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UNITED STATES
NUCLEAR REGULATORY COMMISSION

In the matter of:

MEETING OF THE ADVISORY COMMITTEE ON
REACTOR SAFEGUARDS, SUBCOMMITTEE ON
RELIABILITY AND PROBABILISTIC
ASSESSMENT

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ASSESSMENT. :

Room 1046 Tenth Floor

1717 H Street, N.W.,

Washington, D.C.

Wednesday, February 6, 1980

The Subcommittee on Reliability and Probalistic Assessment, met, pursuant to notice, at 8:30 a.m., Mr. David Okrent, Chairman of the Subcommittee, presiding.

PRESENT

WILLIAM KERK - ACRS

CARSON MARK

J.C. EBERSOLE

CHESTER P. SIESS

MYER BENDER

HAROLD W. LEWIS

NOSER SINGPORWALLA

SAMUEL LAUNDERS

WALTER LIPINSKI

CARL MICHELSON

ELBERT EPLER

STEVEN DITTO

EDWARD C. ABBOTT

WILLIAM E. KASTENBERG

GARY R. QUITTSHREIBER

AND OTHERS

P R O C E E D I N G S

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

This a meeting of the Advisory Committee on Reactor Safeguards, Subcommittee on Reliability and Probalistic Assessment.

I am David Okrent, the Subcommittee Chairman. The other ACRS members present at this time are Mr. William Kerr, Carson Mark, Chester Siess, we expect some other members to be joining us later in the day.

Also, in attendance are ACRS consultants, Steven Ditto, Noser Singpurwalla, Samuel Saunders, Walter Lipinski, Carl R. Michelson, and Elbert Epler, not in alphabetical order.

We will have some other consultants joining us this afternoon. ACRS fellows Edward Abbott, William Kastenberg are in attendance. David Johnson also.

The purpose of this meeting is to discuss the development of response to Congressman Udall's letter of July 27, 1979 concerning consistency of actual component failure experience with that projected in WASH-1400 and the probability of occurrence of the September 24, 1977 Davis Besse and the March 20, 1978 Rancho Seco events using WASH-1400 methodology.

This afternoon the Subcommittee will be discussing Nuclear plant risks versus risks from other electricity

generating methods.

The Subcommittee will also spend some time discussing the development of quantitative safety goals for nuclear power plants.

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

Mr. Gary Quittshreiber, is the designated Federal employee for this meeting.

The rules for participation of today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on January 22, 1980. A transcript of the meeting is being kept and was made available as stated in the Federal Register notice. It is requested that each speaker first identify himself and speak with sufficient clarity and volume so that he can be readily heard.

We have received no written statements from members of the public. We have received no requests for time to make oral statements from members of the public.

Mr. Bender of the ACRS has now joined us also.

We will now proceed with the meeting, I believe the first order of business is for Mr. Vesely of the NRC Staff to discuss the NRC conclusions regarding consistencies of actual component failure experience of

that projected in WASH-1400 and what new data is obtained.

Thank you.

MR. VESELY: I have three slides that will summarize the status of our data activities. As you know we we did have a questionnaire, sent that out to approximately 30 individual companies asking them of their data sources.

We sent the ACRS copies of the responses we received on that survey. In detail, this slide summarizes the variations that we obtained and that people are using as generic failure rates. This is not plant specific now, this failure rates that are being used in the field for principle components. Pumps, motor operated valves, relief valves, diesel generators,circuit breakers, scram rods.

WASH-1400 is shown as the circle on the slides so that WASH_1400 lies within the ranges, you can see the LER's, for example, on relief valves. WASH-1400 for relief valves have had two numbers.

The power operated relief valves and safety valves that is why the two circles.

MR. KERR: You said something about these numbers are being, I didn't understand your opening statement.

MR. VESELY: These numbers are being used in the field. Our survey asked the respondents or asked the individuals what data they were using for risk analysis, for

nuclear risk analyses.

MR. KERR: Okay, I understand. I didn't know what you meant by being used in the field.

MR. VESELY: Our conclusion is that WASH-1400 is not inconsistent if we are still working on an updated data base for the IREP, the Integrated Reliability Evaluation Program, our schedule is to have that completed by the beginning of March, March 1.

As from the survey the conclusions are that WASH-1400 point estimates are not inconsistent. We are going to assign larger aero spreads on WASH-1400 data, roughly a factor of 10 on each side, as compared to a factor of 3 that was used in the original report. That is for active components, for passive components we still have roughly a factor of 30.

I show you another slide that compares our own sources -- that is NRC's, data sources, LER's WASH-1400 and NPRDS. This is, again, average data averaged over all plants and average failure rate.

You can see the scatter just among our sources is roughly a factor of 10, with not one source being higher. We do have problems, some of these differences, for example, between NPRDS and LER's and WASH-1400, are the way the data are averaged.

NPRDS calculates a failure rate for each plant

and then averages the individual failure rates regardless of whether that plant had 10 hours of population time or a thousand hours, we in LER's and NPRDS, of course, take the total number of failures over the total population hours, over all plants that essentally weights the individual plants by the number of hours that they have accumulated, still assumes that all the plants have essentially constant failure rate.

That can make two to three orders of magnitude difference. You can see one plant having one failure in 10 hours, another plant having one failure in 1000 hours. The 1 over 10 completely dominates the way that NPRDS averages, and you come out with essentially 1/2 X 10 to the minus 1, or about 5 X 10 to the minus 2. Where as we would come out around 10 to the minus 3. So, even in that case, there is a factor of 50 in that one simple example.

So, we are going back into NPRDS and re-evaluating these data to at least have consistent ways of estimating failure rates, average failure rates.

Plant-to-plant variation, we have sent down a first NUREG. We sent the ACRS this on pumps. We are getting-- you have the drafts of these, we are getting the finals out, this is the one that has come out, we will have 4 more of these NUREG's out within the month.

This shows WASH-1400 estimates with plant-to-plant

variations.

One has to be careful with these plant to plant variations because of lower bound, actually, if you will, it goes down to 0, because some plants have reported no failures in the time periods that we have examined. Those dotted lines just show the plant variation for plants reporting failures.

So, there is a factor of 10 to a factor of 30 variation, on plant-to-plant variability on top of this factor of 10 variability that people are using for generic failure, just average failures.

Data are messy and they have been. We have a lot of variability in data, plant-to-plant variability is larger than what, I think, WASH-1400 predicted. Also, the uncertainties in the average failure rate are larger than what WASH-1400 orginally predicted, and I think that is consistent with what Lewis has indicated that the uncertainties are larger and we are finding that.

That is where we stand with our data. At this time, we will have the update. We are coming up with -- because of the uncertainties in data we are coming up with several data basis, if you will, for our studies.

One is an average failure rate base, essentially, a new WASH-1400, with larger aero spreads. We are not seeing that much change in the point values with WASH-1400

the median values, we will see larger aero spreads, in general. I don't think the aero spreads or the median values will change.

We are also coming up with forum LER's higher failure rates for those plants to have shown to be a factor of 3 to a factor of 10 higher than WASH-1400.

There are a collection of plants that as you looked in the LER's that indicate that they have significantly higher failure rates than indicated in WASH-1400. Perhaps, 20% of the plants have a factor of 10 higher failure rates for their average, in looking at the average over those plants.

We are going to put those failure rates into our analyses to find their impacts on unavailability and systems.

Our position right now is that the data are not in the shape to simply believe point estimates or to do any absolute risk evaluations.

MR. BENDER: Bill, if you were to start with the premise that the WASH-1400 study and evaluation formal had an upper bound of something like 1 and 20 valves, for core melt, and you took this different spread in data and use it, what direction is that likely to leave the probability of core melt to?

MR. VESELY: WASH-1400 one in 20 thousand was

actually, a medium value, 5 X 10 to the minus 5. The upper value was 3 X 10 to the minus 4. We are re-evaluating WASH-1400 with this data, we do have it as a project. Brookhaven is doing that for us.

I would expect the median value in the upper bound to increase by, I would say, the median --

MR. BENDER: Order of magnitude or factor of 2, or what?

MR. VESELY: I don't think the order of magnitude. It may be a factor of 3 kinds of evaluations we have seen so far, but, again we are still in our evaluations. That is not conclusive at this time.

MR. BENDER: Is there any liklihood that looking at this will change the order of concern about certain types of accidents? Smaller LOCA's versus something else?

MR. VESELY: No, I still think we will still have that concern. I think it will show some other sequences to be significant that we discarded, that WASH-1400 discarded as being insignificant. I think that is the thing that we have found so far.

With regard to the human errors, I did have that human error workshop December 5 through 7, which we reported to you those data, that those are still being prepared. We will get the human factors hardbook March 31. We are going to review that for approximately a month

and a half, that NUREG is scheduled for publication in May. May 15.

MR. BENDER: What are you doing about things like fire probabilities? Are they being factored into this part of the study?

MR. VESELY: No, we are not doing anything. In fact, we have stopped the fire probability work and decreased the flood probability work to funnel that money into IREP right now. So, we are doing -- I have stopped, I have no prior projects going on nor flood projects going on.

MR. BENDER: Was that a wise thing to do?

Was that an arbitrary decision or have --

MR. VESELY: No, we thought about it, and I think, Bob Venarro or Frank Rowsome there -- we can talk about that. We had to make priorities and the decision that we made was that models and results coming from IREP can be used and will be used for flood analysis, fire analysis, but that is a very big program that is taking a large portion of our funding and we had to set some priorities. But, no, I don't have -- we have to recognize. I think I reported to the ACRS last time in California, about our reduced expenditures in these areas.

MR. BENDER: I think I wouldn't argue with the need to do some things more extensively than they have been at the expense of cutting other things to 0, I guess I would have

to think some about whether that was an action that should have been given more thought.

MR. VESELY: The fire work cut to 0 and the flood I have got about a hundred collecting essentially elevations location information which will supplement the IREP. Again, that is about a one and a half man effort, getting information on elevations and barriers which will be added to IREP, but, I have no systems analyses or modeling going on in flood.

MR BENDER: One other point that has come up any number of times, is the matter of the premise on which the reliability and the data is being developed. The credit that is being taken for doing things right or wrong. Whether the engineering premises on which the equipment is being designed and are appropriate to the reliability bases, what is being done about those things?

MR. KERR: Would you explain Mr. Bender's question for me, Mr. Vesely?

MR. BENDER: Do you want me to enlighten you?

MR. KERR: I would understand the answer better if I understood the question.

MR. BENDER: I will try to elaborate a little bit. If, for example, we are selective about what the particular liability makes as a premise for establishing the likelihood of causing an action, and happens to be

used in a way which is not appropriate to the valve, how do I account for that in a reliability base?

MR. VESELY: Other than in our data analysis, we are expecting, as you will see in the LER's, we are extracting causes of failure due to inadequate operation, design, human errors, other than identifying to contributions and data, I don't have anything particular doing on that.

We did talk and Lewis had recommended that human would be examined for mitigation as well as initiation of accidents. That, I think, we are holding until the IREP program.

MR. BENDER: I want to make sure we are talking about the same thing. Human initiation of accidents is one thing and the errors that are made in selection of equipment for particular application, is entirely different. It is the latter question that I really am challenging right now.

MR. VESELY: We are not doing anything particularly direct to that question, other than, for example, collecting field data. I don't have any projects or the Staff has that are focusing on that question, that design kind of question.

MR. KERR: Mike, in a sense, if you have a large enough population of data and you assume that people are making these kinds of mistakes, don't you take that into account with the data, or are you talking about something

else?

MR. BENDER: The data may be biased in the wrong direction. As a matter of fact the data may have come out of one industry to be applied to a different industry.

MR. KERR: No, I am saying if one is collecting data on power plants to have some sort of representative numbers of valves. These are suppositions that may not be true.

MR. BENDER: If the data were valid, of course, I think my concern is really that they are collecting data from all sorts of places, just mixing it all up, and using it as though it applied generally. They are protecting themselves somewhat by using this error span and broadening that.

MR. VESELY: I have to say that we are not only doing that, for example, for those plants in that we are getting plant specific data on the components, active components and the components that we get from LER's, for those plants that do show high failure rates, and there are some plants that are a factor of a hundred higher than the average. We do plan to put in those plant's specific failure rates into our models to find the impacts for that plant, when we analyze that plant 1 and 2, you really can't mix the data together. We have done that as a generic but we are also keeping it apart for other studies. For

example, a per demand or a per hour failure, failures that are related to time that occurred because their component sits has a per hour kind of behavior. Failures that occur when the components demand it because of stress or per cycle, cannot be lumped together with the per hour failure.

Also, design problems are kind of interesting because you can't really treat those as a per hour failure because if they are installed wrong and their design -- if they have a bad design, that component is going to be in a fail condition until it is caught. It is bad when it is installed, it is inadequate, and those have to be treated separately.

We are doing that, in our studies, we see a lot of sensitivities, a lot of different kind of evaluations having to be performed because of these contributions and because of the variabilities that we are seeing.

MR. BENDER: I agree. I can't make a statistical argument for what I am saying, but I think if you look very carefully at the power plant problem you would find about 3/4 of them are misapplications, and about a 1/4 of them are problems arising from the fact that the equipment didn't work the way it was designed to work. There was something physically wrong with a valve of some sort.

It would be nice to really have some better

information on that.

MR. VESELY: We have tables, and one thing that we did with the LER's and has taken us time is for the information LER's has given us, is to separate out the design cause failures, the human errors, in fact, we had to go back to the sometimes -- the detailed LER's or even to the plant logs, to get some of these causes.

So, there are extensive tables where these different causes of failures are separated, but, I believe you are right.

I don't see this as an end to answer, I see this as a beginning. In fact, we are going, for example, into plant logs, as I reported to you last time, with the IEEE and we have collected analysis on the 10 plants, 40 thousand failures, and having to go into the maintenance logs and plant logs to really dig out these causes and why the component failed.

LER's are not very good on giving causes of failure. It is very gross categorization and it is up to the individual in making out the LER, we find a lot of inconsistencies.

MR. BENDER: One other point before I stop. You made a comment about the fact that you are buying a specific failure rate seen in different plants to your analysis. There are a number of ways to do that.

One is to select, say for the particular plant, the failure rate is higher, and another is to say if the failure rate is that high on this plant, I have to assume that it is that high on all the other plants.

Neither one of those two assumptions is very good, but they may bracket the problem. How are you dealing with it?

MR. VESELY. We are doing essentially both of those where we are -- you said if you look at those plants that have had, and LER's that have had, more than one or more failures, they have a failure rate as an average about a factor of 10, a factor of 3 to a factor of 10 higher than WASH-1400.

We are putting that average in to see the impact. Also, if the specific plant we are analyzing has high failure rates, and we are seeing as you see in LER's that often times it is not simply one component of the plant but the plant average of all the components tends to be an a order of magnitude higher than the average over the plants perhaps maintenance philosophy causing these kinds other common kinds of problems.

We are sticking that those high failure rates -- we are planning to take those high failure rates for the plant into the model. The problem is their systematic effect. When one component is high, you can have all of

them being high, and the variations don't cancel out if you were -- for example, if you were going to assume a log normal to do some air propogation.

Our concern is the systematic effect where all components in a plant are high and causes compounds of one components a factor of 10, a redundancy is now a factor of 100 higher. That can have as large of an impact or larger than any common cause effect.

We are planning to do a lot of these sensitivity studies because it is not simply getting one number and comparing it to some criteria, I don't think we can do that at this time.

MR. OKRENT: Were there any components for which the new data clearly suggests a significant shift in the previous failure rate should be used in the future?

MR. VESELY: Yes, and it shows, but the shifts are again about -- one of the ones, for example, are pumps, turbine pumps, which are, as you see the shift there is upward by a factor of 10 to a factor of those plants who reported failure, we are seeing a turbine about 10 to the minus 1 per demand, and WASH-1400 at 10 to the minus 3. There is a factor of 100 if you include those plants that did not report any failures on turbine pumps, you get an overall LER average of about 7 X 10 to the minus 3.

MR. KERR: What is meant by a turbine pump here?

Is it steam driven?

MR. VESELY: Yes.

MR. KERR: A steam turbine driven pump?

MR. VESELY: As opposed to a motor driven pump.

MR. DITTO: Do these failures include ones like the ones that happened at Arkansas recently, where they got water in the steam lab and they started but tripped off on over speed?

MR. VESELY: No. These do not. Those are kinds of failures that are separated as different kinds of problems but they are not classified here as a failure to start.

MR. LIPINSKY: I would like to return to Mr. Bender's line of questioning.

When you went to reliability failure rates, the failure rates are functions of stress levels. Now, you stress in terms of temperature pressure and humidity, consequently, if you have a nominal set of values you can come up with the failure rate corresponding to these nominal values. But, if the stress level for a component deviates from the nominal value, then, you would expect to see the failure rate change.

In fact, accelerated testing is based on changing the stress level and trying to show you do have a correlation between the nominal value and the new value and how to correlate the value obtained under accelerated testing was

what you would expect from nominal failure rate.

How do you account for this? Do you think your error bands will indicate the variations in stress levels phenominal?

MR. VESELY: I don't know. We are trying to use NPRDS which attempts to categorize some of these stress levels.

Field data are dirty in a sense that they don't -- we cannot get the stress levels environment of what we are seeing. Field data is a large -- a significant portion of failures are due to human error and human causes, maintenance causes, which tend to dominate, swamp these kinds of environmental effects.

In our inplant data, we are trying to extract those components that do see radiation environments, for example, we have separated those as opposed to those that don't. We are looking at safety grade equipment versus non-safety grade to see if there are differences. That is in our inplant data.

But, right now, we have not been able to extract those kinds of differences, or those kinds of effects although, right now we are seeing a factor of a 100 -- a factor of 10 to 100 variation among plants for the same effect, a component of the same manufacturers, and diesels are an example where we are seeing as much as a factor of

100,10 to the minus 1 to 10 to the minus 3, on diesels.

So, we are getting a large variation from plant-to-plant variations, part of it could be due to these different applications of stress effects. Right now, I guess, all these effects are compounded and we have not separated them.

MR. LIPINSKI: I have another question on your data point estimate variation view graft, you have scram rods failed insert,and you're showing the value of 5 X 10 to the minus 2, and you are showing that being obtained from LER's?

MR. VESELY: You have to be careful -- yeah,

MR. LIPINSKI: What is the other one where you just have the points?

MR. VESELY: That is 10 to the minus 6, and that is what you add on.

MR. SAUNDERS: The data source.

MR. VESELY: WASH-1400, LER's, and NPDRS. I think NPDRS is.

MR. LIPINSKI: That is considerably higher by a factor.

MR. VESELY: That is right. You have to realize that those are really two different failure definitions going on here, LER's and NPDRS are failure to insert to 95%. Even if they inserted to 95%, it would be counted

as a failure. It is a partial failure, and there are different -- WASH-1400 is no insertion at all. LER's and NPRDS is insertion at 95% or less.

MR. LIPINSKI: Is this a single rod or all of them?

MR. VESELY: Single rods.

That is clearly identified in -- I am glad you brought that up, that is a case where the failure definitions are different and the only case that is shown on the slides.

MR. LIPINSKI: I was looking at that in connection with the deliberations that this one is certainly higher.

MR. VESELY: Yes, but, again, it is a very conservative definition of failure. In fact, in the LER's it is not even classified as a failure, it is just a partial insertion.

MR. BENDER: Bill let me try one more before we get off the subject. Walt reminded me of a point which I think has come up many times, too.

We have virtually no data on the survival characteristics of some components under seismic events, and things of that sort. If it is important to the evaluation to know that reliability base, what do you do?

MR. VESELY: I am not really not -- the Staff right now is not addressing the seismic question, as you know, it is a large project being carried out with Livermore, where their approach -- one approach is to soiicit estimates

from experts and I would hope they would be doing a great amount of sensitivity studies. In that case, I would think that the best you can do is sensitivity analysis.

MR. BENDER: I will take the example of the pump thing which you have studied, the turbine pump, and you got some data associated with it. Most of it is failure characteristics under normal operating service. I am not sure what the demands are on the system. What determines the effect on reliability of a power plant as a whole. Whether it is the normal service or tragic conditions that might have to be dealt with.

It looks to me like there is a whole area of uncertainty that is sort of being set aside. How do we know that we are setting aside the right part?

MR. VESELY: Well, I guess, our approach is to go after that for which we do have data and which we do see some sort of bouncing and then go after these other effects.

Turbine pumps is the case. You can Turbo Pump reliability -- unreliability is quite high. You are talking about 10 to the minus 1 per demand, or failure. So it is 10% chance of failure every time you demand.

That is the average with about a factor of 3 about that, that we are seeing from plant-to-plant variations. There is a 30% failure probability that we

are seeing at these plants.

So, our concern right now is to identify those kinds of contributions which helps to focus on areas that we have to start investigating as to what is causing that.

Our reaction and our interaction with this office of Analysis and Evaluation of Operating Data is to help identify these problems with our techniques and approaches so that further follow up and action can be taken. We have done some scatter plots for example, on plant-to-plant variability, and you will see, perhaps, 10 or 15% of the plants always standing out. A factor of 10 higher, a factor of 10 to a factor of 30 higher than the average. We don't believe that is LER variability. We looked at LER variability and the most we can ascribe is about a factor of 2. When you see a factor of 10 to a factor of 100 on some sheet components such as pumps, I think there is a flag up there that that means further investigation. That is what we are trying to do right now.

MR. MICHELSON: Before you leave, I have a couple of questions.

One is that when you are reporting LER data, over what period of time do you look?

MR. /ESELY: We broke it up into two periods 72 to 78 and 76 to 78, to see if there was any difference in the standardized text spec reporting versus non-standardized

technical specimen. We did not see any differences.

MR. MICHELSON: For the handouts, what period of time are you reporting?

MR. VESELY: I think that is '72 to '78, no, '76? '76 through '78. When we looked at '72 through '78 we did not see any significant differences.

It is interesting because we did not see any it was a concern and it was voiced by several offices that if you had looked at 76 through 78, when new spec or tech spec reporting requirements were instituted that you would see some differences.

Again, to the kind of precision that we were after the factor of 2 or more kind of things we were concerned with, we did not see any difference.

But, they are -- in the LER they are broken up into different periods.

MR. MICHELSON: When you identify a failure to start, does that mean a failure to start for any reason?

MR. VESELY: We have broken it up into, yes, failure to start. We have with command fault and without command fault, where failure to start means failure. We have shown it.

Any reason, for example, it could be human, be electronics, we have also gotten failure to start from mechanical failures. It is about a factor of 2.

MR. MICHELSON: Let me clarify my question. Failure to start can be caused by a device that is very remote from the device being reported.

It might be a control module on some other part of the plant that caused the failure to start. Now, that failure to the control module is sometimes reported as a failure to the control module having nothing to do with the pump and other times they might in the process of reporting the LER mention the fact that the pump didn't start.

So, you missed the data in one case, perhaps, and not in the other. How do you account for these when you say failure to start is it apparently for any reason?

MR. VESELY: We did, again, separate the LER's into failure to start, which we call without command fault that is, the pump itself failed and the failure to start with command faults where you did have some control of the device. We had to make special LER searches for the control.

MR. MICHELSON: You have to look higher for some of these because they are quite remote from the place of entrance.

MR. VESELY: We did that.

Again, as we said in our LER reports, these numbers that we are coming up with are gross kind of

estimates. A factor of 10 kind of precision. I wouldn't put any more precision than that.

When we start getting -- when you start trying to get precise figure 8 estimates, let us say a factor of two or less from LER's, I don't think you can do that just because of the reporting system.

We have found roughly 50% of the failures are due to the control on pumps and 50% are due to the pump itself.

MR. MICHELSON: It is probably safe to say that a number of reasons for a -- a number of possible failure to start situations were completely overlooked, because they were associated with an LER that never quite mentioned the fact that the process of failure of a given component fianlly that the pump failed to start. It just wasn't in the LER and therefore it is lost.

So, these are not conservative answers.

MR. VESELY: Not necessarily, but, you have the other side in which an LER failure, and we have tried to extract those out, may be a spec violation where it started but it did not come up to head quickly enough and did not develop pressure quickly. We tried to separate those out and didn't count them.

Again, this is why the variability of about a factor of 10 kind of precision.

MR. SAUNDERS: But, the laternative [illegible]ing these so-called bias reports for the statistician to make up LER's on their own. That is the danger which is even worse.

MR. VESELY: That's right. That's a good point. We wanted to get this out as a start. We can go from here but we need this out as a basis on which to develop better data. We got to have something, we got to start somewhere.

MR. MICHELSON: I guess the point being though, that the situation might even be worse than indicated by your results.

MR. VESELY: It indeed could be.

MR. KERR: It depends it seems to me to some extent on what you are going to do with the data.

You understand rather well that our control modules work and if you then are going to try to predict the failure of the system, an energy pump system to fail to start, it seems to me the data on control modules were permitted to make an accurate prediction even though you are looking at LER's, you may not have picked up every time a pump failed to start because of a failure of a control module.

On the other hand, if you are looking at a total system and trying to predict the behavior of this system you may be in trouble.

I think your point is very well taken. I am not

sure I know if there is much one can do about it if there is absolutely no mention of the failure to start in the LER.

MR. VESELY: In system models, for example, in the grosser system models, pump failure to start is identified on that module as including everything but some of the more detailed fault free system models separate out the mechanical or the pump failure to start itself and the control failures and then you can go after the data on the controls.

This is why we did want two estimates here. It depends very much on the level of modeling.

MR. MICHELSON: There is one other problem you get into; i.e., how do you handle the problems of fluid induced vibration, for instance, which causes many kinds of failures to occur one at a time, and sometimes in combination?

In reading the LER you are never quite sure how to put it together, because the LER may have related to this number failure that occured on that particular day. In a pattern of years, there are a number of failures occuring on the system which would greatly reduce the reliability of the system if you put it together in a train of events.

How do you handle -- it is similar to the

environmental NDS except here it is a vibration induced influence.

How do you account for that?

MR. VESELY: If LER's do not explicitly identify that as a cause, we would not find it. I have to say one thing, it is interesting in the LER's that you do find, this is a last point, in addition to these plants averages you do find specific components in a plant that can be very high failure rates. We have identified that it can have high failure rates and reoccurring failure where the same component, for example, there is a pump which has failed half a dozen times during the year, and there is a question.

Perhaps, there are 20 to 30 component pumps like that.

The same pump is failing again and again.

MR. MICHELSON: Yet, it may have nothing to do with the pumps, and there might be a problem with the suction head available, or whatever.

MR. VESELY: Or else we would guess that the repair or maintenance wasn't performed correctly, the time that it was prepared, and they finally got it up and repaired after 5 or 6 tries. We don't know. They are indicated and so you can have some sore thumb components.

The summary is that because of this variability we are going to have to do a lot -- I feel, a lot more sensitivity analyses, in our reliability evaluations and to look at the impacts of these different kinds of variabilities that were seen.

CHAIRMAN OKRENT: Thank you, Dr. Vesely. I think Mr. Abbott has some comments.

MR. ABBOTT: Just one or two. I basically agree with what Bill said on the data and the data has to be used very carefully.

The LER and NPRDS data failure rates are based on different things. For example, the NPRDS system may report as a failure of safety related pump the fact that packing plant leaked. It is really not a failure of the pumps. The pump can still develop proper flow rate and discharge.

On the other hand, the licensee of them reports don't necessarily look at all the failures that occur at any given safety related system.

I would say between 80 and 90% of the reports are submiteed to the Commission are a result of entering the limiting condition for operation in the technical specifications.

That means, therefore, that the equipment fails when the LCO is no longer applicable, that that failure will not be reported.

There have been some efforts in the last year or two to make sure that more things are reported through the licensee of that report but it has mostly been due through the efforts of the pricipal or resident inspector.

The basis for these data are just, they don't compare to one another, so you can't make smoothing or averaging of the data in order to come up with meaningful failure rates on each individual component and then, in turn, apply it universally to all 70 light water reactors that exist in the country.

It just doesn't make any sense. I don't think it is dark though, I think if the problem can be corrected if a more cooperative effort between the licensee and the NRC Staff has undertaken, to understand, Number 1, what does the Staff consider as a failure rate? I mean, a failure.

What data is available at the plant to use that definition of a failure to come up with a failure rate. Then, using those numbers in a faultry to determine what the overall failure rate is for that particular system and then use a ventry to come up with the final series of WASH-1400 events, and come up with the probabilities.

You can't ignore the plant people in getting this done. I don't think you can sit here and expect to take data which is generated from requirements which vary from NPRDS all the way to LER's and expect to come up with anything meaningful.

So, basically, I agree with what Bill said. You have to be very careful with this type of data.

MR. MICHELSON: Let me comment on your comment with wonderous regard, and that is, indeed you do have to define failure, the packing plant for instance, is a good example. That is a pump pumping from a tank into the reactor, the leak is suseptable. If it is a pump cycling water from the containment factor of the reactor that leak is a very unacceptable.

So, the same kind of failure in one case is truly a failure to be recorded, in another case it is, perhaps, not significant.

MR. SAUNDERS: It seems to me that the problem is somewhat analagous to that of hospitals. The science

is well known in each of those, but it seems to report of failures with the detriment of their own reputation they are very loath to do that.

We have to have a supply of state uniform reporting facilities because the use of the word bias, Dave, I think is incorrect.

As you say, the data just doesn't apply on a specific instance where it was generated. To do any kind of average is not.

CHAIRMAN OKRENT: You remind me of a recent story my daughter brought home from a job as a medical assistant.

The hospital called and said so and so had expired, she said what, she said he expired. She said you mean he died? She said yes.

MR. KERR: It strikes me that this is a serious problem and I don't think we should solve it until the LER's are now looked at as something like a traffic ticket or worse, and they are, in a way.

Someway, we could establish a climate which a certain number of failures are expected, I don't see how you avoid them, and somehow have some common objectives toward dealing with these. It seems to me our data and our safety might both be enhanced.

CHAIRMAN OKRENT: You know, I was talking to

someone in the UK about this question. How does one get a better approach to acertaining failure experience in a plant and this was, I think, at a time there was a question about the NPRDS. The particular individual I was speaking to said that a method that he had seen used in the UK which seemed to be effective was to arrange for a graduate student interested in the area to spend 6 months or some extended period at a plant, working with the plant personnel going through the information, knowing exactly what happened in each event following it up, writing it up in detail, and sort of writing a rather complete report. This would be his master thesis.

This seemed to be an effective way of getting the desired technical information.

I am not sure whether --

MR. SAUNDERS: We should be allowed to report British graduate students.

CHAIRMAN OKRENT: There are graduate students in the United States.

You don't have to have a graduate student but the idea is that the plant personnel are too busy to do it, but you want that kind of attention over some extended period of time. Not just going back to try and dig into old records, which is different situation, incomplete information.

MR. ABBOTT: I think that based on what Bill and

I have done over the last 3 or 4 weeks here that it can be easily demonstrated to a plant, I was a former plant operator, that this type of information in the WASH-1400 type of analysis is to their benefit.

So, if that can be clearly demonstrated, then, I don't see why it is not unreasonable that plants would do it. I don't think that it is that difficult. A couple of nubs here, he and I did it, and it was no big deal.

We managed to get through it and without a great deal of familiarity with particular plants involved, either. A couple of shift supervisors, and one engineer could do the type of analysis that was done in WASH-1400 without a great deal of difficulty in my estimation.

However, before any plants are going to undertake that, it is going to have to be demonstrated to them on the part of the Commission, that is going to be that to their benefit. Both from the safety point of view and perhaps ordering their own priorities within a plant to fix safety items. That is just a personal opinion.

CHAIRMAN OKRENT: Let's see, according to the agenda, we have a period of time in which the Subcommittee and the consultants hopefully can arrive at conclusions as to what we would recommend to the Full Committee, they say, in responding to that part of Congressman Udall's letter of July 27, that we have been addressing.

Let me again read the part of the letter which is relevant. He said that I understand that the ACRS is nearing completion of its examination of licensee event reports. I would hope that the report of this inquiry would address the questions of the consistencies of actual component failure experience with that rejected in WASH-1400, for example, with the valve failure experience approximates inferior rates used in WASH-1400 calculations and so forth.

I have been assuming that as a possible response we could note that we have the cooperation of the NRC Staff and jointly have written a large number of institutions asking for them to submit such data that they have and give several responses in this area. The NRC Staff have prepared a table showing how these fit together. There was a table in the previous material that they sent to us.

If it were so wished, we could use one or more of the view grafts just used by Dr. Vesely today. I think the plant variation one, for example, is of some interest so that we could have this sort of summary information. Then, I presume, we could make a few comments like, as we heard today, and I think as we knew that the uncertainties in the data appear to be larger than those predicted in WASH-1400 and there is a very considerable variation in the plant which has to be considered.

We might note that some of the components do show seemingly significant higher failure rate and I think, Dr. Vesely mentioned that turbine pumps are one. I don't know whether he said that diesels themselves also do this or whether just some diesels.

MR. VESELY: Just some diesels. I think the average of the diesel, we come up at about 5 X 10 to the minus 2 as compared to 3 X 10 to the minus 2 for WASH-1400. The large plant-to-plant variability, again.

CHAIRMAN OKRENT: I don't know. How does this seem as a possible approach to a response to this part of Congressman Udall's letter?

MR. MARK: It somewhat frightens me to think of turning over a chart such as that plant-to-plant variation.

I don't know that we could figure out what on earth to do with it here. I can't believe that he could there.

What it can succeed in doing is making them think there are really terrible questions and maybe there are.

But, this doesn't prove that. I think we should be much more careful in what we say to him then letting him read this and say, My God, the factor is somewhere between .2 and .2 X 10 to the minus 4, and nobody knows where it is. It may be true that we don't know where it is. But, I don't think I know from looking at this what

we are looking at.

The LER's are different from the surry plant, which is all that is represented in WASH-1400, and the B and W plants are different from the other, and we don't know those differences enough to give him anything which he might regard as numbers.

I think we can fairly tell him that I am looking into it. The fact that different suppliers have complete somewhat different numbers than those used in WASH-1400. That the reporting scheme is badly in need of some uniformization. Without that we can't give him an answer to that question in the simple form he put it. I would rather them steer for the kind of estimate that Kastenberg and Abbott, I am not quite sure who put it together --

MR. ABEOTT: The three of us.

MR. MARK: The three of you -- tried in filling in the gaps for B and W systems in particular, which were not treated in WASH-1400, tried to make estimates giving those tentative numbers, I am not sure how tentative you say they are, would be better than giving him one of these or any of these assurance.

CHAIRMAN OKRENT: By the way, the material that Kastenberg, Abbott, and Bickel did is to be covered in the second part of this morning's session.

MR. MARK: My feeling is that that, with all the

apologies that are necessary to make about it, would be a better sort of package to send to Udall, along with the explanation taht WASH-1400 doesn't cover the cases, and it is necessary to do the work in order to cover those cases. Here is the first pass at such work.

CHAIRMAN OKRENT: I guess, I, myself, read the letter as asking two different questions. One was just how did the failure rate data used in WASH-1400 which was obtained from various sources and somehow put together in judgment applied and numbers derived, and so forth, how do these compare with what one is learning from LER experience -- in fact, as part of the LER report our Subcommittee wasn't trying itself to extract the failure rate data, and that is what we wrote when we responded to this letter. We will have to do something separately.

I think, myself, that the plant-to-plant variation is something one has to think about. It is non trivial. I think that we have to note that it exists and has to be evaluated.

MR. MARK: I don't disagree with that at all, David, but, I think that my own feeling about this is that I don't begin to understand what we are looking at. We are looking at a difference in reporting for one thing. We are looking at a difference of definition as to what constitutes failure, and if we had something

in which those differences were easier to identify, I wouldn't mind sending it. At least we take it seriously. Absolutely.

MR. LIPINSKI: There is one important point about LER's. LER's in themselves do not allow you to tell failure rates. I would like to address that question to Dr. Vesely.

How do you calculated failure rate from an LER? All you know is the failure, you do not know the successes.

MR. VESELY: Yes. In fact, that was the major effort in this task, was to separately collect the number of attempts and those sources are identified in the LER and NUREG, and we did that, for example, with a technical specs going to the plant box. That was a separate effort and is not reported in LER's, and that is right.

MR. LIPINSKI: That is an important point, that the LER's themselves do not allow you to calculate the failure rates because the successes are not recorded.

MR. VESELY: That what takes a great amount of time for these LER novelties. That the identification of populations of successes and standby time are obvious times which are all not identified.

MR. LIPINSKI: How long have you been in service? If it is per demand, how many demands took place?

MR. BENDER: Dave, with regard to the response to Udall, it seems to me that any report that we put out should include in it some commentary trend. The trend that is I thing that it is inevitable that some plants that have had a misapplication, for example, a piece of hardware, will show a very high failure rate unique to that plant.

It is not of the equipment, it is the fault to the way in which the plant was designed. We can't say naything about how to go about how to go about correcting those, and we are stuck moreor less with just using the average historical result of all the equipment that has bee used.

I think just by using the air plant analogy, you can always argue that things are going to be improved, and if we are not getting those kinds of improvements, we are somewhere near to projecting ourselves toward suicide and I think we ought to make a point of that trend evaluation.

That is really the value of looking at the LER. If we don't make that point, I think we just missed the whole purpose of the thing.

MR. VESELY: I would like to say, with regard to the pump and all of our LER reports, we do identify trend, any time trends, we have not seen any significantly different, time difference or learning. We have not seen failure rates for the past 6 years. We have looked at 7

years go down. We have not seen any significant time frame.

MR. BENDER: I am not trying to select the pumps that example. Some things will not change and will not have a change of events.

MR. LIPINSKI: The diesel is a specific example. They were identified as a problem very early, years ago. Based on their failure rates, has anything been done to the physical design or operation of diesels to include their liability or are we still living with their poor performance?

MR. VESELY: We are still living with high variabilities on diesel. 5 X 10 to the minus 2, 8 X 10 to the minus 2. Almost 10 to the minus 1.

MR. LIPINSKI: There isn't any learning factored into improving their liability?

MR. VESELY: If it is, we have not seen it so far in the LER data, or in the any other data.

MR. ROWSOME: You can find isolated examples in which plants have had a recurrent failure mode that they cracked down and eliminated. But, you do not see a large trend across the industry.

MR. BENDER: Let me take the notorious vavle that we had trouble with at TMI. Presumably, the plants that understood the problem develops and corrective actions that

take care of it.

Plants for example found a way around the difficulty and reduced the frequency of that failure, substantially.

It seems to me that that kind of thing ought to be factored into any evaluation of failures nor do they have some understanding of dealing with their problems properly. We will find out the ones that have just left the problems go on and on, are the ones that are ultimately going to have the bad accidents.

MR. MARK: It seems to me that something which really should, at least this is how I feel at the moment, would be entitled to tell and could happily tell Udall, would be that partly in response to this question, we have discovered the need of a great deal of work to get uniform data available to answer questions of this sort. Maybe you will say that we knew that before, but I don't think we talked this much about it before.

CHAIRMAN OKRENT: No, I don't know, also, I don't know that we can get uniform data, I don't expect the data to be uniform.

MR. SAUNDERS: Uniform reporting.

MR. MARK: That leaves better reporting. In fact, it occurs to me that the shift technical supervisor whom I have never been able to figure out what it was he was

going to do, would be an excellent guide for getting these reports down.

CHAIRMAN OKRENT: By the way, if I could comment in this regard, during the last week or two, among the mountains of mail that come to me, I think I seen something that says the Commission plans to have rulemaking on making NPRDS mandatory, and so that will provide an opportunity to discuss what it is or what it should be or should it be or so forth. If the Committee wants to offer comments in this regard, it should decide it has this interest and so proceed.

Let's see, I think, can I get Epler, because he has been patiently for five minutes. Maybe inmpatiently, I don't know.

MR. EPLER: I might suggest one possibility of putting in the reply to request to Udall. That is that we might congratulate someone for the excellent job that has been done to minimize the effect of these failures in spite of the high rates required in certain areas.

For example, I looked at dozens of traumatic events, and perhaps, if I can't find any where the failure to component has been the major contributor. There have been, from time to time a period as a minor aggravation but not as a prime concern.

I might suggest something you would not put in

letter, that we do have failures that are not receiving as much attention. For example, just lately I saw an LER from Brunswick 2, where a pump had been out of service, and the crew was dispatched to a line requirement. The disconnected the coupling to allign the pump but they disconnected the wrong pump, so they had two pumps out of service for 7 hours. Now, I looked at the WASH-1400 to see the failure rate for that component and it turned out that they had used up 200 years of unavailability by that one piece of misinformation.

Now, I think if we can get someone to do as good a job on fixing that part of the plant as has been done on the components, we might make some progress. Now, I wouldn't recommend that that go on the letter.

CHAIRMAN OKRENT: Dr. Kerr?

MR. KERR: Mr. Vesely, I wanted to see if I understood your earlier comments on the data, and the first slide that you showed called data source point estimate extremes, I believe it was accompanied by a comment that these were data being used in the field that it was your conclusion after examining the relationship between the WASH-1400 data and these data, that WASH-1400 data were within the range of those being used in the field. But, that you had concluded that perhaps, the uncertainty range should be a factor of 10 rather than a factor of 3.

MR. VESELY: It is roughly, yes. It would be larger.

MR. KERR: Now, I have tried to put those statements in some sort of correlation with the table called plant-to-plant variation in which one would conclude, I think, that WASH-1400 data were not within the range of in mitigation with the plant variation.

I don't want to make a statement there because I am less certain of the significance of those data.

MR. VESELY: If you will, those minumum, because if you look at plant variations, we have already shown the variations for those plants reporting failures.

Some plants reported no failures, so the lower bound of those dash lines actually goes down to 0.

There are plans in the 6 years we have looked at that have no failures and their failure rate in the lower bounds is essentially 0. So, we had no failures, we only got upper bound in the failure rate, so that indeed the plant variation goes from that upper bound shown to really 0. If you include the plants that did not report failure in the reporting time.

We have shown those plants that reported one or more failures. A significant portion of them, perhaps, 25 or 30% reported no failures, and that was true in all cases of components in that contrary so that --

MR. SAUNDERS: So that should be in red in the top of this.

MR. VESELY: That is right. So, that all we are seeing in a design are plants reporting failures. So, if you include all the plants now, I think WASH-1400, will still be in the range just because of our lack of information, on the plants that we have seen no failures as far as a small amount of data.

MR. KERR: If you were responding to Udall's letter, you might conclude that WASH-1400 data were within the range of compliant data that we have seen?

MR. VESELY: I would extend those dash lines in that chart to which you use that for Udall down to 10 to the minus 4 extended all the way down. We will include all those plans and yes, I think a generator, but, again we are having a large plant-to-lant variability.

I think our conclusions are that the WASH-1400 averages are within the ranges, but that there is a significantly large plant-to-plant variability, that I would not think was recognized by WASH-1400.

There is two variabilities here. There is a variability in what people are using as an average which is at first chart, and on top of that there is a plant-to-plant variability which is as big or larger, which compounds.

MR. KERR: Thank you.

MR. SIESS: What bothers me is if you extend those lines down, then it looks like you are lumping all the operating plants in the one statistical stew which is almost meaningless. The plant-to-plant variations were not random, they are deterministic.

MR. VESELY: That is right.

MR. SIESS: You just can't take that mish-mash and put it all together. You are going to get 6 orders of magnitude uncertainty.

MR. VESELY: Well, I think we can't put it together if you --

MR. SIESS: We are not interested in the average of 70 plants, we are interested in probably the worst ones.

MR. VESELY: We want to do both. We want to, in our studies, examine design variability where we assume all components that they have the average and looking at what design effects are and then we want to put in the individual plants failure rates to find out what the impacts on the operation or from the operation, the actual component history.

So, we are planning to do several types of analyses that average is only to compare design and design effect.

MR. SIESS: You don't know whether those differences are due to design maintenance, operation, the training of the operators, or what.

MR. VESELY: What we are trying to do is --

MR. SIESS: This isn't the difference between surry and zion, it is the difference between the utility and the people.

MR. VESELY: Yes, in fact, this is the point where if you look at at plant-to-plant variability, I don't believe that you can read this as a random variation when you have a plant you stick in the particular plant's failure rate hwere you have them. They are all high, and you make them high, and you have systematic effects.

On the other hand, you do want to compare, I believe, design variations when you try to separate out and compare a surry design with a design design and where I do use an average data. We are doing several kinds of analysis the ame way we are going to look just at hardware parts and then we are going to try to incorporate the human and common causes to look at their potential contributions.

MR. SIESS: At some point, we are going to try to find out why there are plant-to-plant variations, I hope. Those that are designed, obviously, we say that is a better design, if there are operations we would like to know why they have got it.

The Staff has gone through an awful lot of lessons learned recommendations to improve operations but they are

all based on Three Mile Island, and I don't know how many of them are based on looking at LER's and this kind of experience.

MR. SAUNDERS: Nobody could stop AMROS for display of variation in certain stitch requirements whether or not they are deterministic. Certainly, you should do that. On the other hand, nobody can stop people from misinterpreting that data as being due to some statistical cause when in fact it is not.

So, errors are bound to occur in both ways.

MR. KERR: Well, almost any data point is not deterministic, is it?

MR. SIESS: In your want ads.

MR. SAUNDERS: In God's eyes, certainly that is true, no doubt about it.

MR. LIPINSKI: On plant specific data on safety diesel generators is an example, given a specific plant with low performance on those diesels, lower reliability, one would ideally like to take and calculate the probability of the loss of all power of offsite and onsite. If that plant is not capable to withstand that event, then, we have a problem.

MR. SIESS: Somebody has mentioned that.

MR. LIPINSKI: But, the lumping only gives you a guide in terms of the total average performance, but if

you are at the bottom of the line, then, you really have to take a close look at the specific plant and see what that means, or that particular plant.

MR. EBERSOLE: I recently visited one of the Duke plants and suddenly found that it is PORV valves, the blocking valves simply can't be closed against the dynamic flow, and I recall a similar finding about Mr. Michelson some years ago, that the containment valves that didn't have a ghost of a chance of closing against the dynamic head. So when you look at the statistical data, I think we don't have to recall that the test data that you are feeding into the computer process, is meaningless because the specifications for the test are not meaningful. So, there can be a complete whole through which all this can fall, if you don't qualify a test to make it realistic.

There were a number of years in essence we had no containments. We thought we did and they were quite extensive probabalistic studies that held forth on the general safety of the nuclear plants with every allowance placed on the containment.

We had no containment.

MR. KERR: One had no containment if one had full pressure of the WASH LOCA but the containment in some situation might have been needed and which would have been effective.

MR. BENDER: That is really the problem with the whole data business, we are going to find that when we look at it, it is developed around circumstances that are not exactly correlatable with certain events under which the equipment has to be used.

Somehow we have to deal with that.

MR. EBERSOLE: How do we deal with that?

MR. BENDER: Dammed if I know, but it requires more proof testing than I think is available to us right now, or better analysis of the equipment that we have.

MR. MICHELSON: Dr. Vesely, when you looked at plants which you called good performers and those that you might have called poor performers, did you go back and attempt to determine whether there were differences in the reporting attitudes, were there some people reporting everything and other people reporting only a very few things, or certainly this is a possibility. Did you look into that?

MR. VESELY: We found that for a number of cases, we went back and found that there were some plants that are better reporters. We did not find that to be more than a factor 2 effect, in fact, a factor of less than a factor of 2. We found, for exeample, plants that had high failure rates for some components and now have high failure rates for others.

So, there was even a variation within a plant.

We are talking here of orders of magnitude factor of 10 and factor of 100 kind of effects. That is hard to try to envision one plant reporting 1/100 of that of another plant. We have done some tests, the factors of 2 are the most that we can ascribe to LER variations in talking in with. You might want to talk to him about that.

We don't thing this variation, this large variation than we say so in the report are qualified by these large variations we are talking factors of 10 or 100, we feel are much more than simply reporting differences.

MR. MICHELSON: That is a remarkable factor. It has got to be more than equipment differences, too, because the industry doesn't buy that many different kinds of pieces of equipment and in such similarity.

So, somewhere there has got to be an explanation, it is hard to believe that it is all in the maintenance although, I guess, if it got bad enough, it could certainly pick up. I find it difficult to believe that a factor of 100, and there is something wrong in planning the information maybe I am looking in the right part of the LER file or something.

MR. VESELY: That is a possibility.

MR. MICHELSON: Because, if you remember, AG&G did a beautiful job on relief valves. It pointed out

how good brown surry relief valves were and because they had missed completely finding them in the LER file. When they saw them, I am sure they changed their conclusion, but they lost them because they didn't look in the right place.

I don't know, maybe there are failures on these plants that you haven't found because they are buried, and the fellows aren't reporting them maybe like you think they are reporting them.

So, they are lost in another file.

It is a factor of 100, it is a hard one to explain on any rational basis.

CHAIRMAN OKRENT: Any other questions?

MR. SINGPORWALLA: Maybe I would like to make a few comments, recognizing that I am not completely familiar with what is going on.

Number 1 is that the numbers that we are considering here are extremely small and the differences and the factors that you see are completely within the realm of the range of numbers that we see.

The second point is pertaining to the lumping of the data. That is what one does, one does any statistical analysis. The main reason why one does these things is because one cannot account for individual causes or it would be really difficult to account for them and that is

why one lumps on these data together.

So, personally, I don't see anything drastically wrong with the reporting information which has arisen from various sources as long as we make it clear that this information has arisen from various sources, and therefore, the variabilities are rather large.

As we narrow down the sources, or as we eliminate the various sources, the variability will go down.

So, I would propose in your letter to Congressman Udall, that this kind of chart be presented with a clear indication as to what -- with a key explaining what these things mean. We cannot get away from this. We come back again and have the same kind of problems, because we cannot eliminate all possible causes that attribute to the range of the day.

MR. SIESS: I may be unsophisticated statistically, but to me there is some difference including data over a wide range, and including data over a wide range where I think the distribution may be strongly bimobile, and I suspect that is what you have here.

If you go down to 0, you have got a whole bunch down at 0 and another bunch up here. If it is presented that way, it doesn't give me any problems, but it is the wire on the graft that goes over 10 to the 6 range, and nothing to tell me it is bimobile. I think something has

left out but that I am interested in.

CHAIRMAN OKRENT: Any other?

MR. VESELY: That is why on the graft we just showed that those plants that did record we consider those plants, we consider that they do cluster. If you just look at the plants themselves, that report they are clustering, this is why on the graft it shows we didn't go down to 0.

CHAIRMAN OKRENT: I have a feeling we might be able to use any time I can save in this session in the next one. I am going to propose we take a 10 minute break now, and when we come back we will begin the next topic, which is a little more complex.

(Whereupon a 10-minute recess was taken.)

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CHAIRMAN OKRENT: The subcommittee will go on to the next part of the agenda which is discussion of the 1977 Davis Besse and 1978 Rancho Seco. We'll first hear from Mr. Ralph Cohn of the NRC staff.

MR. ROWSOME: You are getting three handouts, one of them is a backgrounder on the instruments themselves; and includes a little bit of a probabilistic analysis. A second handout is a memorandum to Ray Frailey for you all which is closer to a ghostwritten job of an answer to Congressman Udall; and the third is a set of slides on an alternative questions which I think are illuminating than asking a question: what would WASH-1400 methods and data of predicted as the probability or frequency of occurrence of these incidents?

Would you like me to go over very quickly a summary of what these incidents were, what happened at Davis Besse and Rancho Seco?

CHAIRMAN OKRENT: Very quickly. Go ahead.

MR. ROWSOME: Here's a summary of the circumstances, comparing TMI -- all three of these incidents begin with what the principal -- initial disruption of the reactor coolant system was a feedwater trip, although in each case the specific cause was different.

At TMI and Rancho Seco, the reactor power was substantial. At Davis Besse, it was not. In TMI and Rancho Seco there was considerable core burnup, in Davis Besse there was

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not. In each case the reactor tripped. In the Davis Besse incident, the operators got to it before the automatic tripped, set point was reached. Main feedwater tripped off.

At TMI and Rancho Seco, the auxiliary people that are assistant failed to come on promptly, failed to come on automatically.

At TMI and Davis Besse, the pressurizer relief valve opened and stuck open. At Rancho Seco, the relief valve was gagged closed, a safety relief valve performed the same function, it opened, but closed properly. There was no stuck valve in that incident.

As you know, there was a misleading pressurizer level indication in both TMI and Davis Besse. In all three cases, high-pressure safety injection started.

Now, Davis Besse has attracted particular interest because it was a precursor to TMI, it took very nearly the same course. The details of the origin of the feedwater trip differed, but the accident took much the same course after that. Rancho Seco is a much different accident, and the interesting feature there did not entail the stuck relief valve, but entailed a common load failure of a non-nuclear and nonsafety grid instrumentation power supply which caused the integrated control system to go haywire; interferred with an automatic actuation of the auxiliary feedwater system and disrupted many of the instruments on which the operators depended to

control the plant.

I don't believe I have slides for this, but I have described in the draft letter a way of addressing Congressman Udall's question. I point out that the probability you would assign to an historical event is entirely an artifact in how broad a class of events you take to be representative of that event. You can get any number between one and zero, depending on how narrowly you draw the class of events for which the probabilities are defined.

The way which is most natural for the reactor safety study is to consider the class of feedwater transients as a class. WASH-1400 assigned the probability to that class of events, and did not break down with a few notable exceptions the many contributors to feedwater trips, and, in fact, predicted that between one and ten feedwater trips be expected per plant per year in the roughly 30 years of B&W plant operating experience that had been accumulated before March of last year. One would have therefore expected somewhere between 30 and 300 feedwater trips in B&W plant with the WASH-1400 best estimate of 100. In fact, there was 150 indicating pretty good agreement with the WASH-1400 numbers for feedwater trips.

MR. EPLER: Per train of feedwater?

MR. ROWSOME: No, that's --

MR. EPLER: Two trains?

MR. ROWSOME: -- no, that's a total trip of the main feedwater.

There's basically two trains of main feedwater, but they're highly interdependent, they're not intended to be single failureproof. They're 50 percent trains.

MR. MICHELSON: Before you go on, would you clear up one point for me?

The relief valve at Rancho Seco opened and then closed. In the process of losing the instrumentation that they lost, did they retain the ability to know that the leaked valve had closed again which they would have gotten from their temperatures?

MR. ROWSOME: I don't know.

MR. MICHELSON: Could they have stuck open the relief valve and never had known it, at least for some period of time?

MR. ROWSOME: I don't know how that tailpipe temperature monitored, whether it's affected by the faulted buss or not.

MR. MICHELSON: Same question on the QRB if it happened to have been ungagged, would they have known --

MR. ROWSOME: Again, I don't know the power supply for that temperature indicator.

MR. MICHELSON: Keeping in mind that it's in the QRB case, it's more than just temperature indication that

they could use.

MR. ROWSOME: Right.

MR. ABBOTT: The power supply that failed --

MR. MICHELSON: But they lost a lot, I think they lost more than that in the process. They lost a lot of instrumentation that you would find useful, but not what you would call safety-related.

MR. ABBOTT: That's right. That's the non-nuclear instrumentation busses X and Y.

MR. MICHELSON: Right.

MR. ABBOTT: Why the Y buss?

MR. MICHELSON: And I think that it's possible that these temperature indications were in that group.

MR. ABBOTT: That's probably true.

MR. MICHELSON: In which case, they could have got into a TMI kind of situation and not even known it for some period of time.

MR. ROWSOME: That would have --

MR. DITTO: How often do you suppose the WASH-1400 study would have predicted loss of main feedwater concurrent with the loss of a great deal of the controls in monitoring equipment such as appeared at Rancho Seco? Would that have been picked up in any of those --

MR. ROWSOME: Well, yes and no. That is a highly plants specific kind of susceptibility. A study was made of

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Surry in the course of doing the reactor safety study to look for the classes of events in which a common cause could give rise to the feedwater trip, and also degrade the reliability of the auxiliary feedwater system. They found one example, it was one of the dominant contributors to the risk, and that was station blackout, loss of AC power. They did not find a dependence on a non-safety grade instrument power supply to be of such a class. But, in fact, there isn't that dependency in Surry because the auxiliary feedwater system -- the other start system -- the auxiliary feedwater system is safety grade and does not have, so far as we know, any dependencies on the non-safety grade equipment which could induce the feedwater trip. That is other than the whole AC power situation, whole station blackout situation.

MR. MICHELSON: One more thing, could we clear up? Even though the Y buss was the one I understood failed, I also understood that it had well over half of everything on it. Could you give me an idea? About two-thirds, wasn't it? And so thus being X and Y doesn't mean --

MR. ABBOTT: That's right.

MR. MICHELSON: In fact, the Bell folk kind of understood that really all the good stuff was on the Y buss.

[Laughter.]

MR. ABBOTT: The good stuff was the front panel indications of pressure and temperature.

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MR. DITTO: Is it not true that given the short circuit that occurred, everything else followed with stuff working pretty much as it was supposed to work?

MR. ROWSOME: The short circuit occurred, a fuse failed to open.

MR. DITTO: It failed to open because of the way the power supply was taking care of short circuits, mainly by a current limit, which --

MR. ROWSOME: It should have tripped; it was overloaded. It should have tripped well ahead of the circuit-breakers.

MR. DITTO: In the analysis I saw, it said that there was a current limiting on the output of the power supply which prevented the fuse from breaking and caused a low voltage, which, in turn, pulled everything out of the wall.

MR. ROWSOME: I think the fuse was set at a power level that was about, in an order of magnitude smaller than the overcurrent trip set point on those breakers. I don't know the numbers, but my impression is that the fuse should have blown, and it was a failure in the fuse that allowed this to happen.

MR. DITTO: I don't think so. I think that you'll find that --

MR. ROWSOME: My information --

MR. DITTO: Do you remember that wall, whether the

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fuse was really short on current?

MR. LIPINSKI: No, all I know is that the fuse failed to open as it was expected to open. I don't know why it --

MR. DITTO: Anderson did a study, and he says it was the current-limiting device that prevented the fuse from working, and that makes quite a difference.

MR. ABBOTT: At the time that we talked with the plant, we talked to the resident inspector -- that was about two or three weeks ago, they did not know why the fuse did not blow.

MR. KERR: Maybe they should talk to Anderson. Who's Anderson -- I don't know.

MR. DITTO: Anderson reviewed that for NRC, works at Oak Ridge.

MR. MICHELSON: One other slightly different tact on this question -- have you ever gone back to look to see what a cup of coffee would have done instead of the dropped light bulb, is there -- have you gone back to those circuits to see what high leakage will do to them, this sort of thing? Water can get into those switches very easily since they are essentially open at the top. Have you looked at the possibility of an operator spilling a cup of coffee on a panel and getting into these kinds of difficulties?

MR. ROWSOME: We certainly have not. Steven Hanauer in the unresolved safety issues task force has that

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piece of the generic issue dealing with station blackout that deals with degraded voltage conditions, and his people in the broader context of grid upsets, like the Millstone incident in which there was an undervoltage condition in the grid, have been looking at that and developing a regulatory position on that.

I am not aware of anybody who is specifically looking at degraded conditions in the control room on instrument busses.

MR. MICHELSON: You can buy that same switch with a little plastic cap that keeps the water out. People don't like it because it's a little harder to operate, but it's a correctable situation, but somebody has to say, "Correct it." Of course, you say that only after looking into the consequence.

I would think somebody would certainly want to look into it.

MR. ROWSOME: Well, maybe we need to add to chapter 15, the coffee cup incident.

MR. MICHELSON: The coffee cup is something people worry about from time-to-time.

CHAIRMAN OKRENT: Since we're on this point, it's not clear to me what the mechanism is to ascertain whether either the responsible group in NRC has this question in mind or that the licensees would look at it, because the PAS group is not the Operating Reactors Division. If there is a point here

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that you think warrants looking at, I think maybe a useful mechanism would be for you to write a little memo -- it doesn't have to be too long, and we could then forward it to Mr. Gossick or whoever has that job now, I'm not sure, asking that --

MR. ROWSOME: Bill Derricks.

CHAIRMAN OKRENT: Thank you. -- asking that the question be examined, unless Dr. Kerr has already flagged this.

MR. KERR: If it's in the category of operating experience, I know to whom you should send the memo.

CHAIRMAN OKRENT: There are two different things. One is -- I'm sorry. One is to identify matters, another is to see whether the proper action is being taken in licensing arena, and it's -- I don't propose to ask Michelson write a memo back to somebody who's supposed to be looking at identification.

MR. MICHELSON: The same problem came up when people started getting worried about fire in the control room, wondered, you know, well, are you going to put a hose in the control room, you know, to take care of those kind of problems. That's just the wildest thing you could possibly do, you even have to be worried about fire extinguishers, and what have you, and when can you turn a fire extinguisher on a panel? It's something that needs to be looked into, and I'm not familiar

enough with the circuits, the low voltage, the solid-state circuits -- they're very tricky things, even on leakage currents. Somebody really needs to look into them to make sure that -- well, they're not water-permissible in any form in the control room -- or other chemicals for that matter.

MR. BENDER: Can I get back and ask a question about the feedwater trip business?

CHAIRMAN OKRENT: Go ahead.

MR. BENDER: You cited some frequencies for the B&W plants. If I were to exclude the B&W plants, what answer would I get?

MR. ROWSOME: My impression is that the central estimate of about three feedwater trips a year is applicable to all four light water reactor vendors to as good as that number is in any individual plant, there is a good deal of variation. We see more feedwater trips in the first year or two of service, 10 or 20 a year is not uncommon. During the first third of core life in the first core, first six months or a year of burnup; then in mature plants, mature Westinghouse plants, have shaken that number down to the order of one a year, a little better. I don't have comparable numbers for mature plants of the other vendors, but the three a year seems to be within half an order of magnitude either way, a reasonable average for the whole industry.

MR. EBERSOLE: You see a difference between the

turbine-driven feedwater pumps and motor-driven, they're both --

MR. ROWSOME: Right.

MR. EBERSOLE: I would suspect the motor-driven pumps are intrinsically the more viable, but I don't know.

MR. ROWSOME: Most of these, I believe, are command faults and they're not due to simply failures in pumps. Most of them are spurious trips originating from main feedwater isolation system, or turbine trip or things like the default in the demineralizer at TMI. There are faults -- a significant percentage, but by no means the dominant contributor, comes from the buss tripping out spontaneously.

MR. BENDER: I guess I'm surprised at that. I thought tripping out simultaneously -- spontaneously -- wouldn't involve all the pumps at one time, normally, would it?

MR. ROWSOME: It's fairly common for a cascading fault to occur when that happens, even though in principle the accident could furlough down to about 60 percent power and make it through. It's quite likely that you'll get a low-level trip on steam-generater level before the control systems have had a chance to respond, or something.

MR. BENDER: Only one feedwater pump trips out?

MR. ROWSOME: One feedwater pump trips out, yes, that's quite common. That happened at Davis-Besse, incidentally, in this one incident. Only one pump or one train of feedwater was checked out, although I should say in this case, it was

the control valve that shut, so one steam generator was fully isolated. But it's fairly common in plants in which one out of two pumps that are hettered (?) together and than split to the two steam generators, for a single-pump trip to cause a full feedwater trip indirectly through the mechanism, out-of-spec, steam-generator level before the reactor has had a chance to readjust to the new power level at which one-pump operation could be sustained in equal degree.

MR. BENDER: Are you trying to sort out these various kind of design pecularities? It seems to me that when you're looking at these kinds of faults and trying to discriminate between the things that cause the fault would give some insight to what you ought to do about fixing the --

MR. ROWSOME: Anyway this is not --

MR. BENDER: A bare statistic is just a problem to deal with.

MR. ROWSOME: PAS is not engaging in any research into the constituent causes of feedwater trips except insofar as those causes might have a common mode of potential that we should worry about because of permanent risks significance. For example, if what causes feedwater to trip out also degrades the reliability of instruments or emergency feedwater, then we care about it. We want to spot that. And we'll attempt to identify those in the IREP survey of the operating plant.

MR. BENDER: Well, I'm not trying to wreck the PAS

effort. I don't even want to pretend that that's the direction I'm aimming, but it seems to me when we're talking about this particular kind of thing, that just talking about this statistic without trying to say what kinds of things we might have to do to evaluate it would be just hiding behind the numbers. In my mind, if we're going to talk about the feedwater trip, we ought to know more about it than just what the statistic is.

MR. EBERSOLE: For a case in point, you might find the motor-driven main feedwater system more reliable than the turbine-driven, because they're not, for instance, dependent on the condensers. On the other hand, they're dependent on the feedwater consate (?) pumps which are motor-driven, therefore, they have an independency on an AC system anyway, but they don't have the contributor from the condenser failures, and a host of other. smaller things, sources of failure, than that.

It would be nice to have a mix of these, but the designers never mix them.

MR. EPLER: There is one aspect of this discussion which I really appreciate if you could clear up.

When we were discussing the Atlas problem with B&W many years ago, it felt that some of the plants were experiencing a complete loss of feedwater as often as once a year, which I thought was rather shocking that they would tolerate

that, through many partial losses of feedwater. B&W, at that time, assured us that complete loss of feedwater would be held to less than one per 30 years, and, therefore, would not be an anticipated transient. Now, frankly, there has been a great deal of confusion about what really happened. What is your notion now of the rate of complete loss of feedwater?

MR. ROWSOME: Well, if you're speaking speaking of --

MR. EPLER: Not auxiliary, just main feedwater.

MR. ROWSOME: Main feedwater is three a year.

MR. EBERSOLE: Three a year, complete loss?

MR. ROWSOME: That's right.

MR. EPLER: Atlas is only interested in main feedwater, because auxiliary feedwater, by law, has got to come out or the show's over.

MR. EBERSOLE: From an Atlas point-of-view --

MR. EPLER: That's what we were talking about, Atlas point-of-view. Once per year, would be pretty high.

CHAIRMAN OKRENT: Your memory is correct, the vendors didn't want to analyze the complete loss of feedwater because they said that was not an anticipated chance and that's a tight show. But that's really not part of today's agenda.

MR. ROWSOME: To return, if you wish, to Congressman Udall's question, I get the feeling what he was asking, really, was: What do these data in this context, what do these

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experiences tell us about the trustworthiness of WASH-1400, and what should he as a Congressman think about the many recommendations to make more extensive use of this assessment technique? And I think that the phrasing of the question about the Rancho Seco and Davis-Besse incidents have led us into thinking too narrowly about the probability issue and not about, what I believe to be, the underlying issue in the Congressman's mind or his staffer's mind about the usefulness of risk-assessment techniques in either predicting or interpreting such incidents. And so I suggested some alternate questions that you might choose to address in the course of responding to the Udall query, such as these: Did WASH-1400 consider or predict accidents of this type? Could WASH-1400 methods have alerted analysts of the possibility of such accidents if the methods had been applied to the affected plant? Would improvements in the methodology or data are needed to properly consider such sequences in risk assessment; and, finally, can WASH-1400 methods serve a useful function in analyzing actual experiences? And I think we can draw some inferences that would help to eliminate the answers to these questions for the Congressman.

Did WASH-1400 predict it? There's obviously a yes and no answer. There's a level in which the answer is yes, in a very abstract level. Yes, WASH-1400 did predict accident classes involving transient cause, stuck-open pressurizer relief

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valves. No the probability was not appropriate to B&W plants because of design differences. If such studies had been done on B&W plants, it's reasonable to infer that a roughly correct probability frequency-of-occurrence for that class of events would have been found.

It is exceedingly unlikely that the reactor safety study or application of its techniques would have unfolded the precise details of the sequence of events in great specificity.

For example, at Davis-Besse the reason the PLRV stuck open was that there was a missing relay in the logic cabinet. The relay served the function to establish a dead-ban between the open and closed set point of that valve, and in the absence of that relay, there was no dead-ban and the valve deterministically chattered between full-open and full-closed until it failed. Deterministically inevitable, given the human error of having left the relay out of its socket. WASH-1400 would not have highlighted that as the causal mechanism, but would have identified the class of events, and given you, roughly, a right ballpark figure for how often to expect it.

In the Rancho Seco case, accidents involving common-mode transients, common-mode failures, that give rise to transients and degrade the reliability of the equipment necessary to respond to that transient, certainly I considered

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in WASH-1400 and studies of that kind, it is improbable that the particular short-circuit and fuse failure would have been picked up, but it is plausible to expect a study like WASH-1400 applied to Rancho Seco would have identified the common dependence of the integrating control system, and through it, the auto-start of the auxiliary feedwater system, the control of the main feedwater system, and the support of many of the instruments on which the operators depend.

The common dependence could, very plausibly, have been identified in such a study and flagged for attention.

CHAIRMAN OKRENT: Could I pursue that a minute?

Do you think that if Rancho Seco had not occurred, that tension, and if TMI-2 had not occurred, and you had asked the vendor or the utility to do a failure modes and effects analysis, using whatever kind of assumptions people doing such analyses say are reasonable to put in, that they would, in fact, have picked up the Rancho Seco failure-mode or they would have said, "That's too many failures, it doesn't come in to the group that we're going to include in our look."

MR. ROWSOME: In a sense, it was a single failure because NNIY is a single power supply, so a failure-mode effect analysis should have identified -- should have explored the consequences of an interruption of power on NNIY. Whether they would have succeeded in anticipating everything that followed therefrom, and they would have done their fault-effects

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analysis well, is another issue.

I think the -- there's a problem with failure-mode effects analysis that goes beyond the fact that it is a single-failure analysis. There's a problem of finding analysts with the wisdom to sit down while they're filling out a table and explore the consequences of a fault through the many systems across the many interfaces into operator behavior and the like. And that we need some technique other than simply a IEEE guide on failure-mode effect analysis to aid the analyst in charting the consequences, and I think it is a part of PAS charter to help develop or objective to help develop such tools.

The way we hope to catch these things IREP is to do a fault-free, not merely on the support system, like auxiliary feedwater, but to do fault-frees on initiating events. When we take a class of events like feedwater trip, we're going to treat like aN essemble with the single probability for the ensemble. And it's necessary for us to flush out those -- that subset of causal mechanisms that do have the common-mode failure potential, and I think the most systematic way I know, with existing tools and procedures, to find those potential common-mode failures is to do a fault-free omni-initiating event, not with the intent of using that to give us a better probability for main feedwater trips, but as a way of identifying in a qualitative sense where there are dependencies in, say, the main feedwater system or the various support sysstems which could induce a feedwater

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trip.

They could also degrade the reliability of the operators or the automatic system to deal with the feedwater trip.

CHAIRMAN OKRENT: Let me see if I understand something.

If we treat the originating Rancho Seco cause as just failure of that buss, and we take the ensuing events as all automatically occurring from failure of this buss, is that an acceptable single failure to the staff?

MR. ROWSOME: I can't speak for NIR, but since it is non-safety grade, they, in principle, ask licensees, ask applicants to take no credit for non-safety grade equipment, but not to specifically analyze every hypothetical failure or combination of premutation failures in non-safety grade equipment.

It's not clear whether the SRP, for example, would want them to hypothesize that. I believe that it would not.

CHAIRMAN OKRENT: What is it you're telling me?

MR. ROWSOME: That the regulation is ambiguous on the point.

MR. MICHELSON: I recollect in the case of Rancho Seco that the power supply failures led to situations wherein control systems were working but the instrumentation system wasn't working, and so control systems were controlling on

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fictitious imput --

MR. ROWSOME: That's true.

MR. MICHELSON: -- which led to a lot of strange things to happen.

Now, certainly, that's something you have to take into account on all non-safety systems as it may relate back in to affect some safety systems. That certainly has to be a legitimate single failure to consider.

MR. ROWSOME: Well, there's a serious flaw in the design concept represented by this incident in that there was no safety-grade system to actuate auxiliary feedwater in this plant or that class of events for loss of main feedwater.

There was a safety-grade actuation system for auxiliary feedwater that was associated with high-pressure ACCS actuation signals, so we've got a safety-grade autostart when the containment pressure went up or when the electrical system pressure went down, but a simple, uncomplicated loss of main feedwater did not produce the safety-grade actuation signals of the auxiliary feedwater system. If they had done so, in the days in which that plant were licensed, auxiliary feedwater was considered an engineering safety feature, then this problem would not have arisen.

MR. MICHELSON: Maybe you missed the significance of my remarks. My concern was not the loss of feedwater, the fact that many of these situation you get feedwater when you don't want it, such as overfill the steam generator,

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fill the main steam line, whatever. You've got to look at that sort of situation unless you're willing to analyze the consequence of it. You either show that the consequence is acceptable or you put in systems that prevent the event from happening.

MR. ROWSOME: That's true, but it's less of a concern to me because I don't see such a direct path in the -- such a high probability path to core damage or coremelt -- as I do from just a total interruption of all feedwater together with no ECCS actuation which is, in fact, what this incident demonstrated was possible.

MR. MICHELSON: Well, this isn't the time to debate that particular point, but I don't agree with it. But I think it's a time to point out that main feedwater systems are just as much a concern if they fail to shut off as when they shut off accidentally. You better look real carefully at both possibilities.

MR. LIPINSKI: More important, in Rancho Seco, in addition to the feedwater system was the indications provided to the operator. They only had control-grade instrumentation. The safety-grade instrumentation was present, was in a locked cabinet outside the control room boundaries. That information was not available to him during the course of the incident. One has to carefully review, what information do you display the operator, and what is its reliability?

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MR. EPLER: There's another aspect of this that needs to be aired. It has been traditional that a control system will failure catastrophically, no matter how, but it will fail catastrophically, your protection system must be able to take care of that. And that its primary mission. Now, it must be said that the consequences of this failure was overcooling, too rapid cooling; that's the only consequence that I found in this. Now, it must be that the consequence of too rapid cooling was not sufficient concern to warrant a protection system to protect against it. That's an assumption.

Or I might assume that it were of sufficient consequence to take measures against it. That I've replaced with a problem of inhibiting rapid cooling. When I start inhibiting rapid cooling, I'm beginning to interfere with the proper function of the protection system, and I'd be afraid to do that.

So, it looks like we're sort of stuck, unless we legislate that that rapid cooldown is no more than a violation of tech specs, and it's not a safety problem. If we can't do that, then we are in a bit of a pickle.

I would like to say at this point that the defense against a control failure, which, in turn, if it occurs too often, will challenge protection too often, is to reduce the frequency of control failure. I think maybe we should address this question as simply a control failure in which we would like to somehow reduce the frequency of its occurrence.

And we don't do this, ordinarily -- we're talking about single failure or redundancy, we attack the problem more directly.

I'm appealing to you to keep, please, separate in your minds these functions of protection and control.

MR. EBERSOLE: Was the design that was deliberately put together, with due consideration of the effects of its failure, or simply it went together in a topsy fashion?

MR. ROWESOME: There's no evidence in the FSAR that a failure analysis for the nonsafety-grade equipment had in fact been done.

Based on my experience with architect engineers and reactor vendors, some thought was probably given to it, but no systematic analysis and very little documentation. It was just what the design engineer felt was the sound design, and there's no rules to the contrary.

MR. EBERSOLE: That you shouldn't do that.

MR. ROWSOME: That's correct.

MR. EBERSOLE: Thank you.

MR. LIPINSKI: What was the fixed branch or sequel? Has that system been modified? or is it still functional?

MR. ROWSOME: I don't believe that a request for modification occurred until the post-TMI bulletins and orders required a safety-grade autostart system for the emergency feed-water system. To this day, the overcooling situation has not been addressed by a order to modify control systems, and the possibility of no feed-water at all of any kind, no cooling of any kind, being a consequence of this

event, was addressed only, only after TMI.

MR. LIPINSKI: Well, the analysis of the particular event concluded that there wasn't any damage to the reactor vessel, because of the prior operating history had not been a known fact. Had this occurred later in life, it's not clear what the results would have been.

MR. ROWSOME: True.

MR. MICHELSON: Could you clear up a point on, on Rancho Seco? Was the, was the problem really the overfilling of the steam generator? or was the problem the lack of additional feedwater?

My recollection was they were quite concerned when they got this one of overfilling the steam generator. And so there was an abundance of water; an abundance of water was their problem, not a lack of water. Is that a good recollection?

MR. ROWSOME: The historical event -- in the historical event the auxiliary feedwater system was started by accident, because the drifting and faulted steam-generator level indication happened by chance to drift into the ravine, which gave a, an autostart signal to the auxiliary feedwater system.

Had that not happened, no cooling system of any kind, primary or safety would have received an actuation signal. And operators would not have had the instrumentation

necessary to tell them that they needed to do something about that.

MR. MICHELSON: Maybe my source of information wasn't right.

MR. ROWSOME: Now when --

MR. MICHELSON: I thought the main feedwater getting incorrect signals was continuing to deliver; and even though the operator didn't know it, he had already filled the convergence --

MR. ROWSOME: That's true, and after the accidental actuation of auxiliary feedwater that did cool, do some cooling, that did alert -- let's see; I've forgotten the exact sequence of events, but it's in one of the handouts here.

The operator noticed auxiliary feedwater system was on, and then manually initiated main feedwater, recognizing by virtue of that discovery that it had been off in the interim, didn't trust his, his steam generator-level indications, as in fact he ought not to have done, but was interested in assuring that the coal was cooled and inadvertently overcooled the system.

The overcooling produced a fallen pressure of the primary cooling system, to the point that the EECS injection set point was reached, which conked back up the primary system and, at the same time, also gave that one safety-grade

autostart signal to the auxfeed system, the second start signal it had received.

The whole thing -- which then added auxiliary feedwater on top of the main feedwater flow that the operator had been delivering which then further amplified the over-cooling trench.

MR. MICHELSON: Yes, that was really the problem, because he could deliver so much water so fast, with main feedwater, wherein the auxiliary feedwater's really relatively slow; so it really was the main feedwater that, as I see it, got him into this difficulty.

MR. ROWSOME: That's as it happened, and it happened that way because accidentally the wandering steam generator level happened to drift. Had it not, you have an equally plausible and perhaps more probable outcome, which is rather worse, which is the one I have been discussing.

Both of them had some elements --

MR. MICHELSON: You mean the loss of all feedwater.

MR. ROWESOME: The loss of all feedwater and no indication that it's necessary to go feed-and-bleed.

(Pause.)

MR. LEWIS: Aren't the details of that scenario in one of these handouts that we have, because I keep forgetting?

MR. ROWESOME: Yes, the thicker of them has an account of it. I belive the staff report also has an

account of it, that highlights some of the features between the two of them, you have a fairly complete picture of what happened.

(Pause.)

I'll very quickly go through these other points and then sit down and let you continue with this.

I thought about what improvements in March 1400 methods and data would be needed to properly treat such sequences in risk assessment.

First of all, what comes out clearly and TMI and the other events is that we do not have in hand now the tools to deal probabilistically with the operator behavior in the face of screwy instruments and ambiguous circumstances. That, I think is the most pressing need if we are to develop a probabilistic risk assessment in the narrow sense of a risk prediction that we could have much faith in.

It would be useful to have a more systematic means to search for common-cause failures. I mentioned the steps we're taking to do that.

It also becomes clear when you look at these B&W plant sequences that partial or brief failures become important, particularly now since anticipatory tricks have been added to B&W plants. If you want to look now at the risk that you might stick a pressurizer relief valve, if, if in fact the emergency feedwater system comes on promptly, it can carry

you through a feedwater trip without tripping the, without opening the pressurizer relief valve.

However, a brief interruption in starting, a brief delay in starting the auxiliary feedwater system, which is negligible with respect to core cooling, but is nevertheless -- gives you a brief interruption in the heatsync -- could open that valve.

So that to do a risk assessment now in B&W plants, one would have to look at brief failures, partial failures, of the auxiliary feedwater system to assess your susceptibility to the TMI scenario, so that we will need techniques to deal at least with, with interrupted function, if not partial failures, to do a better job of risk assessment.

(Pause.)

PROF. KERR: So you conclude that risk assessment is not yet fully prepared to --

MR. ROWSOME: Well, to tell you the truth, I think making absolute predictions in the bottom-line risk is one of the least interesting and least trustworthy applications of the techniques. It would be interesting, of course, if we knew the answer; it would be a very desirable number to know, how safe this industry is.

I don't mean to, to mean to say that isn't interesting.

But where the tools ought to be used with a high

sense of urgency and priority, in my judgment, are to draw essentially qualitative inferences about strengths and weaknessess and systems, is independent way of catching design flaws; it's an independent way of catching loopholes and defensing that, catching oversights or inadequacies in operating procedures, emergency procedures, maintenance techniques, and the like.

That, rather than an absolute risk prediction, I think, is the most valuable application of these, too.

But they're also useful in evaluating operating occurrences. FAA and NASA use fulltree analysis this way. CAMONEE and RAGOVIN used event-tree analysis to organize the "what if" exercise. And risk assessment suggests that the message of Rancho Seco may have been missed, that susceptibility to common-mode main feed trip operator confusion and aux-feed autostart failure --

MR. LIPINSKI: Before you take that up, that operator confusion was only contributed by the fact that he did have safety-grade instrumentation. So operator confusion came about as the second result. But the main lesson he learned from that is to give that operator reliable instrumentation.

Had he had that --

MR. ROWSOME: Another lesson that wasn't learned until PMI and then kind of --

MR. LIPINSKI: I'm sorry.

MR. ROWSOME: Auxiliary feedwater should have a safety-grade autostat.

MR. LIPINSKI: I had read the Rancho Seco report, and I conclude that the operator must be provided proper instrumentation with good reliability.

MR. ROWSOME: No question.

MR. LIPINSKI: Okay.

MR. ROWSOME: No question.

MR. LIPINSKI: But the confusion comes about as a result of having that presented -- that's the part that's missing in your list here.

MR. ROWSOME: This list is a recipe for core melt: one failure leads to core melt. If you interrupt it at any one place, you render it a less serious problem.

Solving operator confusion would be a sufficient, but barely sufficient fix; providing an aux-feed autostart for this scenario would be a sufficient but barely sufficient fix. Doing all these things would be much preferred.

MR. BENDER: Let me ask my question first, if I can.

That's a band you're discussing: one's a minimum, and the other is doing everything.

MR. ROWSOME: Yes.

MR. BENDER: Is there any way to deal with the matter of how much of the band should be applied? Is there any way of drawing a line between the bare minimum fix and doing

everything you can think of?

MR. ROWSOME: There you're opening up --

MR. BENDER: No, I think --

MR. ROWSOME: How safe is safe enough?

MR. BENDER: Because on the criticisms that has come out of the PMI corrective action, from industrial people in particular, is that the staff, in its requirements, is asking for redundant corrections. This redundancy adds something, but I'm not sure how much. And it seems to me we ought to be able to use the probability approach to decide what we're getting out of these various incremental improvements.

MR. ROWSOME: There are two or three barriers between where we are and getting to where you and I would like to see us be. One is establishing concurrence on "how safe is safe enough?" and another is establishing a concurrence on measuring how you stack up against that criterion.

And I guess a third is establishing the data base, both the methodology and the data base --

MR. BENDER: I don't like the first two; maybe the third one is important. There's a matter of a point of diminishing returns has to be looked at.

A does something, B does something.

I don't care "how safe is safe enough?" There's just so far you can go before you run out of values in doing things.

And it seems to me that's one of things that ought to come out of this business.

MR. KERR: Suppose only asks for an incremental risk reduction that can be associated with each change. Is that a goal that's achievable?

MR. ROWSOME: Incremental risk reduction, when you say "risk" in the, in the bottom-line sense, implies you know competing risks; and that opens you up to the whole can of worms.

You can take individual acts and sequences and say, "I want to reduce their frequency of occurrence to a particular value," and then have contstrained the dialogue to a particular set of systems and a particular set of failure modes perhaps, so that you con't have to address the whole huge issues.

MR. KERR: Well, I understood you to say earlier that these techniques were quite useful for comparing two systems, and it strikes me if one can compare two systems by taking an existing system and modifying it by putting on proposed change.

MR. ROWSOME: Yes.

MR. KERR: If one can do this, it seems to me one can indeed, maybe not quantitatively, but at least one can decide whether an emission reduces or increases risk. Even that would be helpful, it would seem to me.

MR. ROWSOME: Well, you can certainly tell about

system reliability. One of the message we're getting from our studies is that system reliability is a tricky measure of risk, though, because accident sequences, scenarios, differ enough from plant to plant that system reliability is, is not a uniform measure that can be applied from plant to plant to relate to risk necessarily.

For example, in a plant which can cool by feed and bleed, as the B&W plants can, auxiliary feedwater is not the only escape route for a total loss of main feedwater. In CE and Westinghouse plants -- or before the Westinghouse plants, where feed and bleed does not appear to be a successful way of cooling the core in the absence of the locant, auxiliary feedwater is your only escape route.

So the reliability of that system means different things in different plants. So one has to be careful in relating system reliability, which is fairly easy to calculate, albeit with some fuzzy-edged uncertainties.

It's a little difficult to transpose that into risk unless one is careful to look accident sequences and not just system reliability.

But, yes, it can be done -- and I think should be done.

(Pause.)

CHAIRMAN OKRENT: Dr. Siess.

DR. SIESS: Frank, your handout has references to a

Figure 1. No Figure 1 in here.

MR. SAUNDERS: He showed it. He showed it to us, though.

MR. ROWSOME: Okay. Well, I'll try to get that to you.

CHAIRMAN OKRENT: Do Iunderstand correctly from a rapid glance at the handout entitled "Evaluation of Davis Besse and Rancho Seco Feedwater Transients" that there was not a bottom line with regard to what the probability of the Rancho Seco Transient was? Is that in here?

MR. ROWSOME: No, there is no probability for it, because -- well, that class of scenarios were considered in March 1400, that mechanism, a nonsafety-related power supply fault was not found, and nothing comparable was done.

I don't know what March 1400 would have done without attempting to do a WASH-1400 on Rancho Seco in the way of estimating a probability for the failure of the NII buss.

So I don't have the numbers for that.

MR. LIPINSKI: You know, I read the letter from Congressman Udall. And he does ask, "Look at the fault trees in WASH-1400." He simply says, "The methodology." And to me, that means that if the fault tree isn't there, I'd develop it for the particular sequence.

And then he says to use the figure rates that existed at that time. In other words, he doesn't ask, "Was

that fault tree in there?" He's saying, "Apply the methodology and go through the calculations, based on the failure rates that were in 1400."

MR. ROWSOME: Well, the number you get is entirely an artifact of how broadly you classify the event; and that he hasn't stipulated. So I can give him any number between zero and one, depending on how broadly you classify the event. I don't think that's a meaningful exercise.

(Pause.)

MR. LIPINSKY: We'd have to discuss in detail as to how you'd proceed to get that broad a range, but in what we'll hear from the presentations by the HRS staff they have bounded the calculation.

MR. ROWSOME: They have made a choice of how broad a class of events they're going to say are representative of that occurrence and have come up with the numbers.

MR. SAUNDERS: Would you expect Congressman Udall to give you the sequence of events he wants you to look at?

MR. ROWSOME: I think he was trying to ask a more meaningful question than one whose answer is an artifact of some assumptions made by the man who is, who's answering.

MR. SAUNDERS: Well, could you, could you, could you just give him any number between zero and one as an answer, see what his response will be?

(Laughter.)

MR. ROWSOME: He'll rephrase the question or get angry or something. I tried to rephrase the question and answer the rephrased question.

MR. LIPINSKI: Well, that's -- that's right. That's -- he's questioning, I think, the adequacy of your phrasing the question.

MR. ROWSOME: No, I'm just -- I think he wants to know, "Can you trust, can you trust the reactor safety study? Can you use it to see these events coming? Or can you use it to make sense out of the events after they have occurred?

And I answered those questions with, but I did not answer the probability question, because I think, viewed literally, that question is, is meaningless.

MR. LIPINSKI: Well, what about total WASH-1400? Because certain sequences were assumed, certain numbers were put in, and results were calculated.

MR. ROWSOME: That was another all-risk assessment. It wasn't a probability for historical occurrence.

MR. LIPINSKY: Well, all he's asking, had you not had this as a historical occurrence, but were given this as one of the cases to be studied in WASH-1400, what would the results have been?

MR. ROWSOME: Well, if somebody wants to give us a budget to go off and do a WASH-1400 on Rancho Seco, I'll give you an answer. But I didn't feel that his query warranted

doing that, or that the ACRS query and PAS warranted doing that.

MR. LIPINSKY: Well, not for the total set of calculations were done, but for the particular event --

MR. ROWSOME: It's not a priori obvious that that common mode failure would certainly have been spotted.

Certainly, the mechanism of the dropped light bulb would not have been spotted.

And I can tell you now, I could get you any number between, oh, 10^{-2} and 10^{-4} with plausible classifications to the breadth of that class of events. Do I take this as the class of events in which both of those current limiting circuit breakers trip? Do I take it as a class of events in which NNIY and only that is failed? Do I take it as a class of events in which either NNIY or NNIX is failed? Do I take it as a class of events of any upset in the integrated control system?

You get wholly different answers.

(Pause.)

MR. EBERSOLE: Your observation on the dropped light bulb forces me to call this to your attention: The illumination system in the control room takes a considerable attention to divide the attachment for the fluorescent or pictures to the top of the control room -- or who does it get qualified?

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However, if aaks the next question: What happens to the bulbs that you stick in them? you'll find that most of them will fall out --

(Laughter.)

And if you have open control boards, you have a compounded problem of dropping light bulbs.

And it's interesting to me finding, and as to the seismic problem. Here was Rancho Seco with a one-drop light bulb, yet we must be faced with an impotent shower of light bulbs.

(Laughter.)

CHAIRMAN OKRENT: Well, I think we'd better go on to the next part of this discussion. I'm not sure whether we'll be able to finish this topic by 12:00 o'clock, but let's see where we get.

I think Dr. Kastenberg is going to summarize these studies that he and --

End 5

T-6:

tape 6
jn-1

MR. KASTENBERG: We took Congressman Udall literally and tried to go through the, quote, unquote, "meeting with analysis," to try to come up with the bottom line number, just to see if we could do it, and in the process tried to learn something. I think we did learn a few things.

Gary is passing out a revised version of a letter which was addressed to Dr. Okrent. I believe you all received a copy of it. There were some ty in it, and those typos have been cleaned up, and there's been an appendix C added to it, which I'll discuss in a few moments.

First of all, for the bottom line number, again, to go through this exercise and see what we can come up with. For Davis-Besse, you recognize some of the numbers that Dr. Ralphson mention: Frequency of feedwater transients, roughly three per year.

We assume that anytime you had success, you have 1^{-p} of failure, where p of failure is a small number, we just let that be one.

One thing we did learn in going through this exercise is that PORD's are often gagged shut when they leak, and Ed Abbott called at the various B&W plants, he found out that half of them were gagged shut, half of them were left open, so rather than having probability of warning for PORD listing, we chose a half because half of them are gagged and they can't open.

jn-2

MR. LIPINSKI: Let me clarify that. Do you mean gagged or blocks all closed?

MR. KASTENBERG: Gagged.

Well, it depends on the plants. Some plants take the -- if you look at the PRB, there's a means of gagging it, like sticking a key into it and turning it down and preventing the disc from lifting off the seam; and the other method of doing it is just shutting the block down. In half the plants, either one or the other was done.

MR. SIESS: And they all have only one PORD?

MR. KASTENBERG: That's correct.

R. LIPINSKI: Because the gagging implies you will not be allowed to operate that opening unless you go down to it and release it.

MR. KASTENBERG: That's right, that's true.

Probability of PORD failing opening, we used $3x10^{-2}$, and you'll note in the handout of Dr. Rostrum, they use $1x10^{-2}$ for WASH-1400.

We were given the $3x10^{-2}$ number by B&W. It's a better number to use, so we used it. So the number is between 1 and $3x10^{-2}$.

Probability of HPIS actuation again, 1 minus a small number, we would just assume is 1.

Probability of the operators will defeat HPIS, so we came up with a number 027. I will give you the rationale for that in a few moments. And then probability that the

realize what's happening and they have to watch the PORD within the required time. At Davis-Besse, they blocked it at around 20 minutes. We come up with a number of 0.999. Again, I'll give you the rationale for that in a few moments.

So, again, if you go through this meaningless exercise, you come up with a frequency for a Davis-Besse type transient, approximately 1.2×10^{-3} for a year.

MR. MICHELSON: Before we leave this question of QRV being gagged, I didn't realize that the plants were using the gag method instead of the block valve. We keep hearing arguments from time-to-time about the degraded conditions we're in. The final heat removal is by means of remotely opening a QRV and boiling the water in the core and passing it out, feeding it enough water to make up.

This tells me that a 50/50 chance that might not work when you need it. I didn't know it was that bad or not.

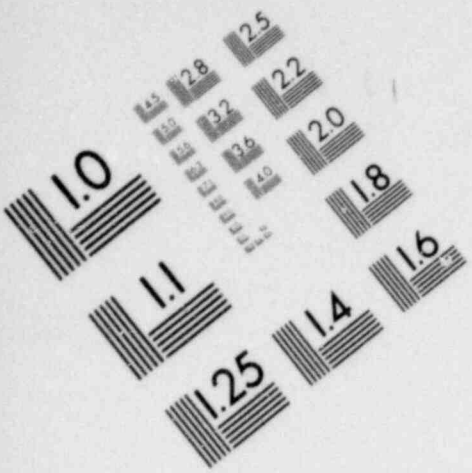
MR. EBERSOLE: Yes, but safety --

MR. MICHELSON: Yes, but, Jess, you remember I want to get the pressure down for some of these -- You can't do anything but sit there and pull and the makeup rates get very low --

MR. EBERSOLE: Maybe we better straighten out a point. Feed bleed is based on -- safety system, not PRV.

MR. ABBOTT: No, it's based on PRV.

MR. MICHELSON: On PRV, generally.



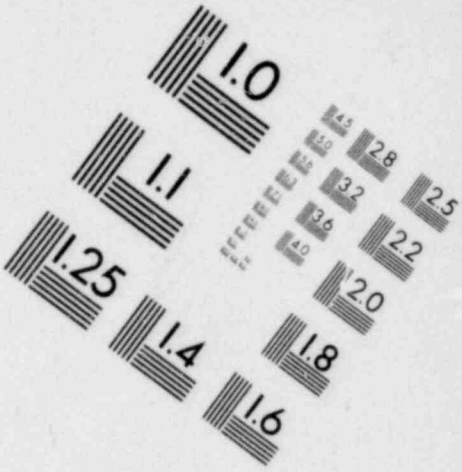
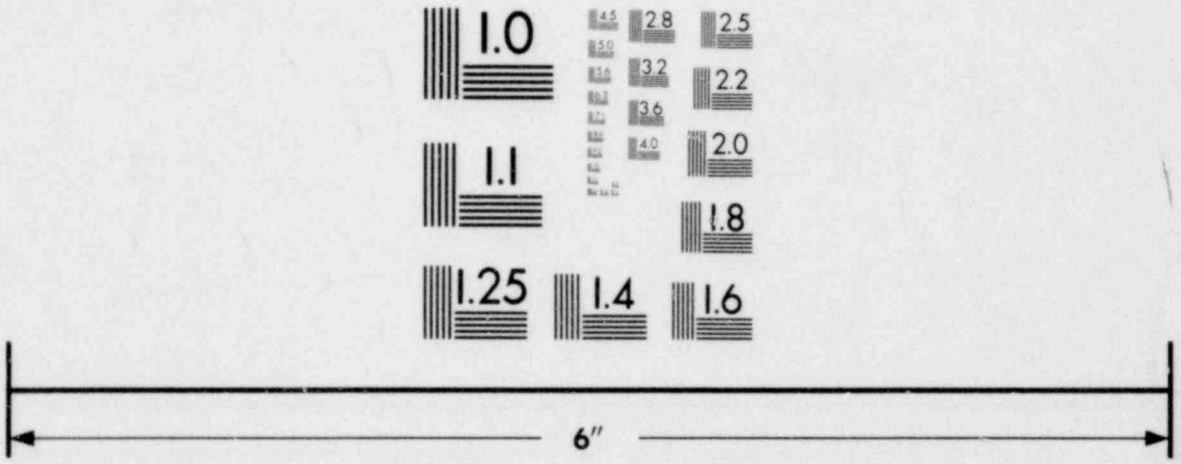
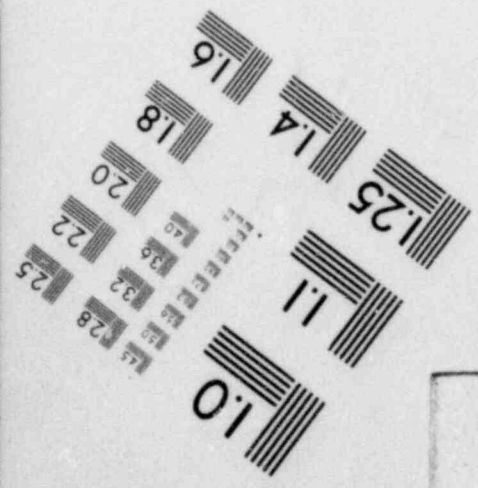
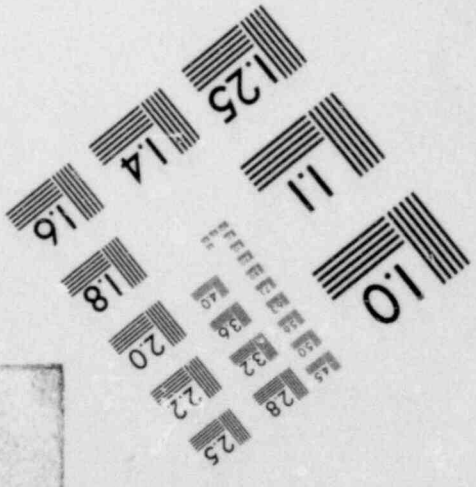


IMAGE EVALUATION
TEST TARGET (MT-3)



MICROCOPY RESOLUTION TEST CHART





MR. EBERSOLE: Feed bleed, unless it's going to lift the safety is no good.

MR. MICHELSON: Some of the plants don't have much of a flow rate if you can't get the pressure down a ways.

MR. EBERSOLE: Well, feed bleed with the present design of PRVs and their block [illegible] belt would not suffice.

MR. MICHELSON: Well, I didn't realize that they were gagged so much they couldn't be remotely opened.

MR. KASTENBERG: I don't want to give you any impression --

MR. MICHELSON: Just wanted to, just want to, before it got away.

MR. KASTENBERG: I don't want to give you the impression that half the valves are, are gagged. Either, either they were gagged or blocked.

MR. EBERSOLE: Okay.

MR. KASTENBERG: We didn't go into the proportion of each -- the set of plants -- as to who gagged them and who blocked them.

MR. SAUNDERS: Rancho Seco was definitely gagged.

MR. MICHELSON: Real important point, though. If they're inactivating the PRVs, then you have to look at the feed and bleed somewhat. Altogether -- and it is set down for all time that -- feed and bleed means through the

PRV?

MR. ROWSOME: We believe so.

MR. VESELY: Yes, that's right.

MR. KASTENBERG: Well. I mean we have to set that down and make it hard. It could mean through the --

CHAIRMAN OKRENT: Well, Mr. Rowsome is the one who told us about the capability of the B&W plants. We'll let him answer the question perhaps.

MR. ROWSOME: I don't have in my head a thermal hydraulic analysis saying whether you can successfully dissipate the kind of heat of feed and bleed through the code safeties or not; I don't know that.

MR. VESELY: You do have one, and that's within the first 2 to 4-1/2 minutes at Three Mile Island. Decay heat was being removed, until the HVI pump was thrown --

(Several speakers.)

MR. MICHELSON: He said "safety."

(Several voices.)

MR. MICHELSON: Some, some -- it depends. This is plant specific of to whether they've got enough makeup to 2,500 pounds to, to do a successful --

MR. EBERSOLE: Well, I certainly think we must clarify what --

Yes, right.

MR. MICHELSON: Be very careful, though -- and

don't -- I am sorry they don't gag the, the PORV.

MR. LIPINSKI: Is it true that all the systems with PORVs --

(Several voices.)

MR. KASTENBERG: I don't know.

MR. KERR: Well, what is the current status of PORVs? Are they safety grade now?

MR. VESELY: They are not, and there's no plan to make them so.

MR. KERR: I don't see how they can take credit for safety functions, which I assume, heat removal is: we use the --

MR. MICHELSON: Well, that's, that's a little bit flaky, all right. But you keep hearing them saying that, that's the way they're going to do it.

MR. KERR: The safety valve is safety grade.

Yes?

MR. MICHELSON: But it only is 2,500 pounds, and you have no way to come down in pressures; you have to be sure you can make up and hold, and eventually answer the question, "Well, how do I eventually get down?" It's a very sensitive question in the case of steam, because if I want to take the heat off the primary and slide down it the way, it would be, it would be very bad to --

MR. EBERSOLE: Carl, I'm going to find that if heat

bleed has been invoked in the safety function, can be dependent on the PORVs and the block valves, then this hoky-poky here that shouldn't have been in the first place, because feed bleed to me intrinsically means the safety valves have to work.

MR. MICHELSON: There, there are number of plants who do not have makeup even at 2,500 pounds.

MR. EBERSOLE: Those are the ones that can't --

MR. MICHELSON: Yes, but there are others who are claiming it who may not have gone through the exercise carefully and they have to --

MR. EBERSOLE: You mean they're invoking non-safety-grade equipment for --

MR. MICHELSON: Right. That's exactly --

MR. EBERSOLE: Oh, well, we'll have to find that out.

MR. KASTENBERG: What may be happening is, the people who are doing the analysis are assuming one set of conditions, and the people who are operating the plant are operating on a different one.

And that -- I think that's what you see here.

MR. MICHELSON: The gag is one thing you don't ever want to fool with for this job. That's only good for doing a hydro or something.

Why don't you go on, Bill?

MR. KASTENBERG: Okay.

Briefly, we, we did the same thing for Three Mile Island; and we get a slightly different bottom line number. Basically, the difference between Davis Besse and Three Mile Island, as we all know, was at the very end. The question comes up as to the operators' recognizing that they have to block the PORV. At Davis Beese they did; at Three Mile Island they didn't. And you get a slightly different number for the probability of that down at the bottom, and you get a, a different frequency for TMI.

And quickly, for Rancho Seco: this is a case where Ed, in his telephone calls, found out that the PORV was definitely gagged in this case. So instead you -- so it cannot open. Instead you have probability of the cold safety valves opening and closing on demand, and they do do that.

And so basically, the number, the bottom-line number that you come up with for Rancho Seco is, as Dr. Rowsome pointed out, how you characterize the initiation of the event. And as, as discussed before, it's very difficult to determine what kind of a frequency you should have for, first, operator dropping a light bulb causing failure of the NNIY; then leading to the feedwater transient. One would have to interpret our number as the general class of loss of NNI, leading to feedwater transients -- the number

we came out with is 8.6×10^{-3} per year, based on some failure rates given in WASH-1400 for solid-state devices.

MR. DITTO: A question on that: apparently, the loss of NNI was the last thing that caused the loss, was the tripping of an undervoltage breaker. There was an undervoltage monitor on the bottom end of the power supply that tripped the breaker that took out NNIY.

Are you telling me that that's likely to happen only once about every hundred years, that you trip a breaker on a particular power buss?

Because it sounds a little bit, a little bit small.

MR. KASTENBERG: Well, again, we interpreted this as a whole family of events, not just the specific event. And this is the difficulty, as discussed before, is how you try and characterize just one event.

MR. DITTO: It appears that if you lose NNIY, you almost certainly have the feedwater transient. Is that not correct?

MR. KASTENBERG: As the reactor was configured on that day, right; that's right.

MR. DITTO: So you're really saying that the probability of losing NNIY is like once per -- 8.6×10^{-3} per year.

MR. LIPINSKI: It's less than that, because it's

summed in with all other events.

(Several voices.)

MR. DITTO: That's right. And it seems like this is an awful small number.

CHAIRMAN OKRENT: Well, can I ask Mr. Ditto: Are you suggesting that the failure rate for such a buss has given the WASH-1400 as much too small?

(Several voices.)

MR. DITTO: I don't think that's part of it. I think that the number here, I think, is much too small, the number I infer from this list, is small, because we know that power supplies that are supplied through breakers occasionally will go. And I think once per hundred years seems like a rather small probability.

CHAIRMAN OKRENT: Now Buck.

Ordinarily, you wouldn't expect to lose that buss, is that right?

MR. DITTO: Ordinarily, but then you shouldn't -- yes.

CHAIRMAN OKRENT: All right. So, in fact it was that breaker failure, coupled with other things, that led to the loss of the buss.

MR. DITTO: I would believe that the breaker itself could, could cause it more often than this. It could cause the rest of the events. Once the breaker fails to

open, then I think you get the event.

MR. LIPINSKI: Now if you translate "once per hundred years," it's 8,760 hours per year. That's so that the breaker has a mean time between failure of one in 800,000 hours. Okay?

CHAIRMAN OKRENT: I need to understand what it is --

MR. LIPINSKI: That initiates the event.

CHAIRMAN OKRENT: Which event?

(Several voices.)

MR. LIPINSKI: The Rancho Seco event.

CHAIRMAN OKRENT: Wait a minute now.

One thing that could be initiated is a loss of feedwater. But that already occurs three times a year. So I don't want to say that that event is 8×10^{-3} per year.

So the question is, what initiates a loss of feedwater, together with a loss of the control instrumentation and so on?

MR. DITTO: All right. The loss of the control and instrumentation comes first, and it causes the feedwater transient --

CHAIMAN OKRENT: All right, now. What are you saying is the probability of the loss of that control instrumentation?

MR. DITTO: I think it is less than this 8 point -- is more than this 8.6×10^{-3}.

CHAIRMAN OKRENT: For all plants? Or for this plant, because of the way it was designed?

MR. LIPINSKI: This plant.

MR. DITTO: I think it's for any plant with this particular configuration, which might be all the, all the B&W plants that have the general arrangement --

CHAIRMAN OKRENT: All right. Now, let me get back to the question, if you were going to look at WASH-1400, and using it arrive at a judgment with regard to the probability of the loss of this particular group of control instrumentation, what would you have arrived at? Without looking at this specific design, and knowing that the, the breaker would lead to all the ensuing things.

R. DITTO: I guess I'll have to beg. I don't know how to answer that particular question, because I don't think you can look at probabilities without looking at the design.

CHAIRMAN OKRENT: But see, what bothers me is (pause) after you look at a specific design and see that it has a, a weak point, you can come up with a high probability for the event, and especially if it's occurred. But even if it doesn't occur, if you look.

On the other hand, if we're trying to look at this as a class of events, there may be a large family of systems that aren't subject to this particular

failure mode, or that -- or before we look, we didn't know -- people just looked, and they arrived at some general estimate that failure of this buss could occur at a certain rate.

And I must confess it's not clear to me that we are being asked to look only at this specific design after we know what it is, to say, "What is the probability for this specific design?"

MR. DITTO: I think, I think that's right. I think that the concern is that when you design a control system, you're obligated to look at the failure modes of that control system in the gross. For example, loss of power through the control system -- and look to see what the consequences are. And, and you have to be able to tolerate those.

As a control-system designer, you might attempt to make the consequences much less; but the probability of loss of power will, I think, almost surely enter your heads as being much higher than once per hundred years.

And so, if a system is susceptible to loss of power, as this one was and as probably many systems are, control systems should do funny things when we take away their power. And this particular one happened to be pretty bad in the sense that it cut off a lot of information to the operator also.

And so, given that set of circumstances, the -- I think it's like Dr. Rowsome said: you have to decide how far back you're going to go to take similarities -- or to see what part of -- what actions you're going to try to predict.

But I think loss of the power supply that is controlling feedwater, among other things, is I think quite likely to give you a loss of feedwater, give you a feedwater transient.

PROF. KERR: You seem to be saying, I think, that you don't trust the 10^{-6} per hour number for failure of solid-state devices. Is that --

MR. ROWESOME: May I interject something here?

There are two power supplies for the NNIY buss. It takes a double failure to, to feed the power supply for that buss, to arrive at a proper predictive WASH-1400 type estimate of the probability of an interruption of frequency, power interruptions on that buss, when we mean to do a fault on it. It is not a single event.

Dr. Kastenberg picked out the nearest number he could find in the tables of event probability which was, I think, a reasonable choice. On the other hand, to literally follow the methodology and data of WASH-1400 for this case, one would need to do a fault tree on that buss, because it has multiple power supplies and it takes a multiple failure

to interrupt power to the buss.

MR. DITTO: But there is a single device that is monitoring the voltage at the output of the power supplies, that goes up and turns both of these on. And if that monitor makes a mistake and says, "It's time to turn off the power supply," this would get both of them.

MR. ROWESOME: The, a full, the short on the buss itself would in fact reduce an overcurrent signal on both of the two overcurrent sensors on the two power supplies. So that is one location where a single failure could in fact give rise to this effect. And one might, in fact, identify that as a design error in the redundancy of the two power supplies in that class of failures -- was not addressed.

There are, however, fuses on all the modes on that buss, so that any fault on the load would require the additional failure or that, as the design was intended, a failure of the fuse as well, to produce that overcurrent situation.

MR. DITTO: I think that we could get this detail present -- but I'd like to look into that, about the fuses. I think there is a question there.

MR. EBERSOLE: Before you get rid of that slide, I think the, the central theme here -- at least, I don't understand -- this transient that we're talking about here is simultaneous loss of main feedwater and off water. Am I

correct? We're talking about the --

(Several voices.)

MR. LIPINSKI: For the first seven minutes.

MR. ROWESOME: Right.

MR. EBERSOLE: But the loss of the aux feedwater was considered to be an important aspect of this. Right?

MR. ROWESOME: It was considered to be so by me and not by anybody else who's commented on it, as near as I can tell. I found that in that incident what appeared to me to be a recipe for core melt staring me in the face, it was averted only by the coincidental drifting of a faulted signal into the set point that caused the actuation of auxiliary feedwater, which then precipitated the overfilling incident.

No one else, to my knowledge, commenting on this incident, has identified that aspect.

MR. EBERSOLE: All right. Then at Davis Besse you had this particular transient identified as three per year.

As -- and on Three Mile you've had it three per year.

Again, this is the same event, I take it. But you're turning around on Rancho Seco, and it jumps down to 8.6×10^{-3}. And I would submit that that's a wrong number, because this is not a contributing aspect of that event, but rather the intrinsic failure frequency of the aux feedwater

pump, coupled with the failure of the main feedwater pump, are the pair of failures that would go back to the three per year that you had for Rancho Seco.

What?

MR. KASTENBERG: You could.

MR. MICHELSON: You could.

MR. DITTO: Sure.

MR. ABBOTT: Sure.

MR. EBERSOLE: So this number at the bottom then is pretty much like the others.

(Several voices.)

Well, I'm saying --

PROF. KERR: There's a logical case to be made for the approach I took, it seems to me.

MR. EBERSOLE: Well, but you can't argue the frequency of the transient here is 8.6×10^{-3} when the over-riding failure function is that in the aux feedwater and the main feedwater subfunctions themselves.

PROF. KERR: That's only if you assume that those all were right.

MR. EBERSOLE: Well, I'm taking that these are the same sorts of machines. I don't know.

MR. KASTENBERG: I have no answer.

Okay, let me just highlight one, one other thing real quickly; and that's on operator failure, because the

question has come up, of how to characterize operator failure in looking at these frequencies. And I think what we, what we came up with is a, an interpretation of what's in WASH-1400. And the data that we used to arrive at these numbers: basically, in WASH-1400 if you read it carefully, you find that the probability of an operator making an error 5 minutes after a large loca is .9 -- seems awfully high. I think he either does it right or wrong. It should be a half, but they give it as .9.

(Laughter.)

If he's totally confused, he could easily do it as right as he could do it wrong.

Thirty minutes after a large loca, they give it as 0.1. And several hours later they give it as 0.01. They give an average error rate under high stress as .2 to .3; and they also tell you that if there is n people involved, you should take p^n. And if -- so basically, what we did then is for throttling the hipsie --

(Audience reaction.)

What's that?

For throttling the hipsie, we assume that that was an average error rate, because it, it occurred several times in these events, over the first half an hour or so. And we assume that there were two operators and a supervisor in the control room; so we took .3 raised to the third power

to give us the error rate of 0.027.

For Davis Besse, failure -- successfully blocking the PORV within the required time -- recall from Dr. Rowsome's slide, Davis Besse was at low power; it was a fresh core. So presumably, the operators had a lot of time to, to determine that they had to block the PORV. So we assigned a probability of .1 that they would do it right, in accordance with this 30-minute number.

And then .1 with three operators.

Then 1 minus that to the success, which they, they were successful.

At TMI they were unsuccessful.

And we chose about 15 minutes as the time that they should have recognized that they had to block the PORV. And in fact, they did not. And the reason we chose 15 minutes -- it's, it may be somewhat arbitrary; but at 15 minutes we felt that the operator had enough indication available to him that it was time to block the PORV and the rupture disc on the quench tank blew at 15 minutes. So we chose that as the time, roughly, that, that he should have blocked the PORV.

MR. MICHELSON: These numbers now are -- we're -- you're, you're assuming that the operator has correct information. And just a question whether he responds to it?

MR. KASTENBERG: Yes. I think we planned that out

in a letter, based on your comments that -- we're assuming these are actual operator errors. In fact, he may have been doing the right thing. If he had the wrong procedure and he's following that procedure, we weren't sure whether you call that an operator error or a design error, quote unquote, if the procedure's wrong.

But we interpret it as a --

MR. MICHELSON: Assume that all the information he's receiving is correct.

MR. KASTENBERG: Right.

MR. MICHELSON: And he just fails to --

MR. KASTENBERG: That's what he -- exactly.

And just one last point. In arriving --

What's that?

MR. LIPINSKI: For the 20 minutes you selected .1 as being next to 30 minutes. But when you went to 15 minutes, then you went to .5. How did you interpolate between the 5 minutes, 30 minutes, ranging from .9 to .1?

MR. KASTENBERG: This way.

MR. LIPINSKI: Straightline? or --

MR. KASTENBERG: No.

MR. LIPINSKI: Inverse curve.

MR. KASTENBERG: Exponential.

And then one last point, which I had not thought of before --

MR. SAUNDERS: Before you take it off, I'd like to just take a big --

MR. KASTENBERG: Okay. Yes.

MR. SAUNDERS: Red variables are certain times the functions on, on normed probability spaces.

I think it's really incorrect to think that every event can be thought of in terms of a random variable. In particular, I don't believe that three people act like three independent guys. They act like sheep, if I understand how people behave.

MR. KASTENBERG: Well, let me show you the next line.

MR. SAUNDERS: All right. Excuse me.

MR. ABBOTT: Yes, I want to get to that point, if I may. You can come back to this.

(Several voices.)

MR. KASTENBERG: I hadn't thought very much of this formulation back in 1975 or '76, when I first saw it in WASH-1400. And it was only until last week, when I guess Ray Fraley kept quizzing me on why we chose the p^n, just as you did. And he spent an hour with me about a week ago. And I sat down, and I sketched this out. This may shed some light on it.

MR. BENDER: Excuse me. You, you all are operating over on the ground rules of WASH-1400.

MR. KASTENBERG: Yes. Yes. Yes. Right.

So this tries to illustrate, I think what it says in WASH-1400, is that: given -- let's take it as an, as an example. Given three people in the control room, and let's let the probability of failure that they do the wrong thing be .1, and you have three people on the control room.

Now what are, what are the three possible, what are the possible combinations of action? All three people can make a mistake; all three people can be successful; two of them can be wrong and one right; and so on. You can look at all possible combinations.

And here's the, the comment you made about people being sheep: in WASH-1400, for failure they only take the top one; that is, all three are wrong.

MR. SAUNDERS: That's right.

MR. KASTENBERG: Any other combination, it's considered correct.

MR. SAUNDERS: Yes.

MR. KASTENBERG: Now, Ed has had a lot of experience working in a control room; and, and he made the point that suppose one of these is the supervisor and the other two are operators; and the two operators say, "Hey, we want to do this," and the supervisor says, "No, we're doing it the other way."

MR. SAUNDERS: That's right.

MR. KASTENBERG: And he's wrong. That would be a failure.

But yet in WASH-1400 it would be considered a success.

MR. SAUNDERS: That's right.

MR. KASTENBERG: And so that's a problem, I think.

MR. ROWESOME: This prescription for estimating human error probabilities is for failure to take actions that are specifically called for in the procedures. It is not for being imaginative enough to realize you need to go to the block panel. It's for -- given that you have a large loca and you have a procedure in front of you for a large loca that says, "You go into recirculation at time T, that none of the three operators would have the presence of mind to do what the procedure says and go into recirculation at time T."

So you're reading into this model rather more than was ever meant to be there in the first place. WASH-1400 methods did not deal with, and gave no credit for, creative operator actions, those not specifically described in the procedures.

So if you do not find in these plans the procedures say, "After a stuck PORV, close the block valve," WASH-1400 would have given it a probability of one.

CHAIRMAN OKRENT: What would it have done, though,

about turning off an HPI? Since that that was not in the procedure.

MR. ROWESOME: Right. It is in the procedure for preventing the water-solid pressurizer, so I would imagine that this is just supposition -- that when Matt and the others went to read the procedures, that they might have picked up on that; but it's, it's, you know, supposition.

CHAIRMAN OKRENT: Okay, well --

MR. EPLER: Let me just, let me just say: when I worked through this, I did not regard them as educated; I used the majority function, two out of three, to see what, see what value that gives. It changes the inclusion quite, quite a bit.

MR. ABBOTT: Oh, sure. This 2-order magnitude difference here.

MR. KASTENBERG: That's right.

MR. EPLER: What's the actual experience when four PORVs start going --

MR. BENDER: Four? or two? Davis Besse and --

(Several voices.)

MR. ABBOTT: So two out of three, they did it right.

(Several voices.)

PROF. KERR: They had an earlier incident.

MR. MICHELSON: Where's the missing relay?

MR. ABBOTT: I thought there were 13 --

(Several voices.)

MR. EPLER: Didn't TMI have a stuck-open PORV before this?

MR. LIPINSKi: Before they were operational, under test. There was a missing relay.

(Several voices.)

MR. EPLER: No, even at TMI, I thought when they did that initial test --

PROF. KERR: Is a stuck PORV a reportable event if you block it immediately?

MR. LEWIS: If I were the operations superintendent, I would say no.

MR. LIPINSKI: I think that's right. I think there's an open question, how many there have been. But I seem, memory niche that there're 13.

MR. LEWIS: The figures we had came from B&W, I believe; not necessarily from the -- I think they, they had so many people.

MR. MICHELSON: But even if it's two out of three, you know, that, that makes WASH-1400 figures pretty far off.

MR. BENDER: The rules of the game don't identify the PORV and its associated block valve as safety features. Isn't the rule really hard just to require reporting of that kind of failure? Is there a possibility that there are many

others we haven't seen?

MR. SIESS: Yes, there is.

(Pause.)

End T-6

CHAIRMAN OKRENT: Well. Let's get the lights.

tape jn-1

CHAIRMAN OKRENT: I'd like to pose the question to the subcommittee and the consultants -- now as to whether in response to the second part of Congressman Udall's letter, we can take something like the following approach that a large number of institutions were asked if they would try to calculate the probability of these two transients. ACRS received no responses in which people actually went through the exercise. This would include the request of all the reactor vendors and their freight, and some foreign groups.

We asked our ACS fellows to see whether they could something in a relatively short time scale using the WASH-1400 methodology with a minimum amount of modification would be necessary and the data, and so forth. Recognizing that there has to be a certain degree of arbitrariness in doing this, and the results we have are those given in the attached memo. We would attach a memo, whatever, in the final form, whatever it is, from our RHS fellows.

MR. SIESS: This was one caveat, that would be using the WASH-1400 data as they would interpret it.

CHAIRMAN OKRENT: Who's the "they"?

MR. SIESS: The people that do this. Which would not necessarily be the same as the way that the people who did WASH-1400 would interpret it.

CHAIRMAN OKRENT: That's right.

MR. SIESS: But if it isn't the same, I guess there's

some criticism of the WASH-1400 report that somebody else can't duplicate the assumptions.

I was assuming that they read the appendices and not just the executive summary.

MR. LEWIS: Should there also be a pedigogical (?) paragraph about how unsophisticated it is to ask for the retrospective probability of a specific sequence of events?

MR. SAUNDERS: I wouldn't use those words, but something like that.

MR. LEWIS: I would normally use nastier words.

MR. SAUNDERS: I know that's true.

CHAIRMAN OKRENT: I'm open-minded at the moment as to how we should frame such a response. Mr. Rowsome has suggested that maybe we should say, "These are the questions that maybe you meant," and respond to them.

At the moment, I am not proposing that we do that in this letter, but --

MR. KERR: I generally agree with your proposed approach. Some of Mr. Rowsome's comments do, however, appeal to me, and if we have time we can do it. It strikes me that we might not quite say that these are the questions you should have asked. You know, perhaps that is what we should say, but rather to say that there are broader question which one might want to explore which have to do with the general usefulness of approach, and then someone could make some comments.

I'm not sure how to word this. It would be

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perhaps somewhere between saying, "Here are the questions you should have asked," and not saying anything.

MR. SIESS: Dave, do we necessarily have to include in our letter Rowsome's approach? Can't we simply transmit his report and a comment or two. I think he made a very good point on not computing the probability, because it compares with finding the event. Now, the significance of that certainly came out in the questions that were being asked about that top probability which is the main one. There's only two that aren't one in there. And is it loss of feedwater or loss of NNIY or whatever, and I think you can seek the one the would come out the answer you wanted. I think there's some advantage in referencing what Frank did.

CHAIRMAN OBRENT: I have a memorandum dated February 6, which is today, from Rowsome to Frailey. It's got "draft" on top. Does that mean anything?

MR. ROWSOME: No, It means that I want to have a shot at rewording it slightly, because I think there's some oversights and limitations. As I reread it, it was too late to get the secretary to retype it last night.

Particularly if you intend to send it on to the Congressman, I'd like to have a shot at phrasing it a little better than I did there, although it's really a matter of fine-tuning the technical language.

CHAIRMAN OKRENT: When would you anticipate we would

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have you --

MR. ROWSOME: A day or two. I know now pretty much how I'd want to change it.

CHAIRMAN OKRENT: I would suggest a day is better than two.

[Laughter.]

CHAIRMAN OKRENT: Like, in fact, could I say within 24-hours, because I think this is on the agenda tomorrow afternoon.

MR. ROWSOME: I tell you what, I'll pencil in the changes I want --

CHAIRMAN OKRENT: That's good enough.

MR. ROWSOME: -- and I'll get my boss, who's sitting here, to concur, and with any luck, I can give you the pencil version of the corrected draft before I leave here this afternoon.

CHAIRMAN OKRENT: That would meet my criteria.

Thank you.

What's been suggested is that we consider forwarding, with our letter to Congressman Udall, the memorandum and I assume also the little document entitled, "Evaluation of Davis-Besse and Rancho Seco Feedwater Transients," that the staff supplied to us, and making a note that we received these, and whatever.

Well, are there any other suggstions that people want to make with regard to responding Congressman Udall's

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letter, at the moment. There are other questions that have arisen during the morning, but I'd like to get back part of it in hand.

Any other suggestions?

Carl?

MR. MICHELSON: I don't have a suggestion, but just some question in my mind after hearing all of this. The answer that I'd like to have if I were writing him, would be: Do we think that the WASH-1400 numbers are lower than they should be or what? Now, some of these results that we see indicate that maybe we're off a factor of 10 to 100.

CHAIRMAN OKRENT: Are you referring now to the data --

MR. MICHELSON: Right. The WASH-1400 predictions are apparently somewhat lower than we might think they should be from the limited experience that was examined, but you can't draw hard conclusions, but you can draw general conclusions. Does the trend seem to be higher or about the same. Well, I hear, you know, both sides this morning, I think.

MR. ROWSOME: Well, we're closer to addressing that, but we have also been doing a couple of other studies which I believe you've heard about, The Auxiliary Feedwater Study and Survey using plant-specific data to plants' susceptibility to core damage through station blackout. Both Bill's data suggest that plant-to-plant variation could result in higher risks from 100 plants than WASH-1400 would lead you to believe,

hb6

although we have no clear indication that the analysis of Surry and Peach Bottom, per se, were -- would be revised substantially by the data that is now in hand. But both the issue of the systems, difference in systems, and difference in data from plant-to-plant lead us to be much less confident than we were when WASH-1400 was written. But you can extrapolate from Surry and Peach Bottom, use generic data, and come up with a good measure of risk in the whole industry. And the pointers that we see both in the data and the system differences from plant-to-plant indicate that other plants may be higher than that extrapolation would lead you to believe.

MR. SIESS: I think that Surry and Peach Bottom are not necessarily a mean or medium plant.

MR. MICHELSON: I think that is significant to report in somewhat generalized words. That's the impression I got in the substance of the whole discussion this morning. We are probably underestimating the risks.

CHAIRMAN OKRENT: If I can comment on your question, which is certainly a good one, although it is not specifically raised in Congressman Udall's letter, my own feeling is that it is not only a question of data, but a question differing systems and, in fact, things that were not included in WASH-1400 at all; for Surry and Peach Bottom, and I guess my own opinion for a long time, in fact, and it begins back when WASH-1400 came out, is that if I were going to have a

best bet, it would be a larger probability than they gave.

I am going, in fact, suggest to the committee that we address that topic, not in our response to Congressman Udall, per se, but when we respond to Commissioner Gilinsky because we do have that on the table for trying to complete next month than to get comments this month, but I think, in that regard, it would -- something needs to be said there, so I'm not proposing deferring the question indefiritely, but to take it up on that forum if the committee's so inclined.

MR. MICHELSON: I agree.

MR. ROWSOME: One brief clarification and I'll relinquish the book. I indicated that we believe the frequency of core damage events for the industry as a whole is probably higher than what WASH-1400 suggested. That does not imply that the risk is necessarily higher because there is also a large body of evidence that we were unduly pessimistic about failure criteria for systems and the magnitude of releases and the consequence models. There's a good deal of evidence that there are compensatory conservatisms on the consequence end. I do not know and do not have the instinctive feel about whether the overall risks predicted in WASH-1400 are high or low in the industry.

I do think the frequency is too low and the consequence per event is too high.

CHAIRMAN OKRENT: Could I ask where I could find written in some detail the studies that you think would

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give backing to your feeling that the consequences are over-estimated in WASH-1400?

MR. ROWSOME: That isn't well-documented now. I am in the process of outlining a paper that will summarize them, but not document them in detail.

Battelle is in the process of modifying the March corel code to reflect what we have learned to -- thus far. We know cores, for example, will take a lot more abuse than we use to think before they actually melt. We've got new data on the volitility of very efficient fragments, and the like. But it is not written down. I can't identify a body of literature to refer to there, but I do want to document it and get it out in the open, so others, like yourself can view it and contribute to the effort in thinking of such things.

It is my impression from what I've read that I don't, myself, calculate consequences; that there is some who question the evacuation model, although it is used in WASH-1400 in a way that the consequences could be a little larger. Clearly, there is still some who say you should use milinear effect rather than the less-than-linear, although there's opinions on both sides. And there are one or two other things of that sort which tend to go in the direction of somewhat higher -- and I didn't know whether you had some argument that countermanded these enough to swing the whole thing clearly

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to lesser consequences given a accident which gets the core-melt. If you're going to say it doesn't get the coremelt, that's another story. I don't want to bring that part into the discussion.

MR. BENDER: Where does containment integrity fit into all this?

MR. ROWSOME: That's a pretty difficult question. You mean in terms of my judgment, the consequences are less severe?

MR. BENDER: Yes.

I'm reflecting now on TMI in a certain sense and the probability that containment might not be effective the next time the same way it was at TMI.

MR. ROWSOME: Yes.

MR. BENDER: Does that factor enter into your thinking?

MR. ROWSOME: Well, certainly, in WASH-1400, and in the studies we intended to do in the future, there is a finite probability considering that the containment was not, is not, isolated, but has a large bypass leakage. A lot of effort has gone into looking at accident sequences that caused that, even if -- contain the bypass -- But there's also background probability. I've forgotten now if it was one in ten or one in a hundred, but the containment simply had an open valve, which clearly would be nonconservative in the

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plant. That has a history that -- one in a hundred, maybe -- Something we should look into.

CHAIRMAN OKRENT: If it's -- Mr. Epler?

MR. EPLER: I I understood correctly, we may have here in the presence of an extremely important point.

Frank, you said a while ago, if I understood it, that it's just by a flute that we had a given sequence, that you might have been in the presence of a coremelt. Now, if I understood that correctly, it may be that we have to consider that here is a core failure, savvy, a very high probability, for which we have no defense. Is that your belief?

MR. ROWSOME: Given the NNIY failure, there would have been no -- we lost and would have inevitably have lost main feedwaters. We only, by coincidence, got auxiliary feedwater and would not have gotten a lot of those dark signals to ECPS. The operators were flying partially blind because of the failure of much of their instrumentation, nonessential instrumentation, on things that I believe should have been regarded as essential.

Under that circumstance, I think it's pretty likely the operators would have actuated some form of core cooling and would have taken some action, even blind, that led to some form of cooling.

Nevertheless, we were on pretty thin ice. Under those circumstance, I believe that to be true, yes.

MR. EPLER: As I interpret what you just said, we do not necessarily have protection against this defect, but we do trust blind operator action. I think we may be -- it's something we ought to look into.

MR. ROWSOME: As I said before, I think the principal problem was one of never treating auxiliary feedwater systems until very recently as an engineered safety feature. Plants, PWR's have been built for years and years in designs in which auxiliary feedwater is the only system that could mitigate a -- could prevent a coremelt given a main feedwater tread. We're equipped with no autostart, safety grade or otherwise, for the auxiliary features.

MR. EPLER: We have seen at TMI and also Arkansas that auxiliary feedwater which is not only redundant, but worse, and is frequently tested. There's no provisions to make sure that it isn't turned off and left off when it's tested.

I guess we haven't been looking at some of these things as carefully as we might.

MR. ROWSOME: I quite concur.

Well, one more point before we leave. In an earlier discussion we talked around the problem, that left me just a little bit concerned. It's true that the operator has no information, but he did have information in another room, another cabinet, that could have taken care of everything. The safety instrumentation. Now, I felt that we were being

tempted to offer to make that available to the operator, please don't, because as soon as we do that, we are over the hill and making the situation worse rather than better. When the operator starts to using safety instrumentation to run his plant, you've got a serious problem.

Was I correct in believing that we were on the verge of proposing that we use safety instrumentation for operation?

MR. LIPINSKI: Let me ask the following question. Do you want to block out all of the nuclear indication that is currently in the control room?

MR. EPLER: I've been tempted to, from time-to-time.

MR. LIPINSKI: Every plant has it, your nuclear instrumentation displayed on those control panels.

MR. EPLER: It is necessary that instrumentation be given surveillance. It is highly essential. But it is also essential that it not be used to control plant.

MR. KERR: I think it would satisfy Mr. Epler if you didn't call it safety grade but made it safety grade. What he wants to do is preserve independence between safety systems and non-safety systems. Isn't that the point you're making?

MR. EPLER: Yes.

MR. KERR: He's not opposed to quality standards being used, he just doesn't want to call a safety system.

MR. LIPINSKI: Now, the philosophy that exists is

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to whether you can extend the indicators from the safety system to the control panels, not have failures in the indicators -- backwards to compromise the safety systems, isn't that your --

MR. EPLER: No, not at all.

MR. LIPINSKI: You just don't have to have the information?

MR. EPLER: I don't want him to use the information to control plant because when that information goes off and the protection system is unable to protect, we can be sure the operator will demand protection, and this we would like not to happen.

MR. LIPINSKI: Well, then you are going to require a completely second, redundant set of highly reliable indicators?

MR. EPLER: Yes.

We need to improve the control instrumentation, please include that, and don't degrade the protection system.

R. EBERSOLE: Have all these studies relating to these particular matters given you any new insight as to what might happen if you had a total DC power failure or some integral thing?

MR. ROWSOME: I'm not aware of any nuclear plant in the country that could survive a total failure of DC power.

CHAIRMAN OKRENT: We may have time to get back to this very interesting topic which, in fact, is in a sense more important than the response to the narrow question -- in fact, Carl Michelson was bothering me for months to try to get real

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of the Rancho Seco transient as part of our TMI-2 implication subcommittee meeting. We did have it on the agenda a few times, and the staff always said they were too busy to analyze it and come in and talk about it. That's, I think, a fair accounting of the history, this time. In fact, Mr. Alpine has come in and given some insight to at least one member of the staff who found time.

I am going to propose we take a five-minute break, and then start the next part of the agenda, which will be at the beginning of discussion of a list of nuclear and non-nuclear energy systems.

We will have one presentation and then break for lunch.

[Short recess.]

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CHAIRMAN OKRENT: We are going to the next part of the agenda, and our first presentation will be by Mr. Gotche who will present some information based on some studies they've been doing on comparative risks from coal or nuclear or the methodology thereof, I'm not quite sure.

Mr. Gotche.

MR. GOTCHE: We -- when I say "we," SAI and I and the project manager for the SAI study talked about this, it kind of caught us by stride. Since the Conaye (?) Study which Dr. Ogrin worked on, it's just came out about that same time, SAI was supposed to be up here to tell you what

they did, and they got snowed in at Oak Ridge.

I have very briefly reviewed the work that SAI did. There is no bottom line in the SAI work because its primarily just the development of a large metric system for interfacing a lot of different models to come up with the bottom line. They had hoped to get something to do that, but they don't at this point. The SAI simply quotes what other people have done.

The SAI work was done -- interesting -- was started by research without the request from NRI. It was received by Tony Vue (?), I believe, Sol Levine, as part of a study to develop acceptable risk criteria, and Dr. Slovak, who is working on the acceptable risk part is also here today.

There was another study which was funded independently by NRR through DSC, not through our shop, but through Mel Erts' shop, and it was coordinated with the work at SAI, and they did come up with some bottom lines in that. This was done by Technichron.

That study, I guess I had some problems with, as it turned out right at the end, and it was too late to do anything about it because I didn't have any more money. They were supposed to develop something comparable to the table s-3 intensia part 51, only dealing with the coal fuel cycle. What happened was, they took the source terms -- and they calculated downwind concentrations using simple -- and, lo and behold with a large stack of typical coal-fired plants, none of the

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concentrations ever approached the level which would have induced an effect in the population exposed, based on laboratory studies of individual pollutants. Unfortunately, that's not the way the real world works, and there are quite often interacting effects. For example, instead of suspicion, there's an interacting effect to clean sulphured oxide and ozone, these were not factored into a study nor were the considerations that which one of these plants will provide electricity for maybe a million people? There's another one maybe 50 miles away, and you have a problem, overlapping clumps. And you look in a regional basis, which is the way they should have done it, and divided by the number of gigowatts electric in that region, they could have -- I think more honestly, normalized to a gigowatt year of electric production, but they didn't. So that's a weakness in that study in the coal part.

I have looked at parts of the Kemeny study. I haven't had time to read it all. I didn't know how many of you had seen it besides Dr. Ogrin, but it's a thick bugger.

From the nuclear fuel cycle part of this thing, it turns out that if there was some work done, and I think there was some original work done -- if it was done, it wasn't used in here. What is in the new Conaye's report of an adaptation of a table from the report which came out last fall from the National Academy of Science, this headed by Conyer Herring, and they had simply adopted what we had in Gizmo in 1976, so two and a half years ago when I read the ACRS on where we were

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We were then ahead of where Conaye is now, because we made some corrections, and Gaismo that came out during the hearing, which was not picked up by the authors of the Conaye study, or the Committee on Social and Public Policy last fall.

So, what I'd like to show you here is a comparison. The good news here is that it doesn't make a lot of difference. I guess that's the bottom line here. But this is kind of where they're at now. The cause -- is Committee on Social and Public Policy, and that was supposed to be just a critical review of the literature, and indeed it was. They did some synthesis themselves, but not a lot.

Some of the major differences in occupation, and really there is only one, right here down the reactor operations, they didn't like the number we came up with in Gizmo, and they more than doubled it, and I haven't found out how or why they did it yet.

CHAIRMAN OKRENT: What's the unit by the way.

MR. GOTCHE: These are those commitments in that first -- a collective dose -- for a 50-mile population. Well, it's further than that. Well, all of them use them -- calculated dose out to a couple thousand miles based on a linear, a straight projectory.

CHAIRMAN OKRENT: That's a thousand years.

MR. GOTCHE: Okay, that's a good question. In Gizmo, the analyses for the general public were limited to 40-year

CHAIRMAN OKRENT: We'll go into the next part of the agenda. And the first presentation will be by Mr. Gotchy, who will present some information based on some studies they've been doing on comparative risks from cold nuclear or the methodology thereof -- I'm not quite sure.

Mr. Gotchy.

MR. GOTCHY: We -- when I say "we," SAI and I and the project manager for the SAI study -- talked about this. It kind of caught us by surprise, since the study which Dr. Okrent worked on came out about that same time, SAI was supposed to be up here to tell you what they did, and they got snowed in at Oak Ridge.

I have very briefly reviewed the work that SAI did, it's -- there is no bottom line in the SAI work, because it's primarily just the development of a large metric for interfacing a lot of different models that come up with the bottom line.

They had hoped to get funding to do that, but they don't at this point. If you, the SAI work simply quotes basically what other people have done.

The SAI work was done -- rather interesting -- was started by, by research, without the request from NRR. It was conceived by Tony Buell, I believe, and Sol Levine, as part of a study to develop acceptable risk criteria -- and, and Dr. Slovik, who is working on the acceptable risk part,

is also here today.

There was another study, which was funded independently by NRR through DSC, not through our shop but through Mel Ert's shop; and it was coordinated with, with their work at SAI. And they did come up with some bottom lines in that. This was done by Techticron.

That study, I guess I had some problems with. As it turned out, right at the end; and it was too late to do anything about it, because they didn't have any more money. They had calculated it -- they were supposed to develop something comparable to the Table S-3 and 10-C of our Part 51, only dealing with the cold-field cycle.

What happened was, they took the source terms or cold-fire claim; and they calculated downwind concentrations using simple Gaussian dispersion. And lo and behold, with a large-stack typical coal-fired plants, none of the concentrations ever approached the level which would have induced an effect in the population exposed, based on laboratory studies of individual pollutants.

Unfortunately, that's not the way the real world works. And there are quite often interacting effects. For example, as suspicion, there's an interacting effect between sulfur dioxide and ozone. These were not factored into the study, nor were the considerations that since when these plants will provide electricity for maybe a million people,

there's another one maybe 50 miles away, and you have a problem of overlapping plumes.

And look in a regional basis, which is the way they should have done it, and then divide it by the number of gigowatts electric in that region -- they could have, I think, more honestly normalized to a gigowatt-year of electric production. But they didn't.

So that's a weakness in that study in the coal part.

I have looked at parts of the Kemeny study. I haven't had time to read it all, you know. It's -- I don't know how many of you have seen it besides Dr. Okrent. But it's a thick bugger.

And on the nuclear fuel cycle part of this thing, it turns out that if there was some work done -- and I think there was some original work done. If it was done, it wasn't done in here. Now what is in the new Kemeny report is an adaptation of a table from the report which came out last fall from the National Academy of Sciences, headed by Conyer Herring. And they had simply adopted what we had in Gesmo in 1976.

So two and a half years ago, when I briefed the ACRS on where we were then, we were then ahead of where Kemeny's is now, because we made some corrections in Gesmo that came out during the hearing, which was not picked up by

the authors of the Kemeny study or the Committee on Social and Public Policy last fall.

So what I could -- what I'd like to show you here is a comparison. The good news is, it doesn't make a lot of difference. I guess that's the bottom line, I guess.

But this is kind of where they're at now. COSCOPP is Committee on Social and Public Policy. And that was supposed to be just a critical review of the literature; and indeed it was. They did some synthesis themselves, but not a lot.

The major -- some of the major differences in occupation, really there's only one, right here down to reactor operations. They didn't like the number we came up in Gesmo; and they more than doubled it, and I haven't found out how or why they did it yet.

CHAIRMAN OKRENT: What's the unit, by the way?

MR. GROTCHY: I'm sorry?

CHAIRMAN OKRENT: What's the unit?

MR. GOTCHY: These are, these are those commitments in Perseram, Perseram, their collective dose. In other words, it's for a 50-mile population. Well, I believe -- no, it's further than that. That's right. Gesmo -- well, all of them used them all, which calculated dose out to a couple thousand miles, based on linear -- a straight trajectory.

MR. MARK: How many thousand years?

MR. GROTCHY: I'm sorry?

MR. MARK: How many thousand years?

MR. GROTCHY: Okay, that's a good question. In Gesmo the analyses for the general public were limited to 40-year environmental-dose commitments.

And so that is the limitation here also. You can see these numbers here are, are generally speaking to the rounded numbers. The bottom line is, is the same. This is without reprocessing.

And this is with reprocessing.

This number here, if you add the column up, comes to 1,200, the same as the number in Gesmo. So I think it's probably just a typographical error.

Since we -- since the staff did Gesmo, we found an error in ICRP-2. It had been sitting there for 20 years, and no one had caught it. It was an error -- or the dose conversion factor for lead 210 which most of you probably realize is one of the daughters of radon 222, coming from from mining and milling.

It grows in during transport and deposits on the ground and adds up in food pathways. And then Gesmo turned out to the be dominant source of exposure for the population in the United States.

Well, it turned out not to be quite true, because

there was a factor of 10 error on the high side in ICPR-2; and, and the reason I guess they haven't been caught is because it was never -- they never looked at the total bodies, the critical organ, for lead 210. And nobody had ever used it, until we did; and we used it for calculating dose conversion factor for the Gesmo analysis.

And so these numbers up here turn out to be about a factor of about 5 or 6 higher than they should have been. The number that I gave two and a half years ago here is the corrected number.

After -- I guess since I last briefed you on this, we went through this radon, series of radon hearings, starting with Perkins. And we did some other corrections, too. We looked at, at that point, since radon is a continuing emanation problem, we did 100-year environmental loss commitments rather than the 40 years. But we also extended the time, which represents this range, from a period of 100 years to 1,000 years.

And this was based on a fairly -- the higher number was based on a fairly pessimistic failure rate for mill tailings. I think that time -- and I, I can't give it to you yet; they're in the process of developing it -- we've also realized that there's a continuing component from mining, particularly strip mining, if it's not reclaimed. And even reclaimed mines may have above-average emanation

rate for that particular region.

So that will be added. It doesn't change the bottom number a great deal, but it will increase it somewhat.

CHAIRMAN OKRENT: I'm sorry. Is the 1,750 number that you give per 100? or per 1,000 years?

MR. GROTCHY: A thousand years.

And the new numbers that they, that NMSS came out with after a couple of years of study by Brookhaven and Argon and, and the staff don't differ -- well, when you get to the bottom line in health effects and you recognize that the health effects are directly proportional to the dose commitments -- and I haven't done this for any other -- just the whole body --

It's really quite critical, by the way, for a lung for radon, because those numbers are much larger than Gesmo, because in Gesmo we had calculated it, a uniform lung dose, when in fact the critical organ there, the target organ's the bronchial epithelium.

At any rate, there were some effects they did which would increase the impact and others which decreased it. And the bottom line came out within a factor of 2, which pleased me a great deal, because we weren't sure at the time we did this how we were going to come out after they had spent a million dollars at a couple of labs, redoing this thing.

The numbers over here -- if you corrected the analysis, it would go from 600 down to 80; that's for a 40-year environmental dose commitment. And from 120 down to 20 here. I think this might be a -- this is an error. That should be a hundred and -- no, I guess that is right: 80.

If you take and add these down through here, you get down to -- it goes from 800 down to 190.

PROF. KERR: The 80 and the 20 are for 40 years?

MR. GROTCHY: Yes. That's what was done in Gesmo in -- the number over here represents 100 years, on the lower end; so it's bigger than that.

PROF. KERR: Earlier, you said, "out to how many miles," and I missed the number.

MR. GROTCHY: It takes it all the way across the United States. The, the model that was used for mining and milling, for example, it's on the order of 2,500 miles.

For some of the other plants, it's like 1,000 or 1,500 miles.

MR. MARK: Why are the correction factors different: 600 going to 80 and 120 going to 20?

(Pause.)

MR. GROTCHY: That's a good question. I'm not sure. At that point. I haven't had much time. I've been in a class all this week, and I'm writing two new regs right now. This was, this is an, this is preliminary stuff. I

have not had time to go back and go through this again, but I think they're correct.

I don't know -- I don't recall right off the top of my head, since they come from about the same locations, the mines and mills are located quite close together and have this almost the same trajectory and same populations.

MR. MARK: It could be that the lead 210 is a different weight, I suppose.

MR. GOTCHY: I'm not sure. It doesn't look right to me either. That's why I thought perhaps that 80 was wrong, but that is what I came up with.

Well, the bottom lines in these things -- and I didn't have time to add this -- when you get to the health effects numbers, where in new reg we had, we considered with reprocessing in new reg 0332, since it tended to upper bound the impact for the fuel cycle, it would be conservative.

And that comes out to be something about, on the order of a tenth of a health effect: .11. The Gesmo-Kemeny's and cost-buck would have come out about 40 percent higher than that.

And the, without reprocessing, it doesn't change significantly. You would have gone from about .05 per gigowatt your electric. For Kemeny's it would have been on the order of a tenth.

So I guess what I'm saying is, certainly at the

lower end if, if you go to a thousand years, the new reg upper-bound number approaches four-tenths of a, of a mortality per gigowatt-year electric.

But the numbers are not a great deal different than what I gave you two and a half years ago.

Now, this is nuclear. In the case of coal, it is obvious the Kemeny's committee had some real gas pains.

(Laughter.)

MR. GOTCHY: And there aren't very many bottom lines in there. There are some for occupational type of exposures. But even those are very carefully worded to say that "well, we expect they might be lower." This is true for transportation also.

MR. MARK: Coal?

MR. GOTCHY: This was the painful part. I don't think Lester Lee would agree with this. But it says, "The analysis establishing sulfate at prevailing levels as an important determinant of mortality has been rejected."

That's pretty strong words. I don't know what we're going to do with this new reg 0332 at this point. We had been using more coal fuel cycle, the work by Leonard Hamilton's group at Brookhaven. That was recommended to us by, by ERDA when, when we went to them and said, "We are not the coal regulatory commission, and we would like to know what you think we ought to use to fairly represent coal in

this comparison."

I understand the numbers, the health effects numbers, have declined, or would decline significantly, from what was in new reg 0332. We had a range there of something like 10 to 140, I believe, per gigowatt-year electric. This would be mortality. And the new numbers would be definitely down toward the low end of that range.

And with the uncertainty involved in both the analyses, it appears at this point that you can't say that one is worse than the other or one is better than the other, because with the uncertainty bounds they overlap significantly.

That's all I got. I guess I could ask you if you had any questions.

MR. BENDER: When it comes to the coal cycle, it seemed to me that report you are quoting from makes a number of points about improvements in mining practices and things of that sort as a, as a condition for, for lowering the mortality rates.

Is there a way of, of, of bounding that? It doesn't seem to me that we, we're going to be able to get an absolute number; but there may be one, some way of saying, "It could be this low." Or "it can't be lower" than a certain amount, no matter what. Is there any way of doing that?

MR. GOTCHY: Well, I don't know. We're awaiting -- this is not the whole Kemeny's report. This is more like conclusions. The, the --

MR. BENDER: I'm not, I'm not asking you to get it out of that report; you won't find it in there.

MR. GOTCHY: That's right.

MR. BENDER: The question I want to ask is, Is there any other avenue we can go to, to get --

MR. GROTCHY: To bound it?

MR. BENDER: To bound it.

MR. GOTCHY: Well, there are ways of doing it; but they involve a lot of assumptions which are very hard to support. We have a computer code called DEMPAC which we funded through NRR, being developed at Argonne, which has both the Winkelstein and the Lavin-Susskind model, health effects models, in it. And these were the classical studies which most people had been using, and which was modified by Brookhaven for their work on Kemeny's. I know Leonard Hamilton was involved in this thing, and, and I know that their models are essentially the same models that we have used when we wrote new reg 0332, although they have been modified, I believe, to reflect best available control technology now, rather than new sorts of performance standard for coal.

We use the new source performance standards.

At that time it wasn't sure that there was going to be best available control technology for coal, say, by 1985.

I don't know how to bound them. Well, I'm sorry. That computer code has a Monte Carlo technique in there for calculating bounds. But again, the bounds are based on the assumption that the model is correct.

MR. BENDER: Well, let, let me take a for instance: if I could assume that all the black lung effect, health effect, could be eliminated, could I get something, some answers that came closer to looking like what you've got here? Or would I have to do something more than that?

MR. GOTCHY: Well, you'd have to do something more than that. Let me say that black lung disease, even though they talk about coal workers' hemocorneosis, I went to a symposium last year in Utah, sponsored by NAAJ, where two physicians got up and said that at least 80 to 90 percent of all coal workers' pneumocorneosis currently being paid for by the Government as a disability related to coalmining was from smoking cigarettes.

So. The other problem is that since they introduced more mechanization in underground coalmining, they increased the dust floating in the air, rather than decreased it. There's a lot more dust in the air when you're running with machines than when you're running with pick and --

PROF. KERR: Excuse me. I don't understand the significance of your statement about two physicians getting up and saying something. I've heard physicians get up and say things --

MR. GOTCHY: Well, they've done studies of coal workers.

PROF. KERR: But have those studies had any review or concurrence or --

MR. GOTCHY: There was no radiologic, I mean x-ray, indication that those guys had ever been coal miners. I mean, they, their lungs were black-filled with coal dust. And, but they, and they were smokers.

And these guys had looked at several thousand coal miners and came to the conclusion that most of the CWP, at least being called CWP and being paid for today by the public, is from smoking cigarettes and not mining coal.

MR. BENDER: But that's like uranium mining. You can also say the same thing, that most of the effects can be attributed to smoking; and that masks all the other effects.

MR. MARK: Is that a conclusion to the --

MR. GOTCHY: That's not quite, that's not quite true.

MR. WILSON: -- is actually closer to the asbestos case, where most of the people get lung cancer

from asbestos, the smokers. But it's definitely a, a syllogism there that studies of, the recent studies have shown that the CWP are indeed, most of them, smokers. But that doesn't mean to say that it is not also coal that's affecting it, just as with the asbestos case, asbestos affects what the workers have, as well as the cigarette.

MR. GOTCHY: The United States position has been that uranium miners smoking and working uranium mines are synergistic also, although the Swedes don't agree with that. They can't find any indications that --

MR. BENDER: Well, I'm not going to argue about whether it --

(Laughter.)

There's obviously a number of viewpoints, and it's just a matter of how you can get some perspective on it. Never mind about whether it's exactly right or not. I don't think you can determine what the limits are likely to be. If you impose the same kind of restraints on coalmining that you impose on nuclear power --

MR. GOTCHY: It would shut them all down.

MR. LAVE: Can I speak to your question for a second about --

Underground mines.

If you were to have all coal come from strip mining rather than underground mining, and all

transportation of coal were to come not from rail transportation or truck transportation, but pipelines for example, and if when the coal was burned you had working equipment for abatement that was now best available technology that never failed, then you could enormously reduce those numbers, as to the number of occupational and public health deaths; and you could probably get them down to the same order of magnitude per gigowatt-hour as the current numbers for the nuclear fuel cycle.

But that's all pie in the sky. That says, "If you really could get this -- if you could put all that technology in and people adhered to it, then all that would happen." But at the same time you would have to make the same, the comparable calculation for nuclear would be: suppose we had all of those factors in effect for nuclear as well, and they all worked all the time. But then if they all worked all the time, you wouldn't have any accidents; you wouldn't have to worry about that component, and so on.

And, and the problem is that you will get advocates for each technology that will tell you that right over the horizon we have technology that will get rid of all these untoward effects. And either you're call these people to their faces "liars," or else you're going to have to say, "Well, I'll believe it when I see it, but in the meantime I'll use past data."

And the past data -- I mean, you can, you can use past data for strip mining; and you'll still have a large number of occupational deaths. And you can use past data on the best available control technologies, and you sti have significant omissions. And so you're still not going to get down to the levels of current experience with nuclear.

MR. BENDER: I want to disagree with that. It seems to me we need to be able to look at incremental, how a mortality rate might be affected by various things that could be done in the fossil energy mining business and use.

And unless we can do that, we're not going to have any way of, of determining what the relative risks are.

MR. GOTCHY: I think one of -- when we're talking about pneumocorneosis as an example, I was just trying to explain to you why it gets so messy. It's because -- one case, you increase the ventilation in a work area to reduce the concentration of coal dust, to reduce the explosion hazard and reduce toxic gases, and you increase the dust loading by resuspending more dust.

And so, you know, you're going -- you're taking care of one thing and making another one worse. So I really can't tell you how it's going to go with best-available control technology in a coal mine, underground coal mine.

MR. LEWIS: What miner -- I'm not an advocate of comparing coal risks with nuclear risks, as everyone

knows, but if we're going to go down the track and one of the, one of the things that people always point out is that the, the nuclear risk is dominated by low-probablility high-consequence events; and therefore, public perception is really entirely different from this game of making estimates of, of how many people get killed per megawatt-year or gigo-watt-years is not really relevant to -- if you're going to do it at all.

But in a certain sense the positions are really reversed from what people normally say, because there is a dominant feature in the coal risk which is an extremely low-probability but fantastically high-consequence event, and that's the so-called CO2.

And the, it seems to me that one ought to add in the probability quite small of killing a few hundred million people, which is really the, the, the dominant risk of the coal cycle. And this is not an entirely whimsical comment, because it's, it's, you cannot get rid of the CO2 in burning coal.

And you may, you may have pie in the sky on other things; but there's no pie in that sky. There's no in that sky.

I wonder if anybody has, especially in view of the new academy study which confirms and makes a little bit worse the CO2 threat -- anyone has actually tried to go down

that track and make some estimate of the expected number of deaths from --

And the probability of 10^{-6}? or something like that?

MR. GROTCHY: I would imagine it would be large. Like you say, because there'd be shifting in the steering winds of the world; and it would change the precipitation patterns all over the world. And --

MR. LEWIS: And it's real.

MR. GROTCHY: One guy -- I remember one fellow calculated that a one-percent reduction in agricultural productivity in the United States causes a million deaths from starvation elsewhere in the world.

MR. LEWIS: I'm not being whimsical. It's real. It's unambiguous. It's true. It's happening. And it's a consequency of burning fossil fuel.

MR. LAVE: Hmm. But the only problem with what you say is that it's unambiguous.

(Laughter.)

MR. GROTCHY: Okay? That is --

MR. LEWIS: Forgive me.

MR. LAVE: The probability is considerably higher than 10^{-6}, maybe on the order of one-half. The problem is that you can make credible arguments that increases in CO2 which lead to warming of the earth would, in

fact, increase the productivity of land in the world, however much it might decrease the productivity of land in particular places.

You make arguments that it would be better for the world. And so the, the sign is terribly ambiguous. And that's why you haven't seen somebody coming out and talking about what the consequences would be of various events.

MR. GOTCHY: And the other problem is that by inducing a greenhouse effect you may put off the next Ice Age for another five or ten thousand years.

MR. LEWIS: The scientist is not so ambiguous in terms of public perception. Public perception is that change is always negative. And so, since we are talking about public perception here, I'm not so sure I --

CHAIRMAN OKRENT: Can I ask whether in the estimates of risk from the coal cycle you have seen anything that allows for long-term risk from solid wastes from the burning of coal?

MR. GOTCHY: Nobody has done that. I, I identified that, gosh, oh, I think it was January '78, as something that was missing from the -- you remember there was a study done to determine the impact of increased coal utilization.

And that was not addressed in there. And that is a, that is a critical question. And, and I wrote a letter which, which Harold Denton signed, sending them back to him

and saying, "Hey, you know, this is fine if you're talking about a few decades; but if you're talking about millenia, you'd better talk about those wastes, because chromium and nickle and cadmium and all those goodies sitting right there in the sludge and, and the fly ash, which may or may not be stabilized for long periods of time, but which are subject to being leeched to surface or ground waters, and enter human pathways that way."

CHAIRMAN OKRENT: If you're going to milling a thousand years, it would seem to me that that would be relevant.

Also, it's not clear to me whether from the mining, whether it's strip mining or deep mining, that there are not risks associated with, oh, liquid pathways -- things getting into the water more rapidly than they would have. And maybe, in fact, there's an augmented release of radon -- I don't know -- from strip mining. But disturbing large areas. It's --

I would speculate, without having done a calculation, that each of those two are probably substantial, compared to a lot of things we see tabulated.

MR. GROTCHY: That's true, although we haven't done it for the nuclear fuel cycle yet either. They're looking at that now. When you go in and, and strip mine, or even deep underground mining, if you intersect that may have been separated by an

to get at the uranium that you may have mixing of and the solution of the remaining radium and that sort of thing which could move through ground water.

Our people worried about that also. The same question of tailing. Since the radium and thorium is still sitting there.

CHAIRMAN OKRENT: Well, what I'm getting at is: this is being looked at for the tailings, at least in terms of the air release; and if we're looking to long periods of time -- and I would say 10 years or 100 years for some of the situations might be enough to get things into the water, from --

Right now there's an awful lot of solid waste from the burning of coal that's just being left sitting, if I understand it correctly.

MR. GROTCHY: I think the current trend is to -- they're supposed to take the fly ash and the, and desulferization sludges and mix them and add something to them to stabilize hem. But current practice is just to dump them together. And the sludge is acidic, and it just has got to leech out the, the trace metals from the fly ash -- over a period of decades, I would say.

CHAIRMAN OKRENT: By the way, in the letter the Committee wrote to the commissioners and to which Commissioner Wolensky raised a question, the Committee was actually

referring to the current state of affairs, I would say. New reactors going on line, compared to other forms of electricity generation currently in use. So it's not what might be done with vastly improved methods 20 years from now. And I just want to note that that was the situation, although I think we're more --

Do you have any other observations you wish to make at this time?

MR. GROTCHY: I guess nothing that probably wasn't in new reg 0332, at least giving a warning that, for example, increased coalization means that there will be more younger workers coming into the coalmining area and that their accident is a lot higher than, than old miners; and so that the accident mortality rate, instead of going down per gigowatt-year electric in the years ahead may actually increase, even with the best of intent on the part of the Federal Government.

PROF. KERR: Is this independently of whether the new workers are male or female?

MR. GROTCHY: No.

CHAIRMAN OKRENT: By the way --

PROF. KERR: I'm serious. Is it likely that the extant rate may be different, depending on whether the predominant population of new workers is male or female?

MR. GROTCHY: In coal mines?

PROF. KERR: Yes.

MR. GROTCHY: I, I don't have the foggiest notion.

MR. LAVE: I, I think the answer is probably yes. That, as I understand it, one of the problems with new workers underground is that they're just not very cautious. And you probably, if you had more women underground, they would probably be more cautious in that initial period until they learned what was going on.

The other --

PROF. KERR: I would think that could be, yes.

MR. LAVE: The other factor that, that at least AMSHA talks about is much more extensive pretraining of miners before they go underground, that you would really have a three to six months' course of having miners work out above ground before they ever went down to do production stunts. And that would lower an awful lot of the initial accidents.

CHAIRMAN OKRENT: It would sure raise the cost per gigowatt-year electric for coal.

MR. LAVE: It certainly would do that.

MR. BENDER: There's also a lot of automation going on in the coalmining business, and that's cutting down on the number of miners per coal mine.

MR. LAVE: Well, I wish that were true. If you take a look at productivity per underground miner,

that hit its peak in the late 1960s and has been falling rather steadily. It seems perhaps to have leveled out since about 1976, but it certainly has not been increasing.

CHAIRMAN OKRENT: You mentioned earlier that the numbers you have been using for health effects from the burning of coal you were anticipating a significant reduction in such effects, because of something, something you were going to get from --

MR. GROTCHY: Oh, this was for the general public.

CHAIRMAN OKRENT: Yes.

MR. GROTCHY: Yes. That's because the Lee-and-Susskind model and Winkelstein model, according to Kemeny's, have been rejected. I think those are very strong. I don't think that's true either.

CHAIRMAN OKRENT: Well, is it because of what was in Kemeny's? Or is there some other reason?

MR. GROTCHY: Somebody else -- I can't remember the authors; I'd have to look it up and review that. In fact, one of the authors -- I think it was Schemel, another author, had redone a study they had done before and rejected their own results. So --

CHAIRMAN OKRENT: Well, I don't know what the --

MR. GROTCHY: The general public, we're pretty shaky condition for calculating health effects.

CHAIRMAN OKRENT: I must say: I saw those words in

the Kemeny's report, and I was a little astonished to see them, and I thought somebody was trying to get an oar in somewhere. But that's just my guess. I'm not a member of the Kemeny's committee. I worked on a panel that was related to the study.

(Several voices.)

CHAIRMAN OKRENT: That's the way I can put it. But the -- I'm not aware of any reason for a general reduction in estimating the health effects from coal. My impression is that if and when the risk impact panel report comes out, their best estimate of the health effects is going to be not dissimilar from what one would get from previous estimates, using something like the Lee-Susskind. Well, that's my recollection.

MR. GROTCHY: I had seen some testimony that, that Leonard Hamilton prepared last year. And he was using, in that testimony, best available control technology in the models that they, I presume, had developed to use for the Kemeny's study. And his numbers were revised downward from the upper, the upper bound by an order of magnitude.

I think they're talking about something like eight deaths in the general public per gigowatt-year electric from sulfer emissions.

MR. WILSON: I wonder if I could say something to help clarify this point. I think there are two separate

things here that -- the adjustment downwards, of Leonard Hamilton's, is an adjustment downwards because of an assumed adjustment downwards of emissions.

MR. GROTCHY: Sulfer emissions.

MR. WILSON: Sulfer emissions -- and for some particular conditions. He's -- in fact, that is not his expertise; and he's taking someone else's numbers on them.

The other question, which is the Kemeny study, is the, is the question of ejection or otherwise of assertion that sulfate is correlated strongly with health effects, which Lee-Susskind's studies are there. That is an open question, I believe, still. The particular section there was written by my colleague, Henry Cohen, of the Harvard Medical School. And we can still believe that statement.

However, it is, I think, wider. I think it is still an open question. Leonard Hamilton himself, since that time some more studies of the same type of legislation independently -- whether it is correlated with sulfate as sulfate or sulfate in particular he does not know.

So Lipputt has some done studies correlating the particulars as particulars.

Now, rejection by Henry Cohen is a rejection basically of the whole lot.

And so the question is, what is the damage function in a sense, what is the function by which you multiply an

assumed sulfate emission or particular emission to get the health effects, is probably uncertain to a factor of 10 at the moment.

One of my colleagues at the School of Public Health, a statistician, has gone through some of the statistical work of Lee-Susskind; and he believes they may have been underestimating by a factor of 2, and that, so there's even, even more than that. So this, the uncertainty goes all over the shop, and there's nothing much one can do about it, in my opinion.

MR. LAVE: And I think that there is at the moment a very large campaign, either scientifically inspired or otherwise, to try and discredit all of the health effects.

One evidence of it was a meeting at the New York Academy of Medicine last year, trying to take a look at SO2 effects; another is a focus on the licensing of a power plant on Staten Island, a full-powered plant. And there are very good scientists who are being gotten in these cases, to talk about wide ranges of uncertainty. And the interpretation they are being led to -- I guess knowledgeable; they are good people -- is that, in fact, these health effects are unproven and, therefore, can be taken to be small, even perhaps zero.

I don't think there's anything in the literature that would support that, but there are good people around.

MR. WILSON: AMA, by the way, are taking the numbers of Hamilton's as --

MR. GROTCHY: They used our numbers, too.

MR. SAUNDERS: The situation seems to be analogous to the early days of the cigarette controversy, which somebody seduced the Grand Old Man of statistics, Sir Ronald Fisher. He was supporting the position that health defects due to smoking were not proven statistically and could not be proved statistically.

PROF. KERR: I must say that your recent comments have clarified things for me.

(Laughter.)

CHAIRMAN OKRENT: Thank you. I think what we'll do is break for lunch; and if we can, let's be back 10 minutes to 2:00. Okay?

(Whereupon, the hearing was recessed for lunch at 1:00 p.m. o'clock.)