor cavities have a bottom at the same level as the containment bottom, and some have a bottom several feet below. Is that a general statement?

MR. FONTANA: It's quite energetic. There is a pathway, it will flow, regardless of the level.

MR. SIESS: You could get it out of the Westinghouse

reactor containment back up on the cavity floor?

MR. FONTANA: You can get it out of some of the designs where there is a keyway.

MR. CORRADINI: Isn't there a down-side risk, too, though? If you're going to claim you're going to get energetic dispersal, I would expect a large amount of aerosol formation.

MR. FONTANA: The aerosol would probably be fairly large and fall back out. We're not really claiming this one way or the other. I'm not saying it's particularly good or bad. I'm just saying it looks like we'll get dispersal.

MR. CORRADINI: Are you then modeling the aerosol formation from such a blowdown?

MR. FONTANA: We are modeling such an aerosol formation. The reason is this dispersal here starts out with either molten debris or something like that. The aerosol you get out of this is going to get big, and big aerosol will fall out. Subsequently, if you attack the concrete, you're going to get a lot more aerosols that are smaller, but even they will agglomerate.

MR.CORRADINI: How do you know that those are going to be big relative to the ones from concrete? Are there some experiments you're basing your judgment on?

MR. FONTANA: Yes. These debris sizes were measured. They dropped fuel into water and measured the debris that came out of it up at Argonne.

MR. CORRADINI: How many tests did that involve?

MR. FONTANA: I don't remember exactly. I thought there were on the order of half a dozen but I'm not sure.

MR. FULLER: Yes, five or six tests. These were similar materials. In addition, Argonne did some work based on thermal reaction, so that they actually looked at core constituents as they would come out of the failed vessel. They did see aerosols but nothing --

MR. KERR: Can you give Mike the references to describe those?

MR. CORRADINI: I'm only familiar with one test at Argonne that they're trying to have published at a meeting, which are two tests and they are low-pressure blowdowns around 2 bars to 5 bars; not your most likely scenario where you are at 2000 pounds.

MR. FONTANA: It doesn't matter. At high pressure it would disperse more, but at the lower ones the vapor pressure would have significant effects.

MR. LEE: Can I also bring up one specific example perhaps you can help me with. Is my understanding that you're concerned about the non-coolability of fine core debris which could imply that when you observe a molten core or core in the melting process perhaps, you might be concerned about shattering fuel elements, so you may not want to dump the available coolant into the molten core? Could a question like

that be answered through your study?

MR. FONTANA: I think with our understanding of the fragmentation model and the size of debris that you're like to get, I don't think there will be any cases where the decision would be not to have it poured on debris. It's kind of a convoluted sentence. What I'm saying is that we should always add water. Fuel water; not core.

MR. FONTANA: You should always add water to hot fuel. I think we're going to come up with the conclusion that there are no cases where you will get things like steam explosion or steam spike that overrides the capability of the containment, and that sort of thing.

MR. LEE: I'm specifically talking about shattering of fuel elements into fine debris.

MR. FONTANA: In the core?

MR. LEE: Yes.

MR. FONTANA: You don't have much in the way of alternatives. You have to cool it.

MR. LEE: One argument I heard once is you may, rather, let it melt a little bit more, and then perhaps you could maintain coolability in the long term; whereas, if you poured coolant water into the molten core, you can shatter the fuel element into fine debris, which will not be coolable, according to some of the studies I have seen.

MR.FONTANA: The question is how fine a debris do you

get when you shatter it?

MR. KERR: It seems to me the evenmore important question is how the hell do you know you're there in a melting core?

MR. FONTANA: You just can't tell. There's another interesting point that's not a particularly good one; that if you've got a core melt on the floor and there's no water on it, the radiant heat transfer from a dry well inner surface could be a problem and get things pretty hot. We're looking into that right now.

(Slide.)

MR. CORRADINI: Your comments about what John said brought me back to what you said before about using that to help the operator's trend beyond the design basis. Here I get the drift that I may not want to do that because I don't know the timing of the event. You can't have it both ways. Either you're going to use it or it's probable that it could be used in terms of operator training, or it's just silly to consider it?

MR. FONTANA: It could be useful operator training to give you the time in which you go from one phase to another. I don't think anything that I've said obviates that recommendation.

MR. KERR: Let me interpret. What he's saying is you use it for operator training when it's not silly to do that.

MR. FONTANA: When it's not what?

MR. KERR: When it's not silly to do that.

(Laughter.)

MR. REED: I want to hark back to the procedures, and this is a chance to do that. One of the major benefits I think we're going to deliver to the utilities are procedures for cores in degraded states. Maybe the only thing you can always do is to assure yourself that you get water into the vessel or into containment, but at each point of the so-called safe, stable state where the core is in a big lump on the core plat, or whether it's on the floor, it's going to guide us in telling the operator what parameters you should be looking for.

The same question that John Lee posed as a question; we ought to decide now and not be guessing what would work when we're near that point.

So it's going to give us procedures now; what's going to come out of the IDCOR is suggestions. It's going to have a number of things you can do and then it's going to have to eventually evolve into a point where Commonwealth Edison, for instance, sees what specific equipment and instruments they have and ways they have for coping with it. So we are going to get core melt procedures where we really don't have core melt procedures right now.

MR. CORRADINI: I think I understand that.

MR. LEE: Just the perspective I've gotten so far still is that a lot of emphasis is placed on developing a new

code called MAAP andtrying to somehow trust those numbers you get, rather than trying to see what kind of procedures or additional equipment may be necessary to diagnose it, and perhaps manage, if possible, a potential accident.

MR. FONTANA: It's not either/or; one has to come before the other.

MR. LEE: I understand both ingredients have to be there, but my perspective has been somehow, that a lot of emphasis is still being placed on the MAAP code. You have a large number of tasks and a lot of effort is being spent there.

MR. REED: I think we're somewhat surprised at the emphasis the questioning is putting on MAAP. I don't think we see MAAP as the end product. I think the only thing we see in MAAP, in our view, is the best description of the phenomenon as it's going to evolve. It seems to be a step beyond what we've had before in MARCH. And it's just an avenue in getting us to some of the other points. You put a lot of emphasis on MAAP, and I don't think we've put that same kind of emphasis there.

MR. BUHL: Let me add to that. I think that's right. I think the thing that we're really putting emphasis on that somehow is missing in this conversation is understanding. One should never start writing procedures or telling operators what to do unless one understands MAAP is only one of many tools which we have developed for achieving that understanding. As

I said at the outset, it was not our desire to develop codes. That was not one of the end products that was even envisioned when this program was put together. No one envisioned developing those computer codes.

What we're looking for and the message that should be clear is that we're looking for truth and understanding, not the computer code development.

MR. LEE: I've not gotten a lot of information so far on what additional physical understanding we've gained over the past two or three years really that could change our direction regarding reactor coolability.

MR. FONTANA: Like, for example, --

MR. BUHL: I think we have to stop here and make a point clear. We're giving, for example, on the slide we're about to present, some of our insights and understanding. What we're not giving you today and what we made very clear at the outset is the reports and products in great detail. First, we don't have the time to do that, but more importantly, those products and reports are undergoing a rather serious peer review among the sponsors, so you should not be surprised that you don't have all the technical results before you today.

What we're trying to accomplish is to give you some of the major insights in these areas, and the fact that you're not convinced or don't have all the substantiating detail doesn't surprise me.

MR. FONTANA: I didn't mention it, and I should mention again that we're hoping to set up a series of technical meetings with the NRC and its contractors where we get into this in some real technical depth. We were hoping to come back and brief the ACRS once more, maybe about a year from now. But some of these tasks aren't even finished yet, and the ones that are are under review.

MR. KERR: I guess it may be that you can't answer the question. It does not seem to me it's unreasonable at this stage to ask what new physical insights have been gained from the work. The answer that we don't want to tell you but we've got them -- if that's the answer, okay. But I don't think the question is unreasonable.

MR. BUHL: If you look at the viewgraph, there are some insights on there at this moment, and we're trying to give you those insights. It's just that my sense of the direction of the questions towards code development in more and more detail was clear at the outset -- you know, that's just not available.

MR. KERR: So the answer to his question really is look at the slide, it will be a good place to start.

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MR. FONTANA: The early MARCH calculations indicated steam explosions. We think those are ultra-conservatively calculated. That is different than MARCH calculation indicated a steam spike that would cause excessive containment pressure and rapid steam generation, and we think that is due to a faulty model in the debris-water interaction.

There are insights that have been gained on fission product behavior, which I was going to get to here.

MR. CORRADINI: Since you used that example, I can't resist, but there is a cross-over here that worries me. That is, a steam explosion is a physical event, and just because it doesn't fail containment, doesn't mean it doesn't exist.

MR. FONTANA: I meant large enough to fail containment.

MR. CORRADINI: One could easily worry about steam explosion that would behave like this MARCH calculation and cause the steam spike, so one physical process that has been analyzed in an unphysical manner may be completely reasonable for a totally different result: that is, a steam explosion causing multiple steam explosions causing steam spike in the containment.

MR. FONTANA: That would have to be traced logically. We will get into that in the process of this

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next program.

MR. KERR: Why don't you give him a chance to finish this. I would sort of like to hear it.

MR. FONTANA: EPRI looked at multicomponent model for release from fuel, and some of the things we are finding out, that cesium hydroxide is the dominant fission product in there. The dominant iodine compound appears to be cesium iodine. There is a high potential that exists for plate-out in the upper plenum.

MR. LEE: I have heard the statement before about iodine and cesium and so on. What physical mechanism do we have that tells us that we should trust these statements?

MR. FONTANA: First of all, you have got to know where the reactor safety business used to be, and that was rather arbitrary statements of fractions released. There was some experimental work that was done in non-steam atmosphere and this sort of thing where the various chemical effects just did not take place. In the reducing atmosphere you are not going to get cesium iodine; you are going to get iodide. There is some fairly complicated chemistry going on in here.

MR. LEE: It's complicated. That's what worries me.

MR. FONTANA: Look at TMI. When you look at the iodine and the gas phases compared to the liquid phase, you

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have got a liquid coefficient of 10^6 and 10^7, and that is practically a direct measurement. When you look at the pH, the availability -- the capability of components reacting with the iodine, you know, there is silver in the control rods and there is a lot of things that could react to the iodine.

It turns out that there is ten times as much cesium as there is iodine in there, so there is plenty of opportunity for iodine to react with the cesium. So these aren't guesses, they are based on some physical background.

MR. LEE: And based on some limited experiments performed.

MR. FONTANA: That's right, and there are experiments being done and being planned at Oak Ridge, and as Tony said, we support these confirmatory experiments. We wouldn't just want to leave this the way it is and have everybody wonder whether this is really the case. But if you do the analysis based on chemistry, you are pretty sure the experiments are going to bear out. We really would like to see confirmatory experiments, but we think this is sound enough that we can hone in on a regulatory decision of some kind in about a year.

MR. LEE: And you feel that the uncertainty associated with the chemistry of these fission product compounds is the temperature and the time sequencing and things like

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that and are not large enough to change the picture.

MR. FONTANA: Since we haven't completed all the calculations, we don't know what the total picture is. We expect -- first of all, we are not bound to any previous pronouncements on what the source terms ought to be. Whatever comes out comes out. We try to put bounds on them. The work isn't complete, so we really don't know what the extent of fission product retention is, but one thing we are pretty sure of is it is going to be lot more than TID-1484, and to some extent less than WASH-1400. How much, we can't really say, but these physical effects I think are taking place, that iodone floats around as a compound, not as a vapor, that you have got aerosols in there that behave like aerosols and not as a vapor.

(Slide)

Okay. Now, the question arises of how do you approach generic applicability? This is really a pretty sticky problem, and that is, how does the things that we come up with apply to all the plants that are out there? And as I said previously, there are areas, largely phenomenological areas, that we think are generically applicable. We have come to some agreement on steam generation and hydrogen generation rates and combustibility, and these are pretty generic; but there are other things that become plant specific, things like containment volume, actual systems for getting

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water into various places and that sort of thing. But as I said before, we have four reference plants. We have had to look at what else is out there. We have prepared a list of key issues that should guide you in making this. We made a list of the parameters of all the plants that are out there. Then we identified some additional plants which are representative, non-reference plants.

Now, that kind of sounds like a circular play on words, but what we are saying is that we have set up the tools to do the analyses on these three plants here, we are doing the analysis in a depth that we know what the tools are but not the depth in which we are doing it for the reference plants.

MR. SIESS: Did you tell us what the reference plants were?

MR. FONATANA: Zion, Sequoyah, Grand Gulf and Peach Bottom. You see, here we picked up the BWR Mark-2, the Combustion Engineering plant at Calvert Cliffs, and this is characterized -- one of the important factors is that it has no penetrations in the lower head, as compared to the others that do. And Oconee, which is a B&W plant. We held meetings and put together the beginnings of teams for these other plants.

(Slide)

As we did all the reference plants, we inspected

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each of these plans and we have modifed the MAAP code so that it is capable of doing analyses on these plants.

(Slide)

Now, we have tasks under way for qualification of MAAP. This is an independent contractor, highly experienced in these areas, to check the models, the code logic and the structure, and MAAP verification, which is an independent, line-by-line verification that the programming conforms to the models. We are in the process of making modifications to MAAP, what we call a daughter code, that can do uncertainty and sensitivity analysis.

(Slide)

We have just completed drafts in the mitigation area on alternative containments, as I said, drawing heavily on the Swedish work, the core retention devices, drawing heavily on the Battelle-Frankfurt work. The staff on safe stable states had its workshop, and that report is nearly complete, and the operational aspects task is nearly complete.

(Slide)

The hydrogen control task is complete. We looked at a lot of potential concepts for controlling hydrogen. The acceptable ones were for small containments, pre-inerting, otherwise igniters, and marginally acceptable, the concept of pre-condition post-inerting. That is getting the oxygen

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concentration down that you can walk around and post-inerting in the event that you could get --

MR. KERR: How small is small: Mark-1, Mark-2?

MR. FONTANA: Mark-1. In fact, I think the ACRS and the NRC has okayed pre-inerting at least for Sequoyah and the Watts Bar plants. There are a whole group of other systems that did not stand up to the criteria. The equipment survivability task is nearly complete. The rigorous method based on PRAs for equipment choice was consistent with choice based on judgment. That is kind of interesting. I don't know if it is all that crucial, but it is kind of interesting because we had our contractor, he went through the PRAs and he looked at the probabilities of various systems and equipments and that sort of thing and came up with a list of 25 important pieces of equipment that ought to be further analyzed with respect to survivability.

And also EPRI had a grey-headed gent identify from judgment the list of equipment that should go into their hydrogen burn test, and lo and behold, it came up to be about the same list, so that tells me that the PRA is probably pretty good.

Also, for most cases it appears if the equipment can survive the design basis accident, it can survive degraded core conditions in general. The design basis accident in

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many cases is more severe.

MR. KERR: You appear to be in agreement with NRC on that, if I can understand their current position.

MR. BUHL: Let me clarify. A point I think is important is that non pre-inerting but igniters, I think, were approved.

MR. FONTANA: What?

MR. BUHL: I think you make the comment that pre-inerting is approved.

MR. FONTANA: Did I say that? No, I'm sorry. I meant igniters. Erase that wherever it is. Run your tape backwards.

(Laughter)

(Slide)

We have an expert review process, as Tony said. We had an expert review group staffed by industry experts. The expert review groups were senior consultants, the steering group and the AIF. They reviewed the task results. The comments are all resolved in a final report by the contractor under our guidance.

(Slide)

That is a pretty extensive process and is one of the reasons why we have not been able to turn over many reports yet, that they are all in this process, and by the time we are through, I think we will have a fairly well-looked

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at report.

Now, the various expert review groups are the hydrogen control, distribution and combustion, prevention systems, mitigative systems, equipment survivability, containment structural capability, safety goal adaptation, ground rules, sequences and risk assessment, human factors and operational procedures, containment analyses, degradation phenomena and source terms. These are all separate expert review groups.

(Slide)

Foreign programs have an interest in IDCOR. We have signed a technical exchange agreement with Germany and Sweden. We have had status meetings with France. They decided not to join, but they were polite, took us out to lunch and everything.

(Laughter)

We wonder why they gave us a three-hour lunch. Erase that.

(Laughter)

We obtained detailed information on Project Filtra in Sweden. They were very cooperative. They gave us practically everything they had on Project Filtra, and they also joined IDCOR. As I said, we obtained extensive information on several core retention devices from Germany. Sweden has joined IDCOR and will use our tools to analyze

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ten plants there, and Japan has joined IDCOR, and there are other people interested.

(Slide)

Now, as Tony said, we have a three-level hierarchy for reports. The bottom level is the technical task reports that the contractors write. The second level -- the top level is conclusions and recommendations report, which is issue oriented. It identifies issues and what our position on the issue is, and the middle level report connects the two. It is a summary or reader's digest of the technical contractor reports, which fill a book shelf about 3 or 4 feet long, and it will summarize what is in those lower level reports and provide a link from the lower level reports to the conclusions on the top level.

If someone wants technical depth, they have got to get into the lower level reports. We have written first drafts of conclusions and recommendations report and a technical summary report. They have been reviewed by the steering group and the technical advisory group. We have gotten a recommendation and are in the process of -- we have written a second draft and we are now in the process of finishing the final draft as information comes in.

The intent is to have an internally-reviewed draft to our steering group on July 1st.

(Slide)

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I have already mentioned the things on these two slides, so that is about where we stand.

As I mentioned, we thought it would be a good idea to come in and talk with you at this time just before the program is completed, and we will be in the process of getting into technical depth in these with NRC and its contractors, and the ACRS, I presume, is invited to all of those, and I guess we will come back and talk to you at least one more time near the end of the program to see where it stands.

MR. KERR: Thank you.

Are there any additional questions?

MR. REED: All of those answers we promised you during Mario's presentation, I'm not sure that we have done. At least, Dr. Kerr, I think that we ought to recycle to the questions you first posed at the beginning.

One of them was: What question was IDCOR designed to answer? I think the answer to that was the question posed by the ANR, and that was should reactors be designed for severe accident? That is what we have directed our program towards.

You had said when will we know if we have answered the question. Well, through the review of the technical experts and our senior consultants and our steering group, I think it is going to be our judgment as to whether or not

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when we look at the technical facts, whether we think we have enough information or whether more is needed.

From what we are understanding, we think we have enough technical background to make some of those judgments, and part of the reluctance with giving you specific -- they have yet to go through a screening, for instance, of the steering group because before we settle on the source term that we are going to put out, we want to be sure of our facts too, and we might want to be more conservative than some of the technical guys are.

You had asked who posed the questions. Well, the initial question was posed by the NRC and certainly resulted out of TMI and the issues that were raised out of TMI. You asked what would be done with the answers, and the technical reports will be given to the sponsors and also to the NRC, and we are going to make a major effort in articulating those technical results to the entire community, the national labs and every forum -- not every forum, but many forums that we think will be useful, and we will certainly get feedback.

And lastly you asked in what context is the work being done: are we assuming that reactors are safe enough? And I think as we went into this work it was our judgment that reactors were safe enough, but by the scope of what we have done, we did not take the approach of looking at a core meltdown, and if those probabilities showed us that it

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was very probable, to stop our work. What we did was to assess core meltdowns in all aspects, looking at the impacts of those, looking at what could be done to mitigate those. So I guess from that context we did not limit our investigations on the premise that the reactors were safe enough.

Those are just a few comments that come to mind, and I don't know if you want to pursue those further.

MR. SIESS: I would like to add one more question, and I hasten to add that the answer is optional. If you had $50 million a year to spend on research, what would you do with it?

MR. REED: I would like to take the option of not answering the question.

(Laughter)

It is certainly not a response that I could give you off the top of my head. I might tell you just a few words in respect to the NRC program, and we are not trying to skate that issue, and I presume it is directed towards that.

We had an opportunity to comment on that program, and the NRC has divided their program into a phase one and a phase two, and I think overall we think that the first phase of that program, which we understand will be finished in the spring of next year, in our view will probably lead them to having sufficient information on the table by which

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to make some regulatory decisions.

We certainly think that what we are going after in our program should give us those answers also, but as we said before, we hope that a lot more work will be done, for instance, in the area of source term. The IDCOR program chose to -- we could have easily spent $10 million on nothing but source term. I think the approach, the assessment we are going to want to take at this time is a very conservative assessment. Now, whether or not all of the things on the confirmatory part of their program is needed, we have the technical experts on our side who will argue either way, and that is the judgment that I can't give you.

MR. SIESS: Thank you.

MR. KERR: Mr. Davis.

MR. DAVIS: Mr. Reed, I seem to be hearing from you and from Dr. Fontana that you really don't have many conclusions yet, that you are still going through an expert review process. You haven't brought everything together, and that remains to be done in the future. It seems to me that is a bit of a conflict, and maybe you can straighten me out if it isn't, with your letter to Chairman Palladino on February 1st in which you state that our work to date gives us considerable confidence that major design changes or new regulatory requirements will not be required.

Can you give me the basis for that statement in

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light of what we have heard today?

MR. REED: I don't view that as a conflict. Obviously, I wrote it; I don't view it as a conflict. It is the way it is leaning. The technical experts seem to indicate that some of the big-ticket items that we have talked about, such as core catchers, such as filtered vents, and probably the jury is still out on filtered vents, it just seems from what we have learned thus far there appear not to be any large items out there.

I think we are going to gain a great deal of insight and information along procedures and management of the accident, and out of that, I think, is going to come back very strongly on the point that Dr. Kerr made, and I am going to make it my business to make sure in our reports we get that point made to the management of the utilities which controls that aspect of it.

MR. DAVIS: Thank you.

MR. KERR: Are there other questions?

MR. LEE: I'm just repeating Dr. Davis' question, basically, but I would like to single out one aspect, and perhaps Dr. Buhl, you could summarize based on the philosophy that you mentioned regarding the understanding of physical phenomena, perhaps, and that item that I would like to get somewhat better physical understanding of today, possibly, what have we learned during the past three years regarding

the containment so that we now can say the containment will survive, perhaps, or we don't have to worry so much about it, perhaps; you know, some kind of general summary of perspective, if you can.

MR. BUHL: Something for the press.

MR. LEE: Perhaps.

MR. BUHL: I think we have learned and I think the data will substantiate that whereas a few years ago we worried a great deal about early containment failure and the effects of early containment failure, by doing this integrated and well thought-out analysis, we are finding that we just don't find those early containment failures the way we thought we did a few years ago. We are finding that better understanding of the physical phenomena is leading to a conclusion that we have a great deal more time to react, the operators have a great deal more time to do something, and that is a fundamental finding.

Now, to substantiate that and go through all the technical details would take a great deal of time, but that kind of finding seems to me very important to reactor safety.

MR. CORRADINI: What is early containment failure versus late?

MR. BUHL: Minutes versus many hours.

MR. KERR: Mr. Tyler, were you going to add something? I thought you were going to get in something

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for the press.

(Laughter)

MR. TYLER: No.

MR. KERR: I think that is a good statement that you made, but let me follow a bit. There was an earlier question about the LOCA. Now, I did not detect anything that says that is a less likely thing than we thought it was, and that, it seems to me, does lead to fairly early containment failure. And while you can perhaps eliminate blowing the head off the vessel and going through the containment, have you convinced yourself that the interfacing LOCA is a lot less likely or can be made a lot less likely than one might have concluded at an earlier time?

MR. BUHL: I think not so much a lot less likely. I think the argument there, the insight we have there is not so much from the probability point of view but rather from the consequences. I said in looking at risk one has two choices, or one hopes to do two things. One is to reduce probability, and the other is to move the consequences to the left. But the interfacing situation, what one can show and what we are doing in those analyses, a good part of which has already been done, we are looking at what really happens in the aux building and other places, and what you find is that some of the assumptions we used several years ago were extremely conservative. And you find, for example, the

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fission product source term, or at least the source term available to the environment is substantially lower than what we used a few years ago. So the insight there is more on that point than on probabilities.

MR. KERR: And that insight has been gained simply by assembling information, which is certainly legitimate.

MR. BUHL: And doing the analysis through other than just containment.

MR. KERR: Let me get back to my earlier question which I guess was not posed specifically enough when I asked. One of the questions you were trying to answer certainly -- the question was raised on the advance notice of rulemaking. It's a legitimate answer if that is what you were really trying to do. From what you have told me today, I would have tended to answer my question by saying we started out trying to identify the dominant accident sequences and we then concentrated on those to see if we could learn more about those that would convince us that either the risk was less or more or something could be done to ameliorate the risk.

I realize my question wasn't specific enough to lead you to that comment and I'm not even sure it's the right answer, but it appeared to me from what I have heard that much of what you have undertaken has been based on an

7joy19

approach which says in order to get a good idea of where we are now, we will concentrate on a representative set of PRAs, look at the dominant accident sequences and then try to examine those in enough detail to see if we believe that the risk predicted by making use of those is a good way of describing current reactor status.

Am I mistaken in that interpretation?

MR. BUHL: I think that is a good statement. I think it is probably only half the statement because we recognized --

MR. KERR: Could you give me the other half?

MR. BUHL: The other half, I think, is that we recognized in PRA work and in accident analysis more generally that considerable uncertainty existed in our knowledge of containment response and in our knowledge of how these various phenomena which have been described drive that containment response, so we put a good deal of our emphasis, in addition to what you described, also on understanding in a better way the data and experiments and other information out there, coupling that and putting it into an analysis of the effectiveness of containment.

We did use those dominant accident sequences, if you will, as examples or as driving sequences for containment, but I think I would stress the containment effectiveness of what we now understand about containment effectiveness

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as the other half.

MR. KERR: It also appears to me that the comments that have been made indicate that you have been guided to a considerable extent by what will be required to eventually demonstrate to the NRC that you have met requirements, whatever they may be. I cannot be critical of that. Clearly one has to live with that regulatory climate, and one does have to satisfy requirements.

MR. BUHL: I guess I don't tend to agree with that.

MR. KERR: I want you to respond. I didn't mean you had been driven. I haven't seen much else. Now, that may be the context of the presentation here. I think you are saying, for example, in your code development work, I am unwilling to rely on existing codes, or saying I need a better one because it will give me a better basis for good calculations, and so on.

I haven't seen a lot which would tell me we really aren't sure -- I guess it wouldn't be politic to put it this way, but let me put it this way, but I'm really not sure the NRC understands reactors as well as we do because they haven't operated one; and here is what we think might be the problem and we are going to try to get to the bottom of this, not to satisfy regulations, because either we think the reactor out there is so safe we don't have to do anything, or because here is what we think might be the problem.

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Now, maybe that is implicit in this size effort, because I recognize it is a tremendous effort of manpower, not just the $7 million that shows up there, but I would guess utilities are putting in a hell of a lot of additional effort with their own people; so it's a tremendous effort.

But I might have expected something in a sense beyond satisfying the requirements of the NRC. I'm oversimplifying and I apologize for that.

MR. REED: Both Fred and I want to take a whack at this.

MR. KERR: But in addition, I would think that you also might have a very strong incentive to prevent core damage. That is independent of public safety. And indeed, it seems to me that prevention of core damage in terms of the survivability of the industry may have more influence -- I don't want to denigrate public safety, in which I am very interested, but this other aspect, it seems to me -- for example, when you put a lot of emphasis on containment, which is well taken, there is a tendency to say, well, we could put up with a TMI-2 because the containment is strong enough.

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You do certainly talk about prevention of core damage. Well, I'll stop. I have missed some things that I would have liked to have seen. Maybe it was because I was inattentive.

MR. SEARS: Actually, you stole about half my answer. I've got to go back a little bit in history. In 1980, there was a degraded core subcommitte of the AIF which was faced with exactly the question you basically outlined here. The NRC was proposing some regulations dealing with degraded cores, and we had a strong feeling that these regulations at that time -- that we didn't have sufficient knowledge to know whether we needed to make changes or not.

And the forerunner of IDCOR was basically trying to answer those initial Federal Register notices. We concluded in there that we had several things; the health and safety of the public and also, the financial risk of the nuclear industry. And one could take either one and say they were the most driving force for protecting the plant and preventing a degraded core.

On top of all those, though, we didn't have anything other than some conceptual knowledge on our own that we didn't believe it was a significant problem which required a major fix. That was the conclusion people were drawing, but it was more a gut feeling than having the technical truth. IDCOR was formed to bring to bear the industry's resources to produce

the technical truth based upon the existing knowledge and, where necessary, to try to fill in gaps. And the reason for producing that truth was so that we would have a means of working in the regulatory environment and not just opposing it because someone wanted something changed. We wanted to assure that whatever change took place was protective of the health and safety of the public, and of our financial investment.

In order to do that, we had to come to grips with what were the problems and what were the solutions to them. And that's how IDCOR was scoped initially, with the questions that were being raised, was to determine the technical facts so we could then know what steps we should be taking with regard to these regulations.

You outlined quite well the questions that we went through. We had a lot of very lengthy discussions as to are we just trying to defeat a regulation or are we trying to achieve safety. And we had a very strong conclusion that we all had a common goal in safety, and that our contribution could be to get our knowledge together so that we did not improperly impede a regulation and, as a matter of fact, that we could contribute to making sure that any regulations that were necessary were supportive of safety and were effective in doing what needed to be done.

If that occurred, our financial security was a natural part of that. You cannot separate safety from the

financial aspect of the utility. And those go together. But we have to have the technical t ruth.

We also didn't want to draw a conclusion midway, jump in with both feet and say that's the end of it, stop it, and then have someone come up with a bit of knowledge later on that caused us to turn around. So we've been very careful in trying to structure our program so that we would get the information, we would integrate it, we would review it, and review it to as great a depth as we knew how, through all the expert review groups, advisors or owner utilities, all the consultants and put together an integrated story that we didn't go after bit by bit.

So I think that's where we're trying to come from, and we are trying to achieve technical truth, and from that truth, then, devise any needed changes or regulations, if they are there.

MR.REED: Fred, you said most of my speech. I understand the question you're posing. I hope out of the prevention section of what's being done are a number of alternatives; just a whole menu of what could be done. And out of that menu,utility management will have to make judgments from a business standpoint, from a public relations standpoint and from the shareholders whether or not, in their view, there are things that can be done that we're not doing now, whether it's hardware or whatever, that they feel is worth doing.

But when we launched off on this program and the kind of instructions that we gave to the technical people was tell us what can be done, helped describe the phenomena so we'll know the timeframes that are involved. So again, I don't know what our final conclusions will be out of all of this, but I hope that we have before us the alternatives that we can choose.

It could be that out of all the alternatives that we have, that we feel that what we have now is okay. I have not made that judgment because they have not -- being a part of this, even this closely, the story has not been put forth in a review-logical fashion so that we can then sit down and start to make those judgments.

MR. KERR: There's another question that I think is important for utility people only and not the NRC. Let's suppose you get the information and that you trust it, and that it tells you to do something or other -- I don't know what it will tell you to do. But Mr. Fontana, for example, indicated that as things now stand, a significant contribution to risk comes from operators. And that's a function, it seems to me, of maybe more than equipment of the operating utility.

Now, as I see the NRC from my perspective, it is forced to regulate through regulations. And regulations in themselves are not very good at insuring safety, because they neglect a lot of intangibles which simply can't be described

well enough to regulate.

So I guess I don't have much faith in the safety and reliability if it has to be insured by regulation. People who are going to be able to see that this information is used and, indeed, put pressure on the people who may not want to use it are the people in the industry. How are you going to make certain that the results of this investment are used by the very people who may not understand it very well and may not want to use it if they do? And I'm sure they're in the minority if they exist, but how is it -- have you thought about how to insure, let's say, not going beyond NRC regulations, because I think there are simply things that can't be covered by NRC regulations.

How is it that you're going to insure that the results of this, of the serious investigation, are used by the people who won't be using them? Or -- I shouldn't ask that question. Have you given some thought to that process? Do you have a plan?

MR. REED: I think Dr. Sears did not jump to the microphone.

(Laughter.)

I think the answer is that we have not addressed that question. We have been very careful in IDCOR both to meet the schedule and to try to meet our budget to stay within our mission. That mission is to put on the table the technical case.

MR. KERR: If I sounded as if I were addressing that question to the IDCOR program, I didn't mean to. I agree, it's outside that. But once you get the results, which are going to cost somebody a lot of money and a lot of manpower, it seems to me you accept them, it's important that they be utilized.

MR. REED Indeed, and I don't have an answer to the question that you've just posed.

MR. KERR: Is somebody thinking about that, worrying about it?

MR. REED: I can't say it's on the table of anyone. However, I would assume -- and I think it's a fair assumption -- that through the INPO process, that the recommendations that come out of this that will set standards for excellence, that they will be assured through the INPO process.

MR. KERR: Well, I'd like to share your -- I do share your hope. I don't know what your evaluation is, but I observed only fairly recently a situation in which a utility had received recommendations from INPO which I think, if they had been followed, would have eliminated a good bit of grief. And I realize INPO is fairly new, but INPO can only recommend.

And I would urge, if it's possible, that some mechanism -- you know, there are coercive mechanims completely outside of the NRC. Assurance is one of them.

MR. REED: INPO is a mighty persuader, though. INPO has evaluated our plants on a number of occasions, and the

recommendations or open items they've had, we've had to respond to. And we've had to respond to the satisfaction of INPO, and to our CEO who is interested in us reaching that. I'm sure there's going to be examples a number of times where there's going to be someone who didn't follow the recommendations.

But you pose a good question and I'm just -- I want to tell you that I don't think I have the answer. Above IDCOR is the Nuclear Regulation Policy Committee of AIF, and the linkage there to INPO.

MR. KERR: Somebody help me interpret where we are on this agenda. Do you have a copy of the same one that I have?

(Laughter.)

Are we at the point at which Mr. Buhl is going to discuss interaction with the NRC? Are we in the discussion and final remarks section?

(Laughter.)

MR. BUHL: I think we're in the latter.

MR.SEARS: I think we're in the latter.

MR. KERR: That was my interpretation of the situation. Are there any further questions that we want to ask of these nice people?

MR. SIESS: Are we going to see them again?

MR. KERR: I see no outstanding questions. I very much appreciate your spending this time and effort, because I know from what you've described, that you're busy. It has been

very helpful to me to have this session, and I think from the comments and questions of our consultants, that they have gained a good bit. We would hope to see you again when you are at a point at which you think it would be profitable to talk with us. And we will try to take advantage of the meeting s with the NRC. I'm sure somebody on the staff or someone with the committee can schedule it, so we would hope to be able to arrange that.

MR. REED: Thank you.

MR. KERR: Any further comments?

Thank you, and the meeting is adjourned.

(Whereupon, at 12:45 p.m., the subcommittee meeting was adjourned.)

CERTIFICATE OF PROCEEDINGS

This is to certify that the attached proceedings before the NRC COMMISSION

In the matter of: ACRS Subcommittee on Class 9 Accidents

Date of Proceeding: April 25, 1983

Place of Proceeding: Washington, D.C.

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

Official Reporter - Typed

Anna Riley / JHT

Official Reporter - Signature