UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY OF NEW YORK, INC.

(Indian Point Station, Unit No. 2)

Docket No. 50-247

Place - Springvale Inn, Croton-on-Hudson, N.Y.

Date - November 3, 1971

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Springvale Inn
Croton-on-Hudson, N.Y.

Wednesday, November 3,1971

The above-entitled matter came on for hearing, pursuant to notice, at 9:00 a.m.

BEFORE:

SAMUEL W. JENSCH, Esq., Chairman, Atomic Safety and Licensing Board.

DR. JOHN C. GEYER, Member.

MR. R. B. BRIGGS, Member.

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M O R N I N G   S E S S I O N

CHAIRMAN JENSCH: Please come to order.

I think at the conclusion of last evening we were discussing the stipulation which had been submitted by the Applicant and which reflected the stipulations of the Citizen's Committee for the Protection of the Environment, Environmental Defense Fund and the Hudson River Fishermen's Association in connection with the future conduct of the proceeding.

The Board has given review to their stipulation and certainly feels the parties are to be complimented on their endeavor to work out procedural specifics for this proceeding and the time schedules which they have set forth in the stipulation.

The Board feels, however, that the stipulation need not be a part of the transcript but the stipulation can, if the parties desire, be mailed to the secretary of the Commission and made a part of the formal public record in the proceeding. There are some things about the stipulation that might warrant your consideration at this time.

The Board understands that the stipulation has been submitted in an endeavor to expedite the presentation of evidence and consideration of the matters in this proceeding and the Board wonders whether enough time has been left to the parties to prepare the findings. The emphasis should be, ABT1

of course, that the findings that are to be submitted are findings of fact. The language of the stipulation just talks about findings and conclusions, and the Board has emphasized that the findings should be of fact, and the Board requests that the rule of the Commission be followed that transcript references be submitted for statements of fact to support findings and conclusions.

If the parties want to expedite the consideration of this case, the proposed findings and conclusions are one way to help, and by that I mean if the parties dig out the source of the data which is reflected it will assist the Board considerably in proceeding to a consideration of the matter. And of course the references can include the formally filed documents which include the FSAR.

But we are also very much interested in having the proposed findings reflect the discussions of the matters in the transcript. I believe it's a fair inferences from many of the submittals of other cases that sometimes the proposed findings and conclusions, for instance, from an applicant, are nothing more than a copy of what the application was when the summary statement was filed at the beginning of the proceedings.

Well, of course, the Board is anxious to have the conclusions of the parties based upon the facts. We like to see the facts set forth with great profusion. We would like to have these matters set forth and findings so that we may hear and can find in the record where these matters are discussed.

Now for instance, in dealing with experimental matters let me take just a supposed instance that may not have any reality or relationship to this case.

If an experiment were conducted something like this, that a person took a match and struck it and ignited the match and placed it against a piece of dry paper, all of which were within a dry full-of-oxygen container, and if the statement was made that the match lighted the paper, we would like to see if reflected in the findings with some specific reference, if there isn't a conclusion contrary to that.

By that I mean not that there are calculations made that don't show that this will happen, if it has happened. I don't know what the Board's consideration will be to some of these experimental data. But I don't feel that the Board will conclude that it is limited by statements in a report in an experimental matter, that these are our tentative findings, further research is needed, and nobody takes any responsibility as to what was set forth in the report. I think many of the reports disclaim any responsibility for their horrendous results that are sometimes set forth.

A match will light a piece of paper. We would like to see if there is any factual evidence to the contrary. Maybe calculations are needed, too, but sometimes seeing it how it is and seeing what happens is more persuasive than all calculations in the world.

I think the Board is interested in having a showing in the findings of fact, what are the important matters reflected by the transcript. I think sometimes the proposed findings and conclusions ignore that the real disturbing matters in a contested case -- If an intervenor, for instance, should set forth factual matters and propose findings of fact and conclusions, then perhaps the reply by the applicant which is provided in the stipulation can deal with the factual response. Ignoring really the substantial problems will not assist the Board in any respect.

I know the parties did not intend to be too restrictive in what they presented. The Board did find it interesting. There was phraseology, I think, in several places that the parties desire and expect the Board to give a decision, well within certain periods of time.

It has been my impression, although I didn't see any specific ruling, that Calvert Cliffs decision may or may not have disposed of that intended restriction when the Court said within the issues prescribed by a regulatory body, the hearing board is the decisional group and not to be limited in what it would consider. I thought a necessary corollary of fact that there wouldn't be a time limitation when they consider but would meet to complete a thorough and independent evaluation of the evidence.

Just so the parties are clear about it, I don't think the Board feels that the desires and expectations of the parties to be too controlling about this. Rather than say we told the Board we want a decision in fifteen days and the complaints start right away that a decision isn't out in fifteen days -- Well, we want the parties to know that the Board doesn't feel that is the basis of a legitimate complaint. We will proceed expeditiously to consideration of all the matters. This case has many serious problems in it. The Board will give very thorough consideration to all of them.

We don't know about this environmental matter. Of course, this is the first case which is coming to a conclusion, perhaps, that involves environmental matters. It is a new field for the Commission. It is a new field for the Boards. Whether four days will do it for hearing time on that, we don't know. We don't want the parties to feel that they told us that they only want four days given over to consideration of these matters.

There is one thing I noticed wasn't in the stipulation. If the Board decides that the case should be reopened to get further evidence, no time is specified by the parties as to when that should be done. Nor if the parties are asked to submit further specifics in reference to the conclusions set forth, that may take some time further.

If you have any suggestions how promptly you will be able to respond to the inquiries of the Board, we will be glad to have your suggestions.

Incidentally, there was an order issued in early October authorizing fuel-loading and subcritical testing. Has that started?

MR. TROSTEN: Yes, Mr. Chairman, it has. The fuel loading has started and--excuse me.

MR. CAHILL: Fuel is all in.

MR. TROSTEN: Fuel is all in, Mr. Chairman.

CHAIRMAN JENSCH: And how long will the subcritical testing take?

MR. TROSTEN: Mr. Chairman, this is a variable, as we have indicated. It could be three weeks, it could be considerably longer than that.

CHAIRMAN JENSCH: Will you put a figure on the considerably longer than that?

MR. TROSTEN: A month or two, perhaps.

The one reason that we indicated in the past that we wanted to start because if there were any problems we wanted to be sure that we could catch them early.

CHAIRMAN JENSCH: Well, I think the importance of f the data is just to assist also in scheduling of this thing.

MR. TROSTEN: Yes, that's right, Mr. Chairman.

CHAIRMAN JENSCH: We endeavored to provide an order for fuel loading in July and because we understood there was great urgency and then later we understood that the facility wasn't ready for fuel loading in July anyway. And so it may

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mean that if the subcritical testing goes on for a month or two or three months that there may be some flexibility in the schedules that will assist in the planning of the proceedings.

MR. TROSTEN: Well, are right on schedule, Mr. Chairman. Everything is moving along very rapidly.

CHAIRMAN JENSCH: Very well. If it takes month or two or three, that willassist the Board in its--

MR. TROSTEN: No, it shouldn't take three months, Mr. Chairman. We are thinking that we might have it done in three weeks. And as I say, perhaps a month or two if we run into problems.

CHAIRMAN JENSCH: Well, we hope you don't run into a problem. But I guess things change from time to time.

Therefore, we will return this stipulation to you for such disposition as you desire. We do compliment the parties on reaching an agreement as to when they will proceed in the matter, and we hope they will adhere to these schedules.

MR. TROSTEN: Thank you, Mr. Chairman.

May I comment on the points you made this morning?

CHAIRMAN JENSCH: Yes, surely.

MR. TROSTEN: First of all, I want to say that Applicant's counsel appreciates very much the guidance that the Board has given concerning the nature of the findings and conclusions and we will certainly endeavor to take them fully into account and to take into account the applicable requirements of the Administrative Procedure Act and the Commission's rules in formulating our findings and conclusions, including, of course, making the transcript references.

Secondly, I would like to make just one observation about some of the phraseology that appeared in here. As I have said, Mr. Chairman, we were in noway attempting to suggest that somehow the Board was bound by the fact that just because the parties desired and hoped and expected that the hearing could be completed in four days, that somehow this bound the Board. We fully recognize, of course, that this is not so. But nevertheless, this does indeed, Mr. Chairman, represent our desire and expectation and represents our best estimate based upon what we know about the situation as to how soon we can proceed, and that is all it represents.

CHAIRMAN JENSCH: Well, let me say that the Board shares your same view. We desire and expect to have this thing over as soon as we could. If we could do it in the next day after the length of time this case has been going on we'd be glad to do it, but I don't think the complexity of the case permits it either by the parties or by the Board.

MR. TROSTEN: Yes, I understand your point, Mr. Chairman.

One reason why we did not go into the matter, for example, of reopening the hearing in case the Board has questions and what time periods were involved is, as the Chairman is fully aware, of course, these matters are just impossible to predict at this time and we just couldn't, didn't see how we could get into that sort of complexity. This represents our best effort to set forth the planning that we intend to follow and what it is that we intend to do.

With regard to the time limits that we set forth for the findings and conclusions, I can only say that we realize that we have imposed upon ourselves an exceedingly heavy burden and we and the Intervenor's Counsel and the Intervenor has agreed to impose upon himself the exceedingly heavy burden for him of preparing these findings and c onclusions that comport with the Chairman's suggestions and the Commission's rules and regulations in these very short time periods, because we do want to expedite the proceeding and we feel that we can make these schedules if we work as hard as we possibly can on them.

So that is the explanation for the short period, rather than a feeling that we want to short-cut the process.

CHAIRMAN JENSCH: Well, I hope you will be able to do that, because as I say I think in some cases we have had the impression that just as soon as the hearing was over and the last witness left the stand the Applicants have been able to come up and say, "Here are our proposed findings," and I don't know whether the proposed findings reflected a single word of the transcript. I mean as if the proposed findings were drawn even before the evidence was taken. We are most anxious to have the evidence.

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: As shown in this transcript, reflected in the proposed findings.

MR. TROSTEN: I understand fully the point that you are making, Mr. Chairman, and will recognize that fact in setting these times. Those short time periods in no way reflected the thought that we were going to submit a new, unduly abbreviated findings and conclusions and we weren't going to comply with the requirements.

The only other point I wanted to make was this.

You have suggested, you have directed that the stipulation not be incorporated in the transcript and that it could be filed with the Commission. You may recall that we offered it as an exhibit. Will you accept our offer that it will be considered as Applicant and Intervenor's joint exhibit in this proceeding?

CHAIRMAN JENSCH: I don't understand the purpose of the offer.

MR. TROSTEN: Well, as I understand the Commission's regulations on the point, Mr. Chairman, the parties are permitted to stipulate as to procedure, which is what we have done, either orally or in writing. We have done it in writing and the parties are permitted to offer exhibits. In another proceeding which the Chairman is aware of a stipulation among the parties was offered as an exhibit. It seemed to me that this was an appropriate procedure. I have no strong feelings about it, Mr. Chairman. If you don't feel we should do this we will simply submit it to the Commission and have it filed in the public document room.

CHAIRMAN JENSCH: I think that will be satisfactory. I think the stipulation contemplated by the rules is that it would be helpful in the proceeding. Stipulations as to facts and admissions and so forth, those things that will obviate the presentation of perhaps some evidence.

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: It will be filed and have the same force and effect as any other matter in the case.

MR. TROSTEN: We will file it with the Commission.

MR. KARMAN: Mr. Chairman, while the Regulatory Staff is not a signatory to this stipulation, I just want the record to reflect and the Board be aware of the fact that we will certainly do everything within our power to expedite this hearing and keep it within the confines of the general plan as outlined by the stipulating parties and the Board, of course.

CHAIRMAN JENSCH: Yes. Of course the Board looks to the Staff for a pretty thorough review of both presentations of evidence, i.e. from the Applicant and from the Intervenors, and we will also hope that the Staff presentations will be related to specific transcript references.

MR. KARMAN: We certainly will try, Mr. Chairman.

CHAIRMAN JENSCH: Thank you. With that are we ready to proceed with further interrogation? Mr. Moore was on the stand. Is Dr. Roll in here?

MR. TROSTEN: Mr. Moore and Dr. Roll are here, Mr. Chairman.

CHAIRMAN JENSCH: Do you have further interrogation of these two gentlemen?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Will you come forward, please.

MR. FORD: Mr. Chairman, in the interest of efficiency I have decided to describe two types of questions that will be included among the questions that I will be asking this morning as a very standard question, and I'd like to make clear my expectations regarding the way in which they will be handled so that we won't have any confusion at all.

The first type of question I'd like to rely on to settle matters is a question that requires simply a yes or no answer. The witness may wish to add some further parenthetical comments. It may be, for example, that he feels that the Applicant's non-consideration of a certain phenomenon, for example, in the accident, give the appearance of some kind of weakness or flaw in his case. In any event, this further justification, parenthetical comment, is not what I am seeking. I am seeking the fact of whether or not the Applicant is considering such phenomena.

Applicant's counsel may desire to put the witness on the stand to solicit direct testimony from him regarding phenomena that I discussed. But my own purpose in the cross-examination with these yes or no questions is simply to clarify a basic matter of fact. I'd like to have it understood that this is the purpose of this question.

MR. TROSTEN: Mr. Chairman, excuse me. I assume Mr. Ford is about to commence his interrogation. Let me say this.

I appreciate the thoughts that Mr. Ford has expressed which clearly represent the advice of competent counsel as to how to conduct cross-examination. I hope Mr. Ford will appreciate that his suggestions as to how Applicant's witnesses should respond to his questions are of interest to us, but that Applicant's witnesses will respond to his questions as they should respond to his questions, and that Applicant will object to his questions or the form of the question depending upon the way I hear the question.

So that if Mr. Ford asks a question which you may consider calls for a yes or no answer, the witness will respond yes or no if he feels that he can respond yes or no, not whether you think he should respond yes or no.

MR. FORD: I understand this. What I am trying to say is, in my own mind there is some gray area between what can be answered yes or no and what requires a full answer. I would just like to secure the benefits of the firm area in which yes or no will do, and parenthetical responses are not responsive to the direct question.

The further kind of question in which I will rely is a question which requests specific reference to experimental results. The answer I am seeking is the name of a document. It is graphical material. Again, in this instance I am seeking comments on whether or not this kind of experiment is anything that is needed or reasonable to do. I am seeking whether or not Applicant's witness can provide a reference to an experimental report on the specific matter that I am questioning.

With that preface, I will begin.

Dr. Roll, to clarify some matters yesterday, could you give us your definition of the term, "eutectic alloy?"

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DR. ROLL: I think--not my definition, but a definition of a eutectic alloy is an intermetallic compound which forms between two metals whose melting point is less than the melting point of either pure metal.

MR. FORD: In the definition of a eutectic alloy, is any stipulation made as to the specific manner in which the two metals form this eutectic?

DR. ROLL: I wonder if you could expand that a little bit, by what you mean by manner in which it is formed?

MR. FORD: In terms of the changes and structures of the metals relevant to eutectic formation, does the definition of eutectic limit the conditions or formation in any way. Is not any two metals that come together and have a low melting point a eutectic? Is that correct?

DR. ROLL: I'm not sure what the answer to your question is. If you are asking from a basic metallurgic consideration and an abstract definition of eutectic, or if you are heading toward the definition as germane to this discussion.

MR. FORD: If the zircalloy iconel eutectic were formed, is it correct that its melting point is 1760 degrees Fahrenheit?

DR. ROLL: That's correct.

MR. FORD: If the zircalloy iconel eutectic were formed and melted at 1760 degrees Fahrenheit, and reacted with water or steam, do you know the most probable form of this reaction and the quantity of heat that would be released?

DR. ROLL: Quantitatively I don't know the most probable form of the reaction nor do I know the quantity of heat that would be released.

MR. FORD: Do you know what upper bound would be put on the reaction to indicate -- Would be on a heat release that would assure us that even if it achieved this upper bound, it wouldn't contribute in any significant way to local cladding damage? That's not very clear. What amount of heat from eutectic alloy reaction with water, zircalloy iconel eutectic alloy with water, what amount of heat could we tolerate before we had problems of local cladding damage?

DR. ROLL: I believe the answer to that question really requires a consideration of the system that is undergoing a hypothesized loss of coolant accident and not a consideration of the reaction per se. Therefore, if there is a quantitative answer that can be put together, I think we should ask it in the context of the application we have in hand.

MR. FORD: Fine. Let's take the relevant system. Let's take design basis accident. Let's take the assumptions specified in the appendix to the interim policy statement governing the Westinghouse computer code. That's the standard situation for our loss of coolant accident analysis. Let's answer in that context, please.

MR. MOORE: The analysis performed for the hot spot calculation for the loss of coolant, as indicated in the application, indicates a peak temperature of 2300 degrees Fahrenheit. Therefore, maintaining all of the conservative assumptions that go into that calculation, I can tolerate no additional energy due to this reaction without exceeding 2300 degrees Fahrenheit.

MR. FORD: If it were correct that the zircalloy iconel reaction released any heat at all, is it then correct that the Applicant's plant would not meet the interim criteria?

MR. MOORE: In a strict sense, the answer is yes.

MR. FORD: In relation to the hot spot, can you describe the proximity of iconel to the 2300 degree hot spot in your calculations?

MR. MOORE: It is approximately ten inches away from any iconel grid, at least ten inches away.

MR. TROSTEN: Mr. Jensch, may I interrupt the questioning with this observation consistent with what the Chairman's instructions had been in the past during these hearings to the witnesses.

In responding to a question, if the witness feels a yes or no answer is appropriate and can give a yes or no answer, but feels that he then must qualify his answer, that what he should do is to answer the quest yes or no if he feels that he can, and then to proceed with the qualification thereafter.

CHAIRMAN JENSCH: I think it is very difficult to lay down a positive rule in that regard. I think a great deal of court proceeding leaves it to redirect qualifications and further explanations that would be related to a specific question. I think administrative hearings do permit some explanation by way of qualification. I infer from the statement by the interrogator for the Intervenors that he feels the proceeding would move along a lot faster if we did get some yes or nos and go on from there. I don't know that that is possible in all instances. I think if the witness has some reservation, he should so indicate. If the explanation takes a great deal of time, we have to leave it up to the redirect. I think if it can be briefly explained, it may be helpful at one place in the transcript to have that.

MR. TROSTEN: That's what I am proposing, Mr. Chairman. The problem is in order to move the hearing along properly, we obviously, after all of the cross-examination and the Board questions are over, are going to have to examine the record and determine whether redirect is necessary. On the other hand -- And this is going to take some time. If we can get it done at that point, it would save time.

CHAIRMAN JENSCH: I infer that the suggestion by the interrogator for the Intervenors may have been prompted somewhat by the answers yesterday. I think the witnesses in proceeding to answer are anticipating what might be involved with some ramifications and they proceeded to give the explanation before they answered the question. I think if they reversed the process, answered the question, then if there was some brief explanation or qualification given so that we could have it disposed of. But we did get a lot of speech-making both in the questions and in the answers.

MR. TROSTEN: I agree, Mr. Chairman.

CHAIRMAN JENSCH: Will you proceed.

MR. MOORE: Mr. Chairman, may I clarify an answer I gave previously?

CHAIRMAN JENSCH: I take it that's directed to "yes in the strict sense."

MR. MOORE: That's the one.

CHAIRMAN JENSCH: With that consideration will you proceed.

MR. FORD: Could the reporter re-read the question and answer that you are going to clarify, please.

CHAIRMAN JENSEH: If she can, if she has it before her.

I think the question was something like this. Is it correct that if the zirc iconel released any heat it would not meet the criteria, and the answer was "Yes in the strict sense."

MR. MOORE: I would like to clarify this. I made a presumption that wasn't really in fact the case in the reactor and that was I assumed in responding to the question that the iconel zirc reaction occurred at the hot spot and it does not. So a heated reaction due to an iconel zirc eutectic would not cause us to exceed the interim criteria.

MR. FORD: Further pursuing the location of this reaction relative to the hot spot, can you give me the axial length of the hot spot rod, the hottest rod, the axial length of the area that is 2300 degrees Fahrenheit?

MR. MOORE: In the calculation I indicated yesterday in the testimony that we divided the core into seven axial increments. So it was one-seventh of the twelve-foot core. In the actual design I also indicated yesterday in testimony that the hotter regions of the core there tends to be a flat area in the axial direction of one to two feet, which I indicated was one of the reasons for the randon failure of rods. I would indicate here though that the hot spot, this one to two feet, may include a grid, but there is a flux depression at the grid. So the flux near in the vicinity of the grid is lower than at the hot spot.

MR. FORD: Could you provide us with a diagram of the hottest rod and place the grids on it in reasonable scale so that we can make sure we have the picture exactly?

MR. MOORE: This is the length of the core. This is the bottom, this is the top. If we look at the axial power distribution, the typical shape has the tendency of a cosine distribution where the peak would occur in the center regions of the core over this one or two-foot length that I was talking about. This location of this peak may shift in time with operation, but it does maintain that kind of flatness.

So this area may or may not include a grid. In this particular case if I have a grid here, grid here, and a grid here, what I have shown you is the power distribution, ignoring the effect of the grids. Now when you actually measure and calculate and measure the power distribution which includes the grids at each grid location there is a flux depression because of the neutron absorption characteristics of the parasitic metal in the grid.

And this reduction in power at the grid is the reason it is clear that the hop spot never occurs at a grid location.

MR. FORD: Can you give us the scale, the size of the grid, the size of the flux depression, or do you contend that the large grids that you have drawn and the large depressions are to scale?

MR. MOORE: Well, the grids certainly aren't to scale. The flux depressions are reasonably to scale. This is in the order of a five per cent or more reduction.

MR. FORD: Now over what length of the rod does this flux depression take place? How large is the grid? Is the grid three inches, half an inch, two inches?

MR. MOORE: The grid's about an inch and a quarter long.

MR. FORD: Are you sure of that or is that just the distance between Dr. Roll's fingers?

MR. TROSTEN: Mr. Chairman, we think this may be in the safety analysis report.

MR. MOORE: It certainly is.

MR. TROSTEN: If we are able to find this we would be able to produce this.

MR. MOORE: If you'd like an exact answer, it is in the document.

MR. TROSTEN: Would that be satisfactory if we could find it?

CHAIRMAN JENSCH: If it doesn't take too much time. In the meantime, I think the witnesses are somewhat inconvenienced by having this easel up here. I think the cleaner's men have taken it up in order to get it out of the traffic lane. But if that can be placed down there the witnesses may return to the stand. The witness can step down to use it and it may be more convenient.

MR. MOORE: Would you like me to look up the exact number?

MR. FORD: Well, I think we can go on.

MR. TROSTEN: All right. We will look for it, see if we can find it.

MR. FORD: Now when you said that a grid is ten inches from the hot spot do I take it that when you are talking about a hot spot you mean the very middle of this one or two foot area, the flat area, is that correct?

MR. MOORE: Yes.

MR. FORD: And when you say it's ten inches away do you mean there is ten inches above that is a grid and ten inches below it is a grid?

MR. MOORE: These are all, of course, approximate numbers. You have to look at the actual flux distributions. But what I was answering was the effect of the grid depression. Then I was taking the kind of span I can have between the grids and indicating that because the flux is depressed at the grid that should the flat, relatively flat peak occur in the vicinity of the grid it is actually depressed at the grid.

MR. FORD: That's not what my question was. My question was is there a grid ten inches above the exact center of the hot spot and ten inches below? Yes or no, please.

MR. MOORE: I guess I can't answer the question directly. The first point is the hot spot never occurs at the grid. You asked me how far from the grid --

MR. FORD: Thank you.

MR. MOORE: -- could the hot spot occur. I indicated that ten inches. That was strictly on the basis of looking at the span between grids and knowing the hot spot never occurred at the grid. That's how I derived the number.

MR. FORD: Let me ask this. Do you want to change your answer that you will find grids ten inches from the hot spot?

MR. TROSTEN: Excuse me, Mr. Chairman. May I say that I am giving to the witness a portion of the safety analysis report where either all or most of the information that Mr. Ford is asking for I understand is contained and I am referring this to the witness to refresh his recollection on this matter.

CHAIRMAN JENSCH: Very well. Can you identify that page?

MR. TROSTEN: Yes.

MR. MOORE: This is figure 3.2.3-9 of the final safety analysis report.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Excuse me. Can you repeat that reference, please.

MR. MOORE: Figure 3.2.3-9 in the final safety analysis report.

DR. ROLL: Just for clarification, the question I think you are asking was how far could the hot spot be from the grid.

MR. FORD: The standing question is, Mr. Moore do you want to change your answer on the question of -- Your answer that there were grids ten inches from the hot spot?

MR. MOORE: Could I have the exact question repeated regarding my answer to see whether I want to change it?

MR. TROSTEN: While the question is being repeated Mr. Larsen is handing Mr. Moore a note concerning the scaling factor that is contained in that page reference.

MR. FORD: I can repeat my question at any rate.

CHAIRMAN JENSCH: Very well. Proceed that way.

MR. FORD: The question was how far were grids from the hot spot and the answer was ten inches, and that is the answer I am asking you whether you want to change.

MR. MOORE: The hot spot could be as far as ten inches from the grid.

MR. FORD: As far as--you mean it could be significantly closer?

MR. MOORE: Yes.

MR. FORD: How much closer? What is the range?

MR. MOORE: It is at least an inch away.

MR. FORD: One inch away from the grid?

MR. MOORE: Yes.

MR. FORD: Proceeding from one inch away from the grid to the grid, can you give us the precise information on those LOCA temperature differences during the accident?

MR. MOORE: Not from the top of my head. We have looked at the effects of axial conduction. These are insignificant.

MR. FORD: Can you tell me, does the computer code simulate the grid effect on LOCA temperature?

MR. MOORE: Which computer code?

MR. FORD: The computer code you have used to analyze the loss of coolant accident, heat-up from which you calculate 2300 degrees Fahrenheit?

MR. MOORE: Which effect of the grid?

MR. FORD: The effect that the temperature at the grid would be as you indicate here--I don't know how firm a figure that is. You indicate temperature immediately at the grid would be five per cent less than the temperature from smoothing the curve over the grid. Does your computer code from analyzing the emergency core cooling effectiveness and core heat-up during loss of coolant accident simulate that efffect of the grid?

MR. MOORE: To correct the statement you made, I did not say that the temperature was five per cent less. I said the power was five per cent less.

MR. FORD: What is the temperature of the rod compared to an inch away?

MR. MOORE: At least 150 degrees.

MR. FORD: Does the computer code simulate that 150-degree difference seven times? It is not 150 degrees from all areas. Does it simulate the differences in axial temperature due to the effect of grid?

MR. MOORE: Yes, because it calculates the temperatures of rods with different power levels.

MR. FORD: I'd like to know whether explicitly in the code it has some parameters reflecting the presence of grid spaces and it computes their effects on LOCA cladding temperature.

MR. MOORE: Which effect?

MR. FORD: The effect that the cladding temperature there is significantly lower than in the portions of the rod between the grids.

MR. MOORE: Yes, because the code calculates the temperatures of rods which are the equivalent of five per cent lower in power.

MR. FORD: Let me understand that. The code calculates the temperature of rods that are five per cent lower in power than the rod in question?

MR. MOORE: As I indicated in the testimony yesterday, we calculated temperatures of various power level rods in the core. The power level represented by the power level at the location of a grid is included in this analysis.

CHAIRMAN JENSCH: I wonder if the question was answered. This may be one of the problems that he has had. I think you were asked, does the code calculate this difference from the hottest rod. Wasn't that the question? The question is, does it or does it not? Can you answer? That is as to what you said yesterday or last week.

MR. MOORE: The answer is yes on the same basis.

MR. FORD: My point of reference in the question is the hot rod in the hottest region of the core.

As I understand your answer, what you are saying is, you compute the temperature for other rods in the core, that are five per cent less power than the hot rod. Is that what you are saying?

MR. MOORE: That's correct.

MR. FORD: What I am concerned with is whether for the hot rod the five per cent differences in power at spacers is computed for that rod. Is that done?

MR. MOORE: This is one I can't answer directly. There is essentially no effect, and therefore we don't compute it. There is no axial conduction.

MR. FORD: Let me understand you.

MR. MOORE: Essentially no axial conduction.

MR.TROSTEN: Let me interrupt. I am having a problem understanding an aspect of the questioning in terms of foundation for the question.

Can you tell me, Mr. Ford, are you directing your questions to a particular portion of Part 3 of the Commission's Interim Acceptance Criteria, the Westinghouse evaluation model? In other words, the recipe for calculation of ECCS performance. Can you direct me to the particular paragraph or section, or is there a particular paragraph and section to which your question is directed? I am having a little difficulty grasping whether you are doing that or whether you are not. I'd like to know that so it could help me to understand your questioning.

MR. FORD: I see. I am concerned with one of the many gaps in the Interim Police Statement and the computer code. I am concerned with a variety of chemical-metal-water reactions that are not considered at all in these codes, metal-water reactions which various recent experimental data indicate can prove very significantly to local temperatures during an accident, and extensive cladding damage. The specific metal-water reaction I am concerned with at the moment is the reaction between the zircalloy inconel eutectic and steam. I am concerned to find out how the Applicant's analysis contained in the computer code, which does not consider this, how it would be different if it did.

MR. TROSTEN: I thank you for the explanation. I recognize this as being one of the principle points of concern in the critique by the Union of Concerned Scientists in October. But what I am still not clear about is whether this point that you are raising is related to a gap in the four criteria themselves, or whether it is related to a gap in the technique used for evaluating compliance with the criteria, or whether it is both.

MR. ROISMAN: I think at this point, Mr. Chairman, it is difficult to know which it is because we are having some difficulty in pinning down the facts. We need to know how we go about computing where the hottest spot is in the core and what account they take of factors which they are suggesting, as Mr. Moore has this morning, will somehow or other reduce the effect of these metal-water reactions on affecting the hot spot. As you remember, we got into this discussion from Mr. Moore's qualification of his earlier statement that said that the iconel eutectic metal-water reaction could produce temperatures which would exceed the interim policy statement standard of 2300 degrees Fahrenheit if they occurred at a hot spot. Then he tried to explain why it wouldn't occur at a hot spot.

Now we are trying to find out how it is taken into account in the code in computing. We now got the hot spot an inch away from the place where the metal-water reaction is going to occur.

I am trying to see in the individual rod, are they able to show us how the code takes account of this little pressure dip in there. From that we may either come up with a conclusion that interim criteria have a fault or that the criteria finds that the Applicant's code has a fault.

CHAIRMAN JENSCH: Will you proceed.

MR. FORD: I am not sure whether there is a question outstanding.

CHAIRMAN JENSCH: If there isn't, will you pose another.

MR. FORD: The computer code computes the temperature history of a single rod in each of its discreet considerations of the core region; is that correct?

MR. MOORE: Yes.

MR. FORD: The output of the computer code consists of temperature predictions at different axial levels; is that correct?

MR. MOORE: Yes.

MR. FORD: There are, for the hot rod in the core, as I understand it, seven temperatures predicted along its axial length; is that correct?

MR. MOORE: Yes.

MR. FORD: The axial lengths, as I understand it -- Are any of these seven temperatures predicted by the codes specifically assigned to the temperatures at the exact location of grid spacers?

MR. MOORE: I'm not sure I understand the question.

CHAIRMAN JENSCH: Read that question.

(The last question was read by the Reporter.)

CHAIRMAN JENSCH: Can you try that yes or no or explain it?

MR. MOORE: Not specifically, no.

MR. FORD: So that is it correct, then, that the computer codes do not explicity simulate the presence of grid spacers?

MR. MOORE: The computer code that I am discussing that is used in the accident analysis does not specifically calculate the temperature at a grid. It calculates temperatures for power levels representative of grid locations. What you fail to understand is that there is no significant effect along the length of the rod of the temperature at one part of the rod to the remainder of the rod, and that is why we do not consider specifically the axial location of the grid.

MR. FORD: I am not talking about axial conductance at all.

MR. MOORE: Excuse me.

MR. FORD: Have I ever asked you a question about axial conductance?

MR. MOORE: You have asked me questions on the effect of the heat at the grid on the temperature at the hot spot. I can get the temperature from the grid to the temperature to the hot spot without some axial conduction.

MR. FORD: The molten iconel zircalloy eutectic, do you expect that from spacers above the hot spot, that it is possible that this molten liquid, by gravity, will migrate down closer to the hot spot?

MR. MOORE: Absolutely not.

MR. FORD: You don't expect -- What basis do you have that gravity will not move a molten metal in a downward direction?

MR. MOORE: As we indicated in the testimony yesterday, our tests showed we didn't have any molten metal of any sufficient magnitude which would flow.

MR. FORD: I am asking you if the eutectic formed, would it flow?

MR. MOORE: I am not in a position to refute gravity this morning.

MR. FORD: Is that a yes answer?

MR. MOORE: Let me clarify it. We are not just talking about gravity. We have other effects. We have surface tension. We have the cooling process of the molten eutectic which will solidify. We have upward forces due to the steam-water flow. So I won't conjecture.

MR. FORD: Can you guarantee that if the eutectic melted at 1760 degrees and you are above that temperature, can you guarantee that surface tension cooling processes and upward forces would be sufficient to prevent the migration of this material to the core hot spot from an inch away?

MR. MOORE: I will not conjecture.

MR. TROSTEN: Mr. Chairman, may I just ask a question that has to with the procedure that we are following this morning and the timing of it? Yesterday we discussed the matter of Dr. Roll's availability and we expressed the hope that Dr. Roll could be released by noon today. It's 10:15 now and most of the questions appear to be directed to Mr. Moore. Could Mr. Ford endeavor please to complete his cross-examination of Dr. Roll so that he could be free by noon?

MR. ROISMAN: Mr. Chairman, if Mr. Moore chose to answer the questions, that's his business. The questions have been directed to Dr. Roll and we are still talking about eutectics and metal-water reaction. If the judgment of the two witnesses on the stand is that Mr. Moore's the more qualified, there is nothing we can do about that. If Dr. Roll would rather answer these questions, we'd be glad to have his answers.

MR. TROSTEN: All right. Thank you, Mr. Roisman.

Let me put it this way then. If Mr. Ford feels that he wants to direct his question to Dr. Roll would he endeavor to complete that by noon and then we will take the responsibility if we end up keeping him here longer than noon.

MR. FORD: Please understand from this point until further stipulation that all questions I ask are directed to Dr. Roll.

Now the question, repeating the question which Mr. Moore just discussed, can you guarantee -- I am not asking for your conjecture, I am asking for your offering of experimental or analytical data -- Can you guarantee with any analytical or experimental data that surface tension cooling processes and upward forces would be sufficient to prevent the migration of any molten zircalloy iconel alloy over the one inch distance from the grid spacer to the hot spot?

DR. ROLL: Because we do not think the phenomenon occurs we have not done such calculations, therefore we cannot offer the guarantees that the phenomenon which we don't think will occur is going to propagate.

MR. FORD: The answer is therefore?

MR. ROLL: No.

MR. FORD: No.

CHAIRMAN JENSCH: I think that that is a good way to start, in reverse, if you can. Glad to have your explanation and your reasons for thinking it. You have not got calculations and so forth, that's fine. I think it will help us move along by finding out where you are going to light right at the start.

MR. FORD: Can you tell me in terms of the output of the computer code, assuming that we have had a -- That this area defined the axial region of the rod or one of the seven axial regions of the rod for which temperature was computed and it had a grid spacer in the middle of it, would the code compute one temperature for the entire area and not separate temperatures for the areas with grid spacers and the area at grid spacers?

MR. MOORE: Yes.

MR. FORD: In computing the temperature for the entire area would it simulate any of the phenomena associated with grid spacers' influence on local temperature?

MR. MOORE: Grid spacer influences on local temperature?

MR. FORD: Yes.

MR. MOORE: No.

MR. FORD: Decreases in them?

MR. MOORE: No.

MR. FORD: It wouldn't simulate that? So that is it correct that if we assumed that there was eutectic alloy formation in an axial level including a grid and if we wanted to include heat from that the computer code would not distinguish between the location of this heat on the rod; it would just add it to the total heat?

MR. MOORE: That's correct for that computer code.

MR. FORD: So that your computer codes, if they were expanded to include this extra heat source and if it were in the extra heat at all for the hot power region, then it would give a conclusion of greater than 2300 degrees Fahrenheit.

MR. MOORE: Yes, and in correct conclusion.

MR. FORD: I see. But nevertheless the computer code that is recommended for use with whatever imperfections in it, if it were simply modified to take account of this one extra heat source it would get us above 2300 degrees Fahrenheit.

MR. MOORE: I question your representation.

CHAIRMAN JENSCH: Would it get you above the

2300, and then explain it. Can you say yes or no? Would it get you above the 2300?

MR. TROSTEN: Would you understand the question, because I don't.

MR. MOORE: I do now. I guess maybe --

CHAIRMAN JENSCH: Let's have it read.

(The pending question is read by the Reporter.)

MR. MOORE: No.

MR. FORD: Can you explain it?

MR. MOORE: Would you like a clarification?

MR. FORD: Just the facts, please.

MR. MOORE: Yes. If the computer code were corrected to take account of that additional energy, I would correct the computer code. That is not an imperfection in the computer code and I would correctly calculate the effects.

MR. FORD: I see. Now is it correct that there is already an imperfection in the code?

MR. MOORE: No.

MR. FORD: In that the specific respect that the grid depression of local temperature, that this is not simulated in the code?

MR. MOORE: That is not an imperfection.

MR. FORD: Well let me ask you this. Is it possible with your present techniques to simulate the presence of grid spacers all along the rod that you are doing your calculations for?

MR. MOORE: Yes, it is possible to calculate the axial conduction effects of grid locations.

MR. FORD: I didn't ask about the axial conduction effects. I asked the fact that the temperature is different at the grid spacers versus other areas. Are you capable of simulating that or would you have to greatly expand the number of axial levels that you consider?

MR. MOORE: You are talking about the temperature difference along the axis, right? That's axial conduction effects.

MR. FORD: Excuse me. Does that relate to the effects of one level on another? I am simply asking, computing the differences in temperature, not any conductance.

MR. MOORE: I understand. If I were to calculate the differences in temperature without the effects of axial condution the present code can calculate that difference by merely taking the power level at the grid and calculating the temperature versus time.

MR. FORD: I see. Now is there any experiment or data that indicates the effects of this simplification, avoiding the simulation of grid spacers, is there any calculation which indicates the sensitivity of the results to that simplification to the results?

MR. MOORE: Which simplification are we now discussing?

MR. FORD: Not simulating explicitly the presence of grids.

MR. MOORE: From what standpoint.

MR. FORD: In terms of the output of the code and the code as you know computes maximum cladding temperature and metal-water reactions and so forth.

Is there any data indicating the extent to which the present code's output would be changed if it were revised to explicitly simulate the effects of grids?

MR. MOORE: We are right back where we started some time ago where I said to explicitly calculate the effect of the grids and temperature calculations where I am calculating the effect of the flux depression at the grid that this is calculated by taking the appropriate local power level, as I do in the code now.

MR. FORD: I see. But you don't do this for a single rod. You take a lower power level in other rods. Is that what you answered before?

G2Bt2

MR. MOORE: Yes.

MR. FORD: So that in terms of the fact that you represent a region by single rod, for that single rod you do not simulate the presence of grids?

MR. MOORE: That is correct, but--

MR. FORD: But--

MR. MOORE: May I clarify it?

The simulation there, the only simulation that we would have to include that you are talking about is an axial effect.

MR. FORD: Yes. But I am asking you.

MR. MOORE: Do you agree with that?

MR. FORD: Well--

MR. MOORE: Well, you said yes.

MR. FORD: I am not the witness.

MR. MOORE: You said yes.

MR. TROSTEN: What is happening, Mr. Ford, is that you are continuing merely to ask the question over and over again. The witness keeps answering you and that is why he is trying to get you to either--

MR. ROISMAN: We will show that Mr. Trosten has already testified he doesn't understand the question. I think his comments as to what Mr. Ford is saying are out of place.

CHAIRMAN JENSCH: Let's go on without dispute here

GSBt3

between the witness and the interrogator? Let the interrogator proceed and the witness will be permitted to explain if he feels limited by the question.

MR. FORD: Dr. Roll, to what level do zircalloy water reactions have to be limited before they pose any threat to containment integrity?

DR. ROLL: Are you referring to this kind of a discussion here?

MR. FORD: No. I have completed the discussion of this particular eutectic. I am thinking in terms of the hydrogen evolved and so forth from the zircalloy water reaction, to what level would the reaction rate or the total amount of the reaction, rather, to what level have it to be limited before you threaten containment integrity?

DR. ROLL: Although you directed the question to me I think again I must refer to Mr. Moore because that's really his area.

MR. TROSTEN: We are looking for a copy of the reference in the safety analysis report or other appropriate document to which we can refer the interrogator.

CHAIRMAN JENSCH: I wonder if the question is somewhat unnecessarily complicated. Are you seeking to get the force that would be generated by a metal-water reaction and get that force measured and then we can compare it to this other phase of containment integrity?

2Bt4

MR. FORD: It's not a question of force. It's a question of by-products of the reaction, hydrogen, and so forth.

CHAIRMAN JENSCH: Maybe if you ask that type of thing. I think when you get into containment integrity I think we are thinking that you have to give some measurement. I don't know that Dr. Roll has indicated he has worked on containment integrity.

MR. FORD: I see. I am talking about metal-water reaction's contribution to containment problems. I am just trying to find out where the bounds are.

CHAIRMAN JENSCH: Well, if you can find out what the problem is with the metal-water reaction, and we will see regarding the bounds of the containment.

MR. FORD: Specifically as I clarify it for Dr. Roll in terms of hydrogen what is the maximum amount of metal-water reaction that can take place before you have to worry about containment integrity due to problems associated with hydrogen build-up?

MR. ROLL: You have redirected the question to me, but I am not at all involved in the calculations of containment integrity.

CHAIRMAN JENSCH: Are you seeking to find out what would be the maximum force the hydrogen build-up or incident? I know we can take a look at what the containment will contain.

DR. ROLL: I believe in the calculations, the total system calculations, the hydrogen evolution or the heat effect from the zirc-water reation is calculated, assuming the parabolic rate law, which I believe is an upper bound. That is other considerations would tend to make it less than that. Therefore, the source term in our calculations already are at a maximum level. Addition of water would tend to make the actual local situation more like what we have calculated it to be.

MR. FORD: I see. But I am asking in the parabolic rate calculations, at certain times you add emergency coolant water. It contributes to some metal-water reaction and it also cools. But as you continue this process at certain degraded situations, part of the time the water that you add is doing more cooling than it is generating heat. I am asking for what is the turn-around point when the addition of water begins to add more heat than it does cooling.

DR. ROLL: I don't know for the particular situation that we are talking about.

MR. FORD: I see. You don't know.

MR. TROSTEN: Dr. Roll, is this an area in which you are working?

DR. ROLL: No, it is not.

MR. FORD: That's relating to metal-water reactions, That would be Dr. Roll's field, the amount of force from a hydrogen buildup or incident, is that correct?

DR. ROLL: Yes. If we are relating to containment integrity I think that is another.

MR. FORD: If that is going to be answered by others in other areas, I will defer the question as well.

At what point, Dr. Roll, do metal-water reactions begin to add--zircalloy water reactions specifically, at what point do they begin to add so much heat that the addition of water at that point contributing to the reaction, the addition of that water would aggravate rather than mitigate the accident?

relating to --

DR. ROLL: I believe it is relating to the effects of a calculation of the heat input of the metal-water reaction and the simultaneous heat removal due to the presence of the water. I believe this is really a heat transfer calculation, the substance of which is reported in the documentation of the loss of coolant accident.

MR. FORD: I am trying to relate some computer code calculations for the Turkey Point plant with Indian Point 2. To facilitate this, if it can be done, can you tell me what the differences are in a power output and PWR system design between the Turkey Point pressurized water reactor and Indian Point 2?

MR. TROSTEN: Would you explain the purpose for this question? I don't quite understand it.

MR. FORD: There have been calculations, computer code calculations performed on the Turkey Point plant on a pressurized water reactor of Westinghouse relative to the metal-water reaction rate. I haven't seen the same calculations for this plant and I haven't seen the methodology, of course, in the proprietary Westinghouse code. I'd like to address a number of questions concerning the results of these computer code calculations.

As I understand it, at least in terms of basic design, these plants are first cousins. But in terms of some other influential parameters, I just want to get clear.

I presume that Mr. Moore would have the answer to this question.

MR. TROSTEN: May I ask, which Turkey Point plant are you referring?

MR. FORD: I am referring to Florida Power and Light Company, Turkey Point Nuclear Generating Units 3 and 4. The data I will be discussing is from a preliminary safety analysis report, Docket No. 50219, January 18, 1967.

MR. TROSTEN: Mr. Moore, are you familiar with the information that Mr. Ford is referring to?

MR. MOORE: Not the specific reference. I am familiar with the PSAR but not the specific references he is quoting.

H2Wt1

MR. FORD: The question was, before Mr. Trosten made his inquiry, what is the difference design between Turkey Point and Indian Point 2, overall power level, and so forth.

MR. MOORE: They are both Westinghouse pressurized water reactors. The Indian Point plan has four reactor coolants and the Turkey Point has three. The power of Turkey Point, the operating license power level is 2200 megawatts thermal.

MR. FORD: And Indian Point 2 is what?

MR. MOORE: 2758 megawatts thermal.

MR. FORD: Are there any differences in the emergency cooling system? Do both have accumulators?

MR. MOORE: Conceptually in the same systems they both have accumulators one on each cold leg.

MR. FORD: Do we have another copy of the McLain report? Do you have a copy of the Oak Ridge National Laboratory report? It is entitled, "Potential Metal-Water Reactions in Light-Water-Cooled Power Reactors." It is Exhibit M in this case by Howard A. McLain. It is dated August 1968.

MR. TROSTEN: You say Exhibit N?

MR. FORD: Exhibit M.

MR. TROSTEN: Excuse me. I beg your pardon.

MR. FORD: In the analysis of pressurized water reaction in metal-water reactions, beginning on page 149, completed on page 151, certain calculations for the Turkey

H2Wt2

Point reactor are reported. The calculations present metal-water reaction as a function of accumulator water in the pressurized water reactor.

CHAIRMAN JENSCH: You refer to two pages. I wonder if the witnesses shouldn't have an opportunity to review them. At this time is a convenient time for a recess.

MR. FORD: I'd like to read them out loud just to have everyone understand it. Is that helpful for the Board? I think that's my concern.

CHAIRMAN JENSCH: Yes, it would be.

MR. FORD: Why don't I give it to the Board as well.

CHAIRMAN JENSCH: You are not going to read two pages, are you?

MR. FORD: One large paragraph.

CHAIRMAN JENSCH: Very well. Proceed.

MR. FORD: They are discussing calculations with the Westinghouse code, assuming that the metal-water reaction rate as described by Baker and Just parabolic relation.

It says, and I quote, "Typical results of their calculations--" this is Westinghouse calculations as reported in the PSAR that I noted earlier. "Typical results of their calculations are those for the Turkey Point reactors. With the use of accumulators in emergency cooling systems, Westinghouse calculated that there will be less than one per cent metal-water reaction, and that the cladding will never reach the melting temperature for any size or break in the loss of coolant accident. If the contents of only one of the three accumulator tanks reached the core, there will be about five per cent metal-water reaction resulting from the double-ended failure of the large primary cooling piping. No accumulator action would result in about fourteen per cent metal-water reaction."

CHAIRMAN JENSCH: Would this be a convenient time to interrupt so they may study the context of this?

At this time we will recess and reconvene in this room at 10:45.

(A short recess is taken.)

CHAIRMAN JENSCH: Will you proceed with the interrogation, please.

MR. FORD: Dr. Roll, have you had an opportunity to study the pages 149 and 151 that I referred to?

DR. ROLL: We have reviewed it.

MR. FORD: The Westinghouse calculations of metal-water reactions with the parabolic rate law, that assumes, am I correct, an unlimited source of steam water to react?

DR. ROLL: That's correct.

MR. FORD: In the Turkey Point computations is that an assumption you would not have made?

DR. ROLL: In Turkey Point --

MR. FORD: In these computations reported here they refer as you do to Baker and Just's parabolic relation. Now does that mean that they make the same assumption about the unlimited availability of steam water to react?

DR. ROLL: That's correct.

MR. FORD: Now the result that they gave, that metal-water reaction is a function of accumulator water, have any experiments been done that simulated a primary loop core heat-up in conjunction with the accumulator water? Have any experiments been done on this large scale and the grade tests that contradict clearly the predictions of the Turkey Point computations for Westinghouse PWRs?

DR. ROLL: I believe that's a systems-related question.

MR. FORD: I am looking for a yes or no answer. Have these experiments under those conditions been done?

MR. TROSTEN: Can you give a yes or no answer to that?

MR. FORD: Well, we will all try to get an understanding of that one, too. Have the experiments been done?

MR. MOORE: The question was kind of long.

CHAIRMAN JENSCH: Reread the question, please, Miss Reporter.

(The pending question is read by the Reporter.)

MR. MOORE: No.

MR. FORD: In your calculations of metal-water reactions have you explicitly related them to accumulator systems action?

MR. MOORE: Yes. In that we use the accumulators to cool the core.

JWt1

MR. FORD: Yes. But have you assumed different portions of accumulator water reaching the core and computed metal-water reaction rates over a range of possible accumulator system injection rates?

MR. MOORE: Yes, with respect to containment capability.

MR. FORD: Have you computed the per cent of metal-water reaction with these different assumptions about how much accumulator water reaches the core?

MR. MOORE: Yes.

MR. FORD: Can you tell us if your computations that all accumulator water reaches the core will be at least one per cent metal-water reaction? Is that correct?

MR. MOORE: Repeat that, please.

MR. FORD: If all of the accumulator water reaches the core, there will be less than one per cent metal-water reaction during the loss of coolant; is that right?

MR. MOORE: Yes.

MR. FORD: If one accumulator fails to reach the core, what per cent metal-water reaction rate will there be?

MR. MOORE: The quoted metal-water reaction that has been stated in testimony previously, less than .05 per cent, since we spill one accumulator in the analysis.

MR. FORD: If no accumulator water reached the core, what per cent metal-water reaction rate would there be?

JWt2

MR. MOORE: That answer is stated in the FSAR on page 14.3.4-20.

MR. FORD: What is the per cent metal-water reaction rate?

MR. MOORE: As stated in there, the total reaction was 32.3 per cent.

MR. FORD: The computations that you performed indicate, for some assumptions of accumulator water, that there is less metal-water reaction than was presented in the Turkey Point computation, and for some you assumed that there is no-- you assume there is much more metal-water reaction.

My question is, assuming, for the moment, that--

MR. MOORE: That's not a fair statement. Can I comment on that? It is not a correct statement.

MR. FORD: Let me ask it in more specific detail. If all of the accumulator water reached the core, it is correct that both your present computations and your Turkey Point computations indicate that less than one per cent would react; is that correct?

MR. MOORE: Right.

MR. FORD: If one accumulator didn't reach the core for Turkey Point, they computed five per cent metal-water reaction rate.

MR. MOORE: That is incorrect.

MR. FORD: Excuse me. Less than five per cent metal-water reaction rates.

MR. MOORE: What's the case you are talking about?

MR. FORD: I am talking about the references we just have been studying, that the Turkey Point computations said that if the contents of only one of the three accumulator tanks reached the core, there would be about five per cent metal-water reaction resulting in the double-ended failures of the large primary cooling piping.

That the Turkey Plant indication I am referring to. There are three accumulators at Turkey Point and four heaters; is that correct?

MR. MOORE: Yes. That's where the confusion is. In some cases you are failing them and in some cases you are putting them in.

MR. FORD: To put things on an equal footing, for the situation where they were talking about one-third of the accumulator being lost, they computed five per cent.

MR. MOORE: Yes.

MR. FORD: You are talking about a fraction that is equal to one whole accumulator, I believe, and twenty-five per cent of the other three being lost. Is that your assumption?

In one of your calculations you assumed that all of the accumulator water from one accumulator is lost.

MR. MOORE: That's correct.

MR. FORD: And twenty-five per cent of the accumulator water from each of the other three is lost; is that correct?

MR. MOORE: Yes, that's the approximate number.

MR. TROSTEN: Do you understand which calculation we are referring to now, whether it is Turkey Point or Indian Point? I am getting confused as we go beetween the two. Do you understand the question?

MR. MOORE: I understood that to be Indian Point.

MR. FORD: And it was Indian Point.

CHAIRMAN JENSCH: I think if the witness has difficulty, he should indicate it. Then if the attorneys want to study the transcript later and resolve it later, they may. Proceed.

MR. FORD: So that in the Indian Point case, as I understand it, you are talking about approximately only half of the accumulator water rather than just a third of it being lost; is that correct? You take no credit for one entire accumulator; is that correct?

MR. MOORE: That's right.

MR. FORD: And then for twenty-five per cent for each of the remaining three?

MR. MOORE: Is thrown away.

MR. FORD: So that one and three-quarters out of the four is thrown away. So you take credit in this calculation for 2.25 accumulators; is that correct?

MR. MOORE: Yes. The arithmetic looks all right.

MR. FORD: So that you take credit for 2.25 accumulators so that you assume instead of -- So you assume almost fifty percent loss, yet instead of computing their five percent metal-water reaction rate you only compute as I understand it .5 percent, is that correct?

MR. MOORE: .05 percent.

MR. FORD: .05 percent. So even assuming that more accumulator water is lost you compute to orders of magnitude less metal-water reaction.

MR. MOORE: We are talking about different accumulators.

MR. FORD: Right, right, I understand. But --

MR. MOORE: We are talking about different core volumes, so we are talking about different volumes of water. We are talking about sixty-seven to seventy percent of the Turkey Point accumulator water being lost. That's one out of three.

MR. FORD: That's one out of three. Isn't that --

MR. MOORE: Excuse me. Then I have lost two out of three. I am only using one out of three.

MR. FORD: I see, I see. I didn't get that straight. Fine.

So that you are talking about a case that is slightly less accumulator water being lost but to orders of magnitude less metal-water reaction rate?

MR. MOORE: This is in terms of percentages of available accumulator water.

MR. FORD: Right. Now what experimental data do you have that indicates the sensitivity of metal-water reaction rates to accumulator water, and by experimental data I mean with a simulation, physical simulation of that entire primary loop core heating up and accumulators injecting the things. Do you have any experimental data of that development that demonstrates the sensitivity of, extreme sensitivity of the accumulator water to metal-water reaction rates?

MR. MOORE: No. There are no large-scale tests for the core. You are talking about a very complex chain of events. You are ending up with a zirc-water reaction. And you have to start with the loss of coolant and go through the blowdown, the reflood, the heat-up, the time and temperature, and then the zirc-water reaction.

MR. FORD: Right. Now in terms of simply your experimental philosophy do you see the necessity, since there are as you note so many complicated factors behind any independent phenomenon, do you see the necessity, the experimental necessity for the kind of integral test that I am talking about or do you think that you can just test individual small components of the problem, you know, assuming all the input from other phenomena?

MR. MOORE: I believe it is my opinion that we can properly bound the calculation without a total completely integrated test.

MR. FORD: I see. Now by properly bound meaning the mathematical sense you can study this particular phenomenon, but in terms of experimental confirmation is an experiment on simply a part of the whole phenomenon, forgetting the blowdown, forgetting about heat-up, just starting somewhere well into the transient, and assuming all the divisions that have been established by blowdown, do you think that we need experiments that simulate in one sophisticated experimental situation everything together rather than using mathematical approximations that just tie together models and models and models?

MR. MOORE: That's a rather long question.

MR. TROSTEN: May I ask Mr. Ford a question, when he uses the term experimental, what experiments? I think that it's hard for me to understand the question and it's hard I think for the question to be responded to.

MR. FORD: I am talking about the water reactor safety program which has a variety of experiments on different -- Using a variety of different equipment to simulate loss of coolant accident situation. And I am talking about some of the large-scale experiments that are planned to take place into 1975 or so in which we will actually have a live reactor and have it subjected to loss of coolant transients and see what happens. I am talking about whether or not that is necessary in Mr. Moore's opinion, whether that would make a substantive contribution to the confirmation of these results on metal-water reactions inasmuch as they depend on all the other phenomena of the transient. I am asking him whether that is necessary or whether you can simply take Baker-Just's correlation, which is derived from experimental data that is completely outside of the context of nuclear systems?

I am asking him whether we should have these kinds of integral experiments or whether we can just take empirical correlations and just use them with no hesitation..

MR. MOORE: I count at least four or five questions in it. Do I think it necessary, do I think it would contribute?

MR. FORD: I am purposely trying to find out what your philosophy is, what you regard as convincing experimental confirmation of, in this particular case, the metal-water reaction rates that you compute.

MR. TROSTEN: Is there an outstanding question?

MR. MOORE: I was just waiting for him. I was waiting for you to finish your conversation.

MR. FORD: Thank you.

MR. MOORE: In my opinion the totally integrated test is not necessarily a prerequisite to describe a physical phenomenon and in the case of the loss of coolant I don't think it is a requirement. I think you can get very good indications of what phenomena do occur with these separate effects kinds of experiments that have been performed. With respect to zirc-water reaction I would point out that we have come very close to simulating this through the FLECHT test.

MR. FORD: Now in terms of the water reactor safety research program would you tend not to think that the integral tests were ever really worth their expenditure?

MR. MOORE: I didn't say that. Are you asking that question?

MR. FORD: Yes.

MR. MOORE: It's my opinion we will get useful information out of that test, yes.

MR. FORD: Are there any specific uncertainties that in relation to which the output of these tests will provide useful information?

MR. MOORE: None specifically that I am aware of.

MR. FORD: In terms of the experiments pertaining to accumulator water, are there any that have confirmed in any kind of integral way your own metal-water prediction for Indian Point 2?

MR. MOORE: I am again having trouble relating between metal-water reaction and accumulators. Could we repeat the question again? That's a long train, from the accumulator to the metal-water reaction.

MR. FORD: I see. Well your prediction of metal-water reactions as a function of accumulator water, the total reaction rate, has that prediction of yours been confirmed by any experiments?

MR. MOORE: No specific experiment, complete integrated experiment.

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MR. FORD: I refer you to page 119 of the McLain report. It is a small quotation that I wanted to read. I will read it as you study it. It says, "For large double-ended pipe breaks, it is conceivable that very little water is left in the pressure vessel. The amount of water left in this vessel is important because it is the source of steam for the possible metal-water reaction in the reactor core."

Do you agree with this statement?

MR. MOORE: No.

MR. FORD: What specific aspects of it do you disagree with?

MR. MOORE: I disagree in the context that the way the analyses are performed in that we don't care whether there is a source of steam or not with respect to metal-water reactions.

MR. FORD: I realise that. I am asking the question itself, though. It is true, is it not, as the statement asserts, that steam does indeed, its presence does influence the reaction? I am not talking about what you assume.

MR. MOORE: Yes.

MR. FORD: So that if you considered steam limitations on the reaction, assumptions, conservative assumptions from the point of emergency cooling in terms of loss of accumulator water, those conservative assumptions would be nonconservative from the point of view of metal-water reaction rates, assuming steam limitation at those rates; is that correct?

MR. MOORE: Yes, assuming steam limitations of the rates, which we do not do.

MR. FORD: Is the containment atmosphere filled with air?

MR. MOORE: Yes.

MR. FORD: Is it possible that when vessel to pressurization is fully completely, that is when discharge of primary coolant has ceased through the break, that some quantity of air from the containment will enter the reactor pressure vessel?

MR. MOORE: I doubt it.

MR. FORD: Is there any experimental data from containment systems, experiments, and so forth, that indicates that air would not be able under any circumstance to enter the vessel after the completion of blowdown.

MR. TROSTEN: Before you answer that question, Mr. Moore, Mr. Roisman, am I correct in recalling that this is the line of questioning that pursued with Mr. McAdoo? Am I correct, Mr. Roisman, in recalling that you questioned Mr. McAdoo as great length about the subject that Mr. Ford is now starting to question Mr. Moore about? You have to tell me whether that is so or not. I think it is but I am not absolutely sure.

MR. ROISMAN: Let me talk with him a moment.

CHAIRMAN JENSCH: Do you have the transcripts?

MR. TROSTEN: We can get them.

CHAIRMAN JENSCH: Maybe he can recall that.

MR. ROISMAN: To just answer Mr. Trosten's question, it is true that at one time we did discuss it, but in terms of hydrogen production, the question of whether you would have air that would get into the reactor vessel and also talk about air pockets and possible exposure. I think it was with Mr. Wiesemann, as a matter of fact.

MR. TROSTEN: No, Mr. McAdoo.

MR. ROISMAN: In any case, that is not the same subject, although perhaps a couple of the initial questions are getting at the same subject.

MR. TROSTEN: Thank you.

CHAIRMAN JENSCH: Proceed.

MR. FORD: The question outstanding is whether or not any experimental data in containment systems experiments from any of the blowdown experiments that have been performed, whether that data indicates in any way that it clearly indicates that it would not be possible for air to enter the reactor pressure vessel from the containment at the end of blowdown.

MR. MOORE: I don't know.

MR. FORD: I see. Is there any experimental data evolved by Westinghouse that clearly indicates that it is not possible for this to take place?

MR. MOORE: No.

MR. FORD: In terms of the pressure differences in the containment at the defined end of blowdown and in the reactor pressure vessel, as defined in blowdown, is it clear that there are no circumstances with those relative pressures that air could enter the reactor vessel, assuming, for example, that the injection of emergency coolant water were delayed for some small period?

MR. MOORE: It is really difficult to say. As far as my knowledge is concerned, I think there will continue to be a small pressure difference from sources of steam within the system which will act in the direction of continuing to pushsteam out of the system into the containment; therefore preventing air from coming back in.

MR. FORD: Can you give us the order of magnitude for the small pressure difference?

MR. MOORE: It could be a few psi above the containment pressure.

MR. FORD: I see. Is it possible, from any phenomena occurring in the containment, that in their non-equilibrium phases that they could raise containment pressure at the end of blowdown to be just the other way around, to be one or two psi more than in the vessel?

MR. MOORE: I'm not aware of any.

MR. FORD: You deny, as I understand, that it is possible for air to enter the containment although there is no experimental or analytical data to support this?

MR. MOORE: You are talking about air into the vessel system?

MR. FORD: Yes, into the vessel system.

MR. MOORE: I just don't see that that could be the case just from basic fundamental understanding of flow and pressure differentials.

MR. FORD: I am going to ask questions, assuming this is the case and assuming that there is some quantity of air in the reactor vessel from containment after blowdown. I am going to direct my questions now to Dr. Roll.

At this time at the end of blowdown, is it correct that the maximum core temperature will be within the range of 300 degrees Centigrade and 1500 degrees Centigrade, which is the range of 582 degrees Fahrenheit to 2732 degrees Fahrenheit?

The question to you, is it correct that at this time the core temperature will be, a maximum clad temperature will be within the range of 300 degrees Centigrade and 1500 degrees Centigrade?

DR. ROLL: That is correct, it will be within that range of temperature.

MR. FORD: Assuming that there is some fraction of the rods assumed to have perforated, do you assume that 100 percent of the rods have perforated in your code analysis? Is that correct?

MR. MOORE: As Mr. Wiesemann indicated in his testimony on Monday, yes.

MR. FORD: Under the conditions of temperature between 300 degrees Centigrade and 1500 degrees Centigrade, and assuming the presence of air, is it correct that under these conditions uranium dioxide will react with air, and that U_3O_8 will be formed?

DR. ROLL: I believe that the answer is yes theoretically. But one may wish to qualify that in consideration of time and reaction rate kinetics that could make the answer practically no.

MR. FORD: Would you turn to page 81 of the McLain metal-water reaction. He, in this literature, cites the data on this point. It says, and I quote, "The presence of oxygen or air enhances the oxidation of UO_2. The concern for the UO_2 air reaction is that

U_3O_8 will be formed when the UO_2 is in the temperature range of 300 to 1500 degrees Centigrade."

DR. ROLL: That's what it says, correct.

MR. FORD: There is no qualification there of the kind that you have given. Can you give us a reference for this qualification?

DR. ROLL: I believe if we use this author's reference itself, there is some discussion in there of the kinetics of this reaction under a variety of conditions.

MR. FORD: I'm sure.

DR. ROLL: I'm not sure. I believe it is there. I'm not going to quote the secondary reference of this author, McLain's reference, 151. I believe in there there is some discussion of reaction rates.

MR. FORD: Discussion. But in terms of the qualification that -- As I understand it, you would believe that it is more likely than not that this reaction did not occur?

DR. ROLL: Practically speaking, it is likely that it will not occur.

MR. FORD: In terms of that specific assertion, can you set forth any experimental or analytical data that supports your position?

DR. ROLL: That's what I just attempted to do. I believe that in McLain's reference 151 there is some discussion of this reaction under a variety of conditions. I believe the conclusions that generally the reaction is slow and will not proceed to a significant extent is there.

MR. FORD: Is there any experimental data pertaining to the question?

DR . ROLL: For the third time, I believe in this reference they are quoting experimental data which supports my first statement.

MR. FORD: I see. If the uranium dioxide were converted to U_3O_8, if this took place, would it then thermodynamically be expected to react with the zircalloy cladding?

DR. ROLL: I believe that the reaction of $U3O_8$ and zirconium metal is thermodynamically possible.

MR. FORD: Do you have any experimental data that indicates the likelihood of this possibility?

DR. ROLL: No. Westinghouse has not run experiments to this extent.

MR. FORD: Thank you.

Do metal-water reactions, specifically the standard zircalloy-water reaction, do they influence the formation of methyl iodide?

DR. ROLL: I believe you perhaps caught us both unprepared to answer that question. Can you follow it up?

MR. FORD: Yes. Let me give you the rationale why they may.

If you will turn to page 95 of the McLain report on potential metal-water reactions in water cooled reactors it states --

DR. ROLL: Where are you reading from, please?

MR. FORD: Page 95. It's a section called Effect of Metal-Water Reaction on the Nature of the Released Fission Products.

It states "They, that is metal-water reactions, cause higher fuel element temperatures and formation of eutectics that tend to release greater quantities of fission products.

"Second, they, metal-water reactions, create a reducing atmosphere around the fuel pin that influences the chemical nature of the fission products leaving the fuel element. Some of the fission products in which this is noted are ruthenium, tellurium, cesium and iodine."

DR. ROLL: Excuse me. Can we get together on this for just a minute?

MR. MOORE: Mr. Ford, neither Dr. Roll nor myself are involved in the iodine aspects of the accident. That question would be more properly handled by another witness, perhaps Mr. Wiesemann.

MR. FORD: I see. Well just while we have Dr. Roll here, speaking as a chemical engineer is there any data that you know of, analytical or experimental, that refutes the statement of McLain?

DR. ROLL: I am personally unfamiliar with any data which either refutes or supports the contention of Mr. McLain.

MR. FORD: May I ask Mr. Moore to your knowledge the relase of iodine and its change from elemental to organic forms, that your analysis of that does not include this consideration of this effect of the metal-water reaction?

MR. MOORE: That's not really my area.

MR. FORD: I'm just asking to your knowledge. You don't do this?

MR. MOORE: To my knowledge I don't know.

MR. FORD: Thank you.

MR. TROSTEN: This really is witness McAdoo's area.

MR. FORD: Dr. Roll, can you tell me what experimental programs on metal-water reactions are part of the Atomic Energy Commission's water reactor safety plan? I have looked through the various documents pertaining to that plan and the whole safety research program and I don't find metal-water reactions at all a subject of research. Is that your impression as the cognizant person in the area?

DR. ROLL: I believe I likewise am unfamiliar with a specific program in the AEC, overall program related specifically to this problem, perhaps indicating that the work has been done and reported, has been accepted and no further additional specific work is deemed required.

MR. FORD: Perhaps.

Does Westinghouse have any research underway on any aspect of metal-water reactions?

DR. ROLL: We have none.

MR. TROSTEN: May I interrupt the questioning to ask this. I understand the thrust of the question to be what is the AEC's water reactor safety program, does it cover metal-water reactions. There is evidence in the record as to what the AEC's water reactor safety program is. It seems like sort of a poor way to go about it to ask the witness as to what the record shows.

CHAIRMAN JENSCH: Assuming that's the premise, let's get on with the question. If you don't like what he is doing we can get up and argue it, but let's go on with the questioning.

MR. FORD: Under loss of coolant accident conditions can the zircallow cladding absorb, if there were any, an ounce of hydrogen?

DR. ROLL: During the conditions?

MR. FORD: Yes.

DR. ROLL: Yes, it can.

MR. FORD: Assuming an unlimited supply of hydrogen, what is the potential for the formation of zirconium hydride? Can you give us the form for that reaction?

DR. ROLL: Assuming an unlimited potential of hydrogen?

MR. FORD: Unlimited supply of hydrogen.

DR. ROLL: I believe then you would ultimately, given time and temperature conditions, get 100 percent conversion to zirc hydride.

MR. FORD: Can you tell me what effect this conversion to zirc hydride has on the embrittlement of zircalloy cladding?

DR. ROLL: I believe it's well documented that the presence of zric hydride in zircalloy cladding tends to make the material say less ductile or more brittle.

MR. FORD: In terms of the zirconium oxygen reaction, assuming unlimited supply, assuming two cases: Assuming you have a rod under loss of coolant conditions with an unlimited supply of steam and assuming in my second case that you have a rod with an unlimited supply of hydrogen.

Which formation, zircalloy oxide or the zirc hydride would have more significant effects on cladding embrittlement?

DR. ROLL: The question is somewhat academic because neither zirc oxide nor zirc hydride have much load-bearing capability. Therefore if you set up a totally hypothetical case of unlimited supply of water or steam or unlimited supply of hydrogen and you put no qualifiers on it for time and temperature, then obviously at some time you are going to have complete conversion to zirc oxide or zirc hydride, and I think it's splitting hairs to say which is better. They are both bad but they are both hypothetically--they are practically impossible to achieve.

MR. FORD: Right. I think the example is a little too drastic.

Assuming equal quantities of oxygen available to a rod and equal quantities of hydrogen available and equal volumes, which formation, zirc oxide or zirc hydride, would be the more problematic one from the point of view of subsequent effects on cladding integrity?

DR. ROLL: I really think again we have to qualify the question a little bit more because equal quantities of both are going to lead to a condition in the cladding again, depending on time and temperature and extent of reaction and similar things, and I really don't think that the question that you are asking is going to lead us to a meaningful answer.

CHAIRMAN JENSCH: Well, assuming that it does.

DR. ROLL: I don't know. Given the question that is stated I don't know the answer, but I think the question is still sufficiently general that I can give a sufficiently general answer that says both zirc oxide and zric hydride are deleterious to subsequent mechanical integrity of the cladding.

CHAIRMAN JENSCH: You don't know?

DR. ROLL: I don't know, but again it's a question of degree.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Has Westinghouse or has anyone to your knowledge performed any series of tests relating to zirc hydride formation under loss of coolant accident conditions?

DR. ROLL: To my knowledge I can refer you to our own FLECHT tests and there is a publicly available document which summarizes the results of our FLECHT tests and in these I believe that the degree of hydride pickup in the cladding is reported.

MR. FORD: I see. Were those tests varying parameters with a specific point in mind to obtain data on zirc hydride formation?

DR. ROLL: The objectives of the FLECHT program are really to determine heat transfer information.

MR. FORD: Yes.

DR. ROLL: As a subsidiary benefit we did look at the post-test conditions of the rod. So the tests were not defined and the parameters were not selected with the objective of obtaining detailed information on the formation of zirc hydride.

MR. FORD: So the answer to my question is no.

Is it correct that the stainless steel structure in the core region can form low melting eutectics with any zirconium that may come in contact with it?

DR. ROLL:, That's correct.

MR. FORD: Is it correct--

DR. ROLL: That's correct in the sense that there is a zirconium-chrome eutectic and chrome is a constituent of stainless steel. So in that sense it's a similar--we are looking at the elements that are present and there are eutectics formed with the elements that are present.

MR. FORD: Is it correct that the zirconium iron phase diagram indicates that these eutectics between stainless steel and zirconium, that these could have melting points as low as 924 degrees Centigrade?

MR. TROSTEN: May I have a reference to the diagram to which you are referring, Mr. Ford?

MR. FORD: I am referring to the McLain report Potential Metal-Water Reactions in Light Water Cooled Power Reactors, and the information here is contained on page 8 and it presents --

MR. TROSTEN: Page 8?

MR. FORD: Page 8.

MR. TROSTEN: Thank you.

MR. FORD: Are you familiar with that page?

DR. ROLL: I have the page here. I am not familiar with the reference but I will assume that it is quoted correctly.

CHAIRMAN JENSCH: While we pause I wonder if I got the correct answer. I think the previous question you asked, if Westinghouse had any tests which would indicate the comparative deleterious effects of zirc oxide or zirc hydride and you referred to the FLECHT test and then you asked, "Well, what do the FLECHT tests show?" and I think your answer was , well, that they really weren't set up to get this.

So are we back to the situation that Westinghouse does not have any tests, experimental data, to see whether zirc oxide or zirc hydride is worse than the other, is that correct?

DR. ROLL: I wonder if I could have the question reread, the question that I answered, where my answer was the FLECHT tests were heat transfer tests. I wonder if I could have that question reread. I didn't understand it as you understood it.

CHAIRMAN JENSCH: Do you have it, Miss Reporter? Let's obviate that. Give me the answer.

DR. ROLL: The question I thought I was asked is were the FLECHT tests set up to determine specifically the nature, degree, et cetera, of the zirc hydride reaction. My answer was -- Unfortunately I gave my answer in the wrong order. My answer was no, because it was a heat transfer test, but we did obtain information and have reported said information, relevant information, that we had reported this information in the --

CHAIRMAN JENSCH: Does that show whether zirc oxide or zirc hydride is worse than the other?

DR. ROLL: It does not show which is worse. It merely discussed the uptake of the hydrogen.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Is the form of the discussion such that it's just a report, "There was such and such a zirc hydride formed"? Is there any theoretical analysis of the formation there, of the conditions that brought it about, and of, you know, the general significance of that result?

MR. TROSTEN: If you have a page there would you read it?

DR. ROLL: If I may, can I quote for the record our report of the hydrogen uptake? I am quoting from WCAP 7665, page B-22, paragraph B.4.2. "Although hydrogen analyses were limited to a few selected samples, the absence of visual hydride needles in the microstructures examined, and the low hydrogen values determined quantitatively were persuasive evidence that hydrogen uptake was a low proportion of the potential available hydrogen from the metal-water reaction."

Let me continue then. Next paragraph.

"While it was not confirmed in these tests it would be anticipated from the work of Brown and Hardie that hydrogen precipitation would be influenced by the oxygen diffusion and of the alpha zirconium layer (and of course the oxide film) would contain little or no hydrogen. Excluding the tests of Run 9573 from which hydrogen data was not available, the maximum observed thickness of zirc oxide plus alpha zirconium in this work was less than 2.7 mils or approximately twelve percent of the initial clad thickness. Therefore, the concentration of hydrogen in the remaining thickness of the clad would be scarcely detectable, given the variants, the hydrogen content observed and the limits of analytical accuracy."

That's the nature of our discussion of the hydrogen pick-up in the FLECHT test.

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MR. FORD: I see. For example, the main determinant of zirc hydride absorption, it is correct that in addition to the presence of hydrogen, the main determinant is pressure; is that correct?

DR. ROLL: No clear.

MR. FORD: Is there any experimental data that demonstrates that pressure is not a significant determinant of zirc hydride?

DR. ROLL: There is not to my knowledge experimental data that suggests that pressure is the main determinant. I believe that was the direction of your question.

MR. FORD: No. It wasn't. My point is, is pressure a significant--not the main necessarily. Is it a significant factor in the formation of zirc hydride?

DR. ROLL: Yes.

MR. FORD: Is the pressure involved in the FLECT test influencing zirc hydride formation, was that pressure measured locally in the area of formation?

DR. ROLL: I believe it was brought out in discussion yesterday in a different context. There were pressure sensors within the bundles in the FLECHT test.

MR. FORD: There are two pressure tabs. I am talking specifically in the locations of the rods in which they were inspecting for zirc hydride. Was there any direct measurement in those locations?

DR. ROLL: Let's check the report and see.

Mr. Ford, we suspect there were not detectors in these areas where cross sections were taken of the qualification. There were pressure--

MR. FORD: Two pressure tabs, yes.

DR. ROLL: Along the length of the assembly. These, I believe, show very small pressure differentials along the length of the assembly.

MF. FORD: So that in terms of zirc hydride as a function of pressure, there is no relevant data since pressure wasn't a parameter, the pressure was varied?

MR. MOORE: We were talking--

CHAIRMAN JENSCH: Can he answer that question yes or no?

MR. MOORE: No. The answer to the question is there is relevant data.

MR. FORD: I am saying, since we are looking at the influence of pressure upon zirc hydride, since there is no data on different pressures, there can be no data on its influence. It is only one pressure, is that not correct?

MR. MOORE: No, that is not correct, in the sense you must understand we are talking about pressures of, for example, 60 pounds in a test assembly and talking about differential pressures within the assembly, or along the assembly of two to three psi at most. So that's the largest pressure difference we are talking about that could exist of putting that on perpespective.

MR. FORD: Simply in terms of any kinds of sensitivity analysis relating differential pressure to zirc hydride formation, the results of the FLECHT test, is it correct that they don't contradict your previous statement that you don't have any experimental data explaining the mechanisms, and so forth, of zirc hydride formation?

DR. ROLL: No, we do not have a series of tests run to give us theoretically the effect of pressure on hydride uptake.

MR. FORD: Thank you.

To return to thestainless steel eutectics that we were discussing, I believe we ended with your non-disagreement with McLain to the effect that these eutectics could have a melting point at least at 924 degrees Centigrade.

Do the Westinghouse computer codes account for the formation on this eutectic and its interaction with cladding temperature, local blockage and so forth.

DR. ROLL: They do not.

MR. FORD: On the Baker-Just equation we were discussing yesterday, it is correct that this equation assumes that all the reacted metal is converted into stochiometric zircalloy dioxide?

DR. ROLL: I believe so.

MR. FORD: Is this a simplification of the situation?

DR. ROLL: No.

MR. FORD: You deny that there are lesser oxides of zircalloy formed in the metal-water reaction?

DR. ROLL: To my knowledge, work that has been done, post test evaluation of work that has been done in these zirconium-oxygen reaction indicates ZRO_2 is the product.

MR. FORD: Is there no observation of a lesser oxide, ZRO 1.97?

DR. ROLL: To my knowledge, to repeat, the product is essentially ZRO_2.

MR. FORD: Excuse me.

DR. I say, you have a reference in front of you there. I wonder if--

MR. FORD: This is McClain again, page 14, in which he states, and I quote, "Actually some of the oxygen from steam forms a solid solution and lesser oxides such as ZRO 1.97 with zirconium."

He goes on to contend that in terms of the experimental data that he has, that this is insignificant. I'd like to find out whether you find that true or not. You don't have any data at all? You have never observed this?

DR. ROLL: We have experimental data that I believe supports his contention.

MR. FORD: In your experimental data, have you explicitly measured the formation of these lesser oxides?

DR. ROLL: The work that we are reporting in the FLECHT report, I believe --

CHAIRMAN JENSCH: Could you just give it to us? Have you explicitly measured the lesser oxides? Then you can explain what you wish to.

DR. ROLL: I don't wish to be put in a position of agreeing the $ZRO_{1.97}$ necessarily exists. I am not a basic metallurgist. I say, to my knowledge, the product is ZRO_2. Therefore, I don't wish to answer the question directly because that infers that I am agreeing with the entire premise here.

MR. FORD: No. The question is, in your metallurgical evaluations of these metal-water reactions, have you measured explicitly and have you looked for these lesser oxides?

DR. ROLL: We have looked across a cross-section of zirconium and zirconium oxide samples, and to my knowledge, have not seen or reported anything other than ZRO_2 and zirconium, and perhaps solid solutions of these two materials.

MR. FORD: The techniques that you used to look across the cross-section, do these techniques have the demonstrated capability for finding lesser oxides or identifying them?

DR. ROLL: I believe -- I would rather not comment on that. I'm not a basic metallurgist and I am not familiar in detail with the techniques used, say, in particular, in the FLECHT report or in other work we have done. Therefore, for me to comment on the sensitivity of a method we are using to detect suboxides, I think would be more hearsay than in fact factual evidence.

MR. FORD: Still on the Baker-Just equation, could you turn to page 34 and 35 of the McLain metal-water reaction report. I am concerned here with Section 2.1.3 reaction rates. That's metal-water reaction rates I am concerned with. That is Figure 2.3, a comparison of predicted and experimental rate data with zirconium-steam reaction.

It presents the parabolic relationship derived by Baker and Just. It presents the equation for that. Is that the equation you use in calculating the zircalloy-water reaction? That's the Baker-Just equation.

DR. ROLL: Yes.

MR. FORD: The suggestion is that, in reading, "White, in a recent investigation, suggested the following modification."

There is another parabolic rate equation of the metal-water reaction.

Do the computations that you perform on

metal-water reactions, in that do you use Baker-Just solely or have you performed metal-water reaction rate computations using White's modification of the Baker-Just equation?

DR. ROLL: I don't know personally whether or not other equations have been looked at in the context of the loss of coolant accident evaluation.

MR. FORD: But in your own evaluation of metal-water reactions that you are testifying to, have you used the modifications suggested by White?

DR. ROLL: No, we have not.

MR. FORD: Are you familiar with the modification suggested by White? Have you studied reference seventeen here?

DR. ROLL: We have not gone into detail in White's reference or gone into detail in the nature of White's work.

MR. FORD: Let's go into some small detail about this work and turn to Figure 2.3. Figure 2.3 presents, as it indicates, the prediction from Baker and Just's equation, the prediction from White's equation, and the variety of experimental data from Baker and Just, Boster and Lemon and White. This data indicates, am I correct, in looking at the chart, that at lower temperatures, White's rate equation predicts a higher metal-water reaction rate than Baker-Just?

MR. TROSTEN: Dr. Roll, you have indicated in response to Mr. Ford's question, that you are not really familiar with this work. Do you feel that you are able now, in response to these questions, answer these questions, or would you prefer to study them and then be in a position to respond to question about this chart that you said you haven't looked at or that you are not familiar with? Which would you prefer to do?

DR. ROLL: I would prefer not to get into the discussion of White's work. However, I can't answer this recent question.

MR. FORD: I intended to confine my questioning here to this chart. I presume Dr. Roll would stipulate that the prediction curves are correctly drawn and so forth, and discuss the implications of them with me, at least for the sake of the discussion.

DR. ROLL: Fine. Proceed.

MR. FORD: We can introduce evidence at a later date to produce evidence that the curves are incorrectly or correctly drawn and so forth.

My question is, is it correct that White's rate equation predicts higher metal-water reaction rates at low temperature then Baker and Just's rate equation?

DR. ROLL: At the low temperature regime, it is correct that the curve labeled "White's rate equation" predicts higher rates than the curves labeld "Baker and Just rate equation."

MR. FORD: And at higher temperatures it is just the other way around, isn't it, that White's rate equation preducts lower parabolic rate constants than Bak er and Just rates equation?

DR. ROLL: That's correct.

MR. FORD: Just back on the chart, to talk about the magnitude of these changes, it is very close together but a semi-long scale which makes smaller differences really much larger. It is correct, for example, that when we are talking about these differences in metal-water reaction rates between these two curves, it is correct that on the lowest portion, on the lowest temperatures -- Do you agree with me that we are talking about twenty-five percent differences in the prediction of metal-water reaction rate for temperatures on the order of 1000 degrees Fahrenheit? So that the maximum difference under low temperature conditions -- It seems to me that just reading the chart here, that for the lowest temperature considered, White predicts .5 percent metal-water reaction whereas the Baker-Just equation predicts .37 something or so. So that there is, you know, on the order of White's equations, on the order of twenty-five percent higher than

Baker-Just.

MR. TROSTEN: Do you agree with that?

DR. ROLL: I agree with that.

MR. FORD: In terms of the data supporting the curve here, in terms of the different parameters in the two equations -- You don't have to go into White's work to see the parameters here. Do you have any information any experimental information that would call into question White's parameters?

DR. ROLL: We have none.

MR. FORD: Just in the form of his modification of Baker-Just's equation, do you have any basic criticism on the basis of the modification as reported by this equation here?

DR. ROLL: Again, I'm not familiar in detail with Mr. White's work and therefore it is probably inappropriate for me to comment on a basic difference.

MR. FORD: I am simply saying in terms of your work with which you are familiar, would you have made the modification in Baker-Just that he did, and for your reasons based on your data?

DR. ROLL: The data which we have and which has been reported was looked at merely as a confirmation of Baker-Just, and we did not independently attempt to arrive at a separate equation.

MR. FORD: Just looking at the curves here, would it be correct to say that if we wanted to estimate metal-water reactions conservatively, and the option was between Baker and Just's equation and White's equation, which would we pick for lower temperatures?

MR. MOORE: I will answer that question.

MR. FORD: I am talking about metal-water reactions.

MR. MOORE: You are talking about the metal-water reaction that exists during loss of coolant transient.

MR. FORD: I'd like to have Dr. Roll's answer. If you care to comment on direct testimony, I'm sure I will be glad to have it.

MR. TROSTEN: Dr. Roll, are you competent to answer the question? That's the important point, not whether Mr. Ford wants you to answer it.

CHAIRMAN JENSCH: I think this is important, also. We have had the suggestion from Applicant's Counsel at several times to different witnesses, do you feel you are competent to answer. I know Applicant's Counsel doesn't intend this, but almost has the impression to tell the witness, don't answer, because you really are competent. This gentleman, as I understand it, has worked on metal-water reactions. In order to pick which of two choices to be a more conservative estimate, this apparently is in his field. Maybe Mr. Moore would say, well, you may do that but as a matter of policy, I will override it. I think the question is to this gentleman, what he believes is more conservative from a metal-water reaction. That's why he is called here. I think when you turn to the witness and say, well, I really think you are competent to have this understanding, it is almost like telling him, don't answer. All you have to say is that you are not competent. I don't think that suggestion should be constantly reminded to the witness. All witnesses know that as mentioned them several times. I don't think we need repeat it every time.

MR. TROSTEN: I agree with the Chairman's point. The only point I am attempting to convey, Mr. Chairman is this: We have witnesses present here who are experts in specific fields who have been called by the Applicant or called by the Intervenors to testify in certain fields. When a question is directed by the Intervenors' Counsel to a witness who is a technically trained person and not a legally trained person, and he doesn't fully appreciate whether he is somehow obligated to give opinions in areas where he isn't an expert where he hasn't been called, I spoke. That's the reason why I am trying to make it clear to the Intervenors' Counsel which just of our witnesses have been called for which purpose.

That's the only reason I was making that suggestion.

MR. FORD: It is very clear to me that Dr. Roll is a competent person to discuss metal-water reactions. I'd like, if I may, to pursue my question.

CHAIRMAN JENSCH: Proceed.

MR. FORD: Dr. Roll, the outstanding question is, if we wanted to choose a conservative estimated procedure concerning metal-water reactions, and we are given the data of this chart, the relationship between Baker and Just rate equation and White's rate equation, which would we choose, focusing on the lower temperature half of the spectrum?

DR. ROLL: As we have just recently mutually agreed, at the low range of the graph. The White equation gives a twenty-five percent higher rate than the Baker-Just equation. Therefore, let me answer your specific question by saying, yes, at the low temperature range you would use the White equation. Let me further proceed, however, to defer to Mr. Moore and have him repeat some of the information he gave yesterday on the importance and relevance of the rate selection in this low temperature range.

MR. FORD: Excuse me. I have a line of questions which I'd like to pursue with you.

DR. ROLL: I believe the direction I got was that

I could answer yes or no but I was also given the opportunity to put qualifications on my answer, yes or no, and I think it is important that we put this qualification on the relevance of that answer.

MR. FORD: Your qualifications are appreciated, but I want not Mr. Moore's --

DR. ROLL: You understand what my qualification was going to be?

MR. FORD: You qualified your answer there.

DR. ROLL: My qualification was going to be the relevance of that answer to the overall scheme of things.

CHAIRMAN JENSCH: Maybe Mr. Moore said it yesterday and then it won't have to be said today.

DR. ROLL: Is it apparent to all?

CHAIRMAN JENSCH: Give us the transcript on that sometime and we will take it from that point. Will you proceed.

MR. FORD: Is it possible, in your computation of the metal-water reaction rate, to use different rate equations over different temperature ranges?

Oh, yes. It's mathematically possible.

MR. FORD: I see. Now when we go to the high temperature range I am sure you will agree readily that Baker & Just's equation is more conservative than White rate equation, is that correct?

DR. ROLL: That's obvious from the graph.

MR. FORD: Right. So that if we wanted in terms of the mechanics of the computer code, that continuously computes the metal-water reactions, we could use instead of simply Baker-Just or simply White's rate equation we could use both of them in conjunction, switching from one to the other, approximately 1200 degrees Centigrade or so, is that correct?

MR. ROLL: It's mathematically possible to do that.

MR. FORD: I see. So in terms of your temperatures of reference, however, with regard to the interim criteria is it correct that we would be making our computations, almost all of our computations then, with White's rate equation?

MR. ROLL: The answer is really dependent upon where we determine crossover.

MR. FORD: Right. Well, assuming--

DR. ROLL: And it looks like the crossover point is approximately in the middle of the temperature range covered by the peak rods which are of course the rods of interest. Therefore, it might actually be a--

MR. FORD: As I look at the graph here and I'd

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ask you to look as well, at approximately 1200, that is when Baker & Just start getting more conservative than White's, is that correct?

CHAIRMAN JENSCH: 1200 what?

MR. FORD: Degrees Centigrade.

DR. ROLL: I would suggest less than 1200 but--

MR. FORD: How much less? Is it very difficult to tell? I appreciate they become almost the same.

DR. ROLL: Perhaps 1175.

MR. FORD: I see. So that approximately in terms of the Fahrenheit degrees in which the interim criteria was set it would be going well up into the 2,000 degrees Fharenheit, would be going up then, if we are using the more conservative White's rate equation here, we'd be doing almost all of our better water reaction rate concerned with the interim criteria, we'd be doing it with White's.

DR. ROLL: That's not immediately obvious. Can we look at the graph that we have here?

MR. FORD: It will require a back-of-the-envelope calculation.

DR. ROLL: I think so.

1175 degrees Centigrade is about 2,000 degrees Fahrenheit.

MR. FORD: 1200 degrees Centigrade is what Fahrenheit?

DR. ROLL: Well, I think 1175 degrees Centigrade is approximately 2,000 degrees Fahrenheit, is it not?

MR. FORD: The 1200 degrees Centigrade is 2192 degrees Fahrenheit.

DR. ROLL: It goes up.

MR. FORD: So it's 2100 approximately.

DR. ROLL: I'd like to refer to a graph that has been previously presented in a July 13th submittal Additional Testimony of Applicant Concerning Emergency Core Cooling System Performance and in particular to Figure 10.

MR. FORD: Can you wait for a moment until I get that graph. Can you repeat the reference, please?

DR. ROLL: It's a July 13th submittal Additional Testimony of Applicant Concerning Emergency Core Cooling System Performance.

MR. FORD: The page number?

DR. ROLL: Well, I am looking at Figure 10.

MR. FORD: I'm with you.

DR. ROLL: Okay.

Now in degrees Fahrenheit if the crossover point is 2100 degrees Fahrenheit it appears that that rod as portrayed in Figure 10 spends approximately 100 seconds above 2100 degrees Fahrenheit and approximately less than a hundred seconds under 2100 degrees Fahrenheit. So looking at times one would say it's approximately a trade-off, or that more of the time is spent above 2100 Fahrenheit in terms of oxygen pickup. Inasmuch as we have a strong temperature influence the oxygen pickup would be, a much greater proportion would be on the Baker-Just equation, that is above 2100, than on the White equations below 2100.

MR. FORD: Well, since I am not prepared to accept the computer code calculation I specifically asked my question not with reference to the compute code calculation, but simply with reference to the interim criteria themselves. And the question was that and the temperature range discussed by the interim criteria, most of that range is encompassed in the area where we would be using, based on conservative practice, Thite's rate equation rather than Baker-Just.

DR. ROLL: I can't agree.

MR. FORD: You can't agree?

DR. ROLL: I cannot agree.

MR. FORD: Now by the range I mean most of the range and the range is 2300, I mean is from normal operating temperature to 2300 degrees, that is the range of reference of the interim criteria.

DR. ROLL: Right.

MR. FORD: Relative to normal operations. And is it not correct that it we were being more conservative than we are with Baker-Just that we would be using White all the

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from 600 to 2100, because most of the range for the computations, assuming of course you don't go over 2300 degrees--

DO. ROLL: It is correct that from 600 to 2100 degrees Fahrenheit encompasses more degrees Fahrenheit than from 2100 to 2300 degrees Fahrenheit. However--

MR. FORD: And I wanted--

CHAIRMAN JENSCH: Let him finish.

DR. ROLL: However, we went through this yesterday. It's the time and temperature combination which determines really what is the relevant span or relevant range, and that is why I cannot agree with your statement.

MR. FORD: Well I think that the statement is just a simple-minded one just concerning the size of the range and degrees Fahrenheit.

DR. ROLL: I concur.

MR. FORD: I expected to solicit your concurrence rather readily on that. Thank you.

Can you offer us in terms of whatever experimental or theoretical analysis that you have, can you offer us good reason why your sue of Baker-Just exclusively should be preferred to the use of the combination of White's rate equation for lower temperature ranges and Baker-Just for the higher ones?

DR. ROLL: I believe I can offer two reasons to support our approach.

Number one, data which we have explicitly looked at, that is I continued to refer to this FLECHT data, and other data can be shown to firm the essence of the Baker-Just equation.

And secondly, in testimony Mr. Moore presented yesterday the total neglecting of any reaction below 1800 degrees Fahrenheit versus considering the reaction as predicted by the Baker-Just equation resulted in .0005 per cent difference in the zirc oxide, in the zirc-water reaction. Therefore, we characterized this as being trivial.

MR. FORD: Now referring to our discussion of your statistical support for Baker-Just yesterday, if we took your body of data and plotted it on the axis of 2.3, plotted the parabolic rate constant against temperature, if we took your data and plotted it here and we put Baker-Just's prediction on and we put White's rate equation on, is it correct that-- or let me ask you in terms of the analysis, the loose kind of statistical analysis you perform, is it correct that White's rate equation has every much hope of being confirmed in your sense of confirmation by your data that Baker-Just has?

DR. ROLL: It's surely conjecture on my part but I believe that the data would, inasmuch as the data fit very well to the Baker-Just equation, and since there is a very small difference in the two equations, that I would say yes, the data would tend to confirm the White equation.

MR. FORD: Well, so that your first argument in answer to the grounds for using Baker-Just over the White then you now would delete those grounds?

DR. ROLL: No, I don't think so.

MR. FORD: Let's go into it further.

Now when you talk about your data fit very well, as I recall yesterday, this was some kind of look-see statistical analysis. There was no precise statistical measure of fit, is that correct?

DR. ROLL: I believe that was what we stated yesterday.

MR. FORD: I see. Now so that you can't guarantee for me, guarantee in general, that you performed a rigorous statistical analysis, which is by no means something extraordinarily difficult, that if we performed a rigorous analysis you can't guarantee, can you, that Baker-Just would come out as explaining more of the variance than White's rate equation?

DR. ROLL: No, since we haven't done that I can offer no guarantees.

MR. FORD: Now your first grounds for preferring Baker-Just over White is still the statistical, is that correct, than confirmation by the data?

DR. ROLL: No, I didn't say anything about statistics.

MR. FORD: No, no, no. I mean is it correct that the argument that says that these data points fit very well on our curve, is that a statistical argument?

DR. ROLL: I believe what I said was that the data that we have, as well as other data, confirms the Baker-Just equation.

MR. FORD: Can you define the term confirms?

DR. ROLL: In the context in which I am using it the data indicates that the Baker-Just equation is the appropriate equation to use to predict the time-temperature effects and to be used to predict the zirc-water reaction.

MR. FORD: Now by "data indicates" does that phrase, can we translate it to mean that "we interpret it" or, "it is interpreted in general"? I mean how does data indicate in a non-statistical way? Just to state my point more clearly, I cannot see how there can be outside of the use of statistical techniques, I can't see how you can talk about data confirming an analytical model.

DR. ROLL: If the data all falls on the conservative side of an analytical model, speaking in an abstract sense, and we are concerned about the particular model or particular equation predicting an effect and we show that it always overpredicts this effect, then in my context of use I would say the data confirmed the applicability or the use of this equation or this model to the particular effect in question.

MR. FORD: I see. Now given your definition if you looked at Figure 2.3 in McLain's report, if you look at the three data points there isn't it correct that up to our turn-around point we have two data points, isn't it a correct reading of that chart that the Baker-Just equation is below those data points, it's on the non-conservative side of those data points, whereas White's rate equation is exactly on one and above the other?

DR. ROLL: Yes. Limiting the discussion totally to the context of data presented in this graph.

MR. FORD: Right.

Now do you have any other data which you would add to this graph? I mean does your data give the sources for the data here as reference 41, 1, 20, 28, 17 and 26? Is the data that you have -- I will give you time to check those references. Do you have additional data not contained in those references?

DR. ROLL: Our data is reported in WCAP 7665, I believe. It is not on this graph. So that yes, I do have additional data which could be put on that graph.

MR. FORD: It's derived from --

DR. ROLL: Could in principle be put in that graph.

MR. FORD: It's derived from the different -- Did you take the opportunity to check those four references?

DR. ROLL: No, I did not.

MR. FORD: Could you check them and tell me whether any of the data that you use in your WCAP document, is any of that the same data that's here?

DR. ROLL: None of these references are our reference where our data is reported and discussed.

MR. FORD: Can you give me the reference where your data is reported and discussed?

DR. ROLL: Again WCAP 7665.

CHAIRMAN JENSCH: Any particular page?

MR. FORD: Can you give us the page?

DR. ROLL: I believe Appendix B.

MR. FORD: Is there a bibliography there? This is Appendix B called Materials Evaluation?

DR. ROLL: That's correct.

Is there an outstanding question?

MR. FORD: Yes. Oh, you were giving us references.

I guess you interpreted the appendix as your reference.

Now let me ask you for the six references here on page B 25, which of them provide data on the parabolic rate constant?

DR. ROLL: A quick perusal of the appendix indicates that reference two, which is the Baker-Just report, is the only one that is called out as supplying a rate equation.

MR. FORD: I see.

I have a number of questions. First of all, if you look at reference two on page B 25 is it not correct that the date is incorrectly specified in the reference? I am holding an original copy of the report in my hand. The date is given in your reference as May, 1968. Isn't it correct it's May, 1962?

DR. ROLL: Perhaps.

MR. FORD: Can you tell me in terms of the data -- I have looked through Appendix B in its entirety here and we are talking about the data to put on Figure 2.3, namely the parabolic rate constant. I see no analysis at all. There is no data in Appendix B on the parabolic rate constant, am I correct in that?

DR. ROLL: That is correct.

MR. FORD: No in terms of the data on the parabolic rate constant available from Baker-Just, is it correct that they do not have data covering the temperature ranges less than approximately 2300 degrees Fahrenheit?

DR. ROLL: I am not again -- I don't wish to quote out of the Baker-Just report comparatively. If you are reading it out of the Baker-Just report --

MR. FORD: No. I haven't got to that yet. I am referring to Figure 2.3. It plots Baker-Just data. Baker-Just data is represented by an unfilled-in triangle on the plot. And as far as I can see there is exactly one, one data point from Baker-Just.

DR. ROLL: What is the question now?

MR. FORD: My question was is it correct that Baker-Just as plotted on this chart Figure 2.3 of McLain report that there is no Baker-Just data, I mean this is your one reference, that there is no Baker-Just data covering the low temperature range and that in point of fact the only Baker-Just data that there is covers a temperature range on the order, as I quickly convert centigrade to Fahrenheit, by doubling it, on the order of 3700 degrees Fahrenheit.

DR. ROLL: It is correct that McLain has --

MR. FORD: One point.

DR. ROLL: In Figure 2.3 one data point attributed to Baker-Just, had this at approximately 1800 degrees Centigrade.

MR. FORD: I see. Now Baker-Just is your reference in your WCAP report. Can you tell me does it have -- I can read through the whole report now, but does it have more than one data point and not at 3700 degrees? I have commissioned a quickie review of the Baker-Just report to see if it indeed has more than one data point in it.

DR. ROLL: The data which we have to offer are plotted as measured versus predicted on Figure B 20. The parabolic rate coefficients are not -- Again I am talking on my report, my WCAP, page B 20, where we are showing measured versus predicted and comparing this to the Bkaer-Just equation, I'd say the data points fall below the equation. You are correct we are not reporting parabolic rate constants in this data. And the temperature range of this data is in fact very appropriate to our loss of coolant situation because these data are taken from rods from our FLECHT test.

MR. FORD: Didn't you testify yesterday with reference to Figure B 12 page B 20, didn't you testify yesterday that this data did not provide a confirmation of Baker-Just? It did not cover sufficient range and that you simply relied on the fact that Baker-Just was the accepted equation determining a parabolic rate constant?

DR. ROLL: I don't recall that I said that.

And you feel it's important, then I think we ought to take a look at the transcript.

MR. FORD: Right. We are checking that at the moment.

But let me go somewhat further here. The parabolic rate equation which you use is, as I believe you stated earlier, it is as stated on page 34 of McLain's report.

DR. ROLL: Yes.

MR. FORD: Now in terms of the data available to confirm that equation have you conducted experiments in this area ?

DR. ROLL: I feel we have been here before. We have not set up and run experiments explicitly defined to confirm the Baker-Just equation.

MR. FORD: Now do you want to change your testimony of yesterday referring to the transcript 1870?

CHAIRMAN JENSCH: Will you tender that to the witness and let him read a portion of it.

MR. FORD: He has his transcript. It's open for him.

CHAIRMAN JENSCH: Very well.

MR. FORD: I will wait for you to read it.

CHAIRMAN JENSCH: When the witness has completed his reading if you will so indicate you could propound that question.

MR. FORD: Are you completed?

DR. ROLL: Yes.

MR. FORD: Your statement that "It was not the purpose of our experiment to explicitly cover the range of variables as may be specifically related to the Baker-Just equation," would you care to further elaborate on your thoughts on what range of variables would be specifically related to the Baker-Just equations?

DR. ROLL: Perhaps it should be extended, specifically related to the equations as required for our loss of coolant analysis.

MR. FORD: You want to change it to include what?

DR. ROLL: I don't really want to change the statement. I think the statement is good and it's complete as stated. Now if you want me to change it or suggest a rewrite.

CHAIRMAN JENSCH: When you talk about changing it you could use a quieter whisper; it will help the Reporter. Proceed.

DR. ROLL: I see no reason to change the statement.

MR. FORD: Didn't you suggest there was a phrase you wanted to add on to the end of that first line?

DR. ROLL: Let me make the statement I don't think that the paragraph on 1870, bottom of 1870, up into

1871, should be changed.

MR. FORD: I see. Now you state that "We presented this information in support of Baker-Just and not to derive it."

Now by this information what variables are you referring to?

DR. ROLL: I will refer again to WCAP 7665 Appendix B.

MR. FORD: So that the variable you are referring to on the Figure B 12 that you are talking about that you referred to in that appendix, the variable was predicted oxide thickness in mils, is that correct?

DR. ROLL: That's the reported result as shown on the figure.

MR. FORD: Is that the variable as shown on that chart, sir?

DR. ROLL: That's the dependent variable shown on the chart, right.

MR. FORD: Yes. Now in the Baker-Just equation does that variable occur, is predicted oxide thickness a variable in the Baker-Just equation as stated on page 34 of the McLain report?

DR. ROLL: In the equation as reported on page 34 of the McLain report, W, the weight of zirconium reacted per unit for this area is convertible to zirconium oxide thickness. Therefore --

MR. FORD: No, no.

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CHAIRMAN JENSCH: I don't think you finished. Go ahead. Have you finished?

DR. ROLL: Yes.

MR. FORD: I see. If I had a ton of zirconium oxide and I put it together in a cube, it would have a certain thickness. If I put it together in a thin film over a mile, it would have another thickness. So is there any immediate constant relation between the parabolic rate constant which is simply the weight of zirconium and the thickness that you are giving us?

DR. ROLL: Of course, there is.

MR. FORD: In terms of the parabolic rate constant itself, are there any other factors which would influence the thickness such that thickness is not a surrogate under all circumstances for weight?

DR. ROLL: I think I am getting lost a bit in the terminology here. The parabolic rate constant--again, I am assuming what McLain is reporting in the graph, this parabolic rate constant. That's not rate. That's not thickness. That's $33.3 \times 10^6 \exp\left(\frac{-45,500}{RT}\right)$. That's the parabolic rate constant.

MR. FORD: And it is correct that the units for that are milligrams of zirconium per square centimeter squared per second. That's the unit for the parabolic rate constant.

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DR. ROLL: Yes.

MR. FORD: Outside thickness, is that the unit, the exact unit for oxide thickness so there are no circumstances in which you have the parabolic rate constant operating and not have the exact same thing number as your outside thickness?

DR. ROLL: I lost that question. Could we try it again?

MR. FORD: How is oxide thickness measured?

DR. ROLL: Physically how is it measured?

MR. FORD: It is measured in mils; isn't that correct?

DR. ROLL: Any unit of length. It could be measured in miles.

MR. FORD: In your chart it is measured in mils.

DR. ROLL: Fine.

MR. FORD: That's a unit to a degree. The unit for the parabolic rate constant, which is what is predicted by the Baker-Just equation, the unit for that is not mils. It is milligrams of zircalloy per cubic meter squared, that whole value squared divided by time and seconds.

DR. ROLL: Correct.

MR. FORD: So that's not mils.

DR. ROLL: That's right.

MR. FORD: So that in terms of your chart B 12, that it is only by a process of inference that you give us or derive support from this data on mils to what the parabolic rate constant is?

CHAIRMAN JENSCH: Are you adding, "Is that correct?"

MR. FORD: Yes.

DR. ROLL: No, it is not a process of inference because the way it was stated connotes something less than honest. It is a process of going through the explicit calculation of time and temperature using the Baker-Just equation and calculating a thickness and comparing it to the measured thickness.

MR. FORD: I am talking inference meaning logical inference, such that if you had a number of premises to the prediction of the parabolic rate constant, you will derive a unit in mils. I am just not clear what that whole chain of inference is. It has to be cleared up before we can decide that the chart and Figure B 12 offers any support at all to the parabolic rate constant as predicted by Baker-Just.

DR. ROLL: Mr. Ford, in my mind it is obvious how one takes the equation and takes a time and temperature history to calculate a thickness. This is what is reported in the graph. It is not by a process of inference with a lot of possibly false assumptions. It is merely taking time temperature history, going into this equation and calculating oxide thickness.

MR. FORD: Is there any assumption you make that it is absolutely impossible for it to be false?

DR. ROLL: Given a time and temperature history, there is no other assumption need be made.

MR. FORD: But in terms of all the assumptions you have made, including the ones of time and temperature history, is it possible for assumptions there and parameters there, measurements there to be inaccurate such that the derivation of parabolic rate constant and relationship to this predicted oxide thickness is invalidated?

DR. ROLL: In my opinion, the answer is no. There are no extraneous assumptions which have been made which are serious to the question.

MR. FORD: Can you give me any reference wherein you vigorously relate--you are contending that this data confirms Baker-Just or supports it, rather. Can you give me any place where you specifically present this analysis or do we have to go into it here?

MR. ROLL: To my knowledge, we don't have a public document where we have gone through this explicit comparison.

CHAIRMAN JENSCH: Is this a convenient time to interrupt your examination and break for lunch.

MR. FORD: It is convenient. I don't know in terms of Dr. Roll's--

CHAIRMAN JENSCH: I think Dr. Roll is urgently needed at his office but he is urgently needed here. So possession is nine something. Let us recess at this time for--

MR. FORD: I don't have, in terms of my interrogation of Dr. Roll, aside from the issue that we are currently on, which isn't terribly easy to resolve, any further major areas of cross-examination.

CHAIRMAN JENSCH: Will you proceed then, Intervenor.

MR. FORD: I'd like to start somewhere close to scratch.

CHAIRMAN JENSCH: If we are going that far back, I wonder, would you find it extremely difficult to spend another half hour after lunch?

DR. ROLL: I'm sure it can be arranged for the convenience of the Board.

CHAIRMAN JENSCH: Sometimes these predictions with time and temperature kind of get off.

At this time we will recess and reconvene in this room at two o'clock.

(The luncheon recess is taken.)

A F T E R N O O N   S E S S I O N

CHAIRMAN JENSCH: Please come to order.

Before we proceed, an announcement should be made. It involves the Staff participation. I just had a call from Congressman Dow and he has talked with Dr. Sternglass and Dr. Sternglass is able and willing to participate in this proceeding and I suggested to Congressman Dow that a request would be made to the Staff to get in touch with Congressman Dow's office to arrange a precise time for Dr. Sternglass and his presence.

MR. KARMAN: Have we definitely ascertained, Mr. Chairman, that Dr. Sternglass will come here in any other capacity than as a limited appearee? This may cause some complications.

CHAIRMAN JENSCH: Yes. I can understand it can cause some complications.

MR. KARMAN: We have been this route many times, as you know.

CHAIRMAN JENSCH: As I recall, the suggestion for which the Board is very grateful from the Applicant's Counsel, that Dr. Sternglass might appear as a limited participant,that he'd yet be willing, available for some interrogation in case anybody wanted to have any interrogation with Dr. Sternglass.

MR. TROSTEN: Mr. Chairman, excuse me. I believe the transcript will show, Mr. Chairman, that the suggestion that I made relative to Dr. Sternglass appearing, coming as a limited appearance, was in response to a comment made by the Chairman concerning his appearing as a witness. Applicant does not desire and indeed opposes the notion that Dr. Sternglass would appear at this late date in this proceeding in any capacity actually for the simple reason that a great deal of time is now being involved in having a discussion of the matters which Dr. Sternglass has raised at great length in other proceedings and public forums and so forth, and I would like the record to be completely clear on that point.

MR. KARMAN: Our position would be, Mr. Chairman, that we have certainly afforded many organizations and many individuals who came in at a late date the opportunity, as the Chairman most graciously puts it, to submit in writing any limited appearance statement they had for inclusion in the official record of this proceeding.

As a representative of the Regulatory Staff, I certainly feel it would not be fair for us to say that we should not afford Dr. Sternglass that same opportunity at this late date. I feel that we probably would be constrained to oppose any other intervention in depth or an appearance statement with respect to questions and answers at that point. This could lead to an endless procession of witnesses at a very late stage of the game where we are under constant attempt -- We are constantly attempting to expedite this hearing. I think this would do anything but expedite this hearing.

CHAIRMAN JENSCH: I think the problems you raise are certainly real ones. I know that Staff Counsel can handle them when he considers the matter --

MR. KARMAN: You are suggesting that I communicate with Congressman Dow when I return to Washington on Friday?

CHAIRMAN JENSCH: Yes. For instance, on transcript page 1761, Applicant's Counsel stated, "On behalf of the Applicant, we are very pleased to see Congressman Dow present and to hear his statement. May I suggest the possibility that rather than having Dr. Sternglass brought here to offer testimony in this proceeding, perhaps he could come and make a statement in the nature of a limited appearance similar to the statement that Congressman is making."

I thought it said, and might be willing to answer some questions. I don't find that portion of it. In any event, we are faced with the announcement that Congressman Dow has communicated with Dr. Sternglass. Maybe Staff Counsel could persuade the situation to be handled by a written statement.

MR. KARMAN: I would be pleased to communicate with Congressman Dow and report back to the Board probably the beginning of next week.

CHAIRMAN JENSCH: I think it should be borne in mind, of course, that any appearance by Dr. Sternglass would have to be at his own expense travelwise and per diemwise and motelwise and whatever other aspects there might be. In any event, the Board is inclined to turn that over entirely to Staff Counsel.

MR. KARMAN: Thank you very much.

MR. TROSTEN: Applicant will await further developments from the Staff Counsel. I think the record says what it says. I just call the Chairman's attention to transcript page 1757 where this matter was discussed in addition to the reference that he mentioned.

CHAIRMAN JENSCH: I unfortunately recall that portion of the transcript. As I say, I'm sure we will have a satisfactory solution to this problem after Staff Counsel devotes his good attention to the matter.

Let us proceed. The witnesses have resumed the stand. Is the interrogator ready to proceed?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Proceed.

MR. FORD: I'd like to direct my questions to the reference that the Applicant provided in Appendix B, namely Louis Baker, Junior -Louis C. Just Studies of Metal-Water Reactions of High Temperatures Through Experimental and Theoretical Study of the Zirconium-Water Reaction, Oregon National Laboratory ANL-6548, published in May of 1962, which is a correction of the reference in the Westinghouse document.

Dr. Roll, the title of the study refers to high temperatures. Are you familiar with the temperature range with which Baker and Just are concerned?

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DR. ROLL: Not in detail.

MR. FORD: Are you aware of the fact that the range of temperatures that they are concerned with is oriented toward the analysis of molten zirconium?

DR. ROLL: Not personally going back and refreshing my memory on what is in the report.

MR. FORD: I see. Are you aware that the basic experiments that they are discussing involve hot molten zirconium droplets dropped into water.

DR. ROLL: I'm aware that they did use that technique.

MR. FORD: In terms of the temperature range over which you have applied the Baker-Just relationship in the analysis of Indian Point whose metal-water reactions during the loss of coolant accident, have you covered a range that includes molten zircalloy cladding?

DR. ROLL: No, we have not.

MR. FORD: Can you tell me how you believe one can extrapolate the small amount of data that they present for a high temperature range to the temperature range with which you were concerned in the analysis of the loss of coolant accident transient?

DR. ROLL: How we do it is a calculational technique inasmuch as there is no break point, no statement that the equation is not applicable. Then it's really up to us to make a judgment that we can use it in the range of interest.

MR. FORD: What I really should have stated more clearly is how do you justify extrapolating bare data concerned with this high temperature range concerned with droplets of molten zircalloy and water, how can you extrapolate this to temperature ranges which you have indicated you do not concern yourself at all with in your analysis of the transient here, that you are concerned with just much lower temperature ranges than they use?

DR. ROLL: I believe in the line of questioning which we covered late this morning as well, I am sure we talked about yesterday, too, our justification for the use of the equation in predicting the zirc-water reaction or the extent of zirc-water reaction under the conditions of interest is partly in looking at data in the relevant temperature ranges in interest. Such a piece of work as our own work reported in the FLECHT final report and discussed late this morning.

MR. FORD: Do you mean that this extrapolation is justified because in the dozen or so cases reported on Figure B 12 that you measured oxide thickness, that these were not radically different from what you expect if you believed Baker-Just's correlation?

DR. ROLL: Yes. The data which we present are partial confirmation of the use of the equation in the range where we are applying it in the loss of coolant study for this application.

MR. FORD: Is there further confirmation that you can offer?

DR. ROLL: I believe there are other references which essentially confirm these Baker-Just predictions.

MR. FORD: Yes. When we went to your references the only reference was Baker-Just. Do you have further references?

DR. ROLL: Well again I think you have a context problem. The Appendix B was not an attempt to bring in all the data to support Baker-Just. Appendix B was an attempt to present our data which we had taken for a particular set of tests and compared to Baker-Just. It was not an attempt to compare it to other equations, nor was it an attempt to bring in other data to support Baker-Just.

Therefore, I don't think the fact that we don't have any other references therefore negates the work we presented in Appendix B.

MR. FORD: I'm simply asking you if you can set forth any other theoretical or analytical work beyond the twelve points or so -- I should count them. -- that you are relying on?

DR. ROLL: I think that the work which we discussed yesterday out of Oak Ridge in which there was a discussion, there was the application of some data they had done, and in discussing with the authors that data it is consistent as our data is consistent with the Baker-Just equation. I believe further that the work that's come out of Idaho on investigating this phenomenon is consistent with the Baker-Just equation.

MR. FORD: What authors of what studies are you referring to?

DR. ROLL: I am referring to the article in ANS Transactions which you brought out yesterday, I believe one of the authors was Hobbson, in which we discussed -- We finally determined that that was an application of some data and not data itself, but that work that went into that analysis is in essential agreement with Baker-Just predictions.

MR. FORD: When you say "we discussed" you mean you and Hobbson or you and me?

DR. ROLL: You and I yesterday.

MR. FORD: Yes. I see.

Are you suggesting that the relationship they give here between oxygen penetration to measure the combined thickness of zircalloy dioxide and oxygen-stabilized a phase and time as in their equation I, are you indicating that that is derived from Baker-Just or that is Baker-Just or what?

DR. ROLL: No, I didn't mean to infer that at all.

MR. FORD: Let me give you the Hobbson analysis again and ask you to point out to us by specific reference to this analysis how it confirms Baker-Just.

MR. TROSTEN: Could we clarify for the record what document again you are looking at?

MR. FORD: It's the American Nuclear Society Transactions for the 1971 Winter Meeting October 17 to 21, 1971. I think it's Volume XIV. The article in question was referred to yesterday, by Hobbson of the Oak Ridge National Laboratory. It's on page 701 or 700. 700-701.

DR. ROLL: Page 700.

MR. FORD: Page 700.

DR. ROLL: What I said was that the work done and partially reported here is in agreement with predictions that one might obtain with the Baker-Just equation, and this work covers a range of variables of interest.

MR. FORD: No. Specifically in terms of that document and the analysis that it presents there is there any clear indication from that confirming your assertion that these peoples' work have confirmed Baker-Just?

DR. ROLL: It is not stated in this document that this work is consistent with Baker-Just.

MR. FORD: What work at Oak Ridge are you referring to that purportedly confirms Baker-Just?

DR. ROLL: I am referring to this effort, this experimental effort, which is reported partially in this article.

MR. FORD: I see. But it's not reported at all explicitly. Now what projects, what experiments that are evolving data at Oak Ridge, confirm Baker-Just? Are they doing, for example, more experiments dropping molten zirconium spheres into water, that additional data as confirming Baker-Just, or what?

DR. ROLL: Are you familiar with the work that's reported here? I said that this work essentially confirms Bkaer-Just in the range of interest. They are dropping spheres in water that infers you are above the melting point of zirconium and that is not the range of application in this application here, as I stated before.

MR. FORD: I see. Excuse me. I was only giving that as an example of some experiments here.

What specific experiments are they doing in the area of interest?

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DR. ROLL: The experiments that are reported in the referenced article from ANS transactions essentially support the Baker-Just predictions.

MR. FORD: Let me make perfectly clear the referenced article makes no mention whatsoever of the parabolic rate constant, and presents no data whatsoever on the parabolic rate constant. So can you explain to me how what is in that two-page article, how that presents some data pertaining to the parabolic rate constant?

DR. ROLL: Once again, I did not say the article had the data in it. I did not say that the article supports the equation. I said the work which they had done is partially reported here and essentially supports the Baker-Just predictions.

MR. FORD: I was concerned with the partial. What part of that supports it?

DR. ROLL: I said the work done and partially reported here. I didn't say this partially supports it.

MR. FORD: What work done are you referring to?

DR. ROLL: Work performed by Mr. Hobson and under the direction of Mr. Rittenhouse at Oka Ridge, part of their overall effort. It has been to investigate zirc oxide transformation under times and temperatures. This data has been taken. I don't know that ithas been reported publicly. Perhaps it has been reported in Oak Ridge National Laboratory

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progress reports. I am familiar with the data. I have talked to Mr. Hobson and Rittenhouse several months, if not a year ago. Our agreement was that the data which they had in terms of zirc to zirc oxide transformation kinetics was not radically different. It was essentially the same as those predicted by Baker-Just and is essentially the same as that which had been previously reported by Westinghouse in the Appendix B.

MR. FORD: Have you reviewed the data they are talking about? Do you have any independent judgment on the matter that you could substitute for hearsay?

MR. TROSTEN: Are you making an objection that this is hearsay?

MR. FORD: No. I was going to let him transform it, if he could, from hearsay to something more substantial.

MR. TROSTEN: I was rather astonished to think that you might be making such a suggestion. I'm sorry. Excuse me.

CHAIRMAN JENSCH: Go right ahead. Proceed.

DR. ROLL: I have not personally gone through their calculations. People at Westinghouse have reviewed their data. I have personally taked with Hobson and Rittenhouse.

MR. FORD: Can you not tell me any of the parameters they used at all in their tests? You just know nothing about them completely?

DR. ROLL: No.

MR. FORD: I see.

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DR. ROLL: Again, I am quoting from memory. The temperatures were of interest. That is less than 2,000, to excess of 2,000 degrees Fahrenheit times the order of a few minutes. Less than a minute to a few minutes, as I recall. I wish not to quantify those parameters in that fine data that I have missed some range.

MR. FORD: Does it make any difference, in looking at the Baker-Just analysis, whether you take the room temperature water data or the heated water data in deriving zirc-water reaction rates?

DR. ROLL: I don't know.

MR. FORD: Thank you.

Does the specific--

DR. ROLL: Let me amend that a bit. I don't know in the context that it likely does. It is inconceivable that if you have a lot of data and you are using parts of the date, you are going to derive exactly the same equation as if you use some other part of the data or if you use the data in the whole. It is likely that if you select data to affect a correlation, that you are going to get a different correlation.

MR. FORD: Does any specific understanding that you have of the chemistry of this reaction indicate that it makes a difference, and indicate the kind of difference using the room temperature versus heated water data?

DR. ROLL: I think the answer to that question really requires a person who is a theoretical chemist to say-- in looking at a liquid solid or a liquid liquid reaction. Here are the relevant parameters. I don't really claim to be a theoretical chemist.

MR. FORD: Is the specific alloy of zircalloy in the Baker-Just tests and in the tests which you have related to us through your conversation with Hobson, the same as the alloy of zircalloy in the Indian Point 2 fuel rods?

DR. ROLL: We can pursue that, I think. The alloy in Indian Point 2 is inconel 4.

MR. FORD: Pardon me.

DR. ROLL: Zircalloy 4. It appears with a brief perusal of the Baker-Just report that he used, for the most part, zirconium metal.

MR. FORD: Zirconium?

DR. ROLL: Yes.

MR. FORD: And zircalloy 3?

DR. ROLL: Yes. Much of the data appears to be zirconium. I don't recall what Hobbson and Rittenhouse used.

MR. FORD: In terms of zircalloy 4 versus zirconium, can you explain to us the composition of the zircalloy 4 alloy percentage of different metals and iron, tin and nickel traces and so forth?

DR. ROLL: I think you have characterized it yourself. It is iron, tin and nickel traces on the order of a few percent. There are specific numbers which I don't recall offhand. But they are small amounts. They are on the order of one to two percent total.

MR. FORD: Is there any experimental data pertaining to the Baker-Just equation that establishes the manner in which you may extrpolate his data on pure zirconium to your zircalloy 4 alloy? I refer to experimental data.

DR. ROLL: To my knowledge, which immediately refers -- To my knowledge, there is not a specific reference which investigated the difference in reaction rate kinetics between zircalloy 4 and ziranium metal.

MR. FORD: The Baker-Just equation considers, as I understand it, the influence of particle diameter to the reaction since it was concerned in the main with dropping spheres of molten metal into water. Can you tell me how you can extrapolate from an equation based on the influence of particle diameter? It is quite clear from the Baker-Just equation on site Figure 9 page 19, zirconium-water reaction is a function of particle diameter; that there is a very clear and significant relationship between particle size and reaction rate? Can you tell me how rate equations -- That is influenced by the size of the particle. Can you tell me by consideration pertaining to the size of particle diameter, how that can be extrapolated to a completely different physical situation? You are talking about a non-molten tube rather than a molten sphere.

DR. ROLL: A point of correction. Figure 9 is not as you portrayed it. It is a figure of percent reaction versus mean particle diameter, and not a curve of reaction rate versus mean diameter.

MR. FORD: That's correct. But I think my point is that in the Baker-Just equation they are trying to make an equation that fits data influenced, whose percent reaction is influenced by the particle diameter. I want to know how you can extrapolate an equation that is trying to do justice to particle diameter considerations to the use of this equation in situations where you are not talking about particles at all. You are talking about the rod crystal structure and metal.

DR. ROLL: Baker-Just equation is a specific reaction rate. That is, it is per unit area and it contains explicitly time and temperature to yield a rate per unit area. In deriving the constants, the effects of the surface to volume ratio and the time and temperature considerations for the data itself for various size spheres had to be considered to derive a specific equation. The equation itself does not contain particle diameter. Therefore, one again is not running great risk in applying it to a different geometry if in fact the equation is desensitized through the use of reaction rate per unit area.

MR. FORD: But don't Baker and Just give some theoretical reason why the geometry does make a difference, since they are talking about the slow diffusion metal ions or oxide ions through crystal lattice of the metals? Is this independent of geometry concern?

DR. ROLL: I think we are once again asking very specific questions about the kinetics of the derivation of the mechanism.

MR. FORD: That's correct.

DR. ROLL: What is important is, we have an equation which has been derived from a set of data. The equation is meant to predict the extent of conversion of zirconium to zirc oxide given a time and temperature history. We have reviewed this equation. It has been reviewed by others, and we have proposed, in our somewhat voluminous documentation before us, both specific to this application as well as in our R & D Program Reports, i.e., the FLECHT program final report. We propose that this is an adequate tool to use to predict this mechanism. I think that is the confirmation one should look to.

MR. FORD: Will the Reporter repeat my question, please.

(The last question referred to above was read by the Reporter.)

MR. FORD: Will you try to answer that question.

DR. ROLL: Will the Reporter read my answer?

MR. FORD: I have asked you to consider the theoretical reason that Baker and Just give. Could you indicate whether you agree or disagree with that theoretical reason, and if so, please identify the experimental and analytical support of your position?

DR. ROLL: I repeat. Will you read my answer, please.

CHAIRMAN JENSCH: I don't think that's necessary.

DR. ROLL: I am not qualified to go into a detailed discussion of the reaction rate kinetics, sir.

CHAIRMAN JENSCH: Maybe that's the answer.

DR. ROLL: That was my answer.

CHAIRMAN JENSCH: There was something about that long in the transcript in response to this question. I didn't have the impression that it answered the question as succinctly as you did this last minute. Will you proceed.

MR. FORD: So independent of your ability to go into the reaction kinetics here, are you cognizant of any experiments that have been done to settle the influence of the fact that these tests were derived for specific geometries, the influence of that on the applicability of this rate equation to different geometries and different temperatures?

DR. ROLL: I am unaware of tests performed explicitly to investigate the effect of geometry on the reaction between zirconium and water.

MR. FORD: Can you tell me whether you know of any theoretical data which indicates how changing thickness of the cladding would affect the diffusions of either metal ions or oxide ions to the crystal lattice?

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MR. ROLL: I cannot quote a reference that answers that particular question.

MR. FORD: Thank you.

Now in deriving the equation it is assumed Baker & just assume, as I understand it, that there is always complete vapor film surrounding the droplet. Is that condition the case in loss of coolant transients, that the cladding is always completely surrounded by vapor film?

DR. ROLL: Can we confirm your statement by reference to a page number?

MR. FORD: Yes. On page 24, the last sentence of the paragraph continued over from the previous page says, and I quote, "It is reasonable therefore to assume that there is always a complete vapor film surrounding the metal droplet."

CHAIRMAN JENSCH: Do you have the question in mind?

DR. ROLL: Yes.

CHAIRMAN JENSCH: Proceed.

DR. ROLL: In the calculations there are times when there is less than solid water or saturated vapor in contact with the metal, but that fact does not really enter into our calculations. So I don't know what we have assumed it in our application for this application. So the reasonableness of that assumption--I don't know how to apply it.

In Baker-Just's application he applies it to his derivation of the mathematics of the reaction rate, and it appears that it's perhaps for his conditions a reasonable assumption. We don't have to make that assumption in our application.

MR. FORD: You don't have to make that assumption?

DR. ROLL: No, we don't.

MR. FORD: For what purpose do you have to make the assumption to apply Baker-Just? That is my concern.

DR. ROLL: Not clear.

MR. FORD: Then that's the answer.

Further, Baker-Just is concerned on the remainder of page 24 and page 25 with hydrogen film thickness in the reaction rate. I will give you time to read that over.

DR. ROLL: I have scanned it briefly.

MR. FORD: You have scanned it briefly. Now let me address myself to it in parts. Let me quote the parts I am concerned with.

Beginning at the previous quotation it says:

"It is important to determine whether the process of faseous diffusion can be considered as a steady state process with a constant vapor film thickness or whether it must be treated as a transient process with the continually enlarging film."

Now in the context of their discussion in the calculation of gaseous diffusion rates, can you tell me whether the manner in which they handle this process which they consider

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to be important and the assumptions that they make about it in the analysis following, whether their assumptions are actually the case with regard to this phenomena in the loss of coolant accident metal-water reacting situation?

DR. ROLL: I can't with this brief perusal consider what their assumptions have been and what the alternates might have been and have they made a good choice.

Therefore, the answer to your direct question, I cannot comment on their assumptions. I fail to see how it's relevant to the application of the loss of coolant accident.

MR. FORD: Isn't it the point of their analysis that it is because of the thickness of the hydrogen film involved here that the rapid initial reaction subsides, so that isn't it crucial in determining our reaction rate to get clear on these mechanisms, films that they are talking about?

DR. ROLL: No. It's not actual--if you are taking data and attempting to correlate a reaction rate constant to time and temperature what is important is that you have good data. If you are trying to derive a theoretical basis for why it reacts this way, then it's important to have an understanding of the details of hydrogen diffusion or related effects or the like.

MR. FORD: Now in order to use their data derived from the hot molten droplets do you have to get clear at all on how the kind of hydrogen field around that droplet, whether this is the kind of thing, it's quite influential there they contended, it slows down the reactivity it's already started at a very "rapid initial reaction". It's very important there. Now what I am wondering is whether or not there will be a sufficient hydrogen film in the loss of coolant accident situation to stop a rapid initial reaction between zircalloy and water from continuing as a very rapid reaction, and I'd like you to set forth the data that you have pertaining to the thickness of the hydrogen film that would be surrounding the cladding during the loss of coolant accident.

DR. ROLL: We have no data on the thickness of the hydrogen film that surrounds the cladding during the conditions of interest.

MR. FORD: Do you have any data that disputes their contention that the thickness of this film is importantly responsible for slowing down the zircalloy-water reaction rate?

DR. ROLL: Gee, on the contrary. We might state that the date which we have inasmuch as it tends to confirm their resulting equation in essence confirms some of the assumptions they have come up with.

MR. FORD: I see. Now, can you argue in terms of the fact of this general confirmation which you have said existed--they make a variety of assumptions, have a variety of data. Can you say that this general confirmation confirms

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a specific assumption or you can say that it doesn't challenge the assumptions in general?

DR. ROLL: I believe that the general confirmation which we have shown confirms the general applicability of the equation.

MR. FORD: I see. So that in particular you can't move, or can you, from this relationship of general confirmation to any specific confirmation of this very assumption of the hydrogen film?

DR. ROLL: No, we cannot.

MR. FORD: Thank you.

Now, can you tell us how you can extrapolate this reaction rate in which hydrogen film is very important to a reaction rate under LOCA conditions about which you have no data pertaining to the hydrogen film?

DR. ROLL: I believe I have answered that question.

MR. FORD: Have you answered it in the sense of justifying this extrapolation?

DR. ROLL: I think the question you asked earlier was how do I extrapolate the equation. I believe I gave an answer to that question.

MR. FORD: Thank you.

CHAIRMAN JENSCH: Maybe I missed that but would you try it again, please. How do you extrapolate for the situation?

DR. ROLL: The basis for the validity of our extrapolation is contained in the confirmation which we had presented in this WCAP report as well as other confirmations, that is the overall thickness by the equation is consistent with the thicknesses observed in our experiment as well as others.

MR. FORD: Would you turn to page 27 of the Baker-Just study.

MR. TROSTEN: What is the precise document, for the record, please?

MR. FORD: This is the document that we introduced into the record some time ago. It's the Studies of Metal-Water Reactions at High Temperatures, Experimental and Theoretical Studies of the Zirconium Water Reaction, by Louis Baker, Jr., and Louis C. Just, Argonne National Laboratory document number ANL 6548, May 1962.

MR. TORSTEN: Does this have an exhibit number, Mr. Roisman?

MR. ROISMAN: It's not in yet. It's on the list of documents we have asked to have official notice taken of.

MR. TROSTEN: I see. Thank you.

MR. FORD: In addition, it is the sole reference pertaining to the Baker-Just equation given in Appendix B of theWCAP 7665 document.

Now on page 26 they sum up their discussion of assumptions pertaining to the reaction rate and they state:

"The diffusion controlled reaction rate is then obtained by combining equations 5 and 6 as follows."

And they then present their equation 7.

Now it refers on page 27 after they have defined all the symbols, and so forth, that the principal unknown factor in equation 7 is the Nusselt number, NU, and then they go on to describe how they dealt with this principal unknown.

Can you, of course giving you time to analyze there, fill in this gap, can you study it and give us your comment just to whether you agree or disagree with the analysis they have given here, and state the basis for your disagreement?

DR. ROLL: As I stated before on the basis of a cursory review of the document I don't propose to review their assumptions, consider what the alternate assumptions may have been, and render a judgment.

MR. FORD: Do I have to translate their equation into specific questions for you? Then will you answer? I mean you simply -- You are not prepared to read these ten lines?

DR. ROLL: I am not prepared to comment on the validity of his analysis. I believe that was your question.

MR. FORD: I see. So that as far as his handling of the princial unknown factor in his analysis you have no comment?

DR. ROLL: In the context of this part of his report it is the principal unknown factor. I am not prepared to comment on his treatment in this section of the report.

CHAIRMAN JENSCH: May I inquire a moment, did I understand you to say that this Baker-Just equation set forth in this ANL document which has been identified as the sole reference, and does that mean that that's upon which the Applicant has relied in its analysis?

MR. FORD: That is my impression, sir.

CHAIRMAN JENSCH: Is that correct?

DR. ROLL: No.

MR. TROSTEN: I don't believe that is a correct statement.

DR. ROLL: It's not.

MR. TROSTEN: That this is the sole reliance.

CHAIRMAN JENSCH: Well at least it is one of the factors, one of the bases of reliance.

MR. WIESEMANN: Mr. Jensch, if I may.

We have relied upon the correlation presented in that document which has subsequently been verified by our experimental data as well as others and I believe that the problem we have here is there is some question being raised as to some of the theoretical problems that Baker and Just had in arriving at this correlation, and I think that rather than taking the approach of deriving Baker and Just correlation, as is common in engineering practice, we ran experiments and predicted the results using the correlations, compared those with the observed results and decided on that basis that the correlation was giving an adequate prediction for the range over which we had to use that correlation.

CHAIRMAN JENSCH: Yes, I understand that.

Now I think the problem I have is if you used it at all who is here to tell us how to use it? Because if this isn't the witness that you have who used it then who can tell us how you used it?

MR. WIESEMANN: Well, I think we have explained how we have used it between Mr. Moore's testimony and also Dr. Roll's testimony. But apparently the Intervenors are not satisfied with the fact that we chose to run experiments to verify the validity of the results of Baker and Just as opposed to doing an independent derivation of a correlation and then comparing the correlation to Baker and Just correlation.

CHAIRMAN JENSCH: I think the problem is you used it in part and disregarded it in part and I wondered --

MR. WIESEMANN: No, I don't believe so. I think that in recognition of the fact that the Baker and Just correlation had to be accepted as being appropriate for this particular analysis we performed the experiments and made the comparisons and other people have also done this and we have satisfied ourselves that the correlation -- In other words, Baker-Just did a lot of work explaining how they arrived at the correlation, but the basic result of their work is the correlation, which they presented and recommended to be used for the purposes of predicting zirconium-water reactions, and recognizing the problems associated with that, and I think in a very general way recognizing those factors that the authors presented as being those difficulties that they had to overcome in arriving at a correlation we ran these experiments and the results confirmed the correlation produced results which were consistent with the measured results and therefore justified the use of this for this particular application.

CHAIRMAN JENSCH: Thank you. Excuse me for interrupting. You may proceed.

MR. FORD: Yes, sir. May I have the brief opportunity simply to establish the relevance of this questioning in view of the statements of Mr. Wiesemann.

The point of this line of questioning is that in my study of the Baker-Just work it seems to me to be quite clear, very explicitly stated in their analysis, that they are concerned with very high temperatures associated with the meltdown of a majority of the nuclear fuel following the failure of emergency core cooling system in loss of coolant accident. And my concern is that the variety of assumptions that they made are all tied to that situation. They are tied to molten droplets, they are tied to, for example, what happens when molten core falls into water remaining at the bottom of the vessel. That is the kind of metal-water reaction that they are talking about. What I am trying to explore with a variety of questions concerning the assumptions they used, the nature of their experiments and so forth, is whether their very conditional rate equation based on the conditions of high temperature and different geometries and so forth, whether that correlation can be applied to the radically different situation of the very early phase of a loss of coolant temperature transient.

CHAIRMAN JENSCH: Proceed.

MR. FORD: Now Dr. Roll, we are running now to the principal unknown factor in their diffusion control reaction rate. They state that it is the Nusselt number. Can you explain to us what this Nusselt number is and the significance in estimating it correctly?

DR. ROLL: Nusselt number is the dimensionalist ratio of thermal convection coefficient times particle diameter divided by thermal conductivity.

MR. FORD: Can you repeat that slowly, please?

DR. ROLL: It's H D /K.

MR. FORD: Could you do it in words, please.

DR. ROLL: H is the convection heat transfer coefficient. D is the diameter and K is the thermal conductivity.

MR. FORD: I see. So that in terms of your precise description now is it correct that we can see the role of particle diameter that is involved in this principal unknown factor? I mean is it correct from your definition of the Nusselt number that it is specifically tied to a particular particle diameter?

DR. ROLL: Negative, no.

MR. FORD: It isn't?

DR. ROLL: No. It's a dimensionless quantity and used as correlating variable. D is diameter of a particle or a surface. It can be used in flat plate cases, for example, where there is no diameter as such.

MR. FORD: In terms of the remaining part of my question, the significance of estimating this number correctly, can you tell me what error is introduced by, let's say, a 100 percent error in estimating this number?

DR. ROLL: What error is introduced in what?

MR. FORD: In the extent of the computer level water reaction.

DR. ROLL: No error. The equation that was derived and presented and is used in our analysis is an empirical equation and the Nusselt number doesn't enter into it.

MR. FORD: I am saying, if that equation was based on a different Nusselt number way back in Baker and Just's derivation, how would that influence the final extent of metal-water reaction?

DR. ROLL: The empirically derived equation does not have a Nusselt number in it. Therefore, there cannot be any function.

MR. FORD: Is it correct to state that your results are function of Baker and Just's final equation, their final equation, is a function of the assumptions that they made in deriving it, which includes a value of the Nusselt number?

DR. ROLL: That's incorrect.

MR. FORD: It is?

DR. ROLL: That's incorrect.

MR. FORD: They don't, in terms of their derivation, their equation -- This is not logically limited to some assumption of the Nusselt number?

DR. ROLL: If Baker and Just did what they said they did, even in the abstract, the equation we are using is an empirically derived equation. They go through a theoretical study to shed some more light on the basic chemical kinetics. But the empirically derived equation which we have used in our analysis in support of this application is an empirically derived equation and does not require knowledge of the Nusselt number.

MR. FORD: Let's turn to page 44, please, of Baker and Just's paper. I refer you to Table VI. "The effect of Nusselt number on the reaction rate for a large particle --"

While you are studying it I will describe what the table presents. The table presents different values of a Nusselt number. It associates with each different value of a Nusselt number the final extent of the zirconium-water reaction. For a Nusselt number of two, which is the number that they chose, you get a metal-water reaction, final extent percent, of 6.85 percent. Whereas if you increase the Nusselt number, you decrease the extent of the reaction. So that by the time you get two Nusselt numbers of 12 and 16, at 16 you have reduced the extent of the reaction from 6.85 percent to only 4.65 percent.

If Dr. Roll has had sufficient time to study, I will ask him some questions on this.

DR. ROLL: Again, I have briefly perused what is here.

MR. FORD: Is it clear from this data that a Nusselt number does make a clear difference in the extent of the zirc-water reaction?

DR. ROLL: No, it is not, because it is not clear that this is data. That is, this could be the result of a calculation based on a theoretical model.

MR. FORD: There are, of course, two things that could make a difference, experimentally testing different Nusselt numbers and seeing what differences they make, and on the other hand, analytically plugging in different Nusselt numbers and seeing what difference they make. I am not clear myself, just jumping into this context here, whether this is the result of calculations of a model or as a result of an experiment. My position is, it doesn't make any difference either as an analytical or empirical result.

Is it clear from this table that the choice of this Nusselt number makes a clear difference to the extent of zircalloy-water reaction?

DR. ROLL: It is clear from this table that different Nusselt numbers give different values for final extent of reaction.

MR. FORD: Thank you.

DR. ROLL: Whatever the context of this table is.

MR. FORD: Do you have any experimental or analytical data of your own that would refute that relationship that you have just confirmed as indicated by this table?

DR. ROLL: No, we don't.

MR. FORD: They state on page 28, they are concerned with the simplification involved in applying parabolic rate law as expressed in their equation 8, 9 and 10, that it doesn't apply precisely to all spheres. Do you dispute this?

DR. ROLL: Again, I have not had time to go through the equation and determine what is its applicability.

MR. FORD: Please. I don't mean to solicit --

DR. ROLL: I have no basis for accepting or rejecting it.

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MR. FORD: I see. Why don't I let you take a look at equations 8, 9and 10 and see if they apply specifically for spheres.

MR. ROLL: Again, I'm not in a position to go through the derivation to the equations and review the basic texts on diffusion, mass transfer and heat transfer, and judge whether or not these equations apply for the spheres or not. The authors state that they do not apply precisely for spheres. At least at the time the author was doing the work, he presumably knew the theoretical application of the equations.

MR. FORD: I think I may have asked a similar question before. I would like to clarify the matter fully at this point.

Simply in terms of Westinghouse's research going through this report as I have, you haven't done this yourself?

DR. ROLL: No.

MR. FORD: Did you indicate before that Westinghouse had no document, no topical report or anything indicating that they have gone through and have down this analysis free from basic criticisms; is that correct?

DR. ROLL: I don't know from firsthand knowledge that one went through this, presuming in as great a detail as you have, and determined their validity or otherwise presumably as you have. The fact is, the equation and the basis was received for its applicability, and nothing has been published in our own work or others which causes us to think we have made a wrong choice in selecting the Baker-Just equation.

MR. FORD: But no one that you know of, since May of 1962 when this document was published, has gone through and asked the questions that you have been asked and able to answer in one degree or another?

DR. ROLL: I don't know from firsthand knowledge that that was done explicitly.

MR. FORD: But the main point is, you don't know whether it was; is that correct?

DR. ROLL: I don't. I say, I don't know that it was.

MR. FORD: In terms of simply Westinghouse's research that they do and the attempts that they make to keep up with the state of the art, is it an exception in this case that you come to us as an expert witness on metal-water reactions, but the basic document that you present in your analytical report that you refuse, for reasons of not being able to in the variety of instances that we have gone over, to answer basic data, basic questions about the underlying support for your analysis?

Is this a description of the practice of the vendor with regard to keeping up to date and keeping informed on basic engineering data pertaining to reactor safety systems and reactor accident phenomena?

MR. TROSETN: I object to the question, Mr. Chairman. I will be happy to state my objection to the question.

CHAIRMAN JENSCH: Please do.

MR. TROSTEN: The issue that is involved in this proceeding this afternoon and generally is not exactly whether Dr. Roll knowns the answers to every possible question that Mr. Ford could raise. The issue is whether the applicant has the information available and has put the information into the record or will put the information into the record. It is in nowise proper for Mr. Ford to ask the question of this witness, what are Westinghouse's general practices with regard to checking data, for the simple reason that such a question is in nowise related to the general scope of Dr. Roll's testimony.

For that reason, I object to the question. I believe the Board should rule that the witness is not required to answer that question.

MR. FORD: Mr. Chairman, in response, I would like to state and indicate that I think to a very large extent in areas as complex as the ones we're dealing with, that obviously only a number of issues can be raised and investigated in detail, and that we have to rely to a considerable extent simply on the plain raw expertise and care with which the reactor vendors have conducted their analysis of a safety system performance.

I think the question that I have asked our expert witness on metal-water reaction pertaining to the applicant's major document on water-water reaction, and the questions I have asked into the kind of regard the applicant has taken for in-depth analysis of this, I think the question is perfectly proper if not a very important question to be pursued in this kind of proceeding.

MR. TROSTEN: May I comment?

CHAIRMAN JENSCH: No. I think the question, as I understood it, involves a whole range of Westinghouse operation. I don't think this witness has shown himself to be familiar with the whole operation. I think to some extent it involves a policy matter of the company for which this gentleman doesn't speak. I imagine, however, that you will make contentions somewhere along the line in your question. I think that gets into a matter of argument rather than a question of this witness.

MR. FORD: Can I transform the question two ways to meet the guides that I would accept?

CHAIRMAN JENSCH: Let us just dispose of one matter at a time. The objection is sustained. You may proceed.

MR. FORD: May I ask you, simply related to your responsibility within Westinghouse and in your own area concerned with metal-water reactions, is the practice of relying uncritically on documents such as Baker-Just, is that a standard practice or is this an exception?

DR. ROLL: The premise is we have relied on the Baker-Just equation. Is that really your purpose?

MR. FORD: Yes.

DR. ROLL: I must protest the premises. In nothing I have said--you may have drawn from inference that I said that we have relied critically on the Baker-Just equation. Therefore, for you to ask is the use of Baker-Just typical of Westinghouse practice in the area that I am familiar--if I may add a little bit, it is, have you stopped your wife type question.

I don't admit that we have relied critically on the Baker-Just situation. And I don't think I should answer either way to your question.

MR. FORD: Let me put it in other terms. Is the manner in which you have analyzed personally Baker-Just, is that typical of Westinghouse practice?

MR. TROSTEN: I object to that question as well.

CHAIRMAN JENSCH: I think that's the same question previous to this last one. Objection sustained.

MR. FORD: Typical to the practice concerned in your own area, concerning metal-water reaction?

MR. TROSTEN: I would like to have that question rephrased, Mr. Chairman. I don't think it is clear.

CHAIRMAN JENSCH: Could you restate the question entirely?

MR. FORD: Yes, sir.

Is the kind of analysis that you have performed on Baker-Just the standard kind of analysis that is performed by Westinghouse Research Staff in the area of metal-water reactions?

MR. TROSTEN: I'm sorry, Mr. Chairman. That question is also objectionable. I really believe that perhaps Mr. Ford should consult with Mr. Roisman for further guidance in this area.

MR. ROISMAN: Mr. Chairman, I'm not clear. This man does work for Westinghouse, I understand. He does work in the metal-water reaction area. He is presumably one of their top men or else they wouldn't have brought him here. Mr. Ford's question is straightforward. He wants to know whether or not the extent to which the Baker-Just formula was an independently investigated one as to its validity as is discussed in the transcript today and this discussion, and is typical of the extent to which other formulae related to water-metal reactions, the area in which this witness is a specialist. Did they do more or less? I think that's pretty straightforward and this man is qualified to answer it.

MR. TROSTEN: Mr. Chairman, I don't think it is straightforward in the slightest. A question like that could only -- I would think Mr. Roisman would appreciate -- be answered correctly if Mr. Roisman or Mr. Ford were to ask the question with regard to any other particular analysis. How could the witness answer such a question of such a general nature?

CHAIRMAN JENSCH: Let's not get into a discussion between you folks. I think when you use that phrase, is this typical or standard Westinghouse practice, then he gets into a scope of review that I don't think has been shown by the foundation of this question. I take it what you are trying to find out is, is this kind of analysis the kind that Dr. Roll does in his own work. I think that is a different question.

The objection is sustained.

MR. FORD: Is this the kind of analysis typical in your own work?

DR. ROLL: I think the answer is no. Let me try to characterize here. That is, in areas where we are working now on fuel performance model development and its application, we review the literature and we review our own data quite critically. We come up with a materials performance model which characterizes the data. The selection of the Baker-Just equation was made, as best as we can determine, perhaps five years ago, that that was the equation to use at that time. At that time people who were in the metallurgy department or on the staff of the metallurgy department reviewed what was available. This is a published document and was selected for the zirc-water calculations.

Subject to the selection of this equation for its use, it has been critically reviewed as new data becomes available, as evidenced by our own review of this equation with our own data in WCAP 7665.

MR. FORD: In terms of the review which you refer to in WCAP 7665, are you citing specifically Section E.4.1?

DR. ROLL: I am really citing Appendix B.

MR. FORD: Appendix B is on a number of phenomena. I am trying to find out exactly where in Appendix B. My suspicion is what analysis there is is in E.4.1. I just want to get the reference straight.

DR. ROLL: Again, only Appendix B provides a discussion of our observations, a discussion of the data we took as part of the FLECHT program.

MR. FORD: For the record, I am trying to clarify which part of Appendix B is confirmatory of Baker-Just. There are a large number of metalographic discussions here and there are some comments on Baker-Just in a certain discussion. I think it is E.4.1. I just want to make sure that in terms of our consideration of what the evidence is, that we know what the evidence is.

DR. ROLL: I believe the total discussion must be taken in that contrast rather than an abstract, and say this provides the confirmation.

MR. FORD: For example, could you tell me how Figure B 11, an inverse poll figure for as fabricated heater rods, is that related in an direct or indirect way to your confirmation of Baker-Just?

DR. ROLL: It is merely a statement of fact relative to the texture of the zirconium product.

CHAIRMAN JENSCH: Can you give us an answer to the question? Is it related to Baker-Just?

MR. FORD: The confirmation of Baker-Just.

DR. ROLL: It is not part of a confirmation of Baker-Just.

MR. FORD: Are you trying to narrow down? I see you flipping through it. Are you continuing to try to answer my question?

DR. ROLL: Yes, if you want to press on. Section B 4 with the appropriate tables and graphs -- Section B 3 discusses the oxide thickness measurements, I believe. Section B 2 discusses the preparation which went into the oxide thickness measurements which were then used in Section B4 to discuss the correlation.

MR. FORD: I see. In terms of the graphs that you refer to, there are two of them, of B 12 and B 13. Our discussions on the record yesterday concerning graph B 12 and how this may or may not be related to confirmation of Baker-Just I refer to. Graph B 13 or Figure B 13, in that can you explain to us how this directly or indirectly supports, confirms Baker-Just?

DR. ROLL: I believe the discussion in the text, Section B 4, points out a conservatism inherent in the way we use the Baker-Just equation. That is, we assume that the total oxygen uptake goes into the formation of zirc oxide, and hence using the Baker-Just as all of the oxygen going into zirc oxide, one gets an overprediction of the extent of zirc oxide, which gives us an overprediction of the heat evolution. The Figure B 13 is then an important trail of the relationship between the oxide thickness and the oxygen stabilized alpha observed from the metalography for the particular examples.

MR. FORD: My question is, independent of what these graphs may or may not have to do with the conservatism with which you use Baker-Just, can you tell me how they confirm the validity and the applicability of Baker-Just for your use, specifically with reference to Figure B 13? You have already discussed B 12.

DR. ROLL: B 13 is presented in support of the discussion in the text in this particular section of the report.

MR. FORD: Yes. In support of your discussion of the conservatism with which you use Baker-Just.

Now how does it relate to a confirmation of the validity of Baker-Just itself?

DR. ROLL: As part of the discussion comparing or contrasting our data points with Baker-Just this, the second figure that is, B 13, is presented to show the data on zirc oxide thickness and oxygen-stabilized alpha.

MR. FORD: Now can you tell me if Baker-Just were true what would they predict as the relationship between the maximum alpha layer thickness and the maximum oxide thickness?

DR. ROLL: It would not predict that.

MR. FORD: I see. So the fact that this prediction or this result is not related to any prediction or Baker-Just, is that correct?

DR. ROLL: I believe that's correct.

MR. FORD: So that if this were true it would not confirm Baker-Just and is it also correct that if it were false it would not refute Baker-Just?

DR. ROLL: If this were true what did you say?

MR. FORD: If the relationship you brought here were a true relationship between maximum alpha layer thickness and maxiumum oxide thickness, that if that were a true relationship, so what, it's true! It doesn't have any --

DR. ROLL: It's just data.

MR. FORD: But the relationship here, that there is no logical nexus between Baker-Just equation and the nature of this relationship here, is there.

DR. ROLL: The Baker-Just equation does not predict relationship between these two parameters.

MR. FORD: Thank you.

In your studies of metal-water reactions have you evolved any data concerning the temperature drop across the oxide film?

DR. ROLL: We have not.

MR. FORD: Would you turn to Baker-Just page 31, please. It's Section D, calculation of temperature drop across the oxide film. It says, I quote, "Some authors have emphasized the importance of the heat insulating effect of the oxide film on metal gas reactions."

Can you tell me whether in your analysis of metal-water reactions of zircalloy-water reactions whether you consider the heat insulating effect of the oxide film of metal-gas on that zircalloy-steam reaction?

DR. ROLL: That's related to the detailed calculations of the loss of coolant accident. I'd like to refer to Mr. Moore for that answer.

MR. MOORE: Yes. I indicated in previous testimony we did include the effects of the reaction.

MR. FORD: No. My specific question was whether you considered and analyzed the heat-insulating effect of the oxide film on the reaction. I am not just concerned with whether you considered some relationship between the oxide film in the reaction. My question is whether you consider this heat-insulating effect of the oxide film on the zirc-water, zirc-steam reaction.

DR. ROLL: Yes, we do.

MR. FORD: What experimental data have you evolved, I mean upon which you base the specific manner in which you treat this effect?

DR. ROLL: I don't have any reference right here at the moment, but there are conductivity measurements that have been made on zirc-oxide and its this conductivity that's assumed in the analysis of the effects on the surface temperature of zirc.

MR. FORD: I see. Now is this conductivity measure influential in determining the temperature drop across the oxide film.

MR. MOORE: Yes, directly.

MR. FORD: Yes, I realize that. The point is Dr. Roll indicated a few seconds ago that you have no data on the temperature drop across the oxide film. Do you wish to change that?

MR. MOORE: Do I wish to change that?

MR. FORD: Yes. Do you have any data on that?

MR. MOORE: No. I would say we have data on the conductivity of zirc oxide. I don't recall whether we made any specific measurements in the course of a zirc-water reaction situation, but it's the zirc oxide conductivity that's of interest, and we have data on that, yes.

MR. FORD: What kind of error is introduced into your analysis if you have misanalyzed the heat-insulating effect here of the film? Do you have any data pertaining to the magnitude of this error?

MR. MOORE: At the conditions that obtain during the zirc-water reaction period of time as I indicated in previous testimony the power level of the rods is down considerably. So the total temperature drop across a mil of zirc oxide would be in the order of three degrees, three to four degrees. So a 100 percent error in this conductivity could put the temperature difference off by another three degrees, for example. It's a small matter.

MR. FORD: I see. Now my question was whether you had any experimental data pertaining to the magnitude of the error introduced by misanalyzing heat-insulating effect of the oxide film.

MR. MOORE: That's a straightforward conduction calculation. It's a straight conductivity across the zirc oxide. There is no experimental error -- I guess I would go back to fundamental heat transfer experiments for centuries.

MR. FORD: Yes. But under the specific conditions of the metal-water reactions you have no germane data, experimental data.

MR. MOORE: Do you understand the situation I am talking about?

MR. FORD: Yes, I understand it.

MR. MOORE: I am not sure about the question.

MR. FORD: What I am asking is not whether there is some reason to believe based on an argument and your general knowledge of heat conductance and so forth. I am not asking whether there is some reason to suspect that it would be any problem. I am asking the explicit question, have you performed an experiment that correctly simulated the conditions of metal-water reactions in a loss of coolant accident at which specifically analyzed the magnitude of the error involved in your estimates about reaction rate introduced by misanalysis of the heat-insulating effect of the oxide film.

MR. MOORE: No, because none were required.

CHAIRMAN JENSCH: Maybe this is the time to have a break. At this time let us recess to reconvene in this room at 3:50.

MR. TROSTEN: Mr. Chairman, may I ask just one question about Dr. Roll again. I hate to keep raising it again, but do you think Mr. Ford that you will be finished with Dr. Roll today?

MR. FORD: I think we're probably pretty near the end, yes.

CHAIRMAN JENSCH: Let recess and reconvene in this room at 3:50.

(A brief recess is taken.)

CHAIRMAN JENSCH: Please come to order. Are you ready to proceed, Intervenors' interrogator?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Will you proceed, please.

MR. FORD: Have the control rod materials been tested under loss of coolant accident thermal transient conditions?

DR. ROLL: Yes. The control rod materials that sees the loss of coolant accident is stainless steel, and we have not conducted specific tests related to performance of stainless steel under these sets of conditions. I believe that the performance of stainless steel at temperatures in environmental conditions of interest should be well documented in the literature.

MR. FORD: Now referring to the Oak Ridge report that we have gone through, the McLain report, page 85, I am referring to page 85, Section 2.5.6. I will read it as you study it. It says, "The neutron absorbing material in the BWR control rods is boron carbide and that in the PWR control rods it's silver-cadmium, indium alloys. Both of these materials are normally encased in stainless steel tubes. During unlikely nuclear reactor accident it is possible that the stainless steel could react and expose the neutron-absorbing material. Boron carbide can react with air at temperatures above 450 degrees Centigrade to form E_3O_2 oxidation product."

This is peripheral. I should know. I will stop reading.

This is pertaining only to the PWR. Let me go on to the section controlling PWRs. It says, "For the control rods containing the silver-cadmium-indium alloys no significant metal-water reaction in control rods stainless steel sheathing would take place until the temperatures were greater than about 1200 degrees Centigrade. Little has been found regarding metal-water reaction of silver-cadmium-indium alloys at these temperatures but it appears that the alloy simply flows out of the control rods since it has a melting point in the range of 775 to 826 degrees Centigrade."

MR. TROSTEN: What page of the exhibit?

MR. FORD: As I indicated earlier this is Exhibit M and I am referring to page 85, 86, Section 2.5.6, Reactions with Control Rod Materials.

My question is what analysis and experimental data has Westinghouse evolved in the possibility and significance of these metal-water reactions?

DR. ROLL: The analysis would be a consideration similar to reported here. We do not have experimental data for the silver-cadmium-water reaction rate kinetics. However, inasmuch as they are encased in stainless steel -- In fact the FLECHT tests which were run with stainless steel rods are germane to that consideration. Therefore, for the conditions of the LOCA the stainless steel-water is quite compatible.

MR. FORD: I see. Now you are talking about the FLECHT tests that contain stainless steel rods?

DR. ROLL: That's correct.

MR. FORD: What was in them?

DR. ROLL: Academic. I am just saying in terms of the stainless steel-water compatibility those FLECHT test results which were run with stainless steel rods attribute that to the conclusion that there is not a control rod problem.

MR. FORD: Yes. I am concerned not with reactions beginning on the outside but reactions beginning on the inside. Have you ever subjected the control rods, stainless steel containing the silver-cadmium-indium alloys, have you ever subjected these to the temperatures that would be expected on their loss of coolant accident to see whether in that situation one of the many possible reactions that we observed between containing materials, such as possible reactions between U_3O_8 and cladding and so forth, to make sure that there would be no reaction between the alloy contained in the control rod and the steel of the control rod that would contribute to both melting of the steel and the further release of this material out into the core environment for potential further reaction?

DR. ROLL: One moment, please. The temperatures of the control rods during the loss of coolant accident will be below these temperatures stated here. That is I believe melting points. Or the stainless steel-water reactions, the temperatures of the control rod should be quite low, say less than 1000 degrees.

MR. FORD: Let me get clear on what the source for that assertion is. In the FLECHT test is when you are simulating bundles, an individual bundle, and was there a control rod placed in proximity to the bundle undergoing the thermal transient to determine how the heating of the bundle affects the heating of the control rod?

DR. ROLL: There were control thimble simulations in the FLECHT test.

MR. FORD: Just in terms of the basic geometry of test bundles as we understand it, as I understand the test bundles from your documents. You have a ten-by-ten or seven-by-seven array of fuel rods outside of which there is a channel housing for the purpose of the test. It doesn't, of course, exist in the reactor. How is a control rod simulated here?

DR. ROLL: As I said, the control rod thimbles control it.

MR. FORD: What is a control rod thimble?

DR. ROLL: Should we go back to Section 3 and go through a picture of a fuel assembly?

MR. FORD: Could you draw me a picture of a cross-section of a test bundle?

DR. ROLL: If need be, let us refer to the FSAR. There is a picture in there.

MR. TROSTEN: I believe it is sitting on the table in front of you there, Dr. Roll. Do you have a copy of it, Mr. Ford?

MR. FORD: No.

MR. TROSTEN: Perhaps we can make one available to you.

MR. FORD: Independent of the picturization of the FSAR, is it depicted in the FLECHT test documents?

DR. ROLL: Can I proceed?

CHAIRMAN JENSCH: There may be a couple of questions pending. The last question is, is this picturized in the FLECHT test reports?

DR. ROLL: Let me refer to the Applicant's

Volume II of the safety analysis. Figure 3.2.3-8 gives a cross-section of a fuel assembly. The basic concept of a fuel assembly is that we have a fifteen-by-fifteen array of rods, and twenty of these rods are replaced by stainless steel thimbles or control rod guide tubes. The control rods then, when they are inserted in the core, are inside of these thimbles, but the thimbles are really between the control rod which in turn is another stainless steel rod, and the fuel rods themselves.

Referring again to the FLECHT report, which is WCAP 7665, Figure 2-6 and 2-7, they show the layout of the fuel rods and the simulated guide thimbles that indicates the manner in which the guide thimbles -- The first figure refers to this and describes where the guide thimbles are in the fuel assembly itself, and the second figures are the way in which the guide thimbles were simulated during the seven-by-seven and ten-by-ten FLECHT tests.

MR. FORD: As I understand the Figure 2.6, the thimble is indicated by the cross mark in a set of tubes.

DR. ROLL: That's correct.

MR. FORD: This is very interesting. In terms of the manner in which -- Just the geometry of the control rod thimbles in the reactor itself, are they in similar locations as in this test bundle, or are they between bundles?

DR. ROLL: I refer you back to the figure in the SAR, if you wish to just look at this. You can compare that to the figure in the FLECHT report.

MR. TROSTEN: What document have you just shown to Mr. Ford?

DR. ROLL: Figure 3. --

CHAIRMAN JENSCH: 3.3.2-8.

DR. ROLL: That's correct, from the SAR.

MR. FORD: As I understand the figure, this is a cross-section of how many bundles?

DR. ROLL: That's four bundles.

MR. FORD: Can you show me how the thimbles are represented here?

DR. ROLL: Twenty circles in the midst of the fifteen-by-fifteen array.

MR. FORD: In terms of your simulation as shown in Figure 2.6, what analysis have you performed? These are unheated, of course.

DR. ROLL: The thimbles are unheated, yes.

MR. FORD: What analysis have you performed on both the similarity between the radiant heat transfer in-core to the thimbles and the similar conditions for radiant heat transfer in the simulated bundle? Are they perfectly identical? You are dealing with much smaller bundles.

DR. ROLL: Negative. The rod size, the thimble size and the pitch were identical in the FLECHT test and in the --

MR. FORD: That's the same, yes. But the number of rods in the test, in proximity to bundles and the whole configuration --

DR. ROLL: The best you can do is put eight around a rod.

MR. FORD: It is not clear to me, in Figure 2.6, that this is done at all, that there is any rod surrounding it. Do you mean eight in all directions from the rod?

DR. ROLL: Refer to Figure 2.6, the figure in the lower right-hand corner. It is surrounded by eight rods. The figure in Position C-6 is surrounded by eight heated rods. The thimble at Position F-3 is surrounded by eight heated rods.

MR. FORD: It is eight on the perimeter?

DR. ROLL: Yes.

MR. FORD: In terms of the heat transfer heater, to what extent does the housing heater, the thimble, to what extent have you analyzed it as acting as a heat sink in the FLECHT test bundle? What would be the temperature of the rods around it were it a read rod rather than just an empty unheated thimble?

DR. ROLL: Please rephrase the question.

MR. FORD: What analysis have you performed of the radiant heat transfer from the rods surrounding the thimble to the thimble such that you can then calculate what would be the difference in temperature for a specific location in the bundle? If instead of putting a simulated control rod thimble in that location, you had had another rod which wasn't acting as a heat sink surrounding rods?

MR. MOORE: No analysis. That's not a real configuration. You are saying what if I had a rod where I had a rod thimble, and what would happen then?

MR. FORD: I am saying, this simulated thimble acts as a heat sink with respect to raising energy from the rods around it; is that correct?

MR. MOORE: That tends to. It is a small effect, but it is an effect, yes.

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MR. FORD: What I am asking you is whether you have determined this effect?

MR. MOORE: From what standpoint?

MR. FORD: I mean experimentally. I am asking the manner in which you might determine it would be to run a test with the simulated thimbles in certain conditions and simulated conditions, and run another test in which you didn't have these thimbles but just had more simulated fuel rods. You would be able to compare the temperature of the rods in one test surrounded an empty thimble with the temperatures that they achieved when they didn't have that heat sink available.

In other words, you would be able to determine whether or not you are correct in saying that radiant heat transfer is not significant?

MR. MOORE: Why is not the measurement of the temperature of the thimble in the exact test a measure of whether we have significant radiation or not? For it is partially a measure of that. Have you understood--

MR. FORD: It is a direct measurement. It is a direct measurement of that?

MR. MOORE: Yes.

MR. FORD: What I am further concerned with is how this relates to temperature of the rods that are releasing that radiant energy.

Y2 Wt2

MR. MOORE: They are also measured.

MR. FORD: Is the caution of their release that is in radiant form versus convective cooling, is that determined?

MR. MOORE: It is not broken out in the specific test here in radiant versus convective heat transfer.

MR. FORD: In any of the FLECHT data that you have, is that separated?

MR. MOORE: No.

MR. FORD: Not at all?

MR. MOORE: No.

MR. FORD: Dr. Roll, I refer you to transcript, page 1719. The question I have concerns the use--this is to Dr. Roll. The question I have concerns the assertion by Mr. Moore that experiments summarized and referenced by Baker-Just support his contention that there are no metal-water reactions at or below, I presume, 1800 degrees Fahrenheit during the loss of coolant accident transient.

I'd like to ask you to complete our discussion of Baker-Just. I would like you to indicate which experiments summarized and referenced in Baker-Just, which experiments confirm that below 1800 degrees Fahrenheit metal-water reactions don't occur.

MR. MOORE: Mr. Chairman, could I read my answer again that Mr. Ford is referring to?

The question was: "You said that you predict for

Y2Wt3

this plant no metal-water reaction at 1800 degrees Fahrenheit."

My answer was: "Yes, I predict no metal-water reaction at 1800 degrees Fahrenheit."

That's because, as I explain in the earlier testimony, we cut off the metal-water reaction below 1800 degrees.

MR. FORD: I am concerned with your statement in my question addressed to Dr. Roll. It says, in Mr. Roisman's questions, for the basis of your statement about a metal-water reaction to 1800 degrees Fahrenheit. You say, "These are based on experiments that have been performed by others." That is not Westinghouse. "I'm not aware of any specific Westinghouse experiments in this area. But these are the experiments which were added to and summarized in the reference by Baker and Just that I believe you have from Oregon." That should be Argonne, A-r-g-o-n-n-e.

"That's the basis for the parabolic rate assumption."

I have reviewed the experiments which were summarized and referred to by Baker and Just. I'd like to have Dr. Roll give me his impression as our metal-water witness, as to which of the experiments summarized and are referred to in Baker-Just support Mr. Moore's assertion about metal-water reaction.

CHAIRMAN JENSCH: Since they are conferring Messrs. Moore and Roll, you might ask Mr. Moore.

DR. ROLL: Mr. Chairman, I think it is important that we understand the context of this whole page. We don't

Y2Wt4

have a problem answering the question. But we want to put the whole page of discussion in context. You are asking a specific question on a specific response. It is not clear as to the total context of the response is the way you are referring to it.

MR. FORD: Do you want to make a comment on it?

MR. TROSTEN: For purposes of helping me understand it, are you asking Mr. Moore to verify his testimony or are you asking Dr. Roll to supply information and Mr. Moore said he wasn't in a position to supply it?

MR. FORD: Mr. Moore didn't indicate anything about his position to supply this, as you look at the transcript. He just didn't say. He referred to experiments referred to where Baker-Just didn't say what they were. We are asking Dr. Roll because we want to pursue the basis of these tests referred to in your early confirmation of Mr. Moore's assertion.

Z1Bt1

MR. ROLL: The report by Baker-Just does not say there is no zirc-water reaction below 1800 degrees F.

MR. FORD: No, but--

MR. ROLL: The methodology used by Westinghouse arbitrarily cuts off a consideration of zirc-water reactoin below 1800 degrees F. as a calculational convenience. The justification for doing this was entered into the transcript yesterday morning by Mr. Moore where he stated that if he didn't do that, if he carried the time temperature calculations throughout the entire power history, that part of which is below 1800 degrees F. it results in a .0005 per cent difference in the answer.

MR. FORD: Yes. But now my point is simply that Mr. Moore referred to experiments referenced by Baker and Just. When called upon to give experimental data pertaining to his assumptions that there are no metal-water reactions below 1800 degrees Fahrenheit, I want to find out what experimental data you are talking about here and then I want to pursue what relevance it has.

Let me ask Mr. Moore. Would you simply refer to pages 64, 65 and 66 of Baker-Just and just tell me which of these forty-two references do you regard as containing evidence on the question of whether or not there are metal-water reactions during the loss of coolant accident at less at 1800 degrees Fahrenheit. I am simply asking you

Z1Bt2

for the number of references, not for elaboration.

MR. MOORE: Fine. My answer was predicated upon statements that were made in the Baker-Just paper to which we referred to many times today, where he indicated that the literature of metal-water reaction studies was recieved in two previous reports. Previous results pertinent to zirc-water reaction will be summarized here and he goes on to indicate other references that he has used in oder to embody a large amount of information for these authors in order to come up with a correlation. That's all I was referring to.

MR. FORD: Yes. Now can you indicate in response to my question, referring to his list of references, you are referring to his summary of things that he then refers to, can you refer to his list of references, and telling me which of those studies present the evidence that you are talking about?

MR. MOORE: No, I am sorry, I cannot.

MR. FORD: I see.

Is this .005 that you predicted based on Baker-Just?

MR. MOORE: Yes. .0005.

MR. FORD: Yes.

Now as long as you are unable to make references let's just go through the experiments that Baker & Just summarized and let me address myself to Dr. Roll now. He refers--

MR. TROSTEN: There appears to be an error in the transcript, by the way, Mr. Chairman, on page 1831. This may be contributing to some uncertainty.

MR. ROISMAN: Did you say 1831?

MR. TROSTEN: 1831 in Line 22. May I direct Mr. Moore's attention to Line 22 of page 1831, and isn't that figure .0005 rather than .005?

MR. MOORE: That's correct.

That's three zeroes, five.

MR. TROSTEN: Right. That point, I believe, should be noted for the record.

MR. FORD: Can you give us the primary source for that data so that we can just get it all together?

MR. MOORE: Primary source of what?

MR. FORD: Of the 00 plus another or not another 05 reference.

MR. MOORE: Yes. I indicated in the previous testimony yesterday that that was the calculation of the core-wide metal-water reaction for the double-ended cold leg break, assuming the Baker-Just correlation held over all temperatures rather than just from 1800 degrees and up.

MR. FORD: I am looking for the reference to the report of those calculations.

MR. MOORE: There is no report of those calculations. They are the calculations that Westinghouse has performed.

Z1Bt4

They are not reported.

MR. FORD: It's only through that that we find them?

MR. MOORE: That's correct.

MR. FORD: I see.

Now this is to Dr. Roll.

In terms of the experiments summarized by Baker and Just, these begin on Page 9 and they go on to Page 12. I am not going to go through all of them. I just want to get some of the flavor of these experiments.

He refers on Page 10 to experiments performed by Milich and King, which is reference 6 in his notation, which is W. Milich and E. C. King, Molten Metal Water Reactions, M.P. 5813, Technical Report No. 44, November, 1955.

Now at that report he says, "Milich and King melted zirconium rod and dropped ten to twenty gram batches into water under various conditions of temperature and pressure, reference 6. They used induction heating within a high-pressure autoclave. They found that high pressures and inert gases suppressed the reaction, whereas high steam pressure gave very extensive reaction."

Now in terms of support for what metal-water reaction rate we get at less than 1800 degrees, is it your judgment, Dr. Roll, that this experiment as summarized here is any evidence in that matter?

DR. ROLL: Reading this paragraph, inasmuch as they were dropping melted rod into water it's obvious they are above 1800 degrees Fahrenheit.

MR. FORD: So that the answer is no?

MR. ROLL: Correct, that's right.

MR. FORD: Now let me go on.

He refers in the fifth paragraph on page 10 to studies of the Aerojet General Corporation which are given as references 10-12. I think we will just say that his reference is 10 to 12 rather than reading the names into the record. Let me read his paragraph relative to that.

"Extensive studies of metal water reaction were reported by Higgins and others of Aerojet General Corporation. References 10 to 12, one-inch diameter streams of molten zirconium were discharged into water in one study." And it goes on to give other data about particle reactions and so forth. I don't want to prevent you from reading the whole paragraph if you care to take that time.

Have you finished reading?

DR. ROLL: Okay.

MR. FORD: Now in the test as it's summarized here involving one-inch diameter streams molten zirconium discharged into water, does this data have any obvious reference to metal-water reactions of less than 1800 degrees?

DR. ROLL: Yes, it does. As follows, and perhaps it's going to add to my previous answer.

Z1Bt6

What we are doing here is measuring the net reaction after some time and temperature and with that data from a series of experiments attempting to put together an equation which really defines an instantaneous rate as a function of time and temperature and with its ultimate application being an integration over the assumed time-temperature history.

So that to the degree that every one of these molten samples had to cool down before one could determine what was happening they went through 1800 degrees Fahrenheit and that part at least, you know, that is--it's not direct evidence at 1800, it's not a rate measurement, which is difficult.

MR. FORD: Yes, but--

DR. ROLL: But in a sense we have covered a range of temperatures in these experiments. We have in some way covered the range of interest.

Now further, inasmuch as we, the authors, find that the kinetic constant can be plotted up in a somewhat traditional form as a function of reciprocal temperature, and inasmuch as you have some data which you can infer a line through literally curve fitting, you, even with the higher temperature data, you are getting some competence in the lower temperature performance.

MR. FORD: Now Dr. Roil, the test here says nothing or in any of these tests dropping molten balls, molten spheres of zirconium into water the tests say nothing about raising these incrementals up past 1800 degrees Fahrenheit in the steam atmosphere and studying the metal-water reaction. Isn't it very clear from these tests here, can you point out a test that's different, that simply taking molten metal raised to predetermined temperatures with no analysis of what is involved and what happened to them at 1800 degrees and then just dropping them into a bucket of water, a sophisticated bucket of water --

DR. ROLL: Right. Then they could raise this mass of metal, so be it, up to the temperature of interest, up to the melting point or beyond the melting point in an inert atmosphere. But when they drop it in the water it's going to cool. So it's going to come back down through the temperature range of interest.

MR. FORD: Yes. But in terms of when you end up measuring what went on you don't have -- Or do you claim that you can in this kind of test, that you can take the zirc oxide formation from one of these tests and say so many mils of it came while it was cooling from 4000 degrees fahrenheit to 3000 degrees, so much of it came -- I mean determine this in the experimental stage? I am asking you can you determine that so much came between four and 3000, so much between 3000 and 1800 and then say, "Aha, in this test now we can also say that at 1800 degrees this is what happened."

DR. ROLL: No, you can't do that.

MR. FORD: Thank you.

Now can you tell me in any direct way does this test, this Aerojet General test here, does this confirm the fact that there is no metal-water reaction at below 1800 degrees Fahrenheit? Yes or no, please.

DR. ROLL: No.

MR. FORD: Thank you.

I don't intend to go through the few dozen other tests that I reported here, but if you looked at them do you have any doubt that they differ substantially in relation to the question of demonstrating no metal-water reaction below 1800 degrees from the couple that we have gone through in more detail?

DR. ROLL: Do I have any doubt that the conditions of the experiment, the methodology varied?

MR. FORD: Yes, that these are all basically descriptions of dropping molten spheres into water and they are clearly at temperatures of several thousand degrees Fahrenheit, or I mean 3, 4, 5000 degrees Fahrenheit. It has nothing to do with -- I mean they are simply not at temperatures 1800 degrees or less.

DR. ROLL: No, not clear at all.

MR. FORD: I see. Well, I'd be interested if you can find the test among all these tests dropping metal spheres that relate to 1800 degrees.

DR. ROLL: It appears that the GE references by Epstein at 1523, A Reaction Rate of Clean Surfaces of Solid Zirconium, et cetera, et cetera, so they are talking about surfaces in that study.

MR. FORD: Yes. They are talking about surfaces. What is the temperature range?

DR. ROLL: So there is one range that isn't dropping molten globules into water.

MR. FORD: Fine. For this unique reference it supposedly in summary presents confirmation of what we have been discussing. Is there an indication here or are you familiar with the study itself so that you can tell us what the temperature range of reference is?

DR. ROLL: Well, you asked me a question and then I gave you part of an answer and then you kept on with another one. You are leaving the impression that Epstein's report is the only one that was not dropping globules of zirconium into water.

MR. FORD: I see.

DR. ROLL: If the Board wishes to take the time we can read through all of them and make a commentary whether or not they were molten metal into water. It appears that reference 3 was not such.

CHAIRMAN JENSCH: Was not what?

DR. ROLL: Was not dropping molten zirconium into water.

CHAIRMAN JENSCH: Well listen, the important thing is about temperatures 1800 degrees or less.

DR. ROLL: I don't know. I was asked to confirm or reject the hypothesis that all these experiments were performed by dropping molten zirconium into water and I am attempting to answer that question. Now --

MR. FORD: No, excuse me. There was my hypothesis which now did not stand, that all the tests dropped molten spheres into the water. Now there is at least one test that drops a molten tube or tube whose moltenness is not indicated.

CHAIRMAN JENSCH: Proceed.

MR. FORD: Well, in summary then in terms of the data presented here, you know, as this is our confirmation, can you select anything from the Epstein one that's a tube that directly and generally confirms the assertion that metal-water reactions do not take place, zircalloy-water reactions do not take place at less than 1800 degrees Fahrenheit? Yes or no, please.

DR. ROLL: I don't see why -- The answer is no, there is not a reference here that was a test explicitly up to and including 1800 degrees Fahrenheit, which the results were no zirc-water reaction at that temperature.

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MR. FORD: My cross-examination on the subject of metal-water reactions is completed.

CHAIRMAN JENSCH: Does the Staff have any questions?

MR. KARMAN: No question, Mr. Chairman.

CHAIRMAN JENSCH: Any redirect?

MR. TROSTEN: We may well have redirect testimony, Mr. Chairman. We will have to scrutinize the transcript and make that determination.

CHAIRMAN JENSCH: The Board has a few questions.

MR. BRIGGS: Dr. Roll, I have just a few brief questions that I'd like to ask to clarify a point or two in my own mind.

What is the thickness of the zircalloy clad on the fuel rod, the normal thickness?

DR. ROLL: The nominal thickness of the cladding is 24.3 mills.

MR. BRIGGS: When cladding swells how much thinning occurs?

DR. ROLL: Swells and the loss of coolant bursts, for example?

MR. BRIGGS: That's right. What range of thinning? Do you have any idea?

DR. ROLL: I believe that a fifty per cent thinning, inasmuch as we get fifty per cent diametrical expansion typically, I think that is the range of thinning we are seeing.

Z3Bt2

MR. BRIGGS: So then the clad thickness in some areas would be, let's say, sixteen mills then of course I suppose near a burst why the cladding thins down to something considerably less than that. What is the distribution of oxide in the cladding and in the absence of any swelling? In other words, how is the oxygen content graded from the outside of the clad to the inside of the clad following a loss of coolant accident? Is that clear?

DR. ROLL: I believe so. There is, of course, the zirc oxide solid layer on the outside of the cladding.

MR. BRIGGS: Roughly how thick is that layer? Let's take this in the context of a tube that has an oxide content at the end of the incident of seven and a half per cent. That is the hot spot on your tube.

DR. ROLL: Sir, that would be seven and a half per cent of 24.3.

MR. BRIGGS: That would be seven and a half per cent of 24.3, that's right.

DR. ROLL: There is a difference between the densities of the zirc and the zirc oxide, so there is a conversion there.

MR. BRIGGS: Well, what I am getting at is most of the oxide on the outside of the metal is on the outer layer of the metal, or is it uniformly distributed through the metal or just--

DR. ROLL: No, sir. The oxide is one hundred per cent zirc oxide on the outside and it is essentially base metal in the remaining 22 mils of tubing. There is a gradient then from the base metal through an oxygen-rich metallic phase through a very thin, characterized by black oxide, layer on the order of tenths of hundredths of microns, tenths and hundredths of mils and then into the zirc oxide thickness.

So that it is not distributed uniformly throughout the cladding.

MR. BRIGGS: Well, there is an oxide layer on the outer surface, and what about the region that has oxygen dissolved in zircalloy. Is that an important part of the oxide-containing material?

DR. ROLL: Sir, this is the oxygen-stabalized Alpha that is discussed briefly in our own FLECHT reports and at least in these reports it was about equal in thickness to the ZRO_2 layer.

MR. BRIGGS: Is that a brittle material also?

DR. ROLL: I believe it is, sir.

MR. BRIGGS: Now in the case of the cladding, that was only sixteen mil thick, then the percentage of oxygen in that zircalloy would be above the seven and a half per cent, is that right?

DR. ROLL: That's correct, sir.

MR. BRIGGS: So it might be up ten per cent or thereabouts, I suppose.

Z3Bt4

DR. ROLL: Yes.

CHAIRMAN JENSCH: Do you want him to agree to that?

MR. BRIGGS: He did.

DR. ROLL: Yes, sir, I agreed.

MR. BRIGGS: Is there any information concerning the relationship between middle thickness, nil ductility temperature and oxide content? In other words, if a material is ten mils thick and has ten per cent oxide in it does it have a lower nil ductility temperature than a material that is thirty mils thick and ten per cent oxide content, or are they roughly the same, the nil ductility temperatures?

DR. ROLL: Given the same oxide content or perhaps the same fraction of base metal remaining undisturbed or unaffected by the oxygen we should be seeing the same nil ductility transition temperatures.

MR. BRIGGS: Were the tests that Westinghouse ran run under conditions of various metal thickness or one metal thickness? In other words, is there a reference that I can read about this?

DR. ROLL: The tests that were run were so-called quench tests and were run both with cold tubes and tubes that had been previously burst. So those that were run with unburst tubes were run with this reference 24.3 mil thickness. Those which had previously been burst had lesser thicknesses in the immediate area of the mouth of the burst.

MR. BRIGGS: But there were no systematic tests run in which a metal thickness was buried in the oxide constant and held constant in that temperature?

DR. ROLL: That's correct.

MR. BRIGGS: There was one other question not particularly related to this. I don't know if you or Mr. Moore might like to answer it.

Why is it if the rods, if they are all at the same temperature, if the cladding is all at the same temperature -- Let's change this. Suppose I had ten rods and they were at the same heat rate and the cladding at the same temperature, and this temperature was more or less uniform over a length of two fee, why is it that the rods would not all burst in the same plane or would not all swell in the same plane? Why is the swelling distributed over some length of the rods?

DR. ROLL: Given the premise that the rods are all at the same temperatures and all of the same internal pressure, then there is still the variable of wall thickness, which is a somewhat random variable and appears as manufactured tubing, which would tend to give us an axial distribution, and in fact, circumferential distribution of bursts. The variability in wall thickness or particular in strength of the tubing is characterized by the variability, say, in the yield stress which might run five to ten percent. So that we have a five percent variability in the strength of the tubing which tends to give us a randomness. But then in addition we will get different axial locations merely due to the fact that a perfect uniform axial distribution and exactly the same internal rod pressures simply are not the case in actual practice.

MR. BRIGGS: Do you mean the strength of the tubing varies from position to position in a single tube?

DR. ROLL: Yes, sir.

MR. BRIGGS: So you say the wall thickness is one factor that can cause randomness and swelling location, and strength is another factor? Are there amy other factors that you know about?

DR. ROLL: To a lesser degree, radiation history would contribute to axial randomness. I say to a lesser degree because after some period of operation, perhaps, in excess of six weeks, the data we have says that the yield stress essentially saturates so that further exposure does not cause an increase in yield stress. In addition, the temperatures we are looking at in the loss coolant situation, damage due to irradiation, lattice damage, which is manifested by an increase in yield stress, would be out. So we wouldn't really see this variability due to radiation history being above some temperature perhaps at 1200 to 1500 degrees.

MR. BRIGGS: Skipping back and asking one last question. In the burst rods that were run in tests, was the oxide content of the metal measured in the region of the burst and compared with the oxide content in the full thickness of the rod? I will call them such microscopic measurements. Were they made?

DR. ROLL: In the burst test or the quench test?

MR. BRIGGS: Quench test of burst rods. I'm sorry.

DR. ROLL: I believe the percent oxide content reported is the maximum value seen for several measurements for the particular test which we ran. In terms of percent reaction, this would carry in areas where the oxide had thinned. It would tend to occur there because we have less base metal and dividing by a smaller number as the total base number remaining. The number reported in our own documentation where we report whether or not a particular quench test was retained and had its integrity or not, and related that to percent reaction or percent conversion to zirc oxide, that number is the largest number observed on several seconds. It was not ten per rod, but perhaps two or three per rod.

MR. BRIGGS: What report is that?

DR. ROLL: I believe it is WCAP 7379. Let me confirm that. It is a single rod test. It is 7479-L Volume I.

MR. BRIGGS: Thank you.

MR. ROISMAN: Mr. Chairman, just a couple of things that Dr. Briggs raised. Just a couple of more questions and we will wind up. I just have one.

Dr. Roll, you talked about the variation in the wall thickness in normal manufactured rods. Can you tell me, what is the range in mils for that?

DR. ROLL: The range is like one mil. That may be a half or one and a half. It is not a tenth of a mil.

MR. ROISMAN: The rod tests that were run that are reported in single rod tests, were those tests run with what you would consider -- Perhaps this is something that Mr. Moore would have to answer.

DR. ROLL: That's right. This was production tubing manufactured by the same manufacturer.

MR. ROISMAN: The reason I asked, when I looked at the reports, I notice that the measurement ranged from about twenty-three mils to twenty-seven mils. But that range was a range that you would expect normally if you were purchasing the rods; is that right?

DR. ROLL: Let me check the context of that just a minute.

MR. ROISMAN: Okay. The twenty-seven is reported in Volume II WCAP 7379-L. The others are reported in Volume I, and they were twenty-three and twenty-four mils.

DR. ROLL: I think I can explain that if I can see the report. There were two different lots of tubing used in that. I may be able to clarify that.

MR. ROISMAN: Here you are.

DR. ROLL: I have here WCAP 7379, Volume II, page 2. Our reference wall thickness is 24.3 There is tolerance on that within a lot of tubing. The variability in wall thickness might be on the order of one mil. The tubing used by General Electric in the test reported in Volume II was purchased by Oak Ridge from our manufacturers to an ASTM specification. It was essentially the same as our tubing specification, and Oak Ridge had purchased it to a slightly different thickness, same diameter as our tubing. So the variability from the twenty-seven mils reported in Volume II down to our 24.3 does not reflect the variability in wall thickness to be expected, picking random lots of tubing purchased for the same specification.

MR. ROISMAN: How does it affect the comparison between the results for the same rod in the Volume I test and the Volume II test, assuming identical pressures and identical heating rates?

DR. ROLL: The reason for doing it really was to check -- The reason for doing the GE Cincinnati tests was to check their apparatus and our apparatus. We found essentially no difference. There is a difference in wall thickness from twenty-four to twenty-seven mils, and was attributed to being, again, an insignificant contributor to no difference.

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MR. ROISMAN: Let me see if I understand. In other words, the difference in wall thickness between twenty-seven and twenty-four mils is not a significant difference in terms of when a rod will burst or how much it will swell or something like that, assuming the other things are equal, and you in effect were able to test out your results and the GE results, which are run on different types of apparatus?

DR. ROLL: That's correct.

MR. ROISMAN: Thank you. I just wanted to get that clear.

MR. FORD: Additionally, your answers to Mr. Briggs' question pertaining to wall thickness and nil ductility and embrittlement left me a little bit confused. I was just wondering, in terms of what I have in front of me in the transcript yesterday, page 1964, I wonder if I could refer you to that and make sure that what you indicated to Mr. Briggs is not different from your analysis of yesterday.

The question on 1964 that I asked is:

"Is it your impression that wall thickness is related to embrittlement?"

You responded:

"In my opinion, locally, the integrity of the cladding in the area of burst, which is a very small total area, could be affected by the local metal-water reaction that is related to the wall thickness. The answer is yes.

AA2Wt2

Locally this would have an effect."

Then we go on discussing the conclusion of the Oak Ridge study of Hobson that's already been referenced as the document from the American Nuclear Society's meeting a few weeks ago in which you stated their conclusion, their analysis as you recall, how the nil ductility temperature changes for a piece of cladding with a change in its wall thickness. You quoted their results in that report and you said, "I would agree with that conclusion." That's on page 1966.

Am I to understand that that is your analysis of the relationship between wall thickness and nil ductility and embrittlement, that you do agree with this Oak Ridge analysis as you indicated yesterday, and your answer to Dr. Briggs' question is no change from this position?

DR. ROLL: Page 1966 I believe you said was a--

MR. FORD: I am talking of the portion beginning on page 1964 and ending on page 1966.

DR. ROLL: I believe the question as proposed was, given the same oxygen content but with different thicknesses of cladding, would you expect to see the same nil ductility transition temperatures.

I answered it in the affirmative, yes, I would, given the same oxygen contents. I believe my answer said the same amount of base metal remaining. That is not to

AAWt3

say, given the same per cent reaction or given the same oxide thickness on a varying thickness of base metal, would I expect to see the same ductility transition temperatures, which I think is the postulate of the Oak Ridge people, that if you have the same per cent reaction with the same oxide thickness but different thicknesses of metal and you would have in the immediate area of the burst, you would then see a different ductility response.

I agree with the Oak Ridge statement. If you get local thinning, especially after you get a large build-up of oxide, then perhaps you get different ductility. But if you had a given tubing specimen and you reacted it not to the same extent but to the same oxide content in the base metal, then I would think you would have the same ductility characteristics.

MR. BRIGGS: I think I understood it that way. In looking at the Oak Ridge work, do you understand if the cladding is thin, it would have a larger percentage of oxide than it would if it were not thin? Is that the understanding you have?

MR. FORD: No, sir. It is the fact, I believe, that if the cladding is thin, the percentage of oxides that you get there contributes to more embrittlement problems in that same percentage than in a wall whose thickness hadn't been reduced.

MR. BRIGGS: I think that's apoint that deserves some discussion because that was what my questions were leading to here.

If the wall were twenty mils thick and had been reacted for a particular length of time and a particular temperature, let's say one would have five mils of oxide coming from the outer surface. That would leave fifteen mils of metal. If that wall were thinned and at the same time temperature for the same length of time, then you would still presumably have the same five mils of oxide but now maybe the wall is only ten mils thick. So the percentage of oxide would be substantially increased.

One would expect that the ductility would be substantially less. Was that your understanding?

MR. FORD: That's my understanding of the contribution from Oak Ridge, yes.

MR. BRIGGS: I was just trying to establish whether that was the case here, too.

MR. FORD: Their basic point was that to date the work that has been done on embrittlement has been concerned with rods whose geometry in no way is altered by the accident. Of course, it is known that radiation increases embrittlement. That was one previously known factor. But now the factor that they have contributed --

DR. ROLL: Excuse me. Rather than leave that statement hang, radiation does produce embrittlement, but at the temperatures that are projected for the loss of coolant accident. This irradiation damage is going to be out. So you will see a very ductile material at this high temperature.

MR. FORD: I think the point was, in terms of phenomena which in general influence embrittlement, irradiation is one. That was previously fairly well known. Now the new phenomena which comes is the whole question of cladding thickness. Not just out of burst but just general thinning when the rod is ballooned.

CHAIRMAN JENSCH: Is there any other matter we can take up before the recess?

MR. TROSTEN: Mr. Chairman, I would like to inquire really as to two things: one, does the Intervenor choose to cross-examine Dr. Roll any further, and may he be excused?

MR. FORD: Are you referring to cross-examining Dr. Roll?

MR. TROSTEN: Yes.

MR. FORD: I believe we have completed our cross-examination.

MR. TROSTEN: We would plan to have Dr. Roll excused, if this is satisfactory to the Board.

CHAIRMAN JENSCH: I thought you were going to have some redirect.

MR. TROSTEN: Yes, Mr. Chairman. I'm certain it would take us longer than tomorrow. We would want to resume this next week.

CHAIRMAN JENSCH: And recall Dr. Roll at a later time?

MR. TROSTEN: It may be Dr. Roll or may be additional witnesses, Mr. Chairman.

CHAIRMAN JENSCH: Very well. We would like to discuss the schedule for tomorrow in view of the fact that the parties have indicated that they felt it will expedite the progress of this case if we recess so we can study the various data which was submitted. The Board has accepted that premise. Therefore, we are considering the time of adjournment for tomorrow.

Transportation schedules get a little complicated, and toward the end of the day and toward the end of the week they get complicated. Inquiry is made whether we would pass up the lunch hour tomorrow in order to get in a reasonable amount of time for hearing, but provide for about twenty or thirty minutes of the break at about the usual time, and resume and continue until about 2:30. Would that be agreeable with the parties?

MR. TROSTEN: This is fine with the Applicant, Mr. Chairman.

AA5Wt1

MR. KARMAN: It is agreeable to the Staff, Mr. Chairman.

MR. ROISMAN: We don't have any objection, Mr. Chairman.

There is a question of subject matter that the Aplicant has last evening did make available to us, the three proprietary reports that dealt with rod burst questions. We have been analyzing them today during the session and will continue to do so. That particular subject we may not be able to fill all of the day tomorrow.

Mr. Moore will be the witness who, if I'm not mistaken, will be the one to talk on that. I haven't talked to the Applicant about this. We do have some other safety matters.

There is the in camera session on further security matters. There is the reactor pressure vessel material, and perhaps my colleague here tells me if he can digest this material, he can make it a more fruitful cross-examination.

Since it is proprietary, I will have to talk to the Applicant to see what we want to ask and the charges that we want to make will in any way compromise the proprietary nature of the documents.

Therefore, that portion of the examination would also have to be held in camera. All I am saying is that filling the time from nine o'clock tomorrow morning until

AA5Wt2

two-thirty tomorrow afternoon will be taken up, but may be with some modification of the kind of schedules we talk about. There is no inconvenience to us. I want to make sure it isn't an inconvenience to the Applicant and the Staff.

MR. TROSTEN: Mr. Chairman, I'm not really entirely certain at this point of exactly how much additional cross-examination the Intervenor has. You have the additional document which you have indicated you wish to examine for further cross-examination.

Could you tell me, Mr. Roisman, what additional areas do you wish to cross-examine us on? Let us confine outself to ECCS matters for the time being. What information do you plan on ECCS?

Also, for my information, do you intend that Mr. Ford continue to interrogate?

MR. ROISMAN: Answering the last question first, yes, I do intend Mr. For to interrogate for as long as we can keep him here. In terms of the areas, there is a question of core deformation during blowdown. There is the question of the heat-up of the rods, how those temperatures are computed. There is a question on deformation of the rod that is probably covered in these tests that we have, and some of the earlier ones. The radial flow questions that we started with Mr. Moore and then Dr. Roll was made

made available, and we want to come back to that question.

The codes that are predicting the performance of these various things, which I assume get discussed as they have other questions of the metal-water reaction in the context of the specfici subject we get into, what does the code show with regard to this.

MR. TROSTEN: Yesterday you furnished me with a copy of interrogatories in the Pilgrim proceeding, and a list of questions. Are these the subject of intended cross-examination, or is this for purposes of my general information? I'm not quite certain of the status of that.

MR. ROISMAN: It was more than your general education. What it was intending to do was to see the other areas. The list of the questions I went through to try to get out as a GE reactor, and I wanted to take out those questions which either didn't relate to our case here because of the fact we have a pressurized water reactor, or didn't relate to subjects previously covered. That would be all the questions. I guess theoretically a full enough answer to some of the questions might put off a great deal of additional cross-examination.

MR. TROSTEN: Is it your intention to ask, by way of cross-examination, the questions or the substance of the questions that are indicated on the sheet of paper that you gave me?

AA5Wt4

MR. ROISMAN: Yes. It is not necessarily worded that way. It wasn't like we did with Mr. Moore before on the other question.

MR. TROSTEN: Do you anticipate--and I'm asking this for purposes of planning other witnesses and other matters. Are you able at this point to anticipate when the cross-examination on ECCS will be terminated?

MR. ROISMAN: That's very difficult. I will speak very frankly on the record. The witnesses vary in terms of their capability of answering the questions. Dr. Roll's cross-examination lasted a bit longer than we predicted. Still his answers tended to be fairly succinct and closer to the point. Where we are looking for a no and he knows we are looking for a no, he says no and maybe qualifies it or explains it or does something like that. Other witnesses, as the Chairman pointed out earlier, see where we are going and we are pulling and struggling along. Those kind of witnesses take longer. They shouldn't, but they do. It is difficult for me to know how much kicking and screaming we are going to have to get to the point. I don't think that at this stage of the hearing this is much doubt about what the point is. It is a judgment factor.

Do you have enough experimental data to justify the conclusion that the ECCS will work? You probably know better than we do whether you have that experimental data. In a way it is almost like admissions. We can go on from there. Then the Board will make the judgment as to whether or not the lack of experimental data in a particular area or the lack of complete experimental data or totally reliable experimental data is sufficient enough to cast doubt on the Applicant's case.

But we are giving a lot of kicking and screaming as we go down that road, which I guess is not unnatural. I'm not asking for our case on a silver platter. In short, I think, it will depend on the witnesses as to how quickly we cover it. It will be better if the witness is specifically familiar. Dr. Roll's presence on the subject of metal-water reactions helped greatly because, for most of the questions, he was the man that knew the answers.

MR. TROSTEN: I appreciate your explanation. It is helpful, also, for us to have as good an idea as we can get of your inquiry so we can have the right person here.

Also for our planning for the emergency planning and particularly for the security part of it, do you intend, after the cross-examination on ECCS matters, to cross-examine the AEC Staff and then go to the other matters?

MR. ROISMAN: Do you mean as opposed to going on to other matters with the Applicant?

MR. TROSTEN: Yes.

MR. ROISMAN: No. It will be our intent to take the Staff after the Applicant.

MR. TROSTEN: On all matters?

MR. ROISMAN: On ECCS matters, and take the Applicant on the security plan and the State of New York on the emergency plans after we have completed cross-examination of the Staff on ECCS. Some of this, Mr. Chairman, I might say, we have been attempting to, and we shall continue to do so, to discuss with the Applicant, the Staff, and give them as much guidance as we can. Mr. Ford is opening my eyes to a substantial number of areas. It is a problem of he and I sitting down in the evening and trying to scope out the area of whether the Applicant and Staff know the specific subject areas.

CHAIRMAN JENSCH: You will have an opportunity to do that off the record. Is there any other matter we can consider at this time? If not, at this time let us recess this hearing to reconvene tomorrow morning in this room at nine o'clock.

(Hearing adjourned.)

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