NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

IN THE MATTER OF:

SUBCOMMITTEE MEETING

on

RELIABILITY AND PROBABILISTIC ASSESSMENT

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Tuesday, 11 September 1979

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE MEETING

on

RELIABILITY AND PROBABILISTIC ASSESSMENT

Century IV Room
Airport Quality Inn
Los Angeles, California

Tuesday, 11 September 1979

The ACRS Subcommittee on Reliability and Probabilistic Assessment met, pursuant to notice, at 8:30 a.m., Dr. David Okrent, chairman of the subcommittee, presiding.

PRESENT:

DR. DAVID OKRENT, Chairman of the Subcommittee

PROF. WILLIAM KERR, Member

DR. HAROLD LEWIS, Member

DR. J. CARSON MARK, Member

DR. MILTON PLESSET, Member

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PROCEEDINGS

DR. OKRENT: Good morning. The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Reliability and Probabilistic Assessment.

My name is David Okrent, Subcommittee chairman. The other ACRS members present at this time are Mr. Milton Plesset, Mr. Carson Mark. We expect that Messrs. Harold Lewis and William Kerr will arrive somewhat later. We also have Mr. Samuel Saunders as a consultant.

The purpose of this meeting is to discuss the concept of establishing qu[illegible]titative safety goals for nuclear power reactors, the development of a status report concerning nuclear power plant component failure rates, and a review of the NRC probabilistic analysis staff's research program to help the ACRS develop information for its annual report to the Congress.

This meeting is being conducted in accordance with provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act. Mr. Gary Quittschreiber is the designated federal employee for the meeting. The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on August 27, 1979. A transcript of

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the meeting is being kept and will be made available, as stated in the Federal Register notice. It is requested that each speaker first identify himself and speak with sufficient clarity and volume so that he can be readily heard.

We have received no written comments or requests for time to make oral statements from members of the public. However, I should note parenthetically that when we get into the topic on establishing acceptable quantitative safety goals, I may run the meeting in a much more flexib'' fashion, and members of the public who are present and have contributions will probably be requested to participate.

We shall proceed with the meeting. It is my understanding with regard to the agenda that we will begin looking at the topic of failure rate data and at how the situation has changed since the development of WASH-1400 and how we should proceed to develop a reasonable response to the questions posed by Congressman Udall, by the beginning of the year. And after this, we will look at the question of the Rancho Seco transients, how one might analyze them probabilistically' again, with the idea of trying to have something that can be responsive to the request of Congressman Udall by the end of the year.

DR. MARK: Could you remind me the exact form of the statement of the request by Udall? I don't need the

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words precisely, but it came in a letter. Asking for what?

DR. OKRENT: It came in a letter, and let's see if we have a copy. Dr. Plesset will give you a copy to peruse.

DR. MARK: Thank you.

DR. OKRENT: After this, we will talk about the research program and priorities within the probabilistic analysis safety research program, and maybe perhaps look at the priorities in a broader sense, if that makes sense. And following that, we would get into the question of quantitative risk acceptance criteria or safety goals.

Now, the times we have given for these various matters are estimates and I would hope we don't run beyond the times estimated on the first two items. I don't feel a compulsion to use up all the time allocated on the first item, for example, since — with regard to data, for example, something is better looked at outside of a meeting, okay.

With that brief aside, let me call upon the NRC staff. Mr. Rowsome, are you going to be the spokesman?

MR. ROWSOME: Yes. I had intended to go over exactly this outline, as you have just done.

My name is Frank Rowsome. I am the acting director of the probabilistic analysis staff. I can give you a little better indication of the time we expect to take. The presentation on failure rate data will be led off

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by Dr. Bill Vesely, and will take just under an hour. And we can discuss it as you see fit. But it should be easy to keep it within that time frame.

The discussion of the Davis-Besse and Rancho Seco events will be led off by Dr. Gordon Ed son, and we expect it to take rather less than half an hour, perhaps 15 minutes. Discussion might run on the order of 15 minutes beyond that.

So, I expect we will be ahead of schedule when we reach the third item, the PAS research program. I will lead off with that, and I would imagine that could get into quite extensive discussions back and forth, so that you can cut that off, as you will, to initiate the discussion of acceptable risk. Bill Vesely will take the lead in that regard.

So, I would like to introduce Bill now, if you are happy with that outline, to introduce the discussion of the failure rate data.

DR. OKRENT: Fine.

DR. VESELY: The AS's data program is handled, in large part, by Idaho National Engineering. I will have the Idaho people discuss three of our programs, the LER analysis and what we are obtaining from that program, which will take about half an hour, and Walt Sullivan, from Idaho, will discuss that. John Poloski will talk about the NPRDS evaluations and common cause evaluations that we are performing, and that will take approximately a half an hour. And then I would like to summarize where we are now with regard to updated failure rates modifications to WASH-1400 data and plans that we have for fiscal 1979, fiscal '80.

So, my Idaho people, I would say, had a fairly large auto accident. Some of them are still in the hospital. So we have to modify just slightly some of our discussions.

I would call upon Walt Sullivan, from Idaho, to begin the discussions on LER.

(Slide.)

MR. SULLIVAN: Good morning, ladies and gentlemen.

As was mentioned earlier, we started off on the wrong foot last night. We hadn't been off the plane but about 10 minutes until we got our greeting to Los Angeles, and we are still going to try and give you a satisfactory presentation this morning.

But calling on Mr. Poloski, I believe he will give his section of the presentation. If not, I will attempt to give it for him, but I was not prepared for his section.

As Dr. Vesely said, EG&G Idaho, the company I work for, is primarily taking the responsibility for this LER evaluation program, and, in doing so, we divided the program into three different areas: the analysis of the LER events --

(Slide.)

— The nuclear plant reliability data system analysis, and common-cause analysis. I will give a brief description of the LER analysis, and Mr. Poloski will cover the NPRDS and common-cause analysis.

Just as a brief synopsis, going into the LER evaluation program —

(Slide.)

— The program is coordinated by PAS, and we are providing the technical support. The objective, one of the objectives, of this LER analysis program — I hope I didn't confuse you in going from evaluation to analysis — evaluation program, is the three different areas: NPRDS common-cause, and the LER analysis. I am now talking about the LER analysis leg of the overall evaluation program.

And some of the original objectives were to, first of all, take the LERs and code them into the respective components of data that are contained in the LERs. For example, component type, time that the failure occurred, the mode, the cause, the system effective, and the failure type.

Now, earlier this year we gave you a pretty extensive presentation on exactly what was going on in this area, and I feel that I shouldn't have to cover that anymore today. Is that all right with you?

DR. OKRENT: All right.

DR. MARK: You may have said before, but I would like to be reminded, and I am sure everyone here was part of the group that you addressed before. The LER, as it comes in, raw data, as it is processed through the NRC system, at least, receives very little criticism as to whether the specific event or interpretation of it would be later changed.

What do you do to defend against the fact that things aren't said in the first report or maybe modified in the second?

MR. SULLIVAN: Well, hopefully, the LERs, as they are reported to NRC, will show the modification in the second report, if we can pick it up. Now, I agree that is not always the case. Unfortunately, all we have to work with at this time are the LER reports as received in the data base and sorted for us. And if those modifications have not been made to the LERs, they will not be reported in our report.

DR. MARK: So, within the data assembled, there will be some instances where something is put down as "pump failure," and you put "pump failure," and it might have been

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a fuse.

MR. SULLIVAN: What we try to do in these cases -- that is part of our expertise. We can identify those particular problem areas where it is, in fact, not a real pump failure. And in this particular example, if it was a fuse failure, we would not use it in the pump report. However, if it was a fuse failure that caused the pump not to perform its function satisfactorily, we would include it in the pump report as a command fault, a secondary-type failure command fault.

DR. MARK: But there is an area of difficulty between this and real-life facts.

MR. SULLIVAN: Yes.

DR. SAUNDERS: I would like you to explain to me in more detail the difference between, say, the cause and the mode in the failure type. Give me a sentence that says what is the mode as compared with the failure type.

MR. SULLIVAN: The mode, the failure modes, we are all familiar with, say, for pumps. The pump does not continue to run, does not start. WASH-1400 identifies this environment. The cause is the actual failure mechanism. Hopefully, the root mechanism.

DR. SAUNDERS: That would be, in this case, the fuse?

MR. SULLIVAN: Yes.

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DR. SAUNDERS: What would the failure type be?

MR. SULLIVAN: Was it recurring? Is it a recurrent t· failure; was it a common-cause failure; was it, in fact, a secondary fault?

DR. SAUNDERS: Now, the time.

MR. SULLIVAN: The actual event date on the LER.

DR. SAUNDERS: Can you do that to determine the length of service?

MR. SULLIVAN: Unfortunately not. We haven't been able do to that, no, sir.

DR. SAUNDERS: That's unfortunate.

MR. SULLIVAN: I really don't know how to go about that.

Would you, Bill? You would need more detail.

DR. VESELY: We are estimating at this go-round, constant failure rates, which is the number of failures over the criticality time, to try to attempt, for example, to analyze wearout you have on the installation time, time between successive failures.

DR. SAUNDERS: That's right. I understand that. But as you and I both know in our heart of hearts, constant failure rate is an assumption. A machine doesn't make such assumptions, necessarily.

DR. VESELY: We will talk about that briefly to get this more detailed history of failures from installations to analyze wearout. That is a different data source.

DR. SAUNDERS: Thank you.

MR. SULLIVAN: Yes, sir.

This brings up another point. We are, in this analysis — and I would like to make this clear — we are not attempting to accomplish any risk or safety assessments. That is planned in the future. Primarily, what this initial work is trying to do is to put t data in a form so that we in the future can do these extensive statistical analyses of the data, and also to generate some gross failure rates, just for the failures being recorded in the LERs.

Which brings me to the next point: One of the other goals of this program is to provide these gross failure rates. And in doing so, we have calculated failure rates for the various components that we are analyzing for plants, the NSSSs, PWRs and BWRs, and then overall.

Once we have accomplished these initial goals for each component, we write a component report, submit it to the NRC for review and comment. They, in turn, have sent reports to various industrial people for their comment. And once these comments are pooled, they are returned to us, and we try to update the reports as necessary. Once the reports are updated, our technical editors at EG&G look at the report, try to put it into a polished state, and resubmit it to the NRC and, hopefully, it will be issued as NUREGs in the near futur .

That is a brief synopsis. Are there any questions? I would be glad to entertain questions at any times during the presentation.

(No response.)

MR. SULLIVAN: Let me move along, and I will give you an idea of where we are at and what we are trying to accomplish in the near future.

(Slide.)

Our pump report is probably the report that is closest to being ready to be published. Here, again, due to the paucity of data in the LERs, we cannot break the pumps down other than to the generic class pumps. In other words, I am sure you are familiar with the NPRDS people. They have such reciprocating pumps, and we found that data was not available in the LERs to do that as far as the failures associated with the pumps.

The status of the analysis is that we are in this final stage that I mentioned earlier, going through the technical editors and submitting the report for publication. However, I recently learned that there is probably going to be some changes made to the pump report, and on your handout it says "Tentative Issue Day: October

'79." That may slip slightly.

DR. VESELY: It will slip a month.

MR. SULLIVAN: Just general remarks about the pump reports and some of our findings. We initially wrote the report and calculated these gross statistics for just overall pumps, generic pumps. And we got to thinking that if you are calculating operating failure rates and standby failure rates, some of these pumps don't exhibit characteristics associated with those types of failure rates at all times. We felt that if we broke them into different categories — running pumps, alternating pumps, and standby pumps — we can get better statistics, and that was one extensive change that we made to this report. And we see some satisfactory comments on this.

And in light of that, I have gotten written here some of the more significant observations —

DR. PLESSET: Can I ask a question. When you talk about "pumps" and "failure rates," aren't you really talking about the drive, not the pumps themselves? Are you actually talking about failure of the pump itself, aside from the —

MR. SULLIVAN: We define "pump failure" this way: First of all, we define the "component" as the "pump and the drive mechanism," and we define "failure" as the "inability to meet its designed function" — or "its function in the system."

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And so, to answer your question, if the drive did fail, it was recorded as a "pump failure."

DR. PLESSET: You make no separation?

MR. SULLIVAN: Between the drive and the pump? No, sir.

DR. PLESSET: I would expect that -- I think almost 100 percent of the time it would be the drive.

MR. SULLIVAN: That's not correct. We have a lot of problems with seals. We have a lot of problems with packing leaks and reciprocating pumps. We have problems with the cylinder blocks cracking. We have problems with propellers. We have problems with shaft breakage. And very few motor failures that we recognize in the pump LERs.

DR. PLESSET: Maybe not motor failures, but actuation of the motor.

MR. SULLIVAN: Since we have defined a "component" as "the motor and the pump," any actuation mechanism would be considered in another component.

DR. PLESSET: It would be terribly useful to have the pumps broken down into different types.

MR. SULLIVAN: Indeed.

DR. PLESSET: They are very different in their performance.

MR. SULLIVAN: The LERs do not provide that information. In other words, you can't say that this failure is for a specific-type pump. You might be able to make a subjective judgment and to go to an FSAR and say such-and-such pump is this type pump. That involves two things: a lot of time and money; and another, it is subjective. So, if you don't find any exact data, we are back to where we started at ground one.

DR. VESELY: That is going to have to be assessed, for example, for risk analysis, and we don't need that separation there. When you start evaluating reliability or upgrading pumps or causes, I think that you will need that information. We have to assess what the uses of this data will be in the future to determine what — how much further we go into this LER data.

DR. PLESSET: You may not need it, but somebody else may find it terribly useful.

DR. VESELY: That's fine. And then they may also support this. We have got limited funds here, and some immediate goals with this program.

One of the questions, though: Are the LERs capable of giving that information, and how much effort would be required to get that information from LERs? There are other data sources, in-plant data, where we are spending a significant amount of effort to go through the plant logs which give us this detailed breakdown as to cause, time of failure, repair time.

And so, we are looking at LERs here to get some gross information, not all of the data.

MR. SULLIVAN: I might make one other point, sir. The LERs, from '78 on, the quality has improved in breaking the components down further into the different types. All analysis from '76 through '78. In the future, if this is continued, I think you will see better quality reports in that area, because of that.

DR. SAUNDERS: It would seem to me that someplace the LER ought to have a reference to log, so that if it was desired, you could have access to the information that you need.

MR. SULLIVAN: Let me make this point: I think, if a senior engineer sat down with the LERs and did not try to take them at face value — in other words, just get what is reported from the LER — and actually delved into the — say, it says "1-A reactor coolant pump failed;" then you can go to that plant and look up the 1-A reactor coolant pump, and there the information is available. We did not do that kind of analysis.

I think that might be planned in the future. We have got some time and money considerations, but that, ideally, would be the way to go through this. We are talking about thousands of LERs.

DR. SAUNDERS: I understand that.

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MR. SULLIVAN: That could be possible, from that standpoint. The point I am trying to make is that, as you go through these one at a time, just picking up the data, the information that is directly in the LER, that cannot --

DR. SAUNDERS: That can't be done.

MR. SULLIVAN: But in the future, if the time and money were available, I think, by using these clues that are in the LERs, I think that could be possible.

DR. PLESSET: That seems terribly important to know if a pump throws a blade, this kind of pump throws it pretty often, another reciprocating pump cracks a piston.

MR. SULLIVAN: We are trying to pick up this kind of information and put that in the reports.

DR. PLESSET: This would bear on what you would be doing by way of replacement or improved designs, particularly for pumps that have very vital safety functions.

MR. SULLIVAN: We are trying to identify those as they manifest themselves in the LERs. However, if there are subtleties that don't get mentioned in the LERs -- and there are, because the reporting requirements, as seen by the reportee, vary from plant to plant across the industry, and some plants give you very excellent LERs, and other plants give you one line or two lines.

DR. PLESSET: There are pumps that run for 30 or

40 years, and they don't have the kind of safety function that a pump in a nuclear power plant has. Other pumps are designed to last three or four years. You are familiar with that in automobiles. That is different.

I think that you need to help the designer, the person who is interested in improving safety performance, by getting this kind of information. I don't think that it should be all that difficult.

MR. LEWIS: Maybe they have to reformat the LERs.

MR. SULLIVAN: I think that problem was addressed recently. The LER —

DR. VESELY: That was one of the goals of the NPRDS, to try to provide this more detailed information. That would require — in fact, one of the goals of this program is to recommend modifications or changes to the LER program to incorporate this additional information.

I would say that we in research have changed our position on the NPRDS for making it mandatory because of the lack of information in LERs. We have found that NPRDS has more of the necessary format that would give us this detailed cause information. And our position right now is that it would be too much of a change to the LERs. LERs were not constructed for this kind of purpose that you are talking about.

MR. LEWIS: It is perhaps not clear what they were

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constructed for, but that is another matter.

(Laughter.)

MR. LEWIS: It is not that big a deal. There are about 3000 LERs per year. There are 70 plants. That is one a week per plant, roughly. It is not that big a deal to supply useful detail.

DR. VESELY: That's right. NPRDS has a problem with quality control, but having restructured the format, identifying the population, it is fairly routine a kind of operation.

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DR. PLESSET: But, pump and pump performance is a terribly important thing.

DR. VESELY: In our in-plant data program, we are coordinating with IEEE. That is the first component that we are extracting information on, and that should come up later this year, where we are going to plant logs and constructing all pump failure from 14 representative plants. But because of the inadequacies of LERs and lack of quality in NPRDS we have to go to multiple data source. NPRDS, LERs and in-plant data where we have collected over 30,000 failures from the plant logs.

We have to go to multiple data sources because of a lack of information from any one source.

DR. OKRENT: I am going to suggest we move along. Undoubtedly there are various ways in which this LER evaluation and other things relate to the Nuclear Regulatory Commission's program. Today, if we can, I would like for us to focus on the ways in which it impacts on how we are going to prepare response to Congressman Udall's question.

DR. LEWIS: May I ask one dumb question?

DR. OKRENT: You can ask two if you want.

(Laughter.)

DR. LEWIS: One is my quota.

(Laughter.)

DR. LEWIS: What is an alternating pump?

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MR. SULLIVAN: That is a good question. We had trouble arriving at what to call these pumps. An alternating pump is the pump that we are all familiar with. It runs intermittently.

DR. SAUNDERS: Why don't you can it an intermittent pump?

MR. SULLIVAN: That didn't sound right.

DR. LEWIS: That is what I concluded that you probably meant.

MR. SULLIVAN: Do you want some examples?

DR. LEWIS: I know lots of examples. I just wanted to be sure. That was a lead-up to my second dumb question.

(Laughter.)

DR. LEWIS: I figured that's what you meant. Operating failure rates, it says 1E-5 per hour. It says 4E-4 per demand. Do I infer from those two numbers that the pumps you are talking about run 40 hours per demand on the average? I am looking at the two numbers under "remarks." Alternating pumps are listed as ten to the minus five per hour failure rate. They are also listed as 4E-4 per demand for the failure rate.

In effect, with those together I get 40 hours per demand which is not all that intermittent. That is why I was asking the question.

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MR. SULLIVAN: I am not sure I can answer your question.

DR. VESELY: The analysis for alternating pumps separated the demand failures that occurred in standby and separated the operating failures that occurred while running. They are separate evaluations.

DR. LEWIS: There was a clear distinction between a failure to start and a failure while running.

MR. SULLIVAN: Yes, sir.

DR. LEWIS: There are no things in which a thing ran for a minute and then ground to a halt? Or one doesn't know?

DR. VESELY: In those cases, as a short time, then it was a start failure.

DR. LEWIS: Was that up to you or up to the person who wrote the LER? It was up to you.

MR. SULLIVAN: Yes.

DR. LEWIS: So you had that kind of information?

MR. SULLIVAN: Not in all cases. There were subjective judgments.

DR. LEWIS: I just wanted to understand the data.

MR. SULLIVAN: I understand.

(Slide.)

MR. SULLIVAN: I will expedite this since we are running late already.

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In your handout there is some information, also, for our other components that were analyzed. The control rod drive mechanisms.

(Slide.)

I think we are interested more in results. So we plan to have a NUREG issued on those in October of '79. The -- there are some failure rates there, if you are interested. Moving along, the diesel generators, we plan to have a NUREG issued for them in November of '79. Valves, which was a very extensive analysis, just due to the size of the number of LERs we had to analyze, there were probably 1400 or 1500 in the final analysis. And it is in the process of review, now, and we have received some comments that we feel will probably significantly impact this report. And it may have to be rewritten in light of these comments.

So we say tentatively the report will be issued in December of '79.

DR. OKRENT: Does that mean that if we are shooting for having the input information for response to Congressman Udall by the beginning of December so that we might have time to look at it -- and there will be sources other than this source also -- that we should expect that there will be no input with regard to valves? Or will you be able to give some input?

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DR. VESELY: We will have input, for example, gross failure statistics failure rates before that deadline. I don't think the evaluations will be modified. We have most of those now in the structure and we are rewriting the report itself.

DR. OKRENT: It is important that we keep that in mind, then, from the point of view of preparing this response. We would like to take advantage of that information which is sufficiently far along that it should be included as an evaluation of failure rates since WASH-1400 failure rates were estimated. Even if you don't have a NUREG report ready to go out.

MR. SULLIVAN: I think we are close to the stage you are talking about, right now.

DR. OKRENT: Valves would be an interesting component.

DR. VESELY: A critical component, yes.

DR. OKRENT: Yes.

(Slide.)

MR. SULLIVAN: One other component that we have done a minor amount of work on is containment penetrations and we hope to see a NUREG issued on those in early '80.

Now, let me explain our goals for 1980. Here we plan to continue categorization of the LERs similar to what we have done this past fiscal year, and issue NUREGs for the

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components that we analyze in this LER categorization on the componants, diesels, valves and penetrations that I just mentioned and then we are going to tackle instrumentation and control, which is another very extensive area, just for bulk and volume.

If we are successful there we are going to attempt to report on relays and circuit breakers and interrupters, so optimistically we will have six NUREGs next fiscal year.

DR. OKRENT: At the risk of asking a dumb question, what would you do on instrumentation and control? That is a somewhat broad category.

MR. SULLIVAN: That is a good question. We are asking ourselves the same. We are going to attempt to approach the problem similarly to the way we did valves. Hopefully, a lot of this in-plant work that is being accomplished will provide a lot of our information for the analysis.

As I mentioned earlier, we haven't started this analysis yet. And there may be problems. I am not aware of them at this time. Hopefully they can be overcome.

DR. VESELY: One of the biggest problems with the LERs is identifying the number of successes in a population in which we group the valves and can count either the running time, standby time, number of demands.

We are getting that information right now by

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counting components in FSARs and counting components in representative plants. We are getting more detailed populations from our in-plant data project in cooperation with IEEE where we are actually going into the plants and physically — plants cooperating with us to give that population information. I think that will be —

MR. SULLIVAN: That is one of our biggest problems. Accurate population.

DR. VESELY: There are other programs where we can get these reports out much faster.

DR. OKRENT: I guess that the term instrumentation and control — the term, to me, suggests several different functions.

DR. VESELY: Yes.

DR. OKRENT: And it doesn't in my own mind, it doesn't readily fall in a box as does the term valves. But I am willing to be educated.

MR. SULLIVAN: We haven't looked into the reports in depth, Dr. Okrent, and our questions are very similar to yours. And, in fact, when we get into that area, what was done again is the LERs were sorted on the component instrumentation control which is one of the component codes, and we started the analysis. Then we find out what we have to work with.

Finally, I would like to just mention our LER

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flagging analysis.

(Slide.)

This hopefully will be more of — Dr. Saunders, what he was concerned with earlier, the details of what is going on in these LERs. We are going to look much more extensively into the LERs and try to analyze them for the subtleties, not just the gross report. Things like time trends, anomalous failure rates, things that are not in accordance with what we feel — what was reported by WASH-1400, any recurring failures, any common cause failures and these recurring failures will be also associated with the common cause analysis. Mr. Poloski will talk about that in a minute.

The quality control related failures, human errors and any other significant observations — hopefully, the LER flagging analysis will answer a lot of these questions that not only you but other people have been concerned about, too. We feel it will be very valuable and enlightening.

Any other questions?

(No response.)

(Pause.)

DR. OKRENT: I am trying to understand it from the LER information, looking ahead from any other studies that will be — are being done as part of the program. You will have a change in the basis, for example, for your estimate

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of the probability of small LOCA or very small LOCA or a large LOCA and so forth.

MR. SULLIVAN: I will ask Dr. Vesely to field that question.

DR. VESELY: I would say no. What we are doing now is to use the LERs to derive failure rates. The Germans have derived failure rates. The English are doing sensitivity studies to determine potential impacts on WASH-1400 results. We have not completed our data analysis with our other projects, for example NPRDS and in-plant data, to be able to come up with what we feel is an updated data base.

We are still analyzing various data sources and have not integrated them, and don't plan to integrate them for approximately another year, 1981. Our sensitivity studies — and I have got some sides which were performed on German data — the LERs show no significant impact on WASH-1400 results, principally because the dominant contributors are human errors, common cause failures that have not been changed, have not been modified via these new data sources.

We are undergoing a fairly large human factors program to try to update our human error data, our common cause program. We have not found any of these major significant changes in independent individual component

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failure rates. We have analyzed systems where the German failure rates are a factor of 100 larger on some of the reactor components, valves and pumps, and they have not made a significant difference. Less than a factor of two on the system unavailabilites.

Because of that observation, we don't feel an urgency to update, at this time, the WASH-1400 data base, new standard data base, until we have analyzed all of these data sources.

DR. OKRENT: Well, again, I am at the moment trying to see where we think we will be with regard to responding to Congressman Udall's question, and if I understand correctly you don't anticipate any basis for change in your estimates of different size LOCAs. How about some of the other things, like reliability of offsite power under various conditions? Is there anything we are likely to have there from the NRC program?

DR. VESELY: No. Not at th · time. We will get fairly large individual plant-to-plan variations. Even the LERs are showing this. Larger than what WASH-1400 indicated in terms of error spreads.

DR. OKRENT: This is for which? Offsite power?

DR. VESELY: Offsite power, for example.

DR. OKRENT: Let me understand what you are saying. You do have data on reliability of offsite power,

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plant by plant?

DR. VESELY: No, but for specific different networks.

DR. OKRENT: So then you will be in a position to --

DR. VESELY: If you average those you come out close to WASH-1400. You may get a larger spread than what WASH-1400 indicates, but for WASH-1400 purposes, the results -- we are trying to get an aggregate for a population of 100. To average all this data, it would not be different. We are not talking plant-to-plant. We are getting a lot of plant-to-plant variations and we are not doing plant-specific evaluations.

I thought the Udall letter specifically addressed WASH-1400 in attempting to estimate the aggregate or the average. Plant-to-plant variations, I think, is a whole different question.

DR. OKRENT: I guess that raises sort of an important point. And maybe it is just as well to discuss it for a couple of minutes now.

The letter that is written is, of course, fairly brief and it just says, "Will the LER report address the questions of the consistencies of actual component failure experience with that projected in WASH-1400?" It is not a very specific statement.

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But on the other hand, I don't think we should require that a congressman write a very specific statement. It seems to me what we should do is look at the statement that is written generally and ask in what sense would it be relevant, most useful to respond to it? And it would seem to me that we need to ask ourselves, Are we going to try to include plant-specific information in the response?

And I guess, at least to me in trying to answer that question, I have to ask two more questions. First, is there plant-specific data, and I guess there is in some case. In the second case, would it be possibly relevant to try to look at plant-specific data in responding to this? And how do you ask and answer that? I suppose, does it have some impact on safety? Is it enough to talk only about some things that in our mind represent some average risk, or do we want to think of how plant-specific data might, in fact, affect the risk for a specific plant?

Let me give an example: there might be only one plant that has diesels that fail one out of two times. And all the others have very good records, so when you did your averaging it came out within the WASH-1400 average. And you would say, Well, when we do our average risk calculation, nothing has changed. I would say this specific kind of information would be relevant in preparing a response. We wouldn't leave it out.

MR. SULLIVAN: That's correct.

DR. VESELY: I think we would indicate in the response, the large plant variations on failure rates. But we will talk about this. Frank Rowsome will talk later on our integrated program where we are now constructing plant-specific models for specific systems, specific accident sequences for individual plants. We are tending to put that plant-specific data into those plant-specific models.

When you start putting plant-specific data for Zion into a model for Peach Bottom or Surry, it becomes questionable. We weren't planning to do any plant-specific evaluations. We would indicate larger variations and failure rates for plant-specific components at the component level, but I don't think we can put that plant-specific data into WASH-1400 models and infer changes on the overall risk, without doing plant-specific models.

DR. OKRENT: I don't think we have been asked in this request to translate the failure rate experience, the changes in risk.

DR. VESELY: I think that is inferred. Implied? When you see differences in failure rates, there is the question of the impact on risk.

DR. OKRENT: But, I must say I am taking that part of the letter at face value. It is a question concerning

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component failure experience and it was originally related to the ACRS examination of licensee event reports which are not directly a risk evaluation. The ACRS in no way tried to relate the study, licensee event reports, to some evaluation of risk.

So, I would think in response to this letter, what we should try to do is look at the component failure experience and if there is a significant plant-specific effect noted I think we should not submerge it in some total number. We should note that there are the kinds of variations, whatever they are, with whatever seems to be the error of limits, as you can now estimate them.

DR. VESELY: There is no problem of plant-specific data if, for the LERs, we notice the variation for the reporting requirements from plant to plant, which may be one of the problems. If we are keeping it at a component level where we have observed — where there are large plant variations, I think we should show them, as long as we don't try to translate that at this time into risk limitations.

DR. LEWIS: Isn't there a problem in a certain sense that the letter from Udall represents a misunderstanding of what the ACRS did with the LERs? Because he thinks that it did review the risk data. And secondly, clearly his motivations are different from ours. He is interested in knowing, how good was WASH-1400, whereas we are interested in knowing how safe are reactors in terms of the failure rates of these components or what is the contribution to failure rates of the components. So we do keep getting mixed up in what the motivations are.

My own view would be to take the specific wording of Udall's letter with a grain of salt. It isn't that important to be on WASH-1400, but it is important to know whether the data base for the components, as we now know it, three or four years later is consistent with what was known or used in WASH-1400 at that time.

Udall's letter also contains some genuinely dumb questions at the end, which we should say are genuinely dumb questions.

(Laughter.)

DR. OKRENT: Do you want to indicate —

(Laughter.)

— more specifically, since it is a short letter, what you fault?

DR. LEWIS: I didn't want to go into this now.

(Laughter.)

But I am willing to if you would like.

DR. OKRENT: We might as well. Go ahead.

DR. LEWIS: The end of the letter, he asks, what — determine the probabilities of occurrence that prior to the event would have been predicted on the basis of

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WASH-1400 failure rates and methodology as to the probabilities of the sequence of events that occurred at Davis-Besse on September 24th and Ranch Seco on March 20th.

That is the classic misunderstanding of statistics, in which you pick an event which has happened later, don't describe the universe within which it exists and then ask, what would the probability have been? The answer is always zero for any event. It is just a misunderstanding of statistics. I don't think it is a big deal but I think it is worth saying it.

DR. MARK: It is worth saying to the staff, too, because in one of their reports they have said that the probability is very small but the event happened, which assigns it a probability of one.

DR. LEWIS: That was in an ACRS report two months ago. I remember it.

(Laughter.)

This is like war and peace, it's a continuing battle.

DR. OKRENT: It seems to me that for various events that occur including getting into an automobile accident between the airport and the airport hotel, which happened to some members of this group last night, one can calculate a probability of the event occurring from some methodology and some statistics and I am sure that for the exampl. I just g e. one in fact could come up with a fairly small ue tar sinc. the mileage was limited and whatever other information there was, stops, starts, red lights.

I don't know how sophisticated people are these days in computing the probability of an automobile accident in a city, but one could take this methodology and compute the probability that such an accident would occur to a specific car, which is what we are now talking about. We are not talking about all of the cars that were doing the same run. And you could get a number and it would be pretty small.

DR. LEWIS: But the number would depend so sensitively on — you might choose to ask what is the probability that this particular car with this particular group of members at this particular time on this particular street — and then you get zero and the number would depend entirely on how you enlarge the ensemble into which you submerge this. It is never meaningful to ask about the probability of a single event unless you define the collection of events in which it is submerged.

DR. OKRENT: I think I am able to define ensemble for the event. Drive from whatever airline it was to the hotel, whatever it was, and measure that distance, let's say, and it is not now a question of, Will it happen to four specific individuals. It is to a car making that route and

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so forth.

So I guess I don't have the same —

DR. SAUNDERS: We are addressing the concern that mo'·vates the words of the congressman.

DR. LEWIS: We are having a tec:.nical disagreement here.

DR. SAUNDERS: It is a waste of time, gentlemen. I think all of us understand what Congressman Udall does not.

CR. LEWIS: I don't think we are quite finished yet.

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DR. OKRENT: I think this is somewhat fundamental to what it is we are going to try to do in responding to this letter. If we don't have a specific interpretation of what it is we are going to try to respond to, each person will have a rather different objective in mind. In one case it will be something that requires no more information, and in another case it will require more information than you can generate in five years; and in another case -- it is the one I happen to subscribe to -- I am taking the request for component failure rate literally, that this is the thing that we would respond to. We would not try to factor this into a change in risk, because that is a much bigger job.

I think we need to understand if that is the interpretation or if it is a different one. If that is the interpretation, in what way do we respond. Similarly, there is a question, given the WASH-1400 methodology, which I think is moderately well defined in people's minds, and using the WASH-1400 failure rates, what would you compute, what would you compute for the probability of those specific sequences?

Now, I think those sequences are different in nature, and one of the things that we have to ask ourselves is, what is the ensemble in which we place each of those sequences. That will be part of trying to answer those -- the questions of those two transients.

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I am not assuming that we would say that the

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answer is one or that it is zero on some kind of logic appropriate to either answer.

DR. LEWIS: Now I think we are in general agreement, because in the case of the automobile accident -- and incidentally, I am sorry to hear of it. I hadn't heard of that. If you had flown with me this morning, you would have been safe.

(Laughter.)

DR. OKRENT: Tell the people in the hospital or the ones sitting here with band-aids on their foreheads.

DR. LEWIS: I am very sorry.

In order to give a probability for that, it is absolutely true that one could make an ensemble which is reasonable, which doesn't go to the specific people, the specific car, the specific time, and just ask what is the probability of an accident involving that distance. And that is usually normalized per passenger-mile or something like that, and that is not unreasonable.

And then the numbers in fact are helpful in assessing the safety of driving. The same thing can be done here by abstracting from the specific events and carefully defining the question or rewriting the question in a reasonable way. One can then write a reasonable answer.

My only point is that the answer is absolutely dependent on the way in which one defines the ensemble of

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which these particular events are a member, and one has to say that.

DR. OKRENT: Indeed. And one reason why I thought in fact the ACRS should try to respond to this is so that it could make clear that when one responded to something like this, you had to be careful about how you defined the event you are analyzing.

DR. LEWIS: The reason I said it was a dumb question is, although we can make it a non-dumb question, we have all heard this kind of question often enough to jump to the conclusion that it wasn't thought through as well as if we have just thought it through.

(Laughter.)

DR. OKRENT: I am not going to comment on that area. But again, I did think it is important that there is some kind of understanding as to what kind of response we envisage and what task it is we are going to try to take on in responding to the questions here.

Have we sort of an agreement here at the table?

(Affirmative nods.)

DR. OKRENT: Do you have a comment on your interpretation?

DR. VESELY: You want from us, then, by s e date, the failure rates, be they plant-specific or average, that we have obtained from the LER evaluations and other evaluations,

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so that you can factor these into the Udall response? What do you want from us specifically?

DR. OKRENT: I would like to come back to the overall question of what we want from you after we finish this session -- section of the Subcommittee meeting. We want more. We want some help in looking at what we may get from other sources besides the NRC, et cetera. Why don't we sort of jointly plan how to provide a meaningful response within a limited time at the end of this section of the Subcommittee meeting.

MR. SULLIVAN: If there are no further questions, I will turn the presentation over to Mr. Poloski, who will cover the NPRDS common cause factors.

Thank you.

MR. POLOSKI: Good morning.

I am going to present the analysis concerning the data system at the Southwest Research Institute. Basically, that is a pretty large data base, through -- from its birth up through '78, there is basically the engineering information and failure information they have reported, used for approximately 1300 systems and approximately 150,000 components within about 57 of the plants that are reporting to them. Their data is -- it is more specific than the LER data. They break their data into more factors. The factors: the types of pumps, what types of capacities they have, what types of environments they are exposed to.

So what we are going to do is try to identify factors which will cause -- which show to cause a significant variation in the failure rates within a class of components, say, identify those factors which will cause the pumps to fail. Once we have identified these factors, we are going to try to tabulate failure rate estimates for these factors within a class of components.

And lastly, what we will do is calculate spreads for those factor levels, for the failure rates, the estimates that we get within a class of components. And we will develop the necessary software, computer programs, or whatever research it takes to accomplish these goals.

Basically, what I want to present is more or less the strategy that we have outlined, that we are going to explore this data base at NPRDS. What we have done is divide the analysis into six areas.

(Slide.)

They more or less follow in a time sequence for that evaluation or this exploratory-type analysis. The first one is the data classification, which -- right now all the data is actually classified on the raw data tapes. We have got to break it out and store it on a computer, which will allow us more efficient retrieval of this data. Right now we are in the process of storing all of this data by its various factor levels for a certain class of components onto computers, so we can have efficient retrieval of that data for our other areas of this strategy that we will discuss.

The factors that we are looking at for this data classification, as you can see on your handout, is by plant size, failures, whether demand failures or failures that were observed on normal operation, the total number of failures, the service environment, the temporal proximity, which will be used for common cause analysis, the time in service, and status of the component at the time of failure, the NSSS vendor, the safety class and component manufacturer. Those are some of the factors, that that data already exists, and we are more or less sorting this out right now.

Once the data is classified and these factors are identified and characterized, what we are going to do is basically do a lot of plotting, try to present, look at this data graphically as far as time trends are concerned, plots of total number of failures, failed population fractions, and failure rates versus time or versus the factor levels that the data was broken down into.

DR. SAUNDERS: I don't know what "factor level" means.

MR. POLOSKI: That is plant by plant, size of the valve, two-inch, four-inch, six-inch, these factors that we have identified.

DR. SAUNDERS: That is the word "level" that confused me. Six-inch diameter, that would be a different level?

MR. POLOSKI: Yes, that is a different level than a four-inch valve. It should be different factors, factor levels.

And the plotting will also -- the plotting of these failure rates will allow us to discriminate any orders of magnitude difference than the average failure rates plotted, and so another part of this strategy is to prepare these order of magnitude differences with other failure rates, namely WASH-1400 and the failure rates that we -- or the gross failure rates that we calculate or are estimated with the LERs.

Also, once this comparison is conducted, the next area that -- that we are looking in is, if there is any anomalous behavior that we have seen, then what we are going to do is contact the Southwest Research, the keepers of the data, and find out from them if there are any errors, known errors that exist in the data that we are looking at, to make sure that we do have good data.

If there are no known errors, we will contact the NRC and alert them of any of this strange behavior that we have seen, so they can begin an investigation of -- concerning that information.

As you can see, the problems we are looking at for these anomalous behavior time trends, recurring failures, common cause failures, quality control, human errors, and any other significant items that we can see from the data.

Finally, once this comparison is done, we are going to try to, for the failure rate estimates, to calculate tolerance intervals for our data. That is our area of the statistical analysis. Both the classical and the Bayesian type estimation techniques will be used for tolerance intervals.

Lastly, one additional approach is the analysis of variance. That will be investigated.

I would like to input that one of our basic problems with NPRDS is we don't feel it is useful for risk and reliability analysis at this time, even where there are data, where it is quality controlled, we are seeing large variations in failure rates with size of valves several orders of magnitude; no apparent pattern of irregularity.

And our concern is that these variations may not be due to the size itself, but to other factors that have been compounded or averaged in with the size of the valves; that failure rates are not broken out by plants specifically or by functions. The averaging is not done in a standard statistical manner. Each failure rate from a plant is averaged, given equal weight, and the diffference in operating time does not take into account different sample sizes not taken into account in WASH-1400.

So that we are having to restructure NPRDS for reliability risk analysis at various clarities, from generic down to specific components, to identify the factors causing this variation, where appropriate, at certain intervals.

DR. SAUNDERS: You mean tolerance intervals in a classical statistical sense?

DR. VESELY: Yes. In this case, our concern is that when we estimate, when we do failure rates, that the classical average may not be applicable when we are applying it to a specific component which is one member of this population, and we are trying to bound the behavior of that one component in that one system, and not trying to bound an average or an ensemble.

DR. SAUNDERS. All right. If you use tolerance intervals, I understand there are tolerance factors for normal. You can do it for the Weibull and therefore the exponential, and that's about it. Is that right?

DR. VESELY: Yes. The approaches that have been developed and have been published, yes. And our goal here is try to identify the behavior, whether it is Weibull, time dependent, exponential, that is most consistent or consonant with the data.

DR. SAUNDERS: I see. So that if you think that is sufficient, those two categories are sufficient --

DR. VESELY: That is all we are trying to look at at this first stage. It may not be sufficient and we may have to go into further detail and further research in this area.

DR. SAUNDERS: Okay.

MR. POLOSKI: Our last area in this analysis of the NPRDS system concerns mathematically modeling the data, in other words, trying to describe failure behavior with models. And such models could be the least squares fitting our failure effects models. What we are trying to do is describe the failure behavior with these mathematical models in the simplest way to understand the behavior, a lot more than presently.

This program didn't get started until the last part of the fiscal year. So for '79, what we have really done today is more or less plot the data, the second area that was discussed, where we are starting to get into plotting the data by these factor levels, where the data is being stored on the computer right now. That is presently the status of this NPRDS.

What we are looking at, the components we are analyzing, are the ECCS valves right now, and then we are going to attempt to get the pumps -- they might be -- it depends on how the valves go through. We might do them concurrently. That was more or less the fiscal '80 goals. We are due to analyze the ECCS valves, and then also the

ECCS pumps, and the pumps -- it might be more than just the ECCS pumps. There is not a large enough population of data there and we might look at a larger population for pumps to get better results.

DR. VESELY: I don't see NPRDS data being used in response -- it is not in a shape or form at this time, I believe, to be usefully reported as failure rate estimates to be compared with other data sources. There are too many problems with the data as it now exists in the failure rate records.

DR. OKRENT: As it now sets, it is less meaningful than what was used in WASH-1400; is this what you are saying?

DR. VESELY: The anomalies are at 20 percent. You have heard testimony from other NRC people where even comparisons of NPRDS with LERs shows that NPRDS showed 20 percent reports of what was in LERs, because of loss of mandatory requirements and quality control in plants. We have seen large variation in failure rates with no meaning, three to four orders of magnitude, at least three orders, for example, in the failure rate according to size, with no pattern; and are trying to understand, trying to analyze the causes of these abnormal behaviors.

DR. MARK: Could I ask -- I believe it's said there are about 57 plants reporting now into the NPRDS. There are about 70 plants altogether. Are those missing ones the ones which are most prolific in LERs or just random?

DR. VESELY: We haven't found any correlation, though we certainly haven't looked. There are plants that are high in LERs and high in NPRDS, and there are some that have the opposite.

DR. MARK: It is probably a reasonable sample of the total. Then there is a question about the time of coverage. Your LER time base is '76-'78?

DR. POLOSKI: '76 through '78.

DR. VESELY: We are going back on pumps, back to '72.

DR. MARK: I an wondering if the time base for the LERs and NPRDS is consonant.

DR. VESELY: No. The NPRDS, where we do have sufficient reporting, are approximately one year, '77 -- '78 to '79, at the most two years. If you look at the narrow reporting in LERs and NPRDS, it is consonant. LERs allow you to go back further, to '72. There are approximately 12,000 LERs and several thousand NPRDS.

DR. MARK: But when you are comparing them, as one of the objectives here, you will be able to compare them for the same time block?

DR. VESELY: That is our intent, yes.

DR. OKRENT: Is there anything that you were able to find in NPRDS with regard to a subset of a component of a certain size valve that nevertheless stands out as an anomalously high failure rate, either for a type of plant or for all plants? A low failure rate, you might ask yourself about, well, did they report everything; but if there are lots of reports of failures and if you have some knowledge of the total number of such components either in a plant or in all plants, you have a handle on that situation. So --

DR. VESELY: There are anomalies. There are some components, some valves, which have as high as 10^{-2} per demand, 10^{-3} or 10^{-4}. Our concern is that these anomalies may be due to the way the data are averaged or the way that the populations are estimated or the way the failures are actually manipulated, and may not be real.

DR. OKRENT: By whom? You say averaged or manipulated. By whom?

DR. VESELY: We have the raw data. The estimates that have been produced from NPRDS are in the annual reports of Southwest Research, and they have published average reports. We get concerned that when we look at some of those, the best estimates is not in the 90 percent bound, 90 percent range, which clearly shows in that case a problem in some of their quoting.

So NRC itself has not done any evaluation of the raw data to obtain our own estimates. The only estimation that has been done is by Southwest Research in a very gross manner for their annual report. So we are now instituting these programs to extract the failure rates, to understand

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causes of variations. NRC has not, other than individuals working in the agencies, who may have done this by hand, has not attempted to extract their -- our own failure rates.

DR. OKRENT: Then are you suggesting that, with regard to a response to the questions from Congressman Udall, we would say that before the failure rates available from NPRDS system are reported, more analysis is required; that there is no meaningful information?

DR. VESELY: There may be meaningful information. We have not really assessed what is meaningful and what may be due to the way Southwest simply performs its averaging or its estimation in getting their published failure rates. We have the raw data. I think the raw data in many cases are meaningful. The analysis of that raw data has really not been done to determine what factors are influencing factors, what the populations are, whether you can really aggregate data the way they did.

So NPRDS -- I would not like to criticize NPRDS. I would criticize the way it has been analyzed and -- because it has been analyzed in the reports for one purpose, which is not our purpose, which is not useful for our application, the risk and reliability applications. We can't make a one-to-one comparison for the component failure categories as classified in NP -- or WASH-1400 and NPRDS. We have to redefine and-restructure some of the populations to combine components

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in a similar system or in a population that is similar to that used in WASH-1400.

That is the heart of this process. We have right now raw data. I would not look to any estimates in the annual report as having any confidence.

DR. MARK: I think it has been mentioned, both with the NPRDS and LERs, numbers of failures per demand. How are these demands determined?

DR. VESELY: They are estimated generally by the number of tests performed in a year, usually once a month, or they go to the tech specs on pumps, and the tech specs say, you will test it once a month, then they will assume that you will have 12 demands a year, which corresponds to the number of tests.

DR. MARK: Failures are specifically listed?

DR. VESELY: Yes. That is something else that we are having to check or validate: Are the population and demands actually used, and those which we feel are more representative of the actual demands, the actual population. The population comes elsewhere. That is separately estimated.

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DR. LEWIS: Is there any data base that leads to decisions about the frequency of testing, or is this based simply on experience?

DR. VESELY: I don't know of any data base now. We are getting some information on the in-plant data, where you look at the time between failures and determine time sequences. Even NPRDS. NPRDS has the necessary broad data to do some of the analysis. But constant failure is involved from in-plant data. It is our only source from the length of time required to repair or perform a test or perform maintenance.

NPRDS does not have that information which is important for testing considerations.

CHAIRMAN OKRENT: Are there further presentations with regard to the first general topic?

DR. VESELY: We are doing common cause analysis. and at this time extracting all of the common cause failures that have been recorded in NPRDS and LERs and plant data. They are about 10 percent of the failures.

From LERs, we are getting about one thousand common cause failures. Principally associated with reactor components, common cause failures are multiple components failing on the same day as recorded due to an identifiable common cause, a single human error, a single contamination problem. And I think we will have enough common cause — our

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goal is to analyze the common cause failures to get statistical estimates of common cause failure probabilities to be used in reliability and risk assessment instead of — as a complement to some of the more probabilistic modelling.

We are also pursuing probabilistic modelling and others. But right now, we are identifying common cause failures, trying to observe some patterns, give some basic probabilities of occurrence from our data.

CHAIRMAN OKRENT: I guess —

DR. VESELY: We do have some tentative common cause estimates for valves that we have done and which we will have out in the next several months.

I would recommend that we don't put these into the raw data.

But again, with caveats, if we do put them in, I think it would cause more confusion. I would stick with individual component failure rates rather than getting into some common cause or even human error rates at this present time.

DR. OKRENT: Presumably, if we had meaningful information on either of those two categories as they affect component failure, it would be appropriate to consider including them in the response.

So the question is is there meaningful information?

DR. VESELY: Our plan is we are convening on human errors. We are convening a group of experts task force to re-evaluate WASH-1400's error rates. This will take place December 3rd through 7th from various agencies -- Defense Department, Air Force, and we will have updated human error rates at that time for the basic errors estimated in WASH-1400.

This will come from a consensus estimation from these experts from associated spreads. Whether we want to use those or not, the timing, I think it may be too late because we will not get those estimates until December 7th.

DR. OKRENT: That could fit into our response if that were the case.

DR. VESELY: We will have those on approximately 40 different errors in WASH-1400. We are trying to get a better representation of the kinds of spreads, the kinds of variations that might exist.

DR. OKRENT: If I understand what you have said, at the end of that meeting there might be something --

DR. VESELY: There will be. We are passing out to our experts before that meeting, approximately two months before that meeting these errors and they will estimate and we will convene and we will have these results.

DR. OKRENT: I woud suggest we consider that for possible incorporation into our response.

DR. VESELY: I don't see any common cause at this

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time, us having any meaningful information.

DR. MARK: On your human error data, you mentioned the Department of Defense. What are you going to do about the situation that many of the human errors in the Department of Defense ought to be charged to two-year servicemen and recent inductees. Whereas, in the business we are interested in, most of the people presumably have two or three years' experience in this same job?

DR. VESELY: We are not using experience from the Department of Defense. For example, we are combining or working with their experts, human psychologists and working with teams.

And their experts are going to evaluate the errors specifically described in WASH-1400. And there will be a team of people —

DR. MARK: I was thinking in terms of experience —

DR. VESELY: Out of this conference, we hope to identify further sources of data that may be useful for us. But that was not the immediate thought.

DR. MARK: Thank you.

MR. POLOSKI: Thank you.

DR. VESELY: I have to commend John for talking with three broken ribs. I think he did quite well.

That's all we have on this first topic. Now we have to go down to what specifically would you want the staff

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to do?

DR. OKRENT: Let me raise a few questions that come to my mind and have others then add to this.

It would seem to me we have to decide — not today, but we will have to decide of the new NRC data, which is sufficiently suitable that it should be included as part of this response, and how to handle plant-specific data.

I think that is one thing.

I think we don't want to just present average data. So there will have to be some thought on that.

The next point is how do we get contributions from others than the NRC. And I would say that there are two categories. Domestic and foreign are principal. We have already, or will be asking the safety and reliability directorate in the UK and regulatory groups in Germany and in France whether they have contributions with regard to component failure rates that they think are relevant to responding to this part of the letter from Congressman Udall.

You may already have such information. I don't know. But I think if there is significant information from these groups, and you may have others that you would like to identify, we would like to take advantage of it, if it is possible.

In other words, certainly, in connection with the

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study done on -- the Germans did some evaluation of failure rates and so forth.

So I guess one question is: Which foreign sources should we try to obtain contributions from? And then when we get this information, we will have to figure out how to feed it into some total of information.

The second question is: Are there domestic sources other than the NRC?

It is my impression that there has been some looking at failure rates for maybe specific plants. Maybe EPRI has done it for specific components.

But I wonder if you have thought about the question of contributions from others and what suggestions you have?

DR. VESELY: I take it, then, your approach in this letter is to put all of these various data bases, failure rates and not attempt to distinguish one being better from the other, if there has some meaning, because we are going to end up with perhaps half a dozen or so data sources that may have different values for individual components.

And you are planning to present all of these data sources, the ones we can identify in this letter?

DR. OKRENT: I am not yet at the point of knowing how a report that is attached to a letter, or whatever you want to call it, should be prepared, since right now I don't

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really know what all the new data is that we are going to have. But it is conceivable that we will have information on valves, for example, coming in one form from the NRC. We may have some information on valves coming in the same form, let's say, from Germany or in some modified form. And when we see this information, we will make some decision on how to present it and whether.

The first thing that I am trying to look at is are we getting as much of the meaningful information as we think exists, at least to consider for inclusion in the response?

DR. VESELY: We are working with the LER data. There is also the Project 500 manual to be published by IEEE on electronic components, which gives for certain components a fairly detailed breakdown that tends to -- that manual is out, the German data base.

We have much of that data. The data are different from WASH-1400 by a factor of 10 or a factor of 30, in some cases.

We have, I think, EPRI has done some very good analyses on the control rods and they have some estimates on individual failures.

I think they have done some individual analysis, more no causes, not failure rates. I would have to check on that. DOE, of course, has their data base where they do have

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some information on valve failures, for example, coming from their test reactors, fast reactor data base.

With regard to the Europeans, we have been working with Eric Green, the SRS, and the French with Carnino. The Europeans are establishing and have established through Euratom and CSNI and have for specific plants some specific failures, some specific failure rate estimates.

There is an attempt by that group to try to coordinate and integrate all of the failure rate data. It had not been done.

There are a bunch of individual estimates and the applicability has not been determined.

So, yes, with regard to the Europeans and the domestic, you have over six sources where there are data on, for example, active components. That is the most data, valves, pumps, which are the components which contributed to the most in WASH-1400.

But you are going to end up — there has been no attempt to try to integrate and compare and determine the applicability of one data source from the other at this point.

So we can identify approximately 6 or 7 data sources that we could obtain estimates for the components, component failures which we use in WASH-1400 from these different sources. And in many cases, we have different failure rates, whether you care comparing apples with oranges, whether it is

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plant specific or with environment or what it includes or not. It is not clear at this time.

Some of them give per demand, some per hour, some per both.

DR. OKRENT: I am not sure what it is you are suggesting or saying.

DR. VESELY: I say simply giving data to Udall in this form will cause more confusion than the answer of trying to formulate, giving him all of these sources, all of these different data values, which will be different and will be significantly different in certain cases.

I don't think that we will necessarily answer the letter. I think it will cause more questions than it will resolve.

DR. OKRENT: Presumably, there are tables of data which describe what was used in WASH-1400, both with regard to reliability for starting failure rate running and so forth.

There will exist information on some of these components for some of the considerations involved, data from the NRC or data from the safety and reliability directorate, and so forth, and they may be different.

(Slide.)

DR. VESELY: Here is one which we have done on the German study, WASH-1400 data and its failure rates and the

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failure rates used. And you will see in certain cases orders of magnitude, check valves, reverse leaks, relief valves, failure to open, a factor of 400. Relief valves, premature open, almost an order of magnitude down.

Yes, you do see significant differences in certain components.

DR. OKRENT: All right. What is the problem in reporting that information which we have which we have no reason to disqualify on some basis of incompleteness, or whatever?

DR. VESELY: If that is your intent, then we can supply the data, the different data values that we have access to, as to what are the failure rates used in WASH-1400 in this form.

DR. OKRENT: There might be a column, WASH-1400, there might be a German study, the new NRC data, IEEE. And of course, you will have blank spaces for many components since nobody, or only one person, has any new contribution.

And one can have as many appropriate qualifications as there should be.

It is, in my opinion, better to say these are the differing results and these are the qualifications than to leave the question unaddressed.

DR. VESELY: We can also identify the plant-specific data. If that is what you want, we can get that for you.

DR. OKRENT: Can you?

DR. VESELY: What kind of a timeframe?

DR. OKRENT: We somewhat arbitrarily said that we would try to respond in about six months from August. We have written to the French, Germans, and British and we have suggested that it would be desirable to have such input as we could by early December.

So that in the ensuing one or two months, we could feed such information into whatever else we had and prepare a report. And that six months would get us to January or February, which fits in with your December meeting on human factors.

DR. VESELY: We can give you our input to that in approximately one month, if that is suitable with your timeframe.

DR. OKRENT: By all means. Actually, again, I indicated that we wanted to try to set up some kind of working arrangements.

So —

DR. VESELY: We have also, with regard to our membership in CSNI, have prepared a list of component failure rates such as this asking them for their estimates, particularly the French and the Italians. They are getting that input to us and we can include that in our failure rate list to you.

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In addition, of course, some of these estimates have very different error spreads and uncertainties in WASH-1400. And this would be perhaps identified in separate columns.

DR. LEWIS: I notice that the Germans seem to like two significant figures.

DR. VESELY: Their error spreads, in general, tend to be larger than 1400, as much as an order of magnitude.

DR. OKRENT: I think it would be useful to provide error spreads where we have a basis for putting them on the data.

DR. VESELY: All right.

DR. OKRENT: Should I understand from what you have said that you probably already have the data from the safety reliability directorate from the Germans and the French, that it is likely to be appropriate?

DR. VESELY: We have data for some components that we are specifically investigating, but not all components. We will go back and question them both on additional components to try to complete this list.

Licensing had asked us to do some evluation on the criticality of components. We will go back and ask for additional components.

I think it will be, also be useful for us, the staff, to collect at this time, collate the data that are now

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available and to summarize it in one report.

DR. OKRENT: I think it could be of use beyond responding —

DR. VESELY: We have a hard time from utilities or from vendors trying to get their data. I am not sure how much is there. I am sure that they have data that may be appropriate, that they feel appropriate for specific components.

DR. JOKSIMOVIC: We have our own data bank, and I don't think that we made any detailed comparisons of the type of information that is in WASH-1400, but it can be done very easily.

If you can provide the format, I am sure that we can fill out the format very quickly.

DR. VESELY: We will do that.

DR. OKRENT: Are there any other —

DR. JOKSIMOVIC: Particularly on the common cause failure.

DR. OKRENT: Are there any other groups that you think we should advise of this short-term effort to see whether they have information they can and —

DR. VESELY: I would go to IEEE as another group, which is setting up the same kind of groups to get out a manual like Project 500, but for mechanical components.

That is to come out in June.

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So they have several hundred experts at the various plants using whatever data are available as subjective estimates for mechanical failures.

I think they can provide some estimates for the mechanical components at this time.

We are working with Joe and I would hope that we could have IEEE in on this. I would like to separate the hard data based on actual failures and suggestive estimates, which are expert estimates. They are useful. But we need to separate as much as we can.

DR. OKRENT: Are there other —

DR. VESELY: After we get these data, are you planning then to convene a group or somebody, a task force, to examine the data? Is the ACRS going to do this. With all this data coming in, do you intend to send this to Udall or have some group interpret it or make observations from the data about the spreads or the variabilities?

DR. OKRENT: I only had a tentative idea, and it is very speculative. I thought possibly when we had this information and we had the benefit of your having put it together, we would have it sent to subcommittee members and consultants, maybe a couple of ACRS fellow, have them look at it and then there might be a working meeting set up where representatives from ACRS and the NRC looked at this and sort of first sort of decided on a technical basis whether this jsh

was all of that data that should stay in, or if there was some reason to leave out some of it, or whatever. And perhaps then try to prepare some kind of commentary on the data for the subcommittee then to look at.

And when the subcommittee then felt it was suitable, they would present it to the full committee as a possible response.

Does that sound like an acceptable basis?

DR. MARK: Would you think it is -- it is far from clear that one would confront Udall with tables with numbers of that sort at all.

I say it is far from clear, not excluded, but by no means certain.

His questions require a different answer.

DR. OKRENT: It may be that we have appendices that have tables, but that we have some kind of a one-page or one-and-a-half page response --

DR. MARK: An executive summary.

(Laughter.)

DR. OKRENT: No, not an executive summary; a response that discusses these tables, saying in what areas there was new information that seems to be well founded and where there appear to have been major differences.

So that then if somebody wants to look in the appendix, they would find in detail what you would put into

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words that the editor of the New Hampshire newspaper can understand.

(Laughter.)

DR. VESELY: In looking ahead, the next question, of course, we would expect from Udall is what are the implications?

DR. OKRENT: That may be, but I am not myself going to choose to try to answer a question that may take two years to study in four months.

DR. VESELY: That's right. That is a much harder question and we don't have at this time the modelling.

DR. OKRENT: It is always possible that we will get further questions. And sometimes we have said that we are unable to respond in less than whatever it is.

And at times it could be quite long, depending on what the questions are. I prefer to let that bridge wait for the future.

And when we have this information, it will be the time for the subcommittee to look at it and see if it wants to provide any comments with regard to things other than the actual failure rates themselves.

I don't want to guess now about that.

Are there other things that we should take up in regard to this? It seems like you have it fairly well organized. I think with this discussion, we seem to be in

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fairly good agreement as to what kind of information we should try to get.

We should be able to proceed, and the time scale seems to be about reasonable.

I would say if something is going to be available in February, we will just include it in this.

So what we have when we are sort of closing up the books in early December is what we will report on. That's the way I look at it.

DR. MARK: It was mentioned that some speed of the work, or the extent of it, is at some point restricted by funds. What is the prospect for that as the staff now sees it?

Can we expand the work or at least continue it on the present basis? Or is it in danger of being cut back?

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DR. OKRENT: I wonder if we can pick that up as part of a broader question. We are supposed to take on priorities in the PAS program today.

DR. MARK: That's fine, then.

DR. OKRENT: In fact, if we can get through the priorities in the PAS program, I would like to devote some time to priorities, how this PAS fits in the total research program. In other words, do we think it is in proportion or whatever.

DR. MARK: This is an interesting program, and one would hate to have to see it stop where it is.

DR. OKRENT: That is a general item specifically. If there are no other items here, I would suggest we take a ten minute break before we take on the next topic.

(Recess.)

DR. OKRENT: Is the next speaker here? Let's reconvene.

DR. EDISON: I am Gordon Edison of the Probablistic Analysis Staff. I am addressing the question of Congressman Udall's request for probabilities in the Rancho Seco and Davis Besse events. I must say I am encouraged by earlier discussion this morning to find that a distinguished panel of scientists have somewhat the same feelings that I have wrestled with over the last two weeks, namely the dilemma of wanting to give a reasonable and satisfactory answer to a specific question and not being sure how to give a satisfactory and yet reasonable answer to the question.

So I have done some work, and I might add that I am also encouraged to see that it won't be a solo performance but an ensemble performance, and I have a couple of ensembles here to talk about.

(Slide.)

I would like to begin by saying that I think it is an inappropriate application of WASH-1400 per se to calculate probability on a unique sequence of events. here is part of the reason why.

(Slide.)

This is an event tree for a feedwater transient. This is a Babcock-Wilcox design. The point I would first like to make is that the event tree methodology is, in fact, dichotomous. We have a yes-no answer. At each stage or each protective system, it does not give an answer of what is the probability that a system behaves in a degraded manner. That is, it says, yes, the reactor trips -- so the methodology asks, does a particular protective safety system work such as reactor trip rods and the feedwater, and it says was it successful in performing its function.

It does not ask what the probability is that the feedwater will be delayed seven minutes and then successfully perform its function, or what is the probablity that a pressure relief will be delayed or open above its set point, some small margin, or whatever. It is a yes-or-no kind of answer.

You will see here the end ECCS system. We put in a related kind of logic to show a possible way of handling a degraded situation, but WASH-1400 did not do that. So that to try to predict the exact probability of a unique sequence such as that, say, at Rancho Seco is not possible with a precise application of the WASH-1400 methods and data.

All you can say is that it belongs. We can put it in a category in a sequence along with a number of other series of events which would fit into the same sequence. For example, a successful protection against a feedwater transient, let's say, at Crystal River would fit into Sequence One — loss of feedwater, reactor trips, successful auxiliary feedwater, successful pressure, and perhaps not even — successful use of high pressure injection.

The Rancho Seco event, on the other hand, might have lost its feedwater for different reasons. It might have had a delay in the auxiliary feedwater. It might not have called on the safety valve or relief valve. For example, if the relief valve had locked closed, it would still fit in the same sequence. The events would not be the same. They would not be identical, but you could categorize cBWH

it as being in the same sequence.

Some of these events might be more probably than others. That is, it may be more likely in a successful protection against loss of feedwater that everything would work the way it is supposed to, and the event would shut off. There may be a few accidentals, very low probability that would fit into that sequence.

But what we know about that is that we have average numbers in WASH-1400 for the probability of loss of a system failing to perform a function. I believe the number for auxiliary feedwater was 10 to the minus four per demand in WASH-1400. We also know that auxiliary feedwater systems vary and there are various failure modes of it.

So to try to answer Congressman Udall's question with a precise number for a unique event, we don't feel we can do that. We can simply put it in a category.

Davis Besse — let me first talk about the Davis Besse event. We would see that on this particular event tree as Sequence Two. There was loss of feedwater with a frequency characterized in WASH-1400 as three times a year. The reactor tripped. The auxiliary feedwater system performed its function, not precisely as designed, but performed its function. A relief valve failed to close at Davis Besse, and then it took down sequence to the high pressure injection system which worked satisfactorily, and gcBWH

there was no sever core damage. At Davis Besse, the probability of the relief valve failing to close as predicted by WASH-1400 was 1 X 10 to the minus two, so that you would have an ensemble or an even tree that would look something like this.

(Slide.)

It would predict that the unique series of events at Davis Besse on September 4, 1977, are part of the sequence class. That sequence class is what WASH-1400 would predict, and that is 3 X 10 to the minus two per reactor year.

Now, when we try to apply this to Ranch Seco, it is more difficult.

DR. LEWIS: Can you remind me how long did -- in time — did the sequence at Davis Besse take?

DR. EDISON: That was very rapid. I can give you a little more background on it here.

DR. LEWIS: I was just curious.

(Slide.)

DR. EDISON: That doesn't answer your question, but if I continue on in the comparison —

DR. LEWIS: Were you going to show this later anyway?

DR. EDISON: I don't know if I would have shown it or not, because it takes a lot of time to get into the cBWH

detailed comparison of these three different reactors. I would say that Davis Besse was over in certainly less than a day. It was a couple of hours. But, you see, if I can dig back in my memory, what I am wrestling with is a series of events at Rancho Seco, series of events at Davis Besse, and also TMI at the same time, and it is easy to be flipping back and forth between them.

DR. LEWIS: If you don't know, then you can lend me the viewgraph.

DR. EDISON: The events at Davis Besse were a loss of main feedwater, which was initiated by a faulty buffer card in the logic control system of the steam feedwater rupture control system, which caused a spurious half-trip, the close of one down in the feedwater system, which then caused the level to change in the steam generator -- caused the loss of main feedwater.

At the same time, the auxiliary feedwater was actuated. However, the relief valve did stick open in th primary system just as it came up. This reactor was in a startup phase. It was at very low power. There was low burnup on the core, so there was never a real hazard to the public, I don't believe, with this plant.

High pressure injection was initiated. The operators never really felt threatened by this event. They had the core under control, as I recall, within an hour.

It did not stretch on and on.

DR. OKRENT: My recollection is that they didn't notice that the relief valve failed to close right away, but they did after some fraction of an hour.

DR. EDISON: At 20 minutes, they did not diagnose that the relief valve was stuck open, and they closed the block valve.

DR. LEWIS: That is what I was groping for. At what points did they intervene, because this dichotomous analysis typically runs without human intervention.

DR. EDISON: Yes.

DR. LEWIS: One thing I think is very clear from TMI and all of the other things is that you are just not going to go very many minutes without human invervention, for better or worse.

DR. EDISON: The event was not as severe. It was a milder transient.

MR. ROWSOME: Their first response was to address a partial failure in the auxiliary feedwater system. One of the two pumps did not come up to speed, although the other was performing its function. My recollection is that they got the other pump running quite quickly, didn't they? Even though they didn't need it, they met the single failure criteria. They had one pump running, but they went to work on the other one?

DR. EDISON: Twelve minutes.

MR. ROWSOME: They got it going in twelve minutes.

DR. LEWIS: When did they close the block valve?

DR. EDISON: Twenty minutes.

MR. ROWSOME: They also throttled back on high pressure safety injection, which in this case because of the low power level, was probably an appropriate behavior. They did that fairly early in the incident too, in the order of ten or twelve minutes.

DR. LEWIS: Those are three things they did essentially in the first twenty minutes. Now those don't show on the fault tree, so the fault tree is irrelevant after twenty minutes.

DR. MARK: Is this not the one in which the PORV cycled nine times?

DR. EDISON: Yes.

DR. MARK: It closed eight times and stuck open only on the ninth.

MR. ROWSOME: The valve, itself, failed because of human error. There was a relay missing from the control cabinet for that valve, and it just physically wasn't plugged in. And the relay was part of the circuit which provides the dead band between the open and closed point. Because of its absence, the valve had no dead band and being compelled to go from lock to lock as fast as the thing could cycle it, it chattered until it burned itself out.

DR. LEWIS: This was an infancy problem which would have been discovered eventually, which it was.

MR. ROWSOME: Yes.

DR. OKRENT: It is not necessarily an infancy problem, because it could occur when the plant was an adult also. Somebody in maintenance could have —

DR. LEWIS: It is the bathtub curve. It is more likely at the beginning. The only thing I was really groping for is the original comment that Davis Besse was — I have forgotten already — Sequence Number Three on that list — Number Two is really not quite right as a description of events, because very early in the game there was a lot of human intervention which doesn't appear here.

DR. PLESSET: Did the pressurizer go high off scale?

DR. EDISON: Yes, it did.

DR. PLESSET: That's when they throttled the HPI?

DR. EDISON: No. The pressurizer went high off scale, and they observed that, and I don't have knowledge of whether the operator was clever enough to deduce that that was due to a steam formation or swell or whatever. In a later report, they mentioned that it went off because of steam formation. They did not throttle the HPI at that time. I believe they had already throttled the HPI, and

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they watched it go off scale. I got the impression, although I have no basis for stating it as a fact, that they understood that there was a swell going on in the pressurizer. They turned it off after three minutes, right around three minutes, similar to TMI-2 and that experience.

MR. ROWSOME: Do you know offhand if this occurred before the I&E Bulletins urging caution about running water solid had been issued?

DR. EDISON: I don't know. As far as its applicability to -- of the WASH-1400 approach to the Udall question, we can say the event falls into a category. The probability is not the probability of that event, and I don't know how to give that probability. It gets messier with Rancho Seco.

I think that Rancho Seco is a smaller piece of a category.

(Slide.)

Rancho Seco, we would say it was in Sequence One. That is success of all systems required to prevent core melt. So we make three points.

WASH-1400 did not quantify the individual failure mode for the main feedwater system. That is, it assumed there was a category of transients, whether they be feedwater transients or loss of electric power or whatever. It did not ask in the category of electric power transients how many were due to an out sitting on the transmission line. It took the complete category. It did the same thing with main feedwater. It did not ask how many were due to loss of condensate pump or water getting into an air system, so the number that WASH-1400 used on the data at that time was three feedwater transients a year.

In the Rancho Seco event, no major safety systems required to prevent core melt failed to perform. There was some degraded operation. That is the auxiliary feedwater system was delayed in coming on for some eight minutes. In fact, the reason it did come on was because a steam generator level signal was not available, but the level drifted low and the signal then caused the auxiliary feedwater to come on.

So it came on. It was successful. It performed its function, and WASH-1400 does not ask the reason for that.

DR. PLESSET: Could you remind me what the nature of the main feedwater failure was at Rancho Seco?

DR. EDISON: The cause was a short in the non-nuclear instrumentation which resulted from a maintenance error in which a light bulb was dropped into a socket or into the wiring. It caused a — a fuse did not work, is what it amounted to. The fuse did not work, and it caused the circuit breakers to open in the AC, 120 volt AC, that feeds the 24 volt DC non-nuclear instrumentation. This caused the main feedwater, the ICS, to run back to the main feedwater.

DR. PLESSET: They are doing some repair on the control board?

DR. EDISON: They were repairing a light bulb that had burned out.

DR. LEWIS: What is the probability of that?

DR. EDISON: Then you have the auxiliary feedwater system which was delayed. It still worked successfully but was working in a degraded manner with less margin than it was designed to have.

DR. LEWIS: Why was it delayed?

DR. EDISON: It got no signal. It gets it signal from the steam generator level. The steam generator level -- the level signal was simply drifting when shut off from the DC source. So to try to go back and do something like this and show a probability that the auxiliary feedwater, for example, worked in a degraded fashion, not yes, not no, but a degraded fashion, it ended up yes.

Using the WASH-1400 methodology, the answer is yes. We followed the yes chain across and found out that no, the core did not melt, and that is exactly what happened.

If you want to get down into a little more detail,

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you can possibly go back and ask what is a fraction of losses of feedwater that occurred due to a short in a panel. WASH-1400 did not address that kind of breakdown. It wasn't necessary.

What we can say is that the Rancho Seco event is one of many successful reactions of a plant to a feedwater transient. It is not the most probable reaction. It's probable reaction would not be due -- the cause would not be a light bulb. The most probable reaction would not be a loss of auxiliary feedwater in eight minutes.

The most probable situation would probably not be that they had the relief valve locked closed because it had been leaking, so they went down this path -- Sequence Five, but this probability would be considered to be one, because it had intentionally closed the pressure relieve valve. What I am groping for is how to get this across to Congressman Udall in a way that doesn't make him unhappy because he feels that we are not being cooperative.

DR. LEWIS: I think that is less of a problem than we are acting this morning. I think that he has some very specific questions which I don't think are -- which have two motivations.

One is that he is kind of the daddy of the LER study, and he has heard that it's nearly finished, and he wants to be sure that there are some things which are on his and his staff's minds that are addressed, like the validity of the data base of WASH-1400, now that we are looking at LERS. And he also wants to address the question of whether this kind of methodology, which will prepare you for the kind of things that happened at Davis Besse, Rancho Seco, and Three Mile Island.

I think he wants those questions answered clearly, and I think they can be answered responsively without doing improper statistical analysis, which, of course, we won't do.

DR. PLESSET: You guarantee that?

DR. LEWIS: I guarantee that.

(Laughter.)

DR. OKRENT: I won · if when someone says, I am interested in knowing what you would compute for the probability of some specific event, which means the specific failures which occurred in the event, whether it is appropriate to do it only, let's say, within the framework of this event tree that you have put on the screen.

It seems to me there is an interest in seeing what this methodology does when you try to look at multiple even sequences where several different things occurred, maybe because of a common cause or it may be for other reasons and whether the methodology is meaningful for this, if it is meaningful in what context, and so forth.

So I must say when I think about the Rancho Seco transient, it seems to me I might equally well ask, what is the chance of my losing the various services that I did, due to the particular short circuit, and what I suppose you might say is there way of estimating this using the WASH-1400 metholdology. Or do you have to say this was a transient that incorporated a common mode or things to occur which is not easy to assign a number to, or whatever?

In other words, you might look at the same transient, it seems to me, from another perspective when you try to compute its probability. There are a range of things that occur during it. And with regard to feedwater, it is not only a question that I have it completely, or did I not have it. It isn't that tight. In fact, in this particular sequence of events or this particular series of events at Rancho Seco, the operator did bring the main feedwater back on during the transient at about seven or eight minutes.

DR. SAUNDERS: May I just say that when the fault tree analysis, which is the graphical representation of the structure of failures to be represented as Boolean functions of Boolean events, the events are neither good or bad. The graphical repesentation of that is what is called a fault tree, the analysis of that.

Now the calculation of the probabilities that arise from such structural functions is fairly straightforward. It has been done in '68, and we now how to do that. To make the first step beyond that where you classify things as good, partially degraded, degraded step two, step three, or step four has just now been completed. But that requires that you embed Markov chains into the structure function to do what you, sir, and I and all of us would like to see happen is to embed this into a continuum of degraded performances.

It raises mathematical probabilities, difficulties which I think would require another ten years, so we are stuck to do any quantitative prediction or analysis on what we know. And so that is the dichotomous events, and all we can do is hope for the future.

DR. EDISON: There is still a question in my mind as to what you would like to give to Congressman Udall. That is, if his total interest is how good was WASH-1400 for answering this kind of a question, then other kinds of analyses don't seem to play a role in that answer. If your purpose is to go analyze these events with whatever we now have and -- then we can do something like that. But you would be talking about a considerable effort, that is, looking through data for what are the statistics of shorts occurring in a B&W plant as opposed to, for example, a Westinghouse plant which was analyzed in WASH-1400.

We could do analysis. We can always do analysis and more analysis on our problem. But now you would want to ask the question: Would this be a useful way to divert resources away from improving the safety of reactors?

DR. LEWIS: There is another way of approaching an answer to Congressman Udall. I keep thinking, he is not being malicious. He is asking a question which, while it may not have been phrased in the most efficient way, and therefore is dumb as written, still is a meaningful question to him. One can answer it by going step by step down the event tree. For example, one can say that, although the one at Rancho Seco was really peculiar, because I am sure he has in mind the probability of somebody dropping the damn lightbulb into the guts of the machine -- but one skips that and just goes directly to loss of feedwater.

I can imagine phrasing a reasonable and satisfactory answer that went something like this: Loss of feedwater, three times a year. Well, that is what was used in WASH-1400. How is that running these days? Is that a reasonable number?

I am sure that we have that data, and we could say: Okay, this is the event tree for WASH-1400 or something close to it. In losses of feedwater, WASH-1400 said that the reactor would trip what fraction of the time and will fail to trip some other fraction of the time. The fact is that it has never failed to trip, but the number of times involved is sufficient for whatever the lower limit is in WASH-1400.

So then I could imagine going two or three down, comparing the probabilities from the whole ensemble of events that have occurred which are like that with WASH-1400; after two or three steps, getting down to the point at which the idiosyncrasies of that particular event begin to make it a population of one, and say that at that point statistical analysis is meaningless, because it is then a population of one; and also, at that point operators are beginning to intervene, so statistical analysis doesn't make a lot of sense. I could write something like that that I think would make sense.

MR. ROWSOME: And we can supply you with the data at key points. I would envision it as a one or two-page essay,

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lead off with a one or two-page essay on the limitation of event trees and the problems of ensembles. There is a natural ensemble for Davis-Besse because you can identify the feedwater transients and the stuck relief valve. That is easy to do.

You can say WASH-1400 predicts recurrence intervals for that kind of thing of once in 33 years, with a half order of magnitude high or low, roughly. And that in fact is consistent with experience.

DR. LEWIS: Right.

MR. ROWSOME: It is a little more difficult with Rancho Seco, but one can go through that sequence of progressively narrower ensembles that you suggested, to the point that it becomes nonsensical. I think that is an excellent point.

DR. EDISON: The WASH-1400 data -- that is simply lifted out of WASH-1400.

(Slide.)

That is nothing new. But the numbers don't apply, except in the first one, because we have a one.

(Slide.)

That is at each stage. Even though this particular transient wasn't a severe transient, it was a significant thing. There was a lot of margin loss by these instruments not being available. There is no question that this potentially could have been a much more severe situation. It is one of that

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small fraction of the three a year. It is a more severe successful feedwater transient.

We can do more analysis of different types.

DR. OKRENT: Does somebody here have a description of the Rancho Seco transient? I left my copy on my desk, unfortunately.

DR. EDISON: I have some fairly thick information in my room upstairs on the inspection reports and so forth. Do you have a specific question?

DR. OKRENT: I think it would be useful to discuss a little bit more the specific events that occurred to see how they fit into the framework Dr. Lewis was talking about, and whether there is a single framework of that sort of multiple frameworks or whatever.

You are in this hotel?

DR. EDISON: Yes.

MR. ROWSOME: We can start on priorities now, if you like, and take this up after lunch.

DR. OKRENT: I think it would be useful to come back to this question with the details of the transient more specifically in mind. Why don't we accept the suggestion just made, that we come back to this topic and start it with a five-minute description of just what transpired during the event. And then we will let Dr. Lewis see how he would propose putting this in some kind of framework.

DR. LEWIS: I am beginning to notice that I am falling into the usual penalty for suggesting something.

(Laughter.)

DR. OKRENT: If we can get only one framework that seems logical, and then you may win the prize. But if we can get five, then we may be able to distribute the prize.

(Laughter.)

DR. LEWIS: Do you have viewgraphs on both of those events which go through the sequence?

DR. EDISON: Only to this extent.

DR. LEWIS: I don't want you to show them, but if I could look at them I would be grateful.

DR. EDISON: I do not have a chronology.

DR. LEWIS: You don't. Okay, fine. Then let's forget it.

DR. MARK: Could I ask, is the maintenance which was being done on Davis-Besse a kind of operation which could equally well have been performed at full power, or is it restricted to the 90 percent kind of situation?

DR. EDISON: In the case of Davis-Besse, it was not maintenance. It was spurious --

DR. MARK: Switching lightbulbs on the instrument panel.

DR. EDISON: That was at 72 percent power. That was Rancho Seco.

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DR. MARK: That could happen any time.

DR. EDISON: Yes. The reactor was at 72 percent power, and you would think that one would be able to take a lightbulb out behind the button in the control room and replace that at 70 percent power. As it happened, the fuse failed that was supposed to isolate that button from the rest of the system, and in addition there was apparently some change in the design of the circuitry of the non-nuclear instrumentation system earlier, which made it a little more susceptible to the entire system blacking out as a result of the fuse failure.

That has been corrected, so you won't see this one again, I don't presume.

DR. OKRENT: Let's try to come back to this one, and perhaps even on the Davis-Besse one, if you have a somewhat detailed description of the actual events, bring it along.

DR. EDISON: All right.

DR. OKRENT: I had intended to bring my own, but with all of this paper I seem not to have it.

(Slide.)

MR. ROWSOME: This gives you an outline of what I intend to talk about. We share with you the perception that a major reassessment needs to be made of our priorities and focus. We are just beginning to do that. The job is far from complete. But patterns are beginning to emerge that I would like to discuss with you, and I will follow this outline.

I want to discuss first our thoughts about an integrated reliability evaluation program, effort to develop reliability models for all of the operating plants; second, to give you a management perspective on the exercise in reassessing priorities and focus; and to give you a very brief status report on what has been happening with the improved reactor safety program; and then open up a general discussion of the technical perspectives, the technical aspects of the priorities.

There are many lessons we could have learned from WASH-1400 that didn't really take root until TMI brought them home: As you yourselves have pointed out, the importance of small LOCAs and transients, the importance of human errors, many others like that.

One that has been brought home to us by the auxiliary feedwater reliability study which you have heard described to you, that we did in conjunction with the Bulletins & Orders Task Force in May of this year, is the extreme startling variability of system reliability from plant to plant. We should have seen that implicit in the WASH-1400 results.

Five sequences were found to dominate the risk in WASH-1400, and every one of them related to aspects of system

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design or operating procedures which clearly were not standardized by the regulatory requirements under which these systems had been designed. For example, TML B prime, the accident sequence in the PWR involving station blackout and failure of the auxiliary feedwater system. That is not a design basis accident, and in fact, until the reactor safety study came out, auxiliary feedwater systems weren't even considered as engineered safety features systematically reviewed by the AEC.

Interfacing systems LOCA. Well, we have a standard requirement that there be a double pressure boundary on containment penetrations. But beyond that, nothing approaching criteria that would impose a uniform failure rate on these crucial pressure boundaries. The recognition of the hazard potential of an interfacing systems LOCA, that it is a triple common mode failure that involves a LOCA, a breach of containment, and an inevitable failure of ECCS on recirculation, if not sooner, because of the dry sump, had not really been widely recognized before this study.

S2C was the third dominant sequence in WASH-1400. We found in Surry a susceptibility to small LOCA because the recirculation pumps could start and run on a dry sump before the sump would be flooded by blowdown in certain classes of small LOCAs. In fact, as you look at the several other indications in analytic studies and experience that

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small LOCAs may be important to the risk, they all seem to have elements that suggest either a design error or a failure to anticipate accident sequences.

In the German risk assessment, the small LOCA was also found to be a dominant contributor to the risk, and in this case because of a design in which ECCS high pressure, ECCS cannot be recirculated. Therefore, the operators have to conduct a very rapid cooldown under small LOCA conditions to get the system on the residual heat removal system before the injection tanks are pumped dry. The difficulty in doing this is responsible for the prominence of that class of accidents in the Biblis B study.

And in Three Mile Island, we saw that the susceptibility to small LOCA, related in part to the fact that the pressurizer relief valve was challenged so often in B&W plants, at least before the Bulletins & Orders fix, antedating anticipatory trips, and because of a failure to anticipate the symptoms of a failed-open pressurizer relief valve.

The common elements in these things seem to be a failure to anticipate accident scenarios, accident scenarios that relate to specifics of the design of the plant. So this leads me to a conclusion that we must do a great deal of work, as you yourselves have suggested, to identify accident scenarios with enough resolution to pick up plants' specific idiosyncrasies.

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That leads us to propose a reliability program aimed at developing reliability -- ultimately developing reliability models for all of the operating light water reactors. The first phase in this program is a piece of work we had already started before TMI. It is ongoing now. It is scheduled to be brought to completion in fiscal '80. It is an effort to collect data dealing with design and procedures sufficient to assess the susceptibility of operating plants to the specific accident sequences found to be dominant in WASH-1400.

The larger effort, phase two, the integrated reliability evaluation program proper, is scheduled to start soon and run for about two years. And the objective is to develop plant-specific core damage or melt event trees for all of the operating light water reactors, and to develop fault trees -- "core melt" is a bad word. I should have said core fault -- resolution for the key systems participating in these event sequences for all of the light water reactors.

DR. MARK: When you say all reactors, does this mean 70, or can they be grouped into maybe 10?

MR. ROWSOME: That is what we want to find out as we go along. I don't want to be presumptuous about how generic and how broad a brush we can treat this. Of course, we want to take as much help as we can get from commonality and not reinvent the wheel every time we go through this process, 70 times.

On the other hand, I think one of the central abstract and most important lessons we can learn from Three Mile Island is that even those of us who regard ourselves as unbiased and unprejudiced on the subject of nuclear safety can slip into presumption very easily. And I don't want to be presumptuous here, that having a generic event tree for B&W reactors is going to cover what could prove to be a critical factor important to one of the dominant sequences and one of the design variants.

So that we will certainly be looking at the extent to which we can do this in a generic fashion. I don't want to be presumptuous about it.

EPRI contracted with SAI to develop generic event trees for light water reactors. I have been talking about Bob Erdman, about his experience. The further he pushed it, the more he became convinced that he had to go to greater and greater plant specificity. At first they thought maybe for each of the LWR vendors, we can have one package of event trees; and then, well, then, maybe for each NSSS design, for each of the vendors. And then the variants began to look more and more important as they got into the details of it.

I think his conclusion was that the effort to do generic event trees was doomed to failure. There were common elements and to define what the common elements are would be a very interesting piece of knowledge. It would be very useful, for example, in identifying to what extent we can in simple regulatory language address these problems, and to what extent we may have to get into plant-specific design, plant-specific data.

One of the things that deeply disturbs me is the overwhelming disincentives that are acting on the industry for them to take an active part in the quest for improved reactor safety. The combination of economic incentives and the structure of the regulatory process just provides an overwhelming disincentive for them to not be too creative or too original or too inclined to rethink their investments and their initiatives.

I think the cure for that -- we have got to look for cures for that, and I think one of the ways to look for cures for that is to try to move regulation toward performance-oriented criteria rather than a lot of design-specific criteria; and to know how to do that and do that right, we need to know a lot more than we know now, for which this kind of study would be, I think, an essential foundation.

(Slide.)

A few of the objectives of this program: First and foremost, to identify the outliers, the plants that may have core melts and are more significant and probable. The auxiliary feedwater study suggested there might be, in the absence of recirculation pump trips on BWRs that could give

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you an order of magnitude on ATWS -- there are many clues that there may be some individual plants that may have core melts, that are significantly more probable than WASH-1400, and we want to try to pick those out as quickly as we can.

Second, we want to provide a foundation for a wide range of plant-specific reliability studies. One of the reasons that the integrated reliability program has the outline it does, the choice of event trees, the choice of fault trees, as opposed to reliability block diagrams or go-codes or what-not, is that the fault tree-event tree approach we think is very flexible. It is expandable. You can quantify a fault tree in several different strata, strata defined in terms of the coarseness of fault resolution. And we think -- and we are going to specify a detailed prescription of how these fault and event trees are going to be done; that they be expandable and flexible in such a way that we can go in and use them for studies like fire susceptibility, floods, systems interactions and so forth.

They will not, in the versions we will be developing in this two-year program, have the detail to flesh out those kinds of studies without further work. But we want a foundation on which we can build, a foundation that will accommodate the kind of detail that would be necessary to answer that kind of question.

We want to use it, too, as a framework to bring in the line offices, to bring in NRR, I&E and what-not, to get them to participate in this effort, to get them to get their feet wet in thinking systems, accident scenarios, systems reliability. This would be absolutely necessary if we do succeed in coming up with an acceptable risk criterion, to build a foundation with which they can assess compliance. And it will provide a forum in which we can broaden the base of training and get some hands-on experience with reliability analysis.

Saul has been talking to Harold Denton about getting anywhere from 10 to 30 NRR people to participate in the drafting of event trees and fault trees. This might not be ideal from the point of view of cost minimization or speed with which we can do the work, because we suspect that bringing these guys up to speed may cost us more in time than they will give back in the work they do. But we think that is a price that we must pay to broaden the base of people who have this experience, who have gotten their hands into the process of struggling with these analyses.

(Slide.)

There is a long, long list of projects involving fault tree or event tree work for which these results would be useful. This is only a partial list and it will give you some indication of the scale of applications for this kind of work: methodology applications program, fire, floods, a program to analyze the test maintenance and accident response procedures, seismic issues, program to analyze LER implications, operations evaluation program.

They will need models with which to assess the significance of the events they see. And then there is risk inspection modules, advice we are scheduled to give to Inspection & Enforcement on how better to use their time in risk-relevant ways.

(Slide.)

Studies of ECCS reliability done in NRR, auxiliary systems analyses, improvements to the single failure criterion, reliability analysis of operating systems. limiting conditions for operation, and so forth and so forth.

I don't mean to oversell this. These event trees and fault trees will not be the answer to everybody's problem. They will be too cursory an outline to solve all the problems you might like to ask of the reliability models. But we do want to build a foundation. We do want to build a base.

You may be tempted to laugh at the idea that a group like PAS, which has had such trouble getting out the methodology applications program, the study of four plants, should now be embarking on the study of 70 plants. I do think that it is possible to do.

Sandia has been doing fault trees in the sabotage context for all of the operating plants. And while these

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trees are not quite what we are looking for, their success indicates that it is feasible to do projects of this scope.

The discipline of fault tree analysis is well enough developed that I think we know how to standardize it, to package it, to write specifications to get contractors and others to do this work and to produce a quality product.

There is a bigger question about the event tree analysis. That is much more of an art, much less of a science. And I will touch on that again when I talk about priorities.

DR. MARK: Before you go on, you had on there "flood."

MR. ROWSOME: Internal flooding.

DR. MARK: Does this have to do with the effect of an assumed flood on the machinery?

MR. ROWSOME: Yes.

DR. MARK: It doesn't send you around the country looking at drain spaces?

MR. ROWSOME: The systems indications.

DR. MARK: You referred to the great success with the studies of sabotage. Could you tell us just what the success was?

MR. ROWSOME: I am not fully versed on this program. It is being done through the SAFER division of research in collaboration with the line offices. They have drawn fault trees that go all the way up to unacceptable consequences. They are structured to identify single-point sites where a sabotage act could produce core damage or a breach of containment. One of their criteria for unacceptable consequences. I think pressure boundary is one of them, and failure of the shutdown decay heat removal function is another. I think that there may be a couple of others.

DR. MARK: I am aware of the fact that they have many calculation packages which can compute all of these things if you knew what to put in for input. But that doesn't sound like success in sabotage control.

MR. ROWSOME: The experience has been a success in the sense that they have and are drawing fault trees for all of the operating plants which are not utter nonsense, that are useful for the purposes for which they have been drawn. They do pursue faults through systems to identify a co-locaton, to identify where there are single-point sites and double-point sites, where two different locations in a plant, a sabotage act would be sufficient to give you -- or could be sufficient to give you an unacceptable consequence.

I can't speak to whether that task is successful in the context of dealing with the sabotage issue. I am not confident that it is. But I don't know whether it is or not. I think it is indicative that it is feasible, with reasonable dollars and talent available, to do fault trees

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on 70 plants. That is the only context in which I meant to indicate it, that I wanted to call it a success.

(Slide.)

Now I would like to turn to the management perspective on the reassessment of priorities in focus. This slide is simply intended to give you a taste — I can't seem to find it; I will just talk from the paper. I cataloged some of the activities, classes of activities of the probabilistic analysis staff, and threw out a few, an incomplete list of examples, to give you a flavor for the kinds of work we are doing and the kinds of constraints that places on our time and our planning and our budget.

We are doing a great deal of work in direct support of the line offices, collaborative work with NRR or other divisions, offices, to respond to requests to review documents such as the siting policy task force report, to help lay the groundwork for the operations evaluation group, to assist in emergency planning, to assist in the Lessons Learned Task Force with ways to improve upon the single-failure criterion, to assist Denny Ross' group in the specification studies to be required of licensees.

And this work has grown exponentially in the last several months. It was growing even before Three Mile Island, and if we were to do it all, it could easily occupy a group twice the size of PAS full-time doing this kind of

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thing alone, with no research at all.

You have suggested to us, and we quite concur, that there are any number of applications of probabilistic safety analysis that really ought to be pursued. Here is an incomplete list:

Improve reactor safety; the methodology applications program; we were recently given the station blackout generic safety issue TAP A-44 by NRR to do. We got the DC power issue. Risk ranking of NRR concerns in a number of contexts. The systematic evaluation programs. The RQC category 2 issues; that is, the question of whether some of the ratchets will be backfit or not, is another. There are several others. Risk ranking of research endeavors outside of PAS. Accident precursor analysis. This, too, is a sphere of work which can easily occupy a group much larger than PAS is now.

And finally, there are advances in the state-of-the-art in probabilistic safety analysis. There are many questions we don't know how to answer today that are clearly important, that need attending to: how to deal with operator error, how to deal with common-cause failures, continued analysis of development of failure data, to look more deeply into accident scenarios that could wind up in this intermediate space between the design basis accident and full core melt.

The work developing probabilistic safety analysis methods and models for waste repositories. The liquid pathways work. Many research areas where there is a clear and perceived need for advances in the state-of-the-art.

This is a tremendous scale of work, and there is no way on earth we can accomplish it with our present resources, present number of people, present budget. We can't do all these things. So, there are some very hard choices that have to be made.

(Slide.)

I have a list of seven items here. I hope it is in your Xeroxed copies. This is of the prospective growth in probabilistic safety analysis. The first, we have got to off-load as much of the applications work onto the line offices as possible. Now, that is a goal in and of itself, quite apart from our workload that you all have pointed out. The Lewis Committee has pointed out, your letter in July on the budget, made that point. Numbers of other letters have suggested that probabilistic techniques be brought to bear on the licensing process.

But it is now a matter of necessity if important research applications are not to suffer severely. We need to improve the productivity of the probabilistic analysis staff. We need the maximum possible growth rate for the probabilistic analysis staff. We need improvements in contractor productivity. We need to enlarge the role of RSR and SAFER in risk-related research. Expanded use of reliability studies required of licensees and applicants. And ultimately, possible reorganization of the NRC research and/or the probabilistic analysis staff. Training and adoption of probabilistic safety analyses in line offices.

(Slide.)

We have got some initiatives going in this field. The first is an executive seminar that we are hoping to schedule in the last week of November. The objective is to take about a day and a half, meet in a large hall, hopefully attract a large percentage of the people, from branch chief on up, from the line offices. Sol Levine is talking to Lee and Harold and others to encourage their participation, encourage their support.

The objective of this seminar will be to focus on the future, not rehashing WASH-1400, but to look at the ways in which probabilistic safety analysis, reliability engineering, and risk assessment can be useful in providing a new foundation for regulation. We will be asking one or two of you to participate in this, I think, as speakers.

That will be followed by a rerun of our system reliability analysis course, to a rather smaller audience, that will get into the details of how-to methodology and applications of system reliability analysis. We have initiated an effort to reexamine, overhaul, and expand the system reliability course, very much as you suggested in one of your letters. And I perceive a need to go beyond that and to develop some new courses and educational materials in the process of laying out the topology of accidents -- to use Harold Lewis' lucid phrase -- the scenarios, the event tree work.

This is an area in which there is very little literature and very little organized this-is-how-it-is-done kind of material. I believe there are a lot of resources out there, but they haven't been pulled together. There is the model formed by WASH-1400 itself; there is the barrier penetration model that Carnino in France has developed; there is the levels of assurance concept that Frank Gavigan has worked up in the context of the LMFBR research at DOE.

There are a variety of ways of attacking the problem of classifying and identifying accident scenarios, and we are going to charter -- contract for some efforts to develop training manuals and educational materials that will expose people to these techniques, to these ideas.

Second, as I mentioned before, NRR will participate in the integrated reliability evaluation program. We are going to get them to work on it. Second, there is some evidence of movement in NRR, some initiatives that they themselves have come up with. The Bulletins and

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Orders Task Force came to us for the auxiliary feedwater study. They have come to us more recently asking for advice and guidance on how to specify studies to be requested of the BWR owners group, to address some problems that they envision that they are concerned about in ECCS actuation.

They recognize the possibility that if they were to address these concerns with ratchets, that they might be increasing competing risks that they hadn't foreseen, and that they want a more systems perspective attack on this problem. They asked us to give them our assistance in specifying those studies.

The Lessons Learned Task Force has called for improvements in the single-failure criterion. It is looking to reliability criteria. It is looking to ways to take credit for the reliability of nonsafety systems. They are just beginning to come to us asking for our assistance in that regard.

These, I see, are evidences of the work --

DR. MARK: You spoke of a course to be given to people from branch chief on up, and the people who would do the actual work in this field are presumably people from branch chief on down.

(Laughter.)

I can see you getting the branch chiefs very enthusiastic in thinking of all kinds of problems, but they

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won't have anybody who can apply the techniques unless you give a course for them.

MR. ROWSOME: Right. Well, that's what the training courses are intended for.

Last year, the training courses were mostly conducted in-house with very little contractor support. This year, we are contracting for studies — studies of the course material, the way it is presented, to bring in consultants to do more of the teaching, people who are good educators, as we are not. We are working on courses to fill that need.

DR. MARK: These will be to reach down to staff members?

MR. ROWSOME: Right. The role I see for the executive seminar is not just to get people enthusiastic. I think that there is a broad misconception that risk assessment reliability probabilistic safety analysis is limited to WASH-1400, that's it. And you know and I know that that's not true. But our perspective is not shared by the majority of people in the line offices.

I want to prod them to think about the rich diversity of options for tackling their problems, that exist within this field of probabilistic safety analysis, reliability, and risk assessment. And I think, when the need is recognized and the goals, the objectives, when management says, "Ah nah, there is a tool out there that will solve this problem for me," then training the line engineers will be an easy process. It will follow. It won't be a critical path item, but so long as so much superstition surrounds reliability and risk assessment, that is not going to happen. That is why we established our priorities in that context the way we did.

(Slide.)

Improvements in the productivity of the probabilistic analysis staff. Right now we have bitten off rather more than we can chew. The competing requirements of fire drills, of contract management, and long- and short-range research, of assistance to the line offices, is causing us to give far too little time and attention to any one of these things to do a good job on all of them. We are going to have to develop procedures to be much more hard-nosed about saying what we will do and what we won't do, and attempt to do a good job on a few things rather than a superficial job on many.

The second approach -- research tasks through iterative refinement. One way to do this is to perform quick-and-dirty top-level quantitative studies in conjunction with rather careful, rather good uncertainty analyses, identify which terms dominate our ignorance, if you will, what are most important for successive refinement, and to focus in on those in an iterative fashion.

That has the advantage of giving us preliminary results early, keeping us informed of the state-of-the-art, keeping the world informed of what we know now, what we can do now, and what we don't know; and provides a way of tackling large projects in a way that gives us preliminary results in useful form as early as possible.

In more conceptual cases and in report-writing and verbal tasks, I have found it to be a useful discipline to write the report first, identify where it is weak, and let those weaknesses, that peer review process, if you will, scope the next research step, and then rewrite the report and so forth, to do an iterative process of report-writing. This, too, has the advantage that it produces results quickly; it has the advantage of imposing a discipline among -- on the analyst, of organizing his thinking.

I think too many of our studies have been built tower-by-tower, in an architectural example, and we haven't been able to use the foundation until we were done. And one of the ways to improve our productivity is to build by layers, each of which are complete in and of themselves and useful in and of themselves.

In the analytic work, in the uncertainty context, I discovered this process some years ago when I was doing system reliability analyses at Bechtel, and I worried about doing very coarse, crude analyses, for fear that the omissions in -- would leave studies with very poor treatment of the completeness problem.

My experience has been just the opposite: that by taking a broad view of the problem, you are much less likely to lose sight of the forest for the trees, that you get a better frame of reference on what is important and what isn't, and your productivity is improved to the extent that you can home in on the key weaknesses of the study with much more certainty and much more quickly than if you attempt to do a thorough detailed analysis from the outset.

So that I think the concern I had, and that you may share, that this kind of iterative approach may be vulnerable to serious problems with completeness, is a non-issue. I don't think it is a problem. I think it is a good way of coping with the completeness problems.

You may have other thoughts on that matter. I see some quizzical looks.

DR. SAUNDERS: Since you didn't tell us what the completeness problem was, I guess we couldn't certainly agree with you.

(Slide.)

MR. ROWSOME: We need to do a better job with contracting and contract management, as I believe you have been told before. Getting contracts approved has proved to be a severe bottleneck in our operations. Some contracts have taken of the order of a year to go from the time we initially conceptualized a task to contract approval. We have recently had an example in our fire risk study in which a sole-source contract had been in contract approvals pipelines for six months or more and then bounced, rejected. We had to go back and start over. We have to go back and go competitive or rebuild the justification for sole-source, which seemed overwhelmingly convincing to us, in the sense that it was a study that required of the contractor large amounts of data on fires, and we could only identify one contractor who had that data. It seemed to us to be a pretty compelling case for sole-source, but it didn't fly. So, that research program has been delayed, or will be delayed, the order of a year.

I have commissioned a study to critical path the contracting process and to identify what is wrong with it and what is wrong with our participation in it, what we can do to accelerate this process. I think that is important to our productivity.

We have got, as I mentioned before, to severely limit the number of tasks PAS takes on, to combine related tasks as much as possible. We have got to emphasize the production of usable output.

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DR. OKRENT: I am not sure what the implication is of your item 4: severely limit the number of tasks PAS takes on. You earlier identified a large number of areas, and let's say, even if you divided them into three major areas, you said that you couldn't handle one of those fully.

MR. ROWSOME: We could be gainfully occupied effectively, without anybody twiddling his thumbs, on any one of those. But clearly, a very serious effort has to be made to prioritize what we do.

DR. OKRENT: That didn't even include the ACRS list, so there is probably a fourth category.

(Laughter.)

MR. ROWSOME: When I get to the technical content of the prioritization process, I will work from the ACRS list as my basis to talk to this issue.

DR. OKRENT: What I am getting at is if you severely limit the number of tasks PAS takes on, does that mean, one, the other tasks are not important to the public health and safety, so it is okay; or, two, they are important, but they are not going to be done; or, three, they are important, and somehow somebody else is going to be doing them on a reasonable time scale?

MR. ROWSOME: What I hope to do is develop a realistic and convincing case of how much we can do, and, to the extent that there are things that are important to the health and safety of the public that are not — that cannot be accommodated in that, use that as a basis to argue for reorganization — more funds, reallocation of responsibilities to other groups — to try to solve that problem.

Certainly, I don't want to take a concern that we would all agree is important to the health and safety of the public and say, "Well, we can't get around to that for two or three years." That is not an acceptable answer. You know that; I know that.

DR. OKRENT: So, you are going to tell us what the answer to that is later in this presentation, or that you are going to look at it?

MR. ROWSOME: I am only going to tell you about the process by which we are going to get the because I don't have the answers yet.

DR. OKRENT: Last year, if I recall correctly, PAS said, "We have got about as much money as we can use." So, times have changed.

MR. ROWSOME: I don't think I said that.

DR. OKRENT: But I think I am not misquoting the basic sense: We don't know how to spend more money; there aren't the people who could do it. That was one of the answers.

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MR. ROWSOME: I think there are two senses in which that answer would make sense. One is that with our present staff manpower, we can't manage a great deal more contracts than we are slated to get in the next year or two. Another sense is that the national laboratories, with whom we have been contracting most of this work, are approaching saturation themselves.

On the other hand, we are being forced to go competitive bidding to make much more use of private industry, and there are resources out there that have not been tapped. There are consulting companies; there are people out there, as you well know — I think you made this point yourself, Dr. Okrent — who are good risk assessment people, who are good reliability people, who have not been put to work on this kind of task.

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There are solutions, in principle, to this problem.

(Slide.)

In the course of doing our priority work, we have been attempting to draft a list of the activities that we are doing or have committed to do, or people have suggested that we do. The list turns out to be four or five pages, single-spaced. Clearly, we have got to coalesce things. Clearly, we have got to rank them by risk. Clearly, we have got to organize this effort.

But it has been a tradition, I think, in PAS, to have a large number of disparate research topics and the flood of fire drills and competing requirements on time has meant that very few of these have been carried to completion. Well, we have been coming up short on the bottom line, coming up short on publications, on research results, on getting the word of what we have learned out to you, to the line offices, to the industry.

And we are going to have to pay more attention to completing these exercise, getting our results out in accessible, usable, scrutable form, if you will.

DR. OKRENT: I would like to raise a couple of questions in this area, if I can.

Does PAS feel that before it can make information available in a NUREG or something like this that it must have given it not only what I would call a technical review but a public relations possible kind of review. And this flavor, in other words, to see that you haven't raise undue alarm and so forth and so on. And as a result, is there some substantial delay in when such information would appear as a NUREG?

MR. ROWSOME: Historically there has been a little of that. I am certainly going to resist it. I don't believe that that is appropriate to our role. There are many other reasons, though, that studies like the methodology applications program have been delayed besides that kind of concern for the public relations aspects of the results.

I don't think that is a dominant contributor.

DR. OKRENT: Can you tell me why then, for example, the results from the study of the ice condenser type plant, the B&W type plant — I guess it is a Mark III containment — that one isn't done?

MR. ROWSOME: The others haven't been done — well, they haven't been reviewed. They need rethinking. The event trees contain some errors and several of them would have ben out by now but for the TMI thing.

DR. EDISON: I can address this a little bit. We had problems in the NRC contract in trying to get funding out on a sole source contract on this program and it delayed

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it for two or three months. And then, the occurrence of Three Mile Island diverted our contractors again and the staff, PAS staff, for another good three months, which is a six-month delay right there.

DR. OKRENT: Now I have a harder question.

DR. EDISON: Among other reasons.

DR. OKRENT: What are your criteria for deciding that when preliminary information is developed, either within your own staff or a contractor, for deciding whether or not to advise the ACRS of this information? Not necessarily by NUREG, to advise the ACRS?

MR. ROWSOME: That's a good question. I don't think we have criteria. Perhaps we should think about criterion. Maybe those of you with longer corporate memory in this organization can address that better than I can.

DR. EDISON: We don't have any criteria. In the past, when we have recognized a sequence or something that looked in a particular plant that it should be discussed from a safety standpoint, we have gone directly to NRR to ask them if they want to deal with this, to review it, please.

Whether we should bring it to the attention of the licensing board or whatever, that has happened in a couple of instances.

DR. OKRENT: Do you document this when you do it?

DR. EDISON: The last couple of cases are documented, yes.

DR. OKRENT: I am trying to remember when the ACRS was advised of some question of potential safety significance that came from the PAS group. I would have thought there would be many, frankly, just from the very kinds of things that you were talking about at the beginning of your talk, about how the auxiliary feedwater systems and other systems — there were not clear guidelines, if there were any guidelines. And they grew up in various ways and so forth.

And there are other areas in which you have looked where I would have expected to hear and I don't recall us having heard. And I think there is a deficiency, frankly.

MR. ROWSOME: I think you are right, and I think we will attempt to address that and make it a policy to keep you informed.

DR. OKRENT: I don't think you would find it a happy circumstance to be in the position that B&W now is with regard to those memoranda that were written by some of the ACRS engineers. But that is only one reason. I am really more interested in — other appropriate groups having this information at an early stage and letting them judge whether there is an important safety matter that needs, let's say, early action as distinct from, Let's find out

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more whether this is real, or whatever, is the point of view.

It is my feeling that the research group should not act as a filter.

DR. EDISON: I fully agree.

MR. ROWSOME: As do I.

DR. OKRENT: That is a general comment for all of the research programs. I am just talking to your group now, but as far as I am concerned it applies across the board.

MR. ROWSOME: One last item on improvements in PAS productivity that is rather obvious. To develop collaborative efforts with other research divisions and with the line offices. This is being done and we have set up a number of coordinating task forces within research and some which span groups other than research. We are working in collaboration with, say, NMSS on the waste repository risk assessments and modeling efforts. Mike will talk about that later.

This has grown quite rapidly since Three Mile Island and I think it may become a viable way for us to delegate some of the studies which have historically been done in PAS, but which can and should be taken on more and more by other groups, line offices and the other divisions of research.

(Slide.)

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Improvements in contractor productivity. Most of these things I have mentioned in passing along the way. I will be brief.

We are being forced to go to competitive bidding much more than we have done in the past. It is proving to be a bottleneck in the contracting process but it will have the subsidiary advantage of opening up another sphere of contractors from which we can — with which we can do some of our research study.

I mentioned I have a study going on the critical path for contract commitment. Another one that I have started is a study of the critical factors in contract management, improved task descriptions, schedule, review and output specification. In the past we have had a tendency, I think, to err on the side of leaving these things to the discretion of the government laboratories, to whom we have been giving most of our research contracts. Being insufficiently precise. And to develop a training program and guidelines for our own contract managers in PAS.

(Slide.)

Some examples of the large role of RSR and SAFER in risk-related work is here. Coordinated human factors research involving things from improved reactor safety, improved in-plant accident response, control room designs, simulators, disturbance analysis system, human error or prediction in modeling and so forth, coordinated code development and experimental programs in transient and small LOCA accidents.

We are setting up a three-way collaboration with -- joining PAS, the code development people and the experimentalists to bring our risk perspective to bear on their choice of experiments to run and the priorities for code development. Coordinated research on fuel damage and core melt phenomenology and coordinated research on waste isolation.

(Slide.)

I think we can do a lot more than we have done in the area of delegating studies that are reliability or risk-related to the licensees and the applicants. It is, of course, a line office authority to make such requests of the licensees.

We have on occasion been asked to review or provide guidance and collaboration either specifying such studies or evaluating the results. Examples, of couse, the auxiliary feedwater system reliability study in the spring, failure modes and effects analysis of the B&W integrated control system that has been requested by Denny Ross. Small LOCA transient and inadequate core cooling analyses called for by the Lessons Learned Task Force, BWR, ECCS actuation and control studies that I mentioned before.

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There is one other example -- well, there are some other applications where this might be appropriate. Aspects of the station blackout, susceptibility analysis, failure modes and effects analysis or perhaps fault hazards analysis for control and instrumentation. Auxiliary systems such as instrument errors, service water and D/C power.

In your letter on improved reactor safety, you suggested looking at common mode failures originating from a loss of service air, instrument air, that I assume will be a very design-specific study, that the answer will vary substantially from plant to plant and I think that this might be something we can package and ask the licensees to do.

I see advantages in asking licensees to do such studies above and beyond taking the burden off of us to do the work. I see advantages in helping to get the licensees to think systems, to think reliability, to get them over the impedance barriers, the institutional barriers that have discouraged them in the past from taking up this approach to protecting their own investment in doing their share of the work to assure safety. I think it would be valuable as a technical move and as a pedagogical move to do some careful thinking about how we might specify more reliability-related studies to be done by licensees or applicants.

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Some types of studies, many of which do no have the disadvantage of opening the door to lying statistics are failure modes and effects analyses. It has bothered me for some time that no one has ever really thoroughly looked at failure modes and effects analyses in nuclear plant control systems. It is traditionally done only to the extent of verifying the single failure criteria. It is sometimes done by NSSS vendors on the equipment in their scope of supply. Once in a great while it has been done by architect-engineers on the equipment within their scope of responsibility.

But I have never yet heard of an instance in which these have been integrated into one comprehensive study the way a fault hazards analysis might do. There are computer codes that can simulate logic circuits, that were invented principally to debug microcomputers, desk calculators, electronic watches, that kind of circuitry. They are easy to program. They allow you to model extraordinarily complex systems in a binary on-off approximation. They don't deal with probabilities. They don't deal with failure modes. But they simply simulate how a system is designed to function.

They could be used to extrapolate a failure modes and local effects analysis into a more global effects analysis that looks at the myriad combination of permutations of valve alignments and switch settings. Such studies can be done; they are not difficult. They have not been done. I think they ought to be done.

Those are the principal messages.

(Slide.)

DR. OKRENT: Did you have in mind some procedure whereby licensees or applicants would do this kind of work, for example at the beginning you were talking about this integrated reliability evaluation program. Have you considered whether licensees should be doing that for their plants?

MR. ROWSOME: I think we would probably like to solicit the collaboration and review and perhaps the replication of much of that work by groups like EPRI, to bring them in and get them involved. I don't think I would delegate the lead responsibility to generate the fault trees or the event trees. But that is something we are thinking about. I am a little hesitant about that.

I think it would be hard to standardize. I think it would be specify it well enough that even if we could be assured that they would do good work and that the review process would produce satisfactory results, I think it would be very difficult to standardize the style, the names, the designators, the format in such a way that we would get, at the end, a useful tool that we could use in an expandable pBWH fashion to ask a wide variety of questions across the population of plants.

I think standardization would be the number one concern that I would have if we had 70 separate event tree analyses done by or through the utilities.

DR. OKRENT: Last week we had a short report on the sytematic evaluation program which is a program that the staff initiated trying to do it in-house, asking only those questions of the licensee that it had to to get more information. At least until it was pretty far along. It was for 10 plants and it was originally estimated it might be two years or so. The estimate now is five years minimum.

There is some time scale for these events, like a study of this sort, which makes them more academic than you would like them to be. What you have described in your integrated reliability evaluation program certainly seems in principle like something very worthwhile doing, to me, although I am not sure I would necessarily go at it the same way you have outlined it. Or I don't even know whether it would incorporate the things I have in mind, but nevertheless I certainly think the idea is a good one.

I am not so sure in my own mind that from the point of view of protecting the public health and safety it is likely to move along nearly as fast as I might like it

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to. I am not able to say put all of your resource. on this, for the reasons you have already indicated, so —

MR. ROWSOME: We are struggling with that same question. And all I can do is tell you that we share that concern and that we are struggling for answers and we will look at ways of expediting this through gaining as much collaboration from the utilities as we can and alternative strategies getting there from here with less severe bite on their other resources. And getting there from here in a timely fashion.

I don't see easy answers.

DR. OKRENT: I can think of an "easy answer." Suppose the Kemeny Commission said, We think each licensee should come up with a risk profile of his reactor in 18 months, doing the necessary fault tree and event tree and other kind of analyses and quantifying, in a preliminary way, the most probable or more likely — the important events and so forth. And this was said as something that should be done. And if it were endorsed by the appropriate governmental authorities, I suspect the industry would find the resources.

MR. ROWSOME: They might very well find the resources. It would leave us with a horrendous burden of review which I suspect would fall on PAS. And we would not, then, have the standardized reliability models into which we could plug the new data we are getting, the new models of common cause failure. We would not have, in one computer system, this set of fault trees, this set of event trees. We wouldn't have the foundation.

I would welcome a commitment like that to ask the applicants, the licensees, to do that. I think it would be constructive in the long run although it would help add one more coffin nail to the prospects of resurgence in the nuclear industry, because it would be very difficult -- it would make it more difficult for a utility executive to identify a viable path to an operating license and to be able to predict his budget.

I would like our movement into the direction of probabilistic safety analysis not to unnecessarily close the doors in ways that are not productive to health and safety of the public.

DR. OKRENT: It is not clear to me why knowing more about your reactor should be adverse to the public health and safety. And it is not clear to me that every reactor has to be backfit to current standards and so forth. I do have the feeling if you looked and identified something that looks like an important effect, you ought to go ahead and do something about it.

MR. ROWSOME: I welcome the idea and I think it has more merit than this vantage. I don't see it, though,

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as satisfying all of the needs I perceive that the NRC has for this foundation.

I don't mean to give you the impression that I am negative on the idea of having the licensees do such studies. I have reservations, but I think on the whole it would be desirable that they do this. I am just a little reluctant about using the regulatory authority of the NRC to say that Thou shalt do it.

DR. OKRENT: I guess, that is probably another forum or —

MR. ROWSOME: Let's either go to lunch now or turn to the budgetary material that is more nearly related to your report to Congress.

DR. VESELY: Can I make an interjection?

I am not sure what NRC would do with all of these analyses when we got them, not having the criteria, for example. NRC in the past has tried to do some fault trees, has tried to have the utilities do these analyses and you end up with, So what? So I get these results and so one appears higher than the other, or some probabilities are different than others. What — I don't understand. I don't see — first I see massive confusion. There are so many analyses coming in and NRC not having the capability within PAS to review all of this, I don't see any definite criteria for action being established.

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Having gotten these analyses and reviews, who is going to look at them and decide what is acceptable or not acceptable? We are still trying to tackle that problem.

DR. LEWIS: I can see a vision in which some kind of performance criteria is used which is different than Frank started out with, and then this is probably a necessary first step. If you look at it from that instead of in terms of how you would do and how you would analyze --

DR. VESELY: I see that down longer than 18 months. The time period I suggested is getting this done in 18 months or two years. I don't see having that performance criteria established.

DR. PLESSET: Isn't some of this done now in designing training simulators? If you had a better way of looking at these things you could get a better simulator and train operators better.

DR. VESELY: I think that is being done. We are trying to program sequences, scenarios.

DR. PLESSET: I thought Dr. Okrent was hinting at the possibility of making it much more extensive.

DR. VESELY: But that is a big jump, to go into reliability of quantitative risk assessment. That is a very big jump.

DR. PLESSET: It is. That's true.

DR. OKRENT: Let's see. There is a

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much-celebrated short study on auxiliary feedwater systems.

And in fact out of this the short term Lessons Learned Task Force arrived at some recommendations and I guess there are going to be some plant-specific recommendations. Somebody developed some kind of criteria. Presumably, there were some things that were spelled — should be modified.

I would like to suggest that the problem can be divided into areas where, yes, there is pretty clearly something that should be done; no, there is an area where it is probably okay; and then there is some area in between we are going to have to learn more and think about it longer and so forth.

It would seem to be a mistake not to know about the areas where you really should be changing it because nobody looked, or somebody didn't want to look because he might find something of this sort, or whatever.

DR. VESELY: As you recommended, we are planning to do more of that in-house. That study took approximately one week in-house.

My problem with heavy industry do it is to review it. We essentially have to redo it, anyhow. There is no easy way of reviewing a fault tree or an event tree without doing it yourself, anyhow.

I think we can put the priorities and the time scale in-house, identifying the critical areas and critical systems and doing that in a much more orderly fashion than getting 70 analyses in and trying to review them, which is essentially redoing them.

I don't think you can analyze or check a fault tree or event tree without essentially redoing it.

DR. SAUNDERS: The advantage is that the contractor himself might learn something.

DR. VESELY: I think the process is fine. I think that is a valuable part of it, is going through the process. I don't think coming up with a number or a logic model is — I think that is secondary.

DR. SAUNDERS: I think that is right.

DR. VESELY: I think to load down NRC with 70 logic models or numbers to review would actually hinder health and safety and would stop us from doing some of the more meaningful analyses and reviews than trying to review 70 fault trees or 70 event trees performed by contractors.

DR. SAUNDERS: Reviewing can be more work than doing it yourself from start.

DR. VESELY: But our time scale is not the 18 months that was suggested. That is the problem that we are talking about.

One of the approaches is to try to identify those plants which are riskier than others, if you will, whether from population or older plants and do those first. I think that is also a logical — but you have got to watch the manpower and capabilities.

I think that is your limiting factor, is the manpower capability with the contractors and with NRC. I think you have to go to your time scale and pick up the risky items, do those first based on available manpower. We have found in the past that it is manpower and skills which are limiting.

DR. OKRENT: I am going to suggest that we accept the recommendation that we go to lunch now. And we will do that with the understanding that we haven't agreed on what we were just talking about.

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(Whereupon, at 12:35 p.m., the hearing recessed, to reconvene at 1:45 p.m. of the same day.)

AFTERNOON SESSION

(1:45 p.m.)

DR. OKRENT: If we could reconvene --

I propose that we start this afternoon with the discussion of the details of the Rancho Seco transient and finish that part up and then come back to the priorities and budget and so forth.

So Dr. Edison is up again.

DR. EDISON: This is the chronology of events which starts here.

DR. OKRENT: Would you present it or summarize it for the benefit of the subcommittee?

DR. EDISON: The event?

DR. OKRENT: Yes. The reason why I think it is important to do this is I don't want to go myself completely from memory.

My memory is that when you start looking at the specific failures that occurred rather than the broad block diagram kind of thing on your event tree, it somewhat introduces a little different approach in one's mind as to what is it one is going to try to calculate and so forth and so on.

I don't think there is a single approach that one might take to, at least from the initial point of view, trying to say what the probability of this event is.

DR. EDISON: The event occurred on March 20th, 1978 at the Rancho Seco plant with the plant operating at 72 percent power and occurred when an operator removed the front panel over the lighted button in the control room to replace the burned out light that was behind the button which was no longer lighted.

And in doing so, he dropped the light bulb into the open area and caused a short, an electrical short, which is supposed to be isolated from the rest of the DC system with a fuse. And there is a half-second delay time on the circuit breaker to allow that fuse to operate and isolate the system.

That didn't happen. The system did not isolate and the circuit breakers did, in fact, open, two circuit breakers, parallel, redundant circuit breakers, which fed the 24-volt DC supply —

DR. LEWIS: Is it known what happened to the fuse? They often fail open, but not often closed.

DR. EDISON: I don't know what happened to the fuse. They did recommend that there be a study performed to see if they could change that time delay or even use lower amp fuses.

They had a 5-amp fuse, I believe, and the question was could you use 1- or 2-amp fuses without continuing blowing fuses?

I don't know how the result of that came out.

DR. LEWIS: It may have been an oversized fuse.

DR. EDISON: It might have been oversized, but it was part of the design.

DR. LEWIS: I mean overdesign.

DR. EDISON: That's right. The second thing that I recall from the inspection reports was that the system had a slight design flaw in it, the circuit in the circuit tree such that both circuit breakers normally fail open when this happens and cause both the DC trains to fail.

And they fixed that and it was, I believe, a site specific problem to that one plant.

This failure gave a signal to the integrated control system. I guess it gave it a negative temperature reading such that it rolled back the main feedwater system, and the pumps drove down to zero, which was an effective loss of main feedwater.

At the same time, it cut off the signals to the auxiliary feedwater system. That is, the level signal from the steam generator is normally what is used to actuate the auxiliary feedwater system.

We are talking about a once-through steam generator with a relatively short dry-out time compared to some steam generators.

So when the level comes down, there is a signal given

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to the auxiliary feedwater system to actuate, and it did not get that signal.

What happened was the steam generator's instruments began to drift. Steam generator A drifted down and B upward. Steam generator B drifted all the way full-scale. Steam generator A drifted to zero.

The auxiliary feedwater did not come down until that drift signal brought it on. They performed a test weeks afterwards by drying out the steam generator, again shutting off the signals. And it behaved in the same way. The drift brought it down in 7 or 8 minutes and gave a signal to the auxiliary feedwater system to actuate.

That is not a very desirable way to actuate auxiliary feedwater systems.

At the same time, I can't call that a failure of the auxiliary feedwater system to perform its function. I can call that something else — failed, but it was not a failure of the auxiliary feedwater system to provide its system. It did provide water after 7 or 8 minutes.

DR. OKRENT: There was a delay in it. So there was a failure there.

DR. EDISON: There was a degradation. It was an absolute failure to perform precisely as designed.

DR. PLESSET: Is the signal to the auxiliary feedwater system still that way, actuation from the level in the steam generator?

DR. EDISON: I can't tell you. I don't know.

DR. PLESSET: It would have that leg now, wouldn't it?

DR. EDISON: That's right.

DR. MARK: Was the result of the failure in the electrical system?

DR. EDISON: Yes, the electrical system of the integrated control system.

DR. LEWIS: If the electrical system were working, there would be no lag.

DR. MARK: There was only one fault that put out the electrical system. It's a miracle that the auxiliary feedwater acted on anything.

DR. LEWIS: Under normal operating conditions with the electrical system intact, the delay time between the steam generator level and the turn-on of the auxiliary feedwater system is what?

DR. EDISON: Less than a minute. On the order of 30 seconds.

DR. PLESSET: It was just because of the malfunction of the control board.

DR. EDISON: That's right. Normally, when you shut off the main feedwater, the steam generator level starts down, and those will dry out in a short time, in as short a

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time as a minute and when the level gets down to that level, down near zero, I think it is a 15-inch level reading, something like that, a low, low level signal is sent to the auxiliary feedwater system to actuate.

So it was actuated and it did come on at the end -- at 7 minutes.

DR. LEWIS: I am still a little fuzzy about why both circuit breakers went. There is a single fuse in series with a pair of circuit breakers in parallel.

DR. EDISON: Essentially, that is correct.

DR. LEWIS: That does seem odd.

DR. EDISON: The system apparently operated as designed. It is operated so that both circuit breakers go when the fuse doesn't work.

DR. LEWIS: I understand. These are parallel systems, two parallel circuit breakers leading into the common load and the common load is isolated with a single fuse.

DR. EDISON: Yes.

DR. LEWIS: That actually makes sense.

DR. EDISON: All right. After the auxiliary feedwater system actuated, about that same time the operator believes that he was able to manually start the main feedwater again.

These are steam-driven pumps, turbine-driven pumps. And he was able to manually get that started, and as a result --

DR. PLESSET: Were they all steam-driven?

DR. EDISON: Two steam-driven pumps.

DR. PLESSET: And one electric, or are there just two?

DR. EDISON: I am talking about main feedwater. There are two large turbine-driven pumps. Loop A and B.

DR. ROWSOME: They have motor-driven condensate pumps and turbine-driven main feedwater pumps.

DR. PLESSET: One for each steam generator.

DR. EDISON: One for each steam generator, one for each loop.

DR. PLESSET: That is a different arrangement than the one at Three Mile Island.

DR. EDISON: It is the same. Yes, they have one large steam-driven feedwater pump for each steam generator. The auxiliary feedwater system is set up differently.

DR. LEWIS: At TMI, we never heard about the main feedwater pumps after time zero.

DR. EDISON: They did not recover. In this case, the operator did recover it. Shortly after they recovered it, they had a safety injection, safety feature. The high pressure injection came on at 1600 psi and then automatic actuation of the feedwater system at that point, the auxiliary feedwater system at that point. And that was really what led to the — at that time, diagnosed as the main concern is when the accident is over. And I call it an accident when the transient was over, they found that they were overcooled. They had cooled the plant down faster than they would have liked.

They attribute that to the automatic actuation of the auxiliary feedwater system.

So one of the things they were going to look into is whether they should automatically actuate auxiliary feedwater with the emergency ECCS signal. And B&W is going to go back and do some calculations and look at whether that is desirable or not.

They have looked at three different accident scenarios where it was desirable, small LOCA, large LOCA in the main steamline break.

And in those cases, it was desirable to actuate the auxiliary feedwater with the high pressure injection.

So they were going to go back and see if there was any serious sequences where they would not want to do that.

I don't know what the results of that was.

Shortly into the event, the pressure went up in a few seconds. From the time the main feedwater was lost. The reactor tripped at five seconds. The pressure relief valve had been leaking and so they had closed it, closed the blocked valve behind it and were not using a pressurizer relief valve. They relied on their two safety valves which are in series with them.

One of those did open. I believe the maximum pressure reached was 24, 25 psi.

DR. LEWIS: You mean in parallel with it?

DR. EDISON: Yes, in parallel with it. Only one opened, it is believed, and it did open below its set point.

I had personally surmised that they might have moved the set point down a little bit to compensate for the pressurizer relief valve being locked closed.

I don't know if that is the fact or not. So that valve opened and relieved the pressure, and then the pressure began to drop. They left the high pressure injection pumps on some three minutes, I believe, after it was actuated and then they turned it off, very similar to TMI in that response.

Eventually, the entire thing was terminated because they diagnosed what the problem was and they went out and closed the circuit breakers again and then they found out their readings on their instruments and realized they had cooled down too fast and they had to do something about it.

But for an hour and 15 minutes, they had very little instrumentation. What little they did have, they didn't know if they could trust because they didn't know what had failed. They relied on two particular measurements.

They relied on reactant coolant pressure and on pressurizer level. And both of these were available through the computer.

They did lose the level instrument, but they did have a reading print-out on the computer, so they followed that on the computer.

On the basis of those two readings, they operated high pressure injection manually, intermittently for an hour and 15 minutes until they got their instruments back.

DR. PLESSET: Did they have a temperature read-out?

DR. EDISON: No.

DR. PLESSET: They did not have temperature read-out?

DR. EDISON: That's right. They might have had some, but they didn't know if they could trust it. It was a very serious —

DR. PLESSET: They could have been in a precarious situation.

DR. EDISON: Absolutely.

DR. PLESSET: Just having pressure.

DR. EDISON: Yes.

DR. LEWIS: Is it possible to summarize what these two circuit breakers serve, only the readouts on the instrument panel? What do they service?

DR. EDISON: The channel selection is the main thing. These two breakers were the ones that opened. Here are the 2400-volt feeds for the switching relays.

There were lengthy inspection reports and meetings and personnel were sent out to the plant to meet with the utility and with the designer and so forth. And they carried on quite an investigation.

I haven't done that. I haven't taken that kind of in-depth calling in the utility and making a real issue out of this, which I could.

I am not sure that the question warrants that.

DR. LEWIS: I am still a little fuzzy -- I'm sorry, I interrupted.

DR. PLESSET: I was just going to ask, what were the two pressure readings? One was from the pressurizer and what was the other one?

DR. LEWIS: Pressurizer level and RCS pressure.

DR. PLESSET: They had a direct reading of the RCS pressure, then?

DR. EDISON: I believe they did.

DR. PLESSET: It is surprising that they don't have a direct reading of RCS temperature. It would be redundant and independent of this source that they lost.

DR. EDISON: If they had it, they didn't realize that they could use it. When they went back and analyzed this, they found out that there was instrumentation that was not affected. If they had only known which it was, then they could have read that and trusted it.

And what they did after this was to write some procedures to tell them which instruments would work in the event of this kind of a failure, so that they would know what they could rely on.

DR. PLESSET: There could have been saturation conditions or worse.

DR. EDISON: It could have been a serious accident. They were flying pretty much blind for an hour and 15 minutes.

DR. PLESSET: Yes.

DR. EDISON: If they had gotten into real steam problems or had a sticking relief valve like Three Mile Island, then you might have had steam problems, level problems, and that sort of thing.

It is a possibility.

So this was a serious event. There is no question about it.

DR. LEWIS: I am still fuzzy about one detail. If what was lost in the two circuit breakers was the non-nuclear instrumentation on the panel, then could you tell me again why the auxiliary feedwater system was slow coming on because that is presumably actuated independently of the reading on the control panel.

Isn't that a direct reading directly to the auxiliary feedwater system?

DR. EDISON: Yes, but it isn't the water level per se; it is the instrument that tells what the water level is. The water level was gone, but the instrument that tells it was drifting.

DR. LEWIS: The sensor is in the steam generator, right?

DR. EDISON: Certainly.

DR. LEWIS: There is a read-out on the instrument panel, whether the wiring is such that the sensor reading goes directly to the auxiliary feedwater system or goes to the panel — I am misunderstanding something, I think. What is it?

DR. EDISON: I can't tell you if they get a signal direct from the steam generator or if it goes through the ICS.

My impression is that there is a signal direct from the steam generator level.

DR. LEWIS: Then why did the aux feedwater system come on late?

DR. OKRENT: The signal relies on DC power availability.

DR. LEWIS: But I had the impression from all of this that the DC power that we are talking about services only the instrument panel, the non-nuclear instrumentation on the panel. That obviously can't be true.

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DR. EDISON: It services some switching relays, and the flow indications, level indications, were shut off.

DR. LEWIS: The indications are not what turn on the auxiliary feedwater system. It is the sensor. And in the end, one can only resolve this by looking at a circuit diagram.

DR. EDISON: I would presume that here is a sensor and it is reading a level of the steam generator. And if you shut off its power and it continues to read at high level, that there will be no signal sent to the auxiliary feedwater.

DR. LEWIS: Well, a sensor is in a completely different place from the readout on the panel. It is not quite clear to me whether shutting off the power to the panel turns off power to the sensor.

That would seem to be -- I wouldn't design something that way.

Anyway, you don't know what's bothering me.

DR. EDISON: No, I am not enough of an electrical engineer to be able to answer some of your questions.

DR. LEWIS: That's all right. I'm not, either.

DR. ROWSOME: I know exactly what is bothering you, but I don't have the answer, and it bothers me, too.

DR. LEWIS: Thank you. I'm glad that somebody understands me.

(Laughter.)

DR. OKRENT: In any event, the reason why I thought it was relevant to have you describe some of the things we have been talking about, and I don't think we need to try here to get them all down, is that, at least to me, the initiating event here was not just the simple loss of feedwater.

There were many things connected with the loss of the DC buses.

And if you were to take the various things that failed because of loss of the DC buses, individually, take the probability of each of these things occurring, I think you would get a very low probability, indeed.

So one could go through that exercise and show that this is nonsense, or something, I suspect.

The question of what is the probability of losing DC buses is maybe something we should address, at least in my own mind.

And also, the chances that the loss of these would cause what it did.

Now how you go from the one to the other, I don't know. I don't know whether all of the B&W plants would do this or all PWRs, or just Ranch Seco, or whatever, had some unique characteristic.

But this, again, would enter somehow into an estimate of probabilities.

In any event, as I said before, I think there is more than one perspective that needs to be considered in trying to respond to the question, what is the probability of the Rancho Seco transient?

And you were putting one on the board which was sort of a different one to me than looking at it from the point of view of the very different initiating event, in fact.

Now maybe we ought to get back to the question that Dr. Lewis was talking to earlier.

DR. LEWIS: I don't think that we can resolve it.

DR. OKRENT: I don't want to try to — earlier on, when we were talking about the Rancho Seco transient, the question that was on the table and which I interrupted until we could have a somewhat more detailed description of it was how does one approach trying to — I suppose you would say estimate the probability of this transient and place it in some kind of perspective using probabilistic methodology?

By the way, I think when Congressman Udall says WASH-1400 methodology, he doesn't mean only use the event trees you have got and not some other kind of probabilistic methodology and so forth.

So I am reluctant myself to say we can't do anything here because somebody else will and they may come up with, for example, all of the failures that occurred and string them together as if they are random, which they are not. And I don't think you would agree if somebody did that.

If you wouldn't agree to that, what would you agree to and how can we generate something meaningful here without, you know, turning all of Sandia and all of their supporters — I don't mean financial supporters; I mean supporters for financial gain.

(Laughter.)

To work on it.

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DR. EDISON: We can do analysis.

DR. OKRENT: You can, c-a-n?

DR. EDISON: Yes, we can do analysis, and it is something we would have to, I think, put on the list of priorities there, Frank, because it is not the kind of thing, I think, that one guy knocks out in two weeks. It is not that simple.

MR. ROWSOME: It might be, it might not. I think we have to answer the most recent question, that is, why did this have such serious impact on what should have been safety-related equipment, which should have had nothing whatsoever to do with the non-nuclear instrumentation. Resolving that question is important.

Then we will have to look at the structure of the fault and the circumstances of the fault and the fault consequences, and develop a succession of progressively narrower event classes in which this belongs, and describe the probabilities of each until we get to the point that it is absurd.

I guess we don't have enough data on what happened to be able to identify what those successive steps are here. But we will sit down and try to do that when we have a better picture of what in fact happened, and get back to you. And I see no alternative to that.

DR. LEWIS: There do exist detailed chronologies for these two events, just as we have for TMI, and I may even have them in the half-cord of paper I have gotten from NRC.

DR. OKRENT: I almost brought a quarter of an inch on the Rancho Seco transient with me.

DR. LEWIS: I probably have them somewhere.

PROF. KERR: Two near-misses.

DR. OKRENT: That might be one of the sets.

PROF. KERR: I don't mean that you almost -- I mean that you almost brought those.

(Laughter.)

DR. OKRENT: Are there other groups besides the NRC in the U.S. that you think -- contractors that might be able to make a contribution in analyzing the Rancho Seco transient, since it is a little different?

DR. EDISON: I am sure there are.

MR. ROWSOME: You might ask NSAC if they have thought about it.

DR. EDISON: It is not a quickie. It takes a week or two just to dig into all of the reports and the sequence and get familiar with the events and the details of it, before you can even really get moving on it.

You may have seen this one before and had special reports on it two years ago. But it was brought cold to the group. They have to find out how the plant works.

DR. PLESSET: It seems to me I have heard a lot of talk about how vital the D.C. trains are, and you have them duplicated. And here, by an event of probability zero, you knock them both out at once. It seems to me that is not a very good design arrangement.

Maybe you have to put a little catch under the light bulbs or put them in separate boxes. I would suggest the latter.

DR. EDISON: They did make a change. When they take a light bulb out now, they put in a little nonconducting plug of some kind. But that is not a very satisfactory arrangement.

DR. PLESSET: Why aren't they in separate cabinets from the beginning? I thought they were supposed to be pretty much independent of each other.

DR. EDISON: I guess they did have another set of instrumentation in the next room in a cabinet, not in the control room. That was available, but not easily accessible to the operator. It was in the cabinet in the next room.

DR. OKRENT: Which the operators didn't know about?

DR. EDISON: Either he didn't know about it or they weren't convenient to him such that he could use them conveniently.

DR. OKRENT: 75 minutes -- well, as I think Mr. Rowsome suggested, analysis of this transient maybe has more interest than just being able to respond to

Congressman Udall's letter. In fact, I think you earlier said there is a need to look at control systems.

MR. ROWSOME: Yes.

DR. OKRENT: This is an example of something that hadn't been thought about enough clearly before: How many things should be tied together this way or whatever? So you are -- your suggestion is that maybe NSAC or EPRI or whatever might be able to respond to this?

MR. ROWSOME: Yes.

DR. OKRENT: And with regard to NRC, could you guess by when you think you would be able to say whether -- you can't put it high enough on a priority list, or yes, you will be able to provide something by mid-December?

MR. ROWSOME: I am sure we can provide something by then. I am sure we can satisfy the response to Congressman Udall's letter by then. We may conclude that, because of systems interactions implications or something like that, the subject deserves a great deal more research. But we will certainly satisfy --

DR. OKRENT: So we would have at least one response to look at, and possibly say, here is a reasonable analysis?

MR. ROWSOME: Yes.

DR. OKRENT: Any other things on this point now? And the Davis-Besse one, I assume, you look upon as a more simple one to provide response to?

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MR. ROWSOME: That's right.

DR. OKRENT: So we don't have to worry about that.

Okay, thank you. I think it was helpful. I am glad you brought it, even if I didn't.

DR. EDISON: I can send you a copy of that?

DR. OKRENT: I have more than one copy, so it is a double failure or something.

(Laughter.)

DR. OKRENT: Next we will get into the discussion of priorities via the budget.

DR. MARK: On the last slide we saw, it listed seven reliability studies. Can you tell us what you mean by "human error susceptibility studies"? Is that to determine whether people with blue eyes are more susceptible than people with brown eyes?

MR. ROWSOME: There is a survey being done, and perhaps Gordon and Bill can fill you in on the details better than I can. But the objective of the study I believe you are referring to is to look at the reactor safety study and assess the sensitivity of the important accident sequences to the human contributions to those events. And it is being done -- I believe this is correct; you-all correct me if I am wrong -- by writing out the -- essentially, the expression for the probability of occurrence of these event sequences at a level of detail that shows the contribution, putting in probabilities

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and varying the probability of the human contributor, and looking at the rate of change of the probability of core melt via that sequence due to the rate of change of the --

DR. MARK: It is purely formal --

MR. ROWSOME: Purely a formal sensitivity study of the human errors --

DR. MARK: It won't unearth the question of whether, if a control panel were arranged differently, human error probabilities would go down?

MR. ROWSOME: No. There are other efforts looking at that, but that is not what I mean by the sensitivities.

DR. SAUNDERS: It says "susceptibility," not "sensitivity."

DR. MARK: It says "susceptibility."

MR. ROWSOME: I may have meant that to be -- yes, it is coming back to me. I meant that to be an umbrella term that would embrace both of these.

DR. SAUNDERS: I see.

(Slide.)

MR. ROWSOME: The latest draft of the decision unit called risk assessment, which we might better call system reliability analysis or some such name, is this -- I have split out a new item, the integrated reliability evaluation program.

The $50,000 there for fiscal '79 is the kickoff of

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that survey of operating plant susceptibility dominant sequences in WASH-1400. The estimates for fiscal '80 and '81 are our projections of what it will cost. They are very tentative preliminary projections of what it would cost to do the event trees and fault trees work on the operating plants.

The other line items are the same ones you are familiar with. The '79 figures reflect as spent out, rather than as planned.

You will notice we didn't use any money to improve WASH-1400. The training programs were done largely in-house and didn't eat into the program support budget. They were present, but not a deficit on the program support budget. The plus and minus signs on the total for fiscal '80, including the Three Mile Island supplement, are areas where we are thinking about shifts in priority indicating a direction, and I will address those when I get to the technical content of the priority issue.

(Slide.)

I want to bring you up to speed on what has happened to the improved reactor safety decision unit. The Commission elected not to endorse the supplement for improved reactor safety in fiscal '80. They did so on the basis of an acknowledgment by us that we were already negotiating with the Department of Energy to follow the OMB request that as much of this as possible be delegated to them, that they should

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be in the business of trying to improve reactor safety and not the NRC position, with which we are not altogether sympathetic, and I gather you-all aren't, either.

We are proposing in the '81 budget to fight this battle with OMB again at the level we have been hoping to start this program with in the last two years.

(Slide.)

DR. OKRENT: Let's assume that Dr. Siess will address that program in some other Subcommittee.

MR. ROWSOME: All right.

(Slide.)

You want me to --

DR. OKRENT: That is with regard to the research to improve reactor safety.

MR. ROWSOME: We will set that aside until that Subcommittee meets.

(Slide.)

I thought I would use your outline of priority issues as a framework to discuss the thought processes we are going through to resolve our priority problems. You were urged in the preamble to these comments on the budget that we needed to reassess priorities and focus. We certainly agree with that. We are going to do it.

We are going to do it, really, from the ground up, trying to avoid presumption and avoid undue weight to simply continue programs.

Study anomalous transients and small LOCAs, yes, we quite agree with that. Then you started to talk about something a little bit more specific. You suggested accident studies --

PROF. KERR: Remind me what an anomalous transient is.

MR. ROWSOME: Since you all invented the term, you may have a better definition than I do.

(Laughter.)

PROF. KERR: I withdraw the question.

(Laughter.)

PROF. KERR: If we invented it, I know what it means.

(Laughter.)

MR. ROWSOME: Accident studies that go beyond the design basis accident to melt, and on from melt in through atmospheric pathways and liquid pathways to public health and safety consequences. We certainly concur that we have to do a massive amount of research in accident scenarios leading beyond design basis accidents. We are addressing that and intend to address that through the integrated reliability evaluation program, through methodology applications program, through our collaboration with RSR in code development and experimental program.

We are, as you know, doing some work to improve the CRAC model for modeling the atmospheric dispersion of releases and public health consequences. We have and are contracting for modifications of that model to accommodate site-specific mesoscale meteorological effects, so that we can do a better job with site-specific analyses. Our Sandia contractors are looking at alternative strategies for evacuation.

The Germans, incidentally, used a rather different model in their risk assessment than we did. Our model had evacuation proceed as soon as possible. Their model assumes sheltering, that what you do in the short run is to ask people to go indoors, and that you evacuate them in the inner radii after the cloud has passed or after an elapsed period. I forgot the number, but I think it was in the order of 15 or 20 hours, whichever is shorter. And that in the outer radii people are evacuated on the basis of measured levels of contamination and projected doses into the future. Quite a different strategy.

Our consequence model -- people tell me that the effect of the two strategies in minimizing committed dose is about equal, that it doesn't seem to make much difference, from the point of view of risk assessment, which strategy you follow. They are roughly equally effective, but that is largely coincidental. They are quite different strategies.

As you know, we have a program at Sandia to

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develop an improved liquid pathways model. That effort has slipped and it may slip some more. The preliminary results indicate that the uncertainty spreads will be so broad that we may not be able to draw a useful conclusion from the answers at this point, although it looks as though it will provide a basis to be discriminating in future research, to use the principle I suggested earlier of directing research to resolve dominant contributors to uncertainties, that we have learned enough to know where we should look from here, but we haven't learned enough to get good useful answers yet.

(Slide.)

DR. OKRENT: Would you say that again, and maybe in different words?

MR. ROWSOME: Our preliminary reading of the progress reports on the liquid pathways indicates very broad uncertainties, too broad to be useful in resolving what I believe to be your interest in determining the value impact of core catchers, for example. It may be useful in assessing comparative site-related risks, how much more hazardous some sites are than others, but only by providing pointers, because the uncertainties in these analyses are so broad that it may be difficult to rank in comparative terms atmospheric with liquid pathways of one site or another.

But we have certainly made progress, in the sense that models have been made of liquid pathways that have not been modeled before, and the systematic treatment of uncertainties will give us clues we did not have access to before about where the key focal points of successive research ought to be aimed.

DR. OKRENT: In a previous meeting, I had been given the impression that the studies you were doing of liquid pathways were site-specific.

MR. ROWSOME: Yes.

DR. OKRENT: And that they would, hopefully, provide information which would provide some basis for judgment on the two areas you mentioned, whether liquid pathways represents an important distinction among sites, and also whether there is a big enough effect for an acceptable site to look to warrant a core catcher or some other means of mitigating it.

Now, if I understand what you said, in contrast to what was in WASH-1400, where the problem was sort of dismissed as not very important for the land-based sites, all land-based sites or whatever, you say that the uncertainties are so big that there is really little basis for making any judgment.

MR. ROWSOME: Gordon, correct me if I am wrong, but the model assumes there is no interdiction. There is still the fact that the liquid pathways have a fairly long characteristic time. And so I wouldn't want to leave you with the impression that I perceive or that we perceive that the liquid pathways could rival atmospheric pathways as a hazard to the public health and safety. I don't think they do.

Nevertheless, the importance of interdiction in some form is, at least in my mind, still an open issue.

Can you all fill us in a little bit better on what that program is coming up with?

DR. EDISON: There is a part of the program to look at interdiction, to see what might be done to make recommendations. But there is no model. There is no quantitative reduction factor or anything like that put into the estimates based on interdiction. They have just now been completing the first draft of the food model, and we started to get some results out. And it is like all first draft computer programs. The early results come out and hit you in the face and don't look sensible at all. And they are going back and looking at numbers.

So it is really not clear what the results are going to be, the numerical results that come out of the analysis. We should know within a month or two.

DR. OKRENT: My impression is that they are really not looking at specific sites. They have some kind of synthetic sites.

DR. EDISON: They have -- they are looking at river pathways, they are looking at large body of water, lake pathways, estuaries. They have -- they are almost site-specific, but they are not exactly site-specific. They have

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looked at some sites that are very close to the characteristics of some existing reactors. And most of the reactors are sited in the East, as it happens, at the moment. And so the sites tend to favor that predominance of that portion of plants that are located in the East on rivers, as opposed to large bodies of water. So that they are doing more work on river pathways plant sites than, say, on a lake plant.

But they are looking at a Great Lake-based plant, and they are not looking at 70 individual sites.

We could give you more details on this program at another time, if we could bring in the project manager.

PROF. KERR: What I am hearing seems to indicate that what is being done is primarily writing of computer programs to arrive at some physical model.

DR. EDISON: Right.

PROF. KERR: And the data that you need to get accurate results exists?

DR. EDISON: The best data that exist appear to be from the TVA plants. I don't know if it is Watts Bar, one of the plants that has more data available than other plants.

PROF. KERR: The uncertainties that you see are not uncertainties in the physical data, but uncertainties in your ability to computerize the physical model?

DR. EDISON: Yes. The dispersion characteristics in the food-way model itself, how the dose is passed between

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fish.

PROF. KERR: How will you finally decide how large your uncertainties are? Do you have a way of judging that?

DR. EDISON: I don't at this time. I can't tell you at this time.

PROF. KERR: How far are you going to go with this before you try to decide whether the results will have any usefulness on the basis of uncertainties?

DR. EDISON: I have to say that I agree with Frank that the uncertainties are significant, that if you simply based your conclusions on some total stack of uncertainties you would say, I can't believe anything. I don't think that is the case in this program, that you can't believe anything, because I think that there are -- there are some uncertainties in your modeling assumptions, but as long as you caveat those and know what they are, at least you will have a smaller uncertainty in what you have calculated.

PROF. KERR: Is there some point at which you decide this is worth carrying further or there is no point in spending any more money because we aren't going to get any useful results? How do you make that decision?

DR. EDISON: Yes. We are going to make that decision within a very short time, on the order of a couple of months. They are putting together an interim report which we are now calling a final report, and we are going to look

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at that and make a decision as to whether we should do anything further and go in another direction or not, and see what we have.

Right now we have been putting together the write-up on the models and the assumptions and so forth, with no results yet. All of the work has been done to get this model together, and just now we are starting to get some numbers out that have to be clarified.

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MR. ROWSOME: It will produce a substantial body of information with which to tackle the decision process of where we go from here. The contributors to the uncertainties have to do with things like the surface area of the melt to the groundwater, how finally does this glassy glob fragment down there. That is a big contributor to the uncertainty.

Another big contributor is the parameters that characterize the dispersion of fission products through the ground and to water bodies. Another contributor has to do with the biological cycle: what organisms take up what isotopes.

PROF. KERR: I can believe that it is complicated.

MR. ROWSOME: We may find that to reduce the overall uncertainties, we may have to know more about the melt or we may have to know more about the biological cycles. The biological thing may be the weak link. So, we can focus -- we can first of all assess how much more information we need to know to make policy decisions about things like core fractures; we can estimate how long it would take to get there.

PROF. KERR: What I am trying to find out is how, when you get the computer program written, running, and you start getting numbers out of it -- I assume this is what you

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to be --

PROF. KERR: Any model that hasn't been built before is an innovative model.

MR. ROWSOME: We are not tracking on the same thing. We are not communicating.

PROF. KERR: I guess we aren't.

DR. OKRENT: Let me put the question in a different perspective. For some time, I have had the sensation that the country seems to be a spending a lot of time trying to estimate the risks from disposal of high-level wastes when you put them away where you want them in what you want them, et cetera.

MR. ROWSOME: Yes.

DR. OKRENT: This event, if we were to have a reactor core melt into the ground, would be a much less controlled condition. It has not been obvious to me that if I were to do an estimate of the risk, whatever it is, from controlled high-level waste storage and core melt via liquid pathways from 100 or 200 reactors, it is not apparent to me that the liquid pathways effects from the reactor wouldn't be much larger.

MR. ROWSOME: One thing we are learning is that they are subtle. The isotopes get taken up into aquatic microorganisms that can pop up in obscure places in the human food chain. Ice cream has within it kelp, and that

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turns out to be one of the dominant pathways, we now think. We are discovering useful things from this study. It will not be the comprehensive study that will put this issue to bed forever. We know that's true. How good the study is, we don't know. I don't believe we ourselves are capable of assessing how good the biological models are or how complete they are, how accurate they are, how good the parameterization of the quantitative portions are.

We are going to have to work on this subject some more. There is no doubt in my mind about that.

DR. PLESSET: The kelp is for floating nuclear plants?

MR. ROWSOME: No, for land-based plants that are on rivers that communicate with tidewaters.

DR. PLESSET: On river banks.

MR. ROWSOME: Yes.

DR. OKRENT: What isn't clear is how regulatory decisions are going to be made and what benefit they are going to get from the research program you have, and when.

Actually, as you well know, decisions are made by no decision, as well. And I am sure there are lots of uncertainties in this overall thing. But what I am not sure of is how one gets enough of a focused approach to provide what one thinks is meaningful input into the decision process.

BWH MR. ROWSOME: Well, I am not sure I can answer your question, except to say the way I envision this evolving is that we will look at the uncertainty spreads, we will look at what can be inferred, we will perhaps come to the conclusion that certain classes of sites are all right as is -- maybe we can make that judgment or perhaps there will be another class of sites in which we say this could be a real problem warranting research and we ought to be focusing on this aspect or that aspect or another aspect.

We will go through a kind of decision theoretic response to the new input we now have at our disposal, evaluating that and making decisions on the basis of new knowledge.

DR. VESELY: I would like to interject. The liquid pathways, there is no statistical analysis of uncertainties being done on that program at the present time. Many uncertainties are being estimated by the people, sort of subjectively. That program, as it is scoped, has no uncertainty evaluations. We can incorporate that, or consider that, the question of how do you assess the uncertainties, whether they are large enough or unacceptable. Uncertainties aren't being calculated.

PROF. KERR: My question wasn't meant to elicit a very sophisticated answer, necessarily. I just was trying to find out how, when you get through, you decide the program is telling you something that might be useful or it is garbage. I wouldn't know how to do it with something this complicated, and you have given a lot more thought than I have, and I was curious.

MR. ROWSOME: Our conclusion is that we have to bring in a wide variety of specialists to make that evaluation, because it is such a multidisciplinary thing.

PROF. KERR: I would think that before you presented a program to a variety of specialists, you would want to have some kind of a feel yourself. If you are convinced it is garbage, then it seems to me there is no point in turning it over to a group of specialists and saying, "Do you think it is garbage, too," with the hope that they can find some merit in it that somehow has escaped you. You first have to decide yourself, don't you?

DR. VESELY: I don't think the staff has addressed that question, whether it is garbage or not.

DR. EDISON: We are not going to get a final number out of this program.

PROF. KERR: Don't you almost have to, before you get down to the final details, ask yourself, "Has this mass of information that we have collected, and at least our preliminary considerations, convinced us that we can eventually get something out of this that is worth spending a lot of effort and money?" And you have to make that

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judgment; don't you?

MR. ROWSOME: Yes.

PROF. KERR: It seems to me it is irresponsible not to.

MR. ROWSOME: But having chartered a research program, having funded this work, having brought it to two-thirds, three-quarters of the way to completion, it is part of research policy not to censor the work of our contractors, so that this piece of research will be published regardless of whether we think it is worthless or not.

The question is: Where do we go from there? What inferences do we draw from it? What credibility to assign from it? And that is a set of questions in my mind that can't be answered until we have probed its robustness, which we will attempt to do by trying to put together a group .. sufficiently diverse talents to say whether it is good or bad or its uncertainties need more quantification or where to go from here.

This is exploring at the outer limits of what we know, what we know how to model, what we know how to estimate. And any advance in the state-of-the-art, I can't predict where it is going to go from h're. I think the justification in taking it this far is clear. We ought to be worrying about these, as Dr. Okrent has suggested, BWH because it is not a priori obvious that the liquid pathways are negligible.

(Slide.)

RSR, with some coordination and collaboration with PAS, is doing a number of studies related to molten core phenomenology. Sandia has done some calculations of the feasibility of in-vessel melt retention. We do not now have planned a value impact analysis of core catchers. We will assess that when we know a little bit more about the results of the liquid pathway study.

Item E, recommended power burst —

DR. OKRENT: Before you leave D, the question applies, really, back to Item C. You have shown some things which sort of are ACRS comments, and alongside of each you have listed some things that relate to it. But these things that you listed mostly were there before the ACRS made its comments.

And what isn't clear to me is whether you are proposing some important change in your previously planned research program to respond to the ACRS comments. So maybe we could come back to Item C after you tell me on Item D whether in fact there is something that you are going to do that was different from what we heard before we wrote the July 1979 NUREG-0603.

MR. ROWSOME: Item B, we haven't gotten to the

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point of making a decision. We have definitely decided to pay rather more attention to the space between design basis accidents and core melt. The accident scenario, where it is suggested by the integrated reliability program and the effort to develop event trees for core damage scenarios that may stop short of melt.

In this context, we have not really — I guess the best way to say it is simply admit we haven't made up our mind whether this needs more attention or perhaps less attention on the basis of the priority review. We are at this stage really just collecting —

PROF. KERR: Item D?

MR. ROWSOME: Yes, Item D. We are simply — I am trying to get in my own mind with my people a clearer picture of the scope of studies that have been suggested or are ongoing. The ways in which we can get better productivity by changes in our way of doing business or by coalescing studies into families of interrelated efforts to look at the budget, to look at the resources available to us, and contractors to look at technical merits and political expediency, usefulness in the licensing process, and then make the hard decisions.

I appreciate the feeling communicated in your letter on the budget, that molten core phenomenology and the disposition of molten cores is something important that

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needs to be looked at. But whether that will surface near enough at the top of our priority list to warrant more attention than it has gotten in the past, I can't answer now, because there are too many terms in that equation and I don't want to do a slap-dash job of this priority review.

DR. OKRENT: How about Item C on the previous viewgraph?

MR. ROWSOME: Item C is certainly getting -- Item C-1 is certainly getting more attention than we had planned to do before.

DR. OKRENT: Can you tell me specifically where there would be more?

MR. ROWSOME: The collaboration with RSR in prioritizing code development and experimental programs is new. The integrated reliability evaluation program is new. The effort to develop event sequences for core damage scenarios is new.

DR. OKRENT: All right. I would say that we were told about the plants to look at new scenarios and so forth at the time we wrote 0603, but not the -- whatever you call it -- the integrated reliability evaluation program, which we tended to recommend toward. Okay. Thank you. I wanted to know the areas.

MR. ROWSOME: C-2, atmostpheric pathways. At the moment, we have not planned or scheduled anything we had not previously had going. But we certainly intend to look at that again. The same thing is true for the liquid pathways research. As I indicated, it is becoming clearer to us that we are going to have to look at many paths through the decisional tree of what to do with the study we have now, approaching a report status.

DR. OKRENT: I would like to make a comment. It is always conceivable to me that a program is generally important, whereas a specific thing being done is not being well done and vice versa. You may have a program that is not very important and the work is terrific. And what we are looking for is the right combination of those parameters.

(Laughter.)

MR. ROWSOME: Right.

(Slide.)

E is not in our bailiwick, but I included it for completeness.

F, research in steam explosions. I think I generalized that a little bit. I think that we have to look a little harder than we have in the past at the whole class of accident sequences in which one has common mode, prompt containment failure in conjunction with a core melt. Interfacing systems LOCA is another subset, along with steam explosions. Vessel uplift, when you get melt-through at

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pressures is another item that I think deserves a little bit more attention than it has received.

DR. OKRENT: What kind of uplift did you have in mind? Due to what force?

MR. ROWSOME: Due to the blowdown of a reactor vessel at pressure when the bottom melts out. The plating melts out. That was looked at in WASH-1400 and was concluded to be a non-problem, but a non-problem with margins that could lead to the possibility that in some plant designs it might be.

Battelle Columbus has recently, a few months ago, done a back-of-the-envelope calculation riddled with uncertainties that says it is a problem, and Sandia critiqued it and said it probably isn't a problem but we ought to look at it further to see if it is a problem.

I believe we should pay more attention than we have paid to prompt containment failure in conjunction with core melt. And in that sense, I agree with the spirit of your message on looking at steam explosions. We really need to research this area better. I would be inclined to say we would do a little more in this area than we were planning to do at the time NUREG-0603 was written. Whether we will do it or whether RSR will do it, I don't know, but it is something I am going to push.

DR. OKRENT: Somewhere in some draft report or

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other I saw an estimate, at least for a certain class of core melts, the probability of a damaging steam explosion was one in 10 to the four, which is sort of a very small probability. But I don't know how one knows —

MR. ROWSOME: The early numbers in WASH-1400 were largely drawn out of the air.

DR. OKRENT: I would say this one was also drawn out of the air, but it was smaller. The air was further away or something.

(Laughter.)

MR. ROWSOME: Sandia, working for RSR, has done some studies. I am not really up to speed on those studies.

My impression is that they perceived — and are proceeding on the basis of the belief — that we need to know more about the deterministic phenomenology of these processes, and that it is not a subject for probabilistic scenario work in the context of steam explosions. We simply have to know more about the physics and chemistry and mechanics of cores slumping into the lower plenum of reactor vessels, and that it is appropriately a responsibility of RSR to do this.

(Slide.)

Siting studies. You had suggested doing a range of comparative and absolute risk assessments across a variety of sites, perhaps actual sites or hypothetical

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sites, to do essentially sensitivity studies on site characteristics to determine their risk significance.

The report of the siting policy task force has called for some more research along these lines. To tell you the truth, we haven't given much thought to it. We have been caught up in other things, and I can't tell you now whether we will embark on something in this area or not.

I think my inclination would be to wait until we have a handle on the nature of the results of the liquid pathway study before I would venture to assign a priority to this. Would you recommend another policy?

DR. OKRENT: Personally? Yes. And the ACRS, I think, did suggest that such studies be done. I only took a rather hurried look at the report put out by the task force on siting, and they had some numbers in there. And it would be nice, for example, to know what the significance of a half-mile exclusion zone and so forth is and what is the significance of 20 miles. And, of course, they did have a recommendation concerning the need to consider liquid pathways in the future.

Now, how do we proceed to implement that, if you come and say, "All I managed to do was find there are a lot of uncertainties which are almost boundless within reasonable numbers." People are going to make decisions, and decisions can be a variety, including that "We don't

BWH know enough to make more decisions." That is also a decision.

So, there is some reason, in my opinion, to try on some of these things to come up with the focused approach that will provide real help to the people involved in making regulatory decisions and with caveats, if necessary. But if you will tell them only that you have a lot of uncertainties, they probably suspected that.

DR. MARK: I thought one thing, at least in my mind, at various times, whether it is a source of this list or not, I am not sure, the increasing of the capability of the crack code is a thing or which one knows work can be effective and is needed. And that isn't quite so deeply buried in uncertainties as some things, and that would, by all odds, be worthwhile.

MR. ROWSOME: Right.

DR. MARK: To get it off a flat central continental plain and be able to discuss something that is near some water or a valley.

DR. OKRENT: But before embarking on any long-term program or at least an extensive program and effort, I think I would like to see some kind of preliminary estimate on the sort of more elementary bases as to what one thinks may be the differences, and are there likely to be important differences, and, if so, how are they going to arise and what do we have to do to take them into account.

I don't know whether you always have that at the beginning of a research program. Maybe you do.

MR. ROWSOME: We are going to move to that. As I indicated earlier when I was talking about the administrative focus, we need a much better task definition and much better coalescing of related tasks less a scatter shot of dozens and dozens of little isolated tasks, and we need to conduct studies in such a way that we get preliminary results out early and not wait until we have spent 200 years building a cathedral before we can move into it, before we can make some use of it.

I think those comments are responsive to the abstract points you are making. Beyond that, I would simply say, if you total up all of these things that would be nice for risk assessment people to do, you come up with a total that is roughly an order of magnitude than we presently have the resources to do, and some hard decisions will have to be made.

I am not prepared to give you the answers to those decisions yet, because I can't do that well, yet. But that, too, will have to be an iterative and ongoing process.

I appreciate that you don't find those answers very satisfying. I don't find them very satisfying either.

(Slide.)

Your suggestion on the NUREG on plant operations that we need to do more work to identify research needs, I think is interesting. I think it casts a different perspective than we had. We had quickly identified the need to do better simulators, to do better control room design, to develop a disturbance analysis system, to accelerate our work on human reliability prediction, of human reliability data collection.

But we have not sat back, as your suggestion implies, to sort out exactly where research should be most profitably focused. We were a little presumptuous in charging off and finding avenues to a solution.

I do want to follow your suggestion and take some time to identify where research needs are greatest and how we can close the loop and produce useful results. Ray DiSalvo has been made Chairman of the Coordinating Task Force on the human factors work for RSR, SAFER, and PAS, and he is and will be thinking along thse lines.

(Slide.)

This is a list of things we are doing. Now one of the things we have done since we made a presentation to you before you drafted the NUREG is to split out the

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specification of status monitoring from the disturbance analysis system. As you know, NUREG-1.47 already stipulates a requirement for status monitoring equipment.

One strategy, of course, is simply to make that a retrofit and require it of all plants, but we have identified the weakness in it in the absence of provisions to identify multiple failures and to identify plant configurations within which surveillance testing for further maintenance might be a risky proposition.

Let me give you an example. The status monitoring equipment called for in the NUREG would not tell an operator that while he has his startup transformer out of service for repair, that putting the plant in a half-tripped condition would enhance the risk of loss of off-site power substantially and that perhaps when he has the startup transformer out of service, he should be a little more discriminating than he would normally be in the kind of maintenance or surveillance testing that would put the plant in a half-tripped condition.

That kind of multiple failure or implications for operations of maintenance of prevailing plant status is not well treated in that NUREG, and we want to split that out from the disturbance analysis work in part because we think we can bring it to useful fruition faster than design specifications or research initiative to look at a smart black box that would attempt to diagnose in real time what is happening in the course of an accident.

So that is an initiative we have taken lately in part in response to our own perceptions and in part as stimulated by your NUREG.

PROFESSOR KERR: What is the initiative you have taken to look more carefully at how one --

MR. ROWSOME: To segregate from what we had before, to look at disturbance analysis and status monitoring with a somewhat longer focus for disturbance analysis and another with a shorter fuse, if you will, to look at strengthening some limitations we perceive in NUREG-1.47.

Am I communicating?

PROFESSOR KERR: Yes.

MR. ROWSOME: Transient simulation in research in licensing. I believe that RSR was already proceeding in this direction before the NUREG called for it. Since then, we have established collaboration with PAS to get our input into the RSR effort to develop criteria for and research tools in improved simulation.

In systems behavior and interaction, we share the perspective that that is very important. The integrated reliability evaluation program is intended, among other things, to provide a foundation for such work. It will not

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in the form that we envision it being brought to completion by '72, have the details or the fault trees necessary to do this properly or the full spread of the systems.

DR. OKRENT: You mean '82.

MR. ROWSOME: But it is a tool that we believe we should be developing now to enable us to do this kind of work in the future. Since the NUREG came out, we have been getting together with your Bulletins and Orders Task Force, and we are scheduled to get together this week and next week with the Lessons Learned Task Force in a number of collaborative efforts.

One of them — several of them entail our helping to specify studies to be done by licensees. I spoke of that before. That is, in part, a response to your suggestions here and, in part, a response to our own perceptions of need, applications of probablistic methodology.

DR. OKRENT: Excuse me. You are not setting up anywhere some group who focus on systems behavior interaction and try to think of its ramification?

MR. ROWSOME: No. We haven't done that.

DR. OKRENT: And as far as you know, RSR isn't either?

MR. ROWSOME: Not in — except that in their Systems Analysis Group does that, but I don't think I would construe that as being responsive to your desires. I think

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this is a good candidate for the kinds of studies one does ask licensees to do, in part because I think many of these concerns will hinge on plant to plant -- on non-standard features on design details of individual plants, and in part, because to do a proper job of it, one needs to have proximity to a lot of plant design data. That is much easier to come by in the industry than it is in the NRC.

I think we would be inefficient, and I don't think we can afford to be inefficient in considering the work load and the other priorities around. I think it is an important area.

And where I can assure you we can give it more attention is in the context of the advice and guidance we give on the specification of studies to be required of applicants and licensees.

DR. EDISON: Can I interject something here. We do do, from time to time, things in this area at the request of NRR to assist them in making decisions on whether changes that they might require of applicants or licensees are safe changes.

So in a piecemeal way, we do things like this. In fact, we are considering some now with the BWR actuation, ECCS actuation case. But we do not have an organization to do that.

DR. OKRENT: Maybe I will try a comment here. As

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written in NUREG-0603, the term "systems behavior and interaction" was used. Now if you take just the systems interaction part, as we have sometimes discussed it, I think it would be quite plant specific as you say. But I will speculate that there are operational considerations. In fact, you alluded a little bit ago to the possibility that there could be interactions when you make one change on other parts of the system.

There could be in these behavioral characteristics, which are important to safety -- it seems to me the NRC staff overall needs to think out how much do they have to understand about these overall behavioral characteristics in order to do their job.

MR. ROWSOME: There is another dimension in which these things will be addressed — a dimension that doesn't show up in a discussion of the PAS budget. As you know, the whole world of NRC and NRR in particular has been turned on its ear by Three Mile Island, and it is quite clear that major overhauls of the system will take place, perhaps in response to Kemeny, the Kemeny Commission Report, perhaps in response to the Ragovin Report.

A lot of thought is being given, and not in abstract terms, to major overhauls of the system. Now we saw Steve Saul here with you that it is not being adequately addressed. I suspect that that perception has spread

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substantially through NRR since Three Mile Island. There has been, in a narrow sense, some discussion between Saul, me, and Steve about our taking a larger role in the systems interaction generic safety issue, but I think all of that may be rendered moot by the kinds of organizational changes we may see in the NRC in the next six months or a year.

I think the perception is getting through that this kind of problem, the accident scenarios, the credibility of accidents that go beyond Class VIII accidents may make a real change, may make a real difference. I see that coming. I see movement. In a way, I think it would be too narrow viewed of us or me to say that I'm going to allocate so much of my budget in the next fiscal year to tackling this systems interaction question and that systems interaction question and so forth.

I think we are going to be overcome by events. We are going to passed by history, and we will have to tacle this organizationally, perhaps at a policy level. Is not this your perception?

DR. OKRENT: Do I think there may be changes? Yes.

(Laughter.)

MR. ROWSOME: I read the principal focus PAS being the application and development of probablistic methodology. As I indicated before, we are going to try to

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accelerate the process by which the more routine applications get moved to the line offices and accelerate the pace at which — with which RSR and SAFER begin to work in these areas.

But it will remain a principal focus of PAS to conduct applications as well as to improve the state of the art in this methodology.

(Slide.)

PROFESSOR KERR: To conduct applications?

MR. ROWSOME: To perform applications studies, to apply the methods to the 70 plants and so forth.

(Slide.)

The water specification and crack growth items are not in our baliwick, although I might say that I believe you, Dr. Okrent, have pressed in the past for a further look at pipe break phenomenology. I share with you the feeling that we have not put to bed the issue if the pipe cracks that have been showing up in both BWRs and PWR feedwater lines and the like. I believe we have been naive in the past to treat system reliability analyses, pipe breaks, with uniform failure rates. And it is supposed to apply in all terms and all circumstances. I don't think it is very likely that you are going to break a pipe at a steady-state full power operation. I think it is more likely that you will break pipes during thermal transients when the rates

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of change and temperature in the pipe are as great as they are after a trip in many cases.

I think pipe breaks in association with transients, in association with water hammer and the like, is much more probable at steady-state power generation. That has not been reflected in a risk assessment work in the past, and I suggested to Bill Vesely that he look at the conditional probability of pipe breaks, given transients and the like. He rolls his eyes and says, "Oh, my God, what a difficult task that would be."

He has some very convincing arguments of why that would be a prohibitively time consuming and difficult exercise. On the other hand, I think we ought to do a scoping study and get a feel for the problem, and at least keep our eyes open in our future applications of probablistic risk assessment and reliability safety analysis to the very real possibility that the pipe break susceptibility is not a uniform thing in time.

Disturbance analysis, as I have indicated, we were on that track before you recommended it. We concur with your recommendation. We had set out a collaborative effort under Ray DiSalvo to coordinate the work, not only between PAS and RSR but also with the Department of Energy collaboration about which you will hear more when you get a presentation on improved reactor safety.

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You had a number of specific recommendations for the risk assessment decision unit. You had suggested fazing out the work on Class III through Class VIII accidents, and we decided to do exactly what you say, and it will be fazed out with only limited incremental fund commitments in fiscal '80 -- 3, to develop an acceptable risk criterion, yes.

As the rest of this meeting will indicate, we are prepared to collaborate with you, Professor Okrent, in trying to meet your schedule, and a draft criterion for June of '80. Is that your target date? I believe it is. That will entail --

DR. OKRENT: We want to start that topic on time in order to meet that target, so let's try, if we can, to get through this by four o'clock.

(Laughter.)

Now, we have until four on the agenda, so we are not late yet.

DR. LEWIS: You don't have to worry. 1980 is leap year, so we have an extra day.

DR. OKRENT: So we have a day plus twenty minutes.

(Laughter.)

MR. ROWSOME: You had suggested levelizing the expenditures of the fuel cycle risk work. I wouldn't want to do that unless I could pick up alternative funding. I don't want to cut into the fuel cycle budget as it is

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planned in the schedule. It is closely tied into a collaboration with NMSS and SAFER. It is tightly scheduled and well planned. I think even though it may not seem to you, or for that matter to me, to have the urgency associated with some of the reactor work, it has no less importance, and I don't want to be laying a trap for ourselves in which the Department of Energy finally gets its act together and is ready to go and licenses a facility, and then NRC critical paths it for a couple of years while we figure out how to license it.

I think it is important that we make a serious effort to keep up with the state of the art.

(Slide.)

The program is leading the advance of the state of the art in the identification of the risks and of the models and of the disruptive events and so forth associated with geological disposal of waste. The program is also one of our best examples of a multi-office collaboration of a well organized, well planned review process built in and research directed through iterative refinement. The models being developed now are being used in sensitivity studies to identify what the key determinants are of disposal risks, and they will be used in successive refinements, iterative refinements, to be more focused and to be more discriminating in where future research monies will be

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focused, research efforts. So that while I concur with you that it may be a little bit less urgent than reactor work, it is no less important, and the long term priority is just as high.

I will be looking for additional sources to help prop up the overload I see. And if we can find some for this effort, fine. I would be happy to divert the funding increases allocated to this effort to some of the more pressing risk reactor work, but I do not want to gut this program. It is working too well, and its long term importance is too high.

If you like, Mike Cullingford can talk to you in a little more detail about how that coordination is going and how that program is developing.

DR. OKRENT: I suggest we had best get through the items that you have. Let's see if there is time later on but not in this time period.

MR. ROWSOME: Okay.

The flood risk program, I am not fully up to speed on, so I can't tell you that we have done something. But I would like to follow your suggestion and see if this is not a good candidate for reorientation into an iterative refinement type of approach from which we can get preliminary results that could, in fact, be fed into the licensing process in the near term.

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DR. VESELY: The funding has been cut in that program by approximately one half, so it is delayed. Our schedule now is now input until at least '81. We are running about 75K per year.

DR. OKRENT: Have you gotten anything out of the studies that have been done that suggest — or specific sites that the existing design basis for flooding is very good, more than necessary, okay, or maybe a contributor to risk compared to other things?

DR. VESELY: As you know, our reports on the task action plan identified flood as a potential high risk contributor. We have done some preliminary studies on Susquehanna. In those plants, it was indicated that there was high risk comparable to that found in WASH-1400, about 10 to the minus five, 10 to the minus four probabilities, core melt probabilities for those plants. Whether that is specific and how generic that problem is, I don't know.

Some of our investigations have shown design criteria to widely vary. Some design criteria are overspecified, and some are underspecified. I think the analysis had been done. Some are preliminary. It should as much as four or five orders of magnitude variation in the probability of clads to which plants are protected. Again —

PROFESSOR KERR: In other words, you quote a risk ɔc37 12 14

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for core melt?

DR. VESELY: Yes. Probability of core melt. These are very preliminary, in-house analyses. You have to realize the total amount of funding we have had in this project up to this period is $60,000 in four years. This compares to several million in the seismic, so this program is a very low level funded program.

We have additionally just recently input $75,000 into it, but that is our intended funding for the next fiscal year because, for example, the integrated program and the acceptable risk criteria taking some of the additional funding.

DR. OKRENT: The PAS frequently tell us about how they have contributed to how NRR should do its work directly by telling NRR where there was the biggest payoff with regard to risk reduction from this generic item or that generic item or what have you.

Now what I just heard — that maybe floods are an important contributor, and we can only 5, 75K out of — I don't know whether it is 5K or 50K or — it depends — I'm sorry — 75K out of $5 million or $50 million or whatever you want to take it from. And I would like to understand whether somebody in PAS has done a risk benefit or a value impact methodology study and judged that the floor work doesn't warrant any more than the 70K or what.

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MR. ROWSOME: Not yet. Our reassessment of priorities which is in progress --

DR. VESELY: That is still going on. I am telling you at the present time that is our funding. And unless we come up with a reassessment, that is the current budget and the current amount of funding.

MR. ROWSOME: One of your suggestions in the context of improved reactor safety that we set aside in this presentation -- the suggestion you all have made was to move the value impact work out of improved reactor safety and to utilize it in part in our own decision making process and to utilize it in the risk budget.

We intend to do that. I intend to give that high priority, so that we will have that tool available to us for exactly this kind of thing. I had given that some thought.

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DR. VESELY: And I think that the Committee should realize that if we go through this process of assessment and we are -- that we may not be talking about funding until 1981 -- that we are committing fiscal '80 funds now, an d this will take some time, and we may be upping our programs, not in fiscal '80 but in fiscal '81, we are talking about.

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DR. LEWIS: At the 75-K level for funding, is that being spent on plant resistant to flooding or flood models themselves?

DR. VESELY: That money is being spent on plant resistance, in that elevation of components, barriers for the Surry plant. Since the fault trees have already been constructed for that plant, additional information for elevations are being overlaid on those trees for systems effects analysis, consequence effects.

We are not spending that money on the probability of the flood occurring itself.

DR. LEWIS: I vaguely remember a letter that was written by some flood type right after Three Mile Island in which he said the thing that impressed him was that it was the middle of a river, and who claimed that the flood frequency model used in WASH-1400 was 20 years behind what all of the flood types now agreed on.

Is that true?

DR. VESELY: I would think that is right. We are spending and we have scheduled for completion, I would say some time next spring, the updated flood prediction model using the water resources recommended distributions, which is a log gamma distribution, and it gives higher probabilities of floods occurring than in using WASH-1400. That is where we have spent this 60 K, both in-house and with

George Washington University cooperation.

That code, which we call the FLOW code, we are passing at this time to some of the materials people and Idaho people for sensitivity analysis. We will get those models out by the end of this year, and we will start applying them to specific rivers the beginning of next year. We are working with Licensing on that, a transfer of tools.

MR. ROWSOME: To make sure everybody is tracking on this, we are talking about -- we jumped back and forth from two different flood studies, the external flood studies and the internal subcompartment flood studies.

DR. LEWIS: We understood each other.

MR. ROWSOME: I want to make sure that is on the record.

DR. OKRENT: This is the first time, I think, that you have volunteered what the probabilities might be along the Susquehanna. Let me --

DR. VESELY: You have requested a number of times our bases for that. We are getting that typed of, that analysis, and are planning to send the bases for those probability numbers to you within several weeks, two or three weeks.

Our concern was that we did not have -- they were in draft form, handwritten, and we are having those typed. We have some time and those will be sent to you. We understand you have been asking for it for some time.

DR. OKRENT: Going into years at this stage.

DR. VESELY: They were identified in TAPs as potentially significant risk contributors. It was given a fairly high range.

DR. OKRENT: In the most recent one, although there was, I will speculate, in-house information a year earlier. I guess I don't understand, if there is this potential, why it is going to take to FY '81 to augment the effort, if it is deemed to be potentially important.

MR. ROWSOME: Your thinking is ahead of our priority procedure. But I would venture to predict that this might be a candidate for a subject on which we would attempt to get perhaps SAFER involved or perhaps RSR; that if we find that, in sorting out what we can best do with our resources, that important things are left out, that will certainly not be just bandied about, but will serve as a basis for trying to develop more resources for ourselves or to better utilize the resources elsewhere in the Commission.

(Slide.)

We concur with your perception that we need accelerated input into guidelines and procedures from the human error research. Bill has described to you the -- is it best described as a working group, as a seminar, as a colloquial term?

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DR. VESELY: This task force of experts, yes.

MR. ROWSOME: On human error work. We have established the research task force on human factors that --

DR. VESELY: I would like to interject --

MR. ROWSOME: Which is intended to coordinate our several efforts.

DR. VESELY: That is something we did after the recommendations by ACRS. We did enlarge our human factors programs to accommodate some of this, some of these recommendations. That is something that is new. That is something we are doing as a result of the recommendations.

MR. ROWSOME: Disturbance analysis system. As you know, a great deal has been done by the Germans and early work was done by the British. EPRI has done some work on the availability-oriented disturbance analysis system, which they are now visually turning into a safety-related system. The Department of Energy also has a program in this area. Ray DiSalvo is coordinating our work and interfacing with DOE to coordinate the piece of the improved reactor safety program.

We have been working quite hard in getting the collaboration going with the Department of Energy and to coordinate our plans and our priorities to get this work under way. We share with you a sense that this is a high-priority effort.

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The Lessons Learned Task Force, as you know, also called for instrumentation to diagnose inadequate core cooling. We have not yet coordinated with them on that, but we are scheduled to do that this week.

(Slide.)

PROF. KERR: What is it you will do about that instrumentatio:

MR. ROWSOME: We are setting up the organizational structure to proceed with that effort. Our interest in the disturbance analysis system is principally to --

PROF. KERR: I am thinking of the instrument to determine inadequate core cooling.

MR. ROWSOME: As I say, we haven't yet gotten together with the Lessons Learned Task Force on that. I don't really know how they propose to implement that recommendation. It is one of their long-term lessons learned -- that they are working on now.

PROF. KERR: What are you going to do? Are you going to do anything about that? You are just going to listen to them?

MR. ROWSOME: We expect to collaborate with them on drafting, on establishing the requirements, drafting the requirements for the licensees. There are three or four areas where they called for long-term lessons learned, and they are hoping to get that turned around in the next quarter, in the next very few months. I have forgotten their schedule of the completion date, but it is quite close.

In the last week, we scheduled for this week several meetings with them to assist them in several specific areas. This is one, their recommendation that we need to find reliability criteria with which to supplement the single failure criteria and to evaluate the utility, if you will, of non-safety-related equipment in dealing with the accident. And we will be collaborating with them in developing those requirements and those recommendations. But that is in the future. It hasn't really started yet. I can't tell you what shape that will take or how big a piece of that responsibility we will adopt for ourselves.

But we will certainly use our background experience in reliability and risk assessment and risk-based measures of importance to guide that work. Beyond that, since it is scheduled for this week and next, I can't tell you.

PROF. KERR: Thank you.

MR. RCWSOME: Time-dependent failures. I think perhaps Bill would be the best one to fill you in on what we are doing there.

DR. VESELY: Right now we are developing computer codes to handle Weibull distribution, time dependence wear-out. We have again cut that funding to less than one man-year. So it is a methodology development at this time.

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Our problem there is we feel we have to go in and get the data, the basic data, the in-plant data, before we start doing very much more in this area. We are collecting statistical techniques, developing models that will allow us to handle component failure rates, time-dependent component failure rates, to calculate system unavailabilities, core-melt probabilities with the Weibull, incorporate testing, as good as old, as good as new, and maintenance as good as old or as good as new.

But that is about a one man-year effort at this time. It is software development. We are having to wait until we obtain the data from in-plant logs to be able to really analyze the time-dependent effects implied by this data. We are not doing any long-term pooling or any long-term time-dependent reliability evaluations at this time which could be associated with Three Mile Island.

When we start talking reliability of long-term operation, the models we are doing are essentially unavailability models, wear-out of components.

DR. OKRENT: If I were going to supplement what you described in response to this recommendation, it seems to me I would try to get some experienced people together to speculate on where you might get time-dependent degradation and not necessarily over a 40-year lifetime. In some cases, they might say it could show up in -- I don't know, five to ten years. How long has it taken for the cracks to show up around the steam generator nozzles?

DR. VESELY: Working with the IEEE task force on this extension of mechanical failure data, one of the goals of that task force is to come up with these kinds of expert estimates as to where the potentia problem areas and kinds of wear-out behaviors. So that is one of the --

DR. OKRENT: If you had that, you could fit it into the model you are talking about and at least say, in a thought experiment, is my inspection good enough to catch this if this is occurring, and so forth and so on?

DR. VESELY: Yes, and to do sensitivity studies to find the impacts.

MR. ROWSOME: You called for input into emergency planning, rates and types of releases. We have been doing that. We have been doing that quite intensively in the last few months. As you know, there has been a great deal of activity in emergency planning since Three Mile Island, and Roger Blond, who is our sole surviving consequence analysis member of the PAS, has been working almost full-time on exactly this sort of work.

DR. OKRENT: What has he been doing?

MR. ROWSOME: He has been working almost exclusively in support of two things, really. One is the emergency planning effort and the other is the siting policy task force.

Now, one of my high priorities is to find another man for PAS to do consequence analysis.

DR. OKRENT: I don't think you have read Item I on page 3-17 of NUREG-0603 the way it was intended to be read, because what it says is, the NRC recommends a research program be implemented soon to develop means as practical of ascertaining the time, rate, type and amount of radioactivity that might escape from containment into the atmosphere. We are not talking about accident studies. We want something that, if you have a serious accident, helps the people on-site to tell the governor or whatever it is what he would need to know in order to tell the state police what to do, and not have failed to have available things for which technology exists or can readily be developed.

MR. ROWSOME: All right.

Roger Blond, Matt Taylor and Joe Murphy sat down about a week ago and developed, over the course of a day's intensive work, some recommendations that had been requested of us by NRR to identify some criterion for alarm points for use in notification of emergency planners -- the police, the locals, the state authorities; three levels of alarm: something minor is going on, just to let you know; something fairly serious is going on; and then the third level of, this is it, so activate the emergency plans.

What we were doing there was drafting criteria for

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systems failures and symptoms that would be apparent to the operators to use as threshold points for -- that is not what you are asking.

DR. OKRENT: No. It sounds like it could be useful, but it is not really what is here.

MR. ROWSOME: I see one further possibility, and that is that you want new research into containment failure modes and prediction of releases, to make contact with the emergency planning effort.

PROF. KERR: It says 10 curies per minute are going out this little hole.

DR. OKRENT: And better yet, 10 curies of iodine, if you know it is iodine, and when. In the first place, you would want to know what is in the containment; and then, not only what, but how much; and then you have some way of telling you it is not there, it is starting to decrease, and if it is not in containment it must be going out. And that is what the meter would be telling you, crudely speaking, that this amount that was here is now going on.

MR. ROWSOME: Instrumentation to follow the course of an accident in terms of releases.

PROF. KERR: To tell you how much is getting out.

DR. OKRENT: Right now that is your big problem, in my opinion, in trying to give what I will call short-term emergency information. In other words, if you have a day or

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two, it is a different kind of plan. But you mentioned an interest earlier in accidents that might lead to an early failure of containment and where there might not have been all of that time for evacuation beforehand. We would like to be able to tell whoever is responsible off-site what you think is really escaping when, so somebody can correlate this with the wind and you know, at this point, do I really have to start thinking about the people 10 miles away or whatever it is, or only one mile away, or so forth.

It is not the first time we have discussed this topic with PAS. You are just the current representative of PAS. Tony Buhl once said he would do it. And I hope you understand what it is we are talking about.

MR. ROWSOME: I think I do now.

DR. OKRENT: I suspect it is more development than research.

DR. VESELY: I am not sure this is really PAS, anyhow.

DR. OKRENT: Well, it is just that you seem to have all of the other things related to emergencies and so forth.

MR. ROWSOME: That is one of our problems. Because we are essentially the WASH-1400 alumni, we are taken throughout the agency, throughout the industry, as being the authority on everything having to do with real accidents, serious accidents, as opposed to --

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PROF. KERR: That is much better than having people think you don't know anything.

(Laughter.)

MR. ROWSOME: Good point.

It is a good, large responsibility for a little group. And as I suggested before, we may have to think about organizational changes to accommodate the kind of workload that you perceive and that we perceive.

DR. OKRENT: In regard to that, there was a comment at the beginning of this NUREG in which the Committee said, we are really not trying to argue in detail in favor of each of the specific supplements the RSR and PAS thought they would like for 1980, and in fact for the FY '81 budget; that in fact the budgeting should go on; and that we recommend that the program be re-oriented to include our knowledgeable recommendations.

I hope you realize I think that is the flavor in which we are proceeding.

MR. ROWSOME: Yes.

You had made a number of general comments under the -- in the context of risk assessment, one about the name. I think I have mentioned that in passing.

DR. OKRENT: It is really not the name; it is the focus. I think the focus in the past was risk assessment. Very often when we had meetings, we were told that doesn't

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fall in the risk assessment area.

MR. ROWSOME: That may be, again, one of the effects of the WASH-1400 group. But it is quite clear that that historical legacy is being blown away.

DR. OKRENT: Your IREP program, for example, which we, I think, recommended in our first letter on draft WASH-1400, in the past didn't fit into risk assessment.

(Slide.)

MR. ROWSOME: You had suggested using the same kind of resolution of priorities that was done with the generic safety issues in the context of research priorities. Yes, I agree we are doing it. PAS guidance for the experimental and code development programs is one example. PAS coordination of the in-plant accident response efforts is another. Waste isolation research is another.

Of those, the three-way collaboration on experiments and code development is new. PAS participation in the SSMRP, that is old. Core-melt phenomenology, that coordinating task force is new. All of this will be done.

We are thinking about this suggestion in the context of the major overhaul of priorities and focus.

DR. OKRENT: I see. If you were going to look at the whole program in research from this risk perspective point of view within the next few months, I think we would be interested in knowing what you got. It might help us in what

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we wrote to the Congress. Is that your time scale or is it longer?

MR. ROWSOME: I would hope that we would have preliminary results in that much time. I don't think we will have brought it to a conclusion in that much time. We will certainly have had something in that much time and try to get that to you.

Closer interaction with line offices. No question about it, it is urgently needed. We, of course, have perceived it for years. NRR is beginning to perceive it now themselves, as evidenced by the several calls for help from the Bulletins & Orders Task Force, the recent calls for help from the Lessons Learned Task Force, collaborative efforts we have going on with Steve Hanauer's task force for generic safety issues.

We will have to build better bridges to Inspection & Enforcement. I think we have a good bridge now to NMSS, at least in Mike Cullingford's waste repository research. We will need to build better bridges with Standards Development. And there are many other areas in which we have been and should become more integrated, such as the emergency planning efforts.

I don't now remember what this short-term -- what the short phrase "expanded work" referred to, the last item there.

PROF. KERR: It has been treated above, anyway.

DR. LEWIS: It means work expands for the time allowed for it.

DR. OKRENT: You will be here tomorrow, Frank?

MR. ROWSOME: I certainly will.

I perceive that you haven't been altogether satisfied with us. I want you to know that I am not remotely satisfied with it.

DR. OKRENT: As a principal spokesman, we have met him before.

PROF. KERR: Where does he get the idea that we aren't satisfied?

(Laughter.)

DR. LEWIS: There is a clear absence of cheering.

(Laughter.)

DR. OKRENT: We will have to write an ACRS cheer.

(Laughter.)

DR. LEWIS: There is one I would like to write.

MR. ROWSOME: I say that I want to indicate that I have a strong perception that the kinds of things we have been talking about are important, and that we will have -- we as PAS and risk assessment reliability, probabilistic safety analysis, as a family of techniques and methods, will have a very large role to play in coping with Three Mile Island and in the evolving future of licensing. We are not

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now in a position to cope with that workload, and it is of deep concern to me that we get organized in the sense of priorities, in the sense of focus, in the sense of marshalling resources necessary to do that.

That is my number one priority, trying to cope with that problem.

DR. OKRENT: What I am going to suggest is that, if we can, the Subcommittee come back to the question of priorities on this program some time tomorrow; and that we think about it a little, since we will have to address it as part of our contribution to the next research report. But after a break, we will go on to the next agenda item, since we are just about on schedule. I don't want to lose a record for the first time.

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(Recess.)

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DR. OKRENT: If we can reconvene before we begin the next topic, I want to call your attention to the fact that the ACRS is a group that recognizes priority and shall rise to the occasion.

So, Dr. Lewis has prepared an ACRS Chair.

(Laughter.)

DR. OKRENT: And I will read it.

(Laughter.)

DR. OKRENT: I will ask the designated employee to read it.

MR. QUITTSCHREIBER: Hip, hip, hooray. What can we say? ACRS applauds you today.

DR. LEWIS: That was designed to make Frank feel better.

(Laughter.)

VOICE: Frank, did it help?

MR. RANSOME: As a matter of fact, it did.

DR. LEWIS: I don't want this, it's hot.

MR. SAUNDERS: You regret it already?

DR. LEWIS: Yes.

DR. OKRENT: Let me note that Dr. Lewis will have to leave about 5:30 and not be able to be here through the afternoon session, which I think will run until 7:00, if its members can hold out. So, I thought we might ask if he wants to make any comments on the next topic or the previous topic

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before we call on Dr. Vesely to tell us a little about the NRC, what it hasn't and what it has in mind.

DR. LEWIS: We are not having a meeting tomorrow?

DR. OKRENT: Yes, 8:30 until 12:00 or 1:00.

DR. LEWIS: I will be back for that, that's no problem. I can make my comments then or I can make a couple of comments now.

DR. OKRENT: Make a couple now and save a couple.

(Laughter.)

DR. LEWIS: I don't have anything deep to say. I think what we have said about the first time which we are past now which is the Udall letter. I guess I have already said what I think. I guess the answer can be written with some blanks left out, which the proper staff will fill in for us having to do with some of this juncture probabilities and branching ratios as we now know them. I think my personal view is that the general comment on making the point by going to closer -- looser and looser degrees of aggregation as you go along, will get through and will satisfy Udall. I think that is an easy thing.

The hard thing is the problem of setting a quantitative basis for risk. And I think what we are doing now is just right, which is in effect finding out where we are now and where we are going on the analysis of risk so we can get a better appreciation for the degree, the uncertainty, the

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so-called uncertainty bands. And the Udall letter is constructed in that respect by forcing us to think of that again. And I am glad we have a year. I don't see this being done very easily, nor do I see it finding its way into NRC very easily. So, it has to be done in a fairly systematic and defensible way, and I think that is what we are doing. I don't have any deep problem with the current track, nor do I have anything constructive to add to it right now.

MR. RANSOME: Commissioners Gilinsky and Bradford are behind the idea of an acceptable risk criterion, and they may in fact use their muscle to help get it going in NRC, if we come up with something that they can live with.

DR. LEWIS: I think that is precisely the point, Frank, that those two are behind the concept and I don't see any deep resistance on the part of the Commissioners. I see a great deal of reluctance within NRC to change its way of doing business. And in order to overcome that resistance with or without the help of the commissioners, because you know the commissioners come and go, but the staff stays on --

(Laughter.)

DR. LEWIS: -- would require that the case be made in a very effective and critical way and that it be workable. And that is what we are in the midst of doing now. I think we are on track, and I have nothing deep to say about it.

DR. OKRENT: I think Bill Vesely said that he could

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tell us what the staff has been doing and what they envision they might do over the next eight months to try to work with us in achieving our objectives.

DR. VESELY: I will spend about a half hour talking about the acceptable risk program PAS has under way and scheduled for completion in January and then I will talk about some of our proposals for a one year program to come up with the criterion for further review. And perhaps hearings by the NRC Staff.

(Slide.)

The overview of PAS acceptable program as we now have conducted the program consists of two major parts; determination of acceptable risks from nuclear power, societal requirement -- this is subcontracted to Perceptronics with Paul Slovic. The idea here is to do a very general study to determine what factors have to be considered in setting acceptable risk criteria or unacceptable risk criteria, if you will. And then a very different kind of study in which we are preparing the nuclear power fuel cycle risk. The fuel cycle setting up sensitivity matrices.

DR. LEWIS: When you talk about acceptable risks you are talking about risks that will be acceptable to the people, to the Congress, and least of all to us. Comparing it with coal is really not a very important part of that problem.

DR. VESELY: This was a specific request from

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licensing to us to do this task. It is a very small part of the overall program. This is an aside.

DR. LEWIS: So, licensing for their own reasons, whatever they are.

DR. VESELY: Yes, they requested that we do this.

PROF. KERR: It may well have to do with our environmental analyses.

DR. LEWIS: It is not a useful input to the objective of defining what is an acceptable risk for nuclear power. I think the less we fall into the trap of comparing nuclear power with coal, the better off the country will be.

The second point: In order to -- on the first thing for Perceptronics, I don't know who they are -- the question of what is acceptable to society is by no means attributable to such a trivial question. Are they doing interviewing or are they just guessing?

DR. VESELY: One of the techniques they have considered are comparative, or I should say preference techniques. They have gone out and polled and Paul Slovic is coordinator of this group at Perceptronics and they have made some polls, very narrow samples of people.

The goal of this particular work was a review of the techniques and approaches, identification of the weaknesses, date of requirement and no. -- they have identified four techniques and pursued four techniques, and I will go into these.

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It is not to really apply any of these techniques to come up with the criteria. It is the state of the art what techniques are available, what is required, and what problems you get into with these techniques, but not to apply any of these techniques at this stage.

DR. LEWIS: The only reason for pressing it is that as some of our colleagues have emphasized, risk is both the real thing and a perceived thing, and in our year or six months or whatever it is in which we are going to try to do something useful, we will have to understand not only the nature of the quantitative nature of the risk, but also something more than I think we now do about acceptance of it, and that will in the end require that somebody do some serious in depth study of the population.

DR. VESELY: One of the techniques is that --

DR. LEWIS: Please go on.

DR. VESELY: The acceptable risk program has an objective to produce a document describing the state of the art in methods to establish levels of acceptable risk and proposing a plan for research to better utilize the measures. It is a state of the art approach. The contractor is Oak Ridge. The secondary contractor is SAI and Decision Research. The cost is $200,000 in '78 and $300,000 in '79.

(Slide.)

PROF. KERR: Is there any relationship between

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Perceptronics and the ones you just -- I got lost in the transition.

DR. VESELY: They are the same. It is one and the same. Decision Research.

PROF. KERR: Perceptronics and Decision Research are the same?

DR. VESELY: That's right.

PROF. KERR: Thank you.

(Slide.)

DR. VESELY: A typical approach. This is Phase 1, which is contractor will solicit and synthesize input from recognized authorities in a broad spectrum of disciplines. The following methods of determining acceptability will be examined and they will go into each of these techniques and authorities that we have working on this project.

The cybernetic approach, which is what we sort of do now. Comparative analyses --

DR, LEWIS: Why is that called cybernetics?

DR. VESELY: The connection between politics, economics and technical -- that is the name given to the present method.

DR. LEWIS: It is? That isn't what Reiner meant.

DR. MARK: These guys didn't know what he meant either.

DR. VESELY: And then it is comparative analysis. It

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is a multi-attributed type of approaches.

(Slide.)

As I said, the second phase is this comparison of coal and nuclear. What it is is to identify critical areas in the calculation of risk from nuclear and coal cycle. You are using man days lost here. One life is equivalent, I think, to 3,000 man days lost.

PROF. KERR: This is really not an acceptable risk. It is a risk program.

DR. VESELY: It is a risk program which has been attached onto this.

DR. LEWIS: The reason I reacted to the comparative risk as any part of the gain that we can be involved in is that we would have to base acceptable nuclear risk on the acceptability of coal risk. Plus, I would have to be in a position of attacking coal for being too risky because I support coal power, too. And, in my view -- now, I am going to say something that is subjective. The entire issue is one that is between the forms of energy that we actually can have, which are nuclear and coal, and the forms which are visionary, which we don't have. That is the thing on which I would like to understand public response much more than I really do.

DR. VESELY: I think the coal versus nuclear was instituted because of a reaction to Inhaber's work in which he did compare nuclear versus coal. And there was a lot of

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criticism of that. And we wanted -- licensing wanted to understand the uncertainties and certainties of that.

DR. LEWIS: But that view was that solar energy is risky. The comparison between coal and nuclear was a minor part of Inhaber because of criticisms that came on after that in changing some of the equivalents and assumptions, nuclear became very high.

There are a number of specifically detailed things.

DR. VESELY: I don't think the SAI work -- it is only about 20 percent of the whole effort here. I think it has been --

DR. LEWIS: I am not attacking it as the budget, just as part of the logical structure we are trying to build up. I think it doesn't belong. I may be the only one who feels that way.

DR. VESELY: There has been some study and some work where they have compared coal with nuclear.

DR. LEWIS: Of course there have been. There have been studies about Mickey Mouse. This project was not to pick an approach and say we are going to use this specific criterion just to look at all of the different approaches.

PROF. KERR: Is this a user task? Licensing asked you to do it?

DR. VESELY: Yes.

PROF. KERR: You don't know what they want it for,

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but they want it. They just want to feel good about -- because they don't feel good about Inhaber or do they need it in licensing, or do you know? Maybe you don't know.

DR. VESELY: I can find out on that.

DR. LEWIS: My concern is not of it happening, my concern is having it appear as part of this structure that you are describing to us. I think it doesn't belong there.

MR. RANSOME: The structure he is describing is the structure that was planned and in existence long before our collaboration and our deadlines and our caucas was conceived on this. It is historical artifact, and your comment may be well taken that we need to reorient somewhat. It is manifestly obvious that we are going to have to reorient somewhat if we are going to meet this schedule.

DR. VESELY: Until now the acceptable risk program was looked upon as a fairly low priority effort in which you lumped things together and they went to a long-term effort with no intent of coming up with any criteria for many years. So, anything that was related at all to the acceptable risk was sort of lumped into this effort.

DR. LEWIS: In that context I understand, but I hope we can get a coherent effort.

DR. VESELY: I want to emphasize and focus on acceptable risk efforts, per se, and not the coal versus nuclear. The objective of the report and the output of this

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effort in January, 1980 will be a report. I have sent to the Committee a detailed outline of the contents of that report. And the study will take a comprehensive critical look at philosophical, political, institutional and methodological issues critical to determining acceptable levels of safety. You'll see the goals of the report are to compare critique, past and present approaches, suggest new approaches, serve as a focus for constructive debate and outline a long-term plan for bringing research analysis and public input to bear on the development of responsible and justifiable criteria for nuclear safety.

PROF. KERR: Is this the outline to which you are referring? (Demonstrating).

DR. VESELY: Yes.

PROF. KERR: This is going to be written by February of 1980?

DR. VESELY: It is written. There is a draft which will be issued in 1980 for peer review.

PROF. KERR: They wrote the book and then they found somebody who would be willing to support it.

DR. VESELY: No, I don't --

PROF. KERR: I don't see how they could have written it so fast, otherwise.

DR. VESELY: Again, we have been doing this for over two years.

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PROF. KERR: Okay.

DR. VESELY: That draft was issued several months ago. It is after collecting these various parts that -- these were sections already written. The general overview of this report structure is definition of problems, defining scope and limits of the analysis overview, methods proposed as guidelines for risk policy,requirements such methods must fulfill such as logical soundness, institutional and political acceptability.

(Slide.)

When I talked about the specific methods to be analyzed, these are the cybernetic processes in which decisions and standards are forged through the dynamic interplay of the political and economic measures. This characterizes the kinds of decisions, kind of process we now undergo. Some people have called it muddling through.

PROF. KERR: Do you know what the first sentence means?

DR. VESELY: No, I don't pretend to know the origin of the cybernetic process.

PROF. KERR: What the dynamic interplay of political and economic measures --

DR. VESELY: That is characterization of the national process interplay of the political and economic measures. The reports will go into some of the origins of the

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characterization of that process. I don't see how that name got to be associated with the particular process. I don't know.

DR. MARK: Do you mean anything different than if you just crossed out the first word?

DR. VESELY: No.

DR. MARK: Fine.

DR. VESELY: Again, there is --

PROF. KERR: How about if you strike everything except the last word?

DR. VESELY: Fine.

(Laughter.)

DR. LEWIS: If they have in mind doing some case studies in which standards, for example, speed limits or something like that --

DR. VESELY: There are collections of past tense of utilizing some of these techniques, that are in for example multi-attribute utilities vary where these have been applied particularly -- this isn't theoretical approaches to the -- whether the airport in Mexico should be built. Traffic policies in the sense of whether you want additional roads. Those case studies, mainly decision theoretic, where they went and asked a decisionmaker questions which gave his utility, essentially his utility function. That is, the last technique decision analysis approach. There have been applications of

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this on some small scale decisions. These techniques have not really been used on the kind of scale that we are talking about.

PROF. KERR: I am not trying to be critical. I am trying to understand what the English says.

DR. LEWIS: These are four separate methods. These are the four things you alluded to at the very beginning.

DR. VESELY: Yes. The comparative analysis of existing safety standards for analyzing and offering a basis for future standards. The expressed preference approach, in which appropriate groups of citizens are asked directly how safe is safe and what formal methods are used to establish a utility function?

(Slide.)

For example, on the comparative risk example, a question addressed is this: Perceived versus calculated risks, how does risks from nuclear plants compare to other hazards? This work will not attempt to answer the questions, but to identify questions that have to be asked. Questions that have to be considered. How you go about proposals and how you go about doing, achieving such answers to some of these questions. One doesn't necessarily expect answers, but one hopes to not obfuscate any further.

(Laughter.)

DR. OKRENT: Not everybody here is against this study on comparative risks.

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DR. LEWIS: I am not either; in the right context.

(Laughter.)

DR. VESELY: These are some of the questions that are being addressed.

(Slide.)

Some examples of questions: This is the expressed preference technique. How do you obtain a representative sample? How do you construct questions so they analyze the kinds of techniques? You have to realize we have people working on this task.

I show you the kinds of individuals we have involved in this task.

(Slide.)

We have economists. We would not have the decision-makers at this time directly involved, the engineer, the decision-maker. We are looking at, if you will, the theoretical considerations, the general considerations and attempted to address these psychological aspects, the economics, the geographic implication that any criteria, practical criteria or real criteria we'd have to consider, have to address. It is a very general theoretical program. The author of this program was on questions and state of the art techniques.

DR. OKRENT: These are the people who will be working after January, or who are working --

DR. VESELY: These are the people who are preparing

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specific sections and chapters in that text, in that study. As of January they will not be working on this task, and we'll talk of how I'll propose to utilize these in this specific task. I will talk about the new task of establishing at least a straw man criteria, but this task is scheduled for completion in January of 1980.

DR. OKRENT: Could you give me two sentences on what you had in mind on geographic implications of risk acceptability?

DR. VESELY: Demographic studies, really. Very practical age distributions, sex. Effective risk on a different strata, different --

DR. LEWIS: These aren't geographic things.

DR. VESELY: Demographic.

DR. LEWIS: It says geographic.

DR. OKRENT: It is demographic.

The other thing is, do you recall what specific areas Spence did with regard to economic aspects?

DR. VESELY: No, I do not. I think in that hand-out there is a special chapter on economic considerations. There is a whole chapter on that.

DR. OKRENT: We will let it gc for now.

DR. LEWIS: Let me ask one thing: I am really trying to understand.

DR. VESELY: Certainly.

DR. LEWIS: A typical American citizen I was talking

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to the other day said to me that -- said you are all wrong. If people aren't afraid of nuclear power because it is unfamiliar, they aren't afraid of it because it is invisible. I don't know whether that was a deep word or a dumb comment. But where in this list would such questions appear.

DR. VESELY: For example, the psychological aspects of risks and public perception of risk. Slovic is attempting to address those questions. Keeney may be doing it in some of the utility decisions.

PROF. KERR: On a previous slide you gave examples of questions to be addressed. I thought you said something about these questions weren't going to be answered, they were just going to be posed.

DR. VESELY: And the method of addressing it.

PROF. KERR: For example, I don't think any of those people would have any difficulty answering a question, whether the public is irrational in their ability to make decisions. The answer is clearly yes. It doesn't mean that it is bad. You don't have to do any research to answer that question.

DR. VESELY: But how do you incorporate that into the criteria or into evaluating a criterion? They are trying to address that kind of thing.

PROF. KERR: How is the method of answering a question -- I might have known about that.

DR. VESELY: Those sorts of things -- in fact, in

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that particular area it is a question of standard kinds of survey response.

DR. KERR: The book or whatever will probably in the main answer questions like that; won't it?

DR. VESELY: Parts of them, yes.

That is how you go about correcting, extracting information which will allow you to get unbiased responses to questions, but how you actually set up a survey to ask questions which will allow you to -- the specific questions you asked to infer a criteria on nuclear power. That is not going to be addressed. You are going to have to -- how you survey or what specific questions you ask the public on nuclear power to infer acceptable risk, I don't think we are going that specific.

PROF. KERR: They won't develop the question there, but they will give guidelines on how you go about doing it?

DR. VESELY: That's right.

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DR. VESELY: Again, you have to understand the scope of that program. It was not intended to get out any specific results and our goal was specific criteria to get out these results and we — from that program we went to identify errors of further research, further development.

Now because of the ACRS request and the request from Udall, we want to talk of how we can implement a short-term program within the next year to come up with straw man criteria.

I would like to propose to offer our approach and what we have done.

PROF. KERR: We also don't want to publish anything that would inhibit the use of straw. It is, after all, a biomass.

DR. VESELY: That's right. It's renewable.

DR. LEWIS: And without it, you can't make bricks.

(Laughter.)

DR. VESELY: This theoretic side I think is very important because any criteria, I think you have to have the experts and the theoretician, as well as the engineers, the public, and decision-makers involved here.

I think even though this past project was quite general, I have to say, to be candid, the intent of this program was not to come up with any criteria.

PROF. KERR: It was really to get the ACRS off your back.

(Laughter.)

DR. VESELY: That's right. Since we can't do that, we are going to propose another program --

(Laughter.)

DR. VESELY: -- in which we may be able to -- where the goal is to get some interim results, some short-term results.

DR. OKRENT: We need a Class 2 chair.

(Laughter.)

DR. LEWIS: Yes, sir.

(Laughter.)

DR. VESELY: I will talk about our proposed PAS risk criteria program which is to establish tentative quantative risk criteria, as we see it, to be submitted for further review.

The timeframe here is to October of 1980. Whether it is June, July, it is essentially a one-year program. I think our position, I have talked with Saul on this and reserach -- it is research criteria which will come out of this. They are intended to be interim criteria to be modified or rejected after the experience is gained in attempting to apply the criteria.

If we come up with criteria, straw man criteria, we intend to ask the experts now, the theoreticians, the psychologists, the implications of these criteria and how we propose to attack this question is shown here.

(Slide.)

To assemble and construct straw men criteria to be critically reviewed for their decision and acceptability implications, their implementation demands and their practical ramifications, particularly regulatory review requirements.

There is one review out that was just presented at the fast reactor survey in Seattle. One of the criteria we essentially want to look at is the feasibility of using WASH-1400 as a goal in that criteria as a standard and modifications and extensions required thereof.

In the licensing process, WASH-1400 is being used as a criteria anyhow for many decisions. And, for example, having done 23 analyses of aux feed systems, we have found, and I believe that the WASH-1400 analysis is representative of a better design, a better designed plant. And of those 23 aux feed systems, over half of those had failure probabilities much higher than WASH-1400.

If we had used WASH-1400 as a criteria, we certainly would have caught Three Mile Island.

And so that is a specific criterion that we are proposing to examine. One of the questions was how do you incorporate the uncertainties? And looking at WASH-1400, not as an "as is" but as it should be. And perhaps not as an acceptability criterion, but more as an unacceptability criterion if it is above WASH-1400, whether that be core melt probability or probability versus consequence. Saul believes probability versus release might be a better release category. Whatever.

If it is above, it will not be accepted. And the exceptions would have to be considered. And if it is below, it would have to go through some additional reviews.

So we are looking at WASH-1400 as an unacceptability criteria.

DR. PLESSET: You said if you had this analysis, you could have prevented Three Mile Island.

That is a little strong.

DR. VESELY: If we had this criteria and had performed the integrated reliability program in the event trees, I firmly believe we would have caught and corrected Three Mile Island before it occurred.

It would have stood out that much as a sore thumb if you calculate the core melt probability.

PROF. KERR: What would have stood out?

DR. VESELY: The core melt probability.

MR. ROWSOME: Two or three things would have stood out. The fact that the PORV is challenged on every feedwater transient should have been caught by a competent event tree

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analysis.

DR. PLESSET: It was designed that way.

MR. ROWSOME: It was designed that way and that gives you such high exposure to small LOCA that you would have, A, not accepted that design feature, and B, it would have led you to analyze that particular class of small LOCAs and you would have presumably caught the fact that you could get water solid pressurizer.

And at the minimum, the operators would have been sensitized to that as a symptom of that class of small LOCA.

DR. PLESSET: I am skeptical of that.

PROF. KERR: B&W has already analyzed and discovered that, as had Michelson.

DR. PLESSET: Others would have, too. Would you have exposed the deficiency in the pressurizer? Would you, in this kind of study?

DR. VESELY: Just from looking at the core melt probability, if you calculate that for the B&W plant, you get almost two orders of magnitude higher than WASH-1400. Using core melt criteria, B&W, even with the largest uncertainty would not have been passed. It would have been unacceptable. To get a 10 to the minus 3 core melt car probability is totally unacceptable and that, you didn't even have to have a competent engineer to do.

DR. LEWIS: That is because of the feedwater transient gn

DR. VESELY: Yes, and the high frequency of demand, of challenge of the valve.

DR. LEWIS: You are suggesting that when WASH-1400 was multiplied by 50 to go to 100 reactors, that was a big mistake?

DR. VESELY: We feel that that extrapolation may not be valid now because of the -- again, the very large design differences in plant-to-plant variations which does surprise us.

DR. LEWIS: You are saying that at that time, the B&W plants were already equal to 100 reactors.

DR. VESELY: Yes. And the multiplication by 50 is, we feel -- may not be characteristic and probably is not characteristic of the population.

DR. LEWIS: I thought you said it was certainly not.

DR. VESELY: Certainly not at B&W. And we have said, as I said, on the aux feed system, we have done 23 systems. Over half of those had two orders or magnitude higher on the aux feed analysis. And this is one of the things that we want to investigate in this study, is the implications of using such a criteria.

One of our concerns is WASH-1400 may be too stringent of a crite ia. If we did use WASH-1400 as a criteria, we may, for example, have to -- and not allow exceptions, have to shut down --

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PROF. KERR: When you say WASH-1400 as a criteria --

DR. VESELY: Specifically core melt.

PROF. KERR: You mean use that core melt probability that was calculated?

DR. VESELY: Yes, as an unacceptability criterion. Anything above that would not be acceptable.

PROF. KERR: I am not trying to — I want to make sure that I understood your point by using it. If you really mean the results of it, of the study —

DR. VESELY: Yes, the results of the study. Now what results, whether it be core melt or probability versus release or probability versus consequence. There is another argument.

Of course, we want to compare other background risks and other criteria. But WASH-1400 was calculated using available techniques — event tree, fault tree techniques and available data.

If we were proposing to attempt to satisfy these criteria by using such techniques, event tree, fault tree, then, essentially, we are comparing apples with apples.

PROF. KERR: I guess I am a little puzzled that you used the results of that one calculation because had it just happened that you calculated the reactors at two orders of magnitude higher risk, you would have exactly the same technique and the same WASH-1400, but you would have gotten a different number because you happened to pick a different reactor.

It seems to me there must be a more nearly rational basis for picking an acceptable risk number. To just pick a number that happened to come out of this study —

DR. VESELY: Whether we pick -- we are certainly going to look at other things. I think WASH-1400 has as much rationality as any other number we pick. It is probably one of the only things we have.

DR. LEWIS: What you are suggesting doesn't depend on that. You could pick 10 to the minus 4. You could pick 10 to the minus 5.

But what you are suggesting as one option, I understand that, is that you simply apply to a plant a criterion that you do a WASH-1400 type analysis on it and the core melt probability shall come out less than some number which you have chosen at random.

That is an implementable standard.

DR. VESELY: We want to examine how to modify that, whether, for example, to take the check valve or to take the check valve out or incorporating other uncertainties and then other criteria such as 10 to the minus 4 have been proposed.

Dave Okrent has proposed some criteria. In Kinchin, some of the Europeans have proposed it. But we see this as

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an iterative process and we want to get something to focus our attention on to start with a straw man and see how good it is and what the implications are.

DR. SAUNDERS: You say that it is an unacceptable criteria, meaning if you did the WASH-1400 analysis and passed; it didn't mean you would license it, but if it failed to pass, it wouldn't?

DR. VESELY: That is our viewpoint at this time.

DR. SAUNDERS: When you say a straw man, you mean simply, you want to see destroyed — you mean when you said straw man, tentative?

DR. VESELY: Set up for criticism for peer review. The goal is to come up with specific criteria, as we see it, for further review by the public by licensing by —

DR. SAUNDERS: By something different than tentative?

DR. VESELY: No, I certainly do mean tentative in the sense that, for example, the integrated reliability program we have going, we are getting a number of programs that will perhaps in the next several years, allow us to update this.

I don't see any criteria as being forever.

DR. SAUNDERS: That's fine. I just want to be clear about how you were using the English language.

You mean something deeper than tentative?

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DR. VESELY: "Interim" is a better word.

DR. SAUNDERS: All right. That's fine.

DR. VESELY: We have contacted our proposal -- our proposal is two-fold to actually formulate these criteria. We propose working with --

DR. PLESSET: I am still disturbed by your very positive statement that if you had made this kind of analysis, you could have prevented --

DR. VESELY: We would have found it.

DR. PLESSET: That is very good ex poste facto. I think you might have other opportunities to commit yourself in advance.

(Laughter.)

DR. VESELY: In fact, that's right. We have, in fact. A survey of WASH-1400 identified the aux feed system as a very -- as a large risk contributor, human error of leaving valves closed -- it is just an observation.

I would like to in any criteria, to use a criteria that says if we had that at that time and had done our job well, would we have caught this?

And I think it is an important question. If the criteria would not have caught past accidents, then it is not a very good criteria.

It depends on what assumptions you make. It is just a test.

PROF. KERR: It seems to me what you want to say is it might have, or there is a high probability. But you are going pretty far to say that it would have.

DR. VESELY: Okay. I think the probability is high enough that I am fairly confident it would have been caught.

DR. LEWIS: I am sort of on Bill's side on this one; that is, if one were to take a few million dollars and put Norm Rasmussen in chains, or whatever you have to do and do a WASH-1400 on each and every plant as a condition for building the plant and take that mass of information and use it in some sensible way, whether or not by just taking the final core melt number or something, you would undoubtedly end up with a safer plant.

DR. VESELY: Here is the problem, and some of the factors to be considered.

DR. LEWIS: Wisdom is good. Knowledge is good. You may not do it well, but it is still better. If a thing is worth doing, it is worth doing badly.

(Laughter.)

DR. VESELY: How much detail — if you have a standard, there are various ways of implementing it. You may not require a very detailed analysis as WASH-1400. You may only have to pick, for example, to include the active components, or to go down on specified systems, do a specified

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accident sequence, which goes back to the recommendation, perhaps, of having plants to do some of these and limit the factors and contributions to be considered at this time and gradually include them as we improve our techniques.

These are some of the factors we want to consider.

PROF. KERR: Bill, I think we are all semi-enthusiastic, maybe even enthusiastic, about further use of probabilistic analysis.

But if one is talking about acceptable risk, I have an idea that decision-makers need to know more than the risk of core melt.

Isnt' there going to have to be some coupling of that to public health or the possibility of fatalities, or something?

Isn't that sort of risk going to have to come into a consideration in some fashion?

DR. VESELY: Yes, when it is to be incorporated. Now we can, of course, incorporate various criteria at various stages. A consequence is —

PROF. KERR: I don't see how you are going to get any general consensus on acceptable risk of core melt. In the technical community, you might.

I am not even sure of that. But in a general sense, it seems to me that one has to go farther than this in establishing consequences. Do you think not?

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DR. VESELY: I think you may have multiple criteria. For example, it is probability versus consequence —

PROF. KERR: I was talking about the earlier question, which was an attempt to define an acceptable risk.

DR. VESELY: Yes.

PROF. KERR: I have assumed —

DR. VESELY: Interim criteria on core melt is workable It is certainly not sufficient.

PROF. KERR: There is a difference between workable and acceptable. I think I agree, the single failure criteria is workable in some instances, but it may not be acceptable.

DR. VESELY: We want to investigate that here in this program. The implementation problems, too.

For example, one option —

PROF. KERR: Of course, there are implementation problems.

DR. VESELY: — look at the core melt as having a criteria and then perhaps having developed site-specific models and improve our capability to now add the probability versus consequence or the health effects considerations.

We are trying to limit the establishing criteria when we are still attempting to develop models data.

PROF. KERR: We started talking about acceptable risk. I thought from the discussion that you were concerned with public acceptability, whatever that means.

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Now it seems to me that you are talking about an acceptability criterion that could perhaps be used in the licensing process for a safety study, and it is identifiable. You can pick a number.

But I hardly see how one can --

DR. VESELY: Core melt would not be acceptable. It might be an unacceptability criteria. They are very different.

Just because the core melt is satisfied, or probability versus consequence. I don't like acceptable. There is too much involved in what is acceptable. It is easier to say what is unacceptable.

PROF. KERR: I am not trying to put words in your mouth. I thought earlier we were talking about acceptable risk.

DR. VESELY: Sure, and how to approach that and what kind of criteria do we propose to examine in this program. And one is going to be core melt and maybe the problem is that it is practicable, it is workable, but the public may perceive that it is not enough.

I think that those kinds of questions have to be --

PROF. KERR: I think the public might perceive that it is irrelevant.

DR. VESELY: I don't think core melt --

PROF. KERR: I don't think core melt is irrelevant, either, but the public might because it might not understand

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the implications thereof.

DR. LEWIS: Bill is emphasizing, and I think correctly, that as an unacceptability criterion, as a hurdle to be overcome, it makes some sense. You wouldn't want a reactor that is going to melt too often.

But, of course, you can't use it as an acceptability because people will say, my God, you mean that you are going to accept a Three Mile Island once a year as long as there is no core melt?

Of course that is silly.

But I think what he is heading toward, and it makes a kind of interim sense, is a set of hurdles -- must not melt too often. Must not release too much too often and make a bunch of those that one has to go by, that in the end leaves you with a feeling that you have come pretty far.

That is an implementable program.

PROF. KERR: And all of the things that are unacceptable and then you assume that everything else is acceptable.

DR. LEWIS: No, because, after all, you have to have a criterion, which is clear; otherwise, it isn't legal. And it has got to be well defined in the sense that somebody knows what he has to do to meet your criteria. And then it is up to you to make a sufficiently and -- hurdles he has to overcome so that you in your heart are comfortable that you have covered most of the bases. You will not have covered all of the bases.

That is life.

But at least you will have done something which is quantitative in the sense that -- which is what this is all about -- in the sense that you have said what it is that has to be done to make a thing acceptable in terms of its risk, instead of in terms of people's visceral feelings.

And I think that is a possible program.

DR. VESELY: Again, one of the outcomes of this program is maybe that core melt is an unacceptable criteria, and that may not be adequate. We want to examine that. We want to keep ourselves open at this stage to examine various criteria. Unacceptable criteria, not only what is acceptable, which is a much harder question.

The kinds of factors that we want to consider in this program are listed here. We want to look at risk from other activities and essentially collect what other people have extracted and evaluate this.

One of the outputs of -- it is a handbook to be issued in January -- will be a collection of other risk activities to try to supplement. And then there is the attainability of a proposed criterion.

If we have a criterion and were to apply it now to our plants, what kind of implications might we have? Would we have to shut down the plant, acceptability or unacceptability criteria, the level of applicability. Do we do the core melt of probability versus consequence or release?

The value implications of the criteria, the means of expressing criteria, how do you incorporate uncertainties, method of demonstrating acceptance?

What kinds of models and data do you describe in attempting to show that you satisfied your criteria and the means of certification?

Did you set up a review process, legal, economic considerations to judge the satisfaction of the criteria?

(Slide.)

We have — our proposal, we have talked to Brookhaven to coordinate the information collection tasks and probabilistic and statistical issues, collection of data, examination of uncertainties bringing together experts on this question, and to utilize our experts that we have gathered together on this general task to look at implications of any criteria that are proposed.

The formulation of the criteria — specifically, one of the criteria to be examined is WASH-1400 implications. And we propose to go to IEEE engineers and professionals to initiate and formulate a national task force on nuclear risk criteria to be reviewed and critiqued by a group outside of the nuclear community for the unbiasedness. But we think the formulation of criteria and formulation of implications has to come within the nuclear community.

The review, the critiquing in this program will come from outside.

PROF. KERR: What did you say -- has to come within the nuclear community?

DR. VESELY: The implications, the actual formulation of the criteria, we would propose and we feel that it has to come from individuals such as IEEE professionals who are acquainted, associated within probabilities and nuclear, and also having the experts on psychology, the human factors.

We propose having these experts that have worked on this past project involved with the engineers in IEEE --

PROF. KERR: When you say the formulation, are you implying in that also the determination of acceptability or do you mean just within the community to formulate an acceptability that is judged more generally?

DR. VESELY: That would be judged more generally. But we want to formulate criteria to be reviewed and judged in a more general fashion. We have talked to the IEEE specifically on the standards committee. They are willing to do this.

We have allocated $300,000 tentatively for this task.

DR. SAUNDERS: The ASA at one point wanted to appoint a group of people to look into it right after Three Mile Island.

What happened to that?

DR. VESELY: We have had contacts with the -- I have forgotten his name -- the president of the society in Washington last may, they are going to work with us as consultants and advisors.

I think this is one way of getting the statistical community involved, particularly on formulation and handling of uncertainties, Bayesian versus classical, and these kinds of questions.

And we have already talked with them.

We would like to have as many of these experts from various groups, AIF, that have the risk acceptability -- ANS has thought up some of the problems.

We have also talked with the English and the French and are very enthusiastic about helping to work there.

And I see us going there and having several meetings and having their inputs. They have thought about this question

DR. LEWIS: To the extent that you bring in public input, the question of acceptability of risk has another dimension which is sometimes called benefits. When there have been interviews and Gallup polls and things like that, they have revealed that people's decisions about what an acceptable risk is hinges very much on their perception of whether the

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thing does them any good.

It can do them good in strange ways. People do climb mountains and risk their lives.

Somewhere, there has to be some way of determining the sensitivity of acceptability to benefit perception. I don't know how the hell that gets done, but it needs to be.

DR. VESELY: That's right. One of the approaches that Paul Slovic has proposed is a survey, the expressed preference. That is very difficult. Again, what we can do in one year, and we may come up with unacceptability criteria hurdles as we call them, Dr. Lewis, and then go on and try to fill out the picture and try to get more of these hurdles and try to get an acceptability criteria. Benefits is a very difficult question in the conduction of polls and surveys that will take time.

Trying to get something in a year is a time constraint. I think the goal is to come up with unacceptability or acceptability criteria, depending on the reasonability and bases for it, and then to identify, specifically identify those projects and surveys that have to be addressed to do — to broaden the picture. Perhaps to address the acceptability criteria in a better manner.

I think one of the ways of focusing this whole debate on acceptability is to propose some criteria which this task is to do, and we want something workable which licensing will also utilize and will accept.

Again, the criteria is fine and may have beautiful characteristics about it, but if it is not implementable, pBWH acceptable by the NRC staff, I don't think it will have much impact on public health and safety so that there is a balance, particularly in the short term.

I think after the long-term acceptability, the probability versus consequence. But how do we get there? Maybe in steps.

PROF. KERR: I applaud what I think is your approach of solving one problem at a time. However, at least before Three Mile Island, had you asked for concerns, many people would have given the long-term waste disposal as a principal concern. Now, it seems to me the acceptability of nuclear power does depend markedly on risk associated with the individual reactor, but it also depends on risk associated with a system.

I take it what your approach is at this point is to try to focus on the individual plant and if one gets something, one can then perhaps extend it to the system.

DR. VESELY: I think Mike Cullingford of the staff should address that. We have been — Mike has been attempting to look at the setting of standards for the rest of the fuel cycle. I think, yes, this short term will be setting for the plant itself — but I think it will have immediate implications on criteria to be used for the fuel cycle. We are looking at that separately. The goal of this program is not to do this.

PROF. KERR: Is it your perception that members of the public are likely to look at it separately or that you are looking at it separately because it seems to be a workable solution to the problem?

DR. VESELY: A workable solution to the problem at this time, within the time constraints and funding constraints.

DR. MARK: I was going to comment on this question.

DR. CULLINGFORD: I think to take the problem piece by piece, especially in the reactor field where at least we know we have less uncertainty and we have WASH-1400 as a base, the methods and so on can be used more readily. In that sense it is commendable.

We have broken with waste management risk and EPA is trying to set standards on public risk and risk assessment, but there the data is much less and the uncertainties are horrendous. I think we have to begin with the best part first, which is the reactor area.

DR. VESELY: We would like your input and certainly views and critique of this proposed approach. And we hope to plan to work with you closely on this.

PROF. KERR: It is hard for me to know how much the difficulty of calculation and the uncertainty enters into a determination of acceptability or unacceptability.

One is arguing here that perhaps one can calculate risk with less uncertainty and with more of a background. This may well — people think you know what you are talking about and they may well find that a particular number is more acceptable than if they think there is a lot of uncertainty I am not sure.

DR. CULLINGFORD: I was meaning to say that, really, without a standard or criteria being -- without one being able to show compliance or non-compliance by some acceptable method, it is not much use in having that standard. It just leads ot subjective argument.

DR. VESELY: Our proposal wants to come up with a standard in a way of attempting to satisfy that standard, to use what models and data and approaches you can use in attempting to demonstrate that you have satisfied that.

PROF. KERR It is conceivable to me that one might find that the public doesn't find nuclear power acceptable at any risk level. If that is the conclusion, then to try to establish what the risk is is useless.

I don't think you will discover that, but it seems to me that the question of acceptability and the question of establishing risk, while not completely separable — I don't think they are separable, is certainly not the same question.

DR. VESELY: Again, whether it is acceptability or unacceptability, it is quite different. We also want to look at criteria that are able — I feel — we feel that the criteria are very closely couple to the needs of demonstrating how these criteria are satisfied.

If we come up with a criteria and don't give the industry specific data or specific models to use, specific —

PROF. KERR: I see the problem. I don't disagree with it.

DR. VESELY: I guess I don't understand, then —

PROF. KERR: I am not sure that has very much relationship to what is acceptable to the public.

DR. VESELY: I think that is a much broader question.

PROF. KERR: And we were talking — our principal thrust here, I thought, was a discussion of the program that was aimed, eventually, at trying to determine what was acceptable to the public.

DR. VESELY: I don't see that necessarily coming out of this one-year program.

PROF. KERR: I don't, either. But I would think that that would be a principal thrust.

DR. VESELY: Yes. And I think we will — there is output we are receiving from our previous study that will help to direct and determine areas that have to be pursued

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in trying to get to that problem. But we look at that as a much bigger problem, longer term. And with the interim criteria we want to come up with some criteria to approach this broader question.

PROF. KERR: It seems to me the interim criteria to which you refer could be done independently of a study to determine acceptable risk. It could be an implementation that one ought to try to use more probabilistic criteria in licensing independent of acceptability to the public.

Here now, you are saying, let's say hypothetically you are replacing the single failure criteria with some other criterion, with risk criteria. That seems to me it can be done completely independently of a study to determine acceptable risk. I don't see the coupling between the two as necessary. I am not even sure they are desirable, the coupling is desirable.

DR. VESELY: Again, we have the longer term study to try to address those factors, those considerations. We see them as two separate programs. This program is to come up with criteria. I think they merge. I don't think you come up with the magical acceptability criteria that the public will accept at once.

PROF. KERR: I am not even sure you can do that. I am sure you can do the other one, probably, almost --

DR. VESELY: I don't think it is as easy as doing

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immediately. I think that is implementable; it is doable in one year. And how far we could go to addressing what is acceptable, what the public will buy, or what we have to do to identify the factors or specific questions or specific things we have to do to try to get answers to that question — I think will come out of the study.

I don't think we will come up with an interim criteria that the public will buy. That will go through a whole review process.

PROF. KERR: It seems to me that in a sense these two suffer from being associated with each other. I don't see — maybe I don't understand the two, maybe I should listen. I will listen some more. I don't see the coupling at this point.

DR. VESELY: We are open to suggestions of better ways of attacking the problem. We think in one year what we propose is all you can do. We have that constraint on determining criteria, acceptability criteria. We have certain criteria we would like to work with. It is implementable. It is a start. I think it is a step in that direction.

DR. MARK: Isn't it so that what you might come somewhere close to in a finite time, a year, is a set of criteria which could be accepted within the industry or agency and which will help you somewhat to put a better pBW4

finger on what the actual total risk may be for those terms which you will have considered. This does not give you anywhere close to social acceptability, but at least provides input which you will need for that, anyway. I wanted to ask separately.

It is my impression, for one thing, one thing which does damage to society is the number of curies released. I think it is true that the number of curies one expects to be released are more for things other than core melt, than for all of the core melts which — or WASH-1400 would forecase. And yet, your remarks didn't bring out determinations or statements about that term in the picture, which will certainly have to be in hand before you go before the public.

DR. VESELY: We are going to address that question as part of this task. That is not coming from WASH-1400.

DR. MARK: Operating releases give a lot of curies. Core melts give more but they don't happen every day.

DR. VESELY: We are planning to use a study on nuclear versus coal which are identifying these curies that are coming from the rest of the —

DR. MARK: They have to be in your hands with some decent descriptions before you go before the public.

DR. VESELY: It is not coming from WASH-1400. You are right. This program will look at these other contributors of background risk. We are proposing this as an approach. We certainly welcome inputs and critiques and suggestions of other avenues, of ways to go.

Also, it may be appropriate now to open it to the public, asking for their comments. I think we want something focused. We want specific criteria proposed that would be acceptable or unacceptable to help focus and which are implementable and which will help to improve the public health and safety no matter how incomplete. If they are done well and if they are reasonable.

Again, I would like to keep the option that after we propose these criteria, that after they are reviewed by the public and peer review that they are deemed unacceptable or unworkable or the industry will not accept them — and that is something not to be considered in this program. We are coming up with straw men criteria which the groups here are feeling unreasonable implementable — are a step in how safe is safe enough, that we want to address. And if we have the experts addressing the bigger question of public acceptability, which we feel will come in the much longer term.

DR. OKRENT: Let me try a couple of questions. Somewhere back in the discussion — I don't remember when — you made a comment that in more recent looks at the

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comparison between nuclear and coal or other possible energy technologies, there was some new factor that people applied to the risks from nuclear which made it much bigger.

Would you tell me what it was you meant?

DR. VESELY: It was essentially — there wasn't any one factor. There was criticism on the uncertainties and the method of handling those factors used in the Inhaber study and by changing some of the assumed health effects, one can get —

DR. OKRENT: Which health effects? I am not sure what you are referring to.

DR. MARK: The correlation between health effects and dose from low-level radiation and the National Academy has put out numbers on this and people are questioning those.

DR. VESELY: A lot of the uncertainty comes from, also, the rest of the fuel cycle, not the reactor per se, but mining, for example.

PROF. KERR: I think you were asking if somebody — if there was some sort of general agreement on the proposed revisions, aren't you, Dave?

DR. VESELY: If that is the question, I don't think there is agreement. The study here is to do a clearly detailed sensitivity study on the impacts of changing various data and parameters as the various critics —

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PROF. KERR: Is it the feeling that you can come up with a fueling that the critics won't question?

DR. VESELY: No. But it will give us a better understanding of the dominant --

PROF. KERR: You will use a different approach than the one Inhaber used?

DR. VESELY: No, the approach will be the same.

PROF. KERR: Different data?

DR. VESELY: Different data.

PROF. KERR: Why didn't he use that data? He didn't know about it?

DR. VESELY: There is more recent data. His was to try to get a best estimate. We are looking more at sensitivities and trying to get some handle on the uncertainties which that report did not pretend --

PROF. KERR: You might get the best estimate number that he got, but you will attribute uncertainties.

DR. PLESSET: They will have an evaluation model.

DR. OKRENT: Which part of the fuel cycle or operating cycle do you expect to find something radically different than he had for nuclear? I am still not sure from your answer what you were saying.

DR. VESELY: I guess I don't expect to find anything different. If you account for uncertainties and if you took that upper bound, the 95 percent upper bound, you

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might find that to be larger than the risk from coal.

DR. OKRENT: If you take 95 percent confidence figures, again, would you expect the important contribution to arise from the reactor or from the way you store tailings or what?

DR. VESELY: I'm not sure. If I would guess, I would say it is not from the reactor in the way risks are computed because it is man-days lost and the large fatalities and the extra importance that the public places on the reactor and the chance of having large accidents are not considered in this risk analysis.

It is curies, man-days, lost, no attempt to incorporate the psychologically perceived risk associated with the reactor and not necessarily associated with his other activities.

DR. OKRENT: You are saying it might be larger because of psychological risk effects?

DR. VESELY: Perceived risk.

DR. OKRENT: Is it thought that the uncertainties for other modes of electricity generation are smaller than the ones in the nuclear?

DR. VESELY: We do not have these particular problems. It is the risk aversion, or fear the public has —

DR. OKRENT: Let me leave out the risk aversion

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part at the moment since I don't know how to quantify that. If I don't have risk aversion in —

PROF. KERR: That is somebody who stays home from work because he is scared. You can quantify that.

(Laughter.)

DR. OKRENT: You can, but I am unable to at the moment. For the comparison, if we leave out questions of psychological effects, is it thought that the uncertainties from an absolute magnitude basis, not from a percentage basis, are substantially larger for nuclear than for the other ways of making electricity that he analyzed?

DR. VESELY: I can't answer that right now. Some of the earlier studies done by SAI indicated yes, that there were larger uncertainties associated with nuclear than with the other — with coal. But again —

PROF. KERR: Larger uncertainties in the health effects?

DR. VESELY: Final risk.

DR. OKRENT: How will they quantify the CO2 effect for coal in this?

DR. VESELY: That is something they have gone back and re-evaluated. And it may change the result in which coal now has larger uncertainties. That program, the results are slated to come out in January.

DR. OKRENT: So you have a draft you are looking

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at now?

DR. VESELY: Yes. And we have had them go back and look at some of the coal coaling ri ks and coal uncertainties.

DR. OKRENT: Are they going to try to do them all at 90 percent confidence?

DR. VESELY: They are trying to do some of it at 90 percent, but mainly the study is to set up a sensitivity, if you will, a computer program that will allow us to vary factors in combinations or to do propagations. If you want some idea of an error spread on the final results by inputting various uncertainties and spreads on the data. They are also coming up with some best estimates on data to be used, as well as some estimates on uncertainties.

DR. OKRENT: I would suggest that if they are going to do one system at 90 percent confidence they should do them all.

DR. VESELY: Yes.

DR. OKRENT: Or they should try, and if they can't they should say why they can't.

DR. VESELY: In a classical sense, as Professor Saunders noted, if you have a 90 percent confidence bound on a complex function of variables, in a classical sense it is very difficult. It is easy to do Monte Carlo but you are not getting a confidence bound.

PROF. KERR: All of these systems are complicated.

DR. VESELY: But to try to get a 90 percent bound on the final result is difficult.

PROF. KERR: I thought you were going to use 90 percent on some systems for producing energy, but not on others.

DR. VESELY: On some parts of that system, some parts of the data or some levels of calculations. But probably we are not going to be able to calculate or propagate those confidence levels through the evaluations to get a final 90 percent confidence on the risk, on the probability consequence or man-days lost. That was difficult.

We have parts of the problem, parts of the model, and have to make estimates and sensitivities on the rest of it.

DR. OKRENT: I think it is difficult. I have little doubt, in fact I am not sure I would consider it possible except with very large numbers for your risk numbers.

DR. VESELY: In the Bayesian sense, one can do that. That is also being done.

DR. SAUNDERS: It doesn't mean a damn thing but you can do it.

DR. PLESSET: That is what I was beginning to

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expect.

DR. SAUNDERS: That is a personal bias.

DR. VESELY: This is one of the problems in implementing the criteria, that when you incorporate uncertainties, do you do them in the classical sense or in a Bayesian sense. If you do them in a Bayesian sense you have to be careful, because the priorities you set up may not have any meaning at all. You may be very sensitive to prior assumptions. These kinds of questions, I think, are very important in establishing criteria or doing risk analyses and attempting to implement.

DR. OKRENT: My feeling is I start one step back, at least, in this comparative study. I am concerned that there are things equivalent to the health going to ice cream that Mr. Rowsome mentioned before, that haven't even been identified for a lot of the energy systems.

DR. VESELY: This study is not attempting to do that.

DR. OKRENT: I think they may, in fact, be significant. They may, in fact, be dominant, for all we know, if we haven't even asked ourselves what they are.

DR. VESELY: The study is now to put in the uncertainties of the Inhaber work and not to enlarge or extend the modeling. It was a rather small task with a very limited goal. I agree with you. I do believe, though, that

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that study will be useful to us in this interim criteria. We will look, for example, at the other risk coming from the rest of the fuel cycle, the curies associated with non-reactors.

DR. OKRENT: Could I ask another question?

With regard to your possible unacceptability criterion, which you have said loosely is WASH-1400.

DR. VESELY: Or some extension of modification.

DR. OKRENT: I would like to understand a little bit more what you mean, and also how you might incorporate some things if you try to apply it.

Do I understand correctly that at least one possible approach, in your mind, is that for some other reactor than the one that was studied, you would do a detailed assessment on all of the paths to core melt that you thought were important and sum up the total contribution and the pass/fail question would be, is the probability smaller or larger than whatever it was you got in WASH-1400, 5X10 -5 or something like that?

DR. VESELY: That is certainly one approach. We are investigating that. Another one is to investigate and to analyze only certain sequences, five or six that have been identified in WASH-1400 as being important risk contributors and only doing those sequences and four or five — involving four or five significant systems and not attempting to be complete at this first stage, and then gradually expanding the sequences and systems to be considered to attempt to do a complete analysis.

We are not proposing that industry plans to do a complete risk analysis. I don't think that is feasible, to do another WASH-1400, particularly on the existing plants. But we can pick up some of the key sequences and some of the key systems and do analyses there and obtain probabilities and compare the probabilities with those in WASH-1400, whether it be core melt -- and if they are above it, incorporating the uncertainties. And they would be judged to be unacceptable unless some modification were made in procedures or design.

DR. OKRENT: This would be using the data in WASH-1400 and the methods in WASH-1400 and taking a point estimate?

DR. VESELY: That is something to be evaluated. The goal would be to use consistent data base and to use in WASH-1400 the most available data, which means going back and reevaluating WASH-1400 with the most recent data, if there are significant differences in failure rates that have been determined from the NPRDS and not necessarily using the point value in WASH-1400 but other options at 90 percent of the bound which you then have a higher confidence and incorporate the uncertainties and define the uncertainties and the error spreads that have to be associated with data to be used in these models that the plants and industry are to apply to attempt to satisfy or -- this criteria.

DR. OKRENT: If I understood what you are saying -- and I will try to take it one step at a time -- if one changed the data from what was used in WASH-1400, you would go back and recalculate the important sequences in WASH-1400 and you would get a new nonacceptability criterion which might be smaller or larger?

DR. VESELY: Yes.

DR. OKRENT: And after you had done this, you would then look at other plants against this new criterion.

MR. SULLIVAN:

DR. VESELY: Using that data.

DR. OKRENT: Using that data. And a limited set of scenarios. And then you seem to think that it would be more meaningful to compare the 90 percent confidence numbers than your median, I guess --

DR. VESELY: That is my personal feeling, and I think we ought to investigate that, because the upper 90 percent in some way of calculating some express formula or guidelines for calculating that certainly incorporate the uncertainties.

The point value, with very large uncertainties, the same two point values, one having very large uncertainties and the other having very small uncertainties, certainly can't be judged to be the same. And some way of incorporating the uncertainty -- the problem with WASH-1400 --

PROF. KERR: The uncertainty is going to exist in different places in different plants?

DR. VESELY: Yes. I think, depending on the design configuration and the event sequences, the systems involved, particularly the design configurations.

DR. OKRENT: You might have one point that is better than WASH-1400 on six of the seven sequences and worse on one of them, let's say, the check valve. Median only has one check valve, and it got by the staff or whatever, but let me -- one check valve, of course, would fail, but something of this sort, so that the sum of these was the same as WASH-1400 on whatever data base you were

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using, but there were differences, as there inevitably will be, among the scenarios you compare.

Now, would you weight some scenarios as being more important than others because they automatically leaded to higher consequences?

DR. VESELY: Yes, I think so. That is being done even now. When we compare — and there have been decisions made where it is not just the probability of core melt, but for each relief category, which gauge you will measure the consequences of relief.

DR. OKRENT: If you are doing that, you might as well go to relief category instead of the probability of getting to release category than to core melt, if you are going to do that weighting.

DR. VESELY: Yes, if you are going to do that. I would say that that is Saul Levine's preference, to go the probability versus release category.

DR. OKRENT: You yourself said that there are some plants that might have flooding as an important contributor. I have to assume there are other plants that have some other feature, seismic, even ---

(Laughter.)

— As an important contributor.

DR. VESELY: That's possible.

(Laughter.)

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DR. OKRENT: I'll bet.

(Laughter.)

DR. OKRENT: With confidence. But these aren't on the list of important contributors in WASH-1400. Now, what would that all mean for your comparison?

DR. VESELY: I don't know if they have to be contributors as events to be considered in attempting to satisfy these criteria. The question is whether floods or seismic should be considered by the plants, in what kind of models and approaches should be used in attempting to quantify them. I don't know. I think that is one of the questions that has to be addressed.

Also, common-cause failures which have been incorporated in WASH-1400, and of course, there are questions on methodology and approaches and techniques used. I think one can put criteria on specified models and contributions to be considered, contributions such as excluding the floods in seismic, in treating them separately in a different —

PROF. KERR: You have to be careful because now you find yourself in the impossible situation of working back to the old review process but adding on additional set of criteria rather than substituting quantitative criteria for semiqualitative ones.

DR. VESELY: Yes. That is very important to

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watch. We can get ourselves into that problem.

But I also think that one can add contributions to be considered, such as floods, in a quantitative manner and identify approaches and data to be used in modeling and quantifying these techniques. And we can bring in further factors and further contributions to be considered, perhaps udating WASH-1400 and modifying our criteria as we identify available approaches and data to be used in quantifying.

If we were right now to, say, incorporate floods and seismic into your risk analyses in some way and not explicitly state the approaches or data to be used and we would leave it up to this review group, the NRC or whoever it be, to determine the acceptability of the data and models to be used without giving any specific criteria or guidelines, we would be back now to a very subjective approach leaving it to the individuals.

I don't know if that is acceptable. I don't know if you get any better results. If you simply state a standard and allow the industry or allow the users to come up with whatever approach they see reasonable and then have this all-knowing review group determine the acceptability of that data. It is very difficult. The seismic is difficult to model and quantify.

DR. OKRENT: What I seem to be getting toward myself — and I use the seismic and flood as examples

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because I know they are difficult and there certainly are at the present time considerable uncertainties — it seems to me we would be heading toward having to add lots of other scenarios. And, in fact, for some reactors, there will be different scenarios than the ones that were dominant. And different PWRs would have different scenarios. You wouldn't want to ignore them, but how do you know whether you should ignore them or not?

It seems to me that you have to look for them; and after you know they are small enough, you can ignore them. I find myself sort of driven toward doing some kind of a full WASH-1400 kind of analysis in order to know what I don't need to be concerned about. That is why I don't see, at the moment, that taking a limited number of scenarios and saying the numbers that we get for reactor X for these scenarios, either one by one or the sum of them, should be smaller in their probability of core melt or in their probability of release, whichever it is.

PROF. KERR: You are making a great argument for standard plans.

DR. OKRENT: Then the Surry reactor, I don't see that as a meaningful strawman criteria because it is not a sufficiently complete set.

DR. VESELY: That may be valid. As part of this integrated reliability program, we are trying to define

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scenarios and event sequences to be considered in the different plants which we feel in some way will incorporate a large portion of the risk. It is going to depend on how successful we are there.

So, I think the inputs from that program and our attempts to identify sequences that are applicable to all the diffferent plants will have a direct bearing on just that question. I have fear, though, of doing a complete WSAH-1400, of the Commission being swamped by all this work and the industry spending a lot of time and effort without any meaningful — having to do it simply for completeness and finding out that 90 percent of the effort was wasted when perhaps you could have done it in a sequential manner, step-by-step manner, and done it much more efficiently.

But those are the kind of questions to be addressed. I think that this program will certainly focus on those specific kinds of questions. Can you identify sequences? Again, there is an argument that even if you can't identify all of them for completeness it is better to identify and take steps in finding some of those sore thumbs and looking at them at least on past experience and then expanding it into other sequences. Whether you go in one big step or several steps, I don't know.

DR. OKRENT: You understand I am certainly not against trying to look at plants to find their weak points, and I am not now advocating that we should take a WASH-1400 type of nonacceptance using a full WASH-1400 analysis. I was just trying to look at what it was you were proposing, to see whether it seemed to me it could get to a point where even the staff could use it in a — in some kind of quantitative risk acceptance way.

And what I guess I am saying and what I do think is a partial selection of scenarios for its trouble — and it wasn't too long ago that the authors of the reactor safety study were, I think, still thinking that they probably included enough important scenarios that it was unlikely that the probability of core melt would be substantially changed by any additional scenario. And yet, today, you said that for the B&W reactors there might have been a different scenario.

DR. VESELY: I think it is the same scenario.

DR. OKRENT: It is a different number, and I have little doubt that there are other specific reactors for which there is a single scenario different than those identified in WASH-1400, where you would get a contribution larger than all of those in WASH-1400 put together.

DR. VESELY: That's right. And I would like to leave the project open to look at both, what are the benefits and disadvantages of doing a complete analysis versus a selective scenario, and our ability to identify

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scenarios to narrow scenarios, to get a more tractable problem.

It is certainly more tractable. Whether you miss too much, it is another question, but I would like to identify, examine both approaches, and the ramifications, implications for manpower and workability.

But, again, we are talking about this, and I think we — this budget, our goal is trying to focus on some of these very specific questions that have to be addressed that we feel have to be addressed in talking about trying to implement a criteria, whether it be acceptable or unacceptability.

And, again, my concern — and I have talked with Saul and Frank — I think one of our concerns in WASH-1400 is that that may be too conservative. In fact, it probably is too conservative, that you may have even some higher criteria. WASH-1400, when you compare it to other activities, is certainly a very low risk. It may not be workable. It may be too stringent.

That is our approach. That is our proposal, and we are planning, with your input, to institute this program, if anything, to provide input to you for the — for your use to the Udall letter.

DR. SAUNDERS: I would like to be able to quote the first of your statement, that if the WASH-1400 study had been made at Three Mile Island, it could have prevented the accident. And at the same time, you finished off by saying the criteria, WASH-1400, is too stringent and has to be relaxed —

DR. VESELY: I think if you had used less stringent criteria, I think you would have caught Three Mile Island. It stands up so much as a sore thumb. I think you would have found Three Mile Island.

Again, I think Milt's comment, "hingsight is always 20-20," I certainly agree, but it is one way of checking our criteria, that if you had had this crite ia and if you had assumed that you had reasonable analysis with which you had identified this, that — then, yes. Would we have done that, or would we have the manpower to really have done that sequence, that is a whole other issue.

But certainly, we would have had — there are a lot of assumptions. Perhaps WASH-1400 can be used as a criteria, but it is something that we have, something very tangible. The licensing staff is essentially using the WASH-1400 criteria as an ad hoc criterion in many of its comparisons, such as aux feed systems. You asked for the basis of the recommendations on the aux feed systems that we came up with, and the criterion that we used, that was WASH-1400.

PROF. KERR: I am puzzled by your statement that

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WASH-1400 is too stringent to be used, when I thought earlier that the numbers that came out of it were calculated from an existing reactor. How can one get criteria that are too stringent if one calculates it from a reactor that has been in operation for several years?

DR. VESELY: Several reasons: One, we believe that it is one of our better designs; two, WASH-1400 did not include all contributions such as floods, seismic, common-cause.

PROF. KERR: Presumably --

DR. VESELY: If we included those in our other reactors, we should include those in WASH-1400.

PROF. KERR: Then you would get a number which is typical of operating reactors.

DR. VESELY: Typical of that design.

PROF. KERR: I don't see why that is unattainable or even too stringent. What is bad about --

DR. VESELY: If you have one reactor which is above that and the rest of it being below that criteria, how much that one reactor affects the overall risk as opposed to these others --

PROF. KERR: It may be more than you want to pay for, and it is certainly not unattainable.

DR. VESELY: All right.

PROF. KERR: I can't believe it is practically

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unattainable if somebody built it and operated it.

DR. VESELY: You have to realize that, of course, our older plants were not built to the same criteria and standards as our newer plants. The question is whether we should have those plants satisfy the same criteria that we do on existing plants.

PROF. KERR: I thought you said that maybe they were so good that they couldn't be attained.

DR. VESELY: I am saying maybe the criteria is too strict, that many of our older plants, many of our plants would not be accepted -- I am sorry.

PROF. KERR: I misunderstood your comment.

DR. VESELY: That was my fault.

DR. OKRENT: Could we -- unless you are tired of standing and you can talk sitting down, if you prefer --

PROF. KERR: What about the people who are sitting who are tired of sitting? I would suggest a break until about 8:30 o'clock in the morning.

(Laughter.)

DR. OKRENT: That is a possibility.

PROF. KERR: That is only because it is 9:00 o'clock by my clock. I am willing to start at 7:30 o'clock in the morning.

DR. OKRENT: Could you take 15 minutes more and then we will break?

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PROF. KERR: Sure.

DR. OKRENT: Let's try 15 minutes more, and we will break early, then.

I guess I am trying to understand what you propose for this risk criteria program. Currently, do you envisage it as focused toward trying to look at whether WASH-1400 can serve as an unacceptability criterion, or is it more broadly based?

DR. VESELY: It is more broadly based. That is one topic to be specifically investigated. Another topic is the criteria proposed by Kinchin. I would have Brookhaven specifically assemble these other criteria that have been proposed. Our main channel, our main means of examining these criteria would be through this national task force that IEEE will serve to set up. It would be much broader than IEE, not only to include IEEE membership, and that will specifically, after six months, to come up with criteria, using — looking at other proposed criteria, WASH-1400, and then to start looking at implications and ramifications of these criteria.

After six months, we could come up with four or five different criteria, WASH-1400 just being one of them. And then, for the remainder, looking at the implications and the ramifications on attainability, method of satisfaction, comparisons to other risks, we see that WASH-1400 — I

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personally see that WASH-1400 would be the principal criterion that would be examined or modified — the results of WASH-1400, and certainly others, as the rest of the fuel cycle would certainly be examined.

PROF. KERR: Bill, this may seem like a facetious question, and I apologize if it does. The Commission is on record, in the eyes of many people, as having disavowed the WASH-1400 study. I don't think it did, but a lot of people think it did.

How is one going to reverse that general feeling and now have this used as an important criterion to determine reactor safety?

DR. VESELY: The criticism of WASH-1400 was generally one it underestimated the risk of core melt, and, too, it underestimated the uncertainties. I don't think that those criticisms in the public's view of that necessarily has any correlation or relation with using WASH-1400 as a criteria.

PROF. KERR: I am saying that certainly one of the considerations of the criterion that you are going to have to take into consideration is that it must have some credibility in the eyes of the public. The Nuclear Regulatory Commission has a responsibility for protecting the public, and if 99.9 percent of the public thinks it is being defrauded, this can't go on.

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There are a lot of people who think, because of newscasts and newspapers, that the Commission disavowed WASH-1400, and if you are going to start using it, I think that you have to give some rather serious consideration to what the Commission must do to correct, modify, rectify, WASH-1400 or to correct the impression that the public has. I really think that this might be a fairly serious problem.

DR. MARK: I think it is partly at least semantic, because you are already proposing not to take the number out of 1400 but to develop on some stated basis estimates of that sort. That is no longer WASH-1400; it is a new reworking of that.

DR. PLESSET: It is reborn.

DR. SAUNDERS: WASH-1500.

(Laughter.)

DR. MARK: You are not going to take the numbers; you are going to use the approach and the method and correct some — you are not going to take this rate of 10-8 or 10-9.

DR. VESELY: I won't do that. I have learned.

(Laughter.)

DR. VESELY: I hear that at every ACRS meeting, by the way.

(Laughter.)

DR. OKRENT: Not from me. This IEEE task force, they are supposed to come up with something six months from now or six months from when they get started?

DR. VESELY: They are scheduled to start October 1.

DR. OKRENT: And they don't know they are supposed to come up with WASH-1400, or they do?

DR. VESELY: They know that they will -- they don't know -- I mean --

(Laughter.)

DR. VESELY: I don't understand the question. The goal is not to come up with WASH-1400; it is to use WASH-1400 as a basis, as one of the bases.

DR. OKRENT: They are not restricted to that?

DR. VESELY: Not at all.

DR. OKRENT: What the Brookhaven people do, are they restricted, or are they supposed to focus on testing out the WASH-1400?

DR. VESELY: Again, we see this as a broader task. The reason for the $300,000 funding, we do not see this as a narrow task, simply to look at WASH-1400. If we were to do that, that could be done in a third of the effort.

PROF. KERR: I don't quite understand what the IEEE group is doing.

DR. OKRENT: Why don't you find out. That's why we're here.

BWH

PROF. KERR: Could you tell me again? It is late at night, and I am probably not thinking.

DR. VESELY: I see the IEEE group as formulating the criteria, the straw man criteria, the interim criteria, whether acceptability or unacceptability, as coming out of this task force.

PROF. KERR: Do they have ground rules? Did you tell them, "Don't forget that WASH-1400 exists"?

DR. VESELY: Yes. And we are working up those ground rules now.

PROF. KERR: Are you going to tell them what criteria to come up with?

DR. VESELY: No, of course not. But we do want industry —

PROF. KERR: What kind of guidance are you giving them?

DR. VESELY: Short-term guidance, we will give them a specific criteria to be examined among the others that they wish to examine. One is 1400. And time constraints that we have: one year. Looking at — these people —

PROF. KERR: Suppose they come up with a result which says we can't do this in six months, we need another $300,000 to carry out a more extensive research project.

DR. VESELY: That is not acceptable.

BWH

PROF. KERR: Maybe $150,000?

DR. VESELY: No. I think we will come up -- no. I think that we will work with these people; I think that we will come up with a criteria. The goal is to come up with criteria for further review, for public considerations, and it could be an unacceptability criteria. It could be that we only at this time the probability versus release category -- and that may be all that that committee feels is reasonable or that is implementable, and that may be the result.

We are not putting a constraint on that group. There are 40 people divided up into separate working groups. We have gotten a very enthusiastic response.

PROF. KERR: They all have to come up with the same result?

DR. VESELY: No.

PROF. KERR: We may get 10 criteria.

DR. VESELY: There will be 10 criteria that will then be considered in this final review, and either one criteria proposed or, yes, 10 criteria will be formulated with the pros and cons, to be further reviewed, considered by ACRS, yes.

I would like to keep it open at this time. This is something -- you have to realize this is something that ACRS, that you people contacted us about two weeks ago. So we are just in the formulation stages, and we think your input is very important.

DR. MARK: You have spoken a couple of times that as if at the end of the year's project, with some criteria examined, one might go with this before the public. I was hoping that I hadn't heard that correctly. You won't then be in any shape -- you might be in fine shape for a peer review.

DR. VESELY: I'm sorry. That's what I meant.

DR. MARK: Fine.

DR. VESELY: I would like to get as much review and

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to discuss with other agencies, EPA, their views and their experiences in attempting to apply the numerical criteria. I don't think those agencies are going to be very useful at formulating. I see them more as critiquing and reviewing something that the task force has proposed, rather than these agencies actually formulating. And I don't see the experts on decision theory or psychological aspects actually formulating. I see them examining implications of any criteria.

DR. MARK: One implication of your suggestion that maybe WASH-1400 would be too stringent, by which you mean the reassessed probabilities for those plants may seem excessively --

DR. VESELY: That is a better word, better phrasing.

DR. MARK: And then you are going before the public, hopefully a year or two later, and say, we don't think any longer that we should build plants as safe as we know how, and that is not going to fly.

DR. VESELY: That is something to be considered. I think we should build the plants as safe as we know how. Whether we apply that same criteria to existing plants that are now out there --

DR. MARK: The ones like Duane Arnold or something.

DR. VESELY: I think that criteria ought to be applied to anything new. The question is what criteria should be applied to existing plants.

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DR. MARK: Some of the existing ones, some of the existing ones may be better, some would be worse.

DR. VESELY: That is a question I would be interested in.

MR. LEVINE: General Electric Company.

Bill, I guess one question that comes to my mind is, there is a lot of interest outside your organization in the industry toward looking at revising what present-day criteria are. Would your group be amenable to looking at criteria that are suggested by other sources, other than those that you are directly working with?

DR. VESELY: Certainly, as long as those suggestions are focusing on this problem of criteria, of setting up these quantitative criteria, yes. In fact, we would like it. There has been a lot of experience in the fast reactor area where standards have been investigated and proposed by the industry and ANS. I think that interaction is very important, or else we are not going to make this criteria fly.

MR. LEVINE: We would be very interested in knowing your schedule.

DR. VESELY: All right.

DR. OKRENT: Could you tell me a little bit, in the remaining minute or two, what it is you think Brookhaven National Laboratories will coordinate with regard to information collection tasks one and two? Is that scoped?

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DR. VESELY: Comparison of risk from other activities, natural and manmade.

DR. OKRENT: There is some body of such information that already exists at Brookhaven. Are they going to go beyond --

DR. VESELY: That is simply a collection of the information, review of literature, a literature review.

DR. OKRENT: Is there any activity in this list that will assess what other federal agencies are doing or have done?

DR. VESELY: That is -- there is a task that Brookhaven will do for us. And also, through MITRE we will be looking to work with the other agencies and see what their inputs and viewpoints are. We propose going either through Brookhaven directly or through MITRE. We see MITRE as the potential -- we have done some preliminary discussions with MITRE and see them as the potential for setting up this peer review outside the nuclear agency.

DR. OKRENT: Under Item 1, it says risks from other activities and phenomena, and I was wondering whether you had some planned effort to find out, if this was thought to be from activities that fall under the jurisdiction of other federal regulatory groups like EPA?

DR. VESELY: That's right.

DR. OKRENT: FDA?

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DR. VESELY: There are two tasks under this title. One is that to contact the other agencies and gather the information on criteria that either are being used or being thought about, ways of using quantitative risk analysis; and two, gathering all of these calculations of risks individuals are exposed to, to attempt to get the background risk or complete the picture of other risks that people are exposed to. That was described, the variety of activities, in WASH-1400. So we see that now as two tasks: going to the other agencies and getting their inputs and viewpoints, and gathering all of the information on proposed criteria, evaluation of risk from other activities.

And we see also Brookhaven assisting us in determining the system and plant implications of any proposed criteria. Also, we see--by the way, Brookhaven serves a convenient mechanism for us to coordinate and interact with other groups. Industry, I think, is in this. I think industry's input is important. I think they've thought about this problem to a great extent and I think we need their input if we want to make this thing workable.

I do see the IEEE task force as formulating that criteria, with interaction from industry and other groups, and having their inputs.

PROF. KERR: It is a tough enough problem that it seems to me wisdom available anywhere --

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DR. VESELY: I don't see, for example, us having the option of going to RFP, a request for proposal. That would take us a year to get out a request and to evaluate them. I think we have to go through national laboratories with the time, with the kind of time scale that we have set up for this problem.

DR. OKRENT: Have you considered whether it makes sense to see if there is some not-for-profit institution or whatever that would try to come at the question of what constitutes acceptable risk from a societal viewpoint?

DR. VESELY: Again --

DR. OKRENT: I thought this document we were going to see is things that come up to the point of how do you try to define acceptable risk. But in the report in January I -- at the moment, I don't expect to see trial definitions.

DR. VESELY: I don't see -- that's right. We have identified -- MITRE is one nonprofit agency. We can work directly through the Air Force as an inter-agency agreement.

PROF. KERR: There are social research organizations at various places.

DR. VESELY: Yes. This is one of the reasons our contractor is Perceptronics and Paul Slovic. We see this report as identifying areas, specific areas to be pursued and to be addressed and to be funded, which would not have direct bearing on these proposed criteria. So --

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DR. OKRENT: What I have in mind is, in a sense, in the Ford-MITRE report on nuclear power, issues and choices, something like that. That group gave some qualitative, semi-quantitative judgments on acceptable risk. I didn't know whether --

DR. VESELY: That is being incorporated. That study is certainly one of those being considered in this report.

DR. OKRENT: I didn't know whether there are thought to be groups already in existence that might be willing to take on the task of defining what constitutes criteria for acceptable risk for nuclear reactors and other technologies, so that you see what they have in mind on a broader perspective and why; not come in from the point of view of the industry, but coming in from the societal viewpoint.

DR. VESELY: We are planning to continue funding of Paul Slovic, for example, and some of his people and these experts, which will work with the IEEE people and us.

DR. OKRENT: That is good and it is important. I think if the engineers don't talk to the social scientists, they will be missing certain things.

But I was looking about something different. In any event, I have a message from my right here that it is past the quitting hour that I had previously set. So let me ask the Subcommittee members to think over their evening meal --

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PROF. KERR: I am ready to write down my homework assignment.

(Laughter.)

DR. OKRENT: -- about how we should organize what the ACRS tries to do, because tomorrow we will start out trying to talk about that. In other words, what is it the ACRS should try to do itself or with its own meetings, or however, and what are the things important for us to either try to do or to identify and get Vesely to spend money on, or whatever it is. Okay.

So just so you don't feel like you are a laggard, breaking off at so early, 6:25.

With that, we will recess the meeting and reconvene at 8:30.

(Whereupon, at 6:26 p.m., the meeting was recessed, to reconvene at 8:30 a.m. on Wednesday, September 12, 1979.)

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T-1-3
CR6837

LER EVALUATION PROGRAM

COORDINATED BY NRR-PAS

TECHNICAL SUPPORT PROVIDED BY
RELIABILITY AND STATISTICS BRANCH
EG&G IDAHO, INC.

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CONTENTS

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LER EVALUATION PROGRAM

I. INTENTION OF PROGRAM

ANALYZE:

A. LICENSEE EVENT REPORTS (LERs)

B. NUCLEAR PLANT RELIABILITY DATA SYSTEM (NPRDS)

C. COMMON CAUSE EVENTS.

II. LICENSEE EVENT REPORTS (LERs)

A. SYNOPSIS OF LER ANALYSIS

1. CATEGORIZE LERs BY

   - COMPONENT
   - DATE OF FAILURE
   - FAILURE MODE AND CAUSE
   - SYSTEM AFFECTED
   - COMMON CAUSE AND/OR RECURRING (WHERE APPLICABLE)
   - ETC

2. CALCULATE COMPONENT FAILURE RATES FOR

   - PLANTS
   - NSSS
   - PWR/BWR
   - OVERALL

3. WRITE AND SUBMIT REPORT FOR EACH COMPONENT TO

   - NRC FOR REVIEW AND COMMENT
   - TECHNICAL EDITING FOR FINAL DRAFT
   - NRC FOR DISTRIBUTION

B. Pumps

1. Data breakdown (refer to Pump Coding Sheet)

   No attempt made to classify by size, capacity, type, etc. All analyses and calculations performed for generic class - Pumps.

2. Status of analysis

   In final stages of converting report into a NUREG. Tentative issue date - October 1979.

3. Remarks

   · Operating failure rates (λ_0):

Running pumps	- 2E-6/hr
Alternating pumps	- 1E-5/hr
WASH 1400	- 3E-5/hr

   · Demand failure rates (Q_D):

Alternating pumps	- 4E-4/d
Standby pumps	- 3E-3/d
WASH 1400	- 1E-3/d

C. Control rod drive mechanisms

1. Data breakdown (refer to Control Rod Drive Coding Sheet)

   All analyses and calculations performed for the generic class - Control Rod Drive Assembly.

2. Status of analysis

   In process of incorporating comments into final draft. Tentative issue date - October 1979.

3. Remarks

·Fail to insert (LERs)	- 5E-5/d
·Fail to insert (WASH 1400)	- 1E-4/d

D. Diesel Generators

1. Data breakdown (refer to Diesel Generator Coding Sheet)

   All analyses and calculations performed for the generic class - Diesel Generators (complete plant).

2. Status of analysis

   Draft report being reviewed by NRC. Tentative issue date - November 1979.

3. Remarks

   • Operating failure rate (λ_0):

     LERs - 3E-2/hr

     WASH 1400 - 3E-3/hr

E. Valves

   Major changes in report may be necessary. Draft is in review by NRC. Tentative issue date - December 1979.

F. Penetrations

   LERs have been coded. Some preliminary sorting complete. Tentative issue date - 1980.

G. FY 80 goals

1. Continue LER categorization

2. Issue NUREGs for components analyzed

   • Diesels

   • Valves

   • Penetrations

   • Instrumentation & Control

   • Relays

   • Circuit closers/interrupters

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3. CONDUCT LER FLAGGING ANALYSIS

- TIME TRENDS
- ANOMALOUS FAILURE RATES
- RECURRING FAILURES
- COMMON CAUSE FAILURES
- QUALITY CONTROL RELATED FAILURES
- HUMAN ERRORS (SYSTEMATIC & RANDOM)
- ANY OTHER SIGNIFICANT OBSERVATIONS

III. NPRDS

A. GOALS

1. IDENTIFY AND CHARACTERIZE FACTORS WHICH CAUSE SIGNIFICANT VARIATIONS IN THE FAILURE RATE FOR A GIVEN CLASS OF COMPONENTS
2. TABULATE FAILURE RATE ESTIMATES FOR GIVEN SETS OF FACTOR VALUES WITHIN A CLASS OF COMPONENTS
3. DEFINE FAILURE RATE SPREADS FOR GIVEN SETS OF FACTOR VALUES WITHIN A CLASS OF COMPONENTS.

NECESSARY COMPUTER PROGRAMS AND ANALYSIS APPROACHES WILL BE ASSEMBLED TO ACCOMPLISH THESE GOALS.

B. STRATEGY FOR EXPLORATORY ANALYSIS

ANALYSIS DIVIDED INTO SIX AREAS THAT FOLLOW ONE ANOTHER IN TIME SEQUENCE.

1. DATA CLASSIFICATION
   - PLANTS (MAY BE LIMITED TO THOSE WITH MORE NEARLY COMPLETE DATA)
   - SIZE
   - DEMAND FAILURES (STARTUP AND TESTING)
   - FAILURES DURING NORMAL OPERATION

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- Total Number of Failures
- Service Environment (steam, water, air)
- Temporal Proximity (for common cause analysis)
- Calendar Quarters
- Time in Service
- Status at Time of Failure (active or standby)
- NSSS Vendor
- Safety Class (class 1, 1E, and 2)
- Component Manufacturer

2. Graphical Representation
   - Time Trend (computer code COUNTESS or other tools)
   - Semilog Plots of Failure Rate versus Factor Levels
   - Plots of Population Fraction Failing
   - Histograms
3. Comparison with Failure Rates Used in WASH-1400 and those from LERs
4. Investigation of Anomalies
   - Verification of Data through Southwest Research Institute
   - Alert NRC about Apparent Problems such as:
     a. Time Trends
     b. Failure Rates Differing from the Average and from those used in WASH-1400
     c. Serious Generic Implications
     d. Recurring Failures
     e. Common Cause Failures

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F. Quality Control

G. Human Errors (systematic and random)

H. Other High Risk Contributors

5. Statistical Analysis

·Tolerance Intervals on the Various Data Hierarchies

·Bayesian Estimation Techniques for Tolerance Intervals

·Verification of Order-of-magnitude Differences as Needed (statistical method to be determined)

6. Mathematical Modeling

·Relationships between Failure Rate and Factor Levels by:

A. Curve Fitting

B. Factor Effects Models

C. FY 79 Results

·Plotting Data/Failure Rate Estimates

D. FY 80 Goals

1. Analyze ECCS Valves

2. Analyze ECCS Pumps

IV. Common Cause Analysis

A. Goals

1. Develop estimation techniques for the model.

2. Identify subsystem of components susceptible to particular common cause failure mechanisms.

3. Analyze each subsystem using Binomial Failure Rate Model.

4. Diagnostic checks whether data satisfies assumptions of model.

5. Put subsystems and mechanisms together to get overall common cause failure rates. Monte Carlo simulation will be necessary.

6. If diagnostic checks suggest more complicated model (Beta-Binomial Failure Rate), Monte Carlo simulation will be necessary.

B. FY 79 Results

1. Identification of components largely done.

2. Theory on model complete. Software for model 80% complete.

3. Theory for diagnostic checks complete. Software has to be developed.

C. FY 80 Goals

1. Issue TREE/NUREG on Binomial Failure Rate Model - End of 1979.

2. Complete Modeling.

3. Issue report on Beta-Binomial Failure rate model.

4. Begin analyzing LER data.

PUMP CODING SHEET

FAILURE MODE CODES

CODE	DESCRIPTION
A	LEAKAGE/RUPTURE
B	DOES NOT START
C	LOSS OF FUNCTION
D	DOES NOT CONTINUE TO RUN
E	MISC (DESIGN/TECH. SPEC. PROBLEMS)

TYPE OF FAILURE CODES

CODE	DESCRIPTION
B	BOTH RECURRING AND COMMON CAUSE
C	COMMON CAUSE FAILURES
R	RECURRING FAILURES
S	COMMAND FAULT
T	RECURRING COMMAND FAULTS

FAILURE MECHANISM CODES

CODE	DESCRIPTION
00	UNKNOWN
01	BEARING FAILURE
02	PERSONNEL ERROR
03	SEAL/PACKING FAILURE
04	DESIGN ERROR
05	LOOSE FASTENERS/COUPLING
06	SHAFT/COUPLING FAILURE
07	BLOWN FUSE
08	BINDING/BOUND/SEIZED
09	QUALITY CONTROL
11	MISALIGNMENT
12	FOREIGN MATERIAL CONTAMINATION
13	BELLOWS RUPTURE
14	PROCEDURAL DISCREPANCY
15	FABRICATION ERROR
16	SHAFT KEY FAILURE
17	BROKEN DIAPHRAGM
18	NORMAL WEAR
19	FAILED FASTENERS/WELDS
20	IMPROPER CLEARANCES
21	FILTER/STRAINER PLUGGED
22	FAILED INTERNALS
23	CORROSION/EROSION
24	THERMAL/OVERSPEED/OVERLOAD TRIP
25	AIR/VAPOR BOUND
26	BELT DRIVE BROKEN
27	MOTOR FAILURE
28	SWITCH FAILURE
29	DAMAGED SEAL SURFACE
30	EXCESSIVE VIBRATION
31	HIGH PRESSURE
32	BRAKE FAILURE
33	CLUTCH FAILURE
34	CRACKED CASING
36	LEAKY FITTINGS
37	FAILED MECHANICAL CONTROLS
38	CONTROL CIRCUIT FAILURES

FAILURE CLASSIFICATION CODES

CODE	DESCRIPTION
D	DEMAND
T	TIME
U	UNKNOWN
N	NOT APPLICABLE

OBSERVATION TIME CODES

CODE	DESCRIPTION
D	ON DEMAND
M	DURING MAINTENANCE
N	DURING NORMAL PLANT OPERATION/SURVEILLANCE
R	DURING RECORDS REVIEW
T	DURING TESTING
U	UNKNOWN

NSSS VENDOR CODES

CODE	DESCRIPTION
B	BABCOCK & WILCOX
C	COMBUSTION ENGINEERING
G	GENERAL ELECTRIC
W	WESTINGHOUSE

POOR ORIGINAL

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PUMP CODING SHEET (CONTD.)

SAFETY-RELATED SYSTEM IDENTIFICATION CODES

CODE	PWR
A	-
B	- AUXILIARY FEED
C	- CONTAINMENT ISOLATION (INCL PENETRATIONS)
D	-
E	- ELECTRIC POWER
F	- CONTAINMENT SPRAY INJECTION
G	- CHEMICAL VOLUME CONTROL - BORIC ACID XFER
H	- HIGH PRESSURE COOLANT INJECTION
I	- PRIMARY AND SECONDARY SAFETY/RELIEF VALVES
J	- COMPONENT COOLING WATER
K	- REACTOR COOLANT
L	- LOW PRESSURE COOLANT INJECTION (RHR)
M	- REACTOR PROTECTION (CONTROL RODS)
N	- NONSAFETY-RELATED
P	- SYSTEM UNKNOWN/NOT APPLICABLE
Q	-
R	-
S	- SERVICE WATER
T	-
U	-
V	-
W	-
X	- REACTOR PROTECTION (PPS)
Y	- CONTAINMENT AIR SAMPLING
Z	- FAILED FUEL ELEMENT DETECTION

CODE	BWR
A	- AUTOMATIC DEPRESSURIZATION
B	-
C	- CONTAINMENT ISOLATION (INCL PENETRATIONS)
D	- LOW PRESSURE CORE SPRAY
E	- ELECTRIC POWER
F	- CONTAINMENT SPRAY INJECTION
G	- STANDBY LIQUID CONTROL (BORON)
H	- HIGH PRESSURE COOLANT INJECTION
I	- SAFETY/RELIEF VALVES
J	- COMPONENT COOLING WATER
K	- REACTOR COOLANT RECIRC
L	- LOW PRESSURE COOLANT INJECTION (RHR)
M	- CONTROL ROD DRIVE HYDRAULIC (SCRAM)
N	- NONSAFETY-RELATED
P	- SYSTEM UNKNOWN/NOT APPLICABLE
Q	- REACTOR CORE ISOLATION COOLING
R	-
S	- SERVICE WATER
T	- STANDBY GAS TREATMENT (NONSAFETY)
U	- CONDENSATE AND FEED
V	-
W	- MAIN STEAM
X	- REACTOR PROTECTION (PPS)
Y	- CONTAINMENT AIR SAMPLING (NONSAFETY)
Z	- FAILED FUEL ELEMENT DETECTION (NONSAFETY)

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CONTROL ROD DRIVE CODING SHEET

FAILURE MODE CODES

CODE	DESCRIPTION
A	-
B	FAILURE TO INSERT DURING NORMAL S/D
C	FAILURE TO INSERT FULLY DURING SCRAM
D	FAILURE TO INSERT TO 90% DURING SCRAM
E	ROD FAILS TO MOVE DURING POWER CHANGES/ TESTING
F	ROD FAILS TO WITHDRAW FROM FULLY INSERTED POSITION
G	DROPPED ROD (PWR)
H	UNCOUPLED ROD/OVERTRAVEL CONDITION (BWR)
I	IMPROPER ROD MOVEMENT
J	EXTERNAL LEAKAGE/RUPTURE
K	DOES NOT OPERATE PROPERLY (SPECIFIC MODE NOT IDENTIFIABLE)
L	MAINTENANCE/REPLACEMENT REQUIRED
T	TECHNICAL SPECIFICATION VIOLATION (NON-FAILURES)

COMPONENT CODE

CODE	DESCRIPTION
CD	CONTROL ROD DRIVE ASSEMBLY

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TYPE OF FAILURE CODES

CODE	DESCRIPTION
B	BOTH RECURRING AND COMMON CAUSE
C	COMMON CAUSE FAILURES
R	RECURRING FAILURES
S	COMMAND FAULT
T	RECURRING COMMAND FAULTS

FAILURE MECHANISM CODES

CODE	DESCRIPTION
00	UNKNOWN
01	PERSONNEL (OPERATIONS)
02	PERSONNEL (MAINTENANCE)
03	PERSONNEL (TESTING)
04	DESIGN ERROR
05	FAB./CONSTRUCTION/Q.C.
06	PROCEDURAL DISCREPANCIES
07	NORMAL WEAR
08	EXCESSIVE WEAR
09	CORROSION
10	FOREIGN MATERIAL CONTAMINATION
11	EXCESSIVE VIBRATION
12	CRDM MOTOR FAILURE
13	SEAL FAILURE
14	FAILED/MISALIGNED INTERNALS
15	CLUTCH FAILURE
16	BRAKE FAILURE
17	BEARING FAILURE
18	FILTER/STRAINER PLUGGED
19	BINDING/SEIZURE
20	FAILURE/FAULT OF COMPONENT SUPPLY SYSTEM
21	CONTROL CIRCUIT FAILURE/PROBLEM
22	FASTENER FAILURE/PROBLEM
23	WELD FAILURE
24	LUBRICATION PROBLEM

SYSTEM CODE

CODE	DESCRIPTION
	PWR
M	REACTIVITY CONTROL SYSTEM
	BWR
M	REACTIVITY CONTROL SYSTEM

FAILURE CLASSIFICATION CODES

CODE	DESCRIPTION
D	DEMAND
T	TIME
U	UNKNOWN
N	NOT APPLICABLE

OBSERVATION TIME CODES

CODE	DESCRIPTION
D	ON DEMAND
M	DURING MAINTENANCE
N	DURING NORMAL PLANT OPERATION/SURVEILLANCE
R	DURING RECORDS REVIEW
T	DURING TESTING
U	UNKNOWN

NSSS VENDOR CODES

CODE	DESCRIPTION
B	BABCOCK & WILCOX
C	COMBUSTION ENGINEERING
G	GENERAL ELECTRIC
W	WESTINGHOUSE

POOR ORIGINAL

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DIESEL GENERATOR CODING SHEET

SYSTEM CODES

CODE	DESCRIPTION
A	EMERGENCY DIESEL GENERATOR SYS. & CONTROLS
B	FIRE PROTECTION SYS. & CONTROLS
C	AUXILIARY FEED

FAILURE MODE CODES

CODE	DESCRIPTION
A	DOES NOT START
B	DOES NOT CONTINUE TO RUN
C	DOES NOT MEET T.S. RATINGS
D	DOES NOT MEET RATED SPEED/LOAD
E	LOSS OF SPEED/LOAD CONTROL
F	MISC (UNAVAILABILITY, DESIGN, T.S. PROBLEMS

FAILURE CLASSIFICATION CODES

CODE	DESCRIPTION
D	DEMAND
T	TIME
U	UNKNOWN
N	NOT APPLICABLE

SUB-SYSTEM CODES

CODE	DESCRIPTION
A	NOT APPLICABLE/NOT SPECIFIED
B	FUEL/LUBE OIL SUPPLY/INJECTION
C	ELECTRICAL SYSTEM (CONTROL)
D	STARTING SYSTEM
E	DIESEL GENERATOR COOLING SYSTEM
F	SCAVENGING AIR SYSTEM
G	ENGINE FRAME/INTERNALS
H	MECHANICAL CONTROLS
I	DIESEL EXHAUST SYSTEM
J	SHUTDOWN AIR SYSTEM

FAILURE MECHANISM CODES

CODE	DESCRIPTION
00	UNKNOWN
01	PERSONNEL OPERATION
02	PERSONNEL MAINTENANCE
03	PERSONNEL TESTING
04	DESIGN ERROR
05	FAB/CONSTRUCTION/QC
06	PROCEDURAL DISCREPANCY
07	DEFECTIVE FUEL INJECTOR(S)
08	CORROSION/EROSION
09	FOREIGN MATERIAL CONTAMNATION
10	OVERSPEED/OVERLOAD/TRIP
11	MECHANICAL/ELECTRICAL CONTROL FAILURE
12	BEARING FAILURE
13	HI/LOW AMBIENT TEMPERATURE
14	LUBE/FUEL/WATER/AIR LEAKAGE
15	EXCESSIVE VIBRATION
16	TURBOCHARGER FAILURE
17	OUT OF ADJUSTMENT/CALIBRATION

OBSERVATION TIME CODES

CODE	DESCRIPTION
D	ON DEMAND
M	DURING MAINTENANCE
N	DURING NORMAL PLANT OPERATION/SURVEILLANCE
R	DURING RECORDS REVIEW
T	DURING TESTING
U	UNKNOWN

11

TYPE OF FAILURE CODES

CODE	DESCRIPTION
B	BOTH RECURRING AND COMMON CAUSE
C	COMMON CAUSE FAILURES
R	RECURRING FAILURES
S	COMMAND FAULT
T	RECURRING COMMAND FAULTS

NSSS VENDOR CODES

CODE	DESCRIPTION
B	BABCOCK & WILCOX
C	COMBUSTION ENGINEERING
G	GENERAL ELECTRIC
W	WESTINGHOUSE

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CR 6837
T-5,6

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PAS SYSTEMS ENGINEERING SECTION

PRESENTATION TO ACRS

SEPTEMBER 11, 1979

I. ESTIMATE OF DAVIS BESSE AND RANCHO SECO EVENT PROBABILITIES

II. STATUS OF RESEARCH PROGRAMS

CONTACT:
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301-492-8377

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EVALUATION OF DAVIS BESSE AND RANCHO SECO

FEEDWATER TRANSIENTS ON 9/24/77 AND 3/20/78

USING WASH-1400 DATA

PROBABILITY ESTIMATES OF DAVIS BESSE AND RANCHO SECO EVENTS BASED ON WASH-1400

<u>DAVIS BESSE</u> (9/24/77)

- o LOSS OF MAIN FEEDWATER = 3/YR.
- o PRESSURIZER RELIEF VALVE FAILS TO RECLOSE = $1x10^{-2}$/DEMAND
- o PROBABILITY OF SEQUENCE CLASS WHICH INCLUDES DAVIS BESSE EVENT ~ $3x10^{-2}$/REACTOR-YR.

9504 316

PROBABILITY ESTIMATES OF

DAVIS BESSE AND RANCHO SECO EVENTS BASED ON WASH-1400

RANCHO SECO (3/20/78)

- WASH-1400 DID NOT QUANTIFY INDIVIDUAL FAILURE MODES OF THE MAIN FEEDWATER SYSTEM
- IN THE EVENT, NO MAJOR SAFETY SYSTEMS REQUIRED TO PREVENT CORE MELT FAILED TO PERFORM FUNCTION
- RANCHO SECO EVENT IS INCLUDED (ALTHOUGH A SMALL FRACTION) IN THE ANTICIPATED THREE FEEDWATER TRANSIENTS PER REACTOR-YEAR

TABLE 1

COMPARISON OF THREE B&W REACTOR INCIDENT EVENT SEQUENCES

	TMI-2 (3/29/79)	DAVIS BESSE (9/24/77)	RANCHO SECO (3/20/78)
REACTOR POWER	97%	9%	70%
REACTOR HISTORY	IN COMMERCIAL OPERATION THREE MONTHS.	~1 FULL POWER DAY OF OPERATION.	IN COMMERCIAL OPERATION 3 1/2 YEARS.
TURBINE	TRIPPED IMMEDIATELY.	DOWN ALREADY.	TRIPPED AFTER 5 SEC.
REACTOR TRIP	AUTOMATIC AFTER 8 SEC. ON HI REACTOR PRESSURE (2355 PSI).	MANUAL (1 MIN. 47 SEC.) BECAUSE OF RISING PRESSURIZER LEVEL.	AUTOMATIC AFTER 5 SEC. ON HI REACTOR PRESSURE
MFW	BOTH PUMPS TRIP IMMEDIATELY.	1 PUMP TRIP IMMEDIATELY 1 PUMP TRIP 58 SEC. LATER.	REDUCED TO ZERO FLOW BY FAULTY ICS SIGNALS (SOME MFW INITIATION BY OPERATOR AFTER 7 MIN.).

TABLE 1 (CONT.)

	TMI-2 (3/29/79)	DAVIS BESSE (9/24/77)	RANCHO SECO (3/20/78)
AFW	NO AFW FOR 8 MIN.	1 PUMP/SG WORKING WITHIN 46 SEC. 1 PUMP "UNAVAILABLE" (TURBINE DEGRADED). AVAILABLE MANUALLY AFTER 12 MIN.	NO AFW FOR 7 MIN.
PRESSURIZER RELIEF VALVE	OPENED AFTER 3 SEC. AND STUCK OPEN. BLOCK VALVE CLOSED AFTER 138 MIN.	OPENED AFTER 1 MIN. 6 SEC., CYCLED RAPIDLY 9 TIMES IN 23 SEC. AND STUCK OPEN (STEM GALLING). BLOCK VALVE CLOSED IN 20 MIN.	PRV OUT OF SERVICE. SAFETY VALVE OPENED AND RECLOSED.
PRESSURIZER	SEVERELY MISLEADING LEVEL INDICATION.	LEVEL INCREASED OFF SCALE.	NO LEVEL PROBLEM.

TABLE 1 (CONT.)

	TMI-2 (3/29/79)	DAVIS BESSE (9/24/77)	RANCHO SECO (3/20/73)
ECCS	HPI AUTOSTARTED (1600 PSI) AT 2'02 SEC. 1 PUMP TRIPPED AFTER RUNNING 2 MIN. 36 SEC. OTHER PUMP THROTTLED TO MINIMUM FLOW.	HPI AUTOSTARTED (1600 PSI) AT 2 MIN. 57 SEC. AND PERMITTED TO RUN FOR 3 MIN. 5 SEC. MANUAL SHUTDOWN BECAUSE PRESSURIZER LEVEL NORMAL.	HPI MANUAL AND INTERMITTENT DURING FIRST 13 MIN. THEN AUTOSTART (1600 PSI).
INSTRUMENTS	MOST O.K.	O.K.	ONLY PRESSURIZER LEVEL AND RCS PRESSURE TRUSTED BY OPERATORS DURING FIRST 75 MIN.

TABLE 2

WASH-1400 FAILURE PROBABILITIES

	FAILURE PROBABILITY
1. MAIN FEEDWATER (TM)	3/YR
2. REACTOR TRIP (K)	3.6×10^{-5}/DEMAND
3. AUXILIARY FEEDWATER (L)	3.7×10^{-5}/D
4. PRESSURIZER RELIEF VALVE OPENS (P_1)*	1×10^{-2}/D
5. SAFETY VALVES OPEN (P_2)	3×10^{-5}/D
6. PRESSURIZER RELIEF VALVE CLOSES (Q_1)	1×10^{-2}/D
7. SAFETY VALVES CLOSE (Q_2)	1×10^{-2}/D
8. ECCS - HI PRESSURE INJECTION (C)	3.7×10^{-3}/D
9. ECCS DEGRADED OPERATION (C^1)	$>3.7\times10^{-3}$/D

*NOT EXPLICITLY DOCUMENTED IN WASH-1400.

Loss of Feedwater Event Tree - Babcock & Wilcox Design

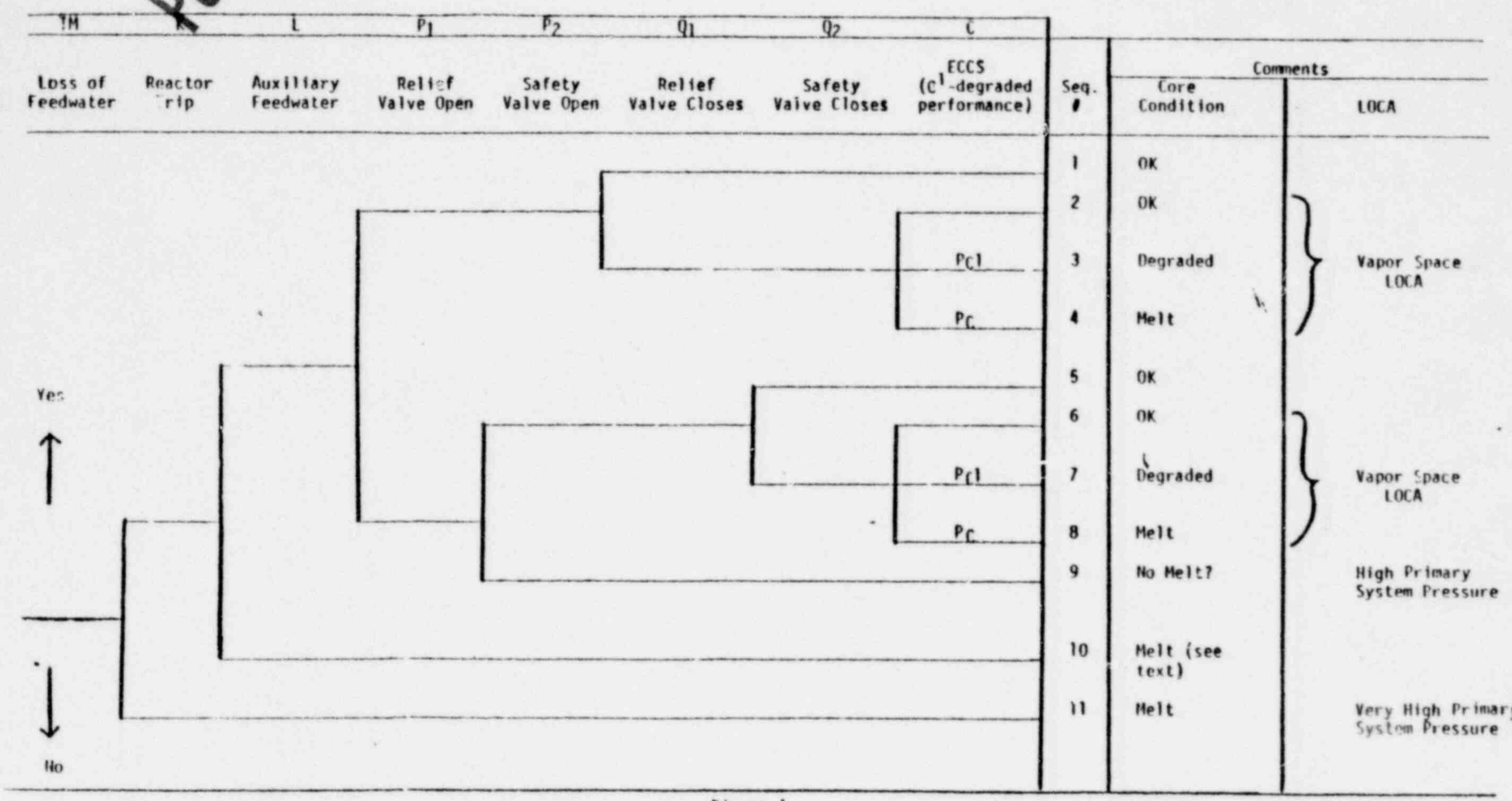

Figure 1

STATUS OF PAS RESEARCH PROGRAMS
IN SYSTEMS ENGINEERING SECTION

ACCIDENT SEQUENCE PRECURSORS

OBJECTIVE: REVIEW LER'S TO IDENTIFY POTENTIAL ACCIDENT PRECURSOR SEQUENCES

FY 30: o DEVELOP CRITERIA

o COMPLETE INITIAL SCREENING OF LER'S

o BEGIN IN-DEPTH SCREENING AND ANALYSIS

STATUS: INITIAL SCREENING INITIATED

LWR SYSTEMS SURVEY

OBJECTIVE: TO PROVIDE A SIMPLE SURVEY OF PLANT DESCRIPTIVE INFORMATION ON EXISTING LWR'S. INFORMATION IS LIMITED TO ITEMS WHICH ARE ESPECIALLY IMPORTANT TO SAFETY RELIABILITY AND RISK ANALYSIS.

STATUS:

- o SYSTEM DESIGN INFORMATION RELEVANT TO FIVE HIGHEST RISK SEQUENCES IN WASH-1400 BEING COLLECTED.
- o 14 OPERATING PLANTS TO BE SURVEYED BY END OF FY 79.
- o ALL 70 OPERATING PLANTS AND NEW OR'S TO BE SURVEYED BY END OF FY 30.

LWR SYSTEMS SURVEY

PROGRAM USES AND BENEFITS:

- o GENERIC STUDIES/ALTERNATIVES
- o EXCEPTIONS IN DESIGN-OUTLIERS
- o COMPARISONS WITH RSS PLANTS
- o AID DETAILED PROBABILISTIC SAFETY ANALYSES
- o TRACKING SAFETY IMPROVEMENTS
- o DAY-TO-DAY ANALYTICAL TOOL

VALUE-IMPACT ASSESSMENT OF
REGULATORY REVIEW UNITS

OBJECTIVES

ASSESS SRP AND GENERIC (STD.) TECH SPECS TO DETERMINE THEIR RELATIVE VALUE FROM RISK STANDPOINT AND THEIR RESOURCE IMPACTS

STATUS

- ABOUT 30% COMPLETE (INITIATED ~9/78)
- INTERIM (PRELIMINARY) REPORT #1 SUBMITTED LATE AUGUST ON SRP FOR BWR
- INTERIM REPORT #2 ON PWR/SRP EXPECTED 12/79
- FINAL REPORT ~8/80
- RESULTS EXPECTED TO ASSIST NRR MANAGEMENT IN:
  - RESOURCE ALLOCATIONS AND OPTIMIZING
  - ELIMINATIONS OF LOW VALUE REVIEW EFFORTS

9904 327

REACTOR SAFETY STUDY
METHODS APPLICATION PROGRAM

OBJECTIVE:

FOR A REPRESENTATIVE SPECTRUM OF LWR DESIGNS, IDENTIFY ACCIDENT SEQUENCES THAT ARE DOMINANT RISK CONTRIBUTORS.

STATUS:

PWR #1	LARGELY COMPLETE
PWR #2	COMPLETE 1/80
PWR #3	COMPLETE MID CY 80
BWR	COMPLETE END FY 80

994 328

RSSMAP APPROACH

- USE WASH-1400 TO HELP IDENTIFY DOMINANT SEQUENCES IN OTHER REACTORS
- ASSESS RISK RELATIVE TO WASH-1400 PLANTS
- USE POINT VALUES, NO UNCERTAINTIES
- RELEASE CATEGORIES SAME AS WASH-1400
- USE WASH-1400 DATA BASE

RESIDENT INSPECTION OPERATION REVIEW

OBJECTIVE: IDENTIFY THOSE RISK-RELATED PLANT OPERATING CONDITIONS MOST WORTHY OF SCRUTINY BY A RESIDENT INSPECTOR. ASSESS EASE OF INSPECTION.

STATUS: TO BEGIN IN FY 80.

TASKS IN FY 80:
- o IDENTIFY AND RANK PLANT OPERATING LIMITS (RISK)
- o IDENTIFY AND ASSESS INSPECTION PROCEDURES (FEASIBILITY)
- o PERFORM VALUE-IMPACT ANALYSIS

994 329

994 330

HAZARDS TO NUCLEAR POWER PLANTS FROM NEARBY TRANSPORTATION ACCIDENTS

OBJECTIVES:
- TO DEVELOP A METHODOLOGY FOR EVALUATING SITE HAZARDS RESULTING FROM NEARBY TRANSPORTATION ACCIDENTS
- TO AID IN ESTABLISHING SITING CRITERIA (RESPONSIVE TO REQUEST FOR RESEARCH NRR 76-17)

TASKS COMPLETED:
- SCOPING STUDY ON PROGRAM ELEMENTS COMPLETED
- COLLECTION OF HAZARDOUS MATERIALS DATA COMPLETED

TASKS IN FY 80:
- ASSESSMENT OF REACTOR VULNERABILITIES
- HAZARDOUS ENVIRONMENTS MODEL
- TRANSPORTATION ACCIDENT MODEL

994 331

LIQUID PATHWAYS

STATUS: MODELING COMPLETE, PROGRAM BEING DEBUGGED

SCHEDULE:
- o FINAL RESULTS ANTICIPATED BY OCTOBER 31
- o REPORT COMPLETED BY JANUARY 31, 1980

SAFETY-RELATED DC POWER SUPPLIES

OBJECTIVE: TO PROVIDE A RELIABILITY-BASED EVALUATION OF CURRENT LICENSING DESIGN CRITERIA FOR DC POWER SYSTEMS AT NUCLEAR POWER PLANTS. TO ASSESS THE PROBABILITY THAT FAILURES IN DC POWER SYSTEMS WILL RESULT IN A LOSS OF DECAY HEAT REMOVAL CAPABILITY.

STATUS:

- o ANALYSIS OF MINIMUM DC POWER SYSTEM AND DECAY HEAT REMOVAL FAILURE TO BE COMPLETED BY END OF OCTOBER 1979.
- o SECOND PHASE OF PROGRAM TO EVALUATE REPRESENTATIVE PWR AND BWR CONFIGURATIONS AND ANALYZE SENSITIVITY OF MINIMUM SYSTEM RELIABILITY TO DESIGN IMPROVEMENTS. TO BE COMPLETED IN FY 80.

SEVERE CORE DAMAGE ACCIDENTS

OBJECTIVE: TO IDENTIFY AND CHARACTERIZE A RANGE OF ACCIDENT SEQUENCES WHICH MAY RESULT IN SEVERE CORE DAMAGE RATHER THAN MELT. THESE ACCIDENT SEQUENCES WOULD INCLUDE TMI TYPE ACCIDENTS OF PERHAPS INTERMEDIATE PUBLIC RISK BUT HAVING A HIGHER PROBABILITY THAN CORE MELT ACCIDENTS.

STATUS: CURRENTLY DEVELOPING SCOPE.

STATION BLACKOUT

OBJECTIVE: TO DETERMINE WHETHER CHANGES IN LICENSING CRITERIA ARE NEEDED TO PROTECT NUCLEAR PLANTS AGAINST A STATION BLACKOUT (LOSS OF OFFSITE AND ONSITE AC POWER).

STATUS: CURRENTLY DEVELOPING SCOPE OF WORK.

994 334

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PAS RESEARCH PROGRAM

PRESENTATION TO THE ACRS SUBCOMMITTEE ON RELIABILITY AND PROBABILISTIC ASSESSMENT

SEPTEMBER 11, 1979

FRANK H. ROWSOME, ACTING DIRECTOR, PAS

OUTLINE

I. INTEGRATED RELIABILITY EVALUATION PROGRAM

II. REASSESSMENT OF PRIORITIES AND FOCUS - MANAGEMENT PERSPECTIVE

III. STATUS OF IMPROVED REACTOR SAFETY PROGRAM

IV. REASSESSMENT OF PRIORITIES AND FOCUS - TECHNICAL PERSPECTIVE - ACRS RECOMMENDATIONS

INTEGRATED RELIABILITY EVALUATION PROGRAM

PHASE 1 - SURVEY (COMPLETE IN FY 80)

- DEVELOP DATA BANK COVERING DESIGN AND PROCEDURES WITH WHICH TO ASSESS SUSCEPTIBILITY OF ALL OPERATING LWR'S TO FIVE DOMINANT SEQUENCES IN WASH-1400

PHASE 2 - IREP--PROPER (COMPLETE IN FY 81)

- DEVELOP PLANT-SPECIFIC CORE DAMAGE/MELT EVENT TREES FOR ALL OPERATING LWR'S
- DEVELOP TOP LEVEL FAULT TREES (ACTIVE COMPONENTS ONLY) FOR KEY SYSTEMS (ALL OPERATING LWR'S)
  - o STANDARDIZED, COMPUTERIZED COMPONENT CATALOG
  - o FLEXIBLE, EXPANDABLE MODULAR TREE STRUCTURE TO ACCOMMODATE WIDE VARIETY OF SUBSEQUENT APPLICATIONS OR EXTENSIONS

PHASE 3 - EXTENSION TO LWR'S UNDER CONSTRUCTION

PHASE 4 - APPLICATIONS

PHASE 5 - INCLUDE CONTAINMENT FAILURE EVENT TREES

OBJECTIVES OF IREP

1. IDENTIFY OUTLIERS -

   CORE MELT SCENARIOS MORE PROBABLE THAN WASH-1400

2. PROVIDE FOUNDATION FOR WIDE RANGE OF PLANT-SPECIFIC RELIABILITY STUDIES

3. PROVIDE FRAMEWORK FOR LINE OFFICE PARTICIPATION

   - NRR COLLABORATION IN EVENT TREE/FAULT TREE DEVELOPMENT
   - PROVIDE LIBRARY OF RELIABILITY MODELS FOR USE IN BOTH RES AND LINE OFFICES
   - PIONEER STANDARDIZED/AUTOMATED DATA* COLLECTION AS PROTOTYPE FOR ALTERED SAR REQUIREMENTS

*(DESIGN AND PROCEDURAL DATA)

PROJECTS INVOLVING EVENT TREES AND/OR FAULT TREES WHICH CAN BE FACILITATED BY IREP

THE FOLLOWING IS A LIST OF PROJECTS ONGOING OR PLANNED WHICH UTILIZE EVENT TREES AND/OR SYSTEM LOGIC MODELS IN THEIR ANALYSES.

1. METHODOLOGY APPLICATIONS PROGRAM (PAS)
2. FIRE RISK MODELING PROGRAM (PAS)
3. FLOOD ANALYSIS PROGRAM (PAS)
4. PROGRAM TO ANALYZE TEST, MAINTENANCE AND ACCIDENT RESPONSE PROCEDURES (PAS)
5. SEISMIC SAFETY MARGINS PROGRAM (RSR)
6. PROGRAM TO ANALYZE LER DATA IMPLICATIONS (PAS)
7. OPERATIONS EVALUATION PROGRAM
8. ACCIDENT PRECURSOR ANALYSIS (PAS)
9. RISK EVALUATIONS OF INSPECTION MODULES (I&E)

PROJECTS INVOLVING EVENT TREES AND/OR FAULT TREES WHICH CAN BE FACILITATED BY IREP (CONT.)

994 339

10. RELIABILITY OF ECCS (NRR)

11. AUXILIARY SYSTEMS ANALYSIS (NRR)

12. IMPROVEMENTS TO SINGLE FAILURE CRITERION

13. RELIABILITY ANALYSIS OF OPERATING SYSTEMS (NRR)

14. LIMITING CONDITIONS FOR OPERATION (PAS)

15. VALUE IMPACT ASSESSMENT OF REGULATORY REVIEW UNITS (PAS)

16. RISK RELATED RESIDENT INSPECTION OPERATION REVIEW (PAS)

17. SYSTEMS INTERACTION PROGRAM (NRR)

18. STATION BLACKOUT ANALYSIS (PAS)

ROLE OF PAS

I. DIRECT SUPPORT OF LINE OFFICES - SOME RECENT EXAMPLES:

- AFWS RELIABILITY STUDY
- HELP NRR SPECIFY STUDIES REQUIRED OF LICENSEES
- IMPROVEMENTS TO SINGLE FAILURE CRITERION
- ASSIST EMERGENCY PLANNING
- REVIEW DRAFT SITING POLICY
- OPERATIONS EVALUATION GROUP

II. APPLICATIONS OF PROBABILISTIC SAFETY ANALYSIS

- IMPROVED REACTOR SAFETY
- RSS METHODOLOGY APPLICATIONS PROGRAM
- STATION BLACKOUT (TAP A-44)
- DC POWER ISSUE (TAP A-30)
- RISK RANKING OF NRR CONCERNS
- ACCIDENT PRECURSOR ANALYSES

III. ADVANCES IN THE STATE-OF-THE-ART IN PROBABILISTIC SAFETY ANALYSES

- COMMON CAUSE FAILURES
- HUMAN RELIABILITY MODELS
- FAILURE DATA ANALYSES
- CORE DAMAGE EVENT SCENARIOS
- WASTE REPOSITORY PSA
- LIQUID PATHWAYS

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PROSPECTIVE GROWTH IN PROBABILISTIC SAFETY ANALYSIS IN THE NRC REQUIRES:

1. ACCELERATED TRAINING AND ADOPTION OF PROBABILISTIC SAFETY ANALYSIS IN LINE OFFICES
2. IMPROVEMENTS IN PAS PRODUCTIVITY
3. MAXIMUM MANAGEABLE GROWTH RATE FOR PAS
4. IMPROVEMENTS IN CONTRACTOR PRODUCTIVITY
5. ENLARGED ROLE OF RSR, SAFER IN RISK-RELATED RESEARCH
6. EXPANDED USE OF RELIABILITY STUDIES REQUIRED OF LICENSEES OR APPLICANTS
7. POSSIBLE REORGANIZATION OF NRC, RES, AND/OR PAS

ACCELERATED TRAINING AND ADOPTION
OF PROBABILISTIC SAFETY ANALYSIS IN LINE OFFICES

I. PAS INITIATIVES

A. TRAINING COURSES

- EXECUTIVE SEMINAR
- SYSTEM RELIABILITY ANALYSIS COURSE
- INITIATIVE TO RE-EXAMINE, OVERHAUL AND EXPAND SRA COURSE
- NEW COURSE(S) IN ACCIDENT TOPOLOGY PREDICTION (SCENARIOS, EVENT TREES)

B. NRR WILL PARTICIPATE IN THE INTEGRATED RELIABILITY EVALUATION PROGRAM

II. NRR INITIATIVES

A. BULLETINS & ORDERS TASK FORCE

- AFWS STUDY
- BWR ESFAS STUDY

B. LESSONS LEARNED TASK FORCE

- IMPROVEMENTS IN SINGLE FAILURE CRITERION
- SYSTEM RELIABILITY CRITERIA
- CREDIT FOR NON-SAFETY SYSTEMS

IMPROVEMENTS IN PAS PRODUCTIVITY

1. DISCIPLINED TIME UTILIZATION

   - BALANCE FIRE DRILLS, CONTRACT MANAGEMENT, LONG AND SHORT RANGE RESEARCH TASKS

2. APPROACH RESEARCH TASKS THROUGH ITERATIVE REFINEMENT

   A. SHRINK UNCERTAINTIES IN QUANTITATIVE STUDIES

      STEP ONE: QUICK AND DIRTY ANALYSIS WITH CAREFUL UNCERTAINTY TREATMENT

      STEP TWO,...N: FOCUS EFFORTS TO REDUCE DOMINANT UNCERTAINTIES

   B. UTILIZE DISCIPLINE OF REPORT-WRITING ON CONCEPTUAL/ VERBAL TASKS

      STEP ONE: PREPARE A DRAFT OF THE "FINAL" REPORT

      STEP TWO: REVIEW THE DRAFT FOR DOMINANT WEAKNESSES-- PERFORM RESEARCH, REWRITE REPORT AND REPEAT AS NECESSARY

IMPROVEMENTS IN PAS PRODUCTIVITY (CONT.)

3. DEVELOP GUIDELINES AND TRAINING IN CONTRACTING AND CONTRACT MANAGEMENT FOR PAS

4. SEVERELY LIMIT THE NUMBER OF TASKS PAS TAKES ON--COMBINE RELATED TASKS AS MUCH AS POSSIBLE

5. EMPHASIZE PRODUCTION OF USEABLE OUTPUT

   - PUBLICATIONS

   - MODELS/TOOLS/TECHNIQUES

6. DEVELOP COLLABORATIVE EFFORTS WITH OTHER RES DIVISIONS AND LINE OFFICES

IMPROVEMENTS IN CONTRACTOR PRODUCTIVITY

1. ENLARGE THE POOL OF CONTRACTORS

   - PRESSURE TO USE COMPETITIVE BIDDING IS ENCOURAGING PAS TO GO TO PRIVATE FIRMS AS WELL AS GOVERNMENT LABS

2. STUDIES IN PROGRESS

   - CRITICAL PATH FOR CONTRACT COMMITMENT

   - CRITICAL FACTORS IN CONTRACT MANAGEMENT

3. IMPROVED TASK DESCRIPTION, SCHEDULE, REVIEW, AND OUTPUT SPECIFICATION

4. DEVELOP TRAINING PROGRAM AND GUIDELINES FOR PAS CONTRACT MANAGERS

ENLARGED ROLE OF RSR AND SAFER IN
RISK-RELATED WORK

1. COORDINATED HUMAN FACTORS RESEARCH

2. COORDINATED CODE DEVELOPMENT AND EXPERIMENTAL PROGRAMS IN TRANSIENT AND SMALL LOCA ACCIDENTS

3. COORDINATED RESEARCH ON FUEL DAMAGE/CORE MELT PHENOMENOLOGY

4. COORDINATED RESEARCH ON WASTE ISOLATION

SPECIFICATION OF RELIABILITY STUDIES TO BE REQUIRED OF LICENSEES/APPLICANTS

- o LINE-OFFICE AUTHORITY
- o PAS REVIEW, GUIDANCE, OR COLLABORATION

EXAMPLES:

1. AFWS RELIABIITY STUDY (DESIGN AND PROCEDURAL DATA ONLY)
2. FAILURE MODES AND EFFECTS ANALYSIS OF B&W INTEGRATED CONTROL SYSTEM
3. SMALL LOCA, TRANSIENT, AND INADEQUATE CORE COOLING ANALYSES
4. BWR ECCS ACTUATION AND CONTROL STUDIES

SPECIFICATION OF RELIABILITY STUDIES TO BE REQUIRED OF LICENSEES/APPLICANTS (CONT.)

POSSIBLE APPLICATIONS:

1. ASPECTS OF STATION BLACKOUT SUSCEPTABILITY ANALYSIS

2. FAILURE MODES AND EFFECTS ANALYSIS OR FAULT HAZARDS ANALYSIS FOR:

   A. CONTROL AND INSTRUMENTATION

   B. AUXILIARY SYSTEMS SUCH AS INSTRUMENT AIR, SERVICE WATER, AND DC POWER

SPECIFICATION OF RELIABILITY STUDIES TO BE REQUIRED OF LICENSEES/APPLICANTS (CONT.)

TYPES OF STUDIES:

1. FAILURE MODES AND EFFECTS ANALYSES
2. LOGICAL SIMULATION
3. FAULT HAZARDS ANALYSIS
4. SCENARIO ANALYSES AND QUALITATIVE COMMON MODE FAILURE ANALYSES
5. PROBABILISTIC RELIABILITY ASSESSMENTS
6. ECONOMIC RISK ASSESSMENTS
7. HUMAN ERROR SUSCEPTIBILITY STUDIES
8. "MANAGEMENT OVERSIGHT AND RISK TREE" (MORT) REPORTS

NRC RELIABILITY AND RISK ASSESSMENT RESEARCH ALLOCATIONS ($M)

	FY 79	FY 80		FY 81
		CONG	TOTAL	
INTEGRATED RELIABILITY EVALUATION PROGRAM	0.05	0.6	1.5	1.51
METHODOLOGY AND SOFTWARE	0.45	1.0	1.6 -	2.09
REACTOR SYSTEMS ANALYSIS AND LICENSING SUPPORT	1.85	1.0	1.7 +	2.7
NUCLEAR FUEL CYCLE RISK	1.05	1.0	1.0 -	2.0
TRAINING PROGRAMS	0.0	0.1	0.1 +	0.1
RELIABILITY AND HUMAN ERROR DATA	0.8	1.2	1.8 -	2.4
ACCEPTABLE RISK CRITERIA	0.2	0.2	0.2 +	0.3
IMPROVE WASH-1400	0.0	0.1	0.1 -	0.3
SUB-TOTAL (PAS)	4.4	5.2	8.0	11.4
OPERATIONS EVALUATION	0.0	0.0	0.5	1.2
TOTAL	4.4	5.2	8.5	12.6
PERSONNEL	22	23	28	30

NRC IMPROVED REACTOR SAFETY RESEARCH ALLOCATIONS ($K)

	FY 1979	FY 1980	FY 1981
IN-PLANT ACCIDENT RESPONSE	450	500	2600
ALTERNATE CONTAINMENT	300	200	800
ALTERNATE DECAY HEAT REMOVAL	150	200	400
ALTERNATE ECCS	--	--	--
ADVANCED SEISMIC DESIGN	--	--	--
SCOPING STUDIES	--	--	400
IMPROVED METHODOLOGY	50	100	300
TOTAL	950(1)	1000(2)	4500(3)

NOTES: (1) INCLUDES $150K PROVIDED FROM CONFIRMATORY RESEARCH BUDGET.

(2) COMMISSIONERS DENIED FY 1980 SUPPLEMENTAL REQUEST OF $3.4M.

(3) PROPOSED TO OMB IN FY 1981 BUDGET.

IMPROVED REACTOR SAFETY RESEARCH ALLOCATIONS ($K)

IN-PLANT ACCIDENT RESPONSE

	FY 79	FY 80	FY 81	COMMENT
HUMAN ERROR SENSITIVITY STUDY	100 (BNL)	100	--	IDENTIFY MOST SIGNIFICANT CONTRIBUTORS
ACCIDENT INFORMATION DISPLAY AND DIAGNOSTICS	200 (ORNL)	200	900	COMPUTER-AIDED DISTURBANCE ANALYSIS
IMPROVED ACCIDENT INSTRUMENTATION	80 (INEL)	100	400	STUDY INSTRUMENTS NEEDED TO FOLLOW REACTOR ACCIDENTS
IMPROVED ESF STATUS MONITORING	70 (INEL)	100	500	DEVELOP REQUIREMENTS FOR AUTOMATED STATUS OF ESF OPERABILITY
SIMULATOR CAPABILITY	--	--	600	DEVELOP REQUIREMENTS TO SIMULATE ACCIDENTS BEYOND DBA
ACCIDENT DATA INFORMATION	--	--	200	DEVELOP REQUIREMENTS FOR DATA TO MEET EXTERNAL NEEDS
TOTAL	450	500	2600	

NRC IMPROVED REACTOR SAFETY RESEARCH

DEVELOPMENTS SINCE JUNE 1979

TECHNICAL

- REVIEWED AND REVISED VENTED CONTAINMENT PROGRAM PLAN (IN PUBLICATION)
- COMPARED REGULATORY REQUIREMENTS FOR RHR, ECCS, AFWS, AND UHS AGAINST GENERIC DESIGN APPROACHES FOLLOWED BY REACTOR VENDORS
- COMPLETED TABULATION OF HUMAN ERRORS IN WASH-1400 AND COMPLETED ANALYTICAL MODEL FOR CONDUCTING SENSITIVITY ANALYSIS

ADMINISTRATIVE

- COMMISSIONERS REJECTED FY 1980 SUPPLEMENTAL REQUEST
- ORGANIZED NRC/DOE COORDINATING GROUP; CHARTER AND MECHANISMS BEING DEVELOPED
- CONTRACTORS SELECTED FOR RESEARCH ON DISTURBANCE ANALYSIS SYSTEMS AND PLANT STATUS MONITORING; FINAL OBLIGATION OF FUNDS PENDING

COORDINATED NRC/DOE EFFORT IN FY 1980 ($K)

	NRC FUNDS	NRC GUIDANCE TO DOE	ADDITIONAL DOE FUNDS	TOTAL
IN-PLANT ACCIDENT RESPONSE	500	1700	600	2800
ALTERNATE CONTAINMENT	200	500	200	900
ALTERNATE DECAY HEAT REMOVAL	200	500	--	700
ALTERNATE ECCS	--	--	--	--
ADVANCED SEISMIC DESIGN	--	300	--	300
SCOPING STUDIES	--	--	--	--
IMPROVED METHODOLOGY	100	--	700	800
GENERIC ISSUES	--	--	500	500
IMPROVED COMPONENTS	--	--	500	500
PROGRAM MANAGEMENT	--	--	400	400
OTHER	--	--	600	600
TOTAL	1000	3000	3500	7500

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FY 1979

DOE LWR Safety R&D Program Activity

		FY 79
1	Technology Management Center Office	700
1A	Administrative Management	
1B	Technical Management	
2	Technology Management Center Support Programs	370
2A	DOE Support	
2B	R&D Selection Methodology	
2C	Safety Overview Document	
2D	Review Group	
3	Risk/Licensing	610
3A	Acceptable Risk	
3B	Quantitative Risk Methods for Design Decisions	
3C	Accident Initiators (See 5E Below)	
3D	Reliability and Safety Methods Development	
3E	Data Base Evaluation	
3F	Safety/Reliability/Design Integration	
4	Structural Mechanics	360
4A	Piping Systems	
4B	Seismic Program Development	
4C	Seismic Interchange Committee	
4D	Structural Mechanics Interchange Committee	
4E	Non-Linear Analysis	
5	Improved Safety Systems	910
5A	Design Constraints Resulting from Considerations of Fire Safety	
5B	Effects of Maintenance and Testing on Safety (Improved Design and Man-Machine)	
5C	Nucledyne Passive Containment System	
5D	Containment Designs for Accident Classes 3 through 8	
5E	Component Failures (Causes and Solutions)	
5F	Valves	
6	Man-Machine, Interface	400
6A	Advanced Monitoring and Operator Assistance Program Plan	
6B	Exploratory Control Techniques	
7	Fuel	20
7A	Extended Burnup Fuel Safety	
8	Unresolved Safety Issues	530
8A	Containment Sump Reliability	
8B	Reactor Vessel Material Toughness	

Total - 3900K

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ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603

I. GENERAL

A. REASSESS PRIORITIES AND FOCUS
- AGREE

B. STUDY ANOMALOUS TRANSIENTS AND SMALL LOCA'S
- AGREE

C. ACCIDENT STUDIES

1. DBA -- MELT
   - IREP
   - RSS MAPS
   - CODE DEVELOPMENT AND EXPERIMENTAL PROGRAM IN RSR

2. MELT -- ATMOSPHERIC PATHWAYS
   - CRAC REFINEMENT AND APPLICATIONS
   - RSS MAPS

3. MELT -- LIQUID PATHWAYS
   - LIQUID PATHWAYS RESEARCH

994 356

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ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603 (CONT.)

D. MOLTEN CORE RETENTION
- IN VESSEL RETENTION STUDY
- CORE CATCHER VALUE/IMPACT STUDY?
- RSR MELT PHENOMENOLOGY STUDIES

E. PBF CORE DAMAGE STUDIES
- NON-PAS

F. RESEARCH STEAM EXPLOSIONS
SUSCEPTIBILITY TO COMMON CAUSE MELT AND CONTAINMENT FAILURE REQUIRES COMPREHENSIVE REVIEW

G. SITING STUDIES - ACRS RECOMMENDS:
- LIQUID PATHWAYS STUDIES
- COMPARATIVE AND ABSOLUTE RA

H. PLANT OPERATIONS - IDENTIFY RESEARCH NEEDS
- HUMAN ERROR SENSITIVITY STUDY
- HUMAN ERROR DATA AND PREDICTIVE MODELS

994 357

ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603 (CONT.)

- COMPUTERIZED STATUS MONITORING
- INITIATIVES IN SIMULATOR DEVELOPMENT AND OPERATOR TRAINING
- MORE MAY NEED TO BE DONE ALONG THE LINES OF IMPROVED PROCEDURES, FMEA, FHA

I. TRANSIENT SIMULATION IN RESEARCH AND LICENSING
   - RSR/PAS COLLABORATION

J. SYSTEMS BEHAVIOR AND INTERACTION
   - IREP
   - OTHER PAS/NRR COLLABORATIVE EFFORTS--SYSTEMS INTERACTIONS, SPECIFICATION OF LICENSEE STUDIES, ETC.

K. APPLICATION OF PROBABILISTIC METHODOLOGY
   - PRINCIPAL PAS FOCUS (IREP, ETC.)

994 358

ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603 (CONT.)

L. WATER SPECIFICATION AND CRACK GROWTH
   - NON-PAS

M. DISTURBANCE ANALYSIS
   - IRS/RSR/DOE COLLABORATION

II. RISK ASSESSMENT RECOMMENDATIONS

A. PHASE OUT RA ON CLASS 3-8 ACCIDENTS
   - WILL BE PHASED OUT IN FY '80

B. DEVELOP ACCEPTABLE RISK CRITERION
   - WILL DO

C. LEVEL FUEL CYCLE RISK EXPENDITURES
   - TIED INTO NMSS/SAFER/PAS COLLABORATION
   - IT IS IMPORTANT THAT NRC NOT DELAY REPOSITORY LICENSING

ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603 (CONT.)

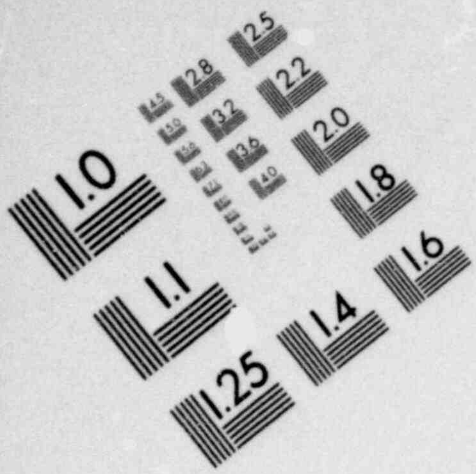
- THE PROGRAM IS LEADING THE ADVANCE IN THE STATE-OF-THE-ART
- THE PROGRAM IS OUR BEST PROTOTYPE FOR:
  - MULTI-OFFICE COLLABORATION
  - WELL-ORGANIZED PEER REVIEW
  - RESEARCH DIRECTED THROUGH ITERATIVE REFINEMENT
- ALTERNATE FUNDING SOURCES WILL BE EXPLORED

D. FLOOD RISKS--PRELIMINARY INPUT INTO LICENSING BEFORE FY '81
- GOOD CANDIDATE FOR ITERATIVE REFINEMENT APPROACH

E. ACCELERATED INPUT INTO GUIDELINES AND PROCEDURES FROM HUMAN ERROR RESEARCH
- CONCUR

F. DISTURBANCE ANALYSIS SYSTEM
- DOE/RSR/PAS COLLABORATION
- LLTF CALL FOR INSTRUMENTATION FOR INADEQUATE CORE COOLING

994 360



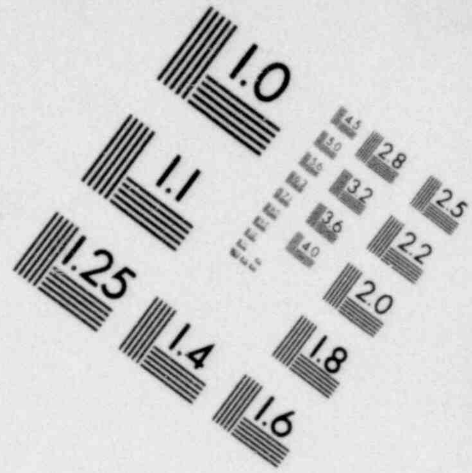
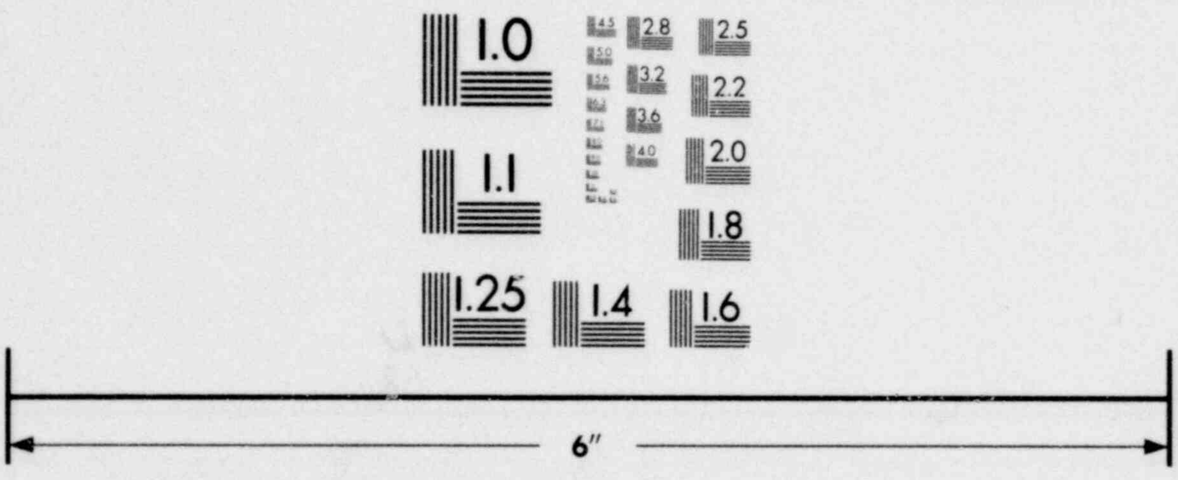
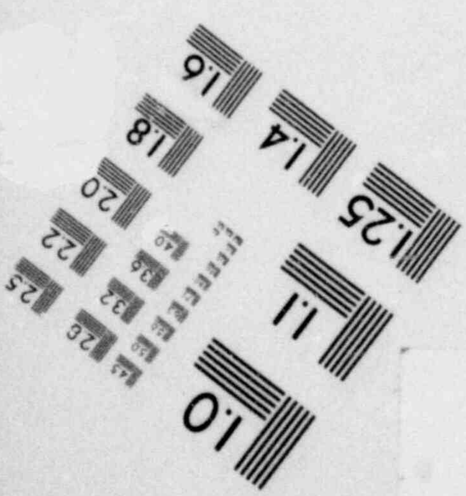


IMAGE EVALUATION
TEST TARGET (MT-3)





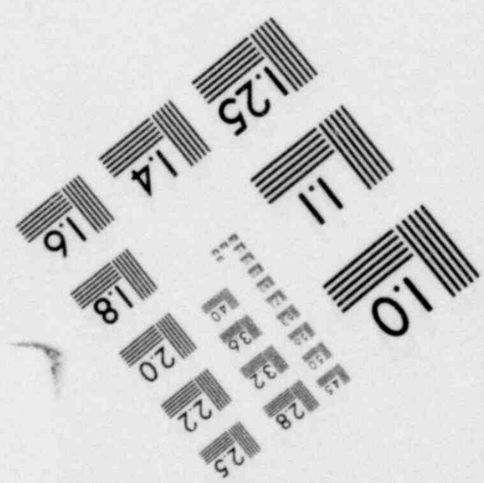
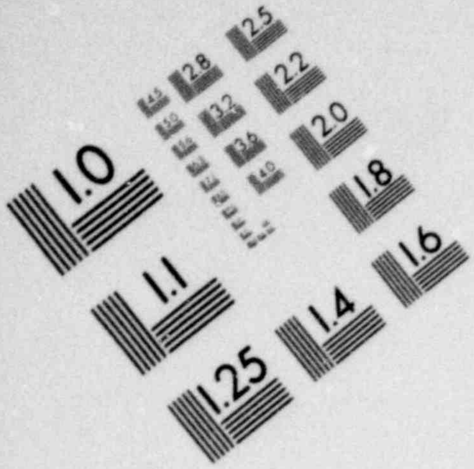


IMAGE EVALUATION
TEST TARGET (MT-3)

6"



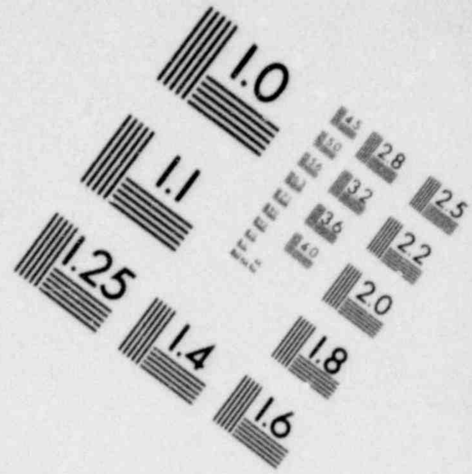
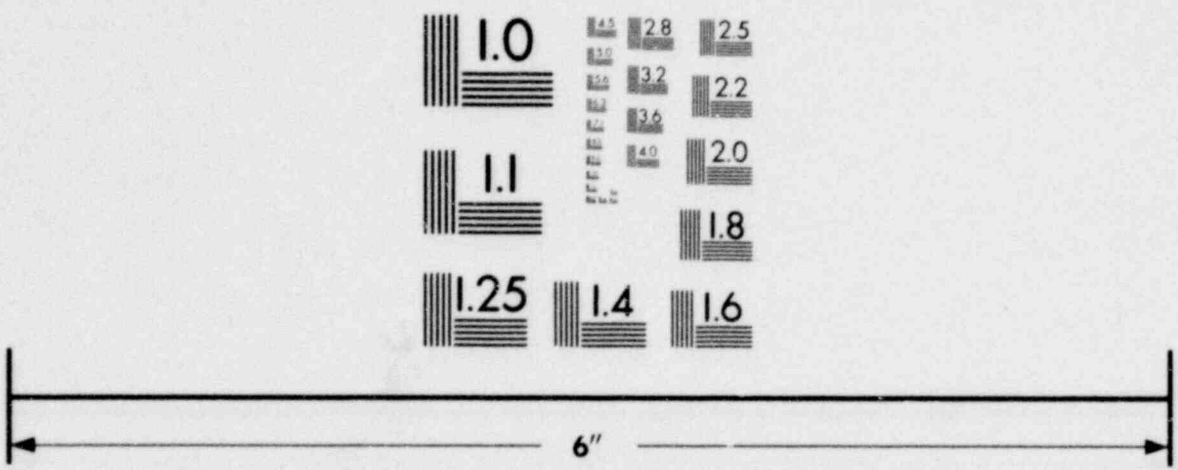
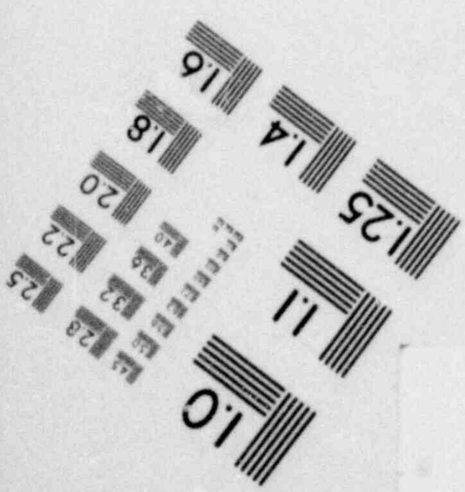
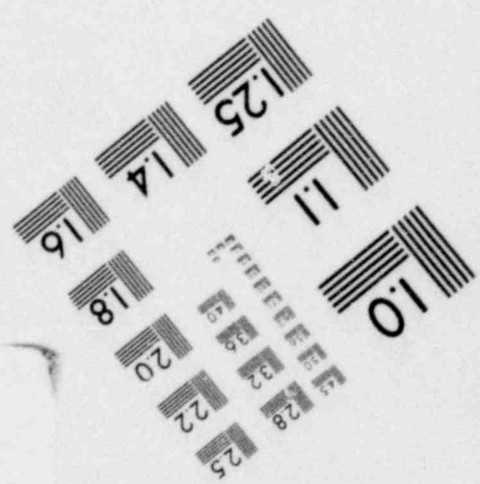


IMAGE EVALUATION TEST TARGET (MT-3)







ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603 (CONT.)

G. IMPROVED TRAINING COURSES
- CONCUR (DISCUSSED ABOVE)

H. TIME-DEPENDENT FAILURES - INSPECTION AND LICENSING INPUT
- METHODOLOGY AND DATA WORK
- IREP
- RISK-BASED RECOMMENDATIONS FOR I&E

I. INPUT TO EMERGENCY PLANNING - RATES AND TYPES OF RELEASE
- HAS BEEN AND IS BEING DONE IN DIRECT SUPPORT OF LINE OFFICES

III. GENERAL COMMENTS ON RISK ASSESSMENT

A. NAME "RISK ASSESSMENT" IS OFF-TARGET
- FOCUS IS ON PROBABILISTIC SAFETY ANALYSIS AND RELIABILITY AS WELL AS RISK

ACRS COMMENTS ON THE NRC SAFETY
RESEARCH PROGRAM BUDGET
NUREG-0603 (CONT.)

B. RISK-PERSPECTIVE RECOMMENDATIONS FOR RES PRIORITIES - RES CONCURS

- REASSESSMENT OF PRIORITIES AND FOCUS
- PAS GUIDANCE FOR EXPERIMENTAL AND CODE DEVELOPMENT PROGRAMS
- PAS COORDINATION OF IN-PLANT ACCIDENT RESPONSE AND WASTE ISOLATION RESEARCH
- PAS PARTICIPATION IN SSMRP, CORE MELT PHENOMENOLOGY STUDIES, ETC.

C. CLOSER INTERACTION WITH LINE OFFICES

- B&OTF (NRR)
- LLTF (NRR)
- SAFETY ISSUES TF (NRR)
- I&E
- NMSS
- OSD
- OTHERS (EMERGENCY PLANNING)

D. EXPANDED WORK - TREATED ABOVE

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OVERVIEW OF PAS ACCEPTABLE RISK PROGRAM

THE ACCEPTABLE RISK PROGRAM CONSISTS OF TWO MAJOR PARTS:

I. DETERMINATION OF ACCEPTABLE RISKS FROM NUCLEAR POWER - SOCIETIAL REQUIREMENT - SUBCONTRACTED TO PERCEPTRONICS AND CONSULTANTS.

II. COMPARISON OF NUCLEAR POWER PLANT FUEL CYCLE RISKS WITH THOSE OF A COAL FUEL CYCLE - SUBCONTRACTED TO SCIENCE APPLICATIONS, INC.

PAS ACCEPTABLE RISK PROGRAM

OBJECTIVE:	PRODUCE A DOCUMENT DESCRIBING THE STATE-OF-THE-ART IN METHODS TO ESTABLISH LEVELS OF ACCEPTABLE RISK AND PROPOSING A PLAN FOR RESEARCH TO BETTER UTILIZE THESE METHODS.
CONTRACTOR:	PRIMARY - ORNL SECONDARY - SAI AND DECISION RESEARCH
COST:	FY 78 - 200K FY 79 - 300K

995 005

PAS ACCEPTABLE RISK PROGRAM

TECHNICAL APPROACH (PHASE 1)

THE CONTRACTOR WILL SOLICIT AND SYNTHESIZE INPUT FROM RECOGNIZED AUTHORITIES IN A BROAD SPECTRUM OF DISCIPLINES. THE FOLLOWING METHODS OF DETERMINING ACCEPTABILITY WILL BE EXAMINED:

- CYBERNETIC APPROACH
- COMPARATIVE ANALYSES (E.G., COAL VS. NUCLEAR)
- EXPRESSED PREFERENCE
- DECISION ANALYSIS

PAS ACCEPTABLE RISK PROGRAM

TECHNICAL APPROACH (PHASE 2)

ISSUES WILL BE SPECIFIED AND NEEDS FOR DATA IDENTIFIED. EFFORTS WILL INCLUDE:

- CONSTRUCTION, DATA EVALUATION AND SENSITIVITY ANALYSIS FOR A MATRIX OF ELEMENTS CONTRIBUTING TO PUBLIC AND OCCUPATIONAL RISK FROM ALL PHASES OF THE COAL AND NUCLEAR CYCLES

995 006

ACCEPTABLE RISK REPORT OBJECTIVES

THE STUDY WILL TAKE A COMPREHENSIVE, CRITICAL LOOK AT THE PHILOSOPHICAL, POLITICAL, INSTITUTIONAL, AND METHODOLOGICAL ISSUES CRITICAL TO DETERMINING ACCEPTABLE LEVELS OF SAFETY. THE REPORT IS INTENDED TO:

A. COMPARE AND CRITIQUE PAST AND PRESENT APPROACHES,

B. SUGGEST NEW APPROACHES,

C. SERVE AS A FOCUS FOR CONSTRUCTIVE DEBATE,

D. OUTLINE A LONG-TERM PLAN FOR BRINGING RESEARCH, ANALYSIS, AND PUBLIC INPUT TO BEAR ON THE DEVELOPMENT OF RESPONSIBLE AND JUSTIFIABLE CRITERIA FOR NUCLEAR SAFETY.

995 003

ACCEPTABLE RISK REPORT STRUCTURE

A. GENERAL OVERVIEW

1. DEFINITION OF THE PROBLEM OF DETERMINING ACCEPTABLE LEVELS OF RISK

2. DEFINE SCOPE AND LIMITS OF OUR ANALYSES

3. OVERVIEW

   A. METHODS PROPOSED AS GUIDELINES FOR RISK POLICY

   B. REQUIREMENTS SUCH METHODS MUST FULFILL (E.G., LOGICAL SOUNDNESS, INSTITUTIONAL AND POLITICAL ACCEPTABILITY ETC.)

995 009

ACCEPTABLE RISK REPORT STRUCTURE (CONT.)

B. SPECIFIC METHODS TO BE ANALYZED

1. CYBERNETIC PROCESSES IN WHICH DECISIONS AND STANDARDS ARE FORGED THROUGH THE DYNAMIC INTERPLAY OF POLITICAL AND ECONOMIC MEASURES.

2. COMPARATIVE ANALYSES IN WHICH EXISTING SAFETY STANDARDS ARE ANALYZED AND OFFERED AS A BASIS FOR FUTURE STANDARDS.

3. EXPRESSED PREFERENCE APPROACHES IN WHICH APPROPRIATE GROUPS OF CITIZENS ARE ASKED DIRECTLY "HOW SAFE IS SAFE ENOUGH?"

4. FORMAL METHODS WHICH USE THE LOGIC OF DECISION ANALYSIS AND ECONOMIC ANALYSIS TO DETERMINE WHETHER ACCEPTING A PARTICULAR RISK IS IN SOCIETY'S BEST INTEREST.

EXAMPLES OF QUESTIONS TO BE ADDRESSED
FOR COMPARATIVE RISK ANALYSES

- PERCEIVED VS. CALCULATED RISKS?
- HOW DOES RISK FROM NUCLEAR POWER COMPARE TO OTHER HAZARDS?
- HOW DO PEOPLE REACT TO HAZARDS WITH LARGE LOSS OF LIFE?
- DO YOU INCLUDE BENEFITS IN THE ANALYSIS?
- HOW DOES ONE SET A STANDARD USING COMPARATIVE RISK?
- HOW DOES ONE HANDLE LONG LEAD RISKS?
- HOW DOES ONE EXPRESS THE RESULTS?

995 010

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EXAMPLES OF QUESTIONS TO BE ADDRESSED FOR EXPRESSED PREFERENCE TECHNIQUES

- HOW TO ACCOUNT FOR INCONSISTENT REPONSES.
- HOW TO TREAT COMPLICATED ISSUES.
- IS THE GENERAL PUBLIC "IRRATIONAL" IN THEIR ABILITY TO MAKE DECISIONS?
- HOW DOES THE METHOD OF ASKING THE QUESTION INFLUENCE THE ANSWER?

INDIVIDUALS INVOLVED IN ACCEPTABLE RISK PROGRAM

ORGANIZATION	REPRESENTATIVE	RESPONSIBILITY
ORNL	FLANAGAN	WORKING GROUP COORDINATOR; R&D SUB-CONTRACT TECHNICAL ADMINISTRATOR.
DECISION RESEARCH	SLOVIC	PSYCHOLOGICAL ASPECT OF RISK AND PUBLIC PERCEPTION OF RISK.
CENTER FOR ADVANCED STUDY IN BEHAVIORAL SCIENCES	KAHNEMAN	
STANFORD PSYCHOLOGY DEPT.	TVERSKY	
HARVARD UNIVERSITY	SPENCE	ECONOMIC ASPECTS OF ACCEPTABLE RISK.
CLARK UNIVERSITY	KASPERSON	GEOGRAPHIC IMPLICATIONS OF RISK ACCEPTABILITY.
WOODWARD CLYDE CONSULTANTS	KEENEY DERBY	USE OF DECISION ANALYSIS TECHNIQUES IN SETTING ACCEPTABLE RISK CRITERIA.
SAI	RHYNE	NUCLEAR VS. COAL RISKS.

PAS RISK CRITERIA PROGRAM

GOAL: TO ESTABLISH TENTATIVE, QUANTITATIVE RISK CRITERIA TO BE SUBMITTED FOR FUTHER REVIEW.

TIME FRAME: OCTOBER 1979 - OCTOBER 1980

UTILIZATION: THE RISK CRITERIA ARE INTENDED TO BE INTERIM CRITERIA TO BE MODIFIED, OR BE REJECTED, AFTER EXPERIENCE IS GAINED IN ATTEMPTING TO APPLY THE CRITERIA.

995 013

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RISK CRITERIA PROGRAM - TECHNICAL APPROACH

- ASSEMBLE AND CONSTRUCT STRAWMAN CRITERIA TO BE CRITICALLY REVIEWED FOR THEIR DECISION AND ACCEPTABILITY IMPLICATIONS, THEIR IMPLEMENTATION DEMANDS, AND THEIR PRACTICAL RAMIFICATIONS.
- AS ONE STRAWMAN CRITERION, SPECIFICALLY EVALUATE THE FEASIBILITY OF USING WASH-1400 AND THE MODIFICATIONS AND EXTENSION REQUIRED THEREOF.

RISK CRITERIA PROGRAM - PROGRAMMATIC APPROACH

- BROOKHAVEN NATIONAL LABORATORY TO COORDINATE INFORMATION COLLECTION TASKS (1, 2) AND PROBABILISTIC AND STATISTICAL ISSUES (4, 6, 7).
- IEEE AND ANS COMMITTEES ON RELIABILITY AND RISK STANDARDS TO INITIATE AND COORDINATE NATIONAL TASK FORCE ON NUCLEAR RISK CRITERIA TO CONSTRUCT PROPOSED QUANTITATIVE CRITERIA.
- ACCEPTABLE RISK EXPERTS TO SERVE AS ONE WORKING GROUP TO ADDRESS VALUE IMPLICATIONS OF PROPOSED CRITERIA.
- SPECIAL CONSIDERATIONS GIVEN TO LEGAL AND REVIEW IMPLICATIONS.

RISK CRITERIA PROGRAM - FACTORS TO BE CONSIDERED

1. RISKS FROM OTHER ACTIVITIES AND PHENOMENA
2. ATTAINABILITY OF A PROPOSED CRITERION
3. ACCEPTABLE OR UNACCEPTABLE CRITERIA?
4. LEVEL OF APPLICABILITY
5. VALUE IMPLICATIONS OF THE CRITERIA
6. MEANS OF EXPRESSING CRITERIA - UNCERTAINTY CONSIDERATIONS
7. MEHTOD OF DEMONSTRATING ACCEPTANCE
8. MEANS OF CERTIFICATION - LEGAL, ECONOMIC AND REVIEW CONSIDERATIONS