NUCLEAR REGULATORY COMMISSION

COMMISSION MEETING

In the Matter of: COMMISSION DISCUSSION WITH ELECTRIC POWER RESEARCH INSTITUTE REPRESENTATIVES ON FISSION PRODUCT BEHAVIOR

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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COMMISION DISCUSSION
WITH ELECTRIC POWER RESEARCH INSTITUTE
REPRESENTATIVES ON FISSION PRODUCT BEHAVIOR

- - -

Room 1130
1717 H Street, N.W.
Washington, D.C.
Monday, October 26, 1981

The meeting was convened, pursuant to notice, at 2:02 p.m.

BEFORE:

NUNZIO J. PALLADINO, Chairman
VICTOR GILINSKY, Commissioner
PETER A. BRADFORD, Commissioner
JOHN F. AHEARNE, Commissioner
THOMAS M. ROBERTS, Commissioner

STAFF PRESENT:

LEONARD BICKWIT, General Counsel
FORREST REMICK, Chief, Office of Policy Evaluations
R. BERNERO
S. TRUBATCH
R. BLOND
R. MINOGUE
W. PASEDAG

ALSO PRESENT:

F. CULLER, President, EPRI
I. WALL, EPRI
R. VOGEL, EPRI

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P R O C E E D I N G S

CHAIRMAN PALLADINO: The meeting will please come to order. The meeting today will consist of a briefing by and discussion with EPRI representatives on fission product behavior. As I indicated at a Commission meeting last week, this topic is one in which the Commission has great interest. The types and amounts of radioactivity that might be dispersed in various nuclear accident scenarios has significant impact on NRC regulations to protect the public health and safety.

Fission product behavior is of particular importance in the siting of nuclear power plants and on emergency preparedness needed to cope with various possible accidents. We therefore look forward to learning as much as possible on this topic from our discussion with EPRI representatives this afternoon.

At this time I thought I would turn the meeting over to Mr. Floyd Culler, President of the Electric Power Research Institute.

MR. CULLER: Thank you, Mr. Chairman. We appreciate the opportunity of discussing this important subject again. I think it has been almost a year since we appeared here, on November 18 of last year, and I wish to comment quite favorably on the quick action and studies that the NRC has made on this subject since, the overall quality

of the reports that have been produced.

We also appreciate the opportunity of working with your staff in trying to unravel the complexities of the situation as we see it. The reason for being persistent in our view of the source term and the conservatism that may be built into it is that we think that it has a profound influence in general on attitudes toward nuclear power, particularly if one accepts the idea that a reduction of a factor of ten or so is justified by data and by analytical approaches, one finds that all the near-term deaths and others are almost erased by this reduction.

More importantly today, our emphasis is focused on the question of reactor siting. We are finding that the source term indirectly and directly is in part responsible for the definition of the perimeters for consideration, the areas of exclusion and evacuation, and as time goes and the possibility of evacuating very large populations either in practice or for real.

Our concern now is that the possible risk on evacuation is much higher than the very probable low doses for radiation potential that might occur from a reactor accident. We are finding, not yet firm, but various states may be expanding the area. We have heard rumors that it is proposed in California that a 50-miles zone be included as an evacuation potential. As a consequence, we welcome and

are grateful for the opportunity of persisting in this important exercise.

Today, rather than concentrating on the issues of siting and those that surround degraded core and other licensing, with your permission we would like to talk only on the issues or primarily on the issues that affect the siting question. Although the discussions have common impact, our intent today is to discuss the source term and actions that we will suggest at the end of our discussions that could be taken to partially mitigate our concerns with the siting issue.

The overall plan for today is to have Dr. Ian Wall, on my left, to make the presentation of the arguments that we have collected, basically justifying our views that the source term as now presented is conservative, that the proposed changes in the 0071, NUREG-0071, are still conservative. We have collected here with us Dr. Richard Vogel, who has prepared the comments on 0071 and 0072, along with Dr. Wall and Dr. John Taylor, which we mailed to you on October 1.

Since March we have been exchanging with staff, as you know, views of data and such on the iodine and source term issue. The letter which covers the presentation for today I prepared in part on the airplane, and on rereading my good friend Ian Wall and Dick Vogel say that I used the

wrong legal term in calling it "licensing." So the transmittal letter will be corrected and sent to you, rather than being distributed right now, because of the probable -- certain confusion in terms. I was writing without thinking.

Last of all, Dr. John Taylor, who is in charge of our nuclear division, would have been here for this discussion, but this happened to be the day that we are pulling together a group to think about the next stage of a large breeder demonstration and we had 30 people in Palo Alto. So John is staying to entertain that group, to see what measures may be there.

Without any further discussion, I would like to turn the meeting over to Ian Wall, and we will finish in, oh, probably 20 minutes or so, and I will sum up and we will be available then for questions and discussion.

Thank you.

MR. WALL: Thank you very much, Floyd. You did say 30. I will try to be very brief.

Could I have the first slide, please, Jack.

(Slide.)

Last November Mr. Levinson and I briefed you on our perceptions on the source term. In particular, Mr. Levinson reviewed past accidents and large-scale experiments which suggested indirect evidence of a smaller source term.

I went through and identified conservatisms in the WASH-1400 work which was completed in 1975, and which shows that a tenfold reduction in the source term stated in WASH-1400 had a major significance.

What I would like to do today is to update that briefing. We will take two slides from the last time as a continuous -- and then move along.

In talking to Bob Bernero just beforehand, he said make sure you define your terms. And so I want to make clear that when I speak about source term I am talking about the magnitude of the release to the atmosphere stated in WASH-1400. I am not talking about TID-14, 84. I am not talking about Reg Guides 1.3, 1.4, or all the release solely from the fuel, local release from the fuel.

This is the table in WASH-1400 which is the release to the atmosphere. It was used in the CRACK program for the consequence calculations.

What we would like to do is to initially focus upon the R&D needs and then move on to review with you the comments that Mr. Taylor forwarded on NUREG-0071.

(Slide.)

Last November we identified in some detail the conservatisms in WASH-1400. And I really would prefer, unless you wish, to not dwell upon these in detail today. They are listed in slightly revised form on the lefthand

side of the slide and we have indicated by the presence of a dot the fact that these conservatisms would apply to each of the major -- each of the risk dominant accident sequences defined in WASH-1400.

I have not included it to keep the slide simple, but they would also apply to the risk dominant accident sequences found in the subsequent risk assessment, such as Zion and Indian Point, Limerick and others. And I think the point is of this slide, is that these conservatisms apply almost universally to each accident sequence.

With about one or two exceptions, a change in any one of these would affect each of the dominant accident sequences, and thus it would change our perception of accident risk from the accidents. So if we have a factor of two or three reduction in two or three of these areas, we believe that a factor of ten reduction in the atmospheric source term is certainly a very reasonable and probably conservative estimate of the reduction which should be achieved.

(Slide.)

CHAIRMAN PALLADINO: Could you identify the settings that say TMLB, S 2 C?

MR. WALL: These acronyms come from WASH-1400. TMLB' is basically a station blackout routine. There are some transients followed by loss of auxiliary feedwater

system and the main power train and a failure to recover electricity within, I think it was, three hours.

The S_2C is a small LOCA followed by failure of -- I cannot remember if it is the safety injection or the recirculation part of the ECCS, or was it the sprays. It was a very major system.

TW is a transient sequence followed by failure or a loss of the RHR system, that is the residual heat removal system, in the PWR.

And TC is the ATWS accident sequence.

Mr. Levine might find my definitions not as accurate as he might like, but they suffice.

(Slide.)

The next slide merely illustrates the impact on our perception of risk from a factor of ten reduction. The main curve there illustrated is taken straight from WASH-1400, namely we show a 10^{-9} probability of about 3300 early deaths, and about 10^{-6} or so probability of one.

If you reduce the iodine and particulates by a factor of ten, under the same assumptions that underlie the main curve one calculates zero early fatalities, and we believe this is a highly significant conclusion or impact, which would influence one's thinking about the safety of reactors and one's perception of regulatory needs.

(Slide.)

The following slide was also shown last November and is merely a statement showing a probable change in the other consequences of these calculations, the early injuries, the latent cancer fatalities, and the area of interdiction. And you see that the early and latent cancer fatalities are only reduced by a factor of five, much smaller, since it is not a threshold phenomenon, whereas early injuries are an equally large reduction because it is another threshold phenomenon.

CHAIRMAN PALLADINO: Is that in our package?

MR. WALL: I guess it is not. That slide was traveling last week. I could not get it until Saturday. So I would be happy to forward you a copy.

CHAIRMAN PALLADINO: Thank you.

(Slide.)

MR. WALL: The question one really has to face -- and here I am getting to the new material from last November -- is in this very large and complex issue, which subjects should one put one's R&D resources and what I -- what we have endeavored to show in this slide is, we listed the same conservatisms on the lefthand side that you have seen earlier and that we outlined in detail last November. We then formed a judgment as to what potential reduction factor might be achievable in a near-term time frame.

And we show there factors from two to ten, to 10

to 100, and quite clearly as a first gauge you want to put your R&D resources in an area where there is a large reduction potential, rather than an area which is rather marginal. So as a first step we put it in a subject which as a 10 to 100 reduction potential, rather than the 1.5 to 2 subject.

The righthand column, we have endeavored to make a game judgment on the viability of such a reduction to the technical community within a two-year time frame. And again, we have tried to show a high, medium and low probability.

Now, if you review that chart you will see that there are basically three subject areas which have a potentially significant reduction factor and also have a medium to high probability of success, and these are the potential retention of fission products in the primary coolant system -- that is number two on the list -- the trapping of fission products in water pools, number three; and the aerosol modeling in containment coupled with the longer confinement period in the containment.

And what I would like to do now --

COMMISSIONER AHEARNE: Why does not number one meet the same criteria?

MR. WALL: Number one is really a subset of number two, as you will see as I develop the theme. I think it is

a much more difficult one to achieve.

COMMISSIONER AHEARNE: I was just going by your criteria here.

MR. WALL: Your point is well taken, sir. We really have to -- I agree with you, it is equally important. It really is needed to support number two, and the simplicity -- I wanted to focus on the four and put that as a subset, as I have shown in this next slide.

(Slide.)

So the four primary areas we see are the aerosol reduction -- transport in primary coolant system, of which the primary source characterization is an important element, and the thermal hydraulic modeling of the primary coolant system.

Now, in the next few slides I have tried to lay out the state of the art in these particular areas, and I would like to just note that, while we are aware of the substantial program within NRC and the Karlsruhe in Germany, within a 20-minute time frame I really cannot do justice, and so I may have overlooked somebody's favorite program. My apologies.

Part of my selection also reflects a two-year time horizon. But I would just like to address these four areas in a little more detail.

(Slide.)

WASH-1400 assumed zero retention of fission products in the primary coolant system. The reason for this assumption was the -- their a priori perception that the large LOCA was the dominant accident sequence. A posteriori, we found that the transients and small LOCA's were the dominant risk initiators, which would have a much longer transport power and hence a potentially much larger retention in the primary system.

The NRC has included in the final version of NUREG-0772 a calculation with the Battelle Columbus' quick curve which suggests a 90 to 99 percent retention in the primary system if you have an adequate residence time, about 1,000 seconds, and a fairly high concentration initially in the primary system.

I believe Mr. Silberg and other people talked to you about that curve last Wednesday. Unless there are any questions, I would like to move on.

(Laughter.)

COMMISSIONER AHEARNE: I thought you were going to show us.

(Laughter.)

MR. WALL: We are quite prepared to in the question and answer period.

We have done a very similar calculation using the HAA-4A code.

CHAIRMAN PALLADINO: That is a code we have had a lot of trouble with. We did have trouble understanding that curve. We may want to come back and ask you to explain it for us.

COMMISSIONER AHEARNE: That curve is traveling.

(Laughter.)

MR. WALL: We have done the same calculation. We have come out with much the same answer. So at least there are computer calculations which suggest a factor of ten, and these were calculations which were made for dry atmosphere, but they are computer codes.

What we need is an experimental validation. Currently there is substantial work in process, and I would rather not dwell too long. I would just like to draw attention to a couple of items here: that the NRC has a substantial program at Oak Ridge measuring fission product release and aerosols from fuel which -- irradiated fuel which has been heated to high temperature.

EPRI has just about initiated a similar program at Argonne. I think the reason for this apparent duplication is our focus is going to be on measuring release rates, whereas the Oak Ridge program will not have equipment in place to do that before -- they told us the first measurement, late '83. That will probably slip to '84, and they will report it, certainly I do not think before '84.

(Slide.)

What are the further needs in this subject area? We have put together a preliminary proposal for a large-scale aerosol experiment at the Marviken facility in Sweden. The Marviken facility is -- was to have been a boiling water reactor. The problems were foreseen and it has been used as a large-scale test facility, you know, for the last seven or eight years. And NRC and EPRI are currently jointly funding a project there.

We have made a preliminary proposal to utilize that facility to mock up a primary coolant system, inject a large source comparable to the source which might be covered -- might be generated from a molten core, and measuring the retention of fission products in that system.

Mr. Minogue has received a copy of this proposal and he is having his experts review it, and I hope that in the early new year we will get together a technical team to iron out the bugs and see whether it is a proposal worth pursuing and one which both NRC and EPRI can fund with an assurance of producing good results in an adequate time frame.

In addition to that --

CHAIRMAN PALLADINO: What sort of costs are anticipated for that?

MR. WALL: They propose initially, in this

preliminary go-round, I think about 31 million Swedish kronas, about $7 million. And we would hope to have a seven-party split on it, so it is in the range of one to $2 million per party.

The proposal has been circulated quite widely in Europe and Japan and Canada, and it is our hope that the Marviken people, who have participation from these countries today, will be able to continue that on for this experiment.

In addition to that, we perceive some need for some other calculational and experimental results, and in particular we would like to see if NRC could, to accelerate their Oak Ridge work to measure release rates at an earlier time period than 1984.

(Slide.)

The next subject area is the water scrubbing of fission products, and here we have -- perhaps for Mr. Roberts, who is not familiar with reactors, a boiling water reactor has a very large pool of water, and in the event of an accident the aerosols and steam and hydrogen pass down into the water, bubbles through the water into the containment atmosphere.

WASH-1400 assumed a factor of 100 if the pool is subcooled below saturation temperatures, but unity, i.e., no retention, for steam-saturated conditions. Now, those

judgments were formed on the basis of elemental iodine being released. If cesium iodide were released, undoubtedly it is going to be more water soluble, and undoubtedly the decontamination factor, i.e. the retention, will be much greater. And the judgments we anticipated were made on the basis of a literature survey by General Electric Company.

We have an RFP out at this time to initiate some experiments to pin down what the -- what the nature -- what the real experimental facts are. I hope to release that contract to do that work on a medium scale next year and on a larger scale in 1983.

(Slide.)

The aerosol transport in containment is in better shape in our opinion than the other subject areas. WASH-1400 used a relatively simplistic code, but as you compare it to the more recent experiments at NSPP it does not seem to represent them very well. So we do not foresee a major gain there.

On the other hand, it would be desirable to have a code which was founded upon physical models rather than one which is a semi-empirical fit to some existing data, and so we have outlined some comments on the NRC projects. And really, I think they are on the right tracks and we would like to work with them and do that.

In particular, we recommend the experiments down

at Sandia on corium concrete tests might consider the incorporation of some fission product simulants there and certainly to emphasize the aerosol measurements there rather than just penetration of the molten fuel into the concrete.

Apart from that, we think this is an area which is going pretty well.

(Slide.)

The final slide is the duration of containment integrity. WASH-1400 predicted an early failure of containment.

CHAIRMAN PALLADINO: Did they predict that or just assume it?

MR. WALL: They predicted it, sir. And it was based upon an overpressurization of containment due to hydrogen burn or overpressurization from steam or carbon dioxide. There have been numerous comments on, I will call it, the idealism of that calculation and both NRC and ourselves have ongoing programs to improve the modeling of that process, which would undoubtedly lead to a later overpressurization, that is the MARCH code.

Recent evidence --

CHAIRMAN PALLADINO: When they predicted early failure, in what time frame was "early"?

MR. WALL: "Early" was about one -- it varied from accident sequence to accident sequence.

CHAIRMAN PALLADINO: Yes.

MR. WALL: For the ones that you count, you are talking about one to two to three hours after core melting. the later Zion PRA, which has just been submitted to your Commission -- to your agency -- predicts a much delayed failure, namely I think 12 or more hours. And this is highly significant because if you have more time there, the aerosols in the containment will have more time to plate out, to fall into the sump water, there is more time for the operators to take recovery actions, there is more time for the population to evacuate, and this all leads to a major risk reduction, which I will show in the next slide.

But I would also like to note that we have completed the first phase of our hydrogen experiments at EPRI and we found that with pre-energized igniters, at least in ice condenser containments, there is no -- the pressure spike will not overpressurize those containments.

CHAIRMAN PALLADINO: Could you clear up for me what you mean by "pre-energized igniters," what form they are and --

MR. WALL: They are similar to those which TVA has introduced into Sequoyah. Now, TVA I believe used diesel engine glow plugs, and these are a more advanced kind of glow plugs which have been developed under our program, and we have done an exhaustive series of experiments with these

glow plugs ignited -- that is not the way -- warmed up and in the presence of a steam environment that one would anticipate, we have injected a steam-hydrogen into the jet -- into the chamber, simulating a small LOCA.

And you find you get a series of small burns and you do not get a large detonation under these conditions.

CHAIRMAN PALLADINO: Are these pre-energized at the time of an accident or are they always there, energized?

MR. WALL: I believe, I am not sure in this, I believe TVA would energize them at the time of an accident and not all the time.

COMMISSIONER AHEARNE: But your experiments --

MR. WALL: Our experiments --

COMMISSIONER AHEARNE: -- pre-energized?

MR. WALL: Pre-energized, yes, sir. And I am sure this data will be brought forth -- brought to you in January as part of the TVA submittal for Sequoyah.

CHAIRMAN PALLADINO: You say they would not be overpressurized, and that depends on their design pressure.

MR. WALL: Yes.

CHAIRMAN PALLADINO: Is this constrained in any way? What assumptions did you have about the ice condensers?

MR. WALL: I believe the assumption there is an

engineering analysis of the containment capacity, the structural capacity of the containment. And the pressure spike which we measured here under these conditions was about 30 psi.

Now, if you add additional steam and/or water sprays or fogs, the pressure spike will be reduced to about 10 psi.

CHAIRMAN PALLADINO: To about 10?

MR. WALL: To about 10. So that even without the water sprays or fogs, we are looking at about 30. And it is my understanding that that is less than TVA's estimates of the structural capacity of their ice condenser containment, but I could be corrected on that.

CHAIRMAN PALLADINO: By "structural capacity" you mean just below the point at which it would break?

MR. WALL: Yes.

CHAIRMAN PALLADINO: It is not a design basis?

MR. WALL: Yes, I believe the criterion the structural engineers use is one percent yield in the rebar. Whether that is what TVA used for the Sequoyah analysis I do not know. But other assessments have used that as a criterion for failure.

CHAIRMAN PALLADINO: This is something that maybe I -- I certainly would be interested in and I am sure the other Commissioners would be interested in understanding a

little better.

MR. WALL: I would certainly be prepared to come and brief the Commission on our hydrogen program. I think TVA, Duke Power and American Electric Power have plans to do that.

The significance of this delay in containment failure is illustrated in this slide here.

(Slide.)

Again, I have the dotted curve as the same curve from WASH-1400 and the solid curve is from the Zion risk assessment. Now, I would have to make clear that there are many things going on in this slide.

CHAIRMAN PALLADINO: Could you just sort of -- what is the ordinate? Is that --

MR. WALL: The ordinate is the frequency of excedance or probability per reactor year.

COMMISSIONER AHEARNE: It is up at the top?

MR. WALL: Up at the top left-hand corner, Mr. Chairman.

CHAIRMAN PALLADINO: Okay.

MR. WALL: Now, Zion has three times the population of the average site in WASH-1400. I believe it also has compensating additional hardware in its containment. So there are many things going on in that comparison and I cannot say precisely that decrement in risk

stated in that slide is solely due to the longer containment confinement period. But it is illustrative of the significance of the issue.

(Slide.)

CHAIRMAN PALLADINO: Zion I gather did not proceed -- well, how -- did they predict delayed containment failure?

MR. WALL: Yes, sir.

CHAIRMAN PALLADINO: How did they do that?

MR. WALL: They have a much more comprehensive -- they have done a much more refined analysis of containment. They have a containment entry which is very well laid out and they have looked at the timing and phasing of the different phenomenon. Instead of assuming that you get a suitable position of the steam and carbon dioxide and hydrogen burn all at once, they have looked at the timing and how fast the heat come come from a debris bed to the water and produce steam, and they have laid this out very carefully.

And I think it is a very substantive analysis. And they have also -- but they are also -- they have utilized some new experimental data on debris bed cooling. But there is also some subjective judgment on the dispersal of debris in the event of such an accident.

And so I am -- we are recommending that this is

significant, this work, that an early review by the NRC staff would be warranted. And I would like to see that the soft spots in the analysis be brought out in the six to nine-month time period rather than a year or two from now.

COMMISSIONER AHEARNE: You mean early review of the Zion study?

MR. WALL: Containment analysis within the Zion PRA, yes, sir.

Work in progress I would rather not dwell on. It is self-explanatory.

(Slide.)

Well, what is the time frame one is looking at in doing this kind of work? We have attempted to lay out this time frame, at least for our EPRI projects. Mr. Culler, when he looked at it and with his long experience, said you are rather optimistic, and I think he is probably right.

But I think what you see here is that the first useable data upon which one might be able to substantiate a smaller source term would be available in the late '83, early '84 time period. The real substantive issue is how quickly we can get this Marviken proposal or an alternative proposal like it, get something of substance out of that, and that is the one which is at the far right.

Now even after you have some usable data you still need -- if you want to make good cheese, it has to age a little bit before one has it, and the same applies to research information. So -- but we perceive the time frame is in that '84 area for research.

Well, gentlemen, my thirty minutes is up. I can stop at this point or I can run through my comments we submitted to you in Mr. Taylor's letter on NUREG-0771. We have some comments, if you are interested, on NUREG-0772 and we can also have Dr. Maravich enlighten you on that confusing curve in NUREG-0772.

I am at your pleasure as to --

COMMISSIONER AHEARNE: I have one question, if I could.

CHAIRMAN PALLADINO: Sure.

COMMISSIONER AHEARNE: If I could go back to that resolvable technical issue, I now have risen to all the issues you presented and I still seem to -- I did not quite --

MR. WALL: Jack, go back to slide four, please.

COMMISSIONER AHEARNE: I could not quite latch on to where you are addressing or see addressed essentially the chemical form of the fission product and release from the fuel. What is getting out into the reactor coolant system?

MR. WALL: Next one, Jack.

We are addressing or what we perceive is that in order to do this, an experiment such as this, large-scale here, you have to characterize that primary source term.

COMMISSIONER AHEARNE: Yes.

MR. WALL: While the work that is ongoing today in this area is not going on at Oak Ridge National Laboratory, there is work going on at Karlsruhe and we are just initiating some work at Argonne. So that is where I am getting at that chemical form of the fission product, what is the chemical species, what does it involve.

COMMISSIONER AHEARNE: I was not sure if I should get the right impression from the presentation that you believe either that is less important than the others or that it is much better in hand.

MR. WALL: I think there are projects -- there are more projects in hand. I think it is a very difficult subject and it is appropriate that there are more projects in hand. In trying to get a coherent presentation I downplayed it underneath the -- it is related to how much we retain in the primary system.

COMMISSIONER AHEARNE: Well, certainly. But as the term you use, it is the primary source term.

MR. WALL: Primary source, yes.

COMMISSIONER AHEARNE: And clearly you have to get a good understanding of that.

MR. WALL: Yes, sir.

COMMISSIONER AHEARNE: We had just -- in fact, that was one of the original points that was clear that we did not have a good handle on it.

MR. WALL: Yes.

MR. CULLER: But it is equally important to know as soon as it is released.

COMMISSIONER AHEARNE: Of course, of course.

MR. CULLER: We sort of bundled it in. It has been combined there, possibly combined the most important piece of information.

MR. WALL: Let me add, Mr. Commissioner, that I have some concerns about large research programs looking -- trying to identify and pin down in every detail that primary source term.

I perceive that as the sandbox in which I and my colleagues have been playing for about twenty years without much gain. So what we are trying to do, the strategy, perhaps, to try and -- I do not want to say leapfrog it and ignore it, because I do not mean that, but try and perhaps span it as to what potential there is and identify the retention on the transport part, rather than get bogged down in what could be a rather long-term, very -- I do not want to say fruitless experiments, but, rather, very complicated experiments.

So our strategy was to try and focus on the transport question and do as much as we have to on the primary source rather than the very detailed and complicated program on the primary source.

MR. CULLER: Joe, would you like us to continue?

CHAIRMAN PALLADINO: I would, yes, because I am especially interested in knowing where the differences -- I will not call them opinions, but a difference of views at the present time between what EPRI says and what the staff has been doing.

MR. CULLER: We summed up our comments in our letter to you of October 1 and we will make this part of the packet.

MR. WALL: No problem. Could you move to slide 15, please, Jack?

(Slide.)

In Mr. Taylor's letter of October 1 -- next one.

(Slide.)

We supported the use of technically realistic accident scenarios and we certainly endorsed the use of safety factors, the traditional approach to engineering -- the traditional engineering approach to uncertainty, and I would like to leave that.

We felt that NUREG-0771 overstated the dominant source terms and, to a large measure, that was due to the

unfortunate timing of the final -- of the draft 771 and the final 772. And we trust that now that the final 772 is out that some of the comments that we have suggested will be incorporated in the final 771.

In particular, the 771 had a very interesting table, 3.5 -- I ought to back up, I guess. The 771 postulated a range of siting source terms -- one through five. I believe they were called accident spectrum groups one through five.

Group one is the highest and really it assumes about a seventy percent release of the nastiest fission products to the atmosphere. Group five is the smallest and it is equivalent to the TID 14,844 source term.

Table 3.5 endeavored to examine the sensitivity of the consequences to changes in the group one source term, and that table is on page 20 of 771. And what they did was reduce the iodine, seisium, tellurium, individually or in combination, by factors of two to one hundred, and the conclusion was reached on the basis of that table, that significant variations in that source term had relatively minor impact on consequences.

We believe that was a misleading conclusion due to the incompleteness of Table 3.5. And with the cooperation of your staff we extended the table to look at the reduction in the Group One source term by a factor of ten and about

one hundred to all species except the noble gases.

(Slide.)

And you see in that table that you get a very significant variation, namely the early fatalities dropped to about one, if you reduce it by a factor of ten, and to essentially zero from a factor of one hundred. And you get lesser changes in the other categories of consequence.

So we would conclude from this table that the variation within the source term is highly significant and has a high degree of impact upon the consequences.

(Slide.)

MR. CULLER: And, if I may, we believe there is reason to hope for a significant reduction in the iodine source term and probably in tellurium and this is why we fundamentally persist in saying that a greater reduction in source term is possible.

Well, it is a significant statement at this point. Excuse the interruption.

MR. WALL: Okay. So on the basis of that statement we would recommend to the Staff that in their final version of NUREG-771 that they consider using the one-tenth Group One source term as the design, with an upward and downward sensitivity from there. And on that basis I redid Group table 3-4 for NUREG-771 here and there you see the original Group One with a factor of one hundred

for a base line and then the revised Group One, which is the smaller line, the second, for reduction.

I did not change Group Two through Group Five, simply just -- it would get too confusing.

(Slide.)

And I have not done a renormalization which presumably the staff would do in their more complete assessment. And we then also changed Table 3.5.

(Slide.)

And there we would suggest that the group -- baseline Group One might be the top line there and that is the one-tenth value from the existing NUREG-0771.

And then we suggested that a variation on that group could be factor -- up to a factor of ten higher -- it is what I have shown in the upper half of the table -- and up to a factor of ten lower, which is what I have shown in the lower half.

And then I think you get a better picture, first of all, of a more realistic baseline, namely the revised Group One, and also a better picture of what the uncertainty in that source term would do to you in terms of the public consequences.

So that is what we have suggested in that letter.

(Slide.)

As I move on, Table 4.2 in NUREG-0772 -- 771 was

-- had large ramifications. The text of 0771 correctly noted that many of the engineered safety features have effectiveness beyond the design basis accident for which they were designed -- for which they were designed. But we feel there are other conclusions to be drawn and, if I move on to just redisplay the Table 4.2, I would note a few things.

(Slide.)

First, the public health risk is dominated overwhelmingly by Groups One and Two and Groups Three, Four and Five, although they contribute to the public health risk, they have a very overwhelming impact upon utilities' financial investment.

And so we suggested that -- I had better back up. Now, where the agency focus, we suggested in the letter, should be on Group Two, since many of the accident sequences which dominate Group One are actually in Group Two with an additional failure.

So an accident sequence would be in Group Two. And then if you say, well, if the sprays failed, the containment sprays failed, it would be in Group One. So, therefore, what you come up with, if you look at the accident sequences in Group Two you see the containment sprays are a very important engineered safety feature and that is really where your focus should be.

As I prepared -- and, on the other hand, accident sequences and equipment which has only impact on Group Five seemed to us to be a potential candidate for some deregulation. Utilities are certainly well motivated to keep that equipment in line without the NRC staff taking their limited resources and applying it to that. And we tried to illustrate that here.

Now, as I prepared for this briefing and rethought about it, I did recognize that my dividing line between Group Two and Group Three perhaps should move to the left one and put the focus in Group Three in Two and limit the utility financial risk to Groups Four and Five. I just wanted to reproduce what we have done in the letter here and not make the oral comments right there.

Let's get some expansion on this. If you look at the Group Two here, also Group Three, you see that containment spray, suppression pool and ice condenser are highly significant to public risk, even though they receive perhaps marginal credit, some of them in some cases, in the current safety reviews.

On the other hand, the recirculation filters, auxiliary building filters -- words fail me. The SGTS steam generators and the main steam isolation valve leakage control have really low effect on this where public risk is felt and these might be candidates that you might wish to

consider for a -- some limited -- limit your utilization, your manpower.

CHAIRMAN PALLADINO: Can I ask you a question? You have here public health risk for suppression pool high. I thought the suppression pool helped bring it low. Is that --

MR. WALL: No. The table is intended to indicate that the suppression pool is highly effective in reducing public health risk in the Group Two accident sequences.

CHAIRMAN PALLADINO: Okay.

MR. WALL: And I think -- and I may be corrected by some of the Staff -- the difference between Group Two and Group One, where it goes down to medium is a factor -- it's functionability in Group One is assumed to be less, and that is the movement there.

So we are suggesting that --

MR. BERNERO: Excuse me. My name is Bernero. The difference is the containments fail and it moots it.

CHAIRMAN PALLADINO: In which?

MR. BERNERO: The difference between Group One and Group Two is some of the engineered safety features worked in Group Two. None of them work in Group One. The containment has failed directly. It is the worst kind of -- it bypasses ESS.

CHAIRMAN PALLADINO: Okay.

MR. WALL: So we are suggesting that basically, I think, this is a very important table and a fine effort on the part of the NRC Staff to help focus regulatory effort, and we would like to encourage that this be extended and work done a little further and to be utilized to help focus the regulatory effort.

(Slide.)

The -- I think this is my last slide. We, EPRI, we are an R&D organization and we do not have the resources to track the ramifications of a reduced source term through all the numerous regulations, but I do not need to tell you that the TID 14,844 source term evades the current regulatory structure, and if one changes the source term or one's perception about it, there are manifold changes.

We have not had the resources or the extra teeth, really, to comment on that. However, we would like to comment on -- reopen the consideration of the ten and fifty-mile emergency planning zones.

I was privileged to serve on the NRC-EPA task force which recommended those emergency planning zones and there was quite some argument as to whether it should be ten or fifty miles or whether it should be five and 25. It came out ten and fifty and we suggested if indeed the smaller source term is substantiated that there is more than adequate grounds for reconsideration, that the ten and fifty

mile emergency planning zones to a factor of two or more smaller.

Dr. Catlin, my associate, Dr. Catlin has pointed out the pervasive influence of these emergency planning zones, which were predicated upon the source term in WASH-1400 to other government agencies and to the states. It disappeared through various sources over at the Bureau of Rad Health and it is really very pervasive, and he will be very pleased to enlighten you on that subject.

Well, we have a summary slide for you. Would you like to pick up?

MR. CULLER: I would like to take over now and close, because I think a few comments on what we see in the future are desirable.

First off, it is not our position that we are working at odds with the NRC on this issue. We think that for reasons given here, primarily that there is additional knowledge to support the idea that the source term can be reduced, both in iodine and in other fission product sequences through a multitude of scenarios and failure that need to be pursued in research and development reasonably soon, that this work should be accelerated.

We are reasonably confident that there is a factor of ten available, rather than perhaps a factor of five, as suggested by other careful analysis done by your staff. We

do not necessarily agree, as you would expect, with all of the analytical approaches and such because they are imperfect in their present form.

Secondly, the other reason for pursuing technically is we think there is additional credit to be given to some of the engineered safeguards in the sequences, either in delay, the potential for release of fission products, or actually suppressing, providing additional containment that has not been factored into the analyses so far.

As a consequence, primarily because of the inpending possibility of having rulemaking hearings on siting, we suggest that because a smaller source term is likely and in our judgment perhaps by a factor of ten, that we need to accelerate the experimental work, both sponsored by NRC, by DOE, by EPRI and others, to collect information on this broad and important subject.

We recognize,as you do, the importance of this, of course, in the emergency planning sequence and as a consequence suggest that the hearings might be delayed until we have more data to sustain the argument that the source term could be reduced.

CHAIRMAN PALLADINO: Which area?

MR. CULLER: The hearings on reactor siting, not the degraded core.

As I said at the beginning, we are unsure as to how to comment on the degraded core. Obviously the iodine source term issue is one of the four or five most important considerations in the degraded core exploration, and we recognize, too, that in addition to the factual information that the discussion of a reduced source terms brings to the basis for licensing, we are introducing possibly a revision to the basic licensing process by considering scenarios.

The industry, as well as EPRI, is concerned about changing the basis of licensing to scenario considerations and think that although we are reasonably sure that risk analysis under the design basis accident approach to licensing has to be considered, we suggest that if we proceed rapidly with the accumulation of data on the source term and the ability of containments to confine the debris from degraded cores, that we will undoubtedly serve the shorter-term issue on evacuation zones, on the siting hearing, and at the same time eliminate some of the most important issues in the degraded core area.

. So today our suggestions, if you will hit the next slide, are basically that the NRC projects, that these be accelerated, that we suggest to you that the one case in which the engineered safeguards have been worked in some detail be docketed early for consideration, that there are opportunities in several programs for both EPRI and NRC to

accelerate specific areas of work, the Marviken experiment being one, the water-scrubbing project and others, to be included.

CHAIRMAN PALLADINO: Do you have a list of the selected NRC projects which should be accelerated?

MR. CULLER: We can provide it.

MR. WALL: We can provide it.

MR. CULLER: Yes, we do have, but it is not --

CHAIRMAN PALLADINO: In what time frame should the acceleration be?

MR. CULLER: Basically we would like to imagine that it would be possible to have data reasonably in hand by the end of 1983 and the beginning of 1984 on all of these experiments and are attempting ourselves to put in place those things that we consider to be important to meet that schedule.

As I said, or as Ian said, in looking at the schedule and what has to be done, I am not absolutely sure that we can, but that is our goal.

As a consequence now, in summing up, basically we truly appreciate the great amount of attention that the Staff and NRC has given to this important issue and find ourselves in both agreement and disagreement with 72 and 71. However, we think that the preliminary evaluation that these two reports present underestimated the potential that is

available for decrease of the source term and, because of its importance, as illustrated by Ian's one-tenth and one-one hundredth, what the potential for easing overall safety requirements of a reduction in source term has, we suggest that we concentrate during the next year in proving out codes and collecting experimental evidence, possibly in expanding the sample collection at TMI-2, where we have the transport already laid out for us, and collect much more information on the distribution of radioactive material in that system.

CHAIRMAN PALLADINO: Floyd, recognizing that we still have data to get, why do you -- aside from differences of opinion, why do you say that the Staff is underestimating the reductions in material that --

MR. CULLER: We think that the data and the codes used in combination do not accurately reflect the available reduction.

CHAIRMAN PALLADINO: Is not part of our problem, then, part of our R&D work to -- should not be also to improve the codes?

MR. CULLER: Of course. I am sorry. I went over that too quickly. I said collect the data and improve the methods of calculation and estimation.

CHAIRMAN PALLADINO: Okay, so --

MR. CULLER: Then to prove out the emerging codes

with data.

CHAIRMAN PALLADINO: Do you use codes different from those that the Staff uses, more improved ones?

MR. CULLER: We use codes that the Staff is familiar with but does not use.

CHAIRMAN PALLADINO: Is that the source of the difference?

MR. CULLER: In part. In part, we use different input data.

CHAIRMAN PALLADINO: Now this is -- is this documented or confirmed input data or is it input data that you just sort of have a feel for?

MR. CULLER: More the latter, more the latter.

CHAIRMAN PALLADINO: What we are facing, Floyd, is we are getting a number of comments and we get letters saying this is a very important subject, and we agree.

MR. CULLER: I understand.

CHAIRMAN PALLADINO: And they say NRC is underestimating the benefits that we can get from this preliminary data. And I am at a loss as to know what to do except I do see that it is important to get the data more quickly, to improving the codes and --

MR. CULLER: To giving more credit to the engineered safeguards, perhaps.

CHAIRMAN PALLADINO: Very well. Okay. But what

shall we be doing in the interim aside from pushing for better codes, more research data and the like?

MR. VOGEL: One particular area would be the behavior of the aerosols in the primary coolant in the primary system.

CHAIRMAN PALLADINO: Is that something where there are no gaps or the gaps -- gaps in information?

MR. CULLER: There are gaps in the assumptions.

MR. WALL: High significance in that area.

MR. VOGEL: No credit was taken for this, as we understand it.

MR. CULLER: We think that --

CHAIRMAN PALLADINO: How should we use whatever data we have now? You say, well, the latest siting hearing and I am personally inclined to do that, because I think the source term is an important point in developing a siting rule, but what other things, in your view, should we --

MR. CULLER: Well, I think Ian's summary of the major areas, the business of reviewing the primary source term, of transport in the primary system and credits for containment in the primary -- investigate that thoroughly. Then leaks in primary to the secondary and trying to take credit for the retention that is inherent in the primary and our views of it and the information we use is slightly different than the Staff has used because of the paucity of

data in the containment vessel, then developing the containment source term and credits for residence time, the movement of aerosols within the containment vessel itself and plate out needs to be considered and pushed in a hurry so that we have better actual data on which to base our estimates of what the release potential might be.

CHAIRMAN PALLADINO: Do you think at the present time, aside from delaying the siting rule should we be changing our emergency preparedness considerations?

MR. CULLER: I personally do because I think we are moving too far in the radius for evacuation. I sincerely believe that there is a reduction potential of a factor of eight to ten easily possible in the source term and that that indirectly will reduce the zone of evacuation.

COMMISSIONER AHEARNE: But do we have -- as Joe asked, do we have the data in hand?

MR. CULLER: No. That is the problem.

What we had was a wide spread.

COMMISSIONER AHEARNE: Yes.

MR. CULLER: Of the upper and lower limits of very imprecise data.

COMMISSIONER AHEARNE: Yes.

MR. CULLER: Since the consequences to the society are so great, the possibility of evacuating very large numbers of people for possibly relatively small accidents

with fifteen-minute signals, we say delay that a little bit and improve the quality of data in this area of uncertainty.

CHAIRMAN PALLADINO: This is what I was getting at, though, Floyd. I am enthusiastic about this whole approach.

MR. CULLER: Yes.

CHAIRMAN PALLADINO: I am even optimistic that it is going to perhaps bring factors such as you are talking about. But right at the moment -- and I am trying to clarify in my own mind -- well, what can we do at the moment? We can delay the siting perhaps. We can -- I do not think we have enough to do anything about emergency preparedness except intuition or feel.

MR. CULLER: Sure.

CHAIRMAN PALLADINO: Is there another area in which we should --

MR. CULLER: What we have suggested, possibly not too clearly, basically accelerate the work at Oak Ridge. We are going to stick some at Argonne on the primary source term and we are going to try to get both rate and total quantity of evolution, look at what is retained in the primary circuit and possibly go as rapidly as we can with the Marviken experiment.

While we were discussing this presentation it occurred to us that we really have not paid enough detailed

attention to what data we may be able to smear out and sample out of TMI-2 and I am concerned a little bit now that we have not planned it carefully enough because we are beginning to talk about spraying it down.

COMMISSIONER AHEARNE: The Governor of Pennsylvania and I share a belief that perhaps we could accelerate getting the data if various organizations were willing to put in some funds.

(Laughter.)

MR. CULLER: Well, we have, quite honestly, EPRI has had $20 million available since the accident occurred as -- and the Department of Energy has had nominally $75 million available. The problem is that nothing moves.

COMMISSIONER AHEARNE: Well, you can perhaps make some available for some other things.

MR. CULLER: As far as EPRI is concerned, we cannot, by the nature of our appropriations, without the utilities doing something about it.

COMMISSIONER AHEARNE: Yes, exactly.

MR. CULLER: As you know, the utilities have said we will pay $190 million toward the cleanup if somebody else does.

Now our part of that action is to follow the research and development. Now I think that it is important that we think about what data may be resident there, how

much of the uranium oxide moved through the circuits and how we are going to sample with a little bit more care than we have.

And one of my purposes in coming today is to offer, again, most certainly, under the informal memorandum of understanding that we have, to work with your Staff in planning an expansion of the experiments, whether or not NRC participates or not. We are going to spend money in this area, not only in the areas we have discussed today but others that go to the degraded core. We do work very closely on it.

Right now we are suggesting accelerating those portions of this activity that will give us data on what has evolved and contained in the primary system, what factors we can take as the materials move through various routes into the secondary, and what allowances we can make in the secondary term and to do that as rapidly as possible, because of the importance both in the degraded core hearing -- it is item number one -- and in the near-term because of scheduling on siting, to obtain a little bit better information on that to prevent what may be an unfortunate set of regulations from emerging that greatly -- beginning tomorrow or at any time you wish we are willing to discuss and to attempt to outline with you and with DOE what we think might be useful.

Now your guys know as much as this as we do or more and it seems to us that if we get together and try to figure out how to accelerate with sort of a singlemindedness this time, work out the codes to be a little bit better, collect data that makes some significant, with the intent of getting a justifiably lower source term and I mean justifiably, not to knock it down foolishly or otherwise. We think it is there.

And because of its importance we would like to proceed as rapidly as possible toward accumulating methods of evaluation and experimental evidence to justify it. And we are willing to work on it. We have got $9 or $10 million or so allocated this year, another seven or eight next year, and I am willing to increase that to start work.

You have money available and what we need to do is to make sure that we get the experiments going in reasonably conceived, overall manner.

CHAIRMAN PALLADINO: Floyd, you are going to provide us with the --

MR. CULLER: We will provide you with a thorough list and, with your permission, we will work part of it with your Staff, if they do not object.

CHAIRMAN PALLADINO: Very much so. Yes, I would like that.

MR. CULLER: It is easier for us to do it that way

than to do it sequentially.

CHAIRMAN PALLADINO: And even indicating if you have choices where you would put your money -- priority, I guess, that is.

MR. WALL: Mr. Chairman, I would like to come back to the question you had earlier.

If I can have slide 4, Jack.

(Slide.)

You are asking what can be done now and I think what I tried to indicate was there is evidence, recent evidence, in several areas of some factor. Now in part that is based upon paper studies and could be open to suspicion and the calculation in NUREG-0772, which suggests that a factor of ten is one of those which we really feel a need for some experimental validation.

And maybe it is not a factor of ten. Maybe it is a factor of five or a factor of fifty, but there is some factor there, I think there would be general consensus. We are quite convinced it is going to be a factor of two to ten in the water-scrubbing and we will try and demonstrate that within the next twelve months or so by some experiments.

I showed you the results of the Zion probabilistic risk assessment which shows there may be a factor of 10 if you examine the containment confinement time in terms of the risk. Now, there are three factors there which our summary of each of them is in the range of 2 to 10. I cannot believe that every one of them is going to disappear. That is why I feel very confident that there is a factor of 10 now here today.

So there are grounds to take an optimistic view and a pessimistic view, and we believe it is very a pessimistic view to say there is no factor; to be highly optimistic and say it is a factor of 100. Our judgment is a factor of 10 is a reasonable basis to proceed in consideration of what should be done and how the flavor of regulations might be looked at over the coming years.

CHAIRMAN PALLADINO: We have great difficulty when we try to change regulations based on inadequate data.

MR. WALL: I fully understand that, sir.

MR. CULLER: We are not suggesting that, Joe. Basically, what we are saying is that if we look at that list just and say all right, some of those, three or four of them, are going to have a factor bigger than two with the information, if you combine that, the probability of getting somewhere around 10 is pretty high. And because it means so much in the two principal areas now challenging nuclear

power in siting and the degraded core, we think we ought to get along with the collection of information as rapidly as we can.

CHAIRMAN PALLADINO: No argument about getting the data as quickly as we can.

MR. CULLER: And we think it would be imprudent perhaps to go ahead with the rulemaking hearings on siting in view of the fact that that stuff is probably up there if we get down and work it carefully; the analytical methods, the models and their true thing with data.

Now, as I said, I am not in a good position to argue the case for degraded core now because of the uncertainties that we all have about challenging the basis of licensing. The same information is useful, however, as we proceed to the degraded core.

I think we ought to do what we can with this subject now. We will steadily maintain our confidence in a higher factor than you have reported in 0771. I do not believe your staff is frozen in on any set of numbers anymore than we are. We are sure that there is a broad range of uncertainty there. Our confidence is that we can get 10 or more out of it, and if we do that, then we have changed the view of many of the concerns about nuclear power and its siting; nuclear reactors and their siting.

COMMISSIONER GILINSKY: You are primarily

interested in the siting impact?

MR. CULLER: I am primarily interested in the degraded core as well.

MR. WALL: Siting and emergency planning.

MR. CULLER: And emergency planning, yes.

COMMISSIONER GILINSKY: I would think the emergency planning is really the most important item.

MR. CULLER: I'm sorry, I was using a figure to include emergency planning. The siting is an important issue but my concern is the emergency planning only right now, as far as a public issue is concerned.

CHAIRMAN PALLADINO: Yes, emergency planning I think would be enhanced greatly if we could show these kind of reductions.

MR. CULLER: That is it. And in fact --

CHAIRMAN PALLADINO: I feel much better having people turn on a radio and it says close your windows and stay indoors for the next two hours, rather than have them run out and get into an automobile --

MR. CULLER: And get killed.

CHAIRMAN PALLADINO: And get right in the plume perhaps.

MR. CULLER: All right, I think we have made our argument, unless you have some other things to add. I most certainly offer a continued set of cooperation, and we will

work with the staff if you think that that is desirable. Ian, do you have anything else?

MR. WALL: No. Are there any questions?

CHAIRMAN PALLADINO: I had a detailed question going back to one of the slides where you talked about pre-energized igniters. Ice condenser containments would not be damaged by nitrogen burn. You said it could stand 30 psi spike with 1% plastic deformation.

MR. WALL: Let me clarify that for you, Mr. Chairman.

CHAIRMAN PALLADINO: I am getting to a philosophical question. As you recall, I was educated in the mid to late thirties when you put safety factors on things and you could design right up to either yield or going to the ultimate strength. And now we seem to be sort of doing that, and I appreciate that airplanes would not fly if you had to put the kind of safety factors that I was educated to put in.

Nevertheless, I am a little -- I am interested in whether this is right the philosophy to take when you say you are going to withstand or be sure that you can handle a pressure spike during a hydrogen burn. Do you have any idea in that direction? I am struggling with trying to accept it, and I have not -- I am not saying I have not accepted it. I just have not yet satisfied my own mind what the

right thing is that we should do.

MR. WALL: If I might clarify what I tried to convey, and apparently I did not do an adequate job. We have done measurements of the pressure spike ensuing from a burning of hydrogen in a degraded core environment, one which has a humid atmosphere.

If we inject hydrogen at a rate which we would anticipate from a small LOCA into that humid environment, the two would have, due to all the heat that was being released and we have a pre-energized igniter operating, the peak pressure that we measured was about 30 psi. If we then have either steam coming with the hydrogen jet, or if we have sprays or fogging present, the peak pressure spike is about 10 psi.

Now, I am taught by my subordinates that the TVA, the ice condenser people, are very pleased with these results since the 30 psi would not cause failure of their containments. I am not aware of what that basis they make that statement.

I referred to the Zion and other studies where they have tried to look at when containments would fail on a realistic basis, and they have used a 1% for a risk assessment purpose; not for design purposes, but for risk assessment purposes they used a 1% yield criterion. Now, that was for risk assessment purposes and not for design.

For design purposes I am quite sure that they Bechtel's of this world utilize the ways that you and both I were taught in our engineering training; namely, to use yield and a factor of 2 safety.

So have I clarified that?

CHAIRMAN PALLADINO: Yes.

MR. CULLER: It is an area in which we really had not prepared. It was more or less -- you will be hearing more about it, Joe, because I think you have presentations coming up.

CHAIRMAN PALLADINO: I wonder if the staff would like to comment on any of the points put forward by EPRI either at this time or later, or raise questions?

MR. MINOGUE: I will comment in general terms, Mr. Chairman. I think there has been in the last year a remarkable emergence, partly because we have been trying to deal with some of the specific issues and understanding phenomenology better. It is clear that there has been, during this interim phase when there is still a lot of uncertainty, more caution on the part of the staff in applying some of these trends than perhaps the gentlemen from EPRI would use. But I certainly have heard much more agreement in this discussion than disagreement.

A key point that has been brought up that I share their concern about, if you look at the issues that really

need to be resolved to really make use of some of this stuff, look at the problem of scheduling the work and prioritizing the work. It is clear that we have to do a better job to think in terms of accelerating the parts of it that have a lot of significance to the fundamental safety issues and rulemaking issues that we have been talking about here, rather than trying to understand all aspects of all phenomenology.

We are working on precisely that kind of assessment right now. We would welcome the input from the people from EPRI. It would be a different perspective, because we are trying to do exactly the same thing; to really shake out of this large number of programs the elements that most contribute to resolving some of these issues in a form that you can use in rulemaking in the regulatory process.

But I really do not have much -- maybe Mr. Bernero might like to add. I have been encouraged by this discussion. I have been encouraged by the very good atmosphere with which we have had a number of program planning meetings with EPRI in various specific areas. I think that as we try to deal with the technical substance underlying the regulatory issues, there has been a lot of progress made in the last year.

CHAIRMAN PALLADINO: Can I ask you one question?

I guess 0772 was issued, but 0771 is out for comment. We have a revised version, am I correct?

MR. MINOGUE: 0771 is more of a licensing document.

CHAIRMAN PALLADINO: yes.

MR. MINOGUE: There was a feeling that there was more of a need to provide outside input into that, and a little longer schedule. I think that was a tougher job in some sense.

There was a comment made earlier about the pervasiveness all through the regulatory process; a lot of things are done that rely on these source terms and rely on these accident sequences, and I think in a very real sense, the 0771 job is harder.

CHAIRMAN PALLADINO: But do I understand correctly after the comments are received then you will be issuing it?

MR. MINOGUE: It is an NRR report, Mr. Chairman, I do not believe there are any plans to reissue it.

MR. PASEDAG: Mr. Chairman, our original intent was to provide a forum for discussion of these issues by issuing that report for comment, and we had intended to collect the comments after we received them and look them over and sort them and so forth, then use them as basic input to the rulemaking processes for the degraded core cooling, siting and so forth. There was not any specific plan to reissue the report, although we can do that if that

is desirable.

CHAIRMAN PALLADINO: It would depend on whether significant changes that you would think would be worth documenting and collecting in a single report.

MR. PASEDAG: Yes, let me make one comment that I think bears on this. And that is, as it was pointed out, the source terms that are quoted in the NUREG-0771 were not the ones that correspond to the final version of NUREG-0772, and that therefore, there would be a need to update 0771.

Again, that was not our intent. Our intent with NUREG-0771 was not to quote a new design basis source term or anything of that nature. We were simply quoting from early drafts of NUREG-0772, but it was our intent that NUREG-0772, which is the technical document, that that would be the place where the source terms would be reported, and those were the proper source terms, the best that we understand them today. Not in NUREG-0771. We simply are quoting examples of how one might prove various source terms from different sequences, and that has been the basis of this five-group arrangement that we have discussed today.

MR. CULLER: But 0771 would stand as a correctable but not to be corrected document. Its purpose was not to define the source term but to define the complexity in the regulatory process.

MR. PASEDAG: Right, and to propose a procedure on

how to deal with it.

(Laughter.)

CHAIRMAN PALLADINO: Well, I think whether or not any other version of this should be published I think is something --

MR. CULLER: It was not apparent that that was the purpose when we read it.

MR. BERNERO: I wonder if I could raise just two comments. One is frequently, we use the term "early containment failure" as Ian did in his presentation, and I sense from some of the questions that there is some misinterpretation of what is the crucial issue there.

It is not crucial that the ultimate strength or the yield strength of the containment, the actual assessment of the failure strength of the containment is not so much at issue as is the calculation of what loadings are put on the containment.

The heart of the matter is describing the phenomenon of core melt and the generation and superimposition of pressure loads. And that is where the real problem, the real uncertainty, is. So it is not out there in the hardware, whether you use -- you know, depending on the type of containment -- 1% yield or the rebar or whatever you use.

Most people have found -- for instance, I think

Ian said 30 psi for the ice condenser. In one of our risk assessments on an ice condenser we used 31 psi. I think Sequoyah comes up with 42 or something like that. That is not the crucial point.

CHAIRMAN PALLADINO: I was not connecting 30 psi with containment failure, but I did have a question of when you speak of containment failure, what assumption, what do you mean by that? What does it imply with regard to release of fission products? You just assume it is all gone?

MR. BERNERO: A large --

MR. WALL: The normal assumption?

CHAIRMAN PALLADINO: No leaking out --

MR. WALL: One of the assumptions in most risk assessments I am aware of, there is presumed to be a catastrophic failure of containment. Now, depending upon the time elapsed for that catastrophic failure, there will be some plating out of the radionuclides within containment. Upon the catastrophic release there wils be a puff release to the atmosphere of the airborne radionuclides.

MR. CULLER: The importance of the iodine, presumably it stays dissolved in the water, most of it, either in the primary or hopefully --

MR. BERNERO: As Ian Wall said in his presentation, there is not so much controversy about the modeling of the transported plate out in the atmosphere of

the containment or in the atmosphere outside. The real controversy and the real issues are inside the reactor coolant system, what accident sequences, how do they progress, what that fuel actually saw, what moved out of it, what got out of the reactor coolant system and how it varies with accident sequence, not as WASH-1400 models it as essentially the same on all accident sequences. That is the fundamental issue.

MR. CULLER: That is the R&D.

MR. BERNERO: Yes. You know, we frequently speak of, we have to work on the MARCH code; that is what we are talking about, this highly subjective input-dependent process of modeling that. That is what the MARCH code does.

MR. CULLER: The data are really critical, in effect; if you have not got any data, you cannot add up all the source terms.

CHAIRMAN PALLADINO: Incidentally, with regard to the ultimate capability of containments, I believe it was the Elk River containment where they tried to dispose of it by blowing it up. I was wondering whether we can get any interesting information from those attempts. I do not know how they --

MR. BERNERO: Again, Mr. Chairman, that is not the crucial issue.

CHAIRMAN PALLADINO: I understand, but

nevertheless, on one part it would be nice to know.

MR. BERNERO: That was not done by a wrecking ball.

CHAIRMAN PALLADINO: That is eventually what had to be done.

MR. BERNERO: From the outside.

CHAIRMAN PALLADINO: That is because they did not get anywhere with explosives, and maybe it was an economic question for all I know. If there is anything to be gained there, it might be interesting.

MR. BERNERO: The other issue I think that is worth commenting on is the area of emergency planning. I stand to be corrected if they see fit. There are at least two people in this room, Roger Blond and Ian Wall, who worked on the NUREG-0396, the joint EPA-NRC task force that worked before the TMI accident on a basis, a logical basis, for emergency planning. That document was published in December of 1978.

It was based in principal part on the reactor safety study risk assessment source term, not on the TID source term but on he WASH-1400 source term. And calculates the parametric -- parametrically calculates the threat of suffering different doses at different distances from reactors. And I think it is fair to say that its conclusion that for the emersion pathway, ten miles was a proper planning distance and 50 miles for the food pathway were

generous envelopes for planning purposes, not for evacuation decisions. That there is nothing that I know of in the record that led to that report or in that report that said if you evacuate, you evacuate ten miles.

CHAIRMAN PALLADINO: Where did that come from? The ten-mile evacuation?

MR. BERNERO: It comes from that report, but it says if you are going to have an orderly process, you will have a FEMA and PEMA, and what other agencies are involved, they will have pre-conceived planned arrangements for dealing with evacuations out as far as ten miles for the emersion pathway and 50 miles for the food pathway, and "ad hoc" it thereafter.

If you look at the technical basis, the likely distances of evacuation are nowhere near that far. The lethal range, even with the WASH-1400 source term, is more like two miles, three miles, in the category 1 accidents or category 2 accidents. So there is strong reason to believe that right now we are even over-interpreting the WASH-1400 source term, which is the basis of that document.

And then what they are saying about emergency planning -- if we are indeed successful in establishing a sound basis for a lower source term, it will make it even more pessimistic.

MR. CULLER: Our understanding was at the time,

FEMA asked for basis for emergency planning and evacuation that there was a statement made basically, well, ten miles, and it just sort of --

MR. BERNERO: You just have to define what the word planning refers to. Planning for evacuation itself is the way many people are interpreting it now, and the state of California, for instance, is even going beyond that and saying if the NRC thinks it is ten miles, we ought to think it is 30 or something like that.

MR. CULLER: It is universally misconstrued because of lack of discussion, just as you have given in the transmittal of this terminology.

MR. BERNERO: I will stand to be corrected. Roger Blond is here.

(Laughter.)

COMMISSIONER GILINSKY: Roger, just a minute. You know, you were saying that this is for lethal doses for two or three miles. Well, that is not necessarily the line where you would draw for evacuation purposes, certainly. As I remember, the ten-mile limit captured something like 70% of the cases that would exceed the guidelines for evacuation. It was something like that.

MR. BLOND: There is one clarification. Within the task force we never recommended evacuation.

COMMISSIONER GILINSKY: No, that is true. That is

something that then got translated here.

MR. BLOND: That is correct. It was our sheltering or evacuation with sheltering as the preferred measure.

COMMISSIONER GILINSKY: Brian always made that clear.

MR. BLOND: Yes.

CHAIRMAN PALLADINO: Why did we change it?

MR. BLOND: It has never been changed. That is still, as far as we are concerned, the recommendation that has been made.

MR. CULLER: But it was interpreted in FEMA differently.

CHAIRMAN PALLADINO: The perception, my perception, is that people are all geared up in case of an accident to start evacuating and go on to --

COMMISSIONER GILINSKY: But to be fair, we did provide for the possibility of evacuation out to that distance if it was called for.

CHAIRMAN PALLADINO: But we are not bound to it if we find in certain cases that sheltering might be the right answer.

COMMISSIONER GILINSKY: Oh, that is right, yes.

MR. BLOND: An attempt to define the recommended procedure within the zone within the ten-mile area as what

would be the preferred measure, and when sheltering would be preferred, that would be the preferred action. And actually, beyond about five miles we did not show a clear preference over -- between evacuation or sheltering. And that was clearly made in the report, I believe.

COMMISSIONER AHEARNE: I think, Joe, if I could, Roger, I think, Joe, you also have to -- you came from the Pennsylvania experience so I am sure you appreciate that when you are taking to a group of people around an accident that is in progress and you are telling them that the people within the first three miles or four miles ought to evacuate; now, for the rest of you past three or four miles, you stay in your houses -- it is useful for at least the planning people to understand how do you handle an evacuation out to farther than that.

MR. CULLER: I think that is entirely fair.

COMMISSIONER AHEARNE: That is true.

MR. CULLER: You get into a terrible discrimination.

COMMISSIONER AHEARNE: Yes.

MR. CULLER: That is another reason why we would like to get the source term down.

COMMISSIONER AHEARNE: A lot of our focus -- at least we tried to make without that much success was in the planning area. We are trying to get people to make -- the

planning system to be set up to be able to handle that.

MR. BLOND: If I could add one thing in this regard. The limits at which you begin to take some response that you would either shelter or recommend an evacuation are set very low by the EPA at this point. Much lower than any of the source term reductions that we are talking about through these mechanisms at or near the levels of the group 3 or 4 accidents. And that would take you out to on the order of ten miles under some calculational conditions.

It is not clear that the reduction in source term is going to directly influence that zone in any manner.

MR. CULLER: I am sorry, I was not aware it had gotten down that low.

MR. BLOND: Yes, it is one of the five rem criteria for an external dose, and that is the protective action guide that EPA has recommended. And in fact some people have indicated that they are too high.

There is one comment I would like to make about the siting work, and I believe it is very important to recognize for the siting work that we are really talking about we are trying to move from the TID-14,844 source term from the design basis accident into a spectrum of accidents and consider the methods that we have used in the past for performing the siting and the relationship that we have used for the source term to the engineered safety features. That

is the key element in the siting work that we are currently doing; the relationships to the detail of the source term, especially within the very high groups, and siting is not making a significant difference to our appreciation of the problem.

But what we are trying to do in the siting work -- and something that I would like you to consider very seriously -- is move away from the -- what had been considered a conservative calculation before this time, but to something that we now feel is more realistic, and that a spectrum of potential accidents be considered in this area.

MR. CULLER: Let me ask a question about that. If the scenarios which are evolved under this plan, sort of a risk analysis kind of function, now are at odds with the scenarios that go in the licensing process itself for the reactor, assuming basically that they will not quite agree, would you be able to sustain the WASH-1400-like analyses, the scenario route, for siting and still be able to license the reactor on a maximum credible accident basis or design basis accident?

MR. BLOND: That is a significant question that we do not have a proper resolution.

MR. CULLER: That is what I thought. I have no question about that. That is true. Maybe Mr. Minogue would like to --

(Laugher.)

MR. MINOGUE: I think that is one of the really tough problems here. All through the regulatory process, the TID-14,844 approach has been used to make all kinds of design decisions, and to work all that back out is going to be very, very difficult because that process would be unduly complicated if you try to use some whole spectrum of accidents. So clearly, one of the real complicating factors on any siting rulemaking is that once you abandon the TID-14,844 approach, the use to which that has been put into review of a lot of system design questions has to be replaced by something equally simple. It was used extensively because it was a simple model, easy to apply.

MR. CULLER: Or there has to be an accommodation between the two systems that is not irrational.

MR. MINOGUE: Or they have to be reconciled in some way.

MR. CULLER: I think there is a reasonable chance for that. I do not know as much about it as you guys do, but --

MR. MINOGUE: I agree with that. I think the difficulty is not so much that the problem is technically overwhelming; it is just procedurally very hard to work out how you do it. It is so pervasive to the regulatory process it is hard to figure out how to work it out without being

more disruptive and causing confusion and chaos; not because it is fundamentally a very technically difficult problem.

CHAIRMAN PALLADINO: Bob, I think at some future time we will probably want to have some discussions on the interaction of the source term, the siting rule and even the safety goal.

Okay, any other questions by the Commissioners?

(No response.

MR. CULLER: Thank you very much.

CHAIRMAN PALLADINO: Thank you very much for coming.

(Whereupon, at 3:42 p.m. the meeting ended.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

COMMISSION MEETING

in the matter of: Commission Discussion with Electric Power Research Institute Representatives on Fission Product Behavior

Date of Proceeding: October 26, 1981

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. Parker

Official Reporter (Typed)

(SIGNATURE OF REPORTER)