NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING

DISCUSSION OF ATWS POLICY

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

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PUBLIC MEETING:
DISCUSSION OF ATWS POLICY

Commissioners' Conference Room
Washington, D. C.

Monday, November 10, 1980

The meeting was convened, pursuant to notice, at 10:05 a.m.:

BEFORE:

JOHN F. AHEARNE, Chairman
JOSEPH A. HENDRIE, Commissioner
VICTOR GILINSKY, Commissioner
PETER A. BRADFORD, Commissioner

PRESENT ON BEHALF OF THE NUCLEAR REGULATORY COMMISSION STAFF:

S. CHILK, Secretary to the Commission
L. BICKWIT, General Counsel
H. DENTON
W. MINNERS
T. SPEIS
J. NORBERG
M. MALSCH
C. GRAVES
A. KENNEKE
G. SCINTO

DISCLAIMER

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CHAIRMAN AHEARNE: Good morning and welcome to the NRC's version of Buckner Boulevard. Those of you in New York City understand the analogy.

We come again to address some further developments and details in ATWS. And without further comment, Harold, since you are the leader of the staff forces.

MR. DENTON: We have two brief presentations this morning. The first will be by Mr. Speis, Chief of Reactor Systems Branch, and he will discuss our systems program that we have under way at Brookhaven. This has been discussed briefly with you before, but he will just describe the scope of our program and our capability independently to calculate the consequences of ATWS.

The second stage will be a presentation by Warren Minners to respond to the Commission's request about issues raised in the article published in the "Nuclear Safety Journal" this summer and to compare our approach with test frequences and transients with those you heard from Mr. La Rouche at the last presentation.

MR. SPEIS: Thank you, Harold.

My name is Themis Speis. As Harold said, I am chief of Reactor Systems Branch. And the subject of my presentation is the BWR plant transient analysis program at Brookhaven National Laboratories.

Through technical experience at Brookhaven

National Laboratory the last few years, we have been developing the capability to calculate the consequences of full ATWS. These calculations have been used extensively to provide input and insight into the staff ATWS position and give us a better understanding of the ATWS indications.

Since the TMI accident, we have amended our technical assistance program at Brookhaven National Laboratory to go beyond ATWS analysis to include other transients. And the objective, of course, has been to develop a better understanding of the response behavior of BWR's, so we can provide the operators with better guidelines during emergencies.

I will provide to you a brief description of the scope of the program, the objectives of the program, and also provide you with some preliminary calculations of the consequences of the Brown's Ferry event, coupled with the limiting anticipated transient, such as closure of the main steam isolation valve.

I will also show the calculations that were done by General Electric and compare them with our initial calculations.

(Slide.)

This slide shows the BNL program scope. Basically, it includes partial scram consequence analyses of the Brown's Ferry 3 type, coupled with anticipated transients, the preparation of generic plant calculation models with other BWR types, in addition to BWR-4, for example, 3's, 5's, and 6's; and also the capability to --

CHAIRMAN AHEARNE: I'm sorry. I apologize for my ignorance. What is a BWR-3, 4, 5, 6?

MR. SPEIS: They are versions of the basic BWR. It is a boiling water reactor.

CHAIRMAN AHEARNE: That I got.

(Laughter.)

MR. SPEIS: They are different versions. Mostly they are improvements in the core. I think the 4 type has a jet pump. The next version had a better ECCS.

CHAIRMAN AHEARNE: They are specific characteristics --

MR. SPEIS: Specific for characteristics, higher fuel density, things of that sort.

(Slide.)

The next vugraph discusses the objective of the program, which should be obvious from the introductory remarks: to develop capability to audit vendor/licensee analyses, develop capability to perform rapid analyses of all BWR's, to determine safety impact of operating transients, and to provide a better basis for decisions involving operating reactors, to develop a better understanding of a transient accident behavior, for developing emergency guidelines and plant operating procedures, as I said earlier, and to give us the capability to independently audit and assess the adequacy of safety features.

I will now go to the next slide, which will summarize the transient analyzed.

(Slide.)

Basically, the transient analyzed was a main steam isolation valve closure from right at power and flow conditions, assuming that 76 of the available 185 rods failed to insert in one-half of the core. This of course was the pattern at Brown's Ferry 3.

Some key assumptions in the calculation involved the starting of the two heat exchangers in the pool cooling mode ten minutes into the transient; also, the introduction of boron into the system ten minutes into the transient. This is the so-called standby liquid control system. These are key assumptions in the calculation, as I will indicate later when I show you the results of the calculation.

CHAIRMAN AHEARNE: The reason they used Peach Bottom Unit 2 is because they were set up to use it?

MR. SPEIS: Yes, sir. We already had such a model at BNL and we corrected that basic model to the key parameters of Brown's Ferry.

CHAIRMAN AHEARNE: Brown's Ferry is a BWR-what?

MR. SPEIS: 4. Both Brown's Ferry and Peach Bottom are BWR-4's.

As you see in the vugraph there, a key system, the so-called automatic recirculation pump trip -- this has been installed already in the Brown's Ferry. It is one of the requirements that have come out from the ATWS position, and it is my understanding that all BWR's will have these installed by the end of the year.

MR. DENTON: That is correct.

MR. SPEIS: The next vugraph provides some more information on the analysis in terms of the input that we have utilized.

(Slide.)

The reason I bring this slide up is that we still depend on General Electric for some numbers. One of the key numbers is the power following the partial scram. We have no capability to calculate that right now, because it requires a three dimensional capability. Therefore, we are relying on GE's calculations.

What we do in our analysis, we adjust the scram in RELAP, which is one of the codes we are using, to give us the ten percent power. As I will discuss later, we have work in process at BNL with the RAMONA Code, which is three dimensional, and it should be available to us in about one to two months.

With that capability, we will be able to calculate the power after any scram, be it full scram or any type of configuration. So therefore we can do truly independent calculations without depending on GE or anybody else for input.

The next vugraph shows the calculations that we have performed, the results of the calculation.

(Slide.)

COMMISSIONER HENDRIE: Shove it over so we can see the ordinate.

MR. SPEIS: The solid line represents the BNL analysis and the axis has been given to us by an analysis performed by GE which we requested after the Brown's Ferry 3 event. The consequence of the transient described is the increase in the suppression pool temperature, which is due to the discharge of skim into the pool from the safety relief valves.

The bulk temperature limit, which is set by the severity of dynamic loads on the pool structure, is about 150 degrees without quenchers and 200 degrees with quenchers in the suppression pool. The quenchers are perforated pipes where the steam exits in smaller jets, thus improving condensation and preventing the formation of large bubbles, which have the potential of condensing on the walls of the suppression pool and thus increasing the loads. And that is the objective of the quenchers.

We can conclude the following from this analysis: There is reasonable agreement between the BNL and the GE analysis, at least up to 15 minutes in the calculation. We have not extended these analyses to longer, which we should be doing in the next few weeks.

The GE analysis carries the transient to a few hours, and the maximum suppression pool temperature derived from the GE calculation was 155 degrees, which is below the limit without quenchers.

The consequences are acceptable for this event, coupled with the limiting transient, as I said earlier, keeping in mind the assumptions of boron entry initiation at ten minutes and residual heat removal activation in ten minutes. If both of them, which are manual options and thus depending on operators, if those were delayed or the operator did not properly opt, then the temperature in the suppression pool could go much higher, exceeding the limits.

The next vugraph kind of summarizes the work that we will be doing with the RAMONA Code as I said earlier.

(Slide.)

As I already have said, the three dimensional capability will eliminate the need to normalize our calculations to GE's, and thus provide us with the power distribution and the level of power following a partial scram or any type of scram configuration.

We will be putting this information, plus all the other details, in a report in the next few weeks. We will make it available if you so desire.

This ends my formal presentation.

MR. DENTON: Isn't it fair to conclude from this that our previous views regarding the need for operator action are confirmed by this analysis?

MR. SPEIS: Yes, sir,

CHAIRMAN AHEARNE: You mean the time that the operator has to react?

MR. DENTON: Operator action is necessary beginning at about ten minutes for the present case, yes.

MR. SPEIS: I would point out, the boron entry as part of the ATWS position is automated also. Also, the flow -- one of the requirements is to increase the flow of boron entry into the core, as part of the ATWS position that you people have been considering at this time.

CHAIRMAN AHEARNE: Thank you.

Vic? Joe? Peter?

COMMISSIONER HENDRIE: Yes, a second thought. If you run the same transient -- let's see, because you do not have the full reactivity capability, you have not got a corresponding run with -- in several stages, less effective rod action.

MR. SPEIS: The power following the partial scram has been calculated to be ten percent. If the scram was greater, it would be higher. If there is no scram, then the power would be whatever it was originally.

MR. DENTON: Do we have a calculation assuming a higher power?

MR. SPEIS: We don't.

COMMISSIONER HENDRIE: What happens with no scram?

MR. SPEIS: These are the full ATWS calculations.

COMMISSIONER HENDRIE: Have you still got about -- how long have you got before the pool temperature begins to get away?

MR. GRAVES: I can give you some numbers -- we have some numbers on the impact of various partial scrams. There were two that were considered.

The Brown's Ferry 3 partial scram ended up with a reactor power about one minute after the scram of about ten percent. And the Brookhaven calculations -- this was obtained by GE by 3-D calculation.

Then there was another half-and-half scram. That is, half the rods in the core fully out and half fully in, which would have given at that same point in time a power of about 20 percent, and I believe a full -- loss of a full scram would be a power of about 40 percent. I believe it is roughly that. We are talking about 10 percent, 20 percent, and 40 percent.

Now, for the -- were you interested, Dr. Hendrie, in the time for a pool to reach boiling temperature?

COMMISSIONER HENDRIE: Yes.

MR. GRAVES: If you --

COMMISSIONER HENDRIE: I assume it just comes down according to the ratio of the power levels.

MR. GRAVES: It turns out that the key part is to turn the reactor off and get on decay heat, of course, because the RHR heat exchangers can handle only about two percent full power. Each heat exchanger can take only about one percent of rated power if the pool temperature were about 185 degrees Fahrenheit, in other words -- which would be fairly high. So it is less than one percent of full power per heat exchanger.

If you are in a situation where the fission power plus decay heat is like ten percent, I think clearly the RHR heat exchangers do not impact significantly the buildup of pool temperature. It would be less than the 20 percent case.

The key part in this calculation really is one, if you turned it on for fission power.

COMMISSIONER HENDRIE: It just pulls down the time in which you have to trigger the liquid poison system.

MR. GRAVES: For the full ATWS you would to towards the automatic actuation at two minutes into the scram. In these calculations, because this equipment is not there, it was assumed -- two cases were looked at. One was for ten minutes after the MSIV closure, which is starting the boron system. That number gave a peak pool temperature of about 153 degrees Fahrenheit about two hours after the event occurred.

If you, on the other hand, said that the boron was delayed in coming in for one-half hour, you still have the RHR heat exchangers on in ten minutes, but now, instead of ten minutes for the boron, you take 30 minutes and wait before you put the boron in. The peak pool temperature would have gone up to 186 degrees Fahrenheit, and that would have occurred about a half-hour into the event.

COMMISSIONER HENDRIE: I see.

MR. MINNERS: You might want to discuss the feedwater effect.

MR. GRAVES: Well, these calculations differ somewhat from the previous ATWS calculations. For example, in the NEDO Report 24222 that came out in one respect, and that is that the main feedwater is now on for about two minutes. Before it was run down to zero feedwater flow in 20 seconds.

The difference is GE is accounting for the fact that steam is in those lines and that the main feedwater pumps are not lost instantaneously as you close the MSIV. There is some time. In that time, the feedwater controllers are trying to address the feedwater flow according to present levels.

So we now have a situation where the HPSI and RCIC do not come in at about a minute or so at the core, because feedwater is coming in. Feedwater is at higher temperature. It is roughly 400 degrees Fahrenheit, whereas the HPSI and the RCIC would be about 120, because they come from the condensate storage tank. So a pound of feedwater is less effective than a pound of RCIC, just because of different temperatures. That is one major difference.

Is that what you were trying to get to.

COMMISSIONER HENDRIE: Which way does it turn out? Does the higher temperature get you a higher void fraction and hold down the power, or is it the cooling effect of the colder water?

MR. GRAVES: The key to the problem, Dr. Hendrie, you get higher power the more water you put in. It is sort of like that. In a sense, you get higher power when you have the recirc pumps on than when off. So if you will see in the slides, Themis has a slide there which will show the impact of the feedwater flow or RCIC flow.

If you turn on the feedwater or the HPCI-RCIC, you will find that you collapse the voids, and when you collapse the voids that is a positive reactivity insertion and the power will go up a short ways.

COMMISSIONER HENDRIE: Yes, yes. But you are saying there has been recently, whatever that -- since I only visit the subject, you know, every month or so, why, that could be several months for me.

You said the calculation now shows that the feedwater would keep running for an extra minute or two.

MR. GRAVES: This was the --

COMMISSIONER HENDRIE: And this would delay the HPCI operation.

MR. GRAVES: There is a slide that shows HPCI going on and off and feedwater going on and off, which you can see.

(Slide.)

The gyrations of the feedwater flow are primarily because of the feedwater controller. Full flow -- it is jumping around rather violently in the first minute or so. The main point is the HPCI water going in in the first about two minutes, and this -- because of this and the boiloff in the core does not occur -- does not drop the level low enough to get HPCI and RCIC on early in the game, as it would have if that feedwater flow was not there.

As you can see, what is happening --

COMMISSIONER HENDRIE: It is not a temperature effect, then, the fact that the feedwater temperature is coming in at feedwater temperatures instead of storage tank temperature.

MR. GRAVES: It is not a key thing.

COMMISSIONER HENDRIE: What about this volume, just the amount of flow? The feedwater is an enormously greater mass flow. You are just keeping the vessel with more water in it and the machine is able to shove it at higher power. Okay.

MR. DENTON: I think one of the things we are striving for in an overall ATWS proposal is a monotonically decreasing power, rather than these ups and downs.

CHAIRMAN AHEARNE: At least as long as it is cc iverging.

Any other questions?

COMMISSIONER HENDRIE: Not at the moment.

COMMISSIONER GILINSKY: How does this case differ, again, from the one at Brown's Ferry? Obviously the power is different.

MR. SPEIS: This calculation is an attempt to simulate the Brown's Ferry event coupled with an anticipated transient. In the Brown's Ferry event --

COMMISSIONER HENDRIE: Let's see. The initial power was lower at Brown's Ferry, and they did not get --

MR. DENTON: There was no --

COMMISSIONER HENDRIE: Did they get a closure?

MR. SPEIS: No, no, they didn't, no.

COMMISSIONER HENDRIE: So they did not get the modeling of the system?

COMMISSIONER GILINSKY: Those are the two.

MR. SPEIS: We coupled the event with an anticipated transient. It did not take place.

MR. DENTON: So they did not get isolation.

MR. SPEIS: They did not get isolation.

MR. DENTON: They worked and got all the rods in. So what we attempted in the model was, if that had occurred in response to a transient at full power, what were the same initial failures of the rods.

MR. SPEIS: All this was in 15 minutes.

COMMISSIONER GILINSKY: What triggered isolation?

MR. SPEIS: There are many things that can trigger isolation: low steam pressure, low water in the vessel, low level water.

MR. DENTON: Turbine trip.

MR. SPEIS: Operator action, inadvertent operator action. It is one of the anticipated transients that we use in our analysis. It is a limiting one because it produces -- it maximizes the temperature and the pressure in the primary system.

MR. DENTON: I think what we were trying to address is, if this had happened at full power as a result of a transient that required isolation, would operator action have been necessary and in what kind of time frame? That is where we were headed; and whether he did have to take these manual actions within about ten minutes to keep the pool temperatures within li·its.

CHAIRMAN AHEARNE: Any other questions?

COMMISSIONER GILINSKY: Let me ask you: How critical is the recirc pump trip?

COMMISSIONER HENDRIE: It is pretty important.

MR. SPEIS: It is important because --

COMMISSIONER HENDRIE: I don't think you make it without the trip.

MR. SPEIS: It slows the transient, the power evolution, and gives you more time to act. But by itself, it is not the only solution to the ATWS problem, of course.

COMMISSIONER HENDRIE: But I don't think --

MR. SPEIS: By going to automatic pump trip, you go into natural circulation, in which you get more bubbles and more negative reactivity, and the transient is slowed down considerably.

COMMISSIONER GILINSKY: What happens to all this if you don't get a pump trip?

MR. DENTON: You remember last time they showed some slides starting from full power with and without pump trip. And without pump trip, the power just goes straight up initially.

COMMISSIONER GILINSKY: How much -- You could trip them manually.

MR. DENTON: Yes.

COMMISSIONER GILINSKY: Just how much time do you have to do that?

MR. DENTON: Let me ask Warren if he has any --

MR. MINNERS: The automatic pump trip is in seconds.

COMMISSIONER HENDRIE: Yes, if you have to do it manually you would have to react pretty darn fast, because you have the machine in this hypothesized transient, as we used to say in the great days --

(Laughter.)

We used to say, this unlikely hypothesized event.

(Laughter.)

You have the machine shutting at full power, and it is delivering what, ten million pounds per hour steam out the pipes, and you slam those valves closed. The pressure builds up in the vessel. In a second or two, the voids collapse, which is a big reactivity effect. And by George, off it goes.

And the recirc pump trip is an attempt to give void formation, reformation on that power rise, its maximum chance to -- but if you don't trip the pumps, as I remember it, things go pretty far pretty fast.

I think it is, you know --

MR. SPEIS: That is one of the reasons --

COMMISSIONER HENDRIE: Ten seconds or that kind of proposition.

MR. SPEIS: In this calculation I show the recirculation pump was tripped in five seconds into the transient.

COMMISSIONER HENDRIE: Which?

MR. SPEIS: In the calculation I presented in the pool temperature.

COMMISSIONER GILINSKY: Has Brown's Ferry installed this?

MR. SPEIS: Yes, yes, yes,.

MR. DENTON: You will recall there was -- that about half the BWR's had put them in by the beginning of the year, and we had ordered the other half to get them in by the end of this year. So I don't know where they stood in that chain without looking.

COMMISSIONER BRADFORD: So there will still be a few that don't have it.

MR. DENTON: Very few by now. The last time I looked, it was scattered by the refueling outages. And the order gave them until the end of this year.

CHAIRMAN AHEARNE: Thank you, Mr. Speis, very much.

MR. DENTON: Warren will now pick up where we left this discussion as to what is the probability of encountering failure to scram and where do the statistics come from for calculations we do.

MR. MINNERS: May I have the first slide, please.

(Slide.)

MR. DENTON: Maybe it would be useful to try to clarify the reasons for the differing views, because they differ quite widely between the industry and ourselves. I think it is fair to say that we have been unable in the last decade to come to agreement on these numbers, and the differences in how you count tests and how you count frequencies are subtle. And where we tend to argue in differences depends on the significance we think that particular issue makes.

It is hard to know, in picking frequencie and number of tests, where reality really lies. There are good arguments made -- and I think both by the staff and occasionally by the industry -- where it is very hard to separate out what the real number is. And what we will do today is throw a lot of numbers at you, to try to show the differences between our views and the industry views.

CHAIRMAN AHEARNE: Did the industry get serious about this prior to -- my impression was somewhere around the end of '78, early '79, is when they first got serious. Is that incorrect?

MR. MINNERS: I guess -- let's see. The EPRI reports came out, I think, about that time. When people started to look at the transient data, I think it is like most other problems. The analysis has gone along and people have gotten smarter and things have changed. In some cases, they have gotten worse. In other cases, they have gotten better. You will find in some cases that you omitted a phenomenon and have to look at it. In other cases, there is a phenomenon you have looked at, you understand it better. And, for example, the pressures have steadily come down in the calculations because I think people know how to do the calculations better.

So I don't know just how serious people got at different times. It has been an evolutionary process.

CHAIRMAN AHEARNE: I was wondering, because at least once that had been raised, one of the reasons this has gone on so long is that the industry never took us seriously until around the end of '78 or early '79. And then, just as they began to take us seriously, the accident occurred and five plants were shut down and such. And as a result, we don't have it as seriously addressed as one would have expected, based upon the length of time the issue has been around.

COMMISSIONER HENDRIE: "Seriously" I guess is in the eye of the beholder. There have been recurring waves of this. When it was first introduced, the initial response was: Gee, why does that bother you? These are very reliable systems. Look at all the testing we have done, and so on.

And sort of over the years, these things go in cycles. And the staff worries, along with its consultants, and will come to either a statement or a restatement of position or some further advance in calculation, and put that out onto the table. And the industry side, in due length, will gather itself up and come back and put a new piece of paper on top of that piece of paper. And after a while, the staff comes along.

You know, it has been going along like that since '69. The industry side has certainly been serious about it at various times. But Warren is certainly right, the level of sophistication and ability to calculate these transients has increased enormously in the decade we have been kicking at it.

MR. MINNERS: I think EPRI has added quite a bit to it. I forget exactly when EPRI was first formed. But as far as that was done, I think EPRI has contributed in a lot of ways to a data base that was not there before.

I think the industry before that tended, each vendor, to serve its own purpose, and would not or could not cooperate. And with a central group to do the calculations, you can get a lot more calculations for your effort than if you are split up.

COMMISSIONER HENDRIE: Yes.

MR. MINNERS: And they also are not quite as wrapped up in the day-to-day commercial aspects of it and can devote their time to collecting the data, which is difficult for the industry group to do. So I think the EPRI work is a good idea.

CHAIRMAN AHEARNE: Go ahead.

MR. MINNERS: Okay. In the paper in "Nuclear Safety," I think it can be brought down to five subjects which I have listed up there, and I will discuss each one and try to give some explanation of the differences between what I would just characterize as the EPRI position and what the staff is proposing or has analyzed itself.

May I have the next slide, please.

(Slide.)

Now, the first thing I want to discuss is what I have given the name of rectification, which is: Has the Kahl failure been fixed? Had the relays in Kahl failed during the test in common mode failure, none of the relays would have worked.

EPRI did a calculation in which they said, since this failure occurred in the first 5 percent, the first twentieth of the operating experience to date, that is, about 900 years, what would be the probability that in the next 19 time periods, if you took one-twentieth of a time period, the next 19-twentieths time period, of not having a failure if the Kahl failure had not been fixed? And they said that was a 2 percent probability, and that is a pretty low probability.

So they concluded that since there was no failure, that Kahl had been fixed. Now, the staff has gone back and believes that the calculation should be done in a different way, and their calculation comes in that the probability that, if Kahl was not fixed and no failure was observed from the time of the failure to the present, that that would be a probability of about 40 percent, and that is not an unreasonable probability.

So we concluded that it is inconclusive of whether, on a statistical basis, Kahl has been fixed or not. Statistically, we probably would have to say that the probability of no failures being observed would have to be something like 5 percent to give a good statistical confidence, and that would say that you would have to have 60 of these time periods, about three times as has been observed.

Nevertheless, I think -- and I think the staff thinks, and certainly the industry thinks -- that Kahl has been fixed, for reasons outside statistics. We know what the problem was and we went back and fixed the problem. So statistics does not give you the answer.

COMMISSIONER GILINSKY: Let me understand what you are saying here. You had a failure of Kahl. Now, that could be -- that could represent a kind of failure which dominates the failures that you could get, or it could be simply one of many classes of failures.

MR. MINNERS: Let me try --

COMMISSIONER HENDRIE: Essentially, you are trying to decide, Vic, whether the Kahl failure remains a failure mode which is possible in the system, and therefore gets included in one failure out of so many reactor years, or whether you have a basis for saying, no, that is not left in the system any more and you ought not to calculate future probability -- probability of future events including it. How does that grab you?

Maybe you better do your own explanation of what you are doing.

MR. MINNERS: I am not sure what your question was.

COMMISSIONER GILINSKY: I am not sure, either. It has been a long time since I --

MR. MINNERS: It is saying that, if I flipped a coin once and got a head and then for the next 19 tries did not get a head, would I say there is something wrong with the coin? The first thing, whatever caused it to come up with a head the first time was fixed, so I would never have a head in the next 19 tries. That type of argument. The numbers are different obviously.

MR. DENTON: The key question is, should that be included in the data base when you go to calculate failure rates or not?

COMMISSIONER HENDRIE: If the statistical universe that you were examining had a certain failure mode in it, the Kahl mode, and that stays in the system, then it needs to be included in calculations of the likelihood of failures in the future. On the other hand, if you were quite sure that you had eliminated that failure mode from the systems all over, then it is no longer germane in the calculation of ow likely things are to fail in the future.

And the argument -- EPRI argues, made some arguments, that it must have been eliminated; otherwise, you would have seen some further manifestation than the original one; that there is only a two percent probability that you would not have seen some further manifestation.

MR. SPEIS: That calculation says, no, no, we go at it a slightly different way. And in our view, the fact that we have not seen a further manifestation really is like a 40 percent likelihood, and that is not unreasonable. So they would say it is inconclusive.

CHAIRMAN AHEARNE: But independent of either --

COMMISSIONER GILINSKY: I will have to examine these on my own.

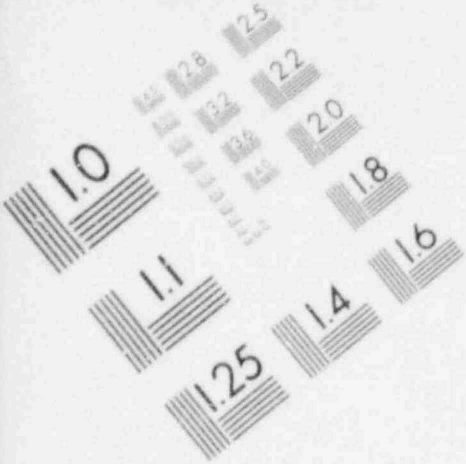
CHAIRMAN AHEARNE: You both agree --

MR. MINNERS: That particular failure is probably fixed. But I think our position would be that that failure is kind of representative of all of the different modes of failure that could occur and to discard it is to discard some information you have about how failure modes occur.

COMMISSIONER BRADFORD: Do either of those calculations take account of the possibility, instead of fixing it, it has simply been made less likely?

MR. MINNERS: No. In either set, it was fixed completely or not fixed, okay? The in between case, I don't know quite how -- I guess you could handle that, but it says it is made sufficiently unlikely that it does not have to be considered, or it has about the same probability as it had before. Those are the two cases.

COMMISSIONER GILINSKY: Let me just take -- suppose you have an urn with 1,000 red balls and a million white balls, and you have taken out one of the red balls, and -- out of so many tries, you find one red ball. Suppose



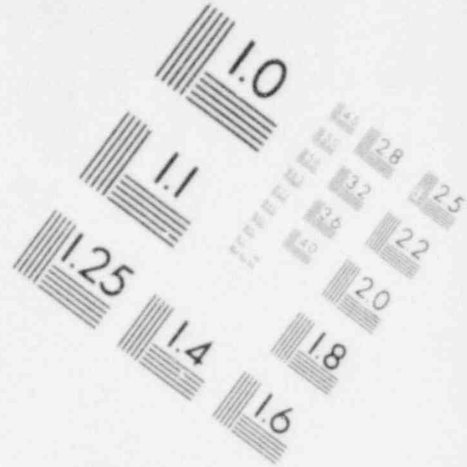
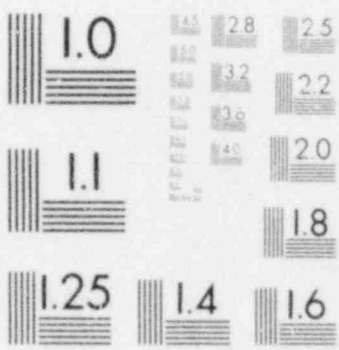


IMAGE EVALUATION
TEST TARGET (MT-3)



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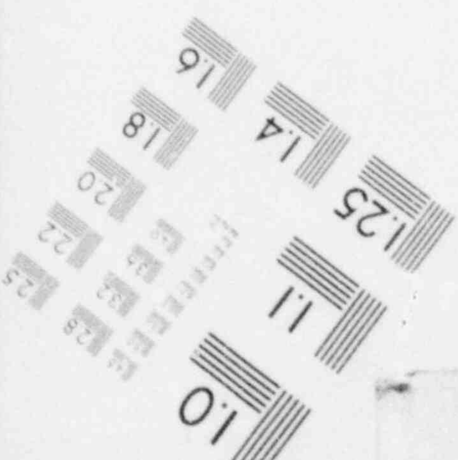
MICROCOPY RESOLUTION TEST CHART



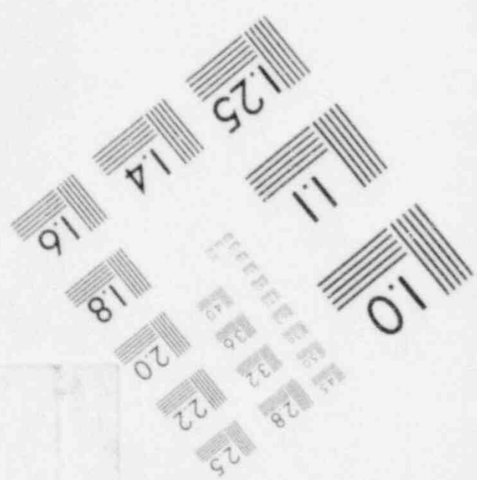
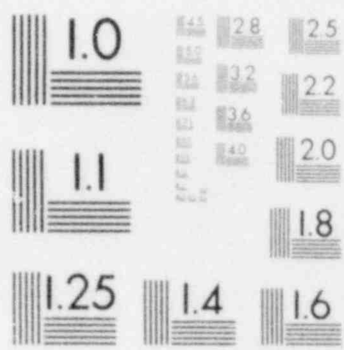


IMAGE EVALUATION
TEST TARGET (MT-3)



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MICROCOPY RESOLUTION TEST CHART

you discard that one. It may not make any difference to what you estimate the probability --

COMMISSIONER HENDRIE: That is right -- that is not a comparable sort of -- you are quite right, but that is not a comparable situation. What you are looking at in Kahl is a common mode failure, defective relay manufacture, which gets all the relays in the system.

Now, is that -- is that still a possible common mode failure of the system or isn't it? People will argue that, no, we understand one well enough so we are never going to get caught that way again. Other people will argue, well, maybe you will get caught. That is what this is.

MR. DENTON: On the basis of your analogy, you assume the same -- you would take that in your data base, one red ball in the drawing. And I think what the staff has done is take the conservative view in this: We are not yet willing to discount that first drawing. We are still counting that as a solid event.

COMMISSIONER GILINSKY: Whereas if you believe there was only one red ball in that urn and you hit on it, you believe you've fixed it. Then you really do think the situation is --

CHAIRMAN AHEARNE: Each of the red balls has slight variations, second level, and there was only one --

that particular one that you --

COMMISSIONER GILINSKY: So it seems to me it depends somewhat on your assumptions about the nature of the problem.

MR. MINNERS: Yes.

COMMISSIONER HENDRIE: Furthermore, when you get all through, as I recall some days long ago, I do not recall that including Kahl in or out or including the NPR in or out or both of them in or out changed my fundamental conclusions about ATWS.

CHAIRMAN AHEARNE: Don't confuse me with the data.

(Laughter.)

COMMISSIONER HENDRIE: It was just that it did not make a big enough swing in the ATWS frequency to take it into a clear, oh, boy, never mind that, or on the other hand, clear and present danger. It just stayed in that middle ground close to the boundary between fix it or not fix it.

MR. MINNERS: If you take the experience to date and looked at whether we had one failure or no failures, at a 95 percent confidence level you would calculate very small difference between the unavailability, only a ratio of one and a half.

(Slide.)

Now, at the 50 percent confidence level it goes up to a factor a little greater than 2. So yes, it makes some difference, but it is not a very large number.

Now, the last line there is what the staff has used, and you will notice that we use 3 times 10^{-5}, and with no failures you were calculating 50 percent confidence, a little over 4 times 10^{-5}, and we are using a smaller number. Now, the reason for that is, because although we calculated 1.1 times 10^{-4}, we said, well, we believe somewhat in rectification and we recognize that the test frequency is higher in there, and so we will reduce that. And it was an arbitrary judgment and we reduced it by that factor of 3.6.

Now, another way to look at this is also illustrated by item 2 there if you have multiple failure modes. And just as an example, it is not a calculation, but if you had 10 modes and each one of them had a failure rate of, say, 10^{-5}, okay, that would give you the probability of 10^{-4}, 10 modes times 10^{-5}.

Now, I go in and I fix one mode of failure. All right, that would only reduce my probability down to 9 times 10^{-5}, a 10 percent change, very small change. So it depends on whether you are dealing with some -- those are just two ways of looking at it. You can look at it where you have one mode which has been fixed or whether you have multiple modes, and we do not know how many modes of failure are presently in the --

COMMISSIONER GILINSKY: That was the example I was trying to bring out.

MR. MINNERS: That is right. So I think about all you can say is that the statistics help you understand the problem, but do not really answer the question for you because you do not know what the physical processes are. You are trying to look at a black box and do some statistics on it, and that is not enough information.

May I have the next slide, please.

(Slide.)

Now, we have test frequency, which is also an imponderable in some cases. One of the problems is to select what an effective test frequency is. We agree and recognize, along with EPRI and the industry, that the protection system is tested in part. It has diverse signals that come in. It has redundant channels. It has redundant breakers, things like that. They were all tested at different rates.

We picked the 12 per month as a reasonable basis for what we thought it was. A lot of the parts are tested at much higher frequencies, and the question is, how can you combine all those frequencies.

COMMISSIONER HENDRIE: 12 per year?

MR. MINNERS: 12 per month. I am sorry.

If you had a synthesis model with fault trees and everything, you could do that. You could combine all of these. But then you have to adopt some arbitrary way of putting the common mode failures in, because there is no good way of treating that in a theoretical sense. So it is very difficult, even though you know that you have these different rates, to combine them into one number that represents an effective rate.

The question is also what is the validity of tests. If I test one part of the system individually, are all those tests equal to one test of the system at once? And that is a question which is hard to answer. Are there interactions between the parts that will not be there unless you test the whole system at once?

Plus the conditions: Do the test conditions equal the conditions you have when the system is challenged by a transient? You have different environmental conditions, possibly, and also you have more interactions. So there is another consideration.

In looking at these higher test frequencies, they only apply to the reactor trip portion of the protection system. That is the electrical part. The mechanical part, the rods and drives, are not tested that frequently. In fact, they are tested at a very low frequency. There are single rod scrams, and then the rods are moved a little bit in a way that tests for common mode failures.

If you have a common mode failure, you would say then, well, gee, no rods move. If I could not move one rod, I would detect a common mode failure. Unfortunately, that did not work at Brown's Ferry. That common mode failure was not detected and would not have been detected by a single rod scram test.

So these higher frequencies only apply to the electrical portion of the system. Now, if you take some of the numbers and look at it for different tests per year and look at the calculated unavailabilities for the electrical system, that assumes that there was one failure, and for the mechanical it would assume no failures, because we have not observed any total mechanical failures, and yes, you do get some changes in the unavailability.

I would point out to you, at the bottom is listed what the staff is using for its evaluations, which is 1.5 times 10^{-5}, which is somewhere equivalent to numerically 50 to 100 tests per year on the electrical portion of the scram system. However, I would also like to point out that under the mechanical we are using 1.5 times 10^{-5}, when in actuality you would have to use something like 3 times 10^{-5} or less, if you are actually just taking the numbers and putting them in the equations, because even though there were no failures it is a much lower test rate.

So we in our judgment have taken this into consideration and used our best judgment, and these are the numbers that we have used for evaluation. I do not think we can claim that they are the truth, because I don't think anybody knows the truth.

May I have the next slide, please.

(Slide.)

Now, transient frequency. I just repeated up at the top what the different estimates were, were about a factor of two higher than EPRI has estimated, maybe a factor of four higher than EPRI has estimated for PWR's. And the differences for these are that we have excluded some transients, including transients -- EPRI has excluded some transients that we have included, and EPRI has included some transients below 25 percent lower.

I think the problems about most of these exclusions have been based on people's qualitative analysis of whether the transient is significant or insignificant. The number of transients that have been analyzed are mostly loss of feedwater and turbine trip closure of main steam line isolation valves, and we all agree those are significant transients.

When you get to the other ones, not as much analysis has been done, and it is more uncertain whether they should be in or whether they should be out. I have not looked at that much detail. Mr. Thadani has, and he has come to the conclusion that he thinks his list is right. But I think he would admit that it is probably a pessimistic view of how many transients should be considered, as opposed to the EPRI view, more of an optimistic view of what the transients are.

There is also a difference in the way we calculated the numbers. The staff said that an appropriate value would be the average number of transients that occurred in the first five years of operation. And EPRI said it should be 40 years average.

May I have the next slide, please.

(Slide.)

This is Dr. ~~La Rouche~~ Lellouche's graph that he presented last time, in which he shows the actual experience versus what we are proposing.

May I have the next slide, please --

CHAIRMAN AHEARNE: Wait. And your argument on his numbers with respect to your numbers basically is the exclusion principle he is applying?

MR. MINNERS: I hope on the next slide I can show you the two changes.

CHAIRMAN AHEARNE: Okay.

(Slide.)

MR. MINNERS: I have plotted what the staff believes the experience to be. The scales are the same on the two. The dotted line represents BWR operating experience, the solid line pressurized operating experience. You notice that the BWR goes off the graph. I left it that way to keep the scales the same, and that goes up to about 15 transients per year.

So the BWR total number -- we say that the total number of ATWS significant transients is higher than EPRI does. That is one factor.

CHAIRMAN AHEARNE: This is primarily due to your 25 percent primarily -- or is it also this judgmental factor you said?

MR. MINNERS: I could not divide it up that way, which is which. It is a mixture of both and I could not tell you which is the primary factor.

COMMISSIONER HENDRIE: What is BWR 75 percent, PWR 50 percent?

MR. MINNERS: The way we calculated it was to go back in and get a transient frequency for all the transients in a BWR, which came out to about 23 per year in the first year of BWR's. Then we looked in detail at those transients for one year and said, how many of these are ATWS significant.

For BWR's the judgment was made that about three-quarters of them would be a significant transient, that you would need a scram, and the other one-quarter you probably would not reed a scr:m. And so then we took the actual total transient frequency of each year and multiplied it by 75 percent to get that number.

We did not look at each year in detail, which I believe EPRI did in more detail. And for PWR's we used the same procedure, except in inspecting the transients we said abo9t half the transients would be significant for an ATWS event and half would not. So we took the total transient frequency in a PWR and multiplied it by one-half. And in doing that procedure, we have a much higher frequency range.

Now, the horizontal lines represent what the staff and EPRI calculated as nominal numbers to be used in further calculations. Ours was based, as I said, on just taking the average of the first five years, we get a higher number. EPRI took the average of the second through the fourth year, averaged that, said that that was equal to -- that rate would be constant for 39 years of plant operation; then added back in the first year of transients and divided by 40.

And when you do that procedure, you would get the lower dotted line, which is a small factor lower.

CHAIRMAN AHEARNE: Okay. So that on one of your previous charts you ha? a staff and EPRI transient frequency. The numbers that you are showing for the staff are the staff numbers on your chart. The EPRI numbers, these are --

MR. MINNERS: The EPRI numbers are not the EPRI numbers. I am just trying to show the difference that the method makes. The EPRI numbers I've shown before are three, four --

CHAIRMAN AHEARNE: 3-1/2 and 1.2 --

MR. MINNERS: And 1.2, which are way down at the bottom. I probably should have put those on Dr. ~~La Rouche~~ Lellouche's chart. But the two horizontal lines are just to show how much change you get by doing the different averaging methods. There is small change.

Now, another question that we have here, although it seems that the transient frequency is going down to a very low rate as the plants mature, what happens near the end of design life? Does the transient frequency go back up or does it stay the same?

The industry contends that with a plant on which they have continual maintenance on it, the transient frequency will stay down. I think the staff would view that as saying, there is lots of big equipment in that plant that you do not change, and that that equipment is going to age, and that your transient frequency, as in other things, will be the bathtub curve and go back up again at the end of design life.

So that is why we thought the five-year experience would be more appropriate. Again, I would say that is a pessimistic view. I would think that the position is an optimistic view -- the EPRI position is an optimistic view.

CHAIRMAN AHEARNE: I guess you are saying both are judgmental and there is very little analysis or data to support one or the other.

MR. MINNERS: I think I'm throwing up a number of transients, that is correct, and the method of calculating it is whether you take a five-year average or extrapolate one year or just mathematical manipulations. And how much truth they have is subject to judgments.

CHAIRMAN AHEARNE: Yes.

(Slide.)

MR. MINNERS: All right. The next subject was, if you add valves to a plant in order to mitigate ATWS, would you really reduce safety or -- will you really increase safety or reduce it? I have tried to characterize EPRI's argument and they said, compare the probability of LOCA given an ATWS. They will say, let's compare the different ways of getting to core melt, given an ATWS.

First you could have a valve stick open, which is termed TK, transient, failure to scram. That would result in core melt. Or a valve could stay closed or it fails to open in an ATWS event, or there are not enough valves --

there are not enough valves. That means you are at a point in the cycle at which the moderator temperature coefficient is so positive that you would exceed the acceptance criteria. On some plants that is like 40 percent of the cycle. And then you do a comparison.

Now, since TK is the same for all of them, all you have to do is compare the unavailability or probability of QP and PMTC, which EPRI does and says that Q is much larger than T and therefore adding valves increases the probability of LOCA. And I guess I would agree with that, with the caveat that if ATWS is mitigated because you have three and four valves on the plant, if ATWS is mitigated and you add another valve, it is not helping you mitigate the ATWS and it just is increasing the probability of a LOCA.

I think a better way to look at it would be, as shown in the bottom of the slide, what would happen if the ATWS was unmitigated. And once again, we presume that TK is a constant and we just compare QP and PMTC. But in this case we only have one valve versus two valves. The presumption is with one valve ATWS is unmitigated and with two valves ATWS is mitigated. And then when you put those numbers through you see a definite reduction in the probability of core melt.

Now, this is one way of looking at it, that all this comparison does is say that if you have an ATWS what is the probability of a LOCA. What you really have to do is look at it in comparison with all the possible core melt sequences that there are. And EPRI did that in their presentation.

May I have the next slide, please.

(Slide.)

They took the power operated relief valve as being a very dominant sequence. Because of this, they estimated the frequency that it would not open under ESF, that is, the frequency that the ECCS systems do not work and the valve sticks open, and then you get a frequency of 5 times 10^{-4} of core melt from a transient coming along, opening a PORV, the PORV sticks, the ECCS does not work.

He then compares that to an ATWS event. Okay, he takes our frequency of ATWS of 2 times 10^{-4} and, with an unmitigated ATWS the probability that you will have a core melt -- the total probability of core melt is 2 times 10^{-4}. He says, well, PORV events are dominant and you should not have to do anything about ATWS events. And adding valves is just going to increase the probability of a stuck-open valve.

But I do not think that that is the way you can look at the problem. I think that you have to take all the cases and compare them, mitigated versus unmitigated, ATWS with PORV dominates and with ATWS dominates. And I have done that.

May I have the next slide, please.

(Slide.)

Okay. This is assuming that PORV's that stick open dominate the core melt probability, and the case one is where ATWS is not mitigated; I don't have enough valves on the plant to prevent overpressure. In the first line I have assumed one PORV valve. It is challenged at a rate of about ten times per reactor year from transients. The probability that it does not open is 3 times 10^{-3}, which is the same number that Dr. ~~La Rouche~~ Lellouche used. The probability that the ECCS system does not work is 1.7 times 10^{-2}, again the same number that Dr. ~~La Rouche~~ Lellouche uses.

So the probability of core melt from that sequence is 5 times 10^{-4}. I am making the assumption that this plant only has one safety valve on it. And I go through the same kind of reasoning. Safety valves are challenged at a much lower rate than PORV's because they have a different pressure. They have a lower probability of sticking open, and I believe both those numbers are conservative.

I took the same failure rate for the ECCS systems and the total probability of core melt is 2 times 10^{-5}. Now we look at ATWS. RPS is the reactor pressure system. Using the staff number of six transients per year, an unavailability of 3 times 10^{-5} for the protection system;

and I am saying that the engineered safety features will not work 40 percent of the time because the plants are in a portion of the cycle for which the moderator temperature coefficient is too positive for 40 percent of that cycle. It is not a mitigated plant.

So I get 7 times 10^{-5} for the total due to ATWS, and when I add those up I get 5.9 times 10^{-4} for core melt in an unmitigated plant.

Now, the second case is to take a mitigated ATWS. The PORV is the same as before. The safety valve is the same as before. I am going to add one safety valve, but the challenge rate of that safety valve is 0.1 transients per year, because when I put in this valve for ATWS I'm going to have it set a little bit higher. No use challenging it more than you have to.

I have taken the same failure rate for the safety valve and for the ECCS and so, by adding that valve, I have only increased the probability of core melt by 2 times 10^{-6} And what have I done for the protection system, which is the last line? Again, it is challenged six times per year. It has the same unavailability, but the probability that it won't mitigate an accident I am saying is 10^{-2}. That is my goal, anyway. I hope if we make these changes we get down to 10^{-2}.

So the probability of a core melt from an ATWS event overpressurizing the plant is 2 times 10^{-6}. When I add that all up, I get 5.2 times 10^{-4}, which is not much of a change. But I would note it is a reduction, and what that says is if you have something which dominates like the PORV does, in the first draft it is 5 times 10^{-4} versus ATWS 5 times 10^{-5} Obviously, that is what "dominate" means. There is no use changing ATWS if it dominates.

Now, the question is, does ATWS dominate. In EPRI's case they took the PORV's, and I think we have recognized that the failure rate of PORV's is very high, and we have done a lot to reduce that failure rate. And we do not think that PORV's now dominate.

So if we go to the next slide and do a comparison with what the safety study would say.

(Slide.)

Presumably, fixing the PORV's we are back down to what the reactor safety study estimated the probability of core melt is. And in case one for an unmitigated plant, the safety study said that all sequences resulted in a probability of core about 3 times 10^{-5}. I have reduced that number from 5 to take into account that there was a little bit for ATWS in the safety study.

And you must remember that the safety study plants were mitigated plants. They presumed or they calculated that ATWS did not result in significant probabilities for core melt, in the PWR at least.

So if we take an unmitigated plant, we have to recalculate the reactor protection system. Again, six transients per year, same unavailability of 3 times 10^{-5}, the same 40 percent portion of the cycle. And I get 7 times 10^{-5}, for a total of 10 times 10^{-5}. And in this case ATWS dominates the core melt probability.

Now, what happens if I fix ATWS? I think the answer is going to be obvious. The reactor protection system, the numbers in the safety study stay the same. The reactor protection system is the same, except when you get to the mitigating systems. And instead of a 40 percent chance of failure, it is a one percent chance of failure, because presumably I fixed it. And so the probability goes down to 2 times 10^{-6}.

But I have to add in the one safety valve and -- but I am setting the set point higher on the safety valve to give it .1 challenges per year. Its failure rate is the same as I have used in the previous slide, and the ECCS failure rate is the same I used before. And I get 2 times 10^{-6}.

When I add that up, I am down to 3 times 10^{-5}, which just says what I think is obvious. If ATWS dominates and you fix ATWS, you will make an improvement in safety.

Now, the last item -- may I have the next slide, please.

(Slide.)

Where EPRI talked about the relative risk of ATWS and other events -- and I have defined "at risk" as the probability of core melt times the probability, given a core melt, that you will release activity, and then you multiply that times the dose. And I have taken the last two terms and defined those as consequences, which is I think what people usually have done.

Now, what EPRI came up with was a very large factor. They said the ratio of the competing risk to the ATWS risk was this ratio of numbers times 5,000.

(At 11:14 a.m., Commissioner Gilinsky left the room.)

MR. MINNERS: The 5,000 was the consequences. Now, in doing that I think that Dr. La Rouche has multiplied the probability twice because the 5,000 -- he derived that number from the safety studies and those already include all of the probabilities of core melt and the probability of release as well as dose. So I think he is double-multiplying in there.

The staff has looked at that and we say the factor is not 5,000, although in the safety study they did use that large a ratio. We would contend that the consequences of a mitigated ATWS were approximately equal to the consequences of a mitigated LOCA. You should have the same containment failure modes and things like that.

And in the safety study they presumed that that was not the case, that the other sequences could release things into Category 1 and 2 consequence ranges, which is very high consequences, and ATWS did not.

Once again, we are in an area where people have not done any very thorough analyses, but based on an engineering judgment of the situation, a core melt that comes from a mitigated ATWS I cannot see having really a lot of difference than a LOCA.

Now, the other case not considered by the safety study, if you have an unmitigated ATWS the potential for bad consequences is greater than from a mitigated LOCA. You have energies being transferred into the containment at a higher rate and you will get faster failure of containment. So we think that the consequences of an ATWS have been underestimated by EPRI and probably in the safety study.

Now, I have tried to explain some of the differences. I don't think that I have come to any that I can say somebody is right and somebody is wrong. But I think there are factors. I would still characterize it as EPRI has a more pessimistic view of all of these factors than the staff does. And I think that I --

CHAIRMAN AHEARNE: More optimistic?

MR. MINNERS: More optimistic than the staff does. And the staff's pessimistic view I think is a more proper way to look at an ATWS event in which I think there are so many imponderables. People don't know how to do the analyses. There is no way to check the analyses in any detail, because you cannot perform an ATWS or you don't want to perform an ATWS. And you must have a margin to account for those things. I think that has been the staff's traditional way of treating safety issues, and I think it is proper to continue that way.

I think we have less margin than we had in such events, LOCA events, that ATWS is being treated more realistically than the LOCA's are.

CHAIRMAN AHEARNE: Joe?

COMMISSIONER HENDRIE: I don't have any questions on the thrust of the discussion here at the moment. I need some more discusion sooner or later on the proposed rule and the fixes, all kinds of things of that nature, but not on this discussion.

MR. MINNERS: Well, you got the memo which we sent down, which gave a rewrite of the rule and gave a table that showed those fixes.

CHAIRMAN AHEARNE: Peter?

COMMISSIONER BRADFORD: No.

CHAIRMAN AHEARNE: Now, were you going to go on or is that the completion of this morning's --

MR. DENTON: We had not planned to cover the proposed changes in the rule, although we probably can if you would like.

CHAIRMAN AHEARNE: No, I would guess -- I think we would have to schedule another meeting. I would like to, before we break, though, ask OGC and ELD to address a question which OGC has raised, and that is namely the issue of the need for an environmental impact assessment or statement. And in particular, I notice OGC said that several years ago ELD advised the staff with respect to that. Marty, would you like to --

MR. MALSCH: I --

CHAIRMAN AHEARNE: I don't see --

MR. MALSCH: Only to say it is a highly controversial rule and you can make some arguments that are not entirely without merit. This has some environmental impact and I see EIS or a good appraisal as basically insurance that you want to have to back up the rule to meet a challenge, either by industry who claims occupational exposure or other environmental impacts, or anti-nuclear groups who would claim that the rule does not go far enough. I would see it as an insurance that you ought to have, given the controversial nature of the subject matter and the arguments you can make on both sides of the issue of environmental impact.

CHAIRMAN AHEARNE: Harold, do you know if there has been environmental assessment done in the past?

MR. DENTON: I am sure it has. Let me ask the representative of Standards if he is aware of where we stand today? I think when it has been looked at before we have come down not requiring a statement, but rather doing it on the basis that the health and safety arguments are carried out today.

MR. NORBERG: Jim Norberg, Office of Standards.

We have prepared an environmental assessment, not the impact statement, and it is still undergoing some staff review. We have a draft of it, but it is not quite satisfactory to the staff at this point in time. But we expect to have it shortly to you.

CHAIRMAN AHEARNE: Do you have an approximate schedule of when you do expect it?

MR. NORBERG: We are hoping to have it down in the next couple of weeks, I would say, depending on how much trouble we have in resolving a couple of the comments that we have gotten which may require us to get some additional data that we need to pull together. And we are not quite sure how much of an effort we may run into there.

But we are going to hopefully get it down just as soon as possible.

CHAIRMAN AHEARNE: All right.

Well, we will --

COMMISSIONER HENDRIE: What will -- how will an environmental assessment strike the legal advisors?

MR. MALSCH: I am willing to look at it.

(Laughter.)

CHAIRMAN AHEARNE: Good of you, Marty.

(Laughter.)

MR. MALSCH: Offhand, I would say there was a decent chance you could write a convincing environmental assessment that would show no significant environmental impact. But we really need to look at it to see.

MR. SCINTO: And we do not have that option. We will look at it.

(Laughter.

MR. SCINTO: If the document -- if the substantive portion of the document enables one to reach the conclusion that there are no significant environmental impacts, then the document will then be an appraisal, in accordance with Part 51. If the document does not support that, then we would recommend the document -- it depends on the substantive content of the document, the ECCS document, and --

COMMISSIONER HENDRIE: If you decide it does not support a finding of no environmental impact, is it going to be insufficient -- of sufficient scope to be stamped a statement, or are you going to have to go back then and spend some cranking away on further analyses?

MR. SCINTO: Well, the staff already prepared for the Commission a fairly extensive value impact statement. With that much work having been done, it does not strike me that there is an enormous amount of work.

MR. DENTON: I think in general, when we finish a really adequate assessment, it is essentially what you need for a draft statement. You may have to add some sections to an assessment in order to turn it into a draft. But you really have pulled together all the information bearing on the issue. You have much of the information you need in order to make it a draft. There are still some balances and alternatives you have to explore.

CHAIRMAN AHEARNE: Let me -- I would like to ask OPE one question. In the paper which they delivered last Friday on ATWS, they mentioned that, quote: "We understand that recent calculations by the Office of Research have shown ATWS to be the largest contributor to core melt risk for BWR's."

Which particular calculations were you referring to?

MR. KENNEKE: I believe the WASH-1400 itself showed BWR ATWS to be pretty close. Our understanding was that they had more recent data. I have not, unfortunately, seen that. I was hoping the representative would be here.

CHAIRMAN AHEARNE: We will have to schedule another meeting. I guess I would like to finally close and say that it was really unconscienable of the Commissioners who were here before I came not to have completed this issue.

(Laughter.)

(Whereupon, at 11:25 a.m., the meeting was adjourned.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

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in the matter of: PUBLIC MEETING - DISCUSSION OF ATWS POLICY

Date of Proceeding: November 10, 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. Parker

Official Reporter (Typed)

[signature]

(SIGNATURE OF REPORTER)