UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON SAFETY PHILOSOPHY, TECHNOLOGY AND CRITERIA

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Nuclear Regulatory Commission
1717 H Street, N.W.
Room 1046
Washington, D.C.

Wednesday, September 3, 1980

The meeting of the Subcommittee was convened, pursuant to notice, at 9:00 a.m.

MEMBERS PRESENT:

D. OKRENT, presiding
M. BENDER
J. C. EBERSOLE
H. ETHERINGTON
W. KERR
W. M. MATHIS
J. J. RAY
M. PLESSET

DESIGNATED FEDERAL EMPLOYEE:

R. SAVIO

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P R O C E E D I N G S

MR. OKRENT: The meeting will now come to order.

This is the meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Safety Philosophy, Technology and Criteria.

I am David Okrent, Subcommittee Chairman. The other ACRS members present are Mr. Bender, Ebersole, Kerr, Etherington, Mathis, Ray and Plesset.

The purpose of the meeting is to discuss matters relating to NRR management philosophy in developing licensing requirements and to discuss cascading failures in nuclear plants.

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

Richard Savio is the designated Federal employee for the meeting.

The rules for participation in today's meeting have been announced as a part of the notice of this meeting previously published in the Federal Register on August 19, 1980. A transcript of the meeting is being kept and will be available by September 5, 1980, it says here.

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 statements or requests for time to make oral statements from members of the public.

I think everybody has a copy of the tentative agenda. We might look at this for a moment, if you like. I understand that Mr. Denton and some others probably are on the shuttle bus which is due in around 9:10, and the agenda says we are supposed to start with them about 9:15.

At any rate, as you recall, one purpose of this meeting is to begin discussion with the staff on the question of cascading failure. For the morning session, Dick Savio and I tried to prepare a list of possible philosophic questions or whatever you want to call them.

First I might ask if there are points that members wish to make, questions they wish to raise on these or other points that they think we should try to include this morning or this afternoon in this general area. This was not intended to be prescribing in the sense that we could not take up other points that fell in the general area.

We hoped that we would have the benefit of an opportunity to talk with the management of NRR. They indicated in August they could not make it then but they would like to be here in September and sort of talk about a group of things at one time.

Any comments?

MR. EBERSOLE: I would like to comment. We should stand up and look in perspective at our (inaudible). I simply cannot see anything, in fact, more simple. By and large I can't help but look at (inaudible) you really say we are not going to look very much more, we are going back to the cosmic view of the picture. (Inaudiible.)

I think we have to look a little bit differently at the detail versus the presciptive and iterate some position that we have not got to yet. And, of course, that gets around to some degree of standardization that we have not yet contemplated.

I would just like to open that as a general topic. I mean I could look at the recent Surry incident. There is a piece of plumbing that an ordinary sewage plumber would do a better job on. (Inaudible.) And I can also look at TMI-2 and maybe with three or four sentences say it is a lousy piece of instrumentation (inaudible), and at Brown's Ferry it clearly -- it was preunderstood that the potential for that sort of event was there. We got to the thing before worse things could have happened.

Rancho Seco and Crystal River to some degree were (inaudible), and try to identify the causes of these things. I think that would do some good.

MR. PLESSET: Jesse, I did not get your objecting to standardization.

MR. EBERSOLE: One would prevent things of this sort by hardlining the details which we do not do.

MR. KERR: One would standardize these acts, so --

MR. EBERSOLE: One would have critical detail that would preclude these things.

MR. KERR: If I can change the subject slightly, I had said before and I continue to be concerned about in the process of making corrections and improvements, we are placing a tremendous burden on the resources of both NRC and operating plants, and we need, insofar as we can, an enterprise to try to continue to encourage NRC to (inaudible), which would permit the people who are making the changes to do them with sufficient thought that they do, indeed, produce improvements.

It may be that I am looking at things from a sufficient distance that I am more confused than the people who are operating the plants, but when I see all of the demands for information changes, I think schedules that are being imposed on operating plants are not realistic. I am concerned that both the NRC staff and the people operating plants are doing so many things so fast that the improvements we hope to effect may get lost in the confusion.

I just don't think that we can do as many things as are being asked for as rapidly as some of the existing schedules would indicate we are trying to do.

MR. BENDER: I have a somewhat different view of the way in which the problems are. First, I am not nearly as optimistic as Jesse about the idea of some more systematic way, which I will interpret as being a term which standardization is going to circumvent. The kinds of problems which will make the newspapers, we always will have some, and they will be advertised as being worse than they are in most cases.

But there are some fundamental things that we need to understand, and one of them is the question of how much interdependence we can tolerate in these plants and whether we understand interdependence well enough to be able to rationalize whether we have done an approach that is effective in separating circumstances; why one accident doesn't impose problems on another.

The second point that seems to me to be pretty important is to be sure that there is time to take action. Most of the events that have been talked about, there was plenty of time for action but the operators did not act in the time which was accessible to them. It seems to me we have to establish an approach that shows that the operators can do things and will do things within specified times.

Then the third one is the question which I think has been brought up by this committee somewhere between 100 and 100,000 times, I am not sure which. That is the question of whether the operators can diagnose accidents. We could sort out those things in our discussion like this. I think we would be a lot better off than we would by just trying to analyze each one of these things individually and come up with separate answers, even though that does run the risk of some generalization.

MR. OKRENT: Any other comments or points the members would like to make now?

Jerry.

MR. RAY: I would like to supplement Bill Kerr's thought that there is need to evaluate, if you will, and assign priorities so that the responders, the licensees can standardize their approach and use their resources to the best advantage. I think some thought might very well be given to making the NRC edicts less prescriptive so as to force the licensees to a more analytical approach from a perspective as to what the overall situation is, and perhaps thereby delve more deeply into solving the problem and improving their operator training and so on.

It is inherent, when they are given certain things to do by certain dates, that that is all they do. They are forced to meet those dates along the specific lines and details that are laid down to them and so they don't -- they are really reengineer situations to the extent that they have the capability of doing it.

I would like to comment on Jesse's point. It is true, perhaps, that a series of silly things, either a stupid act of commission such as testing for leakage with a candle, or a stupid design such as the configuration of that plumbing at Brown's Ferry, are the underlying roots as to why certain things happen.

But it seems to me in retrospection that every one of these incidents, while they might have been initiated by some very trivial oversight or lack of consideration, every one of them has brought out underlying deficiencies that are quite deep and pervasive, such as, for instance, a lack of proper operator training to recognize emergency situations at TMI, which I cannot say is the reason that they had the accident so much as the major reason why the accident became as deep and significant and as damaging as it did.

I would have a problem in the sense of standardization such as Jesse mentions from the viewpoint of having categorized things to fit standardization.

MR. OKRENT: Well, I think, if I may, I am going to move to Mr. Denton. I understand that he has a broken schedule this morning. Somewhere around 10 o'clock there are some other people upstairs who want to talk to him. And part of the group from the staff over here will have to go upstairs, but part will remain. Then I assume sometime later in the morning he will be able to come back down.

So, Mr. Denton, do you want to start?

MR. DENTON: We don't have any planned presentation. We understood you wanted to discuss a litany of items, and we are prepared this morning to go through those with you.

MR. OKRENT: Fine. We don't have a definitive order in which we think these should be discussed. If there are some you would like to take in the period between now and 10 o'clock or whenever it is you have to break, we could try to rearrange things if that would be convenient.

MR. CASE: Taking into account the people who are here now and the people who will have to go to the meeting a little bit later, I suggest three categories: one category, 9 to 10; the second, 10 till the Commission meeting; the third category until lunch, and the fourth category at the lunch.

In the first category, (a) and (b), we would take up right now.

MR. OKRENT: Okay.

MR. CASE: Followed by (c), (d), (h) and (i). And Mr. Schroeder does not have to go to the Commission meeting. He can deal with that.

After the Commission meeting your list is a little bit different than ours. Let me go over it with Mr. Schroeder. General approach, the need for a modification.

That is my (c). And (d), general approach to defining requirements for future LWRs.

MR. OKRENT: Yes.

MR. CASE: (h), approach to developing requirements for ice condenser containment plants, and (i). Then after the Commission meeting, depending on Mr. Stello's availability, I would like to take up what you call the Brown's Ferry event as soon as he is available, sometime after 11, and Mr. Denton gets back.

Then we can go to others which will be primarily by Dr. Ross, leaving for this afternoon the cascading failures.

Does that make sense?

MR. OKRENT: There is one you did not mention, on control room requirements.

MR. CASE: That will be after the Commission meeting.

MR. OKRENT: All right, by Mr. Hanauer.

So it seems like Savio must have been talking to somebody and he had it arranged in the order you mentioned.

Why don't we start.

MR. DENTON: We are ready to start on (a) and (b). We don't have a planned presentation. (Inaudible.)

MR. OKRENT: Well, all right. With regard to the NTCPs then, maybe it would help the members if you could give a one-minute summary of what you think is the approach that you have recommende. I hav? the SECY document here. I don't know if they all have it.

MR. DENTON: Let me ask Darrell Eisenhut to summarize it for you.

MR. EISENHUT: Basically, when we developed that document, there are basically three options we could take. Remember the Commission in connection with the Action Plan developed a set of requirements, post-TMI requirements for OL's. They laid out an approach that covered operating plants.

The one thing that was not included at all was the licensing requirements for CPs and manufacturing licenses. Basically we looked at three options. One was just to go back to the pre-TMI CP requirements, modified by the Action Plan, just to add on the OL Action Plan.

Another option was we could just take no action, period. That would be the other end of the extreme. We could just sit tight and do nothing until we better understood the requirements.

The third approach, which was somewhere in the middle, and the option we proposed was to resume licensing using the pre-TMI CP requirements, augmented by the Action Plan. It required certain additional measures in selected areas.

Those areas were the same areas that were identified in the ACRS letter when we met on this issue some time ago. Basically there are four of them that we had addressed in the paper. The approach that we took was to propose using this as the licensing package for CPs, and we are issuing it for comment.

The four areas we were siting, degraded core rulemaking, reliability engineering and emergency preparedness. So it sort of goes beyond the OL Action Plan in those areas. That approach we are going to be sending out for comment prior to going forward, and that is where we presently are today.

MR. OKRENT: Well, I would assume that the two areas which are possibly complicated, of the four you have mentioned, are the ones dealing with the reliability part and the one with degraded core cooling, since the siting and emergency preparedness ones, I think, will be more clearcut, however you want to put it.

Perhaps you could tell us, for example, in the area of degraded core cooling, what guidance you think you have provided or you are proposing to provide, and why this meets the need, whatever the need is.

MR. DENTON: We can sure do that, but is it best to cover it now or do you want to wait and comment on the proposed rule when it is on the street? You know, we went through these with the Commission before, and you are right: in two of the areas, siting and emergency planning, the Commission has provided definitive guidance to the staff.

On the reliability one, we spelled out in the proposed policy statement which systems are to be covered with risk assessment techniques, and in the degraded core one we specified they are not to foreclose, to the extent practical, the capabilities to cope with the items that are covered in the degraded core rulemaking.

MR. OKRENT: Well --

MR. DENTON: And industry is divided on these topics, too, and one of the reasons for going out for public comment is to give all parties a chance to get their oar in on how to approach it. Industry has proposed that we should not go into mitigation requirements for the CPs; we should just stick to prevention. And they would like to propose systems that would give a 5 to 10 risk reduction in prevention.

We are saying that is not good enough; you have to show that letting you go ahead will not foreclose to any great extent possible outcome of the degraded core rulemaking. So we have asked them to cover those in their responses. So it is not a decision-making paper. It is a paper that elicits public comment.

There is also attached to it a thick NUREG document that spells out the requirements of the Action Plan for CP holders.

One last issue that is being considered before releasing the paper is whether or not to also require the licensees to compare their plant to the staff standard review plan and the current reg guides and justify departures therefrom.

So that is really the last issue that the Commission is considering adding to the paper.

Darrell, would you like to comment more on the --

MR. EISENHUT: No.

MR. OKRENT: I guess I don't know what it means when it says not to foreclose. I can recall back -- I don't know. I suppose more than five years ago PWR designers were not going to foreclose their possibility for maintaining more relief capacity on primary systems in connection with ATWS. That is, they were going to maintain flexibility in design, is one thing we heard.

But not too long afterwards we found that, in fact, once they were proceeding along construction, they had a construction permit, they really did not have any flexibility with what they said. I think that we see that there is a considerable reluctance to go and cut something into a pipe where the hole was not there at the beginning and so forth.

MR. DENTON: Plainly, the way to not foreclose anything would be to wait for the degraded core rulemaking to be over, and whatever the Commission requirements were at that time would be clear. The Commission did not opt for that option, so I think it recognizes that these plants are designed and the requirements we are talking about only apply to those currently filed CP applications.

Though designs are essentially complete, they were all essentially through the hearing process at the time TMI happened. So in order to really avoid foreclosure guaranteed, you have to wait until the degraded core rulemaking is over, which might be years before you could come to that decision.

So I think the approach the Commission is asking for comment on -- recognizing the plants are designed -- there are certain things, perhaps, such as a choice of concrete, don't use lime concrete, go to basalt so you don't get CO_2. Think about hydrogen control. And there is an ice condenser plant or two in this list.

So there are a number of issues that they say don't foreclose, but they sure cannot guarantee it. I think the proposed approach treats these plants as the last of the present generation, and the rule only applies to those six applicants, and I do not expect all of those six to continue to be viable.

There are very few of them that I would expect to continue the process. They have all banded together to await the Commission's policy statement. But it is not intended to apply to anyone but the presently filed CPs. I guess I should make the point, too, that I do not see any new CPs being filed with the Commission for a long time. There is no indication that I get from industry that there is anybody coming in with an application possibly in this decade.

MR. BENDER: You put the attention on these half-dozen plants that are in the construction permit stage. There are about 70 total, I guess, that are to be considered. Just from a practical standpoint, what is the reason for wanting to get those into some better state than the other, for want of a better term, 64?

MR. DENTON: That is clearly an option, too. We identify three options. One is don't treat them any differently than the ones that are presently under construction and just apply the Action Plan requirements as they come along. It seems to me there are some changes that we can accomplish in those plants. We can make them somewhat better.

The other approach would be to wait until all the rulemaking is done and have a whole new generation of better designed, safety plants, standardized, that approach. There are no real incentives for that. You would not find anyone to apply it to in the near future. So I think it sort of recognizes that they are probably better designed plants than the ones that are under construction and we should not be passing up an opportunity to get those gains that are possible in plants that are essentially through the design phase but have not yet started construction.

MR. BENDER: There is a risk-benefit relationship which presumably is being taken into account when one does these things, and I don't even know whether the rulemaking approach deals with risk-benefit in any quantitative sense. Do you have a philosophy that is going to be developed during the rulemaking?

MR. DENTON: No, I do not -- not in that kind of sense. I think this is more pragmatic. As you say, there are almost 70 plants licensed to operate and another 90 or so that are under construction. This would add to the universe of operating reactors.

My own view would be that we should not let them go without recognizing some of the valuable lessons that are in the Action Plan; that we should require those changes. At the same time, I don't see that we would have to await the outcome of every rulemaking we have under way.

You asked me, and that is my view. I do not see safety as being a plateau in which if you meet that, that is acceptable to everyone. I think safety is a goal for

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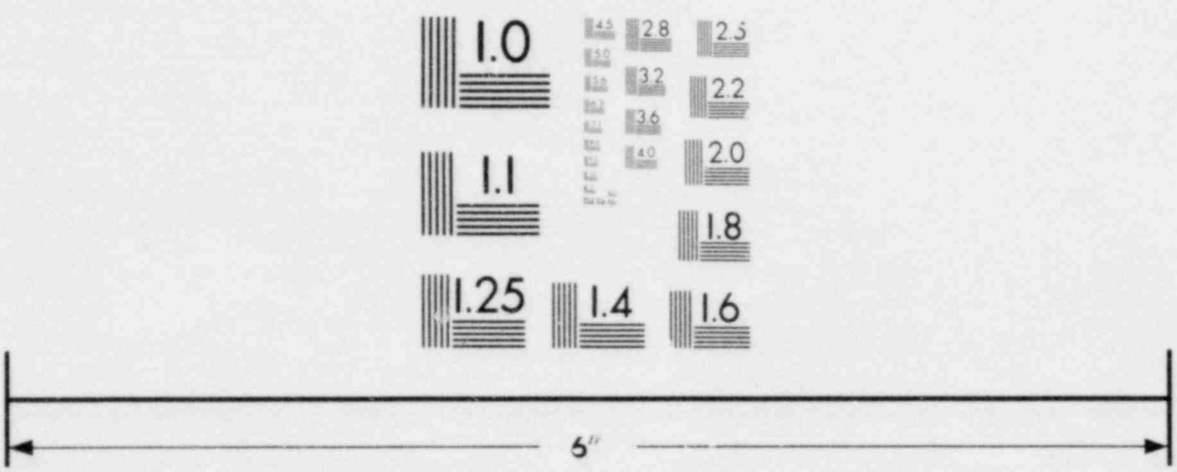

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society and I think we ought to fix where we have the opportunity. I recognize from a risk standpoint these might be better than the average plant in operation today, but I think that is our mission.

You could argue the other way, and I think --

MR. BENDER: I am a rate payer. I have to buy electricity at some rate. One of the things that is influencing the rate at the moment is the rate at which the plants are made available to me for electrical generation purposes.

I envision, if I sit around for a decade or so, there may, in fact, be some kind of a rulemaking. It probably will take about that long. I really would like to see something that has a time frame associated with it that is practical, that gets the plants on line in some reasonable time period and at the same time has some quantitative value in terms of risk reduction, if such is attainable.

If there is no quantitative risk reduction available within a reasonable time frame, I think you are just creating a lot of sound and fury, and I suspect that is what you are doing. I have not heard anything that indicates anything any different from that right now.

MR. DENTON: These plants if they are built will be displacing the burning of coal, most likely. There are not any new oil burning plants being built in the U.S., so society has to judge whether they want to burn uranium or burn coal for these.

I think the proposed policy statement, what it does is say make them a little bit better than plants that are under construction. It does not require a whole new generation, and I see differing people would have differing views. That is one of the reasons for going out with a draft policy statement.

Remember, the Commission was criticized for adopting the policy statement on the OL plants without public comment.

MR. BENDER: There is not anything wrong with public comment. I am all for that. But there ought to be something to comment on, and there is damn little to comment on from what I have seen so far.

MR. OKRENT: Mr. Kerr.

MR. KERR: Harold, I interpreted your earlier comments about possible future licensing to mean that in your view it is unlikely that you will get applications for new plants in the foreseeable future. When I talk occasionally to people in the utility business, they tell me that that is indeed the case. And one of the reasons they give -- I don't know whether it is the right reason or not -- is that the licensing situation is so uncertain that nobody who has to make a judgment would apply for a license now.

You seem to be telling me that we really do not need to worry about licensing new plants because nobody is going to ask to license one. It occurs to me that we may be developing a self-fulfilling prophecy in which the people who are responsible for licensing don't see anybody asking for licenses, and therefore there is no rush about setting up a system in which licenses could be obtained.

And the people who are asking for licenses don't see any way of getting a plant licensed, and therefore they are not going to ask that plants be licensed. Now, it concerns me a little because I think that Congress is a policy-making body in this country if we have one. All I see coming out of Congress up to now says that it is a policy of this country to operate nuclear power plants safely.

It therefore seems to me that the Regulatory Commission does have some responsibility to make it possible for people to get licenses for power plants. I hope we are not getting ourselves in a situation in which the Commission has concluded that since nobody is going to ask for a license, there is no rush about making it possible for people to request licenses.

MR. DENTON: I hope I did not imply that we would not review a license if somebody came in the door, but I am trying to reflect my understanding from talking to people in DOE about load growth in this country, the need for electricity, and the projected number of plants.

There has been a marked downturn in the consumption of electricity and the growth rates, and if you look at the present plants, fossil and nuclear, that are either on the boards or -- the best advice I can get is that this will guarantee that the country has sufficiently high reserve margin throughout this decade.

It is true you cannot project what the long-term growth of the country will be. If somebody is interested in a license, we will certainly develop the requirements or whatever it would take to go that route. My own view is the lack of enthusiasm for nuclear power is not so much the design of the plant but other issues, such as waste disposal, which has become very acute.

MR. OKRENT: Harold, do you really think that if you were the responsible engineer at a utility and the chairman of the board came up to you and said can you tell me what the Nuclear Regulatory Commission will want from the safety point of view of a plant that we would want to start construction on in four years and have operating in twelve years, you could answer the question, knowing everything you know from the inside as well as from the outside?

MR. DENTON: No, I could not answer it today; but I think we could get an answer. It would mean a Commission action. The Commission has not acted on what is required beyond those applications that are under review.

MR. OKRENT: But you say it is a Commission action. My experience in observing groups like the Commission or the Congress or so forth is they need to have a proposal. Sometimes it comes from an individual within the Commission or within the Congress, but they do not spontaneously develop a consensus position, you know, without something to review.

I guess I do not see that your response to Mr. Kerr that, well, if someone comes in with a request to construct a plant, you would review it is responsive to the general issue that is on the table, which is safety philosophy and general design criteria that a new plant should have.

I am not trying to let the nuclear industry off the hook by focusing on the staff. I think, in fact, it would be well if they came forward with their own proposal for what they think future plants should look like. I think, in fact, the NTCP people should also come in with their own specific proposal.

But aside from that, I would hope that the NRC staff would develop something that was somewhat more of a tangible approach to, one, the question of degraded core cooling, and two, the question of what reliability do you need in these plants than I find in the NTCP document that you submitted.

To me, having them maintain flexibility, I can look at it from one point of view and say, well, you know, they really are not going to be able to do very much because once they build it, they cannot change it. Or I can take the other point of view and say, my God, they will make us tear it apart after we have built it.

From either point of view, just having to maintain flexibility leaves one in a very awkward position, particularly if you do not have the large dry containment, which seems to be, at least at the moment, the more passive of the kinds we have been looking at.

Similarly, in the reliability area you have indicated that they look at some specific systems. In fact, they are not necessarily all the systems that are going to be troublesome when one looks at plants five year from now. In any event, what they have been asked to do does not provide a basis for judging whether the current designs are okay. They are all going to meet the single failure criterion, and they can make some small changes to avoid obvious AC dependencies where they don't want it, or DC dependencies where they don't want it.

It does not address the question of should they look for independence, let's say, in shutdown heat removal, which Mr. Bender was alluding to before and Mr. Ebersole has certainly alluded to in the past, or things of this sort.

So something that might come out is a c.iterion some years -- may or may not. But I do not find, again, myself if I were the engineer trying to tell the chief executive yes, I know what we what to do -- from what is here, I think I would have trouble saying yes, I know.

MR. DENTON: Well, I guess the reason for issuing it for comment is to see if industry feels that way. But on the general question of should we gear up and devote a lot of priorities to new designs in unbuilt plants, I guess maybe we see it differently. But I think the 90 plants that are under construction are a big challenge to the staff, the 70 that are in operation, and there really is not much basis that I can determine for giving a lot of the staff's resources to some as yet unidentified need.

It is not I am unreceptive to new designs. Everyone has looked at certain features of plants and has said, boy, we should get this in the next generation of plants. But, you know, I do not feel the need to go generate -- and I get my information on need from the Department of Energy, and it is based on growth in this country and electricity consumption. As the price goes up, you will consume less.

Maybe we are going into a period of consolidation here. It might be a decade before the country decides on its energy course and what role the nuclear will play. By that time we can have several of these fundamental philosophic questions.

But I see this as a period of consolidation and one that requires most of our resources. We assume the plant is built in concrete largely, and we will focus on those operational aspects that have been identified as so critical, so we are putting a lot of our attention in drills, operator qualifications, procedures, things to enhance the safety through the operational aspects.

I don't want to foreclose one, but we are fully occupied implementing the action plan, and I would have trouble justifying spending a lot of staff effort coming up with criteria for new plants when you really cannot establish that anybody out there wants a criteria for new plants.

MR. KERR: Harold, I have not seen any DOE projections that indicated a zero growth rate, nor have I seen any DOE statements that existing plants are going to quit becoming obsolete. This has certainly happened in the past and I think it will continue to happen in the future, so I believe that unless we have a much more serious recession than we now have, that there is going to be a continuing demand for building electrical generating plants.

The rate at which they are built may change. What you have told me I could interpret, and in fact, I really don't know quite how else to interpret it, to say that right now a person who wanted to license a nuclear plant would find it virtually i possible.

That concerns me because it says that the Commission, by its judgment which you have to exercise -- you have a limited amount of resources -- has determined that the nuclear option does not exist for the next ten years or twelve years in the sense of having available the possibility of building nuclear plants.

I really do not see that -- I mean if that is the intent of this administration, I had not interpreted it up to now as being quite that negative, and I certainly have not seen that as the intent of Congress. I recognize that you cannot start reviewing designs that have not been sent to you, but it seems to me that it is necessary to begin establishing a review philosophy.

The degraded core cooling is an outstanding example. You have gone out, I think appropriately, for public comments. But it seems to me within the staff there also needs to be a development of approach to how one is going to try to deal with this. Not that you have the solution today or next week, but that there begins -- I mean some effort is needed, I think, to try to develop an approach which is appropriate to people who come in for licenses.

If that is completely put on the back burner, then it seems to me what we are doing is foreclosing a nuclear option for the next 10 or 15 or 20 years.

MR. DENTON: We are trying to develop an approach for the existing CPs, and that is as far as we have gotten. We have outlined the options in that one. Either let them go the way that they have gotten through, with your approval and the staff's approval pre-TMI, and considering the last of the old generation, hold them up until we can develop these new requirements through whatever procedures it takes.

We have proposed something of an interim approach to apply the new emergency planning criteria to be sure that we are not siting these plants in areas that could not meet the emergency planning rule to apply siting criteria, to require reliability assessments in those systems where we know how to do reliability assessments, and to do what we can to preclude foreclosing the ability to put in filtered containment venting and hydrogen control issues and other such ones that might fall out.

Now, that leaves it open, admittedly. It is not very clear guidance, but at least for some utilities with some designs, they are willing to take that risk and proceed Perhaps others are not.

MR. OKRENT: You talk about gettting guidance from DOE. I vaguely recall seeing somewhere in the past weeks that DOE had been looking at the question of oil prices some years ago and projecting that in 1980 it would be $13 a barrel. So it would seem to me one needs to be a little bit cautious in basing one's broad policy with regard to whether there is some kind of a need for h ving general guidance for future LWRs on firm projections from a particular set of people.

In fact, it seems to me if it is NRC policy not to provide such guidance for future LWRs at this time because they do not have the resources, I think in fact that should be a conscious Commission decision. They ought to say this is our position, and that the Congress or some part of the Congress -- if they don't like it, they ought to know it so they can tell the Commission they do not like it.

I myself think it is a Commission responsibility to have a policy that would require for future plants, whether it is the same as the past or different, and I really do not know what to make of this proposed degraded core rulemaking hearing where all that I saw put out was a series of questions. I did not see in staff proposal for what should be done for existing plants or for future plants, just a series of questions.

I understand that it is a difficult problem, but I would say myself, you know, to put out something like that is really shirking one's responsibility, giving a personal ion.

MR. DENTON: Well, you know, I wish we had a cast of tens of thousands, and, you know, the resources that we used to apply in the old AEC to developing new positions. It s not true that we have those kinds of resources. We are fully occupied. I guess my own concern is I have everybody working on the problems that are carrying on out there in the real live world there today, the problems of Brown's Ferry, St. Lucie, Crystal River, trying to make the plants already built conform to the new requirements.

We could certainly do better in developing requirements for way down the road. It just is not a high priority effort within the staff.

MR. OKRENT: Could I explore that for a minute?

MR. ROSS: Dr. Okrent, if and when we get to item j on the agenda, Dr. Siess has some prepared remarks. However, we do intend to take a position on the long-term rulemaking. Now is not the time. We had intended last summer when we went forth to the Commission with the proposed long-term rule to have a position there. It is just not ripe at this time.

We hoped that answers to these questions would help develop an NRC position, just like the interim rule which is going in front of the Commission tomorrow. It has an NRC going-in position. So will the long-term rule next summer. That is our intent. We are not shirking it. It is just not ripe. We can elaborate on that later on today if you wish.

MR. KERR: Mr. Ross, I saw the questions to which Mr. Okrent refers, and I heard another staff member say that the staff, I guess, from what I could gather, did not want to prejudice the answers they got. So they really were not trying to tell people what they were thinking.

I recognize that one has to exercise some discretion here, but it seems to me that one gets more useful results from commenters if the commenters have something specific on which to comment. I have tried this, and I am sure you have. Even if your plan is incomplete and is a plan that you expect to change -- I don't know what you mean by premature -- but it seems to me you get more meaningful comments and more useful comments if you let people know what you are thinking and they make a comment on it. Even if they tell you it is lousy, that is quite important.

I am puzzled that what -- I realize you were not asking for a popular vote, but from the way those questions were worded, most of them, one could answer them by voting yes or no, and that would be the public comment. That, of course, is absolutely useless to you, I think; whereas, if you could present something that would say we are tentative about this so far and we may change our position markedly but it is what we are now thinking, it would seem to me that the public comments you would get would be much more helpful.

MR. OKRENT: Could I ask --

MR. DENTON: I think this is a very fluid situation and I think maybe it has been painted too starkly this morning. It is not that we are not doing anything. If you go back a year, we were not doing anything on plants that were completing construction. We are recovering from the TMI implications, and what we have moved on so far are those few plants that we know are still interested in CPs.

A year from now, we will be working our way out from under some of the items to start developing more in here, but we cannot do it all overnight, as some of you observed this morning. Some things have to be done before others, and I guess my preference is to use the staff resources that are available for new plants on this handful of CPs who have got over a half billion dollars invested in applications that the staff had reviewed pre-TMI to see if we cannot get some motion there before devoting much resources to possible plants beyond those.

I think they have to call on our priorities more than some company that is still trying to debate coal and nuclear.

MR. OKRENT: Mr. Quittschreiber attended a meeting on August 21 where the staff met with the commissioners. In his memorandum on the meeting, he indicated that you said something. I wanted to see if this is an accurate reflection. I will quote from the memo.

It says: "Denton indicated t t a TMI-2 hydrogen burn would clearly fail the Sequoyah containment, but that all action plan requirements had been implemented at Sequoyah which would reduce the probability of a serious accident at that plant by one or two orders of magnitude."

I am interested in the latter part, that the Action Plan would reduce the probability of a serious accident at Sequoyah by one or two orders of magnitude. Was that an accurate reflection of what you said?

MR. DENTON: No, I do not think it is.

MR. OKRENT: Could you tell me, then, what you think would be an accurate reflection?

MR. DENTON: What I tried to indicate to the Commission is for that type of scenario involving small break LOCAs and operator incorrect behavior during such circumstances, I thought it had been reduced by an order of magnitude. I do not mean that the risk had been reduced by one or two orders of magnitude because of TMI action. Certainly not.

I do not think that is my view at all. I would hope that the TMI actions have reduced the overall risk by a factor of three or more, but I think for that type of accident that happened at Sequoyah, all the training we have given operators in recognizing and coping with subcooling and those kinds of conditions would reduce the chance of that particular scenario leading to that much hydrogen by an order of magnitude.

MR. OKRENT: But you told the Commission that the overall risk may have been reduced by a factor of three.

MR. DENTON: We did not discuss the overall risk at all.

MR. OKRENT: You can see that Mr. Quittschreiber got the impression that you thought the overall risk had been reduced by a factor of one to two orders of magnitude, and the Commissioners may have gotten the same impression.

MR. DENTON: No, I do not think they did. I think the context was clearly just related to operator performance during that type of accident.

MR. CASE: As much as I hate to interrupt, we have to go.

MR. OKRENT: Let's see. Do you suggest we go into the group of items which are two and then go back to one?

MR. CASE: It is probably fair to -- we probably covered item D.

MR. KERR: Mr. Chairman, would you consider a short break at this point?

MR. CASE: At least to the extent that Mr. Schroeder was going to cover it also.

MR. OKRENT: We'll have a short break, ten minutes.

(Recess.)

MR. OKRENT: I want the subcommittee members to note that we are now once again air-cooled.

(Laughter.)

Of course, we are moving back towards soft technology, and if the fan were driven by wind power, I think we would have gone the full route.

Mr. Case.

MR. CASE: I would like to welcome Dr. Siess to help us in the discussions. In this phase we were scheduled to cover C, D, E, and F. I think it is fair to observe that Mr. Schroeder would not have anything to add to the discussion that has already ensued on item D; so I would ask him to cover then only C, E, and F in whatever order he chooses.

Would that be all right with you, Mr. Chairman?

MR. OKRENT: Let's try.

MR. SCHROEDER: With regard to your item on single failure criterion modification --

MR. OKRENT: You're going to have to speak up a little louder, or we are going to have to turn up the volume. The fan provides a background noise level that is pretty high.

MR. SCHROEDER: All I can say with regard to the single failure criterion is along the same lines I think we discussed with the committee when we were discussing the new USI list.

Our reaction at the moment -- and we have considered the committee's letter on that subject -- is that we still believe that the activities to modify the single failure criterion do not lend themselves well to the designation of an unresolved safety issue.

We prefer the approach of relying to a large extent on the IREP activities to in a sense test the efficacy of the single failure criterion as it has been applied by looking for areas where the application of that criteria has not given us reliabilities that we are comfortable with. And where we identify those areas, then move to determine what additional requirements ought to be laid on in those specific areas, either by making those individual topics in USI, as we have in fact done with the decay heat removal USI, or by taking other -- where the course of action is clear, taking appropriate action to generate new requirements, without going through the unresolved issue step.

But we feel a combination of those two building on the efforts and the learning process of IREP is the way we would propose to attack any supplementation of the single failure criterion.

MR. OKRENT: Now, suppose you were the designer of a plant of the type Mr. Denton does not think there will be, namely a future LWR in the next decade. Would you feel happy with the proposed approach that okay, we will design it according to the past staff criteria, including the single failure criteria, and then after we have it built, the staff is going to do an IREP or make us do an IREP and tell us what we should have done and how we should change it.

MR. SCHROEDER: You are asking me if I were the designer? If I were the designer, no, I would not take that approach. If I were the designer, I would recognize that the Commission is moving in the direction of trying to establish some safety goals. And I would expect that a reasonable outgrowth of any such safety goals would be the establishment by the staff ultimately of some reliability goals for certain systems in the plant.

And if I were a designer, recognizing the

Commission has not moved to that end point yet, I think I would set for my own organization reliability goals on given systems in the plant, and I would use the single failure criterion as a minimum requirement. But I would then construct my design to try to meet some established reliability goals of my own setting.

MR. EBERSOLE: Mr. Chairman, I have some real problems with the single failure criterion in this context. The industry ought to frequently, more or less ritualistically, use this sort of thing. And I guess I can argue by going to an extreme, they would be willing to hang the entire plant if it were suspended on a couple of magnets which were driven by micro switches.

I am not using that in the context that those are bad switches, but they are rather delicate. That is kind of the ultimate dependence on the single failure criterion. And they also apply them in such costly and weird fashions, these would be considered identical in the single failure context which is under no thermal stress, no anything. So you will see two such pipes arranged against two such pilings in a relative sense, unreliable supporting systems.

It is a total imbalance in the concept as we have it in the field, and I don't see that it is reasonable that we should just allow that to be perpetuated, that we certainly should require in the interim while we get this other IREP data additional considerations against just the bare minimum of single failure criterion. For instance, we could require diversity as a criterion in addition to redundancy.

I have somewhat of a conviction that the recent incident at Browns Ferry certainly in its implications must extend into the PWR prospects. And I would like you there to perhaps look at the undervoltage details as a case in point.

These devices which are elements of the power circuit breakers in fact may have in many designs typified the minimum use of the single failure criteria against a very heavy responsibility in safety.

MR. OKRENT: Well, I can, I guess, understand that for plants in operation, some kind of probabilistic analysis to the existing configuration would seem to afford perhaps the best way of looking at the plant. And it is not probably too helpful to try to generalize the situation and develop some kind of new criteria for its design. It is already designed and built.

But unless the staff is not in any way able to say and does not plan to be able to say what the basis should be for the design of a future plant for some period of time, it seems to me that it should have some proposed modification if there is going to be any, of the current design criteria, general design criteria.

And I do not find that doing the IREP studies is going to do that for you. It may give you some background information, but it does not do it for you. And much as I have been pushing the staff to try to quantify the reliability of this and to tell me the determined frequency of that, it is not clear to me that you are going to get from here to there by some kind of quantitative approach in the assignment of this reliability of system A and that reliability to system B. I don't think that either is going to work.

So at the moment it sounds to me like there is, you know, not even the beginning of an approach within the staff as to what should be done different, if anything, with regard to the single failure criterion on, say, new plants to be designed. To keep it out of this NCP area is quite complicated.

MR. CASE: Well, I think our position here is mirrored and consistent with the position that Harold was talking about on new LWRs; that for the moment we do not have before us the new design requirements for such plants. We have a program for developing those requirements, which admittedly would take several years. I think that is true in the degraded core area as well as in the single failure area.

What at least some of the individual members' positions seem to be is yes, but you need something today for those new plants in the form of new requirements. Our view is more we don't understand that to be the case. It may be true in theory, but pragmatically or practically we do not know that there is anybody out there today who needs that information.

Now, admittedly, as Bill has pointed out, it might be a chicken and egg position that one causes the other. I don't think that is the case, but I would have to admit that is a possibility. But I think our views on single failure criterion sort of fit that approach, that we hope through use of the IREP studies we will be able to identify some specific weaknesses in following the single failure criterion as a minimum. And when those are identified, we will develop requirements for those areas. And through this process over a period of years one would expect to see changes in the additional minimum requirements beyond the single failure criterion in specific areas, for specific events, or for specific systems.

MR. OKRENT: Dr. Kerr.

MR. KERR: Ed, I guess I don't see why one has to have IREP to identify weaknesses in the single failure criterion. I thought that almost everybody recognized it had weaknesses. The question -- maybe that is what you are saying -- what do you do about it?

And it seems to me what you try to do about it is not to replace it immediately because you probably cannot, but at least to move toward a combination of maybe use of single failure criterion judiciously, which is what I think you now do, and of consideration of multiple failures and situations in which experience indicated they could be important.

Maybe this is what you get from IREP, an idea of where multiple failures can make a contribution. If that is what you are saying, then that seems to me is a lesson to learn.

MR. CASE: That is what I think I am saying.

MR. OKRENT: Well, I might say I could interpret what you are saying as being in effect that the de facto moratorium on construction permits, since the NRC staff is telling me at least that it is going to take several years before they can develop criteria with regard to reliability and degraded core cooling for new plants.

MR. CASE: Well, I would rather say it is a response to what we perceive as a de facto moratorium. Again, it is the chicken and the egg. I say it one way, you say it the other.

MR. OKRENT: In any event, Mr. Plesset has left us, and I see Mr. Siess has taken his seat. I think that maybe we ought to propose for the Friday meeting with the Commissioners, whoever is going to come down, this general topic to see if this is what the Commissioners think should be the situation and is the sitautien and so forth.

I don't know how you all feel, but I think it would be a useful possibility for a topic, and maybe we ought to ask Dr. Savio to chat with Mr. Fraley and Dr. Plesset today about this, okay?

MR. EBERSOLE: In connection with the IREP studies, I cannot help but recall our recent meeting on the incident at Browns Ferry and sort of contemplate in absence of that particular incident how much different the IREP type of study would be as contrasted to now what it must have to say about the presence of unknown common mode failures.

MR. CASE: I agree with you completely. That has been the history of each IREP study we have taken.

MR. EBERSOLE: What are the roots of that? Is it not perhaps inattention to critical detail?

MR. CASE: It is a lack of completeness. It does not consider all possibilities for, (a) deliberately, and (b) because we cannot think of them all.

MR. EBERSOLE: Is that not due in part because you must deal with such a variety of these problems?

MR. CASE: Yes.

MR. EBERSOLE: Doesn't that suggest that if you did not have to deal with all of these but you had a rather rigidly controlled set of problems to deal with, you could do better?

MR. CASE: With fewer problems one can always do better, yes.

MR. EBERSOLE: And then of course --

MR. CASE: That leads to standardization.

MR. EBERSOLE: It does indeed.

MR. CASE: Which is not of much help for the 70 operating and the 90 under construction.

MR. EBERSOLE: No, it is not for those, but somewhere off in the distance I cannot help but think --

MR. CASE: But let's not paint standardization as a panacea either. The committee dealt with the pros and cons of standardization in a letter it wrote twelve months ago.

MR. EBERSOLE: What you have done up to now is truly not a standardization program. It is a random accumulation of sort of a variety of standard approaches.

MR. CASE: I think the standardization that you are talking about is more akin to the Navy program.

MR. EBERSOLE: It is in fact modified to fit the commercial program.

MR. CASE: All right.

MR. EBERSOLE: And I think their experience has been more successful than ours.

MR. CASE: I would hope so.

MR. EBERSOLE: I can't help but think that in the realm of design requirements in the presently existing information available to you regulatory people, there must be a plant somewhere that could be identified that would not really suffer the terrible delays of creeping comprehension of what was in the field and learning about it as it emerged in detail, and then setting the plant back until in fact it takes 12 years to be built.

I suspect that might be an incentive to industry if you could say if you will be properly conservative and definitive in detail, we can cut you loose to build the plant in five years.

MR. CASE: I would hope that would be one of the --

MR. EBERSOLE: I would rather have one of those plants than one that I am presently building.

MR. KERR: You are talking not only about standardizing plants but standardizing the NRC staff, and that is a more difficult problem.

MR. EBERSOLE: I don't know really what you mean by that, Bill.

MR. KERR: You would have to get everybody on the staff to agree that the standard plan was a good plan. Otherwise, in the review process they would find things that were wrong with it, as any good reviewer will.

MR. CASE: There might be things wrong with it.

MR. KERR: Indeed there might.

MR. EBERSOLE: The idea that there would be not so many as we presently have, the field of endeavor would be greatly narrowed.

MR. BENDER: There seems to be something in this argument that says the standard approach will be -- the basic problem still remains if you do everything the same way, you are likely to miss the same thing in every plant.

MR. EBERSOLE: I do not adhere to that idea. The counterpart of that is just the reverse. If that is so, you can fix them all just as easily.

Concerning the single failure criterion and the present need to do something about it, isn't there some sort of need for an expressio' of deterministic approaches mixed with probabilistic requirements on particular systems and elements of a plant, and then perhaps some inclusion of a criterion on diversity of function that would provide a framework that we could all believe in for a while without waiting around for how many years?

MR. CASE: I think just doing the first is not a short, easy job. I am afraid that involves a year or two, and I just do not know enough about your diversity point to debate it with you.

We have always argued that diversity was desirable, but we have not made it a requirement across the board. It is more or a plus rather than a requirement.

MR. EBERSOLE: Mr. Case, I will give you a personal example of what is a concern of mine. We have argued about ATWS for a number of years. We have fairly good cause, at least in the BWRs, to say yes, there was a lurking thing, and perhaps there are others that we don't see now.

I think in looking at that we must rationalize in the PWR area there is also a similar area perhaps which you can look more sharply to find out what it is. And on this matter of diversity I have long been bothered by the fact that one can take the top off power supplies and probe into the guts and find weakness in those designs which are not looked at by the regulatory people, and which may in fact be the counterpart of the Browns Ferry weakness.

I would invite you as a case in point to look at one of the elements inside these things which are the undervolted relays which pilot the mechanical functions of the main breakers. And note in so doing that you have in these small devices, grease, springs, and other things which you put in in simple redundancy in our plants; and it is a very effective way to invoke common mode failures.

MR. CASE: That I will agree with, but I don't quite understand how diversity -- you put together the same switches, oil, coils -- you put together those things and solve the problem.

MR. EBERSOLE: One can apply diversity --

MR. KERR: He has not convinced me either.

MR. EBERSOLE: This could be overcome to a considerable degree by diverse requirements. In this particular case it is just one point, and I really think we should look at the PWRs since the Browns Ferry case. I think it will be in detailed, not in general criteria.

MR. CASE: Then we get back to the many years of discussions we have had on ATWS. Is that the way to approach the problem, try to ferret all the common mode failures or to mitigate the problem and take tha direction? And we we have sort of chosen a reasonable combination of both.

MR. EBERSOLE: I don't see any departure from that.

MR. OKRENT: Well, in fact I think in a sense what Mr. Ebersole was referring to is an area where the staff has changed its position over the years, and many people. There was a time when I think you were proposing to try to look at the weak spot, if there was one, in, let's say, the Westinghouse systems, and to see whether they should be made more reliable.

Your most recent decision was that needs to be done because you have mitigative features. In fact, I think the ACRS position, to the extent there has been one over the years, has been to look at both mitigation and prevention on ATWS. I guess there is some skepticism that either one is going to be --

MR. CASE: Completely successful by itself.

MR. OKRENT: Exactly.

With regard to the single failure criterion, we have been focusing it on future plants, but it does relate to plants under construction which are in various stages and let's say the NTCPs. Do you have anything in mind other than there is an IREP program with regard to these plants and how adequate or not the single failure criterion is?

MR. CASE: You have two classes, the NTCPs and the plants under construction.

MR. OKRENT: Separate them in your discussion, if you wish. I did not hear any major separation in what I had heard.

MR. CASE: If I remember correctly, and I may be wrong, on the NTCP, as you pointed out, there are some specific systems, not all, where the requirement would be evaluate the reliability of selected systems. One of the additional requirements on the NTCP --

MR. OKRENT: It says evaluate.

MR. CASE: That is a beginning.

MR. OKRENT: They will not meet the single failure criteria on any sa[illegible] list of --

MR. CASE: The[illegible] ill meet it, yes.

MR. OKRENT: So.

MR. CASE: Beyond that, evaluate the reliability of those selected systems,, and then what flows from that we are not in a position to say. We have taken a step in the reliability direction by getting information for that class of plants.

I do not believe, and correct me if I am wrong, Frank, for the plants under construction we have anything more in mind than what might follow from unresolved safety issues. And the IREP studies indicate this same sort of thing.

MR. SCHROEDER: That is right. There is some long-range planning about an NREP program which would eventually get around to all the plants.

MR. CASE: Whenever that might be.

MR. OKRENT: Now, what is it you envisage may arise, let's say from the ongoing efforts, whatever they are, as systems are looked at, if you find --

MR. CASE: Or events are looked at, and you find that, as Frank put it, you are not comfortable with the results. You will either then develop some specific new regulatory requirements to take care of that problem or uncomfortableness, or if the cure is not completely obvious at that point in time, designate that issue as an unresolved safety issue.

MR. KERR: Let's take auxiliary feedwater sytems as an example. There was a considerable look at that, and eventually some people asked to make changes. I am not sure on what basis people were asked to make changes. Was there developing during the course of the looking an engineering judgment that systems with a reliability less than something ought to be fixed and the others were okay?

MR. CASE: It was not a specific number. We categorized them more in terms of high, medium and low reliability, if I remember correctly, and I believe moved on both the low and the medium, with some differences in time phasing.

MR. KERR: If one were not intimately involved in the process as the staff was, one could conclude the decision to improve everything was somewhat arbitrary. I mean how reliable should auxiliary feedwater systems be? Should they be ALARA in the sense that you pick something that is available and you say everything else ought to be that reliable, and it is possible? Or does one look at the contribution of that to overall risk, or is there --

MR. CASE: It was more that approach tempered by judgment. The same class of other events that could lead to core melt.

MR. KERR: Not just the staff, but the people who are responsible for eventually making the changes. If they can participate and understand the process, it seems to me that the final result is likely to be better.

MR. CASE: I agree, but that takes quite a long time.

MR. KERR: Nobody understands the process except the people who sort of put it together.

MR. CASE: And then you are accused of prescriptive approaches.

MR. KERR: You are not accused of being prescriptive.

MR. CASE: If the other parties don't understand --

MR. KERR: You explain your philosophy and approach, at least so people can understand how you got there.

MR. CASE: You are less prone to it, but it would not eliminate it completely, I don't think. I agree with you basically, and I think the IREP studies are designed to be that way. There is a lot of communication with the licensee and the discussion of the objectives and the way it should be approached, and that is an attempt to let everybody realize the common objectives.

MR. KERR: Let's take another subsystem. How would one decide what the appropriate reliability finally is? Will it be sort of an ALARA thing, or will you look at the best ones and say --

MR. CASE: No, it is more to reduce the dominant paths to risk.

MR. KERR: To reduce it to what?

MR. CASE: To the same level as other existing paths. And it is more or less an iterative process. I assume one could go through this and get them down in the same range, and then on a second pass attack those that stick up just a little bit.

MR. KERR: No. But is there an eventual goal or are we still far away from that goal, and right now we don't have to worry about what it is?

MR. CASE: I think the eventual goal would be a safety objective and then matching the two approaches. But that is a long time off, and one has to do in the meantime.

MR. BENDER: The single failure criteria (inaudible). Is there any systematic effort to determine whether it is being applied rationally?

MR. CASE: You say rationally. Blindly. Not whether they are meeting it but whether they are meeting --

MR. BENDER: I don't think it is being applied blindly. I think that is --

MR. CASE: Jesse described a more --

MR. BENDER: I think that is overcritical.

MR. CASE: I think IREP is designed to find that kind of application.

MR. BENDER: Let me illustrate with an example that everybody knows about. Diesel generators are often dealt with with a single failure criterion device, and it is such a big system that it challenges credibility to believe you could take a unit and just say one unit has to be operable.

It means there may be a need to break down systems. Jesse has argued that you really need to go into great detail. I don't know where to draw the line, but it seems to me there ought to be some effort made to determine how large a system can be dealt with as a single failure system.

So far we have not been able to tell if there is an effort in that direction.

MR. CASE: I understand conceptually what you are suggesting, but I don't know of any effort along that line.

MR. BENDER: It seems to me that the systems need to be broken down further. I don't know whether I want to go to --

MR. CASE: You are right. Brown's Ferry is a good example.

MR. EBERSOLE: (Inaudible) It is an unfortunate experience.

MR. CASE: I think it is fair to say the staff never conceptualized that system to be so interconnected as it was with other systems. The thought did not cross their mind in their review.

MR. EBERSOLE: Even the scram breakers are a complex system, even inside one can. An experience record can delude you into thinking there is nothing in there. I think that was the Brown's Ferry case. It had 15 good years.

MR. CASE: It was certainly a surprise to us, trying to rationalize that failure with the success rate on BWR scrams.

MR. EBERSOLE: Somehow it casts doubt on most statistical studies of that sort if they don't look into specific detail.

MR. CASE: Yes. Lies, damn lies in statistics.

MR. OKRENT: Well, maybe this is related to the item e which /s Basis for Developing Useful Quantitative Criteria for Allocating Resources to and Resolving Regulatory Issues. Do you want to comment on this?

MR. SCHROEDER: I think you have gotten over into that one with Mr. Case's answer to Dr Kerr that we really have not yet established quantitative criteria; that what we are seeking to do at the moment is to look at the differentials and major contributors to risk and try to push those down into the drafts of the overall risk statement in terms of --

MR. KERR: Frank, if you are talking about major contributors to risk, how do you establish this, by looking back at WASH-1400 or some revised version thereof, or --

MR. SCHROEDER: I think we have little choice in our present configuration but to use WASH-1400 as a yardstick.

MR. CASE: But recognizing its limitations and faults as best we can, to the best we understand them.

MR. OKRENT: I guess I don't know what it means to say, well, we will recognize its faults and so forth but we will use it.

MR. CASE: You use it with whatever grains of salt are appropriate to those reservations. For example, WASH-1400 does not advertise itself to take adequately into account operator goofs such as happened at Three Mile

Island, operators doing the wrong things, as distinguished from operators helping the situation with little or no credit given to that help.

I think that is a factor one has to take into account when looking at WASH-1400 results on dominant risks.

MR. BENDER: (Inaudible) way in which the unreliability numbers were put together.

MR. CASE: I don't know. I can let Frank talk about it. There was an allowance for -- not the kind that happened at Three Mile Island.

VOICE: There is a little bit of truth on both sides of this argument. Operator goofs are implicit in the data and the models used in the reactor safety study and comparable studies in context, like maintenance areas, on motor operated valves showing up in the failure rate of the motor operated valves. Maintenance and operator errors that show up in the LER frequency for components certainly are dealt with.

There was an attempt made in the reactor safety study and serious attempts being made in the current IREP studies to look at operator error in a rather broader context, once an accident scenario has been identified, to think about whether or not the operator might misconstrue his indications and believe he was dealing with a situation different from that with which he is really faced, which, of

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course, is not implicit in the data base for components but can be to some extent predicted by simply putting yourself in the operator's shoes and saying this is the signature of signals I am facing; can I misconstrue this for another scenario the procedures for which would be counterproductive?

You can look for exposure that way, and we are attempting to do that. Of course, this is an area where our presumption to completeness is about as limited as it can be. But you do have something to grasp when you have event sequences defined and scenarios defined.

MR. OKRENT: Mr. Kerr.

MR. KERR: I don't know how to put this, and I am not sure it will be a question or a statement, but I have been impressed at a couple meetings recently by staff reaction which led me to believe that at least some fraction of your staff is very skeptical of risk assessment techniques, and perhaps with justificaion.

In one case a presenter really sort of came out and said I don't think that will ever be practical and we cannot use it. In other case, in a SECY paper there was a presentation of one viewpoint which had resulted from a PAS assessment of a situation which, in effect, said you could probably reduce this risk this way, but there are other risks in this plant that are much more contributory and we think the resources ought to be spent that way.

The response from some other part of the staff was, in effect, well, they did not really look at everything, and if one had a certain type of accident and had, after the accident progressed, somebody standing outside of the Torus playing the fire hose on it, this would change things a lot.

I am really curious as to whether you believe that some significant fraction of the staff outside of PAS really takes risk assessment very seriously.

MR. CASE: Yes, I think they do. In particular -- I don't know what the name of the branch is. Frank's group is chartered just to do that, and they really were the author of that more skeptical risk-based analysis that you spoke of.

MR. KERR: I seems to me there are two extremes. One is you establish your position and then you use risk assessment to reinforce it if it works, and if it doesn't, you ignore it. The other is that you try to use risk assessment in reaching a decision. That and other things --

MR. CASE: I think we probably do both.

MR. OKRENT: Mr. Ebersole.

MR. EBERSOLE: I was going to take a little exception to the identification of the TMI-2 incident as wholly an operator area. I consider it an engineering error as well. Operators tend to be people who do what you tell them to do. If you tell them to look at a parameter displayed on an instrument, they truly believe that is what is being displayed, so their actions will follow that.

They don't attempt to analyze the plant in an engineering context. I think it is a little disservice to operators in the narrow context I use them, not including the whole utility as an operator, to say they did a number of wrong things.

MR. CASE: They were certainly contributing factors to those wrong decisions.

MR. EBERSOLE: I find a similarity between the accident at Brown's Ferry and the one at TMI-2. We are dealing with an instrumentation problem wherein the instrumentation is not revealing the parameter of interest in that. This is a disservice to operators in giving them stuff they should not have, and somehow or other we have to quit doing that or at least qualify the information we give them, which may lead to some lack of faith in what they are seeing. But that might be of value.

MR. BENDER: Which Brown's Ferry are you talking about?

MR. EBERSOLE: (Inaudible.)

MR. CASE: The instruments did not indicate.

MR. EBERSOLE: It is a little like TMI-2. The instrument volume portrayed an (inaudible.)

MR. ETHERINGTON: There has been a big change in the design of that volume in the direction definitely towards safer designs. Is it possible to have a sneaking suspicion that somebody might have recognized this weakness in the original design and not revealed it?

MR. CASE: I do not know of anything that could support it.

MR. EBERSOLE: The current G.E. designs will look like the modifications being proposed on the older plants. Somewhere along the road one can argue that the shortcomings of that design were, in fact, known and left standing, while in the new plants the BWR 6's were perhaps fixed.

MR. CASE: I looked at it as a --

MR. EBERSOLE: The new designs proposed to fix the shortcomings curiously look like the present designs coming out.

MR. CASE: Are not the more NSS portion -- isn't it more a heavier portion NSS design? I don't know.

MR. EBERSOLE: Not that I know of.

MR. CASE: You are not talking about the MARK III's, then.

MR. EBERSOLE: I am merely saying that the new ones --

MR. CASE: All right.

MR. EBERSOLE: -- have overcome the shortcomings of the old one, apparently.

MR. CASE: At least on paper.

MR. EBERSOLE: On paper. I am not sure about the venting. The matter of measuring the parameters of interest is a factor. One then wonders how does it get improved. We are reevaluating what had been done and possibly finding standing deficiencies.

MR. CASE: It is an interesting question.

MR. OKRENT: Can I come back to the wording of the topic we are nominally discussing, basis for development and use of quantitative criteria for allocating resources to and resolving regulatory issues. Earlier Mr. Denton told us that the staff were really all very busy already and they weren't really the sources to look at NTCPs, and certainly not to look at future LWRs.

I would like to raise the question within the context of the plants in operation and under construction. Have you done some kind of a review that tells you that the resources of the staff and that the industry in response to the staff is allocating to the issues in the Action Plan which are defined and which require action, as distinct from the issues which remain to be studied or resolved in some way; that, in fact, this represents a proper allocation?

In other words, that the things being done represent something that is sort of a near optimal allocation?

MR. CASE: Well --

MR. OKRENT: If so, where can I find this document?

MR. CASE: I guess the answer is in proposing to the Commission what actions should be in which category, i.e., done immediately, studied some more, put on the shelf. This balancing was implicit in that process.

It was first, just from looking at the items themselves, and then having reached tentative decisions of which boxes they ought to go into. Then we priced them out in terms of staff manpower and staff manpower availability and made changes based on that, and then reprogramming of staff resources based on the combination, and went to the Commission, and they approved that allocation, that categorization, allocation of manpower and categorization of issues to be worked on.

Then that has been accepted as the current plan and then variations from that plan as they come up from a host of sources, including the ACRS, including Carl Michaelson, including our own review of operating experience and what have you. Those new issues are then considered by a group in Frank's division in terms of manpower costs and risk reduction potential, and decisions are made on working on them at a given pace based on those results. And that is the system that I think you are asking about in this particular question.

As an example, one of the things that we most recently looked is something the ACRS called to our attention: the lack of seismic qualifications on some plant feedwater systems, some ten in number. And an evaluation was made of that in terms of risk reduction potential and probabilities of getting into difficulty, and a recommendation made to the office director as to what kind of priority to assign to that item.

The answer that Frank's group has come up with is generally that it is not an immediate safety problem, that it is a problem that should be worked on over a reasonable span of time and measured in years, three years.

Now, you all may not like the answer, but the process used in coming up with that answer is one that we are trying to describe.

MR. EBERSOLE: That's the case where the aux feedwater systems were found not to be competent, right?

MR. CASE: Right.

MR. EBERSOLE: The backup systems, if you did not have auxiliary feedwater, were thought to be manageable under the circumstances of alternate operation involving bleed feed. Of course, that is not the case either. What that really means is one can say those ten plants from a seismic point of view are bare for three years.

MR. CASE: My understanding is it is not quite that stark, and there will be some verification made of the bases for the analyses that Frank's group went through by site visits.

Isn't that part of the program, too, Frank, to verify --

MR. SCHROEDER: There was a two-stage -- there was this interim assessment which on its surface indicated that three years was an appropriate time span to resolve it, but in fact that interim study was only used as a basis for saying it is all right to go ahead and do a more detailed look at the risk during the three-year period.

MR. EBERSOLE: I am curious about that because it appears to me it would take a fairly minor effort to upgrade the bleed feed reliability potential as against improving the aux feedwater design to make them at least conceptually operable under the circumstances that they will create themselves if you bleed feed.

Do you follow me?

MR. SCHROEDER: Yes. And I think that is one of the options in the three-year study, to decide --

MR. EBERSOLE: Will you give them three years to upgrade a couple of valves and a few wires?

MR. SCHROEDER: That decision has not been made yet.

MR. OKRENT: Do we have the memo --

MR. CASE: I don't think we have sent it down yet.

MR. OKRENT: Why don't you send it down when it exists? I am curious.

MR. CASE: Basically, the matter is before Denton for a decision, and when he would make his decision, what to do about it then, and then we would respond to your letter saying here is the decision, and the basis for it presumably is contained in the attached analysis.

It is just orderly business. As to why --

MR. SCHROEDER: Did you have something?

VOICE: I hope I did not misinterpret the conversation, but I believe you are aware of the experimental program underway, supported by the utilities, in terms of the behavior of the valves under subcooled and two-phase flow conditions.

That information would be extremely useful in assessing the feed and bleed characteristics of a BWR. The basic functionability demonstration of the valves is to be completed, I believe, by July of '81. It seems to me that that would be the most significant piece of information that would be input in any assessment as to the capability of the plant to withstand feed and bleed.

MR. EBERSOLE: I think you are talking about the physical performance of the valve in its ability to deliver two-phase flow.

VOICE: As well as the piping.

MR. EBERSOLE: As well as the piping, which is somewhat a more exotic topic than I was referring to. I was talking about the deterministic aspects of the design, which on that simple basis at the present do not allow the valve to be claimed as a viable alternative in the presence of a containment environment in which you would induce that bleed feed operation.

VOICE: Yes.

MR. EBERSOLE: In terms of the arrangement that exists at present.

VOICE: Yes.

MR. OKRENT: Could I ask whether the position recommended by Mr. Schroeder's group falls in the pattern recommended by Mr. Bernero to Mr. Mattson in a memo dated July 20, 1980, about possible time periods for taking regulatory action?

In other words, there was a suggestion that if something was larger than 10 to the minus 2, you fix it in days, and between 10 to the minus 2 and 10 to the minus 3 you fix it in months, 10 to the minus 3 and 10 to the minus 4 per year you fix it in years.

Does this seem to fall in that pattern in your opinion? If not, where do you think it falls, Mr. Schroeder? Since you suggested that three years would be okay, I would like to get your concept is involved that could be fixed in three years.

MR. SCHROEDER: I'm afraid I'm not conversant enough with that risk study on the feedwater system to tell how it fits into Bernero's list. Do you understand?

VOICE: I think I understand. I do believe I know the memorandum you are referencing. I can only give you an example which you are familiar with; that is, the question of anticipated transient without scram.

MR. OKRENT: I would like to stay within this current topic just for the moment. I am told that there is a recommendation working its way through the staff that the non-seismic qualified auxiliary feedwater system in plants is something that you can spend a few years -- three years I think was the number mentioned.

All I am trying to learn is what is the staff estimate of the possible contribution of this to core damage in, you know, chances per year, so I can calibrate your recommendation. If you tell me you did not make any quantitative assessment, that this was judgment, then I guess I will have to leave it at that, but if you have done it --

MR. CASE: There was a quantitative assessment.

MR OKRENT: Tell me how it came out.

MR. CASE: I guess we don't have the right people here.

VOICE: I don't have the report.

MR. CASE: Why don't we move from this question, and we can get copies of the memo?

MR. OKRENT: Okay. Let's look at -- that is also part of this general topic which is the use of quantitative criteria for resulting regulatory issues. Here is a specific issue. The best way to talk about is in terms of the specific case, that is right, except I did not know that myself.

MR. OKRENT: I am just trying to see whether the staff is adopting these recommendations by Mr. Bernero, or they have some other set of criteria. It seems to me if you have a set of criteria which you think are reasonable to use in connection with action, you ought to advise not only the members of the staff but the ACRS and the public, the Commissioners, and so forth what it is you think is reasonable to use.

MR. CASE: I would agree, but I would doubt that the licensing position would be as definitive -- there would be more overlap. Basically, these tools are used as an aid in judgment rather than the yardstick.

MR. OKRENT: You see, I am trying to understand, and I assume now know, based on reading the memo and our discussion, why you think three years is okay here. But there were five plants, for example, for seismic matters that needed to be shut down while they did certain things.

MR. CASE: It only deals with the one subject.

MR. OKRENT: Since they both involve the probability of earrthquakes, you know, most talk about it in a comparative sense and --

MR. CASE: The older we get, the smarter we get.

(Laughter.)

MR. OKRENT: Well, are there some other set of quantitative action criteria, not Mr. Bernero's?

MR. CASE: No.

(Laughter.)

MR. OKRENT: Well, are Mr. Bernero's used by the staff now, the licensing staff?

MR. CASE: Not to my knowledge. I assume they are used as everything else is used. People consider them, but there have been no directions, nor do I know of any staff view on how they ought to be used specifically.

MR. OKRENT: Well, then, should there be some kind of criteria within the staff for action, in other words, where you think you can quantify something with some certainty that this provides a basis for judgment?

MR. CASE: It would seem difficult, it not impossible, to me to develop such criteria apart from the overall safety objective.

MR. OKRENT: Which is what?

MR. CASE: Which is being worked on by, among others, David Okrent's subcommittee.

(Laughter.)

MR. OKRENT: I have to disagree in fact because that objective in fact, if one works on it, might be different for operating plants and plants be constructed; and it may be in frameworks which are quite different than what would enter here.

In any event, these are real questions. You are faced with operating plants, and you do take actions either to shut them down or not to shut them down or require things by a certain time. And I guess it is not clear to me -- has not been clear to me when you decide that something can go on for a long time or when you decide that immediate action is needed; you know, how this judgment is derived from a risk point of view.

Well, before leaving the subject, there are some things in the Action Plan that were acted on quickly, and there are some things that are still in the form of studies. IREP, the reliability kind of work, degraded core cooling, all fall into that category.

If I wanted to be cynical, and I sometimes am --

MR. CASE: Or give the appearance in any event.

MR. OKRENT: -- I could say things that you decided on are things where -- or things where you made requirements are not necessarily the most important things. They are things that would be easiest to decide on.

MR. CASE: Easiest to do or to decide on?

MR. OKRENT: Decide on. We have a hundred things -- a hundred necessarily no longer -- in fact, that is in part related to Dr. Kerr's comments earlier about allocation of resources. So I cannot tell whether the package of a lot of small things are all of equal importance or in fact there are things you really wanted to put in or others that you want to decide on. And these other things that are more complex may in fact have greater potential impact on the safety of plants. They get more complex, and we are proceeding slowly.

And that is again related to this question of the allocation of resources, as well as the decisionmaking. At what pace do you ask that something be remedied if you think something needs to be remedied?

Can you help me on my skepticism?

MR. CASE: Let me try. I think some of the early on items are less important individually than -- from a safety standpoint -- than some of the items that were relegated to study, and indeed are still under study.

Given that fact, the way one decided whether one could license pending the results of those studies, recognizing their potential importance, was to consider the aggregate risk reduction potential of the steps that were immediately taken in a judgmental way to see if that provided enough assurance of safety during the period of time when the longer range studies were being done.

So although part of the rack-up was done, because of the more pragmatic approach to the problem, it was compensated for by considering where that led you to before a decision was made as to whether plants could or could not operate during this interim period of time while the more detailed and longer term studies were being taken.

MR. OKRENT: Well, I gather you feel you have answered the question. Let me restate it, because I don't feel I know what the answer is.

MR. CASE: I think -- let me give you an example of that with some fear and trepidation. I think our position on the ice condenser problem is reflective of those considerations. We believe that deciding what to do on ice condensers in terms of hydrogen control is a complex question that requires and deserves a period of study before making up your mind on what should be done from a hydrogen control standpoint on ice condensers.

Given that fact, we had to ask ourselves whether it was reasonable to allow the plant to operate in this interim period of time, the new plants and the operating plants. And it was our conclusion yes, that it was, because of the judgmental consideration of all of the steps that had been taken that in our mind reduced the probability of getting into that sequence where hydrogen control was a real safety issue.

MR. OKRENT: That is an important question, and it touches on part of the question I am posing, but I think in a sense not quite essential. I would say in one way it touches it is the following.

In May of -- what year was TMI?

(Laughter.)

'79? In May of '79 one could have anticipated that there were some questions concerning hydrogen generation for ice condenser plants, so one did not have to wait until the summer of 1980, as it were, to try to develop information quickly or whatever to look at the technical aspects.

I think one could have allocated resources. There are some things that sort of stare you in the face, but -- well, let's see. There was one other item Mr. Schroeder was going to touch -- basis for developing schedules for completion of IREP-like studies. I don't think you have told us that.

MR. SCHROEDER: I think I would like Frank Rouse to answer that.

MR. ROUSE: As you are aware, the first IREP study of Crystal River has been out in draft reform for peer review since late spring. We had sent the SAI team for rather extensive rework. I would imagine it would be -- (Inaudible). In parallel with those studies w will be refining the procedure and schedule guide (Ina ible), and we expect by next spring to have a guidebook on how to do such studies on the street, and would expect that NRR will ask licensees to perform IREP-like abbreviated risk assessments.

I should say probabilistic safety analyses because they are not full-fledged risk assessments, using that guidebook commencing next summer.

MR. OKRENT: Yes. I guess the question on the agenda is the basis for developing schedules.

Now, we have heard Mr. Rouse give some thought as to what he thought might be the schedule. On the other hand, he is from the research office, I guess, and I have to assume that somehow NRR would be the one who would be making recommendations or decisions, whichever they may be, on probabilistic safety analyses of either operating plants or subsequent operating plants.

How does NRR decide whether or not such studies should be done and on what time scale by operating plants or plants under construction?

MR. SCHROEDER: As far as the decision on what plants should do them, this is the subject of some considerable discussion in the staff at the moment. There are probably four or five different approaches to criteria for deciding which plants should do risk studies in what order. Some primarily are demographically based, and others are based on vintage and so forth.

There are a number of approaches. We don't have a unified position on that at the moment. There are people in DST who are currently charged with developing a set of clearly stated objectives as to what we are trying to accomplish by doing such studies, and then translate those into some criteria we can all agree on for selection of plants and in what order.

That work is not very far along, although there are a number of opinions that have already been expressed by various parts of the organization. But the integration of those into an agreed upon set of criteria is not very far along. I don't really know what more I can say about it at the moment.

MR. OKRENT: Is there a schedule within NRR for arriving on a --

MR. SCHROEDER: We have charged Mal Ernst and his group to get us a paper with those objectives and criteria for management review in something like the next month.

MR. CASE: The risk type studies that Frank is talking about include IREP as well as Class 9 studies. It is broader than just IREP studies of particular plants, but it will include which plants should have IREP studies.

MR. SCHROEDER: Yes, yes.

MR. OKRENT: Okay. Now --

MR. SCHROEDER: This, by the way, as we would envision it would ultimately be the basis for beginning to plan the systematic evaluation program mandated by the Bingham Amendment to the Appropriation Act, where we are -- our intention is ultimately to review all of the plants, but recognizing that we have to have some set of priorities as to in what order you have to do that review.

MR. OKRENT: If I recall correctly --

MR. SCHROEDER: One element is IREP-like studies, but that is obviously not the only element of that systematic program.

MR. OKRENT: Does the Bingham Amendment require IREP-like studies?

MR. SCHROEDER: No, and --

MR. CASE: Including a comparison with current -- regulations and current regulatory positions.

MR. OKRENT: The single failure criterion would be part of it.

MR. CASE: Yes. The overall systematic evaluation plan could include such a consideration.

MR. OKRENT: But it hasn't.

MR. CASE: It has not so far.

MR. OKRENT: That is right.

MR. CASE: In other words, Bingham directs us to do a systematic evaluation of all operating plants. It requires that we include in that program a comparison of compliance with the regulations and how the compliance is achieved. It does not dictate this is the only way one could complete a systematic evaluation program; and we do not intend to use that as the only measure of the safety of operating plants. It will be one of the inputs into that program.

Others will be IREP and perhaps single failure criterion, but at least we have not gotten to that point. It will also probably include some of the or perhaps most of the issues that were examined in the existing systematic evaluation program.

MR. OKRENT: Jesse.

MR. EBERSOLE: If I understand what you are saying, you are going beyond just ascertaining the single failure criteria, whatever that is, and ascertaining in fact --

MR. CASE: I said we may.

MR. EBERSOLE: Having just said that it meant that, it does not convey a lot of confidence to me that it is a good plan, if it met the single failure criteria plan in all aspects since there is --

MR. CASE: I would hope that a issue-oriented approach might pick up weaknesses in application of the single failure criteria.

MR. EBERSOLE: If I can go back to the earlier discussion on the feedwater system and the seismic case and the reference you made about studies to -- on the relief valves, the original function of these relief valves and the association block valves was considered to be non-safety in the concept of relieving pressure, because that was carried out by the safety valves.

The idea that you could get two unqualified valves to serve the function of reclosing even though neither were safety-grade caliber seemed to support the idea that a multitude or at least two non-safety grade devices would serve a safety function -- in this particular case close the primary loop if you wanted to close it. That is the idea of closing it after you had a non-safety release.

We have found that the responsibilities of these valves are more important than we originally thought, and so now we are putting a lot of effort on them. In a way I think it is like asking a mouse to carry a horse's load in the way we want improvement of these valves, namely the PORV.

We have perfectly good valves that will forcibly and reliably relieve the primary loop in redundant and in parallel configurations if we really want do that; but you go out on the market now and buy those things and handle the primary release through that without any of the horrible R&D programs that we have underway and the questionable results that have come out of them.

There is Arkansas Unit 2 which for some reason that I don't understand has put in some valves that I don't know the nature of -- I suspect they are pretty good valves; at least they are said to be safety-grade -- by which the operator can reach out and manually pull down the primary PWR loop at will in case he wants to do bleed feed. And somehow the operator in this instance or the utility found it advantageous to put these provisions in there.

One of the interesting things is, though, for whatever reason they bypassed all these years of effort that we talk about, so necessary to make decisions, and in fact they did not like that.

VOICE: I will give you my understanding of Arkansas 2 design considerations. I believe Combustion is the NSSS vendor, and they have had some trouble with leakage through the power-operated relief valves; and they decided to get away from those valves, since they, in theory at least, did not perform any safety function.

However, they recognized the need to, on occasion, be able to blowdown the system through some valve arrangement, and they decided to put, I think, a three-inch line.

Now, most pressurizers, if I understand the designs correctly, do have small lines. I think they are like one-half inch lines. On the ANO-2 design I believe there is a three-inch line, and there is indeed a valve. I would suspect that they would also have an isolation valve to satisfy certain criteria that indeed through which they could blowdown.

It is not clear to me at least that that is a better system than having two power-operated relief valves, if indeed they are shown to be able to function properly under the subcooled liquid conditions, because there are some events, some scenarios where it would be helpful to have this additional overpressure actuation capability which is automatic.

The ANO-2 system is totally manual. I am not suggesting that that is worse or better. All I am saying is there are some pros and cons of the two systems; and that you are quite correct, I do understand that ANO-2 has a different kind of scheme. Whether that valve is qualified for water relief is not yet clear to me. I doubt it.

MR. EBERSOLE: I think I am saying that if you wanted to be qualified, there are valves commercially available that would be qualified. And the fact that we delay so long in the R&D program trying to make a device which is intrinsically unsuited for the purpose now intended strikes me as a little ridiculous when we can go out on the market and buy what we need and put it on the plants in short order if we think we need it. I mean, I find it a little ridiculous.

VOICE: I think I appreciate your point.

MR. EBERSOLE: There is a lot more money spent on that than buying good valves in the first place. Do you follow me?

VOICE: I am not clear in my own mind about structural considerations as well as the question of safety valves themselves which could also be exposed to subcooled water.

MR. EBERSOLE: Safety valves are another subject. These are the PORV and the relief valves -- PORV and block valves.

VOICE: The key point was the functionability of the valves included safety as well as power-operated relief valves. The basic requirement was they had to do it for safety valves, and we thought it was just as well that the facility would be there. The capability is there. They ought to test the relief valves also.

So the testing program would have required, even if they had decided to go out and buy some other kinds of valves to replace the PORVs --

MR. EBERSOLE: Are you telling me that the testing program embodies testing of safety valves in two-phase flow relief?

VOICE: Yes.

MR. EBERSOLE: Safety valves as well as PORVs?

VOICE: Yes.

MR. EBERSOLE: It could be safety valves only.

VOICE: It could have been if they had decided to go to different types of valves for a relief function.

MR. EBERSOLE: The single failure criterion, let me ask you about the spread of the continuity and concept -- the incredibility of failure at one end of the spectrum. I can start with -- maybe a good model is the idea that we have wrestled with for a number of years, that if we have sufficient QA, good quality control, we can in fact take such a thing as a steam line and invoke incredibility of failure in its design.

And I would like to get the PAS's view on doing that sort of thing, because I think they could fully assure us, using the probabilistic route, that we are perfectly all right. But it brings up to the surface how far one should go in the PAS techniques against what I might call common sense and good engineering, which does not cost too much more, which is not invoked in the PAS philosophy.

Have I got the picture in front of you?

MR. CASE: In front of me. But I thought perhaps Frank wanted to answer it.

VOICE: I don't quibble with what you have said at all. I don't believe there is anybody in PAS who would argue that the probabilities that we are capable of generating are so robust that one ought to hang one's confidence in the public health and safety entirely on such matters.

I believe that we ought to be applying a concept of defense in depth above and beyond what one may infer by strictly probabilistic approaches. Probabilistic techniques will never predict, except conceivably in aggregate, the vulnerabilities or susceptibilities or weak spots in the individual power plants.

We need procedures, both in design and in licensing and operation to ferret these out and deal with them in ways that -- for which statistics is not adequate.

MR. EBERSOLE: It is an interesting use of probabilistic techniques. If one finds a flaw in the theory that you have redundant items which are not subject to common mode influences and are fully tested and carry the full implemented recipe for random failure, which is the intrinsic part of the single failure criteria, you are dealing with just random.

When one finds weakness in there, then the notable one which we ought to discuss this afternoon is the case of the isolation valves on HPCI systems.

MR. OKRENT: Can we put that off until this afternoon?

MR. EBERSOLE: I am saying in the arguments against doing anything about finding weaknesses in that design, what more often than not happens is the probabilistic technique is invoked as an argument in not doing a change in the design and considered comprehensive in that context.

I think that is a rather odd way of using it, as though it were in fact a comprehensive argument to defend a decision on a deterministic base.

MR. ROUSE: I certainly would not recommend its use this way on a forward fit -- in a forward fit context. In a backfit context, I think once we have licensed a plant, once it has been built, once it is running, once there is a substantial investment involved in it, that we have the responsibility to be as discriminating as we now are about backfits, and to order them in the most cost effective fashion when we judge them to be necessary. So under those circumstances I might embrace the use of probabilities.

MR. EBERSOLE: To a greater degree?

MR. ROUSE: To a greater degree. Before I would personally endorse a backfit order to fix a deficiency --

MR. CASE: That is certainly the vogue. I am not sure of the logic of that.

MR. EBERSOLE: It is the vogue.

MR. CASE: It is the vogue.

MR. EBERSOLE: I can't help but notice time and time again once you find a weakness, sure enough, a deterministic argument will come across as though that were the perfect way to do it.

Thank you.

MR. OKRENT: I am going to suggest we take about a five-minute break, and then we will come back to this item on the Browns Ferry event, because I see Mr. Stello is here waiting patiently and drinking coffee.

(Recess.)

MR. OKRENT: Let's reconvene.

Gentlemen, can we reconvene? I think the next topic is generic implications of the Browns Ferry event. With my glasses it says --

MR. STELLO: Implications.

MR. OKRENT: Who is going to tell us about this?

MR. STELLO: I believe we have some numbers about how the statistics might have changed as a result of looking at what happened at Browns Ferry, and what does that mean in terms of the likelihood of an ATWS condition.

What I thought you wanted to discuss and what I had been thinking about prior to coming here -- and if we don't need to discuss this, I'll skip it -- so let me ask a question.

Is a little bit about what was the basis for deciding to do what we did following Browns Ferry rather than, for example, shutting plants down, was that a consideration? And if we don't need to talk about that, I will skip that part of it. Was that a question?

MR. OKRENT: Well, the agenda item, generic implications, was intended to be a broad one where more than one aspect of it could be covered. It seems to me the one you have just identified would be one. There could be others that come to mind. So why don't you discuss those that come to your mind, and if that does not cover them all, we can raise any additional.

MR. STELLO: If you don't raise any additional questions, I will leave here very unhappy. I will not have -- you will not live up to my expectation.

Soon after we learned of the Browns Ferry incident, I came into the office and asked Harold Denton also to come in, and the purpose for us being there was to examine the very issue, given this event on Browns Ferry, what is the implication -- not so much on Browns Ferry since by that time we already knew it was all right, but how about the other reactors? What do you do with them?

Clearly the first thought is did we learn something that suggested that reactors ought to be shut down, because we found a problem we did not know what to do with. And as the day wore on it became clear that no, this was that part of the BWR system which we knew before and have that common feature for which a failure in that part could lead to a failure in the scram system.

All of the studies that I am aware of in the past pointed out that the drain system on the BWR was a common element and was subject to common mode failures, although the numbers that I recall indicated that the likelihood of that problem was very, very small.

This event clearly said those numbers were wrong. It really is not that small.

MR. KERR: I don't understand how one event can say the numbers are anything but zero.

MR. STELLO: I am not a statistician or married to statistics so closely. To me, if we were dealing with numbers that were on the order of 1 in a million, I would not have expected in my lifetime to have experienced it. Having experienced it, it suggested that perhaps --

MR. KERR: The probability of being struck by lightning is about that, and people are struck by lightning every year, and yet that does not change those statistics.

MR. ETHERINGTON: Isn't it the case of having an unexpected weakness revealed?

MR. STELLO: I am giving you a personal reaction. The frequency at which it was revealed was faster than I thought it would be; so at least for me it raised the question I think --

MR. KERR: The only reason I stopped is because it seems to me this is a very important issue in that when you say 1 in a thousand or 1 in a million to many people -- I did not think that was true of you -- that means it is impossible. And it is very important that we recognize when we say 1 in a million, it does not mean it is impossible.

MR. STELLO: I often think that when I buy one of these lottery tickets that some day I might win, although the chances are 1 in a million. I still buy it with the hope that I am it.

MR. OKRENT: If I can interject one comment, I suspect that a re-evaluation of the probability for the existing system, either at Browns Ferry or many other plants, would no longer lead to 1 in a million.

MR. KERR: That may be, Dave, but I think it important that one not say that the fact that something happens once immediately demonstrates that the earlier probabilities were wrong, because it just doesn't.

MR. OKRENT: No.

MR. EBERSOLE: There is an article that says it does not.

MR. KERR: The article is wrong.

MR. OKRENT: I think the original analysis was faulty.

MR. KERR: That may well be, but if that is the case, it will be demonstrated by things other than the fact that the event happened once.

MR. OKRENT: Yes. In fact, one of the generic implications I want to come to is the reliability of that original 10 to the minus 6 on this particular fault.

MR. EBERSOLE: Does it occur to you that that reliability value which existed prior to this incident with the extraordinary high reliability reflected in it must in fact have been based on ignorance rather than attention to detail of the design?

And does that suggest that most of such things are so unbased on detail --

MR. OKRENT: That is one of the generic issues I want to get at later. Go ahead.

MR. STELLO: The thought was nevertheless there that this is an area where we knew that it had that feature, so it certainly suggested that all BWRs ought to be examined in light of that experience, the issue being let us make certain for ourselves that this problem that occurred in Browns Ferry, for whatever the reasons, are somehow precluded from happening in other reactors. And we set in motion that day a list of requirements which were designed to do precisely that.

There were some additional things that were in there that were kind of catch-all of shutdown and scram your reactor twice, look at this system now very carefully, look at the way it drains water, look at the vent valves, look at the vent arrangements, look at the things which in any way could cause the Browns Ferry type of problem.

MR. KERR: Mr. Chairman, at the risk of being pedantic, I also would urge that one not preclude this, because I do not think you can preclude it. What you can do mainly is make a probability list.

MR. STELLO: I guess I did not -- I did not want to -- I should not have used the word "preclude." If it occurred, find it and correct it, so that it is corrected before there is ever a need to scram. That was the intent. If water is filling the headers, make sure you know about it so you can clear it out.

MR. KERR: I understand what you mean. I just think that in talking to people about what we can and cannot do, it is very important that we not try to convince people that having discovered something, we can fix it with 100 percent confidence. We cannot. That is the reason for all of these various levels -- not that I am telling you anything you don't know -- but I think it is important that our language not get across an idea that we do not mean.

MR. STELLO: Once I guess I heard Dr. Ross explain at a hearing that it is quite possible -- very unlikely, however -- that all of the oxygen over where you are sitting might come over and visit with me for a while, and you would be in trouble.

The likelihood of that is small enough so that neither you nor I need to worry about it. And "preclude" as I used it was used in that sense. We can cause a system to be fixed so that we do not need to concern ourselves any longer with that particular problem. Reduce its likelihood to a sufficiently low level where we are not concerned with it. That is the context that I meant it in.

MR. EBERSOLE: I heard you say we will do this to BWRs. It suggests to me that what now we ought to be looking for as well as fixing the BWRs in this aspect and recognizing this is only one aspect of failure, that we should say where is the dump volume in the PWRs, because I suspect if we look hard enough and in detail, we can find a dump volume that will fail.

One of the characteristics of the kind of test that we do that give binary results -- it worked or did not work; it was a red light or green light, or off or on. It really does not tell you what the margins of behavior were to get that result. You really never knew what last fraction of torque or drain rate or whatever was making you work all the time or not.

MR. STELLO: I understand why you raised the question. What I am trying to do is reveal the thought process that went on that Saturday as we were going through it. Clearly, the BWRs were on our mind. They were the most sensitive reactors, given you had a failure to scram to begin with. And if you had a concern with the PWRs and the scram breakers, those are the issues that have been raised for a long time; they are not new. We know, and we have identified those, as I recall, at least eight years ago. So that again they are not new issues.

But with respect to the BWRs and the sensitivity of BWRs to the failure to scram, we are much more concerned in dealing with that issue. Well, in looking and understanding the problem, Harold and I both became convinced that there was not a need to shut the plants down; that there were things that could be done to deal with this specific issue.

I guess it raised for me, and I am sure we probably talked about it that Saturday, this clearly has implications for moving on and getting the ATWS resolution before those who ought to have it, namely the Commission. And there as an indication on Harold's part that he was going to be moving the paper forward to bring it to the Commission, and I cannot honestly say it is there.

MR. DENTON: That is on the Commission calendar next week, I believe.

MR. STELLO: That is obviously an implication of what we learned at Browns Ferry. It reminds us again, although I do not believe from the pure statistics point of view having one more of these events, having had -- whether you argue it is one or two of them previously, this now becomes three -- that the statistical change, the statistical significance is not that great.

But strictly from a philosophical point of view, it does suggest let's get on with it and let's resolve the issue, which is where we are now.

Denny has some specific information regarding how one would view the difference in numbers as a result of adding the Browns Ferry experience. If you want those, we can give them to you.

VOICE: I am ready to talk about it.

MR. STELLO: Why don't you?

VOICE: Well, as you know, we were in the midst of preparing our Commission paper when the Browns Ferry event occurred. One of the first things we did subsequent to the event, other than what Vic has described, was to look back and see what impact it might have had in terms of the earlier conclusions we had arrived at.

From a statistical point of view in a rigorous sort of way Dr. Kerr is right. Indeed, there is some small probability that this event would have occurred when it did even if the industry estimates of 10 to the minus 6 or so were -- if the industry estimates were indeed correct.

On the other hand, for rare event models we do not have much data, and when we have data that does not fit the model, I would suspect the model is not very good as a minimum.

We looked at this in a rather simple-minded way. Here was yet another event which we characterized as a failure to scram event. Recognizing the conservatism in that assessment -- and I believe you have had ample discussion in terms of the potential consequences of the Browns Ferry-3 event, if it had occurred at full power in conjunction with an anticipated transient.

A fairly simple approach indicated that what -- the likelihood of ATWS is 2 x 10 to the -4. It is now modified to approximately 10 to the -3 per reactor year, if one includes the Browns Ferry event only as being applicable to boiling water reactors.

On the other hand, if one were to take a more global viewpoint which suggests that it is yet indicative of what Jesse described earlier as things we do not really understand, things which are likely to occur, perhaps at a certain recurrence rate, the impact in terms of the change in the likelihood of an ATWS would be from 2 x 10 to the -4 to about 5 x 10 to the -4 -- the simple difference being you bring in the experience we have gained from pressurized water reactors.

And as to the implications, I think it shows up once again that it is very difficult to sit back and do rigorous analysis of a fairly complicated system, and be able to identify all the types of common cause failures one might be exposed to.

Prevention of these failures, while it may be a necessary step, may not be sufficient. Mitigation seems like a fairly reasonable way to go. A number of efforts have been under way to minimize the likelihood of this scenario, if you will.

But let's say for the sake of argument conservatively that there is no impact in terms of reduction of likelihood, which I think is quite silly, in a way, because I do believe there has been significant reduction of the likelihood of this event. How significant I cannot say. But nevertheless, potential impact on various alternatives that we discussed with you would be possible: increase in frequency of unacceptable consequences by a factor of 5 if one assigns the Brown's Ferry event to the boilers only, and by a factor of 2-1/2 if one assigns the Brown's Ferry event to the total population of light-water reactors.

MR. OKRENT: I wonder if I could get back to the original point that Mr. Stello was discussing, namely, the basis on which you judged whether all BWRs could continue running or some BWRs could be considered, or whether they should continue running. Was there any quantitative input into your judgment, for whatever it was you decided?

In other words, did you have some feeling for what might be the risk of an intolerable accident or whatever? I am trying to understand -- when you decided a plant should go down or shouldn't go down, what is the basis -- this must have entered your mind here -- and how it was factored in.

MR. DENTON: I don't think it enters in the sense when you are coping with operational problems, the real decision is do you understand enough of it in the fix to allow them to continue to operate for one day, one week or one month. And even to stop and take the time to do another event tree-fault tree and try to recalculate takes time.

So it does not enter in a very quantitative sense. Does the staff at the site, the staff here feel like we have the thing reasonably under control so we have enough time with these changes that were issued in the bulletin to enable us to consider it more carefully next week? It depends on when these sort of events happen. If they happen during the daytime, we have more staff, we can get more numbers. But when they happen on week-ends or at night, the chances would be that assessment of -- is the situation well enough understood, and we selectively judge whether it is or isn't.

MR. EBERSOLE: Mr. Chairman.

Mr. Stello, when you went to Brown's Ferry, I would be much interested in if you pursued with the operators that since they had an ATWS, they might have had a full ATWS, and what might they have done?

MR. STELLO: Bill, do you know if that issue was raised?

VOICE: I was down there and I understand there was discussion with the operators by the resident inspectors. That was probably a little later in time than the first few days.

MR. EBERSOLE: There was no documentation of what they said -- what they might have done?

MR. STELLO: They had a procedure, if I recall --

MR. EBERSOLE: ATWS mitigation?

MR. STELLO: -- which dealt with the use of the liquid poison control system.

MR. EBERSOLE: They have an automatic pump trip. That was put on beginning about '68 or so.

MR. STELLO: We are talking about the procedure that dealt with bringing on the liquid poison system.

MR. EBERSOLE: Did they have a procedure so as not to compound the problem?

VOICE: That is normal operating practice.

MR. EBERSOLE: They just fell into that then, right?

VOICE: That was part of normal operating plant practice.

MR. EBERSOLE: There was nothing in their post-ATWS procedure to hold the main steam lines open, to your knowledge?

VOICE: Not to the best of my knowledge.

MR. EBERSOLE: Was there any instruction, having injected the boron poison, that they must be careful to keep it in there rather than allow the system to flush it out? It was only a one-shot deal.

VOICE: I am not sure what specifically was in the procedure. That is one of the things we were made very aware of.

MR. EBERSOLE: Are you sure they are aware of it? You are aware of it.

VOICE: At this time I would say yes, they are definitely aware of it.

MR. EBERSOLE: At this time?

VOICE: There has been discussion with the operators by the residents as to what they would do for an event of this type.

MR. EBERSOLE: As I understand it, that system is not designed to cope with any liquid leakage whatever; yet, of course, there is some and there would be much more. If we had a single rod dump valve stuck open, you would have a substantial leakage rate.

MR. DENTON: I think the answer to all those questions is the reason we have gone toward shift technical advisers, trying to strengthen the technical capability of the utility. It is clear in the first few hours or the first day we cannot be very much help back here. Whatever actions have to be taken under the direction of the people there.

So I think that is what I came back to this morning. The emphasis we are putting on the training, the qualifications, the quality of the management at each utility is vitally important. We will never foresee all these things in advance. We hope to get more drills and more drills, and as the learning experience comes in, we will cope more and more with making sure that what has happened is understood.

MR. EBERSOLE: Was there the counterpart of a shift technical adviser there?

VOICE: Yes, there was.

MR. EBERSOLE: Thank you.

MR. STELLO: Dave, I have been thinking about -- did I have actual numbers in mind that Saturday in any quantitative sense. We did deal with numbers in a very gross sense which gave some insights in terms of the numbers of scrams that you know had successfully occurred in BWRs and, in fact, the number of scrams at Brown's Ferry that they had prior to that time, which I was surprised was quite a few. I think it was 26. I think 26 is the number.

The total number of scrams in the BWR was quite large, which certainly gave some notion of the likelihood of this kind of an event, which put it down as a fairly remote possibility since there already had been quite a few. By definition they all had been successful.

We did not sit down and try to answer the question that you were dealing with earlier, the Bernero memorandum, in terms of was this a 10^{-2} or 10^{-3} or 10^{-4}, and had that particular thought in mind in trying to match the numbers and then make the decision on that basis. I think it was more toward do we understand what we have to assure ourselves that we can go into the other facilities and do something to assure ourselves that this kind of problem -- I was almost going to say precluded -- is reduced in terms of possible frequencies in looking at what the sources of the problems are and eliminating them.

That was more the focus of what we were doing rather than in any quantitative way trying to make that assessment. I think that we turned up, in thinking about whether or not there were things that we could do -- that is, not be able to conclude that we understood the problem sufficiently and what to do.

I think at that point then you are really faced with deciding whether or not there would have been a need to shut down. I guess it is always hard to go back and say what would you have done under those circumstances had you not been able to have that assurance? I guess I would have been leaning a lot harder to shutting plants down, aside from the quantitative numbers that may have been available, that is, if we really did not understand what had happened.

MR. OKRENT: Dr. Kerr.

MR. KERR: Did you try to reach any conclusion as to whether this situation presented more risk than, say, the risk that you and the Commission saw when the error in seismic design was discovered and plants were shut down; or was that part of the consideration?

MR. STELLO: Do you mean --

MR. DENTON: I don't think we went back and -- you know, that information is in our data bank, but there has been a lot that has happened since that time. That is, do we know enough to have some alternative corrections in place that you would feel comfortable letting these plants continue to operate? If you don't feel comfortable, then we recommend they shut down.

MR. OKRENT: We are trying to understand your definition of feel comfortable, Harold; and I myself am unable to put my finger on it. Not only would I be unable to explain it to my students, Dr. Kerr, but I would not know how to begin saying I could bound the philosophy. I don't think it is an easy question, don't get me wrong, but it is going to be a recurring question.

MR. DENTON: It depends on the amount of

information you have available and the perceived quality of the information you have available. We have people available giving us input, and the judgment you think they are making. Like we had Denny there that day. You know, if it is a brand new problem and you never thought of it before, you would act one way.

We have given a lot of thought to ATWS. We have reviewed innumerable drafts. There are a lot of imponderables. Maybe some day we can write down criteria that would do it automatically for us, but we haven't been able to do it yet.

MR. STELLO: It is a very simple question that you asked: is there any comparison made? The answer is no. It just was not made. It was not compared to the five-plant shutdown for seismic, nor was it compared to anything else. That Saturday there was no comparison between what we had here versus what we had done in the past month or the past year or the past five years.

The answer to that question is very easy. The answer is there were no comparisons. If you now ask me, however -- well, compare it now, compare it today -- what am I comparing?

MR. KERR: I did not ask you that.

MR. STELLO: I said you could. You could say if I am calibrating myself with the five-plant shutdown in terms of what that risk was, well, how did the Brown's Ferry event stack up to whatever that was? Certainly it would be a fair question to ask today: what do you think about it?

Well, in my view, based on the understanding that we have of Brown's Ferry now today, that there clearly was not the kinds of questions that were raised and the unknowns at least very early in the question of the five-plant shutdown than there were with Brown's Ferry. I think we had much more confidence in Brown's Ferry in terms of our ability, having understood what happened and knowing what it was one ought to do in response to what happened. And we did that.

We could, in fact, require something to be done in this case, where in the case of the five-plant shutdown, we could easily issue a requirement. You have 24 hours. Make your plant meet the seismic criteria. Having done that, you continue to operate. There really was no such thing for the five plants in that context.

We knew what to do in this instance. In the five-plant shutdown, it clearly was going to be a long, drawn out affair.

MR. KERR: You see, I guess I don't believe that your thought process did not extend any farther than you have said because it almost had to -- you had to assume that there was some probability of an earthquake, for example.

Otherwise there is no point in shutting plants down just because the don't meet a seismic criteria.

If you are never going to have an earthquake, it does not make any difference whether plants meet a seismic criterion or not. So it seems to me, consciously or unconsciously, you said there is some non-zero probability of an earthquake, and therefore we cannot let these things operate when we know that they may not be safe.

MR. STELLO: I was not trying to either defend or argue against the five-plant shutdown. I was trying to describe why even today the comparison between the two, in my view, really is not very meaningful, even today, trying to make that comparison, because there really is not anything to compare.

MR. KERR: But I had assumed in making a decision you would give some consideration, at least qualitatively, to the degree of risk involved in uncertainty. I mean there is always some uncertainty. Now, in the five-plant shutdown you said there was uncertainty in what one needed to do to make certain that they met a seismic criterion.

There is always some uncertainty in what you need to do in order to meet any criterion. Along with that it seems to me there is some consideration of the risk involved in having this uncertainty exist, and if the risk involved in having an uncertainty is zero, which, of course, is the extreme case, the uncertainty is irrelevant.

So it seems to me, along with the uncertainty there must be coupled some consideration of the risk associated with this uncertainty. Now, maybe you do not consider this quantitatively, but I just have to think you considered it at least qualitatively.

MR. EISENHUT: Let me make an observation. Based on March of last year when we looked at the seismic shutdown, we had ourselves in a situation where you can argue -- if you want to think about it quantitatively, what the order of an earthquake the size of an SSE was. The facts we had at that time were that in fact that earthquake at those plants, based on the best calculations from the ANE and the utility, would, in fact, cause a LOCA, fail the primary systems, and, in fact, everything we had was telling us that in fact the systems we had to handle the LOCA would fail with high confidence. They were projecting six to ten times yield on the ECCS piping.

So there was really nothing you could do. You are faced with the one single event, namely, an earthquake, by the utilities best calculations and with the ANE, who came back and told us their official answer was pretty straightforward. At those plants if they have the earthquake, you are going to have the LOCA and there is no way you can handle it and there is absolutely nothing you can do.

You can't go tell them to have three more pumps operable. You can't tell them to do anything else. So you are in a situation where -- I think it is always there in the back of your mind quantitatively, but you do not sit down and write out the equation. Sometimes you more explicitly think about it, but in that situation there is very little you can do.

I venture to guess that even today if a utility came in and said for an SSE at my plant, I am going to fail my primary system piping, I am going to fail my backup systems, I don't think you will have much options.

Now, the difference is, as Dick pointed out, in Browns Ferry you were not in that kind of situation and I think there were some things you could do. You had confidence that there was something that could be done that put it in a different situation.

But if you find -- and certainly in my mind, any time an operating plant comes in and says, one event is both the initiating event and can wipe out the situation to the point there is nothing you can do to mitigate the accident, if we are talking in the ballpark of numbers that we are talking about, you are in a very difficult situation.

MR. OKRENT: What numbers put you in a difficult situation?

MR. EISENHUT: It varies in people's minds, of course, and that's why I said for the FSE people at the site can vary anywhere from 10^{-2} to 10^{-5}.

MR. OKRENT: Was there an estimate for 10^{-2} for any of these plants?

MR. EISENHUT: For these plants, probably not. Probably 10^{-3} on down.

MR. OKRENT: Okay.

MR. EISENHUT: But if there was any question -- we were looking at this in 1979, if you will recall -- plants were not built as designed. That is, they did not have the support problem which we went through. Also, in '79 you could very well have considered the design is not as the design was supposed to be.

MR. OKRENT: You have some SEP plants that absolutely face this question and you know it and you have not shut them down.

MR. EISENHUT: But we also -- we don't have the understandind that, given the earthquake event, the SSE design event, if we reach the point where our conclusion is and the A&E's evaluation supports that for that earthquake it is going to cause an accident and disable all of your ECCS systems or all of your systems that cope with it, I think you are in the same situation.

VOICE: (Inaudible).

MR. EISENHUT: That is why I wanted to point out the distinction, because today we sit and we look at the five-plant shutdown and we say, my heavens, there was only some minor fine tuning; there were some extra supports put in. There was lots and lots of paper generated. But we cannot lose sight of the facts that we had at the time when we were forced to face that situation.

MR. SIESS: Someone actually had calculations that said they were exceeding reliable by five or six times?

MR. EISENHUT: Right, between six and ten times reliable.

MR. EBERSOLE: I wish to argue that that number is somehow less conservative than not knowing at all what these factors were, which is true in the current ten plants with non-analyzed aux feedwater systems. You don't have any analysis at all, and therefore it must be implied that these will surely fail; and then, following that, it must be implied also there is no backup system.

MR. DENTON: I don't think that follows.

MR. EBERSOLE: I am saying --

MR. DENTON: It is one thing to say whether you don't know if it will fail or not, and in fact -- and another one to be told by the designer of the system that it will.

MR. EBERSOLE: I don't know which is the best.

MR. DENTON: I tend to take the designer's view when he says the system will not work.

The seismic issue has bedeviled us from day one, and you can still find a wide variety of opinion among engineers as to how resistant these buildings are. And as you well know, the Imperial Valley earthquake, there was a plant very near there that rode through that and went back into operation.

We send out engineers to look at these every time there is an earthquake, and you find equipment was designed for more or less uniform building code practice. It tends to ride through some fairly big examples. I surely would like to have a test plant somewhere near a big earthquake with good instrumentation some day, with no fuel in the core.

MR. SIESS: Let's get Diablo Canyon started.

(Laughter.)

MR. OKRENT: Can we get on to another generic aspect of the Browns Ferry failure. It was mentioned earlier, namely, how is it that the various failure modes for the scram system in fact were there. In other words, now that one has looked at these plants in detail, we find things that you and the vendor I think both agree need to be corrected.

MR. STELLO: Yes.

MR. OKRENT: Let me state just on boilers. In other words, since this was a system that everybody has been worried about, the staff has analyzed reliability one way and the industry has argued about its reliability and so forth, and in fact this was an identified failure mode for a common cause failure --

MR. STELLO: Did you say was not identified?

MR. OKRENT: Was identified failure mode. I don't mean only by WASH 1400; I mean in general that it was only after the event that one went in and saw different plants, different things that you felt really should never have been there. In other words --

MR. DENTON: I think that has always been the case. You design these plants with a lot of margins, the best engineering advice you can get. And after every event we have had, we tend to say, my God, how did we let that go by. When in fact you consider there are thousands of man-years that go into these designs and they are no better than the people that are actually putting them together. And we audit them.

I think we are putting our faith in the overall margins and the redundancy of systems. And each time something happens like the Browns Ferry fire, you know, you go back and you ask yourself, how did that happen. It is no better than the U.S. technology and our regulations and trying to lay on requirements. Each one is a learning experience.

You can ask, how did we not require that system to be a really thorough safety system, and I think the answer is you think anybody can design a drain. That must have been the thought back in those days.

MR. OKRENT: Do you find that an adequate answer, really?

MR. DENTON: Well, I think -- I don't know if it is adequate. But what other answer is there that all of us collectively in this room who have looked at these things for a dozen years, and no one ever focused on this part of the system before? You asked why not.

MR. EBERSOLE: We have such a lofty view of these things that we don't get into the details. In essence, we don't find out the quality that has been given us.

MR. DENTON: Well, I do not think -- you know, if it takes that kind of looking, then we probably don't have the right organization here to do that. The whole review system is structured on sort of an audit and high-level review, and there is a spot check here and there.

MR. OKRENT: But we have a $200-million some a year research program, a small fraction of which is looking at reliability, but none of which is looking at this level of design adequacy. And apparently it is not being done in the industry, either, although I would think myself, if I were the owner of one of those plants, I would do this sort of thing for my own protection.

Have you asked yourself whether in fact the resources, $100 million a year on LOCA, maybe would better all be dropped on that and put on looking at the car keys and their equivalent?

MR. DENTON: I guess I could at it a different way. We had a meeting with IEEE and NRC this morning, and we had people there from NASA and the military and FAA. And we say, how is it that there have been major engineering projects which have turned out well in this country? What is the secret? How can we transfer that technology over to the business we deal in?

And one of the things that emerged from that -- and we had utility people there also -- was that utilities during the construction phase largely are pass-throughs. They take the design from Westinghouse, the AE, they buy it in toto and ship it all to the NRC and get the questions answered; but don't play a very active hard engineering overview of what it is they are doing.

They tend to do better after the plant gets into operation. And we picked up on that with a few applicants. For example, in the Palo Verdi case we told them that if they do the review rigorously and document it in sort of a systems management approach, with our participation, it would be a much better review than just passing the paper through the house. And they picked the DC battery system and they pulled people into the company who were not on the Palo Verdi project. They hired three consultants and they put Bechtel, the designer of the battery systems, through the hoops for about 12 hours, looking not only at the design, but the maintenance procedures, the 40-year life, everything about the battery system.

And we had our staff there, and at the end of that -- and they were using as their guidelines in the review the regulations of the Commission, the branch technical position, reg guides, as well as the company's own internal requirements for batteries.

They found like 15 deficiencies, and that company is convinced they are going to get a better battery system for their review of the battery than if they just shifted it to us.

And I get the feeling maybe we have accepted too much of the burden of responsibility for the review of these things. It cannot be done in Washington. It has to be done back in the offices of the companies that buy it. We have to provide incentives to move it that way.

MR. BENDER: That point is not new. It has been around a long time.

MR. KERR: Mr. Bender, I know you are laboring under difficulty. Can you hold that closer?

MR. BENDER: Can you hear me now?

This is not a new point, and I guess the Palo Verdi thing was illustrative. If you are going to make it a requirement, how would you go about doing it?

MR. DENTON: Well, we have hired people from NASA who do this routinely. When there is a shot going up from one part of their space center, the project is reviewed by a team from another space center. Being the government, they can make it work somewhat easier than small individual power companies can.

What we are trying to do at the moment is move in this direction for these new OLs that are coming in, which does not work too well for plants that are already largely through the process. But take new OLs that are filed, and I am holding out the promise that after we get a few of these under our belts -- the key point is that they meet all of our standards when the company does this.

There was a transcript taken of that meeting I mentioned and we will get a report on DC batteries signed by the people who are on the company's team. And we had our branch chief there participating. So we can find a way to formalize this and get it into the review process.

There are several applicants in the wings who are willing to undertake it, but we have not -- I have not proposed it as a carte blanche yet, because I don't feel comfortable enough that utilities can handle it across the board. But I think there are several areas where the utilities could surely move in that direction now.

MR. BENDER: The AE, at your insistence, has the independent checks, reviewers that were not designers, and that they do these kinds of checks to be sure criteria were being met. Does none of that apply? Is this separate from that, or were we just getting a lot of PR?

MR. DENTON: I don't really know. I have not looked at the AE role in a long time. Vic, do you have a feel for how their QA systems work internally? Are they doing internal system management reviews?

MR. STELLO: All of them basically have a QA system that does pretty much what you suggested: reviews and audits their own work to make sure they are doing it their way, that is, the way their books say to evolve a certain design.

There is another layer of review which the licensee, according to the regulations, really is supposed to do, have a system to independently monitor and make sure that that happens. This is done routinely in all plants.

I think the level of competence that exists in terms of how detailed they get and what kind of detail they get into varies widely.

MR. BENDER: There is not more than a handful of utilities that have enough engineering capability to make the kind of review that you are talking about. It has to be done by some, either large engineering firm who might have an independent setup or some independent contractor hired by the utility.

MR. DENTON: I tink the system works --

MR. BENDER: I am not certain what the thrust of your efforts are.

MR. DENTON: We have two or three efforts going, Palo Verdi and some others, trying individual pieces of the plant, and we have consultants who know how to do these things to make sure it has some value. And I want to get a few more under my belt before proposing we do this en masse.

It is my opinion that utilities do a much better job of changes in the plant after it is in operation, because then they have bigger engineering staffs. It is really their plant and they are vitally concerned about the quality of changes in their plant.

And you are right: Most companies who enter this field aren't all that well staffed at day zero when they first buy the plant. But by the time they are running it they have achieved a sizeable technical understanding of the plant.

MR. EBERSOLE: A point. Mike mentioned large organizations; large organizations might have very large jobs besides doing this sort of thing. And though it be large, you might find you are not getting the review that you thought you might be getting. And I can speak with some practical experience on this aspect.

For instance, there are several large organizations which give no review except just interface review.

MR. DENTON: Another example that is closer to home, perhaps, is the control room design at TMI 1 and 2. The TMI control room is much better designed in Unit 1 than in Unit 2. You might ask yourself how that came to be. Different AEs.

MR. OKRENT: Let's see. We are at 20 to 1:00. We have a long list of items yet on this agenda. What is the schedule of the staff people that are here and how should we reorganize the agenda?

MR. DENTON: I think we would like to get through everything but what you had on the afternoon session, the issues that were going to be covered by Denny Ross. So we are prepared this morning, or to go next after lunch, whichever you prefer, all the morning topics. And I guess cascading failures I would propose to make last on the agenda.

MR. OKRENT: You can be here after lunch for a period?

MR. DENTON: Let me -- Steve only has one item.

MR. OKRENT: I am trying to be accommodating. So what do you suggest? How should we proceed?

MR. DENTON: Why don't we do control room design and then break?

MR. STELLO: I assume I am done?

MR. OKRENT: I think we are finished with the ATWS item for now, if that is what you mean.

Steve?

MR. HANAUER: Rather than give a long speech, why don't I say in a few sentences a summary of our control room approach. I would characterize it as being in two bites. The second bite is easier to describe than the first. The second bite will be a complete human factors review of every control room, with the object of bringing them all that need it up to some standard of operability. Since we don't ha e such a standard, the object of the first bite is to do enough control room reviews that we can go off and write this standards of operability for everybody to do it.

We are now in about the eighth or ninth of what I hope is not an endless series of control room reviews. We have found that control rooms, conventional control rooms -- we have not reviewed any of the cathode ray tube ones, so we will have to do so. We found that they vary substantially in operability.

What we have today is a checklist of things to look at. We spent a week in each control room with a team of about six people. These people include systems engineers and human factors specialists, which we have been using as consultants, and inter-agency people. We also had a team of human factors specialists help us with our first checklist.

We find all kinds of things. We found one control room in which the ventilation system was so loud that if you wanted to talk to someone you had to duck into a side office.

MR. SIESS: What did you say?

MR. HANAUER: Yes.

(Laughter.)

MR. HANAUER: Worse than here.

We found control rooms that run at 100 foot-candles and control rooms that run at 10 foot-candles, both of them outside the range where people can work in any reasonable way.

We found a control room that was so bad that I don't know where to start in improving it. It is fortunately on a plant that is not operating.

We have found some control rooms that were pretty good, by which I mean that when you listed the deficiencies they were really quite small. My own prejudice is that we will find some control rooms that can be fixed up to be quite operable, with some rearrangement, some paint, some grouping, some better procedures; and we will find some control rooms that cannot be raised to a reasonable level of operability that way.

And for those I envision, not tearing it apart and doing it over, but skidding in a supplementary console, which in my ignorance I envision having four or six cathode ray tubes and a good computer that will simply substitute or the existing control room that has an inventory of the right stuff in it, but so poorly arranged and so poorly laid out that it seems impractical to rebuild it into operability.

I will tell you, frankly, the worse one we have seen is Three Mile Island Unit 2. We went recently to Three Mile Island Unit 1, because there is a review of that plant now going on prefatory to taking a position in its restart. And we did a review of its control room in our usual one-week visit. And then, because Unit 2 had come in for such a severe criticism as a result of the Three Mile Island Unit 2 accident, we revisited Unit 2.

And we discovered that Unit 1 and 2 are not identical; they are in a certain way caricatures of each other; and that where Unit 1 has a number of important deficiencies, Unit 2 is substantially worse than that and is in fact the worst one we have seen.

I can give you examples, but I don't think you want that.

Now, the tough question is, are we going to arrive at a standard of operability, or are we simply going to apply that marvelous engineering judgment that we all have so much of, to say which ones have to be fixed and which ones don't. I obviously don't know the answer to that. We are going to give it a good try.

Maybe I ought to stop at this point and take questions.

DR. SIESS: I may have missed it. Did you say you have not looked at any of the, quote, "advanced," unquote, control rooms?

MR. HANAUER: That is correct. We have to do it. But we have not. We have concentrated on the operating plants and those just coming into operation.

MR. KERR: I would have thought that there would be some industrial activity paralleling your own. You have not mentioned that.

MR. HANAUER: There is industrial activity paralleling our own. And we are woefully short of information on it. On September 26th the industry is going to brief the Commission on what they have been doing. We know some of these things. There has been an EPRI program for many years. Their prime contractor has been Lockheed, although they have used Aerospace and they are now using Essex. They have done an immensely valuable piece of work. The reports by Joe Seminara and his colleagues at Lockheed are not only catalogues of the bizarre and the unacceptable, but also very useful checklists of principles and remedial measures which they have devised.

There is also work in progress at the Institute of Nuclear Power Operations, INPO, where they are trying to lay down some requirements. There is also the EPRI-Essex effort along similar lines, which I have not yet seen the real scope of. We are collaborating ourselves with the Bureau of Standards, NASA, and have some talks scheduled with the Department of Defense, who have established some operability standards.

MR. EBERSOLE: Steve, the scope of the investigations; did you ask your operators, what would they do if their control rooms became not unoccupiable, but inoperable?

MR. HANAUER: No, we have not. We have regarded our mission as somewhat different.

MR. EBERSOLE: Oh, okay.

MR. HANAUER: There is an answer for the unoccupiable, and one presumes that something of the same answer would obtain if the control rooms were to become inoperable, assuming that the people in them could recognize that they had inoperable control rooms.

MR. EBERSOLE: As a minor point, did you find that all the fluorescent lights would fall out in a seismic event and create a monstrous Rancho Seco problem?

MR. HANAUER: No, sir.

MR. EBERSOLE: Did you ask them?

MR. HANAUER: No, we did not ask. We did not look at seismic qualification.

MR. BENDER: Some time ago I had the occasion to talk to some of your French counterparts about control room design, and they contended that American designs were somewhere back in the Middle Ages. Are you looking at what the French are doing?

MR. HANAUER: Yes, we are. I saw in 1963 a French control room about the size of my office, essentially completely computerized; and have, in several different forms and in several different ways, recorded my personal view that we are in fact in the dark ages.

MR. EBERSOLE: (Inaudible).

MR. HANAUER: That is quite so. In the Pacific Northwest is a government-designed test reactor in which there are side by side a very modern, cathode ray tube computer-oriented control station for the fueling machine and a control room that could have been designed by my control room group in 1950 for the reactor operations.

MR. SIESS: Steve, I recall some of the builders have very definite ideas about control room design, based on experience. If I am not mistaken, Carolina Power & Light came up with some sort of miniaturized console. Have you looked at any of those?

MR. HANAUER: Yes, I am familiar with that console. The companies do in fact display a large spectrum of concern and approach. The best control rooms we have found are the ones where the operating company had a dominant role in the design approach, the layout and the design details of the control room.

MR. DENTON: There are a couple of other factors, too, that you soon see when you visit a number of plants. One is an increasing tendency of the utilities to bring into the control room operations that used to be done outside the control room. They may have been done at chemical purification stations and so forth. But in the early plants, they were done by operators stationed elsewhere in the buildings. And they are now brought into the control room.

So I feel like the scope of demands on operators are larger in today's versions of plants than they were back in the early days. And another thing that has been called to our attention and I looked at recently is the demeanor of the people in the control room. By that I mean, the formality of the control room, so people know what is going on. And this is something I know the Navy thinks is a very important contributor to control room practice, the degree of formality; and that is missin, and varies widely among the operating facilities.

MR. SIESS: Discipline.

MR. DENTON: Yes; the duty stations of the operators and who can be between them and the panels and all those minor details that, taken together, result in good operations.

MR. HANAUER: I was recently in a control room where the control operator actually has -- wears a jacket with an emblem on it of a specified color, and the shift technical advisor on duty wears a jacket of a different color with an emblem on it. And I think such things are probably the coming thing, that the pride and discipline in the control room is an important factor.

We are working on that, but that is not part of the program that I described, which is directed toward the physical arrangement and layout in the control rooms.

MR. OKRENT: Can you do much to change the physical arrangement? What I heard you say was you thought you might be able to add on certain kinds of information groupings and a better computer.

MR. HANAUER: It is only time and money. You can rip them out and put in new ones. This is enormously expensive and time-consuming and has some negative safety aspects also. It is rather easy to discuss completely changing out one panel. There is money there. But you can do that during a refueling outage if you plan your work.

What I was talking about was some number, which I cannot speculate what fraction it is, of control rooms that we will find to be so seriously deficient that they need a lot of work. And there I was speculating that, instead of tearing out a lot of panels, we should skid in something.

MR. EBERSOLE: Isn't it true that the old control rooms were probably -- (Inaudible).

MR. HANAUER: One would think so. But I am a lot less dogmatic about that than I was a year ago.

MR. KERR: Steve, having made your earlier comment about the TMI 2 control room, do you have any informal estimate in your own mind as to how much that terrible control room contributed to the seriousness of the accident.

MR. HANAUER: I think -- this is now a personal view -- I think that a substantial fraction of the badness of that control room was discounted by the usual heavy training of the operators so they can learn where things are, even though things are very badly arranged.

If you go along with the account of the accident and you enumerate the mistakes the operators made, you do not see any specific things: well, this meter was 20 feet from that meter and they could not look at them both, and that therefore they -- you don't see any like that.

In a more general sense, their failure to perceive what the problem was and to make a couple of critically correct decisions was the overall poor layout of the control room -- was that responsible? I think it would be impossible to say. My instincts tell me it had a fairly small part. But gee, it sure is bad.

MR. OKRENT: Are you developing any approach to status monitoring requirements in the control room as part of what you are doing?

MR. HANAUER: I cannot admit to it, because it has been deferred in the action plan to fiscal '82. What we are doing is trying to make sure that whatever they do and whatever we require will interface with a suitable status monitoring system.

MR. EBERSOLE: (Inaudible). Are you asking that operators, to what degree they meet on a various -- (Inaudible).

MR. HANAUER: We are talking with the operators. We are also walking through some procedures and seeing what they do and whether it is easy for them to decide on the next step and so on. I don't perceive the pattern yet, if there is one.

MR. KERR: You are aware of the old Western Electric experience with illumination. I was struck by this when you mentioned illumination in the control room. They were trying to discover the appropriate illumination, and productivity went up each time. They discovered what was making productivity go up was that somebody was interested in what they were doing, not the illumination.

MR. HANAUER: Yes, sir, I am aware of that. Are there any other points on the control room question?

MR. EBERSOLE: (Inaudible).

MR. KERR: I cannot hear you.

MR. EBERSOLE: I am talking about NSAC-60. Is that part of your discussions with them, what you think you can do here and what are your responsibility burdens and what are they not?

MR. HANAUER: Yes, that is a large part of the basic information. We don't have much of a handle on that. In an effort to get some science focused on that question, we are about to embark on a job task analysis of the control room people, which we will have to do in simulators, because we really want to know what they have to do in accidents, not on the night shift at full power. And that will be the basis for our trying to do a little better in resolving the questions you are talking about.

MR. EBERSOLE: You know, your old co-worker, Harry O'Brien, is the chairman of that.

MR. HANAUER: Yes, I know.

MR. BENDER: Steve, I'm sure you are aware of Reg Guide 1.97.

MR. HANAUER: Yes, sir, I am aware of that.

MR. BENDER: How would your appraisal of the control room design fit with the requirements of Reg Guide 1.97?

MR. HANAUER: 1.97 has two things: One is an evaluation from the system standpoint as to what variables the operator needs to understand. We let the systems people tell us about this, although when we come to the procedures we are careful to make sure that the things the procedures say the operators should check in fact are acceptable and reasonably located and so on for him to do what the procedure says.

What interests us and what falls within our task is to consider, given the Reg Guide 1.97 list of parameters or any other suitable list -- I am well aware of the recent committee comments on the current list -- how shall those instruments be integrated into the control room in which one has to do many things, including the operations foreseen by Vic at 1.97.

So it is kind of a raw material for us.

MR. EBERSOLE: Along the same line, did you ask the operators how they feel about the Nuclear Data Link? Do you see any effects on them for the presence of that machine?

MR. HANAUER: I have not asked them. There are ten other people working that problem. I have a personal view on that, which is that it ought to be possible to delineate the responsibilities and still get the information where it is needed.

MR. DENTON: It keeps coming back to me, every time we activate our response center, that we need a better way of getting data than asking a person on the phone, then having him lay the phone down, and we wonder what is happening and finally we get a reading back. So I do not -- I would hope it would not be seen as a transfer of responsibility, but utilizing the response center, utilizing the technology for data transfer.

MR. OKRENT: It is my impression that the astronauts faced the same questions.

MR. ROSS: A procedural matter for after lunch: Would it be acceptable to do Item J, then E, and then combine Item G with paragraph 3 of the generalized discussion on cascading failures?

MR. OKRENT: You better get together with Savio, because he has given us a new set of letters. I am sorry, we don't have these coordinated.

MR. KERR: The answer to his question is yes, isn't it, because you don't care in what order they cover things, do you?

MR. OKRENT: No, that is right.

I am going to assume that you and Savio will work it out. We do want to, at least if possible, get a brief comment on each of the items, and on some of them cover them in more detail, as time permits. Okay.

We will break and reconvene at 2:00 o'clock.

(Whereupon, at 1:05 p.m., the meeting was recessed, to reconvene at 2:00 p.m. the same day.)

AFTERNOON SESSION

(2:00 p.m.)

MR. OKRENT: We will reconvene the meeting.

I think it is suggested that we take up the next item, comments by Mr. Denton on the general approach to reevaluation of the Indian Point, Zion and Limerick.

MR. DENTON: On those two, Indian Point and Limerick both are doing many WASH-1400 studies. I expect to have these done in the fall. We are also making Indian Point in parallel with mitigating systems so we can come to some decision about whether additional risk reduction is necessary at Indian Point or Limerick.

The Commission also has set down an adjudicatory proceeding to determine whether or not additional safety measures are needed at Indian Point. They are trying to establish what the issues are, and that is running down the track also. The Commission did decide to let Indian Point operate in the interim while this adjudication goes on.

MR. OKRENT: Could you describe a little bit more the adjudicatory proceeding: what you think it is likely to encompass, what is scheduled and so forth?

MR. DENTON: They issued an order on May 30, and that is still the controlling order. What it ordered was adjudication before the Licensing Board on safety issues raised by the intervenors, an informal proceeding to determine what the criteria should be; generic consideration of operational reactors in high population density; and deciding on iterim operations of the plant while adjudication is going on.

So the only one they have come to agreement on is item 4. They still have under consideration what the issues should be.

MR. OKRENT: They meaning the Commissioners?

MR. DENTON: Yes. I really cannot speculate on where it will go, being a party to the proceeding, and what is really being adjudicated is whether I made the right decision or not.

MR. OKRENT: Does the staff have a proposed philosophic approach to the reevaluation of Zion, Indian Point and Limerick?

MR. DENTON: I think we do. Our approach is I would like to see them take that of any other average reactor. We want to know whether they carry an undue societal risk or not. We have had studies by Research that tend to indicate that features in the plant, coupled with the ones we have ordered, do compensate; but we are still completing studies I have ordered them to do. Many of the 1400 are the mitigated features.

MR. OKRENT: Is it only Indian Point that is doing both mini WASH-1400 and mitigating features?

MR. DENTON: And Zion. Zion, I think, is just doing the mini-1400.

MR. OKRENT: Why is Limerick doing only the mini WASH-1400? I am just trying to understand the staff's philosophic approach.

MR. DENTON: I think we had more concern with the plants in operation. Limerick is under construction, and if it turns out that they have an undue share of risk, we have a little bit more time to bring it under control before they go into operation, whereas the other two are actually in operation. If we conclude that it is a high disproportionate share of the risk, I wanted to have in hand the mitigating features right there to choose from so that I could order those stopped.

MR. OKRENT: It would seem to me that the same information would be relevant to Limerick. I can remember the staff coming in and telling the Committee that then Limerick looked like a site as bad as Newboldt Island, and then the staff turned around and told the people at Newboldt Island to move their reactor. So I guess I am still trying to understand the staff's philosophic approach.

MR. DENTON: I think it is the fact that Limerick is not operational, so it is no risk, so I am not incurring any public risk to operate. I am indifferent. If we find the risk is too high, I am under no obligation to license that plant.

MR. OKRENT: Well, yes, but I don't think you really mean it that way. You don't want to capriciously delay them two years from now if you could avoid that by getting information two years early.

MR. DENTON: You asked me the question; I gave you my answer. You may not agree with it, but that is the staff's approach. Any plant that is under construction, we do not have to move quite as fast and tie up reso.rces that we do on any plant that is in operation. So we are much more concerned about those that are actually running, and that is why even the Indian Point and Zion applicants argued forcefully that we should be in a serious mode to the risk studies and the mitigation.

They objected to doing it in parallel, but I think parallel makes sense if they are in operation.

MR. OKRENT: Let me ask a different question, if I can. You mentioned you thought you would like to see Indian Point and Zion introduced at about the same risk as the average reactor.

MR. DENTON: Not carry a disproportionate share.

MR. OKRENT: Now, let me think about that statement in terms of one of my favorite subjects. I could envision a situation where we had a large number of dams and a few of them were above more people, the inundation plane included a lot more people than the others, and I might say to myself, well, I need to make these have about the same risk, and I might proceed to make certain changes so they had about the same risk.

On the other hand, they might all of them have been unsatisfactory because they were all made like the Van Norman (phonetic) dam, okay? On the other hand, they might all of them have been built in so conservative a way and of such high quality that I felt they were really all safer than I needed, in fact even including the one that was above more people than the others.

So going to the average in one case might have left me insufficiently safe in the other case. It might have been sort of a diminishing return on the risk point of view. So I am not automatiacally persuaded that going to the average is the right approach.

Now, can you help me?

MR. DENTON: Well, I certainly understand the pros and cons, but I do not for the moment -- for the moment I can't think of a better way to approach it. There are two requirements that must be met by plants to operate. They must meet the Commission's regulations or have exemption for a good reason not to.

Now, letting the average plants run -- society's tolerance for the average risk of plants -- I am trying to find the outliers. Now, I may be beating on them too hard, but for the moment I propose we have the risk for the average.

MR. OKRENT: Another part of this thing that leaves me a little unsure of what the staff means when it says it is letting them go down the average is during this last decade, I think the staff's concept of what the likelihood of a serious release is is, I would guess, changed by two or three orders of magnitude, or maybe more, if I look at what was said in the Environmental Impact Class 9 documents and what was given in testimony at hearing boards in the early seventies and so forth, as contrasted to what I hear now from Mr. Rousan (phonetic) and so forth about what the probability is of serious damaging core accidents nd so forth for various reactors.

So that must mean in some sense that one's picture of what the average is has shifted markedly. Now, maybe the average was too safe or unnecessarily safe before, but on what basis is the staff deciding that the average remains okay in light of whatever it is that they are learning, particularly if I am correct in my perception that their view of the average is that the average risk is increasing from what they thought it was.

MR. DENTON: I would like to have a better basis, and we have tried to get other agencies to provide a basis for us. I think is self-serving, in a sense, for us to compare uranium with coal or uranium with oil or natural gas. We have tried to get DOE, for instance, to do a comparison, as we do in each of our environmental impact statements do a comparison of relative risk, and we continually upgrade those. And we will be discussing the consequences of severe reactor accidents in those.

Society offers no goal. In fact, my own feeling is that everbody's tolerance of reactors varies widely. As you well know, there are people whose tolerance is zero for reactors and there are others who have a pretty wide tolerance. I don't see much hope personally in ever getting society to agree that $10^{-3,\ 4,\ 5 \text{ or } 6}$ per year is an acceptable number.

We work on it. We occasionally publish stuff on safety goals. But we really have to jump on every chance we get to lower the risks, whatever they really are.

MR. KERR: Harold, I am sure I have no idea of all the difficulties that face this decision, but the problem for people who are operating plants and trying to design them and trying to upgrade them is in having not only a moving target but a target that is hard to comprehend. I mean suppose, for example, the operator wants to know whether he should do something to improve the safety of his plant.

He has to have, I think, more than good will in order to do that because it costs money and he has to justify spending money before public service commissions. It therefore is in his interest to have some sort of objective standard.

If you talk about the average in a situation in which you have wide variations in individual plants, and if you improve those at the high risk end of the scale, you have now, of course, increased or decreased the average risk. So you now have another set of plants that are outside the average.

You can argue that this average is condemned by society and therefore it is an approriate goal, but it seems to me with equal validity you can argue that the spectrum has been condemned by society; so that on a retrospective condemned by society basis, I have some difficulty justifying making changes.

There may be other reasons than that, but what I am hearing seems to me to say that to some extent the target that you are using is one you arrived at by looking to see what society had accepted up to that.

MR. DENTON: I guess I tend to simplify things too much. The other part of this whole thing is the emergency planning, and we have asked FEMA to come up with evacuation times for these high population sites, and you find that they are very long. They are a lot longer than we felt they were.

So it is not just the risk, you know, of a core meltdown per se, but it is being able to implement the Commission's wishes in areas that have very high populations. That is another way of getting to the same point.

People are asking if it takes 8 or 10 or 12 hours to move everybody out within the 10 miles, isn't that not an undue risk compared to a plant where everybody can be moved in one hour or four hours? So what I am trying to do is see whether or not that is a true statement, and if it is, to provide some compensating measures so that I can tell the people who live around these high population sites the fact that it is high population is not being disregarded, because they see they are at a greater risk than a plant that has only a few people around it.

MR. KERR: Well, if you are talking about individual --

MR. DENTON: Let me say one other thing about this. I do not think the public trusts the government in decision making very much any more. They want a shared role in it. I found in trying to vent TMI this spring, the numbers we had for millirems or the comparison to smoking cigarettes really did not interest either the public who lived there or the decisionmakers. That is not the kind of criteria they want to work with.

They want to be assured of low risk, no risk, what have you. I guess it is that kind of experience that in trying to say a certain number of millirems is equivalent to a certain number of cigarettes or is no different than background -- some things just were not effective either at public meetings or meetings of publicly-elected officials.

MR. KERR: Are you telling me, then, that what the public wants to see is a good faith effort to do something, and if you exhibit a good faith effort to try to do something, then the public will be satisfied? Is that it?

MR. DENTON: No. I think they want progress. I think they think reactors are unsafe and they want to see how to improve the process so they don't have to worry about them being nearby. I think if they see an aggressive regulatory program so that they know we are looking after their concerns in making the plant safer, they will accept the plant. But if they don't, if they perceive we are only staying with the status quo --

MR. KERR: You feel the slope of the curve is important rather than the point at which one is at a given time.

MR. DENTON: I feel both are important, but they all meet the Commission's regulations with regard to individual dose. But I think there are special programs in the high population sites that ought to be looked at and dealt with, and that is what we are proposing to do.

MR. KERR: I think we all agree with you. What I am trying to do, and I think what Mr. Okrent is trying to do is to understand how one knows when he has dealt with the problem.

MR. DENTON: I guess I am not one to decide in advance. That is why I want to get the study in here to see what the numbers are and then come back and get the advice of the Committee on what we ought to put in, if anything. So I defer the decision on how much is enough.

If they come back and can reduce risk by a factor of 100 for a $20 investment --

MR. KERR: But see --

MR. DENTON: I don't want to make a predetermined choice of a factor of 3.3 or .2 or .9 is enough until I see what I am buying. So I would like to see what can I get for such an investment.

MR. KERR: No, but at some point you have to decide that here is a plant that you don't have to do anything to. Maybe you don't. Maybe you are going to look at all plants and say how much could I buy for $20? And if you can buy something, you spend it. Is that sort of what you are saying?

MR. DENTON: I don't have any problem not
requiring anything more than we are requiring on new plants like Sequoyah when they are in very remote areas. We are taking a hard look now at our ability to do emergency planning, taking a much harder look at the competence of the facility, the staffing level, the training, and we are getting really to the heads of the company to make sure they are devoting resources to these.

I am not quite as comfortable in looking back at all the plants. I just don't want to automatically assume that every plant we have licensed is good enough. We picked off the high population ones to explore in depth. I am not sure we will require any change, but I want to get it out on the table as to whether it is necessary or, if so, what would be sufficient.

MR. BENDER: Harold, you made a point earlier that evidently the public is not very receptive to varying risks, the comparisons, say, of cigarette smoking to nuclear power. What confidence do you have -- and given you can provide these incremental improvements -- that they will get any further comfort from the planning of the vented containment or some kind of core catcher of undefined design.

MR. DENTON: You know, you are able to read the public as well as I am, in a sense.

MR. BENDER: I do not claim any knowledge, but it looks to me like the perception in one case is the perception in another. They are not going to understand either one. It is all public relations.

MR. DENTON: Well, I guess I feel the public is turned off on regulation of many types, but I do not think they are turned off on the regulation of a major safety hazard such as dams or reactors. I think if their perception is that we are always explaining that the plant is safe enough and it is okay and so forth, then they lose confidence in the government's ability to deal with the problem even when we are dealing with it.

The way to be sure that we are on top of it is to meet with the local officials, to meet with the people, the critics at the plant, and take their concerns and try to show that over the next 40 years we will do what we can to make them better, recognizing that they will never be perfect.

I think we have had some successes in areas where people raised issues and we thought we were good enough, but people are persistently raising the same issues and we ignore them. Then we are told we are not doing our job. I guess I feel good about cases where up in Midland recently, the majority of the intervenors withdrew from the case because they are confident that the staff and the Corps of Engineers can solve the problem.

You can argue that the applicant is right and they are not going to sink anyway, but we are pushing and we will pursue it to the end.

MR. BENDER: Like at North Anna.

MR. DENTON: It is regional.

MR. BENDER: It suggests to me you are doing nothing but catering to the whims of the local population. I have to believe that once you make a decision that says it is desirable to have some improvement at a plant in the Northeast Middle Atlantic part of the country, then the average plant neighbor will want to understand why he is not privileged to have the same thing. I think you have not addressed that question.

MR. DENTON: Originally the government role was something on negative freedom, freedom from contaminated food and freedom from war and crime in the streets and so forth, and that was an adequate function of government. There are a lot of people today who want positive freedom, who want to influence where Highway 66 goes in their neighborhood, who want to influence how the reactor that is near them operates, and they frankly are not satisfied to have some mandate come out of Washington that tells them every plant in every region of the country is the same.

People do have differing concerns, and it does not bother me to address in Georgia the concerns of the Georgian elected representatives, and address in other cities their concerns. I think you are operating with a margin wherein you will not be able to make clearcut, scientific decisions. It is not a question of compromising the fundamental engineering capability of a plant, but what it is trying to recognize is the concerns of the people who live around these things and who bear the cost of any accidents that happen and who derive whatever benefits happen.

So I think you have to be -- we the regulators have to be sensitive. If some segment of the population wants a plant that is three times safer and they are willing to pay for it, why not do it?

MR. BENDER: If I knew and you knew who "they" were, we probably could reach some understanding. But "they" turns out to be a few people who are pressing very hard for some very expensive improvements. The other "they" involves a very large complement of people who are shouldering the burden of cost by not being asked.

MR. DENTON: We tend to get into these black and white discussions, but you will find most of the plants, in spite of what is ongoing, look about the same. But I have met with officials in Alabama and officials in California, and I try to do for those elected officials what I can, and their legitimate concerns where they don't want to be cut from the same cookie mold. 1 y want something a little bit different.

It does not bother me to adjust the process to recognize their unique circumstances, whether it is high population sites or low population sites or high seismicity. They are not all the same.

MR. BENDER: While we are on elected officials, if I were a regulator as you are, trying to consider the circumstance over a period of 40 years, I would not let my actions be governed by the circumstances that are involved in the short-term electoral process. You seem to have that dominating in your whole approach.

MR. DENTON: As long as it moves in the direction of safety, I guess it does not bother me.

MR. BENDER: It does not necessarily move in the direction of safety. Adding things that are complications that are not necessarily provable and workable, and adding complications to the installations and jeopardizing the installations during the time they are being installed cannot necessarily be termed in the instance of safety.

I think you have not looked at that aspect at all. As a matter of fact, I think you are suggesting things be added to the plants without even having a conception of how they would be added. I think you have not tried.

MR. DENTON: In the case in point, Indian Point and Zion, we have not added anything yet in the way of core retention, filtered containment venting, hydrogen control. What we are asking for is studies. We have not hesitated to add those things based on our own experience in control room operations we think move in the right direction. But in Zion and Indian Point, we have not yet added any of the things that apparently concern you, like filtered containment venting.

MR. BENDER: You have not established any criteria upon which people could decide whether it is acceptable or not. How is anybody going to propose something if there are no standards for determining the adequacy of an installation? I have not seen any of that in the documents which you set forth for the public to comment on or to respond to.

MR. EBERSOLE: I don't see any way for the public to measure what they are getting against some incremental cost.

MR. BENDER: There is no standard for measurement. There is no suggestion of what should be the standard, and there is no way of determining what the reliability is of the things that are being suggested.

MR. EBERSOLE: I never have heard of any discussion of incremental cost to buy whatever you might buy.

MR. BENDER: That is one of the things that needs to be considered.

MR. EBERSOLE: I think it is entirely valid.

MR. OKRENT: Harold, you earlier indicated that for a parent plant at a relatively unpopulated site -- you felt that things were probably okay and you did not look for improvements. I would like to make a couple of comments in that regard.

First, I think you need to consider the potential effect of an accident on what you might call the sources, whether it is water or farmland or whatever, in arriving at an overall judgment on consequences of an accident.

And if you have in mind some kind of an ALARA criteria, in other words, that one might consider spending the $20 or whatever was the figure for some improvement, that in terms of both risk to public health and safety, but also what I will call risk to the sources, economic effects, however you want to categorize them, I think to exclude the latter may leave out an important factor, if not perhaps the more important factor for many considerations.

So with that in mind, it is not clear to me if one thinks one should have an ALARA principle for accidents -- and in fact, I think one should for reactors and for other kinds of facilities -- why one would not look at all plants and not just the few at the upper end of the spectrum, if you are thinking about is there some improvement that can be made and the cost effect.

I am not trying to define what is cost effective. That is a societal decision, let's say. But in any event, I just wanted to make what to me an important philosophic difference from what you were proposing.

A question more specific to the Indian Point/Zion thing is you earlier mentioned that your own -- not your staff but the NRC staff had arrived at some tentative numbers on the probability of accidents at Indian Point and Zion that were less than the average or whatever. And certainly the licensee has come in with numbers that are far less than the average.

On the other hand, we have only recently seen where the scram system on BWRs was subject to faults that were not included in the reliability study. And I think we are all conscious of the fact that there are mechanisms like sabotage.

I wonder if you think you would be in a good position to rely on risk evaluations to say yes, in fact, I have a factor of 10 lower probability of a serious release than the average with, you know, a high degree of confidence, a high enough degree of confidence that you can say this justifies not doing something else, assuming you had in mind your original goal of making these reactors like the average.

For the moment for, purposes of discussion accepting your modus operandi and just posing this question about whether the risk quantification can be reliable enough to give you a -- for example, a confident feeling that the chance of a serious accident -- I will use a number -- is more like 1 in 100,000 than 1 in 10,000, or more like 1 in 50,000 than 1 in 5,000. You take your number, but one of those is a pretty small number. That is the point I am getting at.

MR. DENTON: Let me just respond summarily, and then we can talk about the numbers. But I don't want to overplay the use of risk assessment. I think -- bear in mind we are still doing a deterministic review against those old saws such as single failure and so forth. I think they served a very valuable purpose, and they do establish some sort of level of risk. So when we use a risk assessment, it is k nd of an orthogonal look at the plant to see what this deterministic approach has done.

So probabilistic approaches look great until you get into them in considerable detail. Then you never can get the experts to agree on the numbers. If there were closer agreement among the experts on some of the issues such as seismic, it would be easier to use it; but when people range all over the map, a decade either way, it gets difficult to get an answer.

MR. ROSS: Let me give a partial response, Dr. Okrent.

We are currently engaged -- currently we are working on a staff report that is supposed to be finished, and that includes review at the office director level by the end of this month on a document that we intend to file in the TMI-1 restart proceeding.

It is related to a Board order to relate the fixes that the Commission specified in its order to some probabilistic goal. The Board wanted to know in the particular areas of operator training, auxiliary feedwater improvements, and small break loss of coolant how did we know that we went far enough -- and when I say "we" I mean the Commission order -- how did the Commission order go far enough in achieving some numerical goal of safety?

We have hesitated several times this year -- it is calendar year -- in filing to the Board. We made filings. It was not quite what the Board had. The Board supplemented the order. We had various questions. And in June we submitted about a 50-page document. It has some event trees drawn on it, and it related all of the fixes that we did to what is now referred to as the close analogs of TMI-2.

There are various accident sequences that relate to small break loss of coolant and loss of all feedwater, operator error in terms of terminating or interrupting ECC. It still was not enough, and then in August -- August the 13th, just about two weeks ago, the Union of Concerned Scientists filed a motion in the prehearing conference for a summary disposition in the matter. And quoting back the staff's own words that we had said in these earlier pleadings that we did not know how to calculate the safety benefit that accrued in a numerical sense, that accrued from the various TMI fixes. And they quoted us correctly. That is what we did say.

What we are trying to do now is to accumulate historical perspective of everywhere that the staff has spoken to use of numerical safety goals, probabilistic risk assessment, or whatever in doing the business that we do. We have a team of about five or six people that are presently writing this report. We hope to have the first draft out a week from Monday and have two weeks of internal review and then file it with the Board. That is our plan.

In doing so we will consider such things as the recent Appeal Board decision on St. Lucie-2 where they said that since the likelihood of all loss of AC power was what they perceived it to be, and it was too high a number, they wanted the plant to be designed to withstand loss of all AC, both onsite and offsite.

We will consider such things as the WASH-1400 studies. We have done other studies which we discussed with the committee on the probability of an out of sequence rod drop accident for BWRs. And we have a list of about 20 different historical events that relate to this subject.

What I think this report is going to say is as far as how the staff does business, it is routine business. We are doing a standard review plan. And in particular, how we did TMI-1, we did not propose fixes that achieved any specific numerical improvement or decrease in the various coremelt sequence numbers.

I think this report will be about half of an answer. It will be the negative half which says we do not do business that way in general. I believe there is a subsequent meeting of this subcommittee or a related subcommittee coming up in early October. I hope that, you know -- I am not trying to terminate this discussion, but I am trying to hold a promise that we are have an introspective look at how we do business.

We intend to document it. We intend to file it with the Board. We would be glad to discuss it at a subsequent subcommittee meeting in the very near future.

MR. OKRENT: Well, thank you, but that does not really address the point I was trying to make about what I think is a real difficulty in trying to assess a reactor design -- let's say Zion or Indian Point -- that has a factor of 10 less chance of, let's say, coremelt than the average reactors that you have.

And I think the problem arises that that factor of 10 gets you down to a rather low number unless the first number is very, very high, at a point where in fact you would have to fix it.

MR. DENTON: It is easy to count people.

MR. OKRENT: Yes.

MR. DENTON: But it is harder to know that you have in the plant --

MR. OKRENT: I will make one other point, and I am doing this primarily to again point out what I think is a difficulty here. As you indicated earlier, when people do probabilistic analyses, they differ. It is fairly easy for someone who needs to make a decision to either find an analysis that he likes or to have only one made that happens to fit the direction in which he wants to go. That does not mean that it is necessarily a sound basis for it.

MR. DENTON: I blow hot and cold on the use of risk assessment in the li ensing process. At times I have been very anxious to move that way, and other times I haven't.

If you recall, we have been asked by the Commission what is the -- if we let B&W reactors continue under construction, for example, and we tended to answer from a deterministic standpoint. We asked Research to do a study for us. That study has been underway now for at least a year past its due date. It is very hard to come to a final conclusion about whether or not B&W plants basically have more risk than other types of plants. For example, it is just hard to bring them to a close if you are operating in a decisionmaking mode that is a lot shorter.

MR. OKRENT: Just so my remark is not misunderstood, I am not against trying to use probabilistic methods. I am urging caution and quality control. I think this is something the staff itself should devote a substantial amount of resources to. We recommended it in the Safety Research Program.

MR. SIESS: I have heard the comment made twice by two rather different people, one, Harold Denton, and the other, Frank Rousan, that the problem with probabilistic analysis is that the results are so uncertain. And in each case within the context of the statement there was the implication that the deterministic judgmental method is not uncertain; and I don't really think that is true.

The uncertainties are more obvious in a probabilistic assessment. People with good judgment usually can put the uncertainties in their judgment. That is why their judgment is good. So I don't think there is that much difference.

It is certainly an aid to judgment. As someone once said, some people use statistics like a drunk uses a lamppost -- for support, not for elimination. You could the same thing with probabilistic risk assessment. You could use it to support your position before a lice..sing board, o: you could use it to illuminate your understanding of the problem and seek out things that you might not find otherwise.

I don't think it is a final answer, but it is a very powerful tool, and there is nothing wrong with it simply because the uncertainties are there. You are not going to get rid of the uncertainties just by sweeping probabilistic risk assessment under the rug.

MR. OKRENT: Thank you for stating my opinion so eloquently.

(Laughter.)

MR. EBERSOLE: (Inaudible.)

MR. SIESS: You can do it to support it.

MR. EBERSOLE: (Inaudible.)

MR. SIESS: The one thing you could do with it is not believe it.

MR. OKRENT: Well, any other comments in this area? You may or may not be aware, Harold, that in the discussion on TMI at the full committee meeting last month, TMI-1, I asked the staff man who was here whether consideration had been given to treating TMI-1 in a manner similar to Indian Point and Zion.

MR. DENTON: I was not aware of it, but we have been asked by the Commission to think about other plants that need the Indian Point/Zion/Limerick type treatment. And I polled the staff for candidates, and I think I got back nine different lists. The Emergency Planning Group had their own favorite list.

Some people had population out to ten. Some people had population out to fifty miles. There are a few high population sites by common accord, such as Fermi, that would appear candidates. And I have given this job to the

Division of Safety Technology to look at all the possible ways of identifying others, and there probably will be other people that we ask to do the same sort of studies that we have not yet identified.

It goes to Dr. Kerr's point that we move down the list, the ability to distinguish one from the other becomes less and less. Indian Point, Zion, and Limerick, we have hit the clear high population points in the country, and as you begin to pick out others, they become less and less obvious.

MR. OKRENT: Well, maybe we should go on to the next topic then. How does the staff want to proceed?

MR. ROSS: I believe we are ready for the ice condenser which used to be item E.

MR. OKRENT: Okay.

(Slide.)

MR. ROSS: We were asked to --

MR. DENTON: Let me start this one, Denny, by trying to recap where we are.

We have proposed to the Commission, based on Sequoyah, that to issue the license for full power, recognizing that the efficacy of the systems there had not yet been proven and that the staff was not that concerned about the risk during this interim period of operation. And I base that on the fact that Sequoyah is in the same kind of risk space as an average plant like Surry and Peach Bottom.

It was not an outlier. It was a standard plant at a low population site. And that a lot of effort had gone into reducing the risk of plan* since TMI, especially for small LOCAs. And I also thought I had the advice of the ACRS that it was not undue risk. In fact, I thought they had a really good program which was likely to show that igniters would work, and over the next few months they are in a startup mode, that they would be shutting down after a few weeks to do some filter replacement.

So the total core inventory, by the time we reached a decision, in my mind did not present an unreasonable risk, and the re[illegible]tions did not require it, that they be designed for it.

But in our presentation today I do not want to imply that I am opposed, you know, to waiting until the igniters are fixed. I have a feeling it is not necessary. And Denny can go through and explain the total program.

But it goes back to the point that I was trying to make earlier, that if there is a chance to improve safety some how, I am for it. I don't think in this case it is necessary.

With that introduction, Denny, why don't you tell them what we know about the ice condenser?

(Slide.)

MR. ROSS: This discussion is slanted toward the TVA family of ice condensers. We recently have gotten a letter from Duke Power with respect to McGuire. They are pretty much following the TVA chain. They are also sponsoring their own risk.

Battelle-Columbus did a risk assessment study of Sequoyah, so I think the comments would be for all of the ice condensers we expect to be licensed in the next few years.

Also, in terms of paperwork, TVA is filing this week some time a very large document, about 700 pages, that deal with the general matters that I have on these three slides: safety -- I don't have any slide on schedule, but the work I will describe we hope will be finished in the next two or three months.

One of the ingredients of what is known as the interim distributed ignition system is: is there any adverse effects? So we expect to review -- expect TVA to file information that we would review if anything went wrong.

The primary adverse effect is probably an unanticipated local detonation, which is this item here. The potential consequences of -- there have been very crude preliminary calculations to shows that the steel shell could stand a very short pulse width accompanying a detonation. Whether it would withstand the quasi-static pressure, it depends upon how big a sphere one postulates reached a detonatable mixture and detonated.

The whole concept of the distributed igniters is to burn the hydrogen more or less as it comes off rather than waiting for a containment boil at some level. We expect the TVA report that is coming in to discuss this in detail in terms of the efficiency of how well the igniters would work, the general approach on source of hydrogen. And TVA so far has used some studies by Battelle-Columbus on postulating various degraded core sequences, sequences that lead to melt as the source term for hydrogen.

(Slide.)

They are using a new computer code called CLASIX which is just an elegant ice condenser code that accounts for burning at preset levels. And the combustion products flow through the ice exchange energy and then interact with the upper compartment spray and have further energy exchange.

A large part of the filing that is coming in this week should describe CLASIX. We have seen no report on it yet.

The purpose is to burn it such that the yield strength is not exceeded. Preliminary calculations show that indeed the igniters would achieve this function. So we are just getting information. A lot of the work that we will do will be reviewing the code.

We also have a crude confirmatory analysis method using the MARCH code, which is not anywhere near as elegant as CLASIX is reported to be. We will do some audits with MARCH for whatever merit they may contain. We would like to do a preliminary evaluation over the next 60 days.

(Slide.)

MR. EBERSOLE: Will this include consideration of the relative ease of ignition in a heavily saturated steam environment versus dry to determine whether the rates of combustion are more governed by --

MR. ROSS: The question is will the igniters work.

MR. EBERSOLE: That is right.

MR. ROSS: There are several experiments going on. We are sponsoring some experiments in Livermore using about a 10 cubic foot steel shell with the actual igniters that TVA is going to use and done in a steam-hydrogen-air mixture over the range that one projects from the various hydrogen source terms from MARCH calculations.

There is a test facility in Massachusetts at the Fenwall Laboratory, a 144 cubic foot vessel. Again, this will have hydrogen, air, and steam with the TVA igniters over the range of conditions; and this is a little more dynamic in that I think they have a fan blowing air past the igniters to get a little more representative test condition.

We have people at this laboratory today looking over this experiment. TVA is sponsoring these tests, and they expect to be finished this month, at least for the first test series.

The efficacy of the igniters -- that is, will they work, how long will they work, what mixtures of hydrogen, steam, and air will they ignite -- should be revealed by this. This is input then to the computer code like CLASIX so that one knows where to set the user input numbers on ignition start and ignition end.

They hope by igniting at relatively low concentrations of hydrogen, like 7 or 8 percent, that the burn will not be to completion. It might burn down to 3 or 4 percent and you would have more burns, but they are less energetic than if you had one big burn.3

Repetitive burns gives the ice and the sprays the time to do their function. It limits the peak pressure. This is pretty much where we are with the distributed ignition system. I think Duke Power is probably about a month or two behind, roughly. They are following TVA. They are contributing to the TVA work, but we have not gotten the depth of material or into technical discussions with them yet that we have had with TVA.

That is pretty much what I wanted to say on this subject.

MR. ETHERINGTON: When you say ignited 8 percent and burned out to 4 percent, what is the basis for that, that you don't have a uniform mixture, or what?

MR. ROSS: The flame would not propagate downwards. There is a limited -- there is little or no data on the turnery mixture of steam, hydrogen, and air. That is, at what point does it ignite, and how complete does it burn? The binary mixture of hydrogen and air, there is some data that says, if you can ignite it at 7 or 8 percent, it will not burn to completion.

So, the basis is extrapolation of binary data to a turnery mixture. I think it is because the plane does not propagate downward if there is low concentrations.

MR. SIESS: Is there some reason why you have not mentioned D. C. Cook?

MR. ROSS: We have had no discussions with Cook. It is not being overlooked. We have recently sent D. C. Cook an information package which consists of a preliminary staff report on Sequoyah where we discussed a number of things, and we also sent them the Sequoyah Commission transcript from a week or so ago.

Among other things, there was discussion by the individual Commissioners that they pretty well thought individually that source terms of hydrogen greater than 50.44 ought to be considered.

MR. DENTON: We alerted Cook to the issue, and they don't --

MR. SIESS: You don't consider the whole issue urgent enough that you need to look at an operating plant as compared to near term or plants under construction?

MR. DENTON: We recommended that on the very small containments, the Mark I's and II's, the inerted. That led to the discussion about the several BWR's operating non-inerted. I thought the question on the somewhat larger ice condensers could be deferred for a period of time, but I agree that ice condensers should solve this problem in the long term. There is a risk reduction that can be accomplished by accommodating hydrogen in ice condensers, but I did not see that as critical.

MR. SIESS: The staff did not feel it was so critical that you needed to do anything about the operating plants. You are concentrating on the NTOL's to make the fixes that can be made before operation.

Now, this is the staff's position. The Commission has recently raised a question about Sequoyah which, if it holds, would then apply equally to D. C. Cook, would it not?

MR. DENTON: Correct.

MR. SIESS: The concrete containment at D. C. Cook does not make any significant difference, I think.

MR. ROSS: It is different. We were sponsoring a structural calculation yield and ultimate for it by the Ames consultant. I have not seen the numbers yet, but it may not -- I don't think it makes that much difference.

MR. SIESS: It will have a higher -- what was the design pressure for Cook?

MR. ROSS: Fifteen, I believe.

MR. SIESS: It won't come out that much different.

MR. ROSS: No, they have one feature -- they have a lower compartment spray, and Sequoyah does not. It probably would be significant. Cook has a lower compartment spray.

MR. DENTON: And the next one in line would be McGuire, which is different still.

MR. SIESS: How is McGuire different?

MR. DENTON: It was designed by still a different

AE.

MR. ROSS: Its shell is 50 percent thicker than Sequoyah.

MR. SIESS: Fifty percent thicker than what part of Sequoyah?

MR. ROSS: Where the thin section on Sequoyah is one-half, McGuire is three-quarters, and so on. They both get bigger as they go down.

MR. DENTON: None of the three are identical that I consider in the same category. There is the operating plant at Cook. There is Sequoyah under consideration, and McGuire, that will be finished in a month or so. We have alerted them all. They have had owners' group meetings, and they are all involved.

Then there are two operating BWR's that are not inerted, either, Hatch and Vermont Yankee. So, it is timely to come to a decision on the ice condensers so that we can backfit if necessary and front it likewise, depending.

MR. EBERSOLE: Since you mentioned the lower compartment sprays, I am obliged to ask a question about it. Have you established the most rapid condensation rate and therefore depressurization rate of the lower compartment when it is filled with 100 percent vapor and is suddenly filled with cold spray?

MR. ROSS: You are referring, of course, to a partial vacuum.

MR. EBERSOLE: Oh, yes.

MR. ROSS: I don't know whether that has been done or not. I can find out.

MR. EBERSOLE: Originally, that was the reason that the spray disappeared from Sequoyah.

MR. ROSS: Since the full committee is going to take this matter up tomorrow afternoon, it is a 24-hour answer you will get.

MR. OKRENT: Just as an aside -- maybe not such a small aside -- if one is going to consider measures for hyrdogen control on ice condensers, one has moved beyond the ordinary design basis, and one could have any of several approaches in mind.

For example, it could be that substantial hydrogen buildup is more probable than the more serious degraded core accident, and if so, by dealing with it, in fact, we are making a substantial reduction in risk, even though we are not currently dealing with the next one.

Or, we are going to deal with both of these, but this is the one we are able to deal with first, and we plan to follow the next one alone. Or core melt is more probable, that this is one we know how to deal with at the moment. Or, you know, there are other variations of this.

Is there some one of these or a fourth or fifth one that would currently define the staff's position, would you say?

MR. ROSS: I think Frank Rousan may want to speak up for Bob Bernero. I think the staff viewpoint is a factor of 4 or so is what they would expect to be the reduction in risk for an ice condenser that would accompany things like inerting or a distributed ignition system that worked. Frank, is that a fair reflection?

MR. ROUSAN: That is right.

MR. DENTON: I think a factor of 4 is worth going after.

MR. SIESS: I thought that same analysis said that there was a greater reduction in risk for hydrogen control in an ice condenser than there was reduction in risk for Mark I or Mark II BWR's. Just looking at the relative values and the relative staff actions on the two, I don't find them in correlation.

MR. ROSS: You are back to the lamppost argument, because the risk argument does not support the viewpoint that the additional Mark I's ought to be inerted, if that is your point.

MR. SIESS: Yes.

MR. ROSS: That argument was based on other factors than --

MR. DENTON: That reflects an approach which finds that those small containments rupture anyway, and therefore being able to cope with the hydrogen provides little additional risk reduction.

MR. SIESS: I was just looking at the relative risk determination and the relative degree of urgency assigned by the staff. I think either action can be justified without looking at the other. By taking one compared to the other, they do not seem to make a lot of sense.

MR. DENTON: We have always had this gut feeling that small containments like GE ought to be inerted. It may be true that sure enough, they will fail due to --

MR. SIESS: At what confidence level?

(General laughter.)

MR. DENTON: Plants that have operated successfully inerted. We see little downside in doing it that way, and I think we ought to control hydrogen in these ice condensers. The only issue before us is, I think, do we require it to be demonstrated before or after the operation of the plant?

MR. EBERSOLE: I would like to point out one advantage that inerting brings in a lefthanded way.

MR. DENTON: Fire reduction.

MR. EBERSOLE: That is one, but the one I think that is quite significant is, it mandates in the beginning a concept which I think should be pursued universally, and that is, you keep sensitive and frequently maintain instrumentation and other garbage out of a potentially hostile environment. It is unfortunate in the advance of the technology of the BWR's that they are now invoking in the BWR an abandonment of the original concept of where this sensitive instrumentation with frequent maintenance is to be put, and now they are embracing the PWR design, which puts it right inside the containment, which means you must march in and fix it all the time, but worse than that, you have to invoke a tremendous R&D program to demonstrate that it will work in the hostile environment at all.

So, one of the advantages of the small containment and the inerting that went with it was, you did not have to cope with your backup relief that the apparatus or post-LOCA or post-accident functions -- you did not have to worry about it not working.

MR. DENTON: Even burning the hydrogen raises some question about equipment qualification. There were some signs in the TMI containment of the hydrogen ignition. Not a lot, but you could tell something had gone on.

MR. EBERSOLE: That will include a pressure shock as well as the temperature.

MR. ROSS: As far as I can tell, the score is, the last item on the agenda has to do with the non-safety grade equipment.

MR. OKRENT: There was also one -- let's see. Were you going to tell us about your recommendations for action on degraded core cooling, in other words, what your approach was? Is that right?

MR. ROSS: I thought we had covered that. If you want more on that, Dr. Speiss could give some remarks.

MR. SPEISS: Basically, what I can do for you for ten minutes or so is summarize where we are. I gather from this morning's conversation that you people have read the -- at least the long-term rulemaking, and you were not too happy with some of the sophomoric questions that were raised.

MR. OKRENT: I don't think we were trying to indicate we were unhappy with the questions. I think there was a question as to whether it would be useful in addition to these questions or in place of to have a proposal, a tentative proposal for people to look at and say, this is good or bad for the following reasons.

One of the reasons that we are going that way for the long term is to get as much information as we can.

MR. KERR: Mr. Speiss, redundancy in this company is considered desirable, and here I think it is necessary. We both are making our suggestions based on the assumption that the approach that we recommend will elicit more information. We were not suggesting that the staff provide a plan, because we thought it would cut off the flow of information, but rather because we thought it would perhaps produce information, more of it and more useful information.

So, when you tell me that the staff approach is being used because the staff thinks that you want information, I guess if you can somehow convince me that your approach will produce more information than publishing a proposed plan, I guess I would find it convincing, but I have seen no evidence up to now that that will produce more useful information.

Do you have some way of --

MR. DENTON: This is a two-edged sword administrators face all the time, but if you go out with a proposal, everyone says, you have it cast in concrete, you are not willing to listen. If you don't go out with one --

MR. KERR: Harold, I do not think that is true at all. I have been an administrator for more years than you have, maybe not done as much administration, but I have never gotten a response from people that when you give them something and you tell them it is a draft plan, that it is cast in stone.

I mean, if you operate so that people have confidence in your statement that this is a draft, and we want comments, and when they get comments, you take them into consideration, then people will not consider it cast in stone.

MR. DENTON: I think we have heard your point over that. Maybe we ought to describe what is on the Commission calendar for tomorrow, which we call the interim rule on degraded core, which we are proposing be in place while they consider the long-term rule.

MR. SPEISS: I will discuss both of them. If you will recall, the action plan called for an interim rule and a final rule. Basically, the interim rule, as Harold said, has been completed. It will be considered by the Commission tomorrow.

It consists of -- It is SECY 8399. It consists of hydrogen management, in-plant radioactivity considerations resulting from core degradation, and items that are categorized as decision-making involving detection instrumentation, training for core damage considerations.

Under hydrogen management, we are proposing, as has already been discussed today, that Mark I and Mark II be inerted. We are also proposing that pending the final rulemaking, which will consider the hydrogen management in its totality, all the licensees do analyses of how to take care of the hydrogen problem.

Also, we are proposing that dedicated penetrations be made available for plants that rely on external recombiners or plants that utilize a purge system, hydrogen purge system, and, of course, external recombiner capability for all plants that rely on hydrogen purge systems.

The rationale for inerting, we are proposing immediate inerting (inaudible) 8 or 9 percent to achieve the ultimate capability of the containment, whereas for ice condensers you have to go to a factor of three or four. That has been discussed. We are treating ice condensers on a case by case basis.

For large, dry containments, we see no problems right now. We feel confident the long-term rulemaking will take care of this problem.

MR. SIESS: You require recombiners for all the large, dry containments?

MR. SPEISS: Yes. The combiners, of course, take care of hydrogen up to 5 percent.

MR. SIESS: Okay, but those that now have purge, you will require recombiners.

MR. SPEISS: Yes. The items that I categorized as decision-making involve -- I have a list here -- detection for inadequate core cooling, accident monitoring, training to mitigate degraded core accidents. These are the items that are already being studied. All we are trying to do is codify them into a regulation right now.

MR. ETHERINGTON: You require hydrogen recombiners in the dry containments. Supposing someone comes in with a dry containment, and in view of the improvement of the igniters in the ice condensers, they want to install igniters. Would that be acceptable in a dry containment?

MR. ROSS: We have not looked at that. The requirement was so that no one would ever have to purge again. We had not envisioned that at this time. It is an interesting thought. It may be a loophole that we had better watch out for. We did not intend that the igniters be a substitute for the internal recombiner. That was not the intention, but I think the design basis events are probably quite different. But it is an interesting question.

MR. OKRENT: With regard to hydrogen in a large, dry containment, can it accept the hydrogen which would accompany a 100 percent metal water prior to ignition of any of the hydrogen?

MR. SPEISS: If we reach the design conditions, which are around 50 psi, it is around 65 percent. If you burn 65 percent of the hydrogen, you reach the design pressure. If you burn 100 percent of hydrogen, you are still within the limits of a dry containment.

MR. OKRENT: That assumes that your starting pressure was what?

MR. SPEISS: It is --

MR. OKRENT: Was it atmospheric -- Okay.

MR. ROSS: It does not have the additive LOCA or steam line break on it. It does not start at 50 pounds. That is my recollection.

MR. OKRENT: And you reach detonative concentrations in any of the large containments if you do not burn?

MR. SPEISS: Yes.

MR. OKRENT: I guess I do not understand, then, your comment about -- I cannot remember the exact words, but they sort of left the impression that hydrogen was not too important a question for large, dry containments.

MR. SPEISS: It is not.

MR. DENTON: TMI had an explosiion (inaudible).

MR. OKRENT: Yes, but that -- Do you have some basis for judging that something that led to the equivalent of 100 percent of the core zircaloy reacting is sufficiently improbable that it does not have to be considered? Or what is your thinking?

MR. SPEISS: We feel that the probability of 100 percent versus 10 percent is much higher. Battelle-Columbus has done some studies. They have indicated that the probability of getting 10 percent of hydrogen in containment versus 50 percent is (inaudible). These types of numbers --

MR. OKRENT: Ten percent is an order of magnitude more probable than 50 percent?

MR. SPEISS: Yes.

MR. OKRENT: I would like to see the studies.

MR. KERR: So would I.

MR. BOSS: You had a question about information. You said, what did we have with respect to pressure effects on large dries. The interim rule would require each large, dry containment owner to produce that number in a period of six months from the date of the rule.

We have only done a couple of calculations ourselves, and they have not covered the full range starting from LOCA conditions and arbitrary amounts of hydrogen. We don't have all that information in. We only have a very few calculations of our own, and nothing from the industry.

MR. SIESS: I got an impression from what was said a minute ago that may be wrong. I would like to be sure. The interim rule has been listed as a series of very prescriptive requirements. In answer to Mr. Etherington's question, I got the impression that the requirements are really what you are after, that you have not really formulated performance criteria, or have not stated them.

Denny says the object is not to have to purge again, or somebody said that. Can this stuff be expressed in terms of what you are trying to do under certain conditions rather than how you want the licensees to do it?

Right now I am not concerned about how _ ι promulgate a rule, but I think to understand what you are trying to do, I would rather try to understand it in terms of your assumptions and your objectives.

MR. SPEISS: As I said, the rule contains three distinct areas, hydrogen management, a number that I classified as decision-making -- they involve six or seven items which came out of the TMI action plan. We have precise criteria for those six or seven items. One of them is, for example, high point bending, high point bending.

MR. SIESS: That is not --

MR. SPEISS: No, it is -- In addition to that, we have sent out explicit criteria on how to design --

MR. SIESS: That is exactly what I am asking about. That tells somebody exactly what you want them to do and how you want them to do it. It does not say what you are trying to accomplish. There may be other ways of doing what you are trying to accomplish, or what you are telling them to do may not be the way to accomplish it.

Just like Denny says, this may fall between the cracks. You can fall between prescriptive cracks. You can fall between performance criteria cracks.

MR. ROSS: Let me take a short answer. If you look at the prescriptive portion of the rule as an example, "Facilities that rely on purge systems as as primary means for controlling combustible gases following a LOCA shall be provided with the capability to instal external recombiners following the start of an accident."

That is a prescriptive specific thing. You look at the statements of consideration. You have more in there of what it is trying to accomplish. What Mr. Etherington was saying was -- you see, distributed igniters are not covered by the rule at all. They are not mentioned anywhere in the rule. But if a person were clever enough to come in and say, if I put distributed igniters in, and if they work to control the hydrogen, then I never need to purge, so I will never need an external recombiner.

MR. SIESS: And you could say that on the basis of the --

MR. ROSS: If a clever person came in -- No, I think the rule is clear enough. I don't think there was a loophole in th rule, but if you looked at the statements of consideration, he might say, well, you know, I meet the spirit of the rule because I control hydrogen.

MR. SIESS: The way you have the rule written, he could not do it.

MR. ROSS: Good. Otherwise, he would not have a way to get rid of the hydrogen so he could go in for recovery.

MR. EBERSOLE: Why can't the rule be written both ways?

MR. SIESS: Why can't you say you don't want to have to purge, and you do want to be able to get in there?

MR. ROSS: You want to get rid of the hydrogen, which is what the recombiner would ultimately do for recovery.

MR. SIESS: Somewhere you should say what you want and give them other ways of meeting those criteria.

MR. ROSS: I think a fair reading of the statements would probably show that we have covered your point, but I would have to take the time to read it, and I don't want to take the time to do that now.

MR. OKRENT: Are we headed in the direction of the long-term approach? What we have heard, I guess, is the short-term.

MR. SPEISS: The long-term rule, the advance notice has come out, SECY 80-357. This is the one that has been cast in a number of questions, 18 in all. The objective is to provide the industry and the public input into the regulation. What it covers -- it talks about the various aspects of degraded core cooling, again, in the form of questions. It talks about design criteria and a number of related things. We are going now for a 90-day comment period, and I think the schedule right now is six months or so to come out with a final rule.

I think in parallel with this we have the Zion-Indian Point studies, where both the utility and the staff are doing studies and application of studies in this area. All that information will be very helpful in coming up with a final rule that would give some direction to express our opinion in some areas.

MR. OKRENT: I am not sure I understood the schedule as this --

MR. SPEISS: My understanding is that the final rule --

MR. OKRENT: Has the Commission published for comment yet this --

MR. ROSS: The Commission is voting tomorrow on whether -- they are voting tomorrow on whether to publish the advance notice of rulemaking. It has not been published.

MR. OKRENT: Okay. As it is currently worded, is it suggested that a final rule would be adopted, did you say six months, after some period of time?

MR. SPEISS: It would take 90 days to get comments from the public, and it will take about three more months for the comments to be assimilated and digested, and come up with a final rule.

MR. OKRENT: No hearing?

MR. DENTON: I think that only starts the process.

MR. SIESS: You cannot write a Reg. Guide in six months. I know you are not going to do a rule.

MR. DENTON: I think this would allow the staff to do a proposed rule, and that would go out for comment and rulemaking or adjudication or whatever. I think it is going to be years before there is a final rule on degraded core cooling.

MR. SPEISS: Six months as a rule to the Commission from the staff.

MR. OKRENT: You don't have any such rule in mind then, any draft concept or so forth, I gather?

MR. SPEISS: Not in the NRC organization.

MR. DENTON: I would hope maybe out of our studies we might come up with something, but at the moment we do not.

MR. OKRENT: Anything else on this topic? Dr. Kerr?

MR. KERR: I have nothing. Thank you.

MR. OKRENT: Okay. We have one more topic.

MR. ROSS: Cecil Thomas wants to speak on the subject of non-safety grade systems. This is a lead-in to Paragraph 3 on cascading failures.

MR. OKRENT: How about a break after this topic?

MR. ROSS: I understand.

MR. KERR: If Mr. Denton is leaving, I think we should thank him for his participation.

MR. OKRENT: Yes. I think it was quite useful, and I hope it can occur more frequently, either with the subcommittee or with the full committee, because I can remember back in the old days Mr. Price had many hours that he spent with the ACRS. We found it useful.

MR. KERR: I found it very useful. I hope you did.

MR. THOMAS: I am Cecil Thomas. I am going to talk about our approach to evaluating the effects of the failure of non-safety systems on plant safety functions.

We are approaching evaluation of this subject from the standpoint of systems interactions. That is, we view the impact of non-safety system failures on the abilities of plant systems to carry out their intended safety functions. This is one aspect of the overall subject of systems interaction.

Later today, after the break, John Stolz is going to describe in a little more detail our overall systems interaction program, and more specifically the activities of our new systems interaction branch. In order not to usurp too much of what John will say, what I would like to do is just highlight the three methods by which we are looking at systems interactions now and hence the ways in which we are looking at the effects of non-safety system failures of plant safety systems.

(Slide.)

MR. THOMAS: The three methods that we are presently looking at involve, first of all, plant operating experience, second, the so-called walkdown method, and three, what I term quasi-analytical methods.

Plant operating experience is probably not the best way to diagnose systems interactions. It certainly has a number of limitations and disadvantages. First of all, the nature of the information does not readily lend itself to the diagnosis of systems interactions, even those that may have occurred. The information presented is more or less aimed at the actual events and the description of the events and the consequences, and not necessarily descriptive of possible interactions that occurred in the meantime.

The method does not lend itself particularly to the postulating of interactions that might happen in the future, but it at best would provide some information about interactions that actually had occurred.

Finally, a major disadvantage of the method is that the information obtained is generally after the fact. Nevertheless, we do think there is information to be accrued from the use of this method. Therefore, we look at it as a necessary but by no means sufficient method of diagnosing systems interactions.

Secondly, the walkdown method was a method that was used by Pacific Gas and Electric Company in their systems interaction program for the diagnosis of seismically induced systems interaction for the Diablo Canyon nuclear plant, and it is expected to be used by PASME in their systems interaction program.

In this method as it was applied by Pacific Gas and Electric Company, safety related systems and components were designated as targets. Non-safety related structures and components were defined as sources. A walkdown team composed of representatives from the major disciplines, electrical, mechanical, structural, and so on, conducted a walkdown of the target equipment.

During the walkdowns, they put themselves in effect in the place of the target equipment and looked around to see what sort of source equipment could prevent or could interact, first of all, with the target equipment, and secondly, would those interactions be detrimental, would they prevent the safety related systems from carrying out their intended safety functions.

So, in short, the walkdown team postulated interactions between source and target equipment using previously established criteria, and they recommended resolutions. The findings of the interaction team were reviewed during an office-based technical evaluation and modifications were made as necessary.

I would point out to the subcommittee that we are presently planning on meeting with the subcommittee on -- I think it is scheduled on October 8, to discuss the Diablo Canyon systems interaction -- seismic systems interaction plan and our evaluation of it.

MR. OKRENT: Will you have something in writing before that time?

MR. THOMAS: Yes. We plan to come out with a report within two weeks. It is in the final stages of review right now. So, basically, the walkdown method we feel lends itself readily to the diagnosis of potential physical systems interactions, and I emphasize the word "physical."

MR. EBERSOLE: That is only in the context that such interactions proceed through space rather than are intertied through the systems themselves.

MR. THOMAS: Not necessarily. There is one interesting aspect that does not require space and as an example, if you have a valve that is powered by non-qualified air or power, the valve has a required or assumed failure mode. It is possible to have a physical interaction on the power source or on an air discharge line of a valve or whatever. That could prevent the valve from functioning. Even though the original initiating event was physically induced, it was transmitted through a process such that the function of the valve could be impaired.

MR. EBERSOLE: What I am saying is, is this walkdown method that you are talking about only complementary to the process intertie evaluation?

MR. THOMAS: Yes. Yes. Yes.

MR. EBERSOLE: You don't have that up there. The process intertie, which is a representation of --

MR. THOMAS: I am considering that --

MR. EBERSOLE: Your background here --

MR. THOMAS: The walkdown method and the quasi-analytical methods are complementary.

MR. EBERSOLE: Okay. The bottom, the quasi --

MR. THOMAS: Neither in themselves -- they are both necessary, but neither in themselves sufficient.

MR. EBERSOLE: Is the quasi-analytical the diagrammatic evaluation?

MR. THOMAS: Yes, yes. Basically those methods -- I will move on to the quasi-analytical methods. They may involve such things as but not necessarily limited to failure modes and effects analysis, fault tree analysis, event tree analysis, and possibly some other ways the methods appear to lend themselves to the diagnosis of what I will call functional systems interaction.

As Mr. Ebersole pointed out. The method looks like it would be a complement to the walkdown method. As you know, some work has been done on the use or application of these methods to systems interactions. The branch currently has contracts with Battelle, Lawrence Livermore, and Sandia Laboratories to help us in developing these m... ds further to the extent that they could be practical and meaningfully applied to the diagnosis of systems interactions, and I think John will talk a little bit more about our contracts in his presentation, but this is something that we are just beginning to look at.

As you know, the Sandia effort was -- the first part of the Sandia effort showed that maybe we bit off more than we could chew. We jumped in over our head. It may not be a practical method to apply the diagnosis of systems interaction during the licensing process. We need to look at other ways to maybe apply these methods, and that is one of the things we are asking these laboratories to help us come up with.

So, in summary, we are pulling a three-pronged approach to the diagnosis of systems interactions and hence to the evaluation of the effect of non-safety system failures on plant safety functions, namely, plant operating experience, walkdown method, and quasi-analytical methods.

MR. EBERSOLE: In respect to the last one, are you accounting for the fact that the interrelated parameters that could be pressure or level or voltage or amperes or whatever in fact can fail in a variety of modes, not just to the extent that they are off or on or low or high, but they can be intermediate to failure, and that you could have an excess of good things like voltage or pressure.

I mean, the whole field of range, the range of movement of the parameter has to be looked at, and the rate at which it moves. Most of the logic has been built on something totally failing instantaneously.

MR. THOMAS: I want to emphasize we are not doing it. This is one thing we are very acutely aware of. It is part of the program to study degrading conditions as well as off or on.

MR. ROSS: I think a good example of what the staff-- it did not turn up through the systems interaction study, but a good example of what you just said is something that Mr. Satterfield could elaborate on. We discovered in our recent Farley II review where the DC power to all six of the auxiliary feedwater control valves came from a single power supply. If one postulated a degraded condition excessive voltage, one could postulate that all solenoids would be frozen, and you have to de-energize to get the aux feed function.

I think the studies showed that would not happen, but it is still a postulated failure mode, and one thing that is going to come out of it is, they are going to have to separate and put it on different buses.

MR. EBERSOLE: We mentioned it in terms of --

MR. ROSS: The cleverness of the reviewer brings things like these -- when it is discovered action is taken.

MR. EBERSOLE: An example is the one looked at a little bit by Carl Michaelson but not yet developed in detail about the implications of progressively failing air, which is a non-safety system on the dump valves, on the Brown's Ferry type scram system, which apparently can do some interesting things simultaneously, and they in fact have the capability to degrade the performance of the boron injection system, since progressively failing air -- I am not quite sure, but I think it may tend to lock open certain valves for which that system has no design allowance.

MR. THOMAS: We are aware of the problem, and we plan to take this up.

MR. KERR: I would hope for the development of a systems interaction division or branch -- what is it?

MR. THOMAS: Division of Systems Interaction.

MR. KERR: We would not make this an end such as redundancy and divergency. Our goal is reliable plants, and not the invention of systems interactions. For example, I just heard the fact that six solenoids as powered from one source is a potential common mode failure, and it certainly is, and then the conclusion was that it is better to separate them.

Now, it certainly is better to separate them if at all costs you want to avoid common mode failure of systems interaction, but if you want to get a reliable system, I don't know whether it is better to separate them or not, and it seems to me one needs to look at this question, and at other similar questions. The fact that you have a systems interaction does not -- it seems to me it does not drive you immediately to another fix. It is so obvious I hate to say it, but what we have to keep in mind, I think, is that the goal of this activity is to finally devise reliable plants and not to avoid systems interactions.

MR. EBERSOLE: The price of (inaudible).

MR. KERR: That risk must be looked at.

MR. OKRENT: Can I ask a slightly different question? On August 12, 1980, the ACRS sent a letter to Chairman Ahearne, new unresolved safety issues, and it suggested a few items that might be added to the list. One was control system reliability, and the committee noted that a related issue to that was the reliability of non-safety system information displayed for use of the reactor operator.

Now, in a sense, that is one category I would say of the general topic of the effect of failure of non-safety systems on plant safety functions. I would be interested in hearing how you plan to examine the question of control system reliability, assuming that you have such plans in mind. It is a topic we and others have identified earlier, just using this letter as a convenient point of reference.

MR. THOMAS: Let me give you a partial answer. It is all I really can give right now. First of all, we have to be careful and make sure -- we have to separate, I think, the difference between systems interaction and systems reliability. Right now, the systems interaction branch is acutely aware of the need to consider control systems failures as a subpart of the overall subject of non-safety system failures and the impact on plant safety functions.

At this point, we are discussing the need to do this within our own staff and with the laboratories that we have asked to take a look at this. We do not have any final recipe yet or even an intermediate recipe for the way in which we would go about looking at this, but at the outset, I think the first step is to ensure that the plant safety systems could accommodate the failure of a control system, let alone the reliability of it.

I think that may be the next question that needs to be answered, but it is something that we are concerned about. It is something that we are aware of, and it is on our list of things to be developed. We have not progressed that far yet. We really just have started. We have a pretty small staff, and we are working along those lines, but we have not come up with a recipe yet.

MR. KERR: Could I interpret that answer to mean that you know the problem exists, but you have not yet done anything about it?

MR. THOMAS: Very succinctly put, yes.

MR. SATTERFIELD: We are presently working with the working group in the development of a standard that would be applicable to instrumentation systems not normally classified as safety. As a part of that effort, we will try to come to grips with some of them. Just how, I am not sure, but the question of reliability -- we also have undertaken a study of B&W, and we propose to discuss that with you.

We see some improvement that might well be made in that completely integrated control system, but all control systems for B&W plants. We are not now sure whether or not such changes can be made. We are going to have to again come to grips as part of that study with what reliability requirements ought to be applied to these systems.

We don't have any answers yet, but I think it is not a matter of the fact that we are not doing it.

MR. KERR: I have not had a lot of experience with standards writing, so don't take my remarks too seriously, but I would assume that if we were going into a problem like this, you would first try to decide if a problem exists before you write a standard to solve it, and it seems to me a basic issue that has to be faced at some point is, does it make sense for a licensing body to set standards of reliability for non-safety systems? Maybe there are other ways of formulating that question, but at present it seems to me in most cases one does not set such standards of reliability and does not look at such systems in any detail.

MR. SATTERFIELD: Certainly at present we don't now.

MR. KERR: But before you write a standard, don't you have to answer that question?

MR. SATTERFIELD: Yes. I think we already have answered that question.

MR. KERR: That is what I wanted to hear. What is your answer?

MR. SATTERFIELD: Whether or not --

MR. KERR: No -- No, whether you look -- whether you set standards of reliability for the non-safety circuits or systems. Okay. The staff has now concluded --

MR. SATTERFIELD: (Inaudible.)

MR. KERR: You have now concluded that the staff should --

MR. SATTERFIELD: (Inaudible.) One, you must demonstrate that the plant design is sufficient to accommodate the variety of events that have occurred, but also you have to make some determination as to the frequency with which that event occurs.

MR. KERR: Yes. But --

MR. SATTERFIELD: I don't think there is any way of escaping it.

MR. KERR: It seems to me, if I understand the philosophy that prevailed earlier, it was assumed that there were parts of the plant that were related to safety, and there were other parts that were not, and the regulatory responsibility was with that part that affected safety. There is not anything illogical about that approach.

MR. SATTERFIELD: I think there is.

MR. KERR: If it can be made consistent. That is, if one indeed can separate parts and say, this part has an effect on safety and this part does not, and I think to some extent one can do that. There are parts that probably one has some difficulty deciding.

MR. SATTERFIELD: I don't think you can define in design those parts of the plant that are required for safety if you do not understand the systems that fail, and thus cause a challenge to those systems. I think that is the part of the picture that we really have not been able to define.

MR. KERR: It would seem to me that one had to understand the system well enough to know whether some particular part does challenge safety or not. I was trying to find the TNO here that came to me recently in which the NRC staff was notified of an unusual event, and this unusual event was that a plant was down for more than two days, and the reason it was down for more than two days was because the cooling system and the state of the electrical generator had malfunctioned.

That could be interpreted as having a great deal of safety significance, but it seems to me on a scale of things from one to 100 I would put that somewhere around one and a half, and one, it seems to me, has to make this sort of judgment when one allocates resources, and it was that sort of thing.

I wonder if you have a group of people that is sort of looking at systems and saying, here is a scheme of reliabilities and we probably are going to have to have some systems extremely reliable and others that aren't so reliable, or maybe there is some other approach. It was that kind of thing I was looking for.

MR. SATTERFIELD: I may be going a little bit too far at this point to say we are going to be able to find some miracle value that would (inaudible).

MR. KERR: I am not suggesting a numerical allocation, necessarily, but it seems to me there could be a scale of things more than, say, one and zero.

MR. SATTERFIELD: I think what we are going to find is, there are a few systems that we normally consider control systems which we really want to center our attention on. We probably to a large extent will ignore most of the others.

MR. KERR: Have you gotten far enough so that you would say the control systems perhaps should be as reliable as safety systems or half as reliable? Have you gotten that far in your thinking? Are you proposing --

MR. SATTERFIELD: I don't think we are shooting for redundancy in all control systems.

MR. KERR: I am not talking about redundancy. I am talking about reliability. I don't see how you can write standards unless you begin making decisions like this.

MR. SATTERFIELD: Maybe I misled you. The standard -- I have not seen the latest draft of the standard, but what the standard would do is to find a way of establishing systems with regard to safety so hopefully there would be a way of --

MR. KERR: It sounds to me as if you are saying that the standard is going to make the decision for you. I would hope that one would make the decision and then try to write a standard to set forth the decision. What is the decision process that is going to be used in deciding --

MR. SATTERFIELD: I don't understand that. It seems to me in designing a plant, one ought to have a pretty good idea of those systems that are important as far as safety is concerned, and you ought to be able to do that sort of a priori.

MR. KERR: Okay. But you use some sort of decision-making process, and it is that I am trying to get at.

MR. SATTERFIELD: It is that sort of thing that is addressed in the standard, hopefully.

MR. KERR: Well, I would think that the standard would be written after one had already made the decision, and the standard would describe how to implement it.

MR. SATTERFIELD: You have to understand what their decision is.

MR. KERR: What sort of place this is going to be used to allocate these degrees of responsibility? In your mind, could you sit down and in five minutes do that? Is that what you are telling me, that it is so straightforward that --

MR. SATTERFIELD: No, I don't think it is very straightforward.

MR. KERR: What process is going to be used then to make that kind of decision?

MR. SATTERFIELD: I think the standard provides a tool by which someone performing the design could arrive at what is important and what is not, and once you have done that, the sorts of things you have to begin to think about for those systems are important to safety. That is the intent. Whether or not it works out that way I don't know.

VOICE: I think it has been covered, but I was involved in the first couple of meetings of the working group, and the way I would characterize what is being done is this. The staff has made a decision that there are degrees of safety relatedness in so-called non-safety systems, and that therefore a standard is necessary to set up the means for or the criteria for assigning these degrees of safety relatedness. That is the intent of the standard.

Once that standard is completed, or the requirements for the system --

MR. KERR: What I am asking you is, what process did the staff use to make the decision as to which and how much these systems are related to safety. That decision apparently has already been reached, you say?

MR. SATTERFIELD: We have not made that decision. We have just made the decision that there are degrees of safety relatedness for the so-called non-safety systems.

VOICE: I think we are still searching around. There is no clear direction at all as to which system -- I think all of us have in our minds systems that we think are important to safety and those that we think are less important, but I suspect there is some difference of opinion among those of us sitting on this side of the room as to which those systems are.

MR. KERR: How are you going to decide other than by writing a set of standards which ones are? Are you going to do it by committee, by vote?

VOICE: There will be some committee work done, yes. Hopefully a lot of good judgment.

MR. EBERSOLE: It will be an interesting search to find that relationship. I will give you an experience in the years when continuity of operation was important rather than nuclear safety. I can recall a case where a domestic water inventory switch shut down ten units at one point in time. One switch, one $40 switch took out ten units.

MR. SATTERFIELD: I don't know any better way to go about it, just to get your feet wet and begin investigating systems that we had not looked that closely at. There is no question on this. There is no question on the fact that -- I think we have ignored those kinds of systems too long. It is not for me to go in and wholesale make changes. At least we will have an understanding that we probably don't have now. That is the objective.

VOICE: I would like to make some personal comments here. I learned a long time ago before you can solve the problem you have to know what the problem is. Control systems are a group of systems that are not to be reviewed by the staff, period, who do not know what is there, who don't know how the systems -- so before you can determine what the degree of the effect they have on safety, you have to realize that and take that for each one of them. If you were to do it, the methods, the tools are available. They are called failure modes and effects analysis.

Now, IEEE 352 is one of the standards developed a long time ago, and it describes the process. For some reason, we have been shying away from using this particular tool for reasons that I prefer not to speculate, because I don't know. We use them sporadically here and there improperly. The significance of using failure effects analysis is very basic, and it is a prerequisite. You have to establish the quality of the system or the quality of the failure modes of a particular system or groups of systems before you start quantifying through the event tree-fault tree analysis what the probability is and what the risks are so the tools are available. It is the willingness to use them. Thank you.

MR. OKRENT: Has the licensing staff put out a research request to the research office to do research on the possible effects of control systems on plant safety?

MR. SATTERFIELD: (Inaudible.)

VOICE: I thought I would discuss that in the next hour. To answer your question, we are going through a series of -- starting off with a state of the art review on a broad brush approach, I will address that next.

MR. OKRENT: If you are going to answer that question for your presentation, that will be fine. Why don't we take a ten-minute break?

MR. RAY: Before you do that, could I ask a question? It occurs to me that one of the avenues of interaction between safety and non-safety systems that is not easily evident from a physical viewpoint is by electromagnetic conduction between high capacity power circuits and the safety systems. Is this being considered in your evaluation, particularly from the viewpoint of what may occur when you have a short circuit in the power systems, physical separation is the answer.

MR. THOMAS: When you say in our evaluation, I would like to take the opportunity to say in the development of our program electromagnetic radiation is one of the items that we are considering, whether we will carry through with it or whether we determine it is important or negligible compared to some of the other higher risk initiators of systems interactions, that is to be determined. I would not want to speculate on that, but it is on our list of initiators to consider.

MR. OKRENT: Okay. Ten-minute break.

(Whereupon, a brief recess was taken.)

MR. OKRENT: Why don't we proceed.

MR. STOLZ: My name is John Stolz. I am with the staff. Before we started discussing systems interaction, we could go over the agenda for the remaining items, and I will give you a picture of what I am going to talk about.

(Slide)

First of all, we include cascading failures in the sense that you could find it within the definition of systems interaction, and I think we will hold off defining any precise definition of what systems interaction means until we get into talking about the program. I think all that stuff will fall out.

I want to point out even before we start that the status of this whole program is that we really still do not have a consensus on the methodology or precise definition or scope, and we will be getting into the program we laid out to try to arrive at all of this.

The first thing I am going to do is to be talking about the status of the program, the background of the action plan, how we are organized to handle it, what we feel our responsibilities are -- this is the new systems interaction branch within the Division of Systems Integration -- what we feel the systems interaction objectives ought to be, and then to get into the program covering the next two-year period.

With that, I think a lot of questions you have will fall out of that, including the concern you had about research, Dr. Okrent. I thought we would discuss the examples of systems interactions or cascades that you mentioned in your letter dated August 12, and also point out, as we agree with you, that these are additional examples of systems interactions, namely, the Browns Ferry 3 and Crystal River 3, and the power supply to the ICS covered in I&E Bulletin 79-27.

We will try to cover all those items this afternoon.

(Slide)

First of all, the Action Plan, Section II.C.3, is a subset of the reliability and risk assessment, and this particular section discusses three elements of systems interaction. One, it points out that we have a commitment to do a review on Diablo Canyon. Cecil gave a brief description of what that program covered.

The second item relates to the Indian Point effort, and back last October 12, 1979, the Committee wrote a letter to the NRC advising the course of action that Indian Point 3 should pursue regarding the systems interaction effort that should be made on Indian Point 3.

The last action on the item plan dealt with the development of regulatory guidance, and they talk about that in the context of what was going on at the time on the unresolved safety issue A17. I will come back on the status of that later on, but those are essentially the three elements that gave a prescription for the amount of money you should allocate. Generally this is what is kicking off our program.

Last April, as you know, there were two principal things that the agency wanted to do as a result of TMI. They wanted to focus on human factors and they wanted to focus on systems. As part of the organization of systems integration, they formed a branch called systems interaction, and I am chief of that branch right now.

(Slide)

Briefly, these are the resources that we think we will need for the next couple of years. When I get into the program, that will flesh out where these are going. Basically this was prepared back in June, so we indicated at that time that we probably could have used about 12 people.

In addition to systems interaction we have oversight functions that were supposed to be performed within DSI, and the numbers in parentheses are what we estimate are being expended and what are estimated for systems interaction alone.

So basically we are talking about anywhere from 10 to 14 people as professionals, professional staff. These numbers here pertain to the program support effort, and I will be describing where those dollars go later on.

To answer your question, this number here essentially represents, Dr. Okrent, an allocation of money, support we will need to deal with systms interaction problems. We feel we may not be able to handle it in any one case, and probably control might be a good example of that. I will get into that later on.

Right now we have seven people in the branch and are in the process of trying to hire a few more.

(Slide)

These are what we feel the responsibilities are of the branch: basically, to establish the program and set up the ground rules. We plan to play a lead role in the systems interaction reviews. That does not mean we are all alone. For example, in Diablo we had assistance from the Mechanical Engineering Branch. We had some help from the Lawrence Livermore Lab.

We plan to merely lead the reviews, get participation from the other branches, mostly within NRR, to help us out. We will be getting some help from branches in systems technology because part of the evaluation that we have to make involves probabilistic methods to decide which systems interaction candidates are more important, how to rank them, how to make decisions, for example, on deciding whether fixes are necessary.

We do plan and we are currently starting to maintain systems interaction listings. Most of the listings we have now are derived from varied information sources, and we do maintain a file, an event file and a history file, to identify systems interactions that have appeared, mainly based on operating experience, adding to that list. We will be adding to that list as we develop methodologies and derive insights from that.

We expect that we will have to be adding or changing regulatory guidance downstream.

MR. KERR: Is there some general way in which you decide what a system is?

MR. STOLZ: Not particularly. We have not really precisely defined what a system is.

MR. KERR: Do you try to distinguish between interaction between two systems and interactions of components within a system, or do you refer to both of these as systems interactions?

MR. STOLZ: We really start off with deciding what events spark the whole train of events that caused a failure. We first screen whether an event is really a systems interaction by trying to decide whether it violated a failure criterion or a failure function, or a safety function, I should say. And if it has not done that, we forget about it.

If there was a safety function violated, then we look into it further and decide which systems were involved.

MR. KERR: So systems interaction, in effect, means an interaction between a safety system and a nonsafety system.

MR. STOLZ: In most cases that will be the case. A broad definition, you could have an interaction between two safety systems. There is nothing to preclude that. You could have --

MR. KERR: I was trying to get an idea of what you were looking at. Are you looking at that as well or are you looking at interaction between safety and nonsafety systems?

MR. STOLZ: Primarily safety and nonsafety because we believe that the vulnerability of the nonsafety systems will probably be the chief contributor to the likelihood of failure rather than a failure of the safety system. But we are not precluding that the two safety systems would not be involved in a broad sense. That would imply that we missed something in our reviews.

MR. KERR: Thank you.

MR. STOLZ: Okay. The type of regulatory guidance that we will be modifying -- for one thing, once we get our rules straightened out in requiring people to do systems interaction as a normal course of business as part of the licensing reviews, we will have to insert that fact into the standard format. If we change the rules or we have to modify the rules to include probabilistic -- use of probabilistic methods in the assessment, we will have to introduce that into our standard review plans. This is what we mean by regulatory guidance.

I might add that systems interaction review of Diablo Canyon would require no regulatory guidance changes. That basically was done based on deterministic methods. The applicant reviewed the plant such that no seismic event would have a damaging effect on any safety function, and he also extended that so it would maintain the single failure design of the plant as originally designed.

So, in that sense it was a rather deterministic review. We do not expect that that may be the case in other reviews. We may have probabilistic methods. We may be looking at functions instead of just a safety train, and in that sense we will have to also come back and do a probabilistic assessment to decide what the likelihood of these chains of events are and make our decisions whether or not we want to fix something based on that.

And that in turn ties into the subject you were talking about this morning as to what criteria are we going to apply to gauge our decisions in terms of probabilistic levels. I think this is also tied into that.

We also have an interface with information sources within NRC, yourself, I&E, AEOD and the Operating Evaluation Branch, Probabilistic Assessment, and Risk and Reliability Branch. All of these people we will have to get information from, these two branches, basically on assistance regarding the evaluation of identified systems interaction candidates to decide how they should be ranked in order of making corrective actions and whether corrective actions are indeed needed.

With industry we have had some brief contacts with NSAC, AIF. I come away with the idea that they are not that heavy into systems interaction, but we plan to follow what they are doing and include them in our exchange of views.

(Slide)

These are our objectives. We really feel by mid-1981 we ought to have a definition and a range of methodologies that we can use for near-term use. We really also have to develop a preliminary systems interaction candidate list to be used for testing the methodologies developed here.

In number 1 we expect that the studies I will be talking about in a moment will develop two types of methodologies, or maybe even a range of them: those that we can use right away with a little more development, and those which are suitable for only long-term use and need further development. And they may very likely be candidates for research effort.

But the problem we recognize, going back to the unresolved safety issues, is that the committee and the staff had reservations about use of fault trees that were developed as part of the Sandia approach, because it really did not reflect the details of systems interactions that were perceived by the committee or by the staff.

The only one that really reproduced was the PORV, and you had to have some special insight to see that pop out at you. So we are asking our people to essentially test the methodologies proposed by reflecting or reproducing the several systems interaction candidates that we feel do represent the type of systems interaction that we are talking about.

We are planning to also develop interim regulatory guidance, standard review plan reg guide to be used by the industry and ourselves by September of 1981. This not mean we are holding off the reviews until then, but it means that we should plan on having the guidance out in interim form at that time.

We also plan to initiate pilot light-water reactor systems reviews by mid-81. That is calendar '81. We have not made a selection yet on what those plans might be. Obviously, they ought to cover a broad range of vendors if, as we suspect, we will not be able to approach the problem using any one method and we will be relying heavily on walk-throughs and physical inspections as an adjunct to other methods.

We probably will have to pick plants that are fairly along in construction, and there are several like that. So I think we can easily pick six plants, and I will describe those in a moment.

Lastly, what do we do with all of the information that we pick up from the systems interaction reviews? We think that we can apply the Lessons Learned from this effort, and the fixes that we feel are necessary will likely apply to other plants, and these can be very easily managed by use of bulletins and information notices that were sent out to other plants to relieve the downstream systems interaction load that you might be putting on other plants.

So we think this has a bootstrapping way of operating, that you can pick pilot plants and extend your findings to other plants.

(Slide)

MR. OKRENT: I am a little skeptical that your pilot plants are likely to be fully representative or even largely representative of the other plants since geometry involving things like how something was run in the field can be important for certain of the interactions and the plants vary so much one from the other.

Certainly you could say those interactions that occurred in the first six plants, the others you should look for. But those that did not occur in the first six plants by no means might not occur even infrequently in the other population. I am a little bit wondering about your seeming optimism about the generic nature of --

MR. STOLZ: I think the way it will work is we will find a problem -- Cecil did not mention it, but, for example, one problem would be, as someone cited this morning, the nonseismic lighting in the battery room. Now, the chances are if we caught that on Diablo, that is prevalent on all the other plants, I would guess.

So that type of thing would be sent out. Now, it may be that certain plants will not have the same problem because of fuel run lines. Okay. In those cases they will report back and indicate that they have looked at your problem and give a report on where they stand.

What we are really trying to address is if we see problems that require fixes on the pilot plants, we certainly owe a review of all of the other plants to see if they have similar problems, and if they do, they ought to fix them. I don't really have a feel for what the percentage will be of similarities.

MR. EBERSOLE: I think you are saying, though, there may be problems on the nonpilot plants which are never detected on your examination of the six pilot plants. It is the other side of the coin.

MR. STOLZ: That is true, and we will pick these up on the second and third waves, which I have not talked about.

(Slide)

The basic program for fiscal year '80, '81 and '82 consists of five elements. The first one which is currently going on is the Diablo Canyon review, which resulted from a commitment that was made following a November ACRS subcommittee meeting back in 1979. It was your subcommittee, Dr. Okrent. The PG&E committed to do a systems interaction that especially considered the seismically-induced events or seismically-induced failures of nonseismic systems and what the results of these might be on plant safety.

The applicant has been working on this since last March. He has had as many as 50 people at any one time working on the job. It is a large, labor-intensive effort. What I was getting at earlier is that hopefully people downstream don't have to apply the same heavy effort if we can pick up some lessons from the Diablo Canyon effort.

But in any event, we are wrapping up our review. We hope to have the safety evaluation report covering this in your hands this month. We have a meeting scheduled with the ACRS on the 8th and 9th of October, and as I mentioned earlier, the basis of the review on Diablo Canyon was a deterministic one. That is, the applicant maintained the single failure integrity of all of his trains where redundancy was required.

Without scooping what we are going to be telling you next month, there were a considerable number of systems interactions found, not all of them that consequential. In many cases the applicant made fixes because it was easy to fix the things rather than analyze them. So you have over 600 interactions that were found, and I believe there may have been about a third of those that required plant fixes. That gives you a feeling for the detail of what can be found in one of these walk-throughs.

I think we recognized, based on the Task Action Plan A17, that limiting your look to fault-tree methods which Sandia proposed, with all the problems it created, namely, not being able to reproduce or recreate systems interactions, we felt we had to go all the way back to square one.

We enlisted three laboratories to start by preparing a state of the art review since last July. They got off a little late, so the schedule is probably maybe delayed from the one I show up here. Hopefully, we hope to get a draft report from them sometime this month.

Following that, we plan to have a peer review among the staff to go over the combined recommendations of this group. In addition to the systematic methods we will be describing based on the state of the art review, one of the major ingredients we need from them are those recommendations that we can use by, say, early '81. We would like to be able to get methodologies that we can use to develop regulatory guidance and to start kicking off light-water reactor reviews among the six pilot plants that we will be selecting.

We expect to get back with the ACRS again on this matter sometime after the peer review, probably in November, and then issue a final report. There are about a dozen methods that will be proposed or are being considered now by the labs that we understand will be reflected in their report. These are the so-called analytical methods.

They will also be looking and evaluating the Sandia work. They also will be considering more failure modes and effects analyses, among other things, so we hope to have a pretty good picture of where we are when we are through with this effort.

Indian Point III, as you know, going back in history, Zion came in, I think, two years ago and presented a systems interaction effort based on the review of LER's, and they screened something like 9,000 LER's and came up with 65 candidates or 67 candidates, and then from those they prepared about half a dozen recommendations.

I think the sense of the committee after reading the transcript is that they were not really too impressed with the results of all that effort, considering the vast number of LER's that were out there, and they suggested in this letter dated October 12, 1979, that is, the committee, ACRS, suggested that Indian Point III apply alternative ways of handling the problem, and that was a combination of failure modes and effects analysis assisted by physical walkthroughs much like we are handling on Diablo.

With this type of guidance, we are asking PASNY to kick off their Indian Points systems interaction review around the 1st of October. We met with them back last July. They were still pretty well occupied with the risk assessment work that had been laid on them along with Zion from the Commission order back in February. Mr. Denton spoke this morning of the status of that particular effort. They should be through with that by September, and they should be free to start the systems interaction review in October.

We plan with Livermore's help and people assigned from other branches to assist us in reviewing other criteria and ground rules that can be applied to the Indian Point systems interaction reivew, and that is what this means. We hope to get a final report on the criteria and methods that we feel will supplement what the Commission -- I mean, what the ACRS provided a year ago, some time around the first of the year.

By April, we should be getting the licensee's study submittal. This does not mean that we will be waiting out the submittal before we start reviewing the plan. We will be mainly concerned in this period about approaches and ground rules that will be followed during the course of the review.

Back towards the end of the fiscal year, we hope to complete the effort. We will get back with the ACRS some time in August, following issuance of an SER in July. Development of regulatory guidance based on the recommendations that we will obtain from the laboratories, we plan on directing those that hold the most promise to be the basis for our regulatory guidance and methodologies that we will be recommending for people to follow in the near term.

This effort will go from probably November through the rest of the fiscal year, with an interim Reg. Guide as an output next September, and based on our experience, a year following that we can probably put out some final regulatory guidance.

In connection with the review of the pilot light water reactor plants, we would hope to have six plants selected by various vendors. We hope to have one laboratory supporting systems interaction lead team of two persons each, so we would have two plants assigned to each team. They do not necessarily have to all start together, but we would expect that we can complete the review in a year. The review does not necessarily have to follow along the licensing path. It can be independent of that.

It appears to us the important thing you have to have on this now is a good set of drawings, a good set of schematics, schematics more than drawings, actually, and that the plant is reasonably well completed so that you can have some useful walkdowns and have a pretty good picture of what the plant looks like.

This effort will last a year, and again we will get back to you probably some time in the summer of 1982. We expect to talk to the ACRS on each one of these phases as we go along, as is noted here. The thing we have not shown is what goes on beyond 1982, and we know that there are -- there may be additional plants that we will select to do systems interaction reviews, and in addition to that, we believe that there will be generic systems interaction problems that we cannot identify what they are now, but we feel this will require a continuing effort on our part.

There will be recommendations that we will be making for real long-range systems interaction methodology which we think are apt subjects for research to take over on. That is generally what the program consists of.

I would like now to direct -- to discuss the examples that were cited in the August 12 letter and discuss those, unless there are some questions on these.

MR. OKRENT: Before we move on to the question of cascading failures. I had before the break asked you whether the licensing staff had requested a research program, a safety research program, that is, on control systems and their possible influence on safety, or however you want to phrase it, and you indicated you thought you were going to answer my question in terms of this presentation.

I must confess if the answer was there, it eluded me, unless the answer is no.

MR. STOLZ: The answer is not no. The answer is -- everything we say is on the fly. We think we will be needing help, but we have not gotten into this far enough to know exactly what type of research assistance we are going to be needing.

Basically, I think the methodologies that we will be developing will be ones of looking at initiating events like a loss of power and analyzing the systems diagrams to indicate what the -- using failure modes and effects analyses to indicate what the results might be of these impacts.

We will also be looking at the effects of these impacts on non-safety failures, and during the course of this, in order to decide whether there is -- what the impact of this in terms of need to make fixes are concerned, we will have to be doing some kind of probability assessment on deciding whether something needs to be fixed or whether the sequence that we are talking about is so remote that we don't have to worry about it.

I think there are two areas that I think we are going to need help in. One is help on the -- keep tuned in on the risk assessment approaches that are being worked on and developed under the IREP program, and the other is to get assistance possibly on further development on improved methodologies that may have been identified by the laboratories.

This can be done under our auspices or it can be done under Research's. I don't know how that is going to work out.

MR. OKRENT: Let me suggest that you really are talking about something other than the point I am trying to raise. In, I think, each of its two last reports, if not more, on safety research, the committee has recommended that the research group develop a program on operational safety or plant behavior. These are different ways of saying similar things. And they are having trouble figuring out what a research program should be, apparently, or at least they did when we met with them a few months ago.

I seem to find a problem here in your recognizing what kind of research program might benefit you.

Now, I think a little earlier there was a discussion about how do you set standards for the reliability or other aspects of controlled systems or systems that are not safety systems, and what we heard was that the staff had a feeling that you needed to have some kind of categorization of these systems, and in some way depending on their impact, but the staff did not have a good handle on what the impact was of various systems for various plants, and that was about where the situation was.

It would seem to me that unless the staff thinks it knows enough about how control systems impact on plant behavior when they function or when they malfunction, and when they malfunction alone or in pairs, or when they malfunction by themselves or together with the malfunction of some safety system or whatever, or if the malfunction is part of a multiple failure thing or so forth, this is something that I have not seen reports on in the literature.

Maybe the vendors have a body of information that they keep to themselves in this area, but I suspect the vendors themselves will have only a limited body of information, since a lot of this relates to what you call balance of plant, and there is a strong interactive effect.

If I were trying to figure out what to do with control systems and their reliability and their impact on safety and so forth, I guess I would try to have the benefit of some fairly broad studies on just what is the nature of the control systems and how do they wander and how do they fail, and what the effects are and so forth.

And I guess I would have put in a research request to the Office of Research, and if I did not know how to specify in detail, I would say, look, there is a general area. We want you to tell us what you think should be done, and if you are not sure, don't tell us five years' worth, tell us six months' worth, but I did not see that on your list, and I want to make it clear, this is not the same thing as what you call systems interactions.

It does fall under the broader category that we had on the agenda, namely, the interaction of what are nominally called non-safety systems and safety. Okay?

MR. STOLZ: Right. Let me try to clear my mind up on this point. I look at your concern as a valid one, but I would think that we would want to gain some insights in direction as to what types of interactions we are talking about between non-safety systems and safety systems, based on operating experience, insights gained from IREP or fault tree methods, and it would seem to me that if we were to ask Research to provide this service right now, it would be just an open blank check asking them to do something for us, to get us a broader look into this picture.

We hope that through use of several labs working concurrently, that they can provide further insights that we can lean on, and if it appears that some of these things fit the research effort better than the areas we are working in, we would certainly direct that area over to them.

I think the problem we see now is that we really do not have enough of a feeling as to what we would want them to do for us, and as you know, the effort on IREP has not exactly flagged all of the detailed operating occurrences that we have experienced, either.

So, we have talked to these people at great length, and we have tried to exchange our common problem, that is, what kind of a methodology can you use, what can you do to fault trees, for example, to make them flag these problems that we are seeing and nobody can see on the fault tree.

MR. EBERSOLE: Aren't you pumping a dry hole a lot of times when you deal with universities and labs because they characteristically have not done this sort of work before? They have done fundamental science, research, you know, post-LOCA investigations on heat transfer, fluid flow, et cetera, et cetera, et cetera.

MR. STOLZ: We are going to find that out. When we first talked to these people, they indicated they have had people that could talk to us who had systems experience. Their backgrounds read very well. I admit that some of the people that we talked to, their experience has been predominantly in the WASH 1400 area, which in one way is good but in another way it locks in their thinking.

You know, there, of course, they are using core melt as a criterion, and in systems interaction, we feel at least one approach is that core melt would not be a suitable event. You want to head off things that either violate the defense in depth or may be unacceptable core damage.

One of the first things we have to deal with is to find what the safety functions are that we want to use on systems interaction, and we want to get a broad expression of opinion on that.

MR. EBERSOLE: By and large, a lot of this is not what I would call nuclear phenomena problems. It is old art. Old heat transfer. Old fluid flow. The old clumping together of a variety of complicated systems to perform intended functions. I would guess there might be a lot of unused or misused talent out of NASA that I certainly would hope would be out there some place looking for a job, since there is no more NASA work.

MR. STOLZ: Rod has met with the NASA people. He can speak to that.

MR. EBERSOLE: They were forced into doing that sort of thing, much more than I believe we have been doing. That is just my opinion. I don't know.

MR. OKRENT: Well, the closest thing that comes to representing what I envision you would be doing if you were doing research on the potential impact of control systems on safety represents a marriage of systems analysis in a deterministic way with the failure modes and effects analysis and the fault tree analysis, not the one or the other by itself.

So, if you are dealing with people who are only dealing with fault tree and event tree types of things, you won't get what you need. If you are dealing with people who do only the thermal hydraulics -- the disturbance analysis comes the closest to it. I don't think it is possible to define a research program.

I think what you are talking about in systems interactions is different than what you would do for this aspect of effect of control systems on safety, and the fact that you are having trouble telling them what to do to -- so you don't need a research program --

MR. KERR: This is an interesting concept, because I had not thought until I heard this discussion that the technical assistance programs were designed to solve very well defined problems which could generally be sought on a scheduled basis, and the research programs were exploratory in nature, and were used when perhaps a problem was not very well defined.

What I seem to be hearing here is that since the problem is not very well defined, it should be handled by a technical assistance program, sort of, and only when it becomes well defined should we turn it over to research.

I may be misinterpreting what you are telling me. I don't know.

MR. STOLZ: I guess what I was asking myself is, what, even if it is a research program, what do we want to get out of it, and I -- you may have thought more about this than I have.

MR. KERR: What you sort of get out of it is more manpower at this stage, because it does require, it seems to me, some thought on the part of people -- I would not think inexperienced people could contribute much to this. Young Ph.D.'s, for example. You need some people with background in systems.

MR. OKRENT: I would have thought so, but I have a student who is just finishing a master's thesis, and he is working towards the Ph.D., that I am willing to match up against anything the average guy you can hire --

MR. KERR: You are giving me exceptions. I will not accept exceptions. If I were looking for talent for this sort of thing --

MR. OKRENT: It depends on the individual.

MR. KERR: And here it seems to me research contracts simply gives you access to some additional manpower for a short time to do the same sort of thing you could do if you had the right staff, and more time and more staff.

MR. OKRENT: Okay.

VOICE: That is the way we did it. We tried to begin thinking about tech assistance projects. In essence, I think we would do what you are describing, trying to define better than we have so far the effects of control system failure. We talked to people at NASA, and they have proposed a contract that we are now considering jointly. I am not totally sure that what they have in mind is necessarily what we want, but it may be further discussion with them, we could match our needs.

The problem with that is, they are not people that have a lot of nuclear plant experience. They have a lot of systems experience.

MR. KERR: This may even be helpful, because I would assume that at some point in this investigation, somebody would ask the question, do we really want to separate control and safety systems. Now, with all the background and tradition in the AEC and everybody else of separating them, I expect somebody ".ll decide the answer is no, but at some point somebody ought to re-ask that question.

It may be one should not make that distinction.

VOICE: I think we are re-asking that question almost daily. I think the judgment was made some time back that you could separate control and safety. I think now what we are doing is bringing them a little closer together. At least that is what some of us have in mind. What the effect of that is, I don't think we can define yet.

MR. KERR: Of course not, and I would not expect you to be able to.

VOICE: I think we are continually re-asking that question, and I think getting back to your point, Dr. Okrent, we are trying to think of ways by which we can get a better handle on the effects of control system failures. I have not personally thought about going to Research to get that done. It seems to me that was something we ought to pursue with the applicants and licensees, since it is their plant.

MR. OKRENT: No, I must say if you are going to be working in this area, I think you need to have a fairly good understanding of what goes on. I think it is impractical for you yourself to develop the calculational methodology to do this, but I have little doubt that given 10 percent of the LOCA budget -- Okay? -- Research could develop the tools and even get you calculations displayed in a way that you could see the effects of different control systems, and in effect they could come close to giving you maybe what some people call an engineering simulator, but not so fancy on the simulator part, more on the engineering.

You would not have to run in real time for your purposes.

VOICE: I was thinking of something a little simpler. We have done very little in the area of control systems thus far. It seems an awful lot can be done simply by inspection. There are a lot of things that I think can probably be discovered by just having somebody who knows something about the systems look at them in some fair detail.

MR. OKRENT: I agree. There are different approaches, and they are not -- each has good points. Just to look at the number of times that control systems have caused something is worthy of something, but that is only a part of the story.

VOICE: I agree. I accept that.

MR. OKRENT: Okay. Well, I think we had better get into the specific aspect of cascading failures. How does the staff propose to go with this?

MR. STOLZ: I thought I would go down one by one as lined up in your letter. Some of these were discussed this morning, so we may not want to spend too much time on them.

MR. OKRENT: Let's try it that way, and we'll see where it gets us.

MR. STOLZ: I think the first example cited was the seismic event that caused a shutdown of the plant, and then there was a concurrent failure of the aux feed systems in at least ten of the plants we know that have these non-seismically qualified aux feed, and then the stipulation was made that you lost then your ability to remove decay heat, and you had do revert to a bleed feed mode in the primary system to remove the decay heat.

And the stipulation was further made that after a period of time, the containment got to such a temperature and pressure that the PORV's exceeded their qualification limits and closed in the so-called failsafe mode. They shut, so now you have no way of getting rid of decay heat except by going through the safeties, and some plants, I guess, their HPI is not enough to overcome the safety settings, so you have stipulated -- here is a situation where, assuming an initiating event and a seismic event, you wind up with decay heat, and we agree that is a systems interreaction.

I think that would be a classic that we would look at. Now, the next question you probably want to be asking is -- well, we will be looking at that -- that will be the type of systems interaction review we will be looking at. Specifically what are we doing about this one example? I understand that this is being looked at. It was discussed this morning with the staff. They indicated a probabilistic analysis had been done which you are getting a copy of, and this presumes to say that you can spend a little time studying the problem before you consider shutting the plant down.

I understand that the staff is planning to conduct a meeting with the subcommittee on extreme external phenomena some time in October. Nobody is here today from the staff to discuss the merits of this review in detail.

MR. EBERSOLE: There was other discussion on that issue concerning the relative relaxed attitude toward this situation as contrasted to the flak that we had last year when we found some of the supports on seismic piping were not what they were supposed to be, and I believe Mr. Denton said that he found more comfort in just assuming that the aux feedwater piping and other systems were reliable on an upset basis than he was having found out that certain piping did not in fact have the proper supports.

I was thinking about that an hour or so later, and I thought, how can you have faith in piping which has had virtually no level of QA approaching that of seismically controlled piping with incorrect supports. This is piping erected and specified and purchased in the commercial context. How can you in fact have more faith in that quality grade and the belief that it will ride through a seismic event as against a rather well-designed seismic system that did not have good supports?

MR. STOLZ: My understanding is that this is precisely the area that we will be looking at with site inspections and things like that to determine exactly how vulnerable some of these systems are to a seismic event.

MR. KERR: I think the examples given in the letter are important, but I think what the committee was -- different individuals on the committee, I am sure, have different approaches, but one of the things I would be interested in is not how you would deal with specific examples, but how do you expect or are you going to develop an approach to dealing with this general class of events?

MR. EBERSOLE: That is the generic aspect of this.

MR. STOLZ: I think one approach that would be considered would be considering top events, which would include all of those safety functions needed to maintain defense in depth. That would be one way of going. And maintenance of your ability to remove decay heat, the ability to go to safe shutdown at subcritical, also the ability to retain your primary reactor coolant, no uncontrolled release of reactor coolant.

Another criteria would be to maintain the integrity of mitigating systems needed to preclude -- needed to mitigate an accident. So, these four are considered top events, and in analyzing these things, you could use event fault trees to site a chain of events that could occur that would lead to a violation of these, and just a mental path on this particular example. This would fit the mold.

You would have a seismic event that would cause in this case loss of decay heat, and we would definitely consider that systems interaction, and an example of one that we should be able to flag in any methodology we develop, and the problem is, now that we have found it, what are we going to do about it? And this was the subject of this morning's talk on this.

MR. KERR: I think most of these examples you would not find if you were just looking for systems that satisfied the single failure criterion, for example, so in a sense there is, to me, at least, a question of how do you know when to look for multiple failures rahter than relying on the single failure criterion? You will not find most of these things unless you look beyond the single failure kind of event, I think.

MR. STOLZ: Well, the fault tree methodology, for example, does not necessarily presume that you are following a single failure

MR. KERR: You are not using the fault tree methodology in licensing mainly, are you?

MR. STOLZ: No, we are not. It is one of the methods we will be considering in systems interaction. For example, Sandia proposed it in its report. The only problem we cited with the effort that they put out was that the construction of their trees was not sufficiently detailed or organized -- organized is a better word -- to flag these particular problems.

MR. KERR: You can have multiple failures without having systems interactions, and if these multiple failures produce serious consequences, and if the probability of multiple failure, however you get it, is sufficiently high, you are going to want to look at it, it seems to me,and in some senses what these examples, or what I would think that they are meaning to point out is that it may not be good enough at the present state of development of the art just to look at single failures, whatever caused the multiple failure, be it systems interactions or just random failures that happen to occur simultaneously, or whatever.

So, to some extent, one of the questions that I would want to see asked is, at what point or what triggers you to say, aha, better look beyond the single failure criterion here in my review process? I don't mean you are going to start using it immediately in the review process, but I assume what you are doing will eventually have an impact on the review process, and you will be able to tell people when it is they need to look beyond the single failure criterion, for example.

MR. STOLZ: I think we are just looking at the single failure criterion right now as a regulatory deterministic way of establishing an arbitrary risk level or we do not have to -- we don't propose to follow the single failure criterion in the long term. We are using it now in Diablo because it is the only tool available to us at this time. We do not have anything to use beyond that that we know works, and we will probably use that as a method on Indian Point, because we do not at this time have any other tool that we know works.

MR. EBERSOLE: Well, I am afraid we don't know that it works. We just have not experienced the consequence of it not working yet. We just got through with Brown's Ferry finding one.

MR. STOLZ: You said the single failure criterion does not preclude you from still getting into details of failures and still using both and coming up with a situation that can violate a safety train, for example, of a redundant system that is required to meet a safety function.

MR. EBERSOLE: One of my problems with the single failure criteria is something like this. It got born at Oak Ridge. Therefore, its original purpose was to provide a pulsed signal to open some contacts which were nice spring relays, which do not have pivots and things like other voltage relays, and it was relegated to the electrical world and the phenomena associated with that, and it virtually looked at no sort of physical potentials for looking at degrading of these electrical pieces of apparatus, and it lived that way a long time.

IEEE 279 went on looking at this problem as though it really began at the transducer, and that it only had to generate a pulse signal to some device, and then it was all over and done because the circuits were dead, and for many years it was not even recognized that there are many things that follow the scram.

That is when life really begins. And there are many potential influences which -- one of which was found out early on, like voltage spikes that could override what was thought to be the independence of electrical, in this case, relays, I believe, or diodes, and now there are only chips which only take a slight peak.

And the concept of the IEEE following this, I think, is a little short in that it really does not go forward to the point of generation of the signal, the validity of the signal that you have put into the transducer, its vulnerability to upsets. I don't think IEEE has ever moved forward much beyond the transducer.

You can correct me if I am wrong. I used do work on standards with ANS, and found out we had to supplement it to include criteria that said, if we are going to use a single failure criterion, we must guarantee the randomness of the failure, and not let the supposed independence of the redundant systems be breached by influence, which is either in the original incident it is supposed to mitigate or comes from other areas not recognized by the electrical people.

The classic one I can remember is where the mechanical circuits which were high pressure lines and the electrical circuits were cross-hatched, so to speak, so that the AE feedwater train would knock up a circuit for the mitigating system, and you would simultaneously kill the integral function because you crossed the specialty lines.

These are the sorts of things about the single failure criterion that bother me. Right at this time, for instance, I don't know but what there are not in the field what are thought to be single failure proof systems which may be manifold, like common hydraulic headers, which themselves are subject to failure.

MR. STOLZ: That is the example you stated.

MR. EBERSOLE: Right.

MR. OKRENT: I guess I want to be surer than I am that when you are addressing the ACRS letter of August 12, in fact, you have cascading failures in mind.

MR. STOLZ: We would include cascading failures, or failures that occurred concurrently.

MR. OKRENT: All right, yes, but I guess what I am trying to get at is, you could have a cascading failure which did not involve a systems interaction, but which nevertheless was a cascading failure, and I would possibly identify one of those as the example of the failure of a safety relief valve to close.

Let's say it stays open in a BWR, and if this led to large oscillations in the piping running down to the suppression pool large enough that you failed one or more of those lines, and this then led to a high pressure in the dry well, that is not the sort of thing you would look for in what you were describing earlier with regard to the Diablo Canyon review.

I don't think it would show, not only because it is a PWR, but because it is a different kind of event, and yet it is in essence a cascading failure, and the failures are not independent. It will only occur if you have the right resonance conditions or whatever it is that it would take, but as I say, I cannot tell that you are factoring that kind of failure mode into any of the kinds of looks that you were talking about, and so this is what I would like to explore when we talk about cascading failures.

MR. STOLZ: Yes.

MR. OKRENT: Is there a hole in what you currently have in the formulation stage so that you need to see what this missing category -- and I think the one I just cited would possibly fit into the status of falling between a crack?

MR. STOLZ: You were really also concerned that these were examples of things that were not considered in the design.

MR. EBERSOLE: And that they might not fit into these systems interaction definition.

MR. STOLZ: Maybe it is a bum excuse, but this is a good example of something that should have been considered in the design. The turbine trip, it occurs as an anticipated transient. We know the relief valves lift. I guess in some of the older plants, the tailpipes from the relief valve discharge were non-seismic.

MR. EBERSOLE: It is not a seismic question.

MR. STOLZ: Okay, and it would seem that as part of the opening of the valve, and assuming one single failure, not the assumption made by GE, it is not independente, we agree on that. As a result of one single failure, you could assume that the relief valve hung open, and then you were forced into an analysis of what the consequences of that were on the tailpipe, and if your answer came out that it was excessive, and you could break it, then you had a problem.

If the answer came out it was okay, then you did not have a problem. It seems to me that that type of issue is really framed within what we normally should be doing now. Obviously, this particular example is part of the Mark I effort, and my understanding is, this happened to be well in hand according to the staff, but I think the point you are raising is, this type of thing, would this be picked up as part of our systems interaction review?

I think we might miss it because -- as a category, we might miss this problem because this would be one we thought would have been picked up as part of our normal safety review on safety systems.

MR. EBERSOLE: The thrust of the question was put in an ACRS letter some months back, where we, in the sense of the Atlas concept of handling things, we looked at not the details of whether this might happen or not, which are pretty much argumentative, but rather, what would occur if it did happen, if we breached or bypassed the suppression pool in the course of running through a single valve discharge, and GE flat refused to analyze that case, which leads you to be suspicious that maybe they analyzed it and put the answer in the door some place.

MR. STOLZ: I think I read that letter. Their argument was based on the tailpipe condition being independent of the relief valve when in fact it isn't.

MR. EBERSOLE: They wanted to declare incredibility of the failure mode which is postulated. That is always a sticky business when one invokes it.

MR. STOLZ: The dilemma I have in helping out on this is where we are talking about safety systems that we normally review as part of a normal staff review. If we hypothesize failures in those safety systems that directly lead to a violation of the safety function, if we include that in systems interaction, then there is no limit to the definition of systems interaction.

MR. BOSNAK: Bob Bosnak of the staff, Mechanical Engineering Branch. In the particular case that you are talking about, where you are talking about the discharge line to the safety relief valve, if you bypass that and bypass the suppression pool, you would in fact go through the cascading events that you postulate in the letter.

We have gone further than we normally do on that line. It is not required to be looked for from the point of view of fatigue, but we are looking at it from the point of view of fatigue, so I guess to answer some of your questions from our normal -- from our normal review, we are catching onto these things, but it is not a systemmatic cascading event type of thing.

MR. EBERSOLE: So the general thrust, we don't have an answer for that?

MR. BOSNAK: That is right, but in this case, if you just arbitrarily postulated a failure in that line, you would lead exactly to the kind of train of events that you are talking about.

MR. EBERSOLE: It might not be the line. It might be the shell of the suppression chamber. It was decided not to run the tests, the low pressure blowdown any further.

MR. BOSNAK: But this one particular event was looked at, and it was determined that the system was not in resonance. So, for that particular mode, you did not have to worry about it.

MR. EBERSOLE: But in this case, the physical evidence of being in resonance was in fact the --

MR. BOSNAK: But --

MR. EBERSOLE: Have you maybe bypassed the need to go into all this by examining what you refused to examine, which was a consequence of just a sharp break of this pipe? You might find it would not matter much anyway, and that would be a more comfortable answer than trying to prove that it could not happen.

MR. BOSNAK: If it did happen, and if it bypassed the pressure suppression path of the normal flow, it would be a problem, and containment system people have looked at this.

MR. EBERSOLE: What did they come up with?

MR. BOSNAK: Very small amounts of bypass would be enough to overpressure the containment.

MR. EBERSOLE: Okay, sufficient is valid.

MR. BOSNAK: That is correct.

MR. EBERSOLE: That is why GE did not handle that.

MR. STOLZ: The argument -- staff agrees that breaking a line would result in greatly exceeding the pressure of the containment.

MR. EBERSOLE: That explains the nature of the GE response. They would rather not talk about it. Like an unmitigated ATWS.

MR. STOLZ: Staff is convinced, I understand, that the analysis of the line as repaired under the MARK I program is satisfactory. Under the long-term MARK I, these loads that will be induced by the event you cited will be within code allowables.

MR. EBERSOLE: Incidentally, the whole business is poor engineering to allow this section of pipe to traverse the void space, and it should just not be allowed, that sort of thing. It is a draftsman's mistake or engineer's mistake to allow that traverse. Do you follow me?

MR. STOLZ: I understand. The next example --

MR. OKRENT: Excuse me. Mr. Ray has a question.

MR. RAY: If I may shoot from the hip, it seems to me that an approach to a method of investigation of the possibilities and the consequences of cascade failures would be the approach that the system planner uses when he is conceiving and testing the adequacy of a transmission system. He will first assume the classical failure, and then he will say, well, what happens if as a result of this, or in consequence of it, or in association with it, regardless, what happens if something else, and he will pick something else, and then the failure of a generator, then a coincident failure; they may be related, they may not be related, and he will then inventory his system, and it is a painstaking effort, and it is an exhaustive analysis in the detail sense.

He will inventory his system, and thereby make up a list of conditions that he cannot tolerate, and then he will proceed to find solutions to it. It may be to add another line. It may be to reconfigure the lines he is proposing to use, and so on. It seems to me that this is the kind of approach that you are going to have to use in the electromechanical or straight mechanical, mechanical, or civil mechanical in the analysis that would be necessary to bring out the conditions of cascading that might result, and which it cannot tolerate.

MR. EBERSOLE: (Inaudible.)

MR. RAY: He will take the position that if it can happen, it will.

MR. EBERSOLE: That is the single failure criteria.

MR. RAY: No, he takes a failure, then he says, now, what happens if another thing happens?

MR. EBERSOLE: That is the single failure criterion, the way we do it here.

MR. RAY: Not mitigation. Suppose he looses a 500 kv transmission line out of Peach Bottom? There is going to be a consequence of that. That consequence he knows is a straightforward single failure analysis. He knows what the shift is going to be between the transmission out of Peach Bottom and the reflected increase in loads on other major transmission lines. That is a single failure criterion.

But he may also say -- he must also say in conjunction with the loss of the line from Peach Bottom, I might lose a generator at Eddystone Station. What are the consequences? That is not a single failure criterion. He is looking into --

MR. OKRENT: That is not unlike what is called the single failure criterion in nuclear, because, you know, say you rupture a pipe and then you lose a diesel.

MR. RAY: One other aspect, and maybe this does fit. Sometimes the transfer of a load from the loss of a major component to another system -- another transmission line is intolerable. It may be that it will exceed the capability of certain of these lines, and a relay system is set up to recognize that and would trip the line off, and he must inventory the system after the failure of the major line, the Peach Bottom 500 kv line, to see that these conditions don't exist, and if they do exist, what the consequences are, and maybe that fits the concept of what you call a cascading failure. It does very definitely represent the cascading that happens on transmission lines.

Frequently an unexpected -- an abnormal system configuration with failure to cover area consequences for reactive loads may exist on the system, and the transmission line will fail, and because of this there is a transfer to a remote location, and that remote location supply is going to cause a coincident condition of low voltage because the reactor flows very definitely do enforce a pattern of voltage on the transmission system, and he has two conditions that result.

The low voltage may look like because of the increased reactor flows that result -- it may look like an overload due to a short circuit, so those relays operate. This would aggravate the situation, and you would find other lines that are overloaded as far as the relays are concerned. Pretty soon, the whole system falls down.

MR. EBERSOLE: I want to mention something about the single failure criterion that goes back some time. In connection with the 15-page letter which was supporting this sort of transmittal, I mentioned the subject of instrument line failures there, and briefly touched upon it in the general letter.

In the course of looking at that, we of course interfaced with the vendor, and I can't help but wonder if this concept still exists. We pointed out that in certain large accident cases like the LOCA or whatever inside the containment that was going to be automatic, that certain systems would be scrubbed, and we said these cannot be validly considered as members of the failures in the single failure criterion concept. These are follow-on failures which are integral to the accident, and if we are going to use the single failure criterion, we must stick to the thesis that we are looking only at random failures in the mitigating circuits, and we cannot take those failures which are consequential of the event being cascading.

Industry took a solid position that these were legitimate first failures, that that is the single failure that we have in the single failure criteria, and subsequent to that, we went to a lot of effort to erase that idea in the ANS standard, which has never, to the best of my knowledge, been picked up -- No, it has been picked up, I think, in 279.

MR. STOLZ: That criterion is pretty clear even now. Any failures that result as a consequence of the assumed failures is counted as the single failure.

MR. EBERSOLE: In that era in case some of them slip by, the first single failure could be one derived from that mitigated as a legitimate design basis.

MR. OKRENT: Well, maybe we had better hear your comments on another one or two examples, and see if there is a pattern somehow.

MR. STOLZ: Okay. The case on instrument line failure, you indicated -- there were two concerns. One was bringing the instrument -- the transducers inside containment, but that is a separate problem. I think the fundamental problem was an assumed instrument line failure of a sensing line leading to a transducer, and there is no question that small size piping are more vulnerable, so you cannot use the argument it is less likely to happen in a big pipe.

As I understand the point, your concern is that the sensing lines are manifolded so that while you may be applying redundancy to the electrical part of the system, you do not have redundancy on the sensing lines, and I think, Rod, you indicated that you do look at these things on a spot check basis even now for that problem.

MR. EBERSOLE: Let me point out something. I heard a reference (inaudible). What I really want to know is this. I found some years ago that IEEE was not enthusiastic about proceeding with the transducers (inaudible). Is it going to be AIS that will take up the thermal hydraulics, or can we somehow get IEEE to extrapolate their requirements into an area which is basically not theirs? I don't know whatever happened.

MR. THATCHER: You are right in the sense that IEEE 279 did in a sense avoided the problem (inaudible). I guess P603 is the designation for the new -- that is the follow-on to 279, which specifically addresses the sensers and also the actuation.

MR. EBERSOLE: (Inaudible.)

MR. THATCHER: I don't think it was industry pressure. It may well have been the lack of, I don't know, overall system review.

MR. EBERSOLE: I would like to have you reopen whatever that standard now says.

MR. THATCHER: In 18.8?

MR. EBERSOLE: Yes. This is the safety system.

MR. THATCHER: Isn't 18.2 the --

MR. EBERSOLE: It is the design specification requirements for safety systems, and it -- recognizing the shortfall of IEEE, it brings it over and it interfaced with IEEE to take it on somehow.

MR. THATCHER: I think in Part 603 we will be doing that, but I think we still have the regulations from 279

MR. EBERSOLE: I guess I would like to get from you what you consider to be the ultimate (inaudible).

MR. THATCHER: I think the standard for newer plants is IEEE 384, which is endorsed by Reg. Guide 1.75, which has to do with independent systems. The requirements in that IEEE standard as endorsed by the Reg. Guide get into the areas, as I said, of instrument analysis, so that would be the one place.

MR. EBERSOLE: Is that found in one of our older plants, working plants, Brown's Ferry?

MR. THATCHER: Tomorrow we will go through some of the discharge volume of GE, and I went out to a lot of the regional meetings. Big Rock Point, that is a fairly old plant. You find on their scram discharge volume, they actually have a good physical system. There is one set of instruments for all four of the level transmitters, but the later ones, there is some separate ones, separate taps into the instrument volumes, but of course the later ones also have the Brown's Ferry configuration.

MR. EBERSOLE: Are the deficiencies recognized in the earlier ones?

MR. THATCHER: As far as the instrument line?

MR. EBERSOLE: You are talking about improvements, yes. An improvement in general can be associated with an existing deficiency. I would hate to see an improvement in a new design without some degree of reconsidering how bad it was in the old one.

MR. THATCHER: I don't think we have systematically gone back.

MR. EBERSOLE: That leaves me a little bit wondering about the status of our older plants.

MR. OKRENT: I have a feeling that it would be useful for you to look at the five short examples in the letter and tell us whether you think each of these -- if it is a relevant one in your opinion -- would be picked up in some way in some systematic process; if so, is this process, you know, suitable in a generic way, or is there -- do we need to think about -- does this say we need to think about other areas of review, or so forth?

Could you sort of run down quickly so we get a somewhat broad brush look at it? Not that the individual ones are not of interest, but we are also trying to see whether there is a general methodology question or not. Okay?

MR. STOLZ: Okay.

MR. OKRENT: Go ahead.

MR. STOLZ: Okay. On the instrument line failure, I am not too sure -- I don't know how to categorize that. It is a systems interaction problem, because that is basic. To me it is just a basic design problem. It is not clear to me that this is something that you would pick up using our methodology.

MR. EBERSOLE: (Inaudible.) It was a way to interface the control and safety systems as a case in point, and you explicitly said that you do not look at instrument line failures except in the context of a small break LOCA.

MR. STOLZ: Rod, you say you do look at these -- you were aware of this manifolding problem.

VOICE: Today, I think we would not allow (inaudible). It is true that probably a number of plants in the past have been licensed, for which situations like that exist.

MR. EBERSOLE: Let me read you an excerpt from the longer letter. A response (inaudible).

MR. STOLZ: I -- Bob?

MR. BOSNAK: That is maybe a little bit of apples and oranges there, because a pipe break criteria, that is a correct statement for the pipe break criteria, but again the pipe break criteria was written almost ten years ago, and it was written to cover the mass energy release of large lines, and I think what you are saying here is that you know, we need to look at these kinds of things differently.

MR. EBERSOLE: Right.

MR. BOSNAK: I do not disagree.

MR. STOLZ: I would have to categorize the instrument line break then as something we will have to consider in our system interaction evaluations. It is not very clear that we have a precise path to track this thing out, but we will have to worry about it. Your fourth example deals with a cascade failure resulting from a short of some heavy electrical equipment inside containment following a failure to isolate that short based on maybe some seismic event resulting in the relays not tripping out. Would you care to discuss this problem? This has been discused before, I think, with the committee.

VOICE: This area is covered by Regulatory Guide 1.63, which requires that all penetration circuits have redundant and independent overcurrent protection. That is both Class 1E and non-Class 1E circuits. Now, in the case of Class 1E circuits that go through penetrations, both of the redundant overcurrent protection devices would also be Class 1E. In the case of the non-Class 1E circuits, namely the reactor coolant pumps, they are still required to be redundant and independent, and as far as single failure is concerned, they have to meet the single failure criterion, which means that in the case of the two breakers, that is, the bus feeder breaker and the motor feeder break, coordination between these two breakers has to be such that either one of them will open a circuit before the time current limit of the penetrtation is exceeded, and both of these breakers then will have to have independent control power supplies in order to make sure that a failure of one control power supply would not disable the tripping of both breakers.

MR. EBERSOLE: That is the standard as it is currently written.

VOICE: Yes.

MR. EBERSOLE: How many old plants do we have that do not have that? Fifty?

VOICE: I do not think it is that many. I do not know.

MR. EBERSOLE: Lots of them, anyway.

VOICE: There are some, yes, I am sure.

MR. EBERSOLE: These have batteries that pilot the overcurrent devices (inaudible).

VOICE: Yes, in a non-seismic building, and they are non-seismic breakers. However, they are the same quality as seismic breakers.

MR. EBERSOLE: Right.

VOICE: A high level of reliability, and likewise, the batteries are so-called non-seismic. They are designed to a high level of reliability.

MR. EBERSOLE: (Inaudible.)

VOICE: That is true.

MR. EBERSOLE: (Inaudible.)

VOICE: That is true, but Class 1E and non-Class 1E, practically speaking, there is not too much difference in the major pieces of equipment.

MR. EBERSOLE: From a modular viewpoint, that is a different problem. You are looking at it from a modular viewpoint, and I would be the first to agree. The components are probably the same. All you did is investigate the standard design and find that they were seismically competent. (Inaudible.)

VOICE: You postulate that there is a LOCA then as a consequence of the seismic event.

MR. EBERSOLE: And I may get the containment penetration.

VOICE: If there is a LOCA, that means that the seismic event has exceeded the seismic design of the primary plant. We do not postulate that. I think that is the basis for accepting the non-Class 1E overcurrent protection.

MR. EBERSOLE: Listen. I will tell you. A couple of months ago I heard (inaudible). It says, yes, in fact, we do now concede that really if we are going to have a LOCA, really, the reason we spent all those millions of dollars in making the mitigation system seismically competent, it would have to be a rational one that we expected maybe to get a LOCA of some sort as a result of a seismic event, and the reason that we haven't come out and done that for all those years is, of course, another rational one.

I. we invoke failures of seismic systems, of seismic events, it would be the worst thing of all, and then we promote a single random failure which would leave you high and dry.

It would be best of all to promote a LOCA, because your other systems would mitigate it, but they would not mitigate a low level system failure. That is the fundamental reason why the association of LOCA's with seismic events has existed all these years.

VOICE: Well, you know, you could continue this line of thought all over the universe. At some point, if you expect to build a plant --

MR. EBERSOLE: Let's just make this point. Here is a case of a penetration vulnerability. As a matter of fact, I am not even sure that the coordination of circuit breakers (inaudible).

VOICE: Not only that. The standard tech specs include requirements for periodic checking of the coordination.

MR. EBERSOLE: The thing I would get at is, we certainly need to make the penetration strong enough to override the current (inaudible). If we could assure ourselves that irrespective of (inaudible). Do you follow me?

VOICE: I follow you, and the first issue of Reg. Guide 1.63, in effect, allowed credit for the infusing of the conductors -- of the cables before the penetration, and we revised it to not take credit for that, but to put in the double overcurrent protection.

MR. EBERSOLE: With the vulnerable DC supply?

VOICE: Yes, of course.

MR. EBERSOLE: Why didn't you put in fuses?

VOICE: Well, double overcurrent protection, if one was a fuse, we would have accepted it.

MR. EBERSOLE: If the batteries live, you are all right. If they don't, you are dead. I use that merely as an example.

MR. STOLZ: Okay. I think this is a type of systems interaction we would pick up. I think the only question here is the likelihood of this chain of events happening. It could be argued it might be small if you conclude that the seismic event is independent from the LOCA.

MR. OKRENT: I don't understand how you conclude that. Dwayne Arnold is sitting there with seven out of eight or eight out of eight large pipes having circumferential cracks, you know, halfway through the wall or more. To assume a seismic event could occur and not at least have some kind of reasonable leak seems to me to be surmised with little justification, if you have that kind of a situation.

So, even if your plant was designed to withstand the whole SSE, that is, you know, the whole plant, and similarly, I think, if you go back and look at the coolant pumps and the seals, and some other things, your chances of having something like the SSE and not getting a small LOCA or maybe something larger, I find -- I don't think those chances are too good.

The staff seems to waiver between postulating a double-ended guillotine break at the earthquake or none.

MR. EBERSOLE: (Inaudible.)

MR. OKRENT: Let's go to the --

MR. STOLZ: The last one that you cited in the letter was the presumed break in the steam line feeding the

HPCI turbine, and as I understand it, you assumed that the isolation valves at the containment were not qualified to accommodate the full discharge originating from the break.

MR. EBERSOLE: If you read the longer document, you would see there was more to it than that. It did state that those valves were never tested in full flow condition that they would experience under those circumstances. Therefore, just the mere exercise of these once every few weeks or whatever was really academic, but more importantly was the fact that if you are looking at the deterministic design and claiming you have complete independence of the function you are going to perform over the accident which you expect to mitigate, you cannot refuse to acknowledge the valve is going to be in a realm of hostility.

If you are going to postulate a break out of that valve, it can well be at the well line where the valve is fastened to the pipe, if you want to put it there. It could be elsewhere along the line. You therefore must consider that that valve is not in fact going to be totally non-influenced by (inaudible).

One can look at this in considerable detail and erect barriers and fences, and TVA, I think, has gone to some considerable trouble to try and erect some sort of qualitative barrier. How good, I cannot say. And I don't know whether Peach Bottom or any other plant (inaudible).

But I am suspicious of the valve, in addition to being suspicious of the flow interrupting capacity.

Maybe this is unlikely, but what are the consequences? The consequences are devastating, because what it does -- We were misinformed by GE a number of years ago. This is a different thing that happens than when the main steam line fails. When the main steam lines fail (inaudible) and carry away the non-safety related mitigation systems (inaudible).

Therefore, I think a hardening of the deterministic approach and not a probabilistic approach is in order. But again, it is just introduced as -- It is introduced as an integral problem which I think ought to be a conscious and particularly visible (inaudible).

Also, it is introduced as another example of the kind of event that may not come out of the system interaction study.

MR. STOLZ: Again, while it fits the broad definition, the systems interaction effort would be looking more toward external common cause failures or interconnecting systems of a non-safety variety. This is the type of thing that really is again in our present review, and the evaluation should be made along those lines.

MR. EBERSOLE: One approach would be to say, is the single failure criterion being applied here?

(Inaudible) absoultely clearly a random character, and are not influenced by the events being (inaudible).

MR. STOLZ: This is an example of where the consequences of a single failure are rather serious.

MR. EBERSOLE: They are all out of proportion to the (inaudible).

MR. STOLZ: Bob?

MR. BOSNAK: I should probably comment on this particular one also. Again, our pipe break criteria have been in existence for quite a while, and they have not changed. I think Dr. Okrent maybe indicated he hoped we were looking at the possible changes, and we are. I would hope that with the advent of the elastic plastic fracture mechanics, we could go to in certain lines and certain materials a leak before break criteria, but that would reduce the forcing functions on these lines.

In this particular case, on old plants, and I took a look before coming down here at a newer plant, LaSalle, that is in-house. This particular line is a four-inch line, the line that goes to the steam turbine, and the valve itself is a motor operated valve. Most of these valves are qualified, not necessarily by tests. There could be a prototype that was originally qualified, but they go back to -- they are analyzed by a combination of tests or tests and analysis, most of them by analysis.

The large steam and feedwater lines that you mentioned before, the valves are designed so that they will close under these dynamic conditions. In the other lines, I think probably because we have not cascaded the effects of a break into the other systems, because I think we intuitively expect that the large forcing functions that you are talking about that you would cascade into these other lines are very extremely conservative.

What we are looking for when we eventually get a more realistic pipe break criteria, that would be something that could be used in a cascading analysis, but anyway, in this particular line, the line can close under 1,140 psi. That is the pressure differential in the line, not necessarily under full flow conditions that would be caused by a guillotine break, but if you had a leak before break situation, I think there would be no question that the valve would operate.

MR. EBERSOLE: I guess what bothers me is the rather vague nature of the concept that prevents the culmination of (inaudible).

MR. BOSNAK: The pipe breaks are at finite locations. They are locations of high stress. We could argue perhaps you would have it in a different location. That might be, but you know, based on our criteria, which has some probabilistic founding, that that is the most likely location for a break, and you do protect against the external, by that I mean the jet, and any pipe whip effects, and you have to provide either limiting restraints or some way to protect the valve against the effects of the pipe itself.

MR. EBERSOLE: Were you here earlier when we were talking about (inaudible)?

MR. BOSNAK: I was not here earlier, but I know people have tried to use our super pipe criteria.

MR. EBERSOLE: Anyway, this is an example of another kind of super pipe criteria (inaudible). There is an interesting history on this thing, by the way. This line also happens to be the line which you may or may not remember was torn out of its foundations, and the hangars pulled down because of (inaudible).

MR. BOSNAK: This is the RCIC on the newer plants.

MR. EBERSOLE: Okay. That is a smaller one. Anyway, this ten-inch line happened to be the victim of an interesting cascade, if you want to call it that, in that the operators had done some work on it, and had closed the valve, which was normally open to the condensate drain, and this allowed the condensate to accumulate at a rather high level in the system.

In their zeal to assure the system was operable (inaudible).

MR. POSNAK: That is correct.

MR. EBERSOLE: So to me it is kind of a mismatch on investment to protect using deterministic arguments against probabilistic arguments or, for that matter, the more sophisticated fatigue analyses. I think it is a case where the deterministic argument ought to stand out on a legitimate basis as being the only solution.

MR. OKRENT: Let me raise the question now. Is there something we would like to hear from the staff now or at some future time? If so --

MR. EBERSOLE: I think I would like to hear a documented statement resolving these issues and specific cases.

MR. OKRENT: Specific cases.

MR. EBERSOLE: Then a statement on the generic implications. How do you find these things? Do you just go into a plant and look at event trees? You won't find them in fault trees. Where are you going to get the expertise to do this? (Inaudible.)

MR. KERR: I missed the question that Jess is asking. What is the question?

MR. OKRENT: The question was to the subcommittee what it was that we would like to hear from the staff further either today or in the future. I hoped it was going to be something for the future.

(General laughter.)

MR. EBERSOLE: I would hope we would take these cases and, you know, make a statement of resolution of them, and then include therein --

MR. KERR: What would a statement of resolution do?

MR. EBERSOLE: (Inaudible) knowledge and resolution of these matters, and then beyond that --

MR. KERR: I mean, is the matter resolution of these issues or resolution of the general problem?

MR. EBERSOLE: These are open-ended issues of a specific character which are also designed to represent generic problems to be found by whatever methodology you generate. There are two questions in the discussion here. One is, how do you propose to handle -- if you want to do anything at all -- the cases cited?

Okay, that is one thing. Then, as a general class of events which are causally related, how are you going to find the rest of these things in the system?

MR. STOLZ: I understand the question. I don't know the answer to the last one. I think we can agree on probably at least two of them, but I am not sure that we see our way clear on the other three.

MR. OKRENT: Jesse, I am going to pose something to you now to think about. As I think you are well aware, it took some years before we started to see an approach to systems interactions like the one that I think you would recommend, and that was after we wrote it out.

MR. EBERSOLE: Yes.

MR. OKRENT: Now, my question, do you think you could write out an approach that might be used to look for cascading effects? I don't want it written out now. I am asking you if you think you could write it out.

MR. EBERSOLE: I will give it a fling, but it is tough. I do not know.

MR. OKRENT: It is really not quite the same.

MR. EBERSOLE: It is easier to ask questions than to answer them.

(General laughter.)

MR. OKRENT: I think there is a methodology that can be used, at least in part, but I do not want to volunteer anything.

MR. EBERSOLE: I would be willing to turn around and look at it. What we tried to do is design a letter.

MR. STOLZ: We will agree to pursue these examples to see if we come up with any flashes of brilliance, and hope to get back with you if we do. But obviously, we are going to be meeting again on this subject several times, an' we will probably rehash these examples.

MR. EBERSOLE: Let me add one to the list. You know, Mr. Michaelson's (inaudible). Now, there has already been and there is in the record a recommendation that we look at parameter failures in a continuity sense upward as well as downward, so we will go both ways, up and down, when we look at failure modes. (Inaudible) that the progressive air failure can in fact lead to progressive leakage (inaudible) but his finding that the air system, which is not considered a safety system, has [illegible]tle influence in the progressive failure mode, but not in the absolute failure is another example of the (inaudible).

I am not so sure but what his finding may in fact not extend to the fact that you now can define progressive air failure as not really (inaudible) but also at the same time producing a rather (inaudible). You can tell me now. Maybe you already know. The valves that control the vent and drains of the scram dump volume fail open on progressive air failure.

I think the logic would be --

MR. STOLZ: We will look into that one.

MR. THATCHER: We should save this for tomorrow. The scram vent drain valves, they fail open.

MR. EBERSOLE: They fail open.

MR. THATCHER: Then that is a continuous leakage path.

MR. EBERSOLE: (Inaudible.)

MR. THATCHER: There are definitely some problems.

MR. OKRENT: Well, let's see.

MR. KERR: If they fail open, they don't prevent scram, do they?

MR. EBERSOLE: If they fail open, they will probably open when they have the failure. No, I am sorry, they close when you --

MR. KERR: When they scram, they close.

MR. EBERSOLE: (Inaudible.)

MR. KERR: That does not make things worse, does it?

MR. EBERSOLE: It does not make drains any worse or any better in a scram context, but it makes things worse from the retention of boron point of view.

MR. KERR: If you get a scram, you don't need the boron.

MR. EBERSOLE: (Inaudible.) Now, water proceeds to continue to leak out through the --

MR. KERR: How did you lose the scram? Apparently you are doing the best you can.

MR. EBERSOLE: Yes.

MR. THATCHER: I think Michaelson is, too, but if you are going after the containment isolation pump, that is another problem.

MR. EBERSOLE: I am talking here in the context of the (inaudible).

MR. OKRENT: For some reason even though it is only 3:15 in Los Angeles it feels like it is 3:15 in the morning instead of 3:15 in the afternoon. I am going to suggest we adjourn in a moment.

With regard to the last topic we have talked about during the past two hours, it appears that it would be useful if some time in the future we could hear more on how you think you should address this controlled reliability aspect. We would be interested in hearing more. We would be interested in hearing whether you developed a generic approach to cascading failures, and Mr. Ebersole has said he is going to try to think on it, and he has asked if you are able to respond to one or all of these specific examples, and that you do so.

MR. RAY: Mr. Chairman, do I understand that we will get periodic reviews of your progress on this systems interaction?

MR. STOLZ: Yes, sir. Each of the five programs that we listed on the wall, we will have scheduled meetings with the subcommittee and the full committee.

MR. EBERSOLE: Do you (inaudible). I take it you anticipate putting in fixes as the need evolves in the course of your investigation.

MR. STOLZ: Well, if it follows the pattern of Diablo Canyon, which it probably will, they do the fixes as they go.

MR. EBERSOLE: Under your pressure or voluntarily?

MR. STOLZ: Voluntarily.

MR. OKRENT: As I say, I am not sure they are generic. Thank you.

I think unless somebody breaks my heart and asks a question, the meeting is over.

(Whereupon, at 6:19 p.m., the meeting was adjourned.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

ACRS/ SUBCOMMITTEE ON SAFETY PHILOSPHY, TECHNOLOGY AND CRITERIA

in the matter of:

Date of Proceeding: September 3, 1980

Docket Number:

Place of Proceeding: Washington, D. C.

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

David S. Par er

Official Reporter (Typed)

[signature]

(SIGNATURE OF REPORTER)

PROPOSED AGENDA

SEPTEMBER 3, 1980 MEETING WITH ACRS SUBCOMMITTEE ON SAFETY PHILOSOPHY, TECHNOLOGY AND CRITERIA ON CASCADING FAILURES IN NUCLEAR PLANTS

- Introduction
  - Cascading failures as part of Systems Interaction
  - Agenda

- Systems Interaction (SI) Status
  - Action Plan II.C.3
  - NRC Organization and Resources

- Systems Interaction Branch Responsbilities

- SI Objectives

- SI Program (FY '80 - FY '82)

- Discussion of SI Examples
  - ACRS Letter to NRR dtd 8/12/80, "Cascading Failures in Nuclear Plants"
  - Crystal River 3 and I&E Bulletin 79-27 Power Supply to ICS
  - Browns Ferry 3 - Scram Discharge Failure

SYSTEMS INTERACTION STATUS

- ACTION PLAN II.C.3 GUIDANCE

- REORGANIZATION OF NRR

  - SYSTEMS INTERACTION BRANCH (SIB)

SYSTEMS INTERACTION BRANCH - RESOURCE REQUIREMENTS

Decision Unit	FY '80		FY '81		FY '82	
	SY	PS	SY	PS	SY	PS
. Operating Reactors	0.3		1.0		1.0	
. Casework	2.2 (1.0)	20K	6.4 (3.0)	150K	9.4 (6.8)	320K
. Safety Technology	(1.5)	240K	(7.0)	482K	(3.4)	200K
TOTALS	4.0 (2.5) *	*260K	14.4 (10.0)	632K	13.8 (10.2)	520K

Current Professional Staff Level : 7

*June Estimate 1/3 FY

( ) Staff resources PMY needed for Systems Interaction

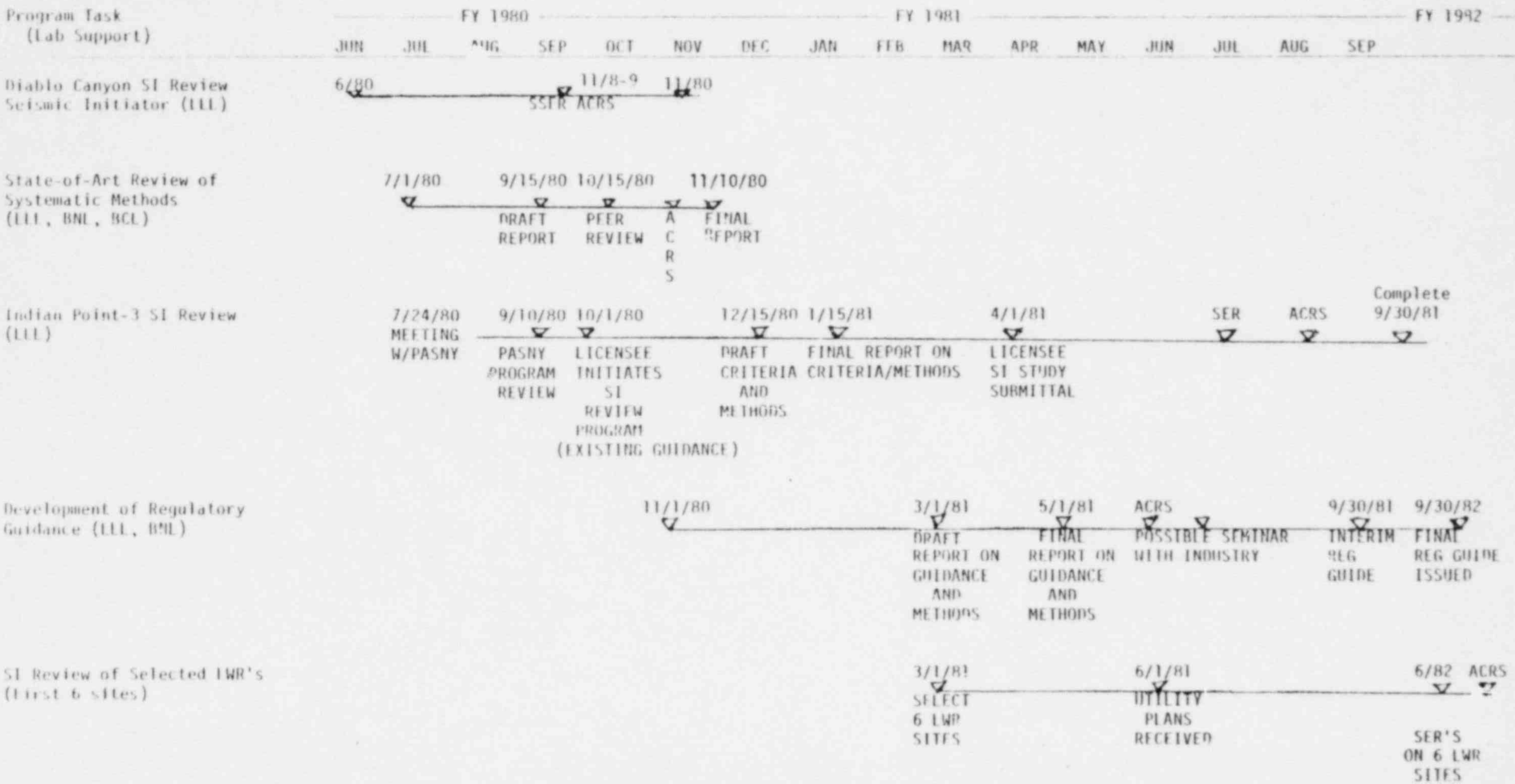
SYSTEMS INTERACTION BRANCH RESPONSIBILITIES

- ESTABLISH PROGRAMS & ESTABLISH GROUNDRULES
- LEAD ROLE IN SI REVIEWS SUPPORTED BY OTHER NRC ORGANIZATIONAL UNITS & LAB CONTRACTORS
- SI CANDIDATE LISTING AND FOLLOWUP ON DISPOSITION OF THESE CANDIDATES
- PROPOSE ADDITIONS OR CHANGES TO REGULATORY GUIDANCE
- INTERFACE WITH INFORMATION SOURCES WITHIN NRC (ACRS, I&E, AEOD, OEEB/NRR, RRAB/NRR & PAS/RES) AND INDUSTRY (NSAC, AIF, VENDORS/UTILITIES)

SYSTEMS INTERACTION OBJECTIVES

- SI Definition and Range of Methodologies Suitable for Near-Term Use (Mid - '81)
- Development of preliminary SI Candidate Inventory to be Used for Testing Methodologies Proposed
- Development of Interim Regulatory Guidance (SRP Reg Guide) for Use by NRC/Industry on SI Reviews (Sept '81)
- Initiate Pilot LWR SI Reviews (Mid - '81)
- Apply Lessons Learned to Final Regulatory Guidance and Follow-on SI Reviews (Sept '82)

SYSTEMS INTERACTION PROGRAM



APPROACH TO EVALUATING THE EFFECTS OF THE FAILURE OF NON-SAFETY SYSTEMS ON PLANT SAFETY FUNCTIONS

- Approaching from the standpoint of systems interactions

- View as one aspect of systems interactions

APPROACHES BEING USED AND DEVELOPED
TO EVALUATE SYSTEMS INTERACTIONS

- PLANT OPERATING EXPERIENCE

- WALKDOWN METHOD

- QUASI-ANALYTICAL METHODS