UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY OF NEW YORK,

(Indian Point Station, Unit No. 2)

Docket No. 50-247

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

Date - November 2, 1971

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

Tuesday, November 2, 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.

I N D E X

CHAIRMAN JENSCH: Please come to order. I believe that the suggestion from the attorneys yesterday was that after a study of yesterday's transcript, they will be prepared to proceed with further cross-examination on the emergency core cooling system; is that correct?

MR. ROISMAN: Yes, Mr. Chairman.

One preliminary matter, which is given what appears to be continuing weather conditions around here which are somewhat relevant. I wonder if we might discuss, just for a moment, when the hearings will reconvene next week, assuming that we don't finish up this week. I would like to suggest that if we don't, it might be helpful at least for us in terms of digesting material, if we schedule to reconvene Tuesday morning rather than on Monday morning, to add one additional, if you will, study day prior to the reconvening next week. It would be helpful for me to know in terms of making plans for when to be back here and on what day that would be. I spoke with Mr. Karman and Mr. Trosten. They did not have any objection to setting Tuesday as the day we would reconvene next week.

CHAIRMAN JENSCH: I think there is a great desire on the part of the Commission that these proceedings continue in successive days as far as possible. I think perhaps if you could give some indication of the necessity of this besides the convenience of it, it would be helpful to the Board in making its judgment.

MR. ROISMAN: My thought was this, Mr. Chairman. We are getting a lot of information to digest that is coming in on this subject really just this week. I think we could shorten the time if we had the full three days, Saturday, Sunday and Monday, to attempt to digest the material and focus our cross-examination for next week on the subjects.

THE CHAIRMAN: In other words, you took evidence, from witness Moore yesterday orally rather than waiting for responses by interrogatories which seemingly would expedite the situation, and a couple of days now would permit you to review the matter and be better prepared to cross-examine, and the net effect of it would be to expedite it and move the case more rapidly at that time than if we did not take a recess on Monday?

MR. ROISMAN: Yes, Mr. Chairman.

MR. TROSTEN: Mr. Chairman, that was exactly the basis upon which I agreed with Mr. Roisman, that I agreed with that this would actually expedite the proceeding if we did this.

THE CHAIRMAN: The Board would give some consideration to that. What is the suggestion of the Staff?

MR. KARMAN: The Staff concurs. We feel that rather than delay it it may very well expedite the matter.

CHAIRMAN JENSCH: Very well. This information that came from witness Moore yesterday of course was your first information of the kind.

MR. KARMAN: That is correct, Mr. Chairman.

CHAIRMAN JENSCH: And you likewise feel that you need Monday, too?

MR. KARMAN: It's not only that testimony. We are going to have lots of other testimony that's going to come up between now and the end of this session, which much of it will not have been previously distributed.

CHAIRMAN JENSCH: Well, the Board has some scheduling problems. The Board is inclined to accept the suggestions of the parties, but at this time no determination will be made. We expect to be able to indicate that situation tomorrow morning.

MR. TROSTEN: Mr. Chairman, may I just ask this question in connection with the Board's consideration of the schedule.

We are intending to go through Friday of this week is my understanding of the situation, is that not correct, Mr. Chairman?

CHAIRMAN JENSCH: Yes, that is correct.

MR. TROSTEN: And I might ask Mr. Roisman if he has given consideration as far as the matter of expediting the schedule is concerned, and the Board might wish to consider this too, to adjourning on Thursday and reconvening on Monday versus continuing through Friday and then reconvening on Tuesday. I mean this is another possibility that I think consideration perhaps ought to be given to. I agreed with Mr. Roisman that perhaps a day, leaving a day out would expedite the hearing, but we haven't discussed the matter of which would be preferable from everybody's point of view.

MR. KARMAN: The Staff would be amenable to either one, Mr. Chairman.

CHAIRMAN JENSCH: But in any event you feel that some time would advantageous to preparation?

MR. KARMAN: I believe it would, yes, sir.

CHAIRMAN JENSCH: Well, let us give consideration to that matter. This suggestion that Applicant's Counsel has made may be very helpful to the Board but we will see how the situation develops as to our own scheduling. We expect to indicate tomorrow morning.

Very well. Are we ready to proceed with further examination?

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: Who is the witness decided from cross-examination?

MR. TROSTEN: Mr. Chairman, we are prepared this morning to have witness Moore resume the stand in order to respond to certain questions that were raised yesterday to which he said he would provide additional information today. So I would like to ask witness Moore to resume the stand, please.

CHAIRMAN JENSCH: Please come forward, Mr. Moore.

MR. ROISMAN: Mr. Chairman, one last preliminary matter. As I had advised the parties, we were hoping that we would be able to have with us to assist us in cross-examination one of the authors of the report, A Critique of the New AEC Design Criteria for Reactor Safety Systems, which was prepared by the Union of Concerned Scientists. I think this is a fair statement to say that this probably represents from the standpoint of Intervenors by far the most sophisticated presentation and analysis on the part of emergency core cooling systems. We feel fortunate to have with us this morning Mr. Daniel Ford, who is one of the authors of that report, and I have discussed this with Mr. Trosten and with Mr. Karman as to whether or not they would have any objection to having Mr. Ford conduct the cross-examination on the emergency core cooling system as a technical expert on behalf of the Citizen's Committee in order to expedite the matter rather than to have the whispering back and forth. And they have asked me if I would have Mr. Ford state for the record what his qualifications are for doing this, and I wonder if I could do that at this time and then ask the Board to rule on whether he will be permitted to cross-examine.

I should point out that Mr. Ford's expertise in the area is acquired outside the normal educational disciplines, to wit, he does not have a degree in nuclear engineering or physics, but instead has worked very closely with Henry Kendall, Professor of Physics at MIT, James MacKenzie.

Also, a nuclear, in the prepration of this report, and his qualifications are more in terms of what he has acquired in the course of this, if you will, intensive study program than in the more formal educational pursuits.

I have talked to him and I am aware that his input into this report is extremely high and it is not merely that he was by any stretch of the imagination an "also" on the report, but a very important part of it.

I'd like Mr. Ford to state in some detail his qualifications, if that is all right with the Board.

CHAIRMAN JENSCH: In order to have it formally on the part of the record we will have him sworn.

(Daniel Ford, sworn.)

CHAIRMAN JENSCH: Will you give your name, address and your present business activity and your background, please.

DR. FORD: My name is Daniel Ford. My residence is 415 Broadway, Cambridge, Massachusetts. My office address is the Research Project on Pollution, 1583 Massachusetts Avenue, Cambridge, Massachusetts.

Professionally I'm the Coordinator of this Environmental Research Project which is funded to Harvard by the National Science Foundation.

I organized a group of nuclear engineers and nuclear physicists with whom we investigated the Idaho Semi Scale Tests starting in the middle of last spring and have been involved since that time in extensive research on all the engineering data pertaining to the Emergency Core Cooling System.

I'm the principal author of the two reports that have been issued by the Union of Concerned Scientists. I have lectured at Harvard University in the program of national sciences on the subject of Reactor Safety. This experience is basically what constitutes my expertise on Emergency Core Cooling matters.

CHAIRMAN JENSCH: Will you give us your educational background.

DR. FORD: I have a Bachelor's degree in economics from Harvard College, which I received in June of 1970. I have been involved full time in economic research, encironmental research since that time. My further educational experience, I have been nominated as a junior fellow of the Society of

Fellows to do Interdisciplinary Research in Environmental Problems which I hope to begin approximately a year from now.

CHAIRMAN JENSCH: What was your work with Professor Kendall? Would you identify him fom the record, please.

DR. FORD: Professor Kendall is co-author of the two Union of Concerned Scientists Reports. The first one was issued in July of this year entitled, "Nuclear Reactor Safety and Evlauation of New Evidence." It was reprinted by the American Nuclear Society in the September issue of Nuclear News, and the second report, for the record, is the critique of the new AEC design criteria which was issued two weeks ago.

Professor Kendall is a nuclear and high energy physicist. He is a professor and on the faculty of the Massachusetts Institute of Technology. His contribution in his scientific background, with that he is perhaps the most prominent high energy physicist in the country today. He is listed by the Atomic Energy Commission in their annual survey in their sponsored research as the principal investigator of electron scattering in their experiments at the Stanford Linear Accelerator Center. His experimental work on the structure of the neutrons is considered in Science Magazine as the single most important contribution of high energey physics in the last thirty years.

CHAIRMAN JENSCH: These Idaho Semi Scale Tests I believe were conducted sometime in November through February.

DR. FORD: November through March.

CHAIRMAN JENSCH: November 1970 to March of 1971. They were, I think, released and made available to the public generally about April or May, I believe.

DR. FORD: That's correct. I believe the first public announcement was a front page in the Washington Post. on May 26th.

CHAIRMAN JENSCH: Since that time, how many hours have you worked with Professor Kendall on this type of subject?

DR. FORD: If you will believe me, I will say night and day since we learned of the tests early in May. We have been working seven days a week on the subject since that time.

CHAIRMAN JENSCH: Is there any comment from the parties?

MR. TROSTEN: Yes, Mr. Chairman.

Under Section 2.733 of the Atomic Energy Commission Rules of Practice, at the request of a party, Mr. Roisman, the Board may permit, as the Board knows, a qualified individual of scientific or technical training or experience to participate on behalf of that party in the examination and cross-examination of expert witnesses.

Dr. Ford could, of course, sit at the table with Mr. Roisman and suggest to Mr. Roisman questions which Mr. Roisman could then place. There would obviously be no harm in that.

On the basis of what Mr. Ford has said, the qualifications that he has stated, it is Applicant's position that Mr. Ford is not a qualified individual who has scientific or technical training. He is not an expert in the fields involved.

Consequently, it is our position that he certainly is not a competent witness to sponsor the document to which Mr. Roisman referred, the critique by the Union of Concerned Scientists, or the other documents to which Mr. Roisman referred. We would most certainly enter an objection to his qualifications as a witness to sponsor any such testimony.

On the other hand, if Mr. Roisman desires to have Mr. Ford either help him with the cross-examination by suggesting questions or in some instances actually to ask the question in lieu of Mr. Roisman asking the question, we would not object to that.

MR. ROISMAN: Mr. Chairman, let me say as I had stated at the outset, that we were not intending to introduce the critique of the AEC interim criteria into evidence under the sponsorship of Mr. Ford; that it was only for the cross-examination purpose.

CHAIRMAN JENSCH: It is so understood.

The Staff.

MR. KARMAN: Mr. Chairman, it would seem that technically under Section 2.733, that there is a serious question as to whether or not Mr. Ford were to qualify as a qualified individual with scientific or technical training. The Staff certainly has no desire to lengthen these hearings by having Mr. Ford pass a question to Mr. Roisman for his examination. I think, for the record, we place ourselves in our position not objecting to this procedure, only in the hopes of speeding along the hearing. We leave to the Board the question as to whether or not the Intervenor has complied with Section 2.733.

MR. ROISMAN: Mr. Chairman, may I just speak to the comments that these two gentlemen have made? First let me point out the 2.733 was obviously written to include people other than people with formal technical training, since it refers to the experience along with the word "training" as listing a qualified individual. The purpose of this Section, it seems to me, is clear. It was in order to attempt to expedite the proceeding. People with special knowledge may very well -- Although this may seem to be an admission against interest. -- may not be lawyers, but other people who have special knowledge that could expedite the proceeding. The only problem I could imagine, if it turned out that Mr. Ford's cross-examination appeared to be pointless to the Board or get us far afield. I would suggest that perhaps the best test of that would be, we are going to have like an hour or an hour and a half of cross-examination until we make that decision. Then Mr. Ford would be perfectly amenable to doing the cross-examination during that period and see if there does seem to be a problem. If not, and we seem to be advancing as I am convinced we will, it seems to me that the purposes that would be served by having a technically trained or experienced person to cross-examine would be better met than this whispering from Mr. Ford to me and I asking the question of Mr. Moore.

MR. TROSTEN: May I make one more comment?

CHAIRMAN JENSCH: Yes. Proceed.

MR. TROSTEN: It has been the Applicant's position throughout this proceeding, and will continue to be that position, that we have no desire to lengthen the proceeding or to complicate it by raising technical or legalistic objections to procedure. We feel this way about the matter: we would like to see the cross-examination proceed. We feel that in no sense, in our view -- And I am not saying this in a pejorative sense with respect to Mr. Ford in any way or with respect to Mr. Roisman. In no sese in our view will Mr. Ford qualify for a technical position involving the complex engineering and scientific and chemical engineering disciplines which are involved here, either by his training or by his experience. On the other hand, in the interest of moving forward with this proceeding, if the Board chooses to follow the practice of allowing Mr. Ford to assist Mr. Roisman to pose these questions, we think we could go forward with it.

CHAIRMAN JENSCH: I think one of the problems that the Commission contemplated in a rule of this kind is to be sure that whatever is undertaken will expedite the development of relevant information. The interrogation of witnesses is not necessarily limited to lawyers. I think, however, that lawyers, from experience, learn to frame questions which are not argumentative and which seek facts rather than argument from the witness. There may be certain techniques that lawyers develop from their experience which better expedite a hearing.

The Board is not convinced that Mr. Ford is qualified to be a witness on matters of nuclear engineering, for instance, but will permit him to undertake cross-examination for a while to see if some of the problems don't become worse. If they do not and the parties are not objecting, we will see if the problem arises specifically. Therefore, Mr. Ford, you are permitted to interrogate witness Moore. Will you proceed.

MR. TROSTEN: In accordance with my understanding of Mr. Roisman this morning, it is my intention to have the witness Moore at this time respond to certain questions that were raised yesterday.

CHAIRMAN JENSCH: Yes. Will you proceed in that respect.

MR. TROSTEN: Would you please, Mr. Moore, identify the points in issue by transcript reference and general subject and then proceed to give your response.

MR. MOORE: Yes, sir.

The first question I will address is the one indicated in the transcript on page 1703 and 1704. 1703, lines 23 through 25 and 1704, lines 1 through 14.

This refers to a question from Mr. Roisman regarding the number of rod burst tests that had been performed. The Westinghouse series of rod burst tests consisted of a series of single rod burst tests comprising a total of about 125 rods which were burst under various conditions. These were unirradiated rods. There were sixteen irradiated single rod burst tests. And the multi-rod burst tests performed by Westinghouse included a total of 168 rods which were burst. So this is the total number of burst rods, over 380, 390.

MR. TROSTEN: Would you proceed, Mr. Moore, unless either Mr. Roisman or the Board questions you with regard to these particular answers.

DR. FORD: Excuse me. Is it preferred that I would interrupt now to ask a question on this specific answer?

CHAIRMAN JENSCH: It might be advisable if you would, yes.

DR. FORD: I am not clear when you give the quantity of rods as 168. Do you mean that there was a test mock-up that had 168 rods in it?

MR. MOORE: No. These were multi-rod burst tests which included, each test run included fourteen rods which were pressurized and there were a total of twelve tests.

DR. FORD: I see. Thank you.

MR. MOORE: The second point is on page 1689, lines five through eleven, where in response to a question regarding the temperature of the fuel rods near the hot rod I replied that I would expect they were in the range of 1800 to 2000 degrees Fahrenheit with a peak rod temperature of 2300 degrees Fahrenheit.

In subsequent checking I found that the rods immediately adjacent to the hot rod will in fact be within twenty to thirty degrees of the hot rod. The numbers that I indicated, the 1800 to 2000, represents the temperature of the average rod in the assembly which contains the hottest rod. I wanted to clarify that point.

CHAIRMAN JENSCH: Thank you.

MR. MOORE: The final question is found on page 1706 of the transcript beginning at line 10 where Mr. Roisman asked for a census of the zirc-water reaction corewide in terms of the percentage of rods affected and the amount of metal-water reaction associated with those rods. And I have a corewide temperature and zirc-water reaction census for Indian Point-2 for the double-ended cold leg rupture, which gave a peak clad temperature of 2300 degrees Fahrenheit. The census was obtained by splitting the reactor core into eight different power regions and then calculating the temperatures of each of these power regions with seven axial increments along the length of the rod. In doing this calculation then we determined at the end of the transient what the heat clad temperature is in each one of these regions and also the corresponding metal-water reaction that was obtained.

Perhaps the best way to summarize this is to indicate the volume of the core which exceeded a given temperature. With this calculation .17 percent of the core reached a temperature greater than 2000 degrees Fahrenheit. 1.6 percent of the total core, by volume, of total cladding by volume, reached a temperature greater than 1900 degrees. 5.2 percent was greater than 1600 degrees and twelve percent was greater than 1400 degrees.

Now the assumption is made in our analysis that there is no zirc-water reaction at temperatures below 1800 degrees Fahrenheit and on that basis there are really only two of these regions which have any zirc-water reaction whatsoever. The first is the region which includes the hot spot, and that has a total zirc-water reaction of 7.5 percent for that small region. And there is a second region of the same size which sees a total reaction of 2.1 percent metal-water reaction for that region. And adding those two together we get a corewide metal-water reaction of less than .05 percent. As indicated in the testimony yesterday the number for Indian Point was less than .07 and in fact is closer to .05 percent.

We have evaluated the effect of assuming metal-water reaction to occur at all temperatures rather than the limitation of 1800 degrees and above, and taking this particular case we obtained seven and a half percent at the peak, at the hot spot, assuming that there is a zirc-water reaction, can be a zirc-water reaction at all temperatures in accordance with the parabolic rate. There is only a difference corewide of .005 percent. That's a trivial difference. So the assumption of 1800 degrees and above is warranted.

In summary, looking at the census for the core we are talking about very limited regions of the core which actuall participate in a metal-water reaction and a very, very small fraction of the total zirc in the core is actuall reactive.

MR. TROSTEN: Not now. Do you have a question, Dr. Ford?

DR. FORD: I have several questions. My first question relates to the interrelationships between eight regions and your calculations. Do you solve the system simultaneously for all eight regions or are these eight distinct calculations with your computer code that you are reporting to us?

MR. MOORE: These are distinct calculations for each region.

DR. FORD: I see. So that a phenomenon such as a radial flow between the different power regions of the core is not considered in your calculations at all?

MR. MOORE: The calculations are performed using the coolant sink temperatures which are obtained by taking an energy balance on the water as it enters the core. The temperature rod, the largest part of the temperature rod occurs during adiabatic heat-up. Where there is no, essentially no flow in the core. And during the reflood portion of the transient where the temperature is turned around there is very little radial redistribution flow.

DR. FORD: I see. Then would you--

CHAIRMAN JENSCH: Excuse me just a minute. I know you intended to answer directly as you felt you could, but are you able to answer the question? I think it was radial flow then is not considered in your calculation, is that correct?

MR. MOORE: That is correct. On the basis of a consideration which says that it is not important.

DR. FORD: Can you tell me whether there is any experimental confirmation of your contention that radial flow is not an important phenomenon?

MR. MOORE: Yes. For one case our FLECHT results in a rod bundle showed that there was very little radial flow.

DR. FORD: I see. But isn't the question that we are talking about now radial flow over substantial regions of the core between these eight power regions, not just radial flow within a bundle?

MR. MOORE: Either case.

DR. FORD: Well, I mean doesn't the bundle wall prohibit radial flow beyond the bundle?

MR. MOORE: There are no bundle walls in our reactors.

DR. FORD: No. I am talking about in the FLECHT test.

MR. MOORE: I am speaking of the radial flow that existed within the assembly in the FLECHT test.

DR. FORD: Right. And I asked you concerning the experimental confirmation, and you cited the FLECHT test, is that correct? So I am talking about simply since the FLECHT test only considered single bundles how can they relate or provide information pertaining to a phenomenon involving flow between large numbers of the bundles?

Your contention, am I correct, was that there is no radial flow between the eight core nodes that you set up, is that correct?

MR. MOORE: No. I indicated that the radial flow between assemblies is small.

DR. FORD: Right. Now can you explain how a test which involves no separate regions, it's only a single bundle, how this can be relevant to the significance of radial flow between regions?

MR. MOORE: Well, because there is really no differentiation between assemblies and within assembly in terms of the rod-to-rod behavior. As you know, our assemblies are open lattice assemblies, so it's very difficult to discriminate what would happen between assemblies and within assembly.

DR. FORD: Now in the FLECHT tests there are single bundle tests of ten-by-ten and seven-by-seven, seven rods, is that correct?

MR. MOORE: That is correct.

DR. FORD: In the test apparatus those bundles are enclosed within a box, is that correct?

MR. MOORE: That's correct.

DR. FORD: Now this box as you note doesn't exist in the reactor itself. That's what you mean by open lattice, is that correct?

MR. MOORE: That's correct.

DR. FORD: So the kind of flow that I am talking about, because of this open lattice, isn't this correct that this can't be considered an experimental situation which closes the whole bundle off completely?

MR. MOORE: Yes. But I am speaking of the flow variations within the assembly that don't have anything to do with the fact that there is a wall around the assembly.

DR. FORD: Right. But I am talking about, am I making myself clear--

CHAIRMAN JENSCH: I wonder if it would be helpful if we used that chart again, that easel or paper, and take a new sheet and Mr. Moore you probably could describe the difference between the two situations. Will you tell us what you are considering within the box and then depict in a lower portion of that easel sheet the eight regions that you had in mind and as to which I understand the interrogator is saying are those situations comparable and are they equal.

MR. MOORE: As far as speaking about the FLECHT test which would have bundles of rods which are within a housing, and then he is asking what about the situation where I have many assemblies and he is asking the question with respect to flow redistribution throughout the core and I indicated we have some data within the FLECHT test where I am looking at flow redistribution between the carious channels for flow within the bundle of rods where there are differences and power generation within the rods. I submit that this is somewhat analogous.

I will agree that there is a scaling effect here to the situation corewide, for now I am looking at the flow redistribution between flow channels between rods and also considering all the assemblies in the core.

CHAIRMAN JENSCH: Can you depict your eight regions that you have in mind in that lower circle to reflect the core? I understand you made seven analyses of eight separate regions. Then you can indicate how generally they would be located within the core, or do you want to take another sheet?

MR. MOORE: Well, there are not specific geometrical regions in that the calculation is performed without interaction between individual regions. So if I have a core and I look at the power distribution in the core and I have a few assemblies which are equivalent to the hot spot. Let's just say that these assemblies--so I have one region in the analysis which calculates what happens in these typical assemblies.

Then I look at an assembly which is ninety percent, has a power of ninety percent.

CHAIRMAN JENSCH: Would you show that on a diagram, too.

MR. MOORE: These could be located, then, in various parts of the core, and in fact, represent a total volume of the core of ten percent. So I've got a region which represents ten percent of the core which will be redistributed in this manner.

Then I take a region which is eighty percent of the peak assembly. So I have, for the hot spot, 1.2 percent; for the ninety percent of the hot assembly power, this is ten percent of the core. This is hot assembly. This is .9 times the hot assembly.

Then I take a region which represents eighty percent of the hot spot, the hot assembly, and that represents twenty-three percent of the core.

CHAIRMAN JENSCH: Will you show that in your core as you depicted that.

MR. MOORE: These regions would tend to be scattered towards the middle regions. The lower power regions, as I continue on down here, tend to be in the outer regions of the core. The hotter assemblies tend to be toward the center of the core.

CHAIRMAN JENSCH: To go back to that FLECHT test diagram, all within a box, do you have the same temperature distribution in that upper sketch as you would for the core, the heat distribution?

DR. FORD: Could you identify which FLECHT report you are referring to?

MR. MOORE: WCAP-7665, page 2-3.

DR. FORD: Is this a proprietary report?

MR. MOORE: No.

CHAIRMAN JENSCH: Proceed.

DR. FORD: The variation in power from rod to rod for this particular set of ten-by-ten test section was 1.1 down to .95.

CHAIRMAN JENSCH: Can you generally indicate where those were located?

MR. MOORE: Well, the .95 tend to be primarily in the outer edges with a few in this region. The 1.1 and 1.0s were scattered within the assembly.

CHAIRMAN JENSCH: You mean scattered near the box sides itself?

MR. MOORE: No. There were some -- There is one up here at 1.1.

CHAIRMAN JENSCH: Mark that somewhere, please.

MR. MOORE: 1.1 up here. Then there were 1.1s within. The point I was making, the fact there is a gradient of 1.5 here, a 1.0 and a 1.1 So I had some variation of power within the assembly.

I don't want to overemphasize this particular aspect. I think we should go back to the basic physical situation in response to Mr. Ford's question. We are talking about the radial flow distribution that you get during the reflood part of the transient which is of interest here in determining the peak temperature.

Under this condition, the main pressure drop in the core is the elevation head. There is very little frictional pressure losses within the core. There is really no large frictional flow redistribution considerations during the reflood phase of the transient. The total pressure drop across the core is about 2 p.s.i. So it just isn't a situation which creates large radial flow redistributions.

CHAIRMAN JENSCH: Just one further question. I won't interrupt any more.

It is your thought that the FLECHT test within that box arrangement is similar enough to the core distribution of heat you depicted on the other sheet so that it obviates any consideration, significant consideration of radial flow; is that correct?

MR. MOORE: That's correct. I think it is corroborative of the basic physical argument. I would also like to indicate that we are only talking with respect to calculating peak temperatures, about a very few assemblies. As I said, the hot assemblies consist of only 1.2 percent of the total core volume.

CHAIRMAN JENSCH: What's the number of rods? Can you tell us what the 1.2 percent would be?

MR. MOORE: That would be about 300 and some rods.

CHAIRMAN JENSCH: Out of the total of what?

MR. MOORE: A little less than 40,000.

CHAIRMAN JENSCH: Thank you. Excuse me for interrupting your proceeding, Mr. Ford.

DR. FORD: I have a large number of questions concerning this deomonstration.

Can you tell me first of all concerning this FLECHT test, what instrumentation did you use to measure the flow in different parts of the bundle?

MR. MOORE: We had a --

DR. FORD: Could you refer to the page number of the report, please.

MR. MOORE: Page 3-111 of the FLECHT report, WCAP-7665.

It indicates we had a pressure tap within the bundle and a pressure tap transducer in the housing wall. We were measuring the differential pressure between these two. As plotted in that figure on page 3-111, very little pressure difference was indicated across the assembly, therefore indicating essentially zero, a very small radial flow.

DR. FORD: These were the only instrumentation that you had this bundle to determine radial flow; is that correct?

MR. MOORE: That's correct.

DR. FORD: You didn't have any instrumentation that determined local boiling conditions, did you?

MR. MOORE: Not directly, no.

DR. FORD: Did you have any instrumentation to determine local coolant velocity?

MR. MOORE: No, we did not.

DR. FORD: Do you disagree with the statement made in the Oak Ridge National Laboratory report, Protection Instrumentation Systems in Light-Water-Cooled Power Reactor Plants, by H. G. O'Brien and C. F. Walker, ORNL- NSIC-29, published in October 1969? It states, and I quote from page 137. I am going to quote the entire section to make sure the context is clear.

MR. TROSTEN: Do you wish to have a copy of this in front of you as Mr. Ford is reading?

MR. MOORE: He may read it and then I will look at it.

DR. FORD: It says, "In many instances, plant variables that must be prevented from reaching safety limits cannot be measured directly, and the values of the safety limit variables must be inferred from measurements of other variables. Although neutron and gamma fluxes can be measured as a function of position in the core (usually in the core coolant channels), techniques for measuring local heat flux, cladding temperature, fuel center-line temperature, local coolant boiling and local flow velocity are not presently available for in-core use. The values of safety limit variables such as these must therefore be inferred from other measurements, together with known or assumed parameters (cross-sections, heat transfer coefficients, et cetera). We feel that research and development is needed on methods for directly measuring the safety variables and thus reducing the need for using the somewhat tenuous chains of inference."

CHAIRMAN JENSCH: Let him read it. When he has indicated he has completed reading, you may propound the next question, sir.

MR. MOORE: Yes.

DR. FORD: The question is whether you agree or disagree with that statement.

MR. MOORE: Well, that includes -- I would say I agree in a qualified manner, which I would like to qualify. That includes a considerable number of measurements, some much more difficult than others. The main point that the author here is making is, I believe, the fact that he is talking about in-core use. This is a key consideration in measuring these kinds of variables to have instrumentation which will stand up under the radiation environment in-core, and also be of such a type that they do not influence the reactor performance. So in that sense I would agree.

DR. FORD: Can you indicate what instrumentations are available to perform those functions outside of the core? For example, FLECHT test bundle. Could you measure it? It is non-nuclear, so some of the measurements are irrelevant. But specifically, for local boiling conditions, cladding temperature and local coolant velocity, do you have instrumentation that you could have used, that could have been used on the FLECHT bundle?

MR. MOORE: There is instrumentation to make local flow measurements. This is really not relevant to the FLECHT test or to the consideration of whether or not we have radial flow in the sense that if you can ascertain that there do not exist large pressure gradients within the bundle, then you have already demonstrated that there is not large flow redistribution. So there was no need in that case to obtain a flow measurement.

DR. FORD: But isn't the experimental situation -- Excuse me. Aren't you referring to this as an experimental confirmation of the fact that there is no radial flow?

You are not arguing on the basis of postulated pressure differences?

MR. MOORE: I'm sorry. I don't understand that.

DR. FORD: Let me rephrase it. In other words, the whole point of our discussion of this FLECHT data is whether or not it provides an experimental confirmation of the existence of radial flow.

Independent of a priori considerations about pressure differential, wouldn't the point of a relevant experiment be to measure whether or not there is radial flow?

MR. MOORE: No, not necessarily.

DR. FORD: That isn't your idea of experimental confirmation?

MR. MOORE: It is not a necessity to measure flow directly.

CHAIRMAN JENSCH: Excuse me. That first part of the question could be answered. I think it was something to this effect: Did the FLECHT test seek to confirm radial flow manners? Can you answer that yes or no?

MR. MOORE: No. It was not primarily designed to do that.

CHAIRMAN JENSCH: What was its primary purpose?

MR. MOORE: To determine the heat transfer that you obtained using bottom flooding into a core.

CHAIRMAN JENSCH: Would secondary considerations include radial flow?

MR. MOORE: Yes. That's the reason we installed this particular instrumentation.

CHAIRMAN JENSCH: Very well. Will you proceed.

DR. FORD: I see.

Is your theoretical analysis of the radial flow question presented in the FLECHT reports or is this an interpretation that you are giving simply on an ad hoc basis in answer to my question?

MR. MOORE: The discussion, the argument concerning why we don't expect radial flow is not excluded in the FLECHT report.

DR. FORD: Can you explain what the chain of inference is between your pressure tab measurement and radial flow phenomena? What information do you have that would indicate that the pressure tab would give you certain data if there were radial flow and give you other data if there were not radial flow?

MR. TROSTEN: May I interrupt the questioning just at this point? I don't want to interrupt Mr. Ford's train of thought or the questioning, but I want to make this one point. We have brought a witness here yesterday in response to one of Mr. Roisman's questions concerning certain zirc-water reaction. He was brought here especially for this purpose, who is urgently needed back in the Westinghouse offices as soon as his testimony is completed today. I would like to inquire of Mr. Ford, if he could, at some convenient time, terminate his questioning of Mr. Moore so that we could get the other witness' testimony on the record and then resume his questioning of Mr. Moore.

CHAIRMAN JENSCH: Would this be a convenient place to interrupt your examination or do you desire to propound a few more questions?

DR. FORD: There are a large number of questions on the matter. I think I could conveniently defer them to later. I planned to go through the transcripts. If I could have five or ten minutes to put myself in a zircalloy-water frame of mind, I would appreciate it.

CHAIRMAN JENSCH: You are going to interrogate on zircalloy-water, too; is that correct?

DR. FORD: The metal-water reaction. That's the witness, whose convenience we are discussing. Could I have a few minutes?

CHAIRMAN JENSCH: At this time let's recess and reconvene in this room at 10:05.

(A short recess is taken.)

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CHAIRMAN JENSCH: Do you desire to have another witness available for your examination after this, Counsel?

MR. TROSTEN: Yes, I do, Mr. Chairman.

CHAIRMAN JENSCH: Will you call the gentleman, please.

MR. TROSTEN: I would like Dr. Jack Roll to join Mr. Moore on the witness stand, please.

CHAIRMAN JENSCH: And he has not been sworn as I recall.

MR. TROSTEN: That is correct.

CHAIRMAN JENSCH: Dr. Roll, would you stand and raise your arm, please.

(John Bernard Roll, sworn.)

CHAIRMAN JENSCH: Have a seat, please.

MR. TROSTEN: Dr. Roll, would you please give your full name.

DR. ROLL: My name is John Bernard Roll.

MR. TROSTEN: Would you give your business address, please.

DR. ROLL: Westinghouse Electric Corporation, Box 355, Pittsburgh, Pennsylvania.

MR. TROSTEN: Please describe your educational background.

DR. ROLL: Bachelor of Chemical Engineering, University of Detroit, 1958. Ph.D., Chemical Engineering, f1Bt2

Purdue University, 1962.

MR. TROSTEN: Would you please give a statement of your professional background and experience?

DR. ROLL: From 1962 to 1964 I was on active duty with the U. S. Army but I signed to the United States Atomic Energy Commission. I was stationed at Germantown, Maryland, in the Division of Reactor Development. From 1964 to the present time I have been with Westinghouse Electric Corporation. My present capacity is manager of Performance Analysis in the Engineering Department, Nuclear Fuel Division.

The doings of this group are primarily to review data from test evaluation programs, interpret this data and model this data and apply it to design and performance analyses of the nuclear fuel rod. The group consists of eight engineers and four technicians.

MR. TROSTEN: Thank you, Dr. Roll.

Now with respect to the question which appears on the transcript Page 1720 relating to the zirconium water reaction, are you familiar with the question that was raised by Mr. Roisman yesterday concerning that matter?

DR. ROLL: Yes, sir. I reviewed the transcript.

MR. TROSTEN: Would you please comment with regard to the question raised by Mr. Roisman.

DR. ROLL: I believe the context of the question was that based upon the results reported in the reference

ORNL document did we have any reason to re-evaluate our application, I believe, of the Baker-Just equation to a computation of degree of zirc-water reaction, and I believe that Mr. Moore provided essentially the answer that I would have provided, that that is no we could not use that single data point to re-evaluate or reapply the Baker-Just equations.

As pointed out by Mr. Moore in yesterday's proceedings, the measurement of the extent of zirc-water reaction was in fact by an inferred route, and there were no direct measurements taken. There was a large uncertainty in the measurement of total hydrogen evolution during the experiment.

The subtraction of other known effects resulted in a fifty per cent uncertainty in the amount of hydrogen which can be associated or applied with the zirc-water reaction, and from this they inferred the two-tenths per cent raw metal-water reaction. This was then compared, presumably by Mr. Roisman, to indicate that perhaps there was more zirc-water reaction here than one would expect based on reported temperatures.

But however, I pointed out in the Oak Ridge report there was not a direct measurement of temperature and they point out that the effects of thermocouple effects themselves and the power distribution with the bundle it enters result in an uncertainty in the temperatures of the fuel during the experiment.

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Therefore, one cannot make a direct inference on reported temperatures and lead yourself to the conclusion that the extent of zirc-water reaction was higher or much higher than would have been predicted by Baker-Just.

I'd like to add further that we have, as a part of our work, in particular under the FLECHT program, reviewed the extent of zirc-water reaction, under what we considered to be much more representative conditions, that is zircalloy clad fuel rods with our particular time and temperature histories and our particular coolant content, that is our particular water conditions, and I believe as reported in the documentation summarized in the FLECHT reports we find very good agreement with the Baker-Just equation, and so we believe in summary that the Oak Ridge report presents a single data point to germaneness to our specific application must be questioned inasmuch as the data point was not, the test was not run to substantiate the Baker-Just equation.

And secondly, in summary, the work that we have done under the FLECHT program and reported in the FLECHT reports we believe reaffirms our use of the Baker-Just equations in evaluating zirc-water reaction under our conditions of loss of coolant accident.

MR. TROSTEN: I have no further direct questions of Dr. Roll at this time, Mr. Chairman.

CHAIRMAN JENSCH: Very well. Intervenors, if you desire to proceed, do so, please.

DR. FORD: Yes. The authors of that Oak Ridge report ORNL 4635 contend that it is the most realistic simulation of loss of coolant accident conditions to date. Do you dispute that claim?

CHAIRMAN JENSCH: I wonder if you'd tender to the witness you wish to question.

DR. FORD: Yes, Mr. Roll, please.

CHAIRMAN JENSCH: Yes. I mean you say the authors of the ORNL report make a certain contention. Will you point to the report wherein the contention is made.

DR. FORD: Oh.

CHAIRMAN JENSCH: And tender it to the witness.

DR. FORD: Let's get the reference and then we will come back to the question in a few minutes.

Can you describe the techniques of FLECHT measurement of zircalloy water reaction that were used in your FLECHT tests compared to the techniques used in the ORNL 4365 tests?

DR. ROLL: The measurements, the techniques which were used in the ORNL reported tests were one of measuring hydrogen evolution which is a direct result of the chemical reaction between zircalloy or zirconium and water and then subtracting from this measured hydrogen evolution other effects, that is the total volume of gas in the system during the tests, the changing volume of gas due to the over-pressurization of the transient itself and then by subtraction of two relatively large numbers arriving at a smaller number which they attribute then to hydrogen evolution during the zirc-water reaction, and from this they infer then the total extent of the zirc-water reaction. In work which we did--

DR. FORD: Excuse me. Just in point of clarification.

In the hydrogen evolution that they subtract from the total zirc-water used hydrogen as a residual, did they consider hydrogen by radiolysis of water?

DR. ROLL: I looked for that specific point in the report. I took a brief scan for review. I did not see that point noted. They may have. If they did not, then the calculated result of zirc-water reaction is on the conservative side.

Let me continue then. The measurement that we took in evaluating the result of our FLECHT test with regard to extent of zirc-water reaction were in fact metalographic cross-sections at various enlargements from which the experienced metalographers can infer nature of the phases in the cross-section. That is they can determine the portion of the original zircalloy which remains as original zircalloy. That portion which is oxygen saturated, that portion which is in fact converted to zirconium oxide. With these direct measurements at a number of cross-sections, one can then calculate explicity the quantity of zirconium which has been converted to zirconium dioxide and the quantity of zirconium which is oxygen saturated from which you can then determine the total quantity of zirconium which has in fact reacted in some way with the oxygen.

This was not done on the Oak Ridge test.

DR. FORD: Can you explain what experimental confirmations there are of the correlation between the metalographic cross-analysis and cross-measurement of hydrogen evolution? Is there a topical -- This is the technique you used in the FLECHT test. Is there a further primary source that confirms this as a reliable technique?

DR. ROLL: I believe the technique of looking at zirconium and zirconium oxide is in itself a primary source of data and need not be substantiated somewhere else. The question is, how do we know what is the extent of zirconium and oxygen reaction. The answer is, you know this by looking at the quantity of zirconium which has been converted to zirconium oxide.

DR. FORD: But I mean the phase analysis in terms of a priori plausibility, as to what you begin with. In terms of the specific quantitative relationship between the phase analysis and the extent of the reaction on prediction of hydrogen. I will concede that the overall plan of going to the metalographic second step in terms of the analysis is plausible. What I am wondering is whether this plausibility has been convincingly confirmed by experimental analysis. That was my question. I am looking for a topical report on the subject.

DR. ROLL: I fail to see how I can answer this question to your satisfaction. The topical reports -- For example, I refer you to our own FLECHT report and reproductions of the cross-sections.

DR. FORD: In my context I am looking for a topical report specifically on the technique of your metalographic analysis. I know you use it in the FLECHT reports, describe it in the FLECHT reports. But in terms of what primary data confirms that as a reliable thing to use, I haven't seen anything. I am just wondering whether you can provide a topical report as a reference.

DR. ROLL: I could --

DR. FORD: Can you come to it readily?

DR. ROLL: I cannot. It is a basic technique which is used to identify phases and structures in materials. It is used in several places. I think the literature on extensive zirc-water reaction, I can't pull out a topical number right now. It is replete with these kind of references where it is explicitly as to the nature and extent of the zirc-water reaction.

DR. FORD: You can't say that you are familiar with any state of the art survey with regard to the use of this technique that would support its use?

DR. ROLL: No, I can't.

DR. FORD: Thank you.

You mention there was a fifty percent uncertainty in the ORNL-4635 estimate of the hydrogen evolved, and therefore of the extent of metal-water reaction. Have you performed a statistical analysis of your own estimate of metal-water reactions, and can you present the various statistical indices of the confidence that we may have in your estimates? What would be, for example, the probability that the 7.5 percent metal-water reaction in the core hot spot, what would be the probability against a no hypothesis that it was zero?

DR. ROLL: The probability of our calculated 7.5 percent being, in fact, zero -- I believe is your question. -- is exceedingly low.

DR. FORD: In terms of whether it is 7.5 or 8.5 or 6.5, what are the confidence limits there? What is the probability that that estimate is 100 percent mistaken, the probability that it is 1/100th of a percent mistaken?

DR. ROLL: The method of calculation which uses, in our analyses -- Not of the FLECHT data but of the application. To the loss of coolant accident, uses parabolic rate equations which we believe are on the conservative side. That is, we may be reaction rate limited by availability of steam or water at the surface. The parabolic rate was derived with essentially an infinite and complete source of water at the surface of the reaction. Therefore, our estimate as presented in our analysis of the loss of coolant accident, would be on the high side. Is that responsive to your line of questioning?

DR. FORD: Let me try to preface my concern with statistical questions with a brief diagram.

Froma statistical point of view, I am talking in terms of the general scheme here, of having a set of experimental data to which we wish to fit some function. Let's suppose, through either just simple straightforward statistical analysis of the data, we fitted a line through the data, or suppose alternatively that this is the line predicted by the analytical models that we use. We can say, with reference to the data point, with reference to the relationship between this predicted line and the body of data, we can compute various statistical indices which say how well this line represents this data. Specifically we can measure and integrate over the whole area what the distances are between the observations and the point.

What I am asking you for is, in terms of the predicted rates or metal-water reaction, how in this situation did you relate statistically the experimental data to the curve that you are using?

DR. ROLL: We did not, for example, in your context, calculate a mean standard of what is predicted and measured. So I cannot give you a typical value. You can relate to the predicted versus measured figure in the WCAP. You may look at that yourself.

DR. FORD: What I am looking for --

DR. ROLL: Did I cover the deviation, no.

DR. FORD: Did you calculate the percent of variance within the experimental data that is accounted for by your relationship?

DR. ROLL: No.

DR. FORD: Can you explain, in terms of this diagram, how you make a conservative estimating procedure? Let me refer to it.

I am assuming here and fitting this main line to this data; that I am doing it by usual statistical techniques; that this particular line, of all possible lines that could be put through this set of data, minimizes the potential error. If we wanted to make a particularly conservative analysis and if we knew that the cladding temperature or the build-up of hydrogen in the containment was worse if this curve had a particularly different position, if it was worse as this curve moves down, then we will say what we will do to be conservative is, we will put this curve below all experimental data so that there is a range in which metal-water reactions might be small or low, and since there being a small or high, and since they being high is not clearly a good thing, we will assume that they are high, and that that is the analysis that relates our experimental data to the analytic model that we use.

Is this the way in which you conservatively derive your metal-water reaction rate from the experimental data?

DR. ROLL: Let me refer to WCAP-7665 figures on page B 20 in particular and on B 23. I think these are page numbers.

DR. FORD: Yes, I see them.

DR. ROLL: I believe the answer to your question, does the prediction, i.e., Baker-Just, go over the top of the data. I think the answer is essentially yes, looking particularly at the figure on page B 20.

DR. FORD: Excuse me. Are you giving the result of a statistical analysis?

DR. ROLL: These are data points.

DR. FORD: Are there any statistical indices?

DR. ROLL: I have already said we do not calculate a standard deviation for these data points.

DR. FORD: Well now let me go back here. As I explained, the conservative estimating procedure that I have sterotyped here, what you would do would be to place the line above all existing data points and therefore whatever the data indicated you were clearly on the conservative side.

Now as I look at the diagram here on page B 20 there is data on both sides of the line. The preponderance of a design one side, but nevertheless there is no clear indication from the diagram that you followed the conservative estimating procedure that I have described schematically here, is that correct?

DR. ROLL: To support your point, I think your figure on page B 20 is germane, that is measured oxide versus predicted oxide.

DR. FORD: That clearly supports my point.

DR. ROLL: Right. Now this is not necessarily a measure on B20.

DR. FORD: Let me clarify the elliptical remark for the record. It clearly supports my point that you didn't use the conservative estimating procedure that I described here, is that correct?

DR. ROLL: I believe we can state for the record that the data points which are reported by ourselves in support of the FLECHT tests where they are explicit measurements of predicted measurements of oxide figures as compared to predicted values, all the data points are overpredicted. That is we are conservative.

The figure on page B 20, 1, 2, 3 are on the line of prediction, and that all the other data points are below it.

Or perhaps you don't understand. The figure, this is measured versus predicted and any points which fall below the 45 degree line are in fact below the prediction.

DR. FORD: Yes. Now of the variety of lines that you could draw above the existing data can you justify in any statistical way the particular line that you did pick in this instance to implement the general kind of conservative estimating procedure that I am talking about?

DR. ROLL: The line that was picked is the Baker-Just equation.

DR. FORD: Yes. But I'm talking statistically did you try to find in the data a correlation, and if that had, you know, more statistical confidence in that than the Baker-Just equation, use that instead.

Let me ask you, to clarify that question--

CHAIRMAN JENSCH: Wait a minute, now. I wonder if you'd just take one at a time here.

First you started out by saying, "Did you find the statistical correlation?"

DR. FORD: Right.

CHAIRMAN JENSCH: Yes or no. Then we will go on to the next question.

DR. FORD: My standing question is did you find any statistical correlation?

DR. ROLL: And my standing answer was no we did not determine a mean standard deviation.

DR. FORD: You didn't look for one at all?

MR. TROSTEN: Dr. Roll, do you understand the question? If you don't understand the question--

DR. ROLL: Well, I am not sure what I can say in addition to what I have already said, and I think for me to try and continue to paraphrase my answer is not going to really be responsive, because I have stated what we did. I have stated here this is data in support of this equation and I think that really is what I should do.

CHAIRMAN JENSCH: I think the question is you might have done something else and he said did you look for some statistical correlation, yes or no?

DR. ROLL: I believe the answer to the question is no, we did not do a statistical fit to the Baker-Just equations.

DR. FORD: Can you tell me in general about the topical reports that have been submitted in support of the Applicant's license, whether it is the general practice in terms of relating experimental data to analytical models that you do not perform the common routine statistical analysis of the data?

MR. TROSTEN: Excuse me, Mr. Chairman. I am not really certain whether that question is one that should be directed to either Dr. Roll or to Mr. Moore.

CHAIRMAN JENSCH: Well, I think it is a little board, but it should be limited to what they did. They can tell what they did. If somebody else has something, call another witness, but I think as far as their work is concerned, the question is proper.

In all of the work that either Messrs. Moore or

Roll did did they--finish your question, if you will.

DR. FORD: In general have you made it a practice to compute the routine statistical indices on a data sample in the process of relating it to your analytical model?

MR. TROSTEN: Mr. Moore, are you in a position to respond to that question? I believe in view of Dr Roll's more limited responsibilities in the licensing area that it would appear to me that if either of you is in a position to answer a question of this nature it would be you.

On the other hand, perhaps it should be addressed to another witness.

CHAIRMAN JENSCH: Well, he will know what he did, I assume, and therefore I think if you limit the question to what he did, and if he doesn't understand the question or something or it has to be rephrased, I think he has been on the stand enough to know that he can say he doesn't know.

MR. MOORE: We have many cases --

DR. FORD: Excuse me. The question was directed to Mr. Roll. I am concerned with --

MR. MOORE: But you have asked a broader question which I feel I should be the one to answer.

CHAIRMAN JENSCH: Can you just answer it yes or no and then explain it?

MR. MOORE: Yes. We use a statistical approach in evaluating experimental data in developing various correlations in the course of our analyses. I think the point here is we are using a generally widely accepted relationship for the determination of the zirc-water reaction and what we did in this FLECHT report was indicate that this was in fact conservative because all of the data points fell below that that would be predicted with that generally accepted correlation.

So in the specific instance we have no need to develop any statistical representation of the amount of zirc-water reaction.

You must understand that when we do these accident analyses many assumption are involved in doing the calculations and we pick these assumptions in the direction to give us a conservative answer.

DR. FORD: I see. Now can you tell me with regard to the fact that this correlation is generally widely accepted, can you tell me why you bother at all if the correlation is widely accepted, why do you bother to relate it to experimental data if you are not going to perform statistical analysis of that relationship?

MR. MOORE: We feel that's the prudent thing to do. The statistical evaluation would indicate a margin of conservatism which we chose not to take credit for.

DR. FORD: Can you tell me in terms of your exposure to experimental science whether the methodology which you call prudent is at all typical of practice in experimental physical science?

MR. MOORE: All I can speak from is my experience in the nuclear industry, and I can surely state that the assumptions that are made in our analyses are very carefully and conservatively derived and in my opinion in a very prudent manner, yes.

DR. FORD: Well, no. My specific question was the principle which you call prudent was simply taking a generally widely-accepted correlation, but not doing any statistical analysis of its relationship to the data. That's the principle that you call prudent, is that correct?

MR. MOORE: That is not correct. The statement I made was we take the widely-accepted relationship -- The prudent thing I mentioned was to check our own data against that when the opportunity arose, which was in conjunction with the FLECHT test.

DR. FORD: Yes. But if you don't do any statistical analysis what does the check consist in?

MR. MOORE: Mere observation of the figure on B20 indicates that all of the data falls below the prediction.

DR. FORD: Does it indicate that of all possible curves that could be related in a conservative way to that data that the curve that you have chosen minimizes all the error?

MR. MOORE: I am not trying to develop a better correlation which more accurately predicts the amount of zirc-water reaction. I already have a correlation which overpredicts the reaction, and therefore I did not choose to pursue it.

DR. FORD: I see. Well, as far as that aspect of the methodology, that concludes my questions.

Now the second aspect of the methodology relates to simply the range of the experimental data that you are talking about. While it makes sense in a limited look at the thing to take existing data and draw a conservative curve in relation to it, what assurance do we have that you have of all sufficient data such that the range is representative of the range that actually occurs in the phenomena?

For example, if we drew this curve here as a conservative estimate of this relationship but in fact we went to do more experiments and the points marched on below that curve, it would turn out on subsequent analysis that we hadn't made a, you know, an ultimately conservative judgment. My question is can you assure us in terms of the range of data that you have assembled that your experiments have covered all of the relevant situations, that they have been performed parametrically with all of the relevant constraints varied so that we have the full range to which we can relate the prediction, the full range of experimental data to which we can relate the analytical model?

DR. ROLL: Bear in mind it was not the purpose of our experiment to explicitly cover the range of variables as may be specifically related to the Baker-Just equations. We presented this information in support of Baker-Just and not to derive it. However, work that has been done in derivation of the equation itself as well as substantiated by others, including Westinghouse, has covered a broad range and continues to show its applicability.

I should refer again to the figure on page B20 where we are talking. We have data points in excess of ten percent, perhaps in excess of twenty percent reaction, and I believe the predicted numbers referred to in this particular application are on the order of seven and a half percent. Therefore, we have overcovered that range.

DR. FORD: What theoretical justification is there for regarding that range as a full range relevant in the experimental situation?

DR. ROLL: Please. I am not clear what you mean by that.

DR. FORD: All right. Let me try again.

What justification is there for the cut-off point that you use in terms of collecting experimental data? I mean you say you have the cut-off point at twenty per cent metal-water reaction. What justification is there for that cut-off?

DR. ROLL: I didn't say we had a cut-off point. I said reports--

DR. FORD: Simply in terms of the sample--

CHAIRMAN JENSCH: Let him finish his answer.

I said he is to let you finish your answer. Proceed.

DR. ROLL: I merely said these data which we are reporting cover a range in excess of that calculated for this specific application, and the range of conditions under which these--the range of conditions in the test itself itself under which the zirc oxide thicknesses came about were selected to be typical of the kinds of coolant temperature and time conditions to be expected for the loss of coolant accident.

DR. FORD: But what justification do you have for basing your analysis of the equation on simply those dozen dates? I mean for example if further experiments were done that indicated that not very far beyond the bounds at which you stopped a different statistical curve would be fit to the data, then that further experimental data would completely change your check on the curve, that it's only valid obviously over this specific range. And what I am wondering is what confidence can we have that your range is broad enough so that we need not worry about changes in our analysis simply based on new experimental data.

DR. ROLL: I believe the conditions of the FLECHT experiments, that is coolant flow, time and temperatures, cover the range of conditions predicted for this particular application. Therefore the range of conditions at which we are determining the zirc oxide reaction cover the range for this specific application.

DR. FORD: In all of the tests, the FLECHT tests that provided data for this analysis, were electrically heated rods used that had their heat source as an internal filament within the rod?

DR. ROLL: That's correct.

DR. FORD: Can you describe how the heat transfer conditions within the rod relate to the extent of metal-water reactions?

DR. ROLL: I believe that it in no way affects the extent of zirc-water reaction. The key parameters are time at temperature and the source of getting this time at temperature is not a factor in the extent of reaction.

DR. FORD: Well, is the temperature gradient of the fuel not a factor in the extent of metal-water reaction?

DR. ROLL: That's correct.

DR. FORD: What evidence have you done with cladding with different temperature gradients to confirm this?

DR. ROLL: Now you have asked a couple of questions. You asked me did the temperature grading in the fuel affect the results. I answered negative. Then you reasked the same question, what evidence do I have that temperature cladding is not a factor. What really are you asking?

DR. FORD: That is a clear error. I am talking about the temperature grading in the cladding.

DR. ROLL: Inasmuch as the detailed reaction kinetics of reaction between zirconium and water is partially a diffusion controlled process, certainly the temperature grading in the cladding is going to have some effect. It has not been quantified per se in our work, but our work was not directed toward obtaining detailed chemical reaction kinetics information. What our work was attempted to do was to take our set of conditions and compare them to the calculations, which we feel we have done.

DR. FORD: Can you tell me what is or what was the temperature gradient in the cladding in the FLECHT tests?

DR. ROLL: I'm sorry. I missed your question.

DR. FORD: I asked, what was the temperature gradient in the cladding in the FLECHT tests?

DR. ROLL: The grading should have been on the order of five or ten degrees from inside-outside across the cladding. This is a rough calculation based on the power in the test rod during the time of the FLECHT test. But whatever it is, it will be the same in the FLECHT test as it is in the reactor inasmuch as the same simulated fuel rod powers were used. That is the power in the rod in the FLECHT test are the same as expected in the loss of coolant accident. Therefore, the gradients are the same.

DR. FORD: Let me understand this. You are telling me what it should have been, in general, and that in addition it should be the same as what was in the reactor. You are not telling me what it was measured as in either.

DR. ROLL: That is correct. We did not measure clad o.d. temperature and clad i.d. temperature. However, knowing -- Again, basic materials like zirconium, and knowing the heat in the rod, one can then calculate very simply the temperature drop across the cladding.

DR. FORD: Inside the cladding influencing the inside temperature of the cladding, there are different conditions when you use -- Or is it correct that there are differences when you use electrically heated rods filled with aluminum with no simulation of the gap between the cladding and the fuel from the situation in the reactor where you are using ceramic UO2 pellets filament and a gas gap between the cladding and the fuel?

DR. ROLL: The test was set up with -- Let me partially modify my previous answer. We did have o.d. thermocouples on the test to measure the heat transfer coefficient which was the objective of the test, not explicitly to measure the temperature drop across the cladding. But particular to your last question, the heat into the fuel bundle was measured and the conditions in and out -- That is the coolant conditions in and out was measured. Therefore, the only way the heat could get from inside the rod to outside the rod was through the cladding. It is not a great exercise to calculate the temperature drop across the cladding.

DR. FORD: Excuse me. You indicate that in order to measure the temperature drop across the cladding you had a thermocouple on the outside of the cladding, and no thermocouple on the inside of the cladding?

DR. ROLL: No, I didn't indicate that. I believe I said that we did have thermocouples on the outside of the cladding. The objective of the experiment was to determine the heat transfer coefficients from cladding to coolant. Therefore, we had the thermocouples on the outside of the cladding. The objective of the experiment was not to determine -- Therefore, we didn't have numerous thermocouples on both sides of the cladding.

DR. FORD: I understand the objectives of the test.

I am concerned with the conditions.

This five to ten degree gradient, could you explain to me, mathematically, how this would be derived from just the information you were given? I believe it was claimed it was an obvious kind of thing to calculate. It isn't to me.

DR. ROLL: We have a piece of cladding or essentially considered a flat plate for simplicity. There is an internal temperature and an outside temperature. In simple geometry, the heat transfer, BTU per hour per square is really simply the thermal conductivity of the material times the temperature difference, i.d. to o.d. in simple geometry.

Now, what we had here was a heat source on the inside and a heat sink on the outside. By measuring both, we knew what the heat transfer was. That is the heat BTU per hour square through the cladding. That's the q/a term. Thermal conductivity of the material is a basic property of the material. So the temperature drop across the cladding can be determined from this as q/a divided by delta k. That's how I said it is a simple calculation. Is that really what you are after?

DR. FORD: The diagram was what I was after. Now I will try to comment on it.

In normal fuel rod there is a gap between the ceramic uranium dioxide fuel pellet and the fuel cladding.

CHAIRMAN JENSCH: If you can get some of your response vocally, Dr. Roll -- I think you were nodding affirmatively. Say yes so we might clarify the statement.

DR. ROLL: I agree. There is fuel inside the cladding.

CHAIRMAN JENSCH: And the gap?

DR. ROLL: And there is a gap.

DR. FORD: The gap in a reactor, after it has undergone some burn-up into its fuel cycle, there will be gas in this gap. Is it correct that the temperature on the inside of the fuel pellet is not the same as the temperature of the inside of the cladding; that the gas-heat transfer coefficient is an important determinate of the temperature drop between here and here? Simply in terms of the heat transfer within a real fuel rod, there is this particularly significant heat transfer coefficient relating to the gap.

DR. ROLL: It is correct that the heat transfer coefficient in the system you are looking at is an important factor in determining the temperatures of the pellet.

DR. FORD: Since it is the heat and the pellet being transferred through the cladding, the gap is also important in determining what the temperature of the cladding is?

DR. ROLL: Not quite correct. We really have fixed this outside temperature and we are calculating temperature increments as we go through the various thermal barriers. Given the flow of heat, this q/a terms and a sink temperature again -- And the sink temperature was measured, and is then a straightforward calculation to the internal i.d. temperature of the cladding. The calculation of temperature across this gap and into this heat source, be it a fuel pellet or an electrically heated heater of some sort, this is a significant factor. However, we are not really looking at this portion. This is a source of heat which is known and determined. We are looking at the conditions across the cladding. The conditions across the cladding are not really determined by the temperature of the simulated fuel inside the test rod.

DR. FORD: Why are you going from the heat? In terms of tracing heat flow, why are you going backwards from the heat sink into the fuel? The heat flows through the fuel or heat source over the gap through the cladding and into the heat sink. So that functionally speaking, the temperature of the cladding in the first instance, the primary source making the temperature is the heat transfer from the fuel. Isn't the fuel and the reactor used to generate the heat?

DR. ROLL: That's correct, but the temperatures in the cladding are not determined by the temperature of the fuel.

DR. FORD: Have you determined, in your computer code analysis, when you postulate the geometry and heat transfer conditions of the typical rod you consider, what heat transfer coefficient you assumed, if any, if you considered it at all, for the gas in the gap?

DR. ROLL: I believe this is in Mr. Moore's area.

MR. TROSTEN: May I interrupt just a moment? Mr. Chairman, I'm sorry for the conference taking place during the discussion, but the reason why we have been doing this is that it appears apparent to us that some of the questions that are being raised are of a somewhat general nature which really are more appropriately answered by another witness. They are really beyond the scope of the direct testimony offered by Dr. Roll.

I think from the standpoint of Mr. Ford's inquiry and from the standpoint of the public who are present here in this room, and perhaps for the Board, it would be preferable that if we were to add another witness to the panel who would be in a better position, I believe, to respond more quickly and more expeditiously to the questions being raised, it might be helpful.

CHAIRMAN JENSCH: I think you can select any witness that will assist in this regard. I thought Dr. Roll was doing a real good job myself.

MR. TROSTEN: I think he is doing a fine job, too.

CHAIRMAN JENSCH: He is explaining what has been undergone. I don't know if his answers are as good as the explanations, but --

MR. TROSTEN: I think he is doing a fine job, too. It is just a matter of not the answers that he is giving but the nature of the questions that are being raised, it seems to me, that it would be preferable that we put another witness on. I would like at this time to ask Mr. Wiesemann to join the panel there.

DR. FORD: Excuse me.

MR. BRIGGS: While Mr. Wiesemann is putting his chair up here, maybe Mr. Ford could tell us what the point is that he is trying to make.

DR. FORD: I was just going to attempt that. In my earlier questioning when I began considering the gradient within the fuel cladding, Dr. Roll answered in the affirmative to my question as to whether the temperature gradient in the cladding would have an influence on the extent of metal-water reaction. The point of my line of questioning has been to see how the heat transfer conditions, as simulated by electrically heated rods, how they relate to the temperature gradients of thecladding in the reactor and how you are capable, without simulating the gap in between ceramic pellets of the cladding, wherein you know it has a very significant determinate of the heat transfer of the whole fuel pellet from the center out to the edge. We know that. I would like to know how, when you don't simulate that, you can correctly analyze what the internal temperature would be of the fuel cladding.

As a follow-up to the affirmative answer that this gradient makes a difference, I'd like to proceed with this whole line.

I am further not too anxious to try to frame my questions for three different approaches. My feeling was that Dr. Roll was without objection technically, giving the information that I wanted. My feeling is that since this pertains to the metallurgical phenomena which he has opened up, I think it would certainly confuse me and not confuse matters to substitute witnesses at this point. I realize that the interrelationships of various phenomena within the accident situation are such that we can have a man for every variable. But I think a little bit of sythesizing at this point would be useful.

MR. TROSTEN: Mr. Chairman --

MR. BRIGGS: Excuse me just a moment. It would help me to know, are you familiar with heat transfer calculations?

DR. FORD: I have studied very carefully the entire FLECHT fuel rod simulation of an actual heat rod. That's why I am so interested in the gas gap coefficient and what problems result from the lack of simulation of that. I think that I can claim at least very substantial presumptive grounds, personally, for the investigation that I am doing. Although it is quite clear to me that basic theory in-depth I cannot fathom.

MR. WIESEMANN: Excuse me. In listening to some of the questions, I have detected a couple of questions which indicate perhaps a fundamental lack of understanding of a couple of things which perhaps if I explained them Mr. Ford would be able to understand how we arrive at the delta T without considering the condition of the gap and whether or not we have the pellets.

CHAIRMAN JENSCH: Proceed.

MR. WIESEMANN: First of all, the question you raised was you didn't understand why Dr. Roll seemed to be working from the outside back to the inside when the heat was coming from the inside.

If we take a situation where this exists where there is no heat being generated, all of the temperatures or all of the components, whatever they are, are uniform. When you turn the heat source on, the heat begins to rise internally. As the temperature rises internally, heat begins to be transferred. The transfer of heat is a function of the temperature gradient across the assemblage. However, at any point in time at any given temperature gradient, the amount of heat flowing from the surface of the fuel, or whatever heating element is used, through this system is the same through each portion.

If it were not the same, or, in other words, if there were less heat being transmitted through the cladding than were being transmitted from the fuel, the temperature would rise. If there was more heat being transmitted through the cladding than through the fuel, the temperature would drop.

So that when we come to an equilibrium condition, we have the situation where the total amount of heat being generated in the fuel is also being transmitted through the clad gap.

Going back to your question about working backwards, if you know the temperature of a fluid, the external portion or this heat sink, as you call it, that you can calculate backward knowing the total amount of heat that is generated. You can calculate backwards and get the temperatures at any point along here. Also, you can take any single element involved here, and knowing the total amount of heat that is being transferred through this expression, this one here or this one that you choose, you can calculate the difference in temperature between these two points. You can't determine the absolute temperature. You need a measurement of a temperature to get the absolute temperature of these two points.

Mr. Roll, or Dr. Roll, was trying to explain to you that the temperature gradient here is the function of heat that is generated in the fuel because that heat is being transferred through the cladding. That's a fundamental law of conservation of energy and continuity that leads to this conclusion that what goes in has to come out on the other side. Otherwise you do not have a steady state situation.

Does that help explain?

DR. FORD: I know what goes in must come out. I am just trying to ascertain whether you have any direct measurements that indicate that what goes in and out on your simulated fuel rod goes in and out--whether it does this in exactly the same way as a real fuel rod.

CHAIRMAN JENSCH: Let's stop right there. That's really the nub of your whole inquiry. Can either gentlemen volunteer. Would you like to direct the question to which one? Dr. Roll is sitting on this side. Would you like to try him first?

DR. FORD: Yes.

CHAIRMAN JENSCH: Dr. Roll, please.

DR. ROLL: In answer to the question of whether we have direct experimental evidence what goes in and out in our tests are the same as what goes in and out in the fuel in the reactor, the answer to that question is no. However, we again revert to the fairly straightforward calculation that both myself and Mr. Wiesemann attempted to discuss, that if you know your source term, your heat source term in units of heat flow per area, and you know the basic properties in the material, it is a relatively simply calculation to determine the temperature gradients through the cladding. That really is what we are interested in. That really is what I believe the line of questioning is meant to characterize.

DR. FORD: Can you explain to me why some of the analytical models for analyzing the core heat-up, why they explicitly simulate the gap between the cladding and the fuel, and why they explicitly simulate changes in the gap between the cladding and the fuel during the course of a loss of coolant accident?

MR. MOORE: In the course of the loss of coolant accident of the gap conductants that is important in determining the initial temperature of the fuel so that we know the stored heat in the fuel, which then must be removed early in transient. The gap conductants later in the transient when we are in the period of adiabatic heat-up, and this is the point in time where we are getting the metal water reaction, the gap conductants is really not important because the cladding and the fuel are heating up together with very little temperature difference between the two, because the power levels are very low. The reactor has been shut down. And it is under these conditions that we are talking about calculating metal-water reactions where the total heat transferred through the clad is very small and therefore the gradients are very small throughout the fuel pellet.

DR. FORD: Well now, can you tell me do the Westinghouse codes explicitly simulate the gap between the cladding and the fuel?

MR. MOORE: Yes.

DR. FORD: Do they assume that the gas in that gap, the heat transfer coefficient--

MR. MOORE: Do we assume a heat transfer coefficient? Is that the question.

DR. FORD: Yes.

MR. MOORE: Yes.

DR. FORD: Can you tell me is that heat transfer coefficient the same on different axial levels of the rod or are they different?

MR. MOORE: No. The gap coefficient varies depending on the power level in the rod.

DR. FORD: Now if the gap closed in a earlier portion of the transient, would this bring about a much greater increase in fuel rod maximum cladding temperature than if the gap were preserved in its usual form throughout the transient?

MR. MOORE: Not necessarily, no.

DR. FORD: Well, what analysis have you done, what sensitivity analysis have you done, to relate the maximum clad temperature to this particular variable, to both the size of the gap and its heat transfer coefficient?

MR. MOORE: We have looked at the effect of an initial gap which is as I mentioned earlier primarily and effect on stored energy. So that we tend to overpredict the amount of stored energy. And then we have looked at sensitivity studies to assign gap coefficients which tend to get the heat out of the fuel, to the cladding, at the worst time with respect to the transient.

DR. FORD: No, no. Explicitly in terms of what assumption do you make about the gap that overpredicts.

MR. MOORE: I don't have the exact numbers right here. I would have to get them. There is an initial gap conductant assumed and that later in the transient a reduced gap conductant is assumed.

DR. FORD: Well, the initial one being higher, does that mean that the heat transfer is greater, and that contributes to redistributing the stored thermal energy more quickly than if you assumed the lower one first? How is that conservative?

MR. MOORE: Well, I am not going to assume a very low, very unrealistic gap conductance at the beginning of a transient.

DR. FORD: Well, but explain to me how you have done this conservatively? According to your statement you estimated that the heat transfer for the gap is high early and low later.

MR. MOORE: Higher earlier and lower later.

DR. FORD: Lower later.

Now on what grounds do you claim that the specific quantitative estimate, that you claim that that is conservative? If it were lower earlier, I mean, or if it's simply lower than the number you choose, that would increase or delay the removal of store thermal energy, is that correct?

MR. MOORE: I am sorry. I missed the question.

CHAIRMAN JENSCH: Let's take the first one. Why

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is the assumption of a higher, whatever the word is here, coefficient--

DR. FORD: Heat transfer coefficient.

CHAIRMAN JENSCH: --more conservative?

MR. MOORE: The assumption of a higher heat transfer coefficient at the beginning of a transient is not conservative per se. The coefficient that's used is a low value considering the actual operating conditions of the fuel and cladding. In other words, we at the beginning of the transient underpredict the gap conductants such that the stored energy tends to be higher. But then it makes sense later in the transient for this gap conductants to be reduced.

DR. FORD: How does the assumed heat transfer coefficient for the gap during the early stages of the accident, how does that relate to the normal heat transfer coefficient for the gap?

MR. MOORE: It is lower.

DR. FORD: By about what fraction?

MR. MOORE: I don't have that figure here.

DR. FORD: Is it ten per cent lower, fifteen per cent lower? Do you have any idea what the order of magnitude is that we are talking about?

CHAIRMAN JENSCH: He is pausing. I wonder would it serve your purpose to get the exact figure at a later time and proceed in the record?

DR. FORD: Yes, it would.

CHAIRMAN JENSCH: Proceed on that basis. Would you do that.

MR. MOORE: Fine.

DR. FORD: Can you set forth the experimental data that you rely upon, perhaps when you present the coefficient itself, the experimental data that you rely upon to demonstrate in the statistical analysis that it is indeed a conservative statement for this coefficient?

MR. MOORE: Yes.

DR. FORD: If you can present that.

Now back to the whole thrust of this line of questioning. Is it a true statement that if you assumed a very low, a much lower heat transfer coefficient for the gap than you do now in the early stages of the accident, is it true that this would result in much higher temperature for the cladding and much metal-water reaction?

MR. MOORE: The results are sensitive to gap conductance. So if I had very low gap conductances, yes, I could get higher temperatures.

CHAIRMAN JENSCH: Higher temperatures at the cladding would result in a greater metal-water reaction, is that correct?

MR. MOORE: That's correct.

DR. FORD: By very sensitive what do you mean in a quantitative way? What per cent change in the heat transfer coefficient to the gap. What would be the per cent change of maximum cladding temperature that would accompany that?

MR. MOORE: I would prefer to give you those figures when I get the exact figures for conductances that's used in the analysis.

MR. FORD: Well, can you?

MR. MOORE: I am not recalling from memory what the results of the parameter studies, the sensitivity studies, actually were.

MR. FORD: I think then if the basic data on the sensitivity will come at a later point I will conclude now our questions with regard to the similitude of the fuel rod, simulated fuel rod heat transfer, to actual fuel rod heat transfer.

We do have additional questions on metal-water reactions, though.

The interim policy statement of the Atomic Energy Commission published on June 29th on Emergency Core Cooling Systems noted that the fuel element cladding temperature of 2300 degrees "has been chosen on the basis of available data on embrittlement and possible ssubsequent shattering of the cladding."

That's Section 4 of the Interim Policy Statement, paragraph A 1.

CHAIRMAN JENSCH: Now I notice Applicant's counsel has handed a document to the witness to let him read it before you propound your question.

DR. FORD: Oh.

MR. WIESEMANN: What was the question?

DR. FORD: I was just calling your attention to that statement about which I was going to ask you a number of questions. The first question concerns or is why is cladding embrittlement a relevant criteria for determining the maximum clad temperature during the loss of coolant accident?

MR. TROSTEN: Mr. Chairman, it seems to me that a question of this sort really ought to be directed to the witness from the Atomic Energy Commission Staff. At some point during the course of the hearing if the question has been directed to the Staff and Mr. Roisman wishes to ask for the Applicant's witness' comments on what the Staff has said or comments on the criteria generally we might not have any objection to that. But I think questions generally directed to why the criteria are what they are and whether they are valid and questions of that general sort really should be directed to the witness for the Regulatory Staff.

CHAIRMAN JENSCH: Well, I think it is a question of semantics here. I think under the analysis that Applicant's Counsel has propounded that questions in that regard were not to the Staff but perhaps as I understand this question it is to these gentlemen who are in the nuclear industry, and I understand he is seeking to find out something as to what would be the available data in their understanding, why is 2300 a significant cladding temperature consideration. I inferred from the question that he was seeking some data from these witnesses as to their understanding of what it is. These witnesses can't speak for the Commission, of course.

DR. FORD: It's our understanding that the development of the interim criteria, in the development of the interim criteria the Commission solicited and received the assistance of the major reactor vendors, that they supplied information in terms of their experimental programs. They conducted computer code calculations and sensitivity analysis for the Commission and so it's our feeling that the reactor vendors are in a very good position to explain, to make a contribution to the explanation of the criteria, and to clarify the experimental support for the criteria.

And since in addition the computer codes of course mentioned in the criteria, the Westinghouse code, is proprietary material, and there is no way -- The Commission itself is not in a position under its disclosure regulations to simply release this proprietary material, so there is no other way except asking the vendors on this aspect of the interim policy statements, asking them germane questions.

CHAIRMAN JENSCH: Has that cleared that up?

MR. TROSTEN: To this degree, Mr. Chairman. Applicant's Counsel has no objection to the witnesses for the Applicant responding as technical experts on matters pertaining to the interim acceptance criteria or on the Westinghouse evaluation model.

CHAIRMAN JENSCH: I think the question should be understood in the context you have just stated and on that basis the witness can consider the matter. Can you do that, please.

Do you have the question before you, Mr. Moore?

MR. MOORE: It might be well if the question were repeated.

CHAIRMAN JENSCH: Restate it, please, if you will.

DR. FORD: Could the Reporter read the question back, please.

(The pending question is read by the Reporter.)

CHAIRMAN JENSCH: Can you handle that, Mr. Moore?

DR. FORD: The question was as I wanted to say, why was the clad embrittlement <u>the</u> relevant criteria, not just a relevant criteria?

MR. MOORE: In determining maximum temperature?

DR. FORD: That's correct.

MR. MOORE: Well, clad embrittlement is, of course, of interest because we want to be sure that the cladding is maintained in a way that we can insure a coolable geometry in the core. So we want to preclude embrittlement which could have the potential for causing shattering of the cladding.

This embrittlement limit has then been related back to an allowable peak temperature.

DR. FORD: My question is more general. It is why in terms of all of the phenomena that could go on in the loss of coolant accident, that is functionally related to maximum clad temperature, why is embrittlement suggested as the relevant criteria? I mean for example, why is not the threshold level for the onset of changes in core geometry, why is that not fed as the relevant conservative place for maximum clad temperature, which would be at 1600 degrees?

MR. MOORE: Well, I can only state my judgment, that's really an Atomic Energy Commission limit, but --

DR. FORD: Well, let me clarify.

CHAIRMAN JENSCH: Let him finish his answer.

DR. FORD: Okay, I am sorry.

MR. MOORE: In order to show that we can tolerate this highly unlikely event, which is this double-ended rupture of primary system piping, our main requirement is to ensure that we can continue to cool the core. It is not one of trying to maintain the core within limits that, for example, would allow you to return to operation. This is a highly unlikely situation. So the design basis is to ensure public health and safety by ensuring ourselves that we can continue to cool the core.

DR. FORD: Yes, but why do you consider the onset of embrittlement as a more important factor in your ability to maintain the core in a coolable configuration? Why do you maintain that as more important than simply the general onset at about 1600 degrees of major change in core geometry?

MR. MOORE: It's not a question of it being more important. It's just in our judgment this represents a reasonable limit which does ensure the fact that you can cool the core?

DR. FORD: Well now, if the threshold level defining major changes in the onset of core geometry were chosen as the basic reason for maximum clad temperature, then is it correct that instead of a 2300 degree max clad chosen on embrittlement consideration we would have something like a 1600 degree max clad chosen on the basis of changing core geometry.

MR. MOORE: I indicated we want to ensure a coolable geometry, not the fact that geometry cannot change.

DR. FORD: Well now, with reference to what constitutes a coolable geometry, you obviously believe that a badly damaged core, from embrittlement, would no longer be, or you couldn't guarantee that it would be coolable. But do you contend that you can guarantee that a core that is simply badly damaged from rod swelling and so forth, you can guarantee under all the conditions that might be postulated, all the range of gas pressures, internal pressures, the heating rates, can you guarantee without scientific qualification that these kinds of changes in core geometry won't interfere with your ability to effectively turn around the temperature transient for a LOCA?

MR. MOORE: I must say that's a very broad question. With respect to the Indian Point reactor and the conditions that are postulated for the initiation of the accident and the conditions that are calculated subsequent to the accident, yes, the core is in a coolable geometry, and this has been corroborated by extensive amount of experimentation with these geometries.

DR. FORD: Yes, but --

CHAIRMAN JENSCH: Excuse me. I wonder if you could go back to the question. I think the connections that he is propounding may be different than those you had postulated.

DR. FORD: I was specifically trying to be quite general. I am talking about all of the conditions that you might postulate under a range of internal gas pressures going from approximately fifty psi to 2000 psi for the rods over a range of internal heating rates during the heat-up going from a range of five degrees Fahrenheit a second to a 100 degrees Fahrenheit a second. These are the experimental conditions that are being used in the current research on fuel rod variances and core geometry changes.

What I am asking about, over that whole range it's one thing for what specific pressures or heating rates, that you selected are, whether you can do it in that circumstance, but I'm asking over the whole range of influential, importantly influential conditions, whether you can guarantee that this emergency core cooling system will be able to function effectively and turn around a temperature increase.

MR. MOORE: The answer to that is yes for these ranges that you are talking about, as long as they are ranges within the expectation of the operation of this reactor.

DR. FORD: I see. Now do you challenge the ranges that are currently used in research on geometry changes in pressurized water reactors, namely the ranges that I indicated of between fifty psi to 2000 psi internal rod pressure and from heating rates from forty-five degrees

Fahrenheit to 100 degrees Fahrenheit per second? Is that range unreasonable to you?

MR. MOORE: No.

DR. FORD: Now within that range can you present experiments over that entire range? I believe there were 168 burst tests. Can you tell me what range of pressures were tested there?

MR. MOORE: Table I in WCAP-7379-L I am looking at indicates a --

MR. ROISMAN: Did you say Volume I?

MR. MOORE: Volume I.

MR. ROISMAN: Mr. Chairman, we have a Volume I here of that report which I gather is normally proprietary. We got a non-proprietary version with the numbers crossed off.

MR. MOORE: Table I. Are Table I and Table II in the report?

MR. ROISMAN: I am not sure the Tables are attached.

Let me see.

MR. MOORE: Page 9.

MR. ROISMAN: Pages 9, 10, 11, 12 and 13 were deleted from our copy.

CHAIRMAN JENSCH: If we took a recess you could confer as to whether there is a release of proprietary information possible. At this time let us recess to reconvene in this room at 11:50.

(A brief recess is taken.)

CHAIRMAN JENSCH: Please come to order.

Intervenors' interrogator, ready to proceed? Will you do so, please?

DR. FORD: Just to begin with the report on the discussion between the Intervenors and Applicants, we are going to arrange to sign agreements covering my access to proprietary material. But I have some general questions about the nature of the tests that I think can be answered. If it is at all proprietary material--I don't know what is proprietary or not. --then I will stop it and defer it to later.

CHAIRMAN JENSCH: Very well. Proceed.

DR. FORD: Are the tests that you have performed over the various ranges of internal pressures and so forth, are they all single-rod tests or do you have bundle tests as well?

DR. ROLL: We conducted both types of tests. The single-rod tests reported in the WCAP 7379 reports and the multi-reports as reported in WCAP 7495 reports.

DR. FORD: And the multi-rod tests are the ones that are the proprietary ones; is that correct?

DR. ROLL: Both tests reports are proprietary. You have a volume 1.

DR. FORD: I have a non-proprietary part. I have a proprietary class 3, which means non proprietary.

DR. ROLL: That's volume 2 of 7379. That work was conducted under AEC contract and was released as a public document.

DR. FORD: On the type of heating that was done, is it a correct statement that all of the single-rod tests used induction heating rather than internal filaments?

DR. ROLL: No. It is not. Our single-rod test used radiant heating, not induction heating. The multi-rod test did use the rods themselves as the heater element. We did not have an internal heater.

DR. FORD: Electric filaments?

DR. ROLL: No. We used electrical resistance of the rods themselves.

DR. FORD: Is the material governing such things, for example, as the temperature coefficient of electrical resistivity for the rods themselves, is that proprietary information?

DR. ROLL: No. That's general available information.

DR. FORD: Does the zircalloy cladding of a high temperature coefficient of electrical resistivity?

DR. ROLL: The coefficient of electrical resistivity for zircalloy is adequate to get the heating rates needed for the purposes, which were practical.

DR. FORD: I am asking for an additional parameter about the experiment, namely, whether or not it has a high temperature coefficient of electrical resistivity, whatever its adequacies in other respects are.

DR. ROLL: It has a temperature coefficient of resistivity which we were aware of and used in desigining the rods. I don't know what context you are asking the question, high.

DR. FORD: Are you aware of the phenomena that will result if it had as molyddenum filaments had?

DR. ROLL: Yes. We did not experience that phenomena.

DR. FORD: You didn't experience any? What was the power difference axial levels? You didn't get any power shifts? Has it any coefficient of electrical resistivity at all? Then you sould get a feedback.

DR. ROLL: We were concerned primarily with degrees per second. We did calibrate the experiment so that we knew for given power input to the total test bundle in the molyddenum rod tests, and we could then, by experiment, determine what power input was needed to get to degrees per second that was germane to the test.

DR. FORD: I appreciate that, but that's not responsive. I am concerned with whether or not, on different axial levels of the bundle, you experienced increases in temperature due to the temperature coefficient of resistivity of the cladding.

DR. ROLL: Of course.

DR. FORD: Fine. What was the magnitude of that power shifting?

DR. ROLL: I didn't say we got power shifting. You asked a question, did we get increases in temperature due to the thermal coefficient of resistivity. I answered in the affirmative. I didn't say we got power shifting.

CHAIRMAN JENSCH: Let him answer.

DR. ROLL: I know what you are seeking. I believe there were some difficulties, experimental difficulties in other tests in the FLECHT program, not our own, relating to the change in resistance of the heaters with the temperature. They got some results which they later said were not relevant. They had problems determining the results because of that. We calibrated our experiment. We calibrated our test rods so we knew what kind of axial temperature grading we were getting during our tests for our set of test conditions.

MR. FORD: I know it is all calibrated and determined. What I am concerned to determine is the way in which it is calibrated, how that relates to the differences, the axial differences in temperature, and how this relates to metal-water reaction, how it relates to swelling, how it relates to embrittlement, and so forth.

CHAIRMAN JENSCH: What is your question?

DR. FORD: There was a comment to explain why--the comment is to provide a statistical, if you will, comparison of between the axial power levels of the test rods and the axial power levels of temperature differences along that you expect or have good reason to expect in a reactor during transients, heat-up transients.

DR. ROLL: Let's try it again.

CHAIRMAN JENSCH: Wait a minute. I think sometimes the question gets a little long because you are explaining why you are asking it. Do you have any statistical --

DR. FORD: Comparison of the axial power levels of your test rods versus the axial power levels of honest-to-goodness rods during a loss of coolant?

DR. ROLL: My answer is, no, we do not. I'd like to append that answer by saying, the power level in these experiments are not germane. Power level is not germane.

DR. FORD: Doesn't the local increase in power level feed back mechanisms that result from any positive temperature coefficient of resistivity, and doesn't that cause power shifting? It is a question of the magnitude that you may want to say is insignificant. Isn't that the whole reason why we are concerned with what the coefficients of resistivity are?

DR. ROLL: I'd like to try to remake the point that power per se is not the important parameter here. We are trying to simulate in rate of heat-up, that is degrees per second. That is what we felt was the important parameter that was the parameter that was measured. I believe our ability to measure this parameter and report it is contained in the documentation.

DR. FORD: You measure this parameter of degrees Fahrenheit per second. Do you measure it at different axial levels?

DR. ROLL: Yes.

DR. FORD: Is the ratio of a given location to mean heat-up rate for your test bundle with its electrical simulation, is that exactly the same as the ratio of that parallel location to mean heat-up rate a real honest-to-goodness rod? May I contribute a diagram?

DR. ROLL: We believe it is. We believe the multi-rod burst test is adequate simulation of power distributions and heat-up rates that will be observed in the actual fuel assembly during the loss of coolant situation.

DR. FORD: I'm sure you believe that. I'm asking what confirmation you have in this specific case, that the phenomenon of the temperature coefficient of electrical resistivity of the cladding does not produce a dissimilar axial distribution of heat-up rates during the accident situation.

DR. ROLL: The distribution of heat-up rates along the rod were within five to ten percent of a mean value. Not necessarily of the objective value but five percent to ten percent of the mean value. That's a rough number, looking at these data.

DR. FORD: In other words, there was no pronounced cosine axial curve?

DR. ROLL: That's correct.

DR. FORD: That it was simply uniform or more or less uniform along a five percent or ten percent variation along the axial?

DR. ROLL: Except for the end, which was six inches of the rods. There were heat losses at the ends of the experiments. Three-foot long rod. We believe the end six inches probably had a lower heat-up rate.

DR. FORD: So that within the relevant eleven feet of rods, the temperature distribution is more or less uniform?

DR. ROLL: No, I didn't say that.

DR. FORD: Within five percent?

DR. ROLL: No. The test had a roughly two foot flat power --

DR. FORD: They are not full-length rods?

DR. ROLL: No, three-foot rods.

DR. FORD: Of course. I see.

At any rate, nevertheless, along the instrumented range that you are concerned with, the power distribution and the heat-up rate was pretty much uniform; is that right?

DR. ROLL: Quite uniform, yes.

DR. FORD: Is this uniform heat-up rate -- That's quite different, is it not, from the axial cosine power curves and so forth that you expect in real fuel rods? The middle is in relation to the mean. The middle of the rods in the plant run about forty or fifty percent in terms of what the accident heat-up rate is just in terms of power density of -- The center point is fifty percent more than the mean; is that correct?

MR. MOORE: That's correct, for the twelve-foot reactor. The purpose of the three-foot tests were about a one to two foot uniform kind of heat-up rate and was to simulate the hottest region of the core looking at that axial power distribution that you were talking about, looking over the region of highest temperatures, which is one to two feet.

DR. FORD: So that an important question about rod failures, such as axial randomness or non-randomness, is not answered at all in this test; is that correct?

MR. MOORE: On the contrary. I disagree. The tests which had a uniform heat-up of over a region of about one to two feet is equivalent to a reactor situation where the highest temperatures, the highest power parts of the rod along the twelve-foot length, over there there is a one to two foot uniform power generation. That's the region of interest. That's why the tests are applicable.

DR. FORD: I see.

Now, is the Applicant's position that you expect local concentration of fuel rod failures, that you don't expect axial randomness?

MR. MOORE: We expect axial randomness within this two feet region, two foot region, as we observed in the burst test.

DR. FORD: So that within the microcosmic world of the test, there is some randomness. But in terms of full length fuel rods, you expect that the fuel rod failure would be concentrated in one and a half to two feet; is that correct?

MR. MOORE: They would tend to be, just on the basis of the power distribution, the heat-up rate, to be concentrated in a two-foot elevation of the rod.

DR. FORD: Within just the small rods, what was the axial distribution observed. What percent, let's say, of the failures were exactly at the mean, the mean length of the heated portion?

MR. MOORE: I would refer you to the figures in this report, WCAP-7495.

DR. FORD: Is that a proprietary document?

MR. MOORE: Yes, it is proprietary. You can see what degree of random this is. It is quite random.

DR. FORD: What, statistically speaking, does that mean? What is the standard deviation?

MR. MOORE: I suggest you look at the figures and arrive at your own conclusions.

DR. FORD: I am perfectly happy to examine them. I don't know what the conditions of the agreement are. Has Westinghouse performed a statistical analysis of the randomness of fuel rod failures from this test data?

MR. MOORE: Yes, in terms of determining the consequent flow blockage that you obtained with these failures?

DR. FORD: In this statistical analysis, with reference to the mean point of the test bundle, what is the standard deviation? What is the mean relationship of failures to that mean area of the rod, and what is the standard deviation?

MR. MOORE: The data reduction of the multi-rod burst tests with respect to the maximum blockage and the correlation of this data is indicated in figure 2 of report WCAP-7495, Volume II. Again, it is a proprietary report.

DR. FORD: That's figure 2 of WCAP --

MR. MOORE: 7495-L, Volume II.

DR. FORD: Thank you. I think it is going to be difficult to continue this transient with proprietary material that can't be discussed. I think I will return to the questions concerning embrittlement that started us off on these tests.

CHAIRMAN JENSCH: Proceed.

MR. FORD: What experimental data has Westinghouse amassed that would assist or on a basis which occurred to determine the point at which embrittlement of the rod becomes a hazard and which could determine which maximum clad temperatures should be set to avoid problems due to embrittlement?

MR. ROLL: The data, the tests that we have run on conditions leading to embrittlement of the rods are what we call the series of quench tests, and these are reported in WCAP 7379, Volume 1.

DR. FORD: I have the non-proprietary Volume 2 of that. In these quench tests it says, and I quote, this is page 6, "Quenching of the samples was achieved by spraying the samples from above with room temperature water."

In a loss of coolant accident situation with Westinghouse, any of the Westinghouse emergency core cooling systems spraying from above with room temperature water?

DR. ROLL: No.

DR. FORD: Would you explain then both with regard to the use of the spray and the use of room temperature water how this quench test simulates the conditions that would occur in the Indian Point 2 reactor with Westinghouse's emergency core cooling systems operating if they don't spray, if they don't use room temperature water?

MR. ROLL: The intent of the test was to subject to rods to a fairly abrupt therman transient and similar to that but not exactly the same as that to be experienced in an actual reactor. And this was accomplished for reasons of experimental problems.

Bear in mind these were done in a hot cell. They had to be done remotely, and the method of spraying was felt to be an ample simulation of the thermal transient that the rods would be subjected to.

DR. FORD: Of the thermal shock?

DR. ROLL: Right, therman shock.

DR. FORD: Well, if this is correct that a spray situation at room temperature water is an adequate simulation of conditions in a pressurized water reactor, but this imply that the data evolved by General Electric and evolved in the BWR FLECHT tests involving spraying things with room temperature water, that all of these experiments provide relevant data for the pressurized water reactor?

MR. MOORE: To what GW data are you referring?

DR. FORD: I am referring to all of the tests that General Electric has performed and tests that have been subcontracted to General Electric by Idaho Nuclear pertaining to the BWR FLECHT tests.

DR. ROLL: I don't think you can make that generalization. I think that conditions here were not really an attempt to simulate a spray from the top. We were attempting to simulate a thermal transient on the rods.

DR. FORD: Right, but I am correct that the only mechanism of cooling, the only mechanism of giving a thermal shock to the rods, the only mechanism of quenching, was something that's quite similar to the other kind of reactors, boiling water reactors' emergency core cooling system.

DR. ROLL: No, I don't think again you can make that generalization. I said for reasons of experimental problems related to heating up and quenching and irradiated fuel rod remotely in a hot cell this is the way the experimenter elected to do the job.

DR. FORD: Right.

DR. ROLL: I don't mean to infer--to go into your broad generalization at all.

DR. FORD: I see. But how do you establish--I mean there has to be some relevant isomorphism between the experimental result and the situation in the real reactor. Are you going to draw any implications from the experimental result to the expected performance of the reactor emergency cooling system and the fact that you cannot in general use BWR FLECHT data with their kind of cooling mechanisms and so forth? It seems to me that the general applicability of this test, this kind of test, is what is in question. I am wondering if you can give me the justification--I understand that experimentally it may be more convenient or may be impossible in the particular way you wanted to set up the experiment to do anything but do it with the spray system at room temperature water that you used.

What I want to know is how the concocted test set-up, how you can infer at all without substantial error the relationship between these test results and situations in your pressurized water reactor during temperature transients.

DR. ROLL: Is your line of questioning relevance of the tests that were run in the report of Volume 2?

DR. FORD: Precisely.

DR. ROLL: Perhaps it's necessary to look at also out Volume 1, but in that series of quench tests there we actually drop the rod into a container of water to get the effects--we did not spray them in tests that we ran reported in Volume 1.

DR. FORD: It does not simulate phenomena you expected to occur in the loss of coolant accidents, that is the rod is going to drop?

DR. ROLL: We felt that was an adequate simulation of the thermal transient that the rods would see. That is time from the higher temperature to the time at essentially room temperature.

DR. FORD: Well, is your time to quench based on--

DR. ROLL: I am sorry.

DR. FORD: Is your time to quench based on a test that involved dropping a rod?

DR. ROLL: That is correct.

DR. FORD: Into a large container of water?

DR. ROLL: That is correct.

DR. FORD: Rather than something that simulated how a rod in the environment of other rods also undergoing thermal changes would be cooled by--

DR. ROLL: The tests that we ran--

CHAIRMAN JENSCH: Wait a minute. Let him finish.

DR. FORD: I will repeat the question.

Does your test or is your correlation of time to quench based on a test that involves dropping a rod into a container of water, how does this simulate the actual conditions under which a rod would actually be if it were quenched, namely that it's in the dynamic environment of other rods also undergoing thermal transients. I just can't see at all the basis isomorphism between your experimental situation and the situation you postulate to exist in a loss of coolant accident.

DR. ROLL: The tests that we ran put more severe transients on the rods than we would expect during the quench phase of the loss of coolant accident.

DR. FORD: Didn't the test in a radically dissimilar way deliver the coolant in a quantity that is not expected -- In a quantity and with the speed that I see no basis for expecting to occur in an actual loss of coolant accident?

DR. ROLL: That's quite correct. In a radically dissimilar and very conservative way the quantity of coolant was delivered to the test rods, and hence the thermal transient or thermal shock on the test rod was more severe than we expect to see in a loss of coolant accident.

DR. FORD: Well, did the test situation give the rod more coolant more quickly than what one would expect in the situation in a real reactor?

DR. ROLL: That's correct.

DR. FORD: Well, exactly how is giving it more coolant more quickly conservative?

MR. WIESEMANN: I think maybe you ought to explain what the phenomenon of thermal shock is.

The phenomenon of thermal shock consists of establishing temperature gradients within a piece of material which result in stress gradients, and if I take a piece of material and I subject this material to a temperature gradient and this material is, let's assume just for purposes of discussion, that this material is not free to move, it's restrained by other materials surrounding it, at this high temperature we have expansion, thermal expansion of the material where the temperature is high which is greater than the expansion of the material where the temperature is low. This means that the material on one side of the specimen is tending to become longer whereas the one on the other side is tending to become shorter, which means the material is in a sense trying to tear itself apart.

One part of it wants to be long, one part of it wants to be short. Now the steeper the temperature gradient, the more rapid the cooling, the steeper the temperature gradient and the steeper the temperature gradient the higher the stresses that occur which tend to pull the material apart.

And it's a well-established engineering fundamental from the standpoint of thermal shock that if you subject a piece of material to higher temperature gradients due to more rapid cooling that you also subject the material to higher internal stresses which tend to cause the material to fail. And this is the basis for our saying that more rapid cooling of these rods is more likely to produce the shattering or breaking mechanism. Because this material is not ductile, if it is brittle, it will break, just as a glass breaks. You have seen this happen probably in your own home where you pour some hot water in a cold glass or cold water in a hot glass and the glass, which is brittle, breaks.

DR. FORD: I see. Could you add on to your diagram the curve describing the relationship between ductility and temperature for zircalloy?

MR. WIESEMANN: I couldn't draw the exact curves, except that ductility --

DR. FORD: Analytically the general shape of the curve.

MR. WIESEMANN: But ductility and temperature are not the only considerations, and I should refer that question to Dr. Roll, because there are some chemical considerations.

DR. FORD: If you are not going to put on the curve, I can't follow it the way I wanted to.

MR. WIESEMANN: Perhaps Dr. Roll could address himself to it.

DR. ROLL: We have --

DR. FORD: Let me ask the question that concerns me that I was going to look at the curve for, namely is the temperature at which you effect cooling in the experiment lower than the temperature at which cooling would be effected if it didn't have this rapid supply of coolant?

MR. WIESEMANN: I think I can draw you a general curve, and that is the general curve of temperature versus ductility is that in general the ductility increases with temperature. Plus if the material is colder it tends to be less ductile, more brittle.

DR. FORD: In discussing an internally pressurized rod at a temperature considerably below the point at which it would perforate, would rod swelling be greater or less as temperature was higher for a rod much below its perforation point?

DR. ROLL: The rod swelling would be greater the higher the temperature.

DR. FORD: Is it also correct that the greater the swelling the greater the potential for embrittlement?

DR. ROLL: Perhaps, but I don't --

DR. FORD: Well let me ask you --

DR. ROLL: I don't see the line of questioning. The greater the swelling the greater the potential for embrittlement?

CHAIRMAN JENSCH: He will give you another one.

DR. ROLL: I say perhaps, but I --

DR. FORD: So that if the temperature and amount of swelling that had taken place in a rod, or you have two rods, which you begin to quench them when they are at different temperatures, they have different swelling, they have different potential for embrittlement, is it correct that when you are cooling rods in this experimental situation you are cooling them at higher temperatures and at lower swelling than you would if they were simply there with less coolant allowing them to swell and going up to higher temperatures and higher ductility? Is the nod affirmative?

DR. ROLL; Continuing, are we still on the ductility temperature? Is this part of your question?

DR. FORD: That's part of it. Would it help if I gave my conclusions?

DR. ROLL: Well, it may.

DR. FORD: Basically the conclusion is that in your experimental situation your cooling rods are not as swollen as they would be expected to be in the transient with much less coolant around them immediately. The fact that the real live rod which would be more swollen would mean that in the area of swelling the wall thickness would be less, the nil ductility temperature of the metal is a function of its wall thickness. In the metallurgic research at the Oak Ridge National Laboratory it was indicated that the embrittlement temperature with reduced wall thickness from rod swelling would be considerably lower than the embrittlement temperature assuming that the rod hadn't swelled up that much.

So the basic point is that from the point of view of thermal shock that analysis is all perfectly adequate. But I'm suggesting through my questions what you haven't taken into consideration is that wall thickness that you are talking about changes during the accident, and in a real situation it's allowed to change and in your experimental situations just dropping it into the water at predetermined temperature, not allowing it to continue to swell before it was finally quenched, that has a very non-conservative experimental -- That's very non-conservative experimental data related to the phenomenon that I'm particularly concerned with.

Now that I have stated my conclusions and arguments and hypotheses I hope you can respond.

MR. TROSTEN: May I just interject at this point. Mr. Ford has in a sense asked the question by stating his own views on the subject, and it seems to me that the only way that the witness can respond is either to say, "Well, I agree with what you say," or, "I disagree with what you say," and just let it go at that. Unless you have something else you can add to that.

MR. ROISMAN: Mr. Chairman, the witness asked to have that conclusion given so that he'd just have a better idea how to answer the question. Mr. Ford's prepared to go through the questions that reach that conclusion step by step, but the witness thought it might be helpful to see where he was going, and that was the only reason that Mr. Ford went on that way.

CHAIRMAN JENSCH: And after having stated that I think I know you intended to end up by saying, "Is this correct?"

DR. FORD: Yes.

CHAIRMAN JENSCH: Now will you try and answer on that?

DR. FORD: I could partition matters.

CHAIRMAN JENSCH: Let's try it this way. He may fully agree with everything you have said and that will move the case along quite well.

DR. ROLL: Let me make some observations and see if I can describe why I don't agree with your conclusion as stated.

First of all, rods which have swollen but not burst have done so because they have not been subjected to high temperatures to get them into the bursting temperature. Therefore, they have not been at high enough temperatures to get them to a point where there is any significant zirc-oxide reaction, zirc-water yields and zirc-oxide. Therefore, the combination of having a swollen rod with sizable quantities of zirc-oxide on it, I believe that combination is almost mutually exclusive.

Secondly, the swelling as it relates to wall thinning, you don't swell and burst and then contract. So that the rods which have burst have shown for their particular combination of rate and pressure, will have shown the maximum swelling for that, as I say, for that particular combination.

And then finally we did run some tests on previously burst rods and these tests, quench tests, with previously burst rods, and these data points with the rest of our data points together comprise our discussion of limits on mechanical integrity of the rods with regard to the quench phenomenon and related to total quantity of zirc-water reaction.

DR. FORD: Let me address the questions to some of the premises that you have raised.

Your first one was all I must say I really caught and I'm focusing on that a moment. You said that if the rods had not yet burst they were not at a high enough temperature for metal-water reactions, zircalloy-water reactions to take place, is that correct?

DR. ROLL: No, it's not. I said if they hadn't burst they hadn't gotten to the temperature at which they would have burst for the particular combination of heat rate and internal pressure. They are always at a lower temperature than their bursting temperature if they haven't yet burst.

DR. FORD: Right. But you are claiming that the temperatures at which they will have nct yet burst are temperatures at which a significant zircalloy-water reaction won't take place.

DR. ROLL: Well, I meant to infer there or meant to leave the impression that if they were at a lower temperature they would have had less zirc-water reactions than if they had actually gone up to this bursting temperature.

DR. FORD: But isn't it the case from the fuel rod failure tests at Oak Ridge that they have a number of failures that don't occur until a range here of 2550 degrees, 2600 degrees. Clearly that's in a range where you could have significant metal-water reaction, is that correct?

DR. ROLL: I am not intimately familiar with the Oak Ridge report.

CHAIRMAN JENSCH: Well at least in any event I think that you should show it to the witness either now or during the recess so that he will have an opportunity to review it.

DR. FORD: All right. Let me ask a specific question. Is it correct that a combinatio of low heating rate and low internal pressure -- By low I mean low in terms of pressure, between fifty and 100 psi and low in terms of heating rate, safe five or ten degrees Fahrenheit a second, is it correct that at those low heating rates and low internal pressures, the temperature at burst will be way up in the 2000 degrees Fahrenheit, in the region of significant metal-water reaction?

DR. ROLL: The conditions that we looked at and as reported in our document, we show that for -- In particular the document is 7379 Volume I, page 45. We have a curve there of bursting temperatures and we say essentially things are going to be burst by the time they get up to 2000 degrees F.

DR. FORD: I see. I don't have that in front of me. I have Volume II, the non-proprietary version of the same document.

This is Table IV, page 11 of the report you just cited.

MR. MOORE: Excuse us. We need Volume II.

DR. FORD: Now directing your attention to Table IV, page 11, it seems to present exactly the data I was talking about. It gives, for example, a low heating rate, low pressure for rod LY-12 and the temperature observed at failure is 2431 degrees. So is this affirmative evidence? And even in tests with higher heating rates but low pressure I call your attention to rod LY-7. You had a heating rate of 100 degrees Fahrenheit per second, but a low fifty psig, and its temperature at burst was 2630 degrees. So is it clear that things get up way above 2000 degrees before they burst?

DR. ROLL: It's our data and we signed off on it. Yes. The temperatures are there, but --

DR. FORD: Well now --

CHAIRMAN JENSCH: Let him finish.

DR. FORD: Excuse me.

DR. ROLL: I'm concurring in the Table that is being quoted. I say yes, that's right. He has adequately read off the data points.

CHAIRMAN JENSCH: The question was at those higher heating rates, 100 degrees Fahrenheit per second and fifty psig, that there will a temperature of 2630 degrees before they burst. Yes or no?

DR. ROLL: Does the data indicate that temperature?

CHAIRMAN JENSCH: Yes.

DR. ROLL: Yes. It's in the report.

DR. FORD: Am I correct in my observation that this is a very clear contradiction of what your idea was of temperatures at bursts of things?

MR. MOORE : That's not true, because what Dr. Roll was referencing was what happens in a reactor, and there is a specific time-temperature relationship in a reactor in terms of how the cladding does heat up, that LY-12 would be 500, 4 or 500 seconds before that temperature is reached. In fact, the purpose of this test was to try to get those kinds of temperatures and we were forced to -- Before we burst -- And we were forced to use very low internal pressures in many cases in order to achieve them. They are not representative of time-temperature history that you get in an actual fuel rod.

DR. FORD: Could you identify which combinations of heating rates and which combinations of pressures, internal pressure and temperature and heating rate, would be expected in the accident? I mean which among these are realistic?

MR. MOORE: Just looking at the Table it's difficult for me to look at all those combinations and try to derive that. I was looking at the combinations of specific interest, the 2630 and the 2431 degrees Fahrenheit, and I was excluding those specific ones as not being representative. It would take me longer to figure out how representative some of the others are.

CHAIRMAN JENSCH: And maybe this would be a good occasion to recess and have an opportunity to work on that over the noon hour. Will that be a convenient place to interrupt your examination?

DR. FORD: Yes indeed.

CHAIRMAN JENSCH: At this time let's recess. We will reconvene in this room at two o'clock.

(The luncheon recess is taken.)

Mgt1

AFTERNOON SESSION

CHAIRMAN JENSCH: Please come to order.

Will the witness resume the stand.

Intervenors' counsel, are you ready to proceed? I'd like to, before we proceed, indicate that there is no smoking in the room. Will you extinguish all burning.

Will you proceed, please?

DR. FORD: I'll wait until all the witnesses are there.

I'd like to begin by simply taking a very quick overall view at the way metal-water reactions are treated than just simply what your codes considered these reactions. At a later point we can go into justification for the way in which they are considered or the justification of not considering them, and so forth.

I'd like to know, first, whether the Westinghouse codes simulate and continuously calculate the way they do zircalloy-water reactions, whether they simulate and calculate eutectic meltings between the iconel springs and the cladding and the liquid-metal reactions that were to accompany that eutectic.

MR. MOORE: The zirc-water reaction is simulated continuously. That is the only reaction that is simulated.

DR. FORD: So that's specifically to the point, there is no simulation at all of the phenomenon of eutectic formation, eutectic melting and metal-water reaction between that eutectic alloy and steam or water in the entire course of the transient?

MR. MOORE: That's correct.

MR. WIESEMANN: As stated yesterday in the hearing.

DR. FORD: The second question is whether the codes simulate and continuously calculate the reaction rate between uranium dioxide and water, the conversion UO3 under the conditions of the accident.

DR. ROLL: Under the conditions of the accident, there is essentially no reaction between UO2 and water.

DR. FORD: But do the codes themselves explicitly what this and simulate the variables which would, under relevant conditions, contribute to a reaction between uranium dioxide and water?

Independent of your feeling as to whether or not it is necessary to simulate this, does the code have the capability?

DR. ROLL: Under the conditions which we see in the loss of coolant sequence of events, there is no reaction between water and UO2. The reaction is not favored thermodynamically. So there is none. Having this framework of conditions, then we say that it doesn't keep track of anything.

DR. FORD: I just wanted to make a survey, whether you consider these. It may be irrelevant. I would like to get an overall view of your metal-water reaction consideration.

Thirdly, do you consider possible and simulate and continuously calculate reactions between the cladding and the fuel itself, that kind of exothermic reaction?

DR. ROLL: We do not consider any effects related to a so-called reaction between the fuel and the clad.

DR. FORD: Do you consider, in your analysis, the collection of uranium dioxide and fuel in the balloons formed by plastic deformation of the rods?

DR. ROLL: No, we do not.

DR. FORD: Thank you.

Now we are back to embrittlement. I'd like to establish some general grounds.

Do you consider the degree of embrittlement as a function of integrated exposure time, exposure temperature?

MR. MOORE: We do the analysis and calculate--the answer is yes, in that we do the analysis and calculate the total amount of metal-water reaction, and compare this to what we determine to be the acceptable limit with respect to embrittlement.

DR. FORD: Cladding temperatures below 2,300 degrees, Fahrenheit, do you expect there will be any significant embrittlement?

MR.MOORE: No. As I indicated earlier in testimony this morning, very little of the cladding has any kind of metal-water reaction at all.

DR. FORD: What correlations have you developed between cladding temperature and the ductility at room temperature in metal?

MR. ROLL: What have we, Westinghouse, developed?

DR. FORD: Yes.

DR. ROLL: We have done no work in this area germane to this particular problem.

DR. FORD: So that in terms of the quench test that we were discussing before, they were concerned with analyzing the phenomenonological effects of just a variety of postulated accident situations, and is it correct to say that they were really not attempts to collect data from which you can demonstrate what the relationship is between cladding temperatures and embrittlement?

DR. ROLL: The experiments that were performed were not directed toward elucidating that the ductility versus temperature curve and hence the determination of nil ductility temperature.

DR. FORD: And so in terms of an experimental contribution to the suitability of a 2300 degree cladding embrittlement, you, Westinghouse, makes no contribution.

MR. WIESEMANN: Would you rephrase that question, please?

DR. FORD: Does the Applicant take the position, does Westinghouse take the position, that any data that they have confirmed 2300 degrees as a reasonable limit with regard to considerations of cladding embrittlement?

MR. WIESEMANN: Not reasonable, but very conservative. The data from the tests we have performed which showed that the shattering phenomenon does not occur at temperatures well above 2300 degrees, and for the zirconium water reactions up to sixteen percent confirms very clearly that the 2300 degrees is a very conservative value used.

DR. FORD: Now the use of 2300 degrees as an indicator of the onset of embrittlement, can you give us your feeling as to how changes in wall thickness of the rods that accompany local swelling, how this increases probability of embrittlement and how this changes the relationship between maximum cladding temperature and nil ductility.

MR. WIESEMANN: Your question assumes that the onset of the embrittlement occurs at 2300 degrees. I don't believe there has been any evidence adduced in this proceeding directed to this end. I think that the general, the criteria that the AEC proposed, said that that temperature is a temperature which when used would be such that you would not have this problem. But there was nothing said that this was the place which would define or demark the onset of this. In fact, I think we made it very clear that we don't have this problem at temperatures up to 2700 degrees, and for zirconium-water reactions to sixteen percent of local areas.

DR. FORD: Now in the tests in which you dropped the rods into a volume of water, were the rods internally pressurized?

DR. ROLL: No, they were not.

DR. FORD: I see. And do we assume therefore, that there was no swelling deformation of the rods?

DR. ROLL: No, you don't infer that because four of the rods that had been dropped had been pre-burst, had been burst prior to this, and then were run back to temperature again and dropped.

DR. FORD: I see.

How many rods were dropped in total?

DR. ROLL: I believe the number is twelve, eight of which are reported in Volume I and four of which are reported in Volume II of 7379.

DR. FORD: I see . So there were twelve rods, four of which had been pressurized and had been burst?

DR. ROLL: That's correct.

DR. FORD: What was the pressurization of the rods and the heating rate?

DR. ROLL: Let me change that previous statement on number of tests. I was thinking a different series of tests.

MR. WIESEMANN: While he is looking for that let's make sure we understand the question. Are you asking for the temperature and the pressure and the temperature, rate of temperature rise for the rods?

DR. FORD: I'm looking for the temperature-time history of the rods, of the initial condition before it was dropped into the water.

MR. WIESEMANN: Just before it was dropped?

DR. FORD: Right.

MR. WIESEMANN: You are not talking about the conditions that led to the bursting in the first place?

DR. FORD: Yes, that's what I said, temperature, time and history of the rods, at what heating rates, and the internal temperatures and so forth.

DR. ROLL: Let me summarize correctly the total spectrum of point test conditions. I am quoting out of --

DR. FORD: Excuse me. There are special reasons why I am only interested if the rods have been internally pressurized and I'd rather just keep my question just limited to them at the moment.

DR. ROLL: Right. I hear you but I want to correct what I said before. I said that we had twelve total quench tests and that number is wrong. Forty-seven quench tests.

Now quench tests with pressurized rods. For quench test conditions for the series of tests with the pressurized rods, the pressures considered were 100, 200, 500, 1000 and 2250 psi.

DR. FORD: Could you give me the source for this data, please?

DR. ROLL: WCAP-7379, Volume I.

DR. FORD: Page?

DR. ROLL: It's Tabe VII on page 19.

DR. FORD: I presume you are looking for the heating rate?

DR. ROLL: Oh, I'm sorry. No, I was waiting for a question.

DR. FORD: Oh.

DR. ROLL: It's not clear that we have it reported in here.

DR. FORD: Excuse me; I didn't hear that.

DR. ROLL: We don't have heating rate reported here. I don't recall what it was for this particular series of tests.

MR. WIESEMANN: This particular test is done to get the relationship between zirconium-water reaction and temperature and whether or not there is a shattering problem with respect to the cladding. Therefore, the heating rate wasn't an important factor. The important factor was the maximum temperature and the amount of zirconium-water reaction.

DR. FORD: Would you repeat the purpose of the test again, please?

MR. WIESEMANN: The purpose of the quench test was to derive the safe range for purposes of determining that we did not have any clad embrittlement problem.

DR. FORD: Now I read in WCAp-7379, is that the document we are talking about, Volume II, I read, this is on page 2, "The purpose of these tests was to compare and correlate General Electric procedures and results in testing ductively heated pressurized tube failure with those of Westinghouse radiatively heated samples."

DR. ROLL: If you read the total context of the paragraph, you will see that an additional twelve tests were run by General Electric on our material for unirradiated tubes. The purpose of those twelve tests on unirradiated tubes, was to compare procedures, that is the methodology that were used in their Cincinnati laboratory versus ours that was done in our Pittsburgh laboratories, not to derive specific information. To see if the procedures themselves gave comparative results, and that's what is stated in the paragraph, if you read the whole paragraph.

DR. FORD: I can't find--I have looked through the entire Volume 2 here. I can't find any comparison of procedures and results and so forth. I asked him to restate the purpose because it is not at all clear.

MR. ROLL: We were asking the question in the context of the quench tests. That's the context that Mr. Wiesemann answered for that particular series of quench tests. These were not quench tests that are being talked about here.

DR. FORD: Excuse me. The title on Section 2 reads, "Burst and Quench Tests on Irradiated Tubes."

MR. ROLL: And these are a series of tests that were burst tests run on unirradiated material to compare results on the apparatus used at Cincinnati to the apparatus used in Pittsburgh.

DR. FORD: Where is the comparison of results. Where is that presented. Is that not in Volume 2?

DR. ROLL: That's correct.

CHAIRMAN JENSCH: Is the witness waiting to discover some data for the question?

DR. FORD: I believe the question is where the results are a comparison between GE techniques and their own, and how is that presented.

MR. TROSTEN: Mr. Chairman, do you want the interrogation to proceed to another matter while the witnesses are looking for that? We can do that if you wish.

CHAIRMAN JENSCH: Let's see if the interrogator can wait for the data or does he want it now before he proceeds.

MR. WIESEMANN: I thought we were answering questions on embrittlement. I don't understand how we suddenly wandered off in an area related to burst tests.

CHAIRMAN JENSCH: A different question, I think, did it. If you follow the questions, you will notice they shift.

DR. FORD: I might suggest that there is a relationship between the bursts in terms of wall thickness and the susceptibility of rod to embrittlement. I might ask on this matter directly, whether the Applicant has studied the work in this area done at Oak Ridge Laboratory. The most recent paper entitled, "Analyses of LOCA Transience in Terms of

Cladding Embrittlement," by D. O. Hobson, of Oakland National Laboratory. This is contained in the 1971 Winter Meeting Transaction of the American Nuclear Society, this past October, Page 700 and 701. I'm asking whether the Applicant is familiar with this work?

CHAIRMAN JENSCH: Could you refer to the document to refresh their recollection.

MR. WIESEMANN: I think one general point--the answer to your question on this is, we are familiar with this work. One particular point I think we want to make again. We have made this before. I believe it was made yesterday. These tests have been performed with rods which had been swollen and burst, where this effect that you are talking about that might possibly result in lower temperatures of embrittlement is included within the data that was used to establish the limits of roughly 2700 degrees and sixteen per cent zirconium-water reaction that were used to bound the safe area.

DR. FORD: It is correct that the Commission has judged higher temperatures than 2300 degrees as regard to possible embrittlement as non-conservative.

MR. WIESEMANN: No, that's not true. In fact, I can interpret the statement you read this morning as saying that you expect--in fact, I can only interpret it that way, that they expect that further testing may result in higher temperatures being justified.

DR. FORD: Specifically with the Oak Ridge testing with which you indicated your familiarity, their test results are that basically at 2300 degrees from their data, you don't expect any cladding embrittlement. But, their data going up to 2400, 2500, 2600 degrees you do get, according to them, nil ductility temperatures that would indicate severe rod embrittlements at room temperature. This is their result. What I am asking is whether you have any specific criticisms of the methodology of this research and whether you can relate to us the procedure of which are critical, and explain how it produces the erroneous view that 2300 degrees is fairly well established by the data, as the point above which embrittlement would be expected.

MR. WIESEMANN: I think the answer I gave you earlier was, quite true the temperature at which embrittlement would be expected is about 2300 degrees, but the 2300 degrees has not been defined as being the threshold of the point at which there would be difficulty in maintaining the integrity of the core cladding in a reactor of the type we are talking about. The mere fact that ductility decreases does not in itself mean that there is a problem in maintaining integrity of the core.

DR. FORD: I quote here. "We evaluate that steam exposure over a range of LOCA transients having peak temperatures below 2300 degrees Fahrenheit causes little embrittlement, unless times at temperature are unusually long. Exposu-- at increasingly higher temperatures result in progressively greater embrittlement, in keeping with the rapid changes in oxidation and diffusion kinetics with temperature in this range."

MR. WIESEMANN: That's a true statement. It is not in conflict with what we have been saying.

DR. FORD: But you would disagree with that, that you get room temperature nil ductilities? The point is that they indicate there would be embrittlement problems and not, at 2300 degrees. Do you disagree with that?

MR. WIESEMANN: We have done tests which we feel show that the range of safe operation is approximately 2700 degrees, and sixteen per cent LOCA metal-water reaction. Based on this, we are convinced that the criteria established by the AEC in their interim policy statement is a conservative one. I see nothing in the evidence that you presented that would say that there was anything wrong with that conclusion.

DR. FORD: More to the point. You are citing what I believe is an experimental result which is not consistent with their experimental result. What I asked you before-- and I'm asking again.

MR. WIESEMANN: But they both show that 2300 degrees is all right.

DR. FORD: What I am asking you now is that in terms of their experimental technique and so forth with which you are familiar, do you have any basic criticisms of thie Oak Ridge report?

MR. WIESEMANN: I don't think we have undertaken to do a critique of the Oak Ridge report per se, since the results of the Oak Ridge test do not affect our conclusions, that 2300 degrees is adequate. If we were trying to prove that 2300 degrees of 2700 degrees or some other temperature of that sort were adequate, I think we might have embarked on something like that and studied all of the differences. But since there wasn't this need, this was not done.

CHAIRMAN JENSCH: I take it the answer to the question is no? The question was, have you any specific criticisms--

MR. WIESEMANN: We don't know if we have any specific criticisms having not done a detailed critique of the test.

CHAIRMAN JENSCH: You think your tests--thank you. Proceed.

DR. FORD: To get the relationship between your test results and their test results let me see whether the Board test results are consistent with this conclusion. This is from page 700 of the report.

"Peak temperatures of 2100 degrees, 2200 degrees, 2300 degrees, 2400 degrees, gave zero ductility values below room temperature. That is some ductility would be retained at room temperature. Peak temperatures of 2500 and 2600 degrees gave zero ductility temperatures of 290 and 1800 degrees Fahrenheit respectively."

Is your experimental data consistent or inconsistent with this experimental data?

DR. ROLL: I think the key point of difference in the interpretation of results between the Oak Ridge people and ourselves, it is just that it's a different interpretation of the results, that they were looking, they had a standard procedure, I believe, in their apparatus, which would be to run the sample up to a particular temperature and I believe that was the whole time there, at which time they would quickly air quench it and bring it back down to the temperature and then measure the ductility. With this standard procedure, with increasing temperatures you are also getting markedly increasing degrees of zirc, zirc to zirc oxide formation, and they are relating it merely then to a temperature of exposure per this standard procedure gives us then a particular ductility or lack thereof at room temperature.

The interpretation that we had put on our tests and in generally our tests are consistent with their tests in that it is time and temperature which is important and that 2300 degrees for whatever it takes to get a large degree of zirc-water reaction could be bad. 2600 degrees for a short time, if that short time at that temperature resulted in less than sixteen percent conversion, we would say would be acceptable. But it's merely, I think, a different interpretation and an application of the results of the test.

Time at temperature is what we would propose as being the relevant combination of primaries to consider. They merely considered temperature per their standard test conditions.

DR. FORD: I see. Then you directly contradict their statement that they integrate exposure time and exposure temperature. They don't just consider an exposure temperature an instantaneous time. You contradict their own description of their experiment.

MR. WIESEMANN: There is a difference between describing the test and correlating the results and I think what Dr. Roll is saying is that we have found that the embrittlement situation is one which correlates to the amount of zirconium-water reaction for temperatures below the 2700 degrees, and that when you consider the nature of the results as reported by Oak Ridge that you have to consider the fact that when they are reporting temperatures and nil ductility values that somewhere behind that is a zirconium-water reaction which is a function of not only the temperature they had but the time they held it there. And for example they could have reported varying ductility values for the same temperature simply because of holding it at that temperature for different times.

And as Dr. Roll is saying the reason that didn't happen was they had a standard test procedure which held the material at a given temperature for a fixed period of time so that you didn't get the results at different periods of time at the same temperature such as what we obtained in ours.

DR. FORD: I see. Excuse me. I am trying to pursue Dr. Roll and I am trying to clarify whether his statement of what the Oak Ridge tests were is consistent with their statements of what they were, and I am afraid that the parenthetical remark from the other witness is interrupting this investigation. And if when we resume Dr. Roll has reviewed Oak Ridge material I'd appreciate the Reporter would read back my last question and Dr. Roll's last answer.

CHAIRMAN JENSCH: Are you ready, Dr. Roll?

DR. ROLL: I am ready.

CHAIRMAN JENSCH: Will the Reporter read the question.

(The last previous statement by Dr. Roll and the last previous question by Dr. Ford are read by the Reporter.)

CHAIRMAN JENSCH: I wonder how much consultation is necessary if we are asking this witness to give his view of this statement. If you have something later to add, do so. I think the panel of witnesses is confusing.

Will you proceed, Dr. Roll.

DR. ROLL: The write-up which you directed is not a description of the test. It's a description of an application of an analysis of those tests, and yet the equation which they used here was derived from the test in which I had said I believe I correctly portrayed their procedure for deriving the data. With that procedure for deriving the data they applied in this article and reached the conclusions in the last paragraph of the article, and I believe that what I said, listening to it again per the court reporter, I don't see any contradiction.

DR. FORD: The point of clarification is you talked about the procedure for deriving the data. The data that I am talking about is the nil ductility temperature calculated with the equation. I am not talking about the method of deriving the individual experimental results themselves. Does that resolve the conflict between us?

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MR. ROLL: I don't think it does. I believe, as I said before, it's time and temperature which is important. The equation has both time and temperature in it. Therefore, one must have both parameters to calculate a degree of oxygen uptake and with the degree of oxygen uptake and going back to their basic data one can then determine whether or not you have no ductility temperatures that are important or not.

DR. FORD: I see. Now when they integrate that equation, integrate the temperature and time at temperature, does that fulfill the procedure that you are talking about?

DR. ROLL: That's correct.

DR. FORD: And well, don't they explain in the paragraph following the equation and that's precisely what they do?

DR. ROLL: They do. They say they integrated the equation over time and temperature for typical transients.

DR. FORD: I see. And that is satisfactory or unsatisfactory to you?

DR. ROLL: That is satisfactory.

DR. FORD: I see. Now then in this procedure which is satisfactory to you you therefore do not question that the nil ductility temperature as a function of cladding temperatures given here, you don't challenge them?

DR. ROLL: For representative time and temperature

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histories.

DR. FORD: You don't challenge them?

DR. ROLL: I don't know that they used representative time and temperature histories. If they state they did, if they integrated the equations, we will have to accept that at face value.

DR. FORD: I see. Now in terms of the experiments that you have done for these temperature references, for 100 to 2600 degrees, have you calculated nil ductility values, zero ductility temperatures?

DR. ROLL: No, we have not.

DR. FORD: I see. So there is no data, experimental data, that Westinghouse has evolved that would calculate, that would contradict the zero ductility temperatures of the Oak Ridge research?

MR. WIESEMANN: Far better than that.

DR. ROLL: That's correct.

DR. FORD: Excuse me, Dr. Roll, please.

MR. WIESEMANN: Far better.

CHAIRMAN JENSCH: I think Mr. Wiesemann until you are called upon to pad up something or add something to this I don't think it's necessary to interrupt. I think that the interrogator has to have his question to the witness. I think there is a lot of confusion among the panelists. They feel they have to confer or supplement, when I don't think the question necessarily calls for it.

Will you proceed.

DR. FORD: Thank you.

MR. TROSTEN: Escuse me, Mr. Chairman. I just want to observe that in Mr. Wiesemann's defense in this respect that the nature of the questions that Mr. Ford is raising are such that he sometimes ranges beyond the scope of a particular witness' previous testimony, the original direct, and it is perhaps understandable, therefore, that one of the other panel of witnesses may feel that the question actually is being directed to him. I appreciated what you are saying, sir, and I agree with the point you are making.

CHAIRMAN JENSCH: Well, if there is any question I think the interrogator can say, "Now I think the other witness ought to answer this." Until he does we will presume the same witness is under interrogation.

Will you proceed.

DR. FORD: I'd like to proceed from the fact that there is no contradiction at the moment between the experimental zero ductility temperatures estimated by Oak Ridge, no criticism of their procedures. I'd like to proceed to a further observation. They contend, and I quote, "We would emphasize that our data are for full wall thickness tubing. Cladding swelling would significantly decrease the thickness and would cause embrittlement to occur at lower exposure temperatures."

Is there any data evolved from Westinghouse research that would challenge this statement in the Oak Ridge research report? I will give you the statement.

DR. ROLL: I have two comments drawn to the conclusion that they reach in the article. The first comment is that in addition to making the general statement that they did the test run for full wall tubing and that any swelling would tend to reduce the thickness of the wall, let me also point out that they did the test with oxidation on both sides, so they are getting twice as much oxidation intake that we could get, time and temperature transient. So that in that degree you have one effect trading to another.

The second point that I would like to make, referring back to our own tests, are we ran a series of time and temperature transients which gave us a degree of oxygen pickup, and these results are reported in the WCAP and for cases where we got up to, in fact, several cases in excess of sixteen per cent reaction, and then subjecting the tubes to what we think are severe thermal transients, tubes which had been previously burst, and hence we were getting the attack on the thin sections, we did not find the tubes failed.

DR. FORD: Yes.

Now I'm talking in terms of -- I realize you have tests pertaining to five internal pressures unspecified, unfindable heating rates, and that you have this small amount of experimental data. My question is whether the data that you have that you can confidently say that that data contradicts challenges or confirms the conclusion that if you considered systematically the effect of plastic deformation, swelling, changes in wall thicknesses, you would have a much lower acceptance criteria than 2300 degrees.

DR. ROLL: The data which we have does not contradict the raw data from Oak Ridge tests. The interpretation of the data with regard to what we were trying to analyze says that the criteria which we have proposed of sixteen percent reaction is acceptable whereas -- The criteria of 2300 degrees F is equally possible and possibly more conservative.

DR. FORD: I'm not sure that's responsive to my question. I know that there is a certain quantity of experimental data available. What I am asking you is whether the data that you have is of sufficient quantity, that it has covered so thoroughly the various aspects of embrittlement, the various conditions that induce embrittlement and so forth, that you have evolved convincing evidence that would resolve the doubt about the acceptance temperature that is indicated by the Oak Ridge researchers.

DR. ROLL: I believe the tests that we have run adequately characterize a limit for a degree of oxygen, or a limit of degree of oxygen uptake.

DR. FORD: Are you contending that the tests that you have done were deformation resulting from five internal pressures, down to five heating rate, that that is adequate to cover the entire gamut of heating rates from 100 to -- Internal pressures from 2200 and heating rates from 5 to 100 degrees Fahrenheit? Simply as a scientist, would you be willing to state that, you know, enough research has been done? Irrespective of whether or not the five tests that you have contradict or support this, are those five tests sufficient in your judgment, involving consideration of all the parameters that influence this? Is that test sufficient or is that body of data sufficient to contradict, in a clear way, the Oak Ridge conclusion that the 2300 degrees would be too high embrittlement cut-off point if you considered swelling of the rods?

DR. ROLL: There are a number of questions there. Let me see if I can find out what they really were.

You were inferring that we had only five tests. That's question number one, did we run five tests. The answer to that question is no. As I had stated, we had run approximately forty tests altogether.

DR. FORD: But I think the point was that there were five tests that involved cladding deformation, that the rods had been burst. So you had the wall thickness change that we are talking about.

DR. ROLL: That's incorrect.

DR. FORD: All forty-seven tests?

DR. ROLL: That's incorrect. We had run eleven tests with previously burst tubing.

DR. FORD: It is eleven now?

DR. ROLL: It always had been eleven.

DR. FORD: Previously you referred me to WCAP-7379, Volume I, Table VII, page 19. You gave me five internal pressures from that. Are you still referring to that, or do you change?

DR. ROLL: You asked the question, I believe, quite a while ago, what pressures did we use for the quench test. The five pressures that I gave you was in answer to that question. We actually ran ten total tests with rods that were pressurized. In addition to that, we ran eleven tests on rods that were previously burst. In addition to that, we ran twenty-two tests on just tubing specimens at various times and temperatures, and in addition to that four we ran -- Four tests on irradiated specimens.

DR. FORD: Can you give me the tests again slowly, please.

DR. ROLL: Ten tests on tubing specimens which were pressurized, not burst. It was just pressurized. Eleven tests on tubing specimens which had been previously pressurized and burst; twenty-two tests on tubing specimens which were neither pressurized nor had been previously burst; four tests on tubing specimens which were irradiated tubing. I believe they were neither pressurized nor had been previously burst.

CHAIRMAN JENSCH: Are you reading from proprietary data?

DR. ROLL: I believe it is. This is 7379, Volume I. I believe the data, the conditions of the test and the numbers of the test are in fact part of the deleted pages in the issue which you have.

DR. FORD: We are concerned with this Oak Ridge conclusion. We are concerned, do you agree, with the effect of changing wall thickness on embrittlement and nil ductility temperatures and function of cladding? That is the issue. That is the assertion.

DR. ROLL: That's the assertion in the Oak Ridge conclusion.

DR. FORD: So we are concerned with wall thickness variations and their impact.

Can you tell me what is the relevance, just to shorten things out quickly, of the twenty-two tests that were neither pressurized nor burst, and therefore, we assume that the wall thickness wasn't affected, and the four tests on irradiated rods that were neither pressurized nor burst. Are they relevant tests to the question of the influence of wall thickness on nil ductility embrittlement?

DR. ROLL: Sure, they are, because they are related to the total metal -- Equivalent metal-water reaction. If the test conditions in these two series of tests are such as to give a particular metal-water reaction, then the results or the conclusions which you derive from those tests are germane to the analysis of conditions with a similar metal-water reaction.

DR. FORD: But the point at issue is -- Am I not correct that it is the wall thickness variation as the parameter that we are talking about altering? It is that parameter which is influencing what the nil ductility temperature is; is that correct?

DR. ROLL: Not clear.

DR. FORD: Let's do a little more basic work then. Is it your impression that wall thickness is related to embrittlement?

MR. ROLL: 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. Locally this would have an effect.

DR. FORD: Can you characterize the nature of this effect? Is it in the direction that the Oak Ridge people assert, namely that if you wanted to avoid this effect, you have to keep cladding temperature lower than if wall thickness wasn't changed?

DR. ROLL: Yes. If you wish to preclude that effect, you would in fact wish to make the limiting temperature lower. But, again, to bring us back to the point of interpretation that it is time and temperature which are important and not temperature per se. So one may not necessarily reduce the temperature if one had control over time at that temperature.

DR. FORD: I see. Well, assuming the same temperature and the same time at that temperature, would the nil ductility be different in the way the Oak Ridge people have asserted if the wall thickness were reduced versus if it were constant?

DR. ROLL: I believe the answer is yes.

DR. FORD: So that we know you are talking about a local phenomena as indeed they obviously were, with that qualification and with the further qualification that you are talking about same time and temperature exposure. You would accept the conclusion of that Oak Ridge report; is that correct?

DR. ROLL: I believe I would accept the conclusion which you have stated. Again, I don't have a copy of that Oak Ridge report in front of me. I don't think it said that that is what would happen or did they indicate the degree to which you would have to reduce the temperature.

MR. MOORE: It is here.

DR. ROLL: We do have it.

DR. FORD: Could you read it aloud, please.

DR. ROLL: You are right.

"We conclude that steam exposure over a range of LOCA transients having peak temperatures below 2300 degrees F. causes little embrittlement, unlessthe times at temperature are unusually long. Exposures at increasingly higher temperatures result in progressively greater embrittlement, in keeping with the rapid changes in oxidation and diffusion kinetics with temperature in this range. We would emphasize that our data are for full-wall-thickness tubing. Cladding swelling would significantly decrease this thickness and would cause embrittlement to occur at lower exposure temperatures."

I would agree with that conclusion.

DR. FORD: Thank you.

MR. TROSTEN: Mr. Chairman, while Mr. Ford is getting ready to propose his next question, may I ask for a five-minute recess so that I could confer with the witnesses? Is that permissible, Mr. Chairman.

CHAIRMAN JENSCH: Well, is it something that is bearing on the interrogation? You can confer with them right here if it will take a moment.

MR. TROSTEN: I just wanted to talk to them briefly. I thought maybe we could take just a two or three-minute recess.

CHAIRMAN JENSCH: It isn't quite our time. Will you proceed.

DR. FORD: With reference to this diagram, I tried to indicate here a phenomenon that we postulate for the sake of this question. It may occur during a loss of coolant accident, namely, that the mechanical damage in the ceramic pellets may be such, with the burn-up of the fuel, that once you form a balloon from local swelling, that the mechanically lucent fuel would migrate within the fuel rod and collect in this balloon.

I'd like to ask a series of questions related to phenomenon.

The first question is, what experimental data has Westinghouse evolved pertaining to the phenomenon of uranium dioxide pellet migration during the course of the loss of coolant accident?

DR. ROLL: We have none.

DR. FORD: You have evolved no experimental data?

DR. ROLL: That's correct.

DR. FORD: Has Westinghouse conducted any analytical consideration of this phenomenon, and does it have any model that would predict the extent to which it would be expected under different transient conditions?

DR. ROLL: No, we do not. In the calculations, we run this phenomenon of pellet chips falling down into the balloon and is not considered.

DR. FORD: Even though you don't have any experimental work or analytical model of your own pertaining to this, is there any data whatsoever, theoretical or experimental, relating to this phenomena that you know of?

DR. ROLL: I believe one piece of information that I am sure you are familiar with, because you quoted from it, is the Oak Ridge document, in which they begin to conjecture that this is what happened and caused otherwise unexplained results.

DR. FORD: If I might refresh your memory, wasn't that in connection with propagated fuel element failures? They were talking about the pellets from that balloon region themselves being ejected. They weren't talking about, as I recall migration and collection.

DR. ROLL: They were talking about what happened to pellet chips.

DR. FORD: But the specific phenomenon of collection in the balloon, that isn't considered?

DR. ROLL: Perhaps.

DR. FORD: In any experimental or theoretical data with which you are familiar; is that correct?

DR. ROLL: That's correct.

DR. FORD: A priori, can you suggest any reasons why if this phenomenon did occur, that the local cladding temperature would not be increased significantly by the presence of this extra material?

MR. MOORE: Under the assumptions that are made for the loss of coolant analysis, the period of time when we get a large temperature increase is associated with the adiabatic heat-up of a transient. The stored energy of the fuel has essentially been removed. We are now operating with a fairly small temperature through the fuel. I would just say, in my opinion, building up some additional fuel in the vicinity of a very localized burst will not have a large contribution.

DR. FORD: You talk simply in terms of stored thermal energy. It is correct that migrating fuel brings with it the decay products that are contributing at the stage of the accident that we are concerned with.

MR. MOORE: That is correct, but that was my point, that the power generation rate of the residual heat is quite low. That's the reason we were discussing the very small temperature.

DR. FORD: I see. But if you increased simply locally the amount of fuel by whatever volumetric percent of the balloon as compared to the non-deformed geometry, I mean just from inspection of the various tests of the fuel rod failures, it's quite clear that, you know, the volume could increase in the area by a factor of four to five. Is that an unfair scanning of just the pictures of the size of these balloons?

MR. MOORE: I think that is an unfair characterization of the increase in volume, but let me further pursue your point.

The thing we have to bear in mind is that not only is the amount of fuel we have there important. The heat transfer capability between that fuel and the cladding is important, and if this fuel is loosely packed in this volume as it would seem to have to be, if it's migrated, and certainly it has to be in some kind of pieces, is then it is not in very good contact at all with the cladding.

So I have difficulty getting heat from the fuel to the cladding to add to the heat of reaction. In fact, we have performed calculations with fuel in the vicinity of a burst rod with the expected load gap coefficients, and you find that the cladding as it heats up due to the exothermic reaction, the zirc-water reaction, that the cladding actually becomes hotter than the fuel and the fuel in that vicinity becomes a heat sink.

DR. FORD: Can you tell me when you refer to it being loosely packed, I suppose we should talk about the density of the fuel. As a function of density do you have data pertaining to -- As a function of fuel density do you have data pertaining to heat transfer characteristics, such that you could in terms of data that you have make calculations relevant to this phenomenon?

MR. MOORE: We have data from which we could attempt to derive an effective heat transfer. The place where we would be unable to quantify the situation would be to actually determine the expected density under this configuration.

DR. FORD: I see. But you haven't done analyses to date which could be gone to in a facile way to relate heat transfer conditions to density of the uranium fuel?

MR. MOORE: Let me clarify it. It's not so much the density of the UO_2 itself; it's the degree to which it is in contact or in close proximity to the cladding. So it's not the conductivity of the UO_2 per se; it's the conductivity between the fuel and the cladding itself.

Of course, the fuel under conditions where you don't have a burst can be in closer contact. If the fuel is going to migrate somehow into this kind of an open volume it clearly is not going to be in close contact with the cladding of that whole region.

CHAIRMAN JENSCH: I wonder if we could get that question answered. We might have it come up again. The question was have you had a study of these heat transfer characteristics and so forth, and you say you do or don't have? You argued something because it wasn't material in your thinking and the question was, that can be answered, do you have any studies.

MR. MOORE: I answered the question there are studies on conductivity of UO_2 as a function of density. I further went on to state that that was not relevant to the question that we are postulating here. Conductivity of UO_2 as a function of density is not a concern.

CHAIRMAN JENSCH: Proceed.

DR. FORD: Can you tell me if we wanted to perform scoping calculations, if we assumed, if we determined failures, what the expanded value is like, say only a factor of two, if we wanted to perform scoping calculations, assuming that it was all densely packed and in contact with cladding, could you do calculations in that way to get some indication of what the maximum conceivable contribution to local cladding temperature could be under conditions which admittedly these heat transfer conditions are admittedly not plausible, a priori?

MR. MOORE: Yes. One could perform scoping studies with arbitrary assumptions.

DR. FORD: Short of actually performing those calculations for us is there any reasonable assurance that the increase in the local heat source of your maximum rod, you have already calculated it at 2300 degrees on the nose, is there any assurance, convincing assurance that you can give us, that this would not be a phenomenon which would push us up over the interim acceptance criteria?

MR. MOORE: The kind of information I would present relative to that would be estimates of the effect of heat transfer between that fuel and the cladding, and it's my opinion that that would end up being such a low value that we would actually have fuel as a heat sink in the vicinity of bursts, and that therefore there would really be no penalty associated with this.

But I would have to perform a calculation with an appropriate gap conductance.

DR. FORD: But you are assuming, as I understand your experimental and analytical position with regard to this problem in terms of the work that you have done and the firm calculations and data you can put your hands on, at the moment the Applicant is not in a position to resolve the doubt as to whether or not this would increase local cladding temperature in such a way that this plant would not meet the interim criteria.

MR. MOORE: I see no real mechanism for getting compaction of extra fuel in those regions and I know of no experimental evidence which would say otherwise.

DR. FORD: Yes. But given the fact that you have no experimental data pertaining to how much compaction would be necessary to cause problems, I mean just given a basic lack of data in consideration of this area, are we to say that at this point there is some doubt of substance as to whether or not this phenomenon would affect the maximum cladding temperature, and your ability to meet the interim criteria?

MR. MOORE: Not at all.

DR. FORD: Thank you.

On the further question which Dr. Roll indicated was involved in mechanical behavior of the fuel pellets, their ability to contribute to fuel rod propagation, do you accept the view of the Oak Ridge researchers in the ORNL-4635 that there was indeed a propagating fuel element failure mode taking place in that transient test and it was related to the ejection of UO_2 pellets?

DR. ROLL: Can we get a copy of the report here?

CHAIRMAN JENSCH: Can you point the witness to a particular section thereof which reflects the statement you have just made?

DR. FORD: I gave away my only copy.

DR. ROLL: You want to get the page number or take this back and we will find one?

CHAIRMAN JENSCH: I think the witness needs the reference in the report that he is asking for, and if you could give it to him, hand it back to him, give him the page number.

MR. ROISMAN: Mr. Chairman, in our statement of questions to the Applicant on October 12, 1971, question No. 34, "To what extent does the Applicant's analysis consider propagation of the failures caused by fragments from burst rods, fuel pellets or other causes?" And we directed their attention to pages 32 and 69 of the Oak Ridge National Lab report 4635.

CHAIRMAN JENSCH: Does the witness have the page of the report?

MR. MOORE: I have it.

CHAIRMAN JENSCH: Can he answer the question from the report?

DR. ROLL: I wonder if I could just -- Is it permissible to reread our answer to the same question yesterday?

DR. FORD: Yes. Well, if that is what you are going to do I am just going to continue further questioning of that. If you'd like to read your answer of yesterday, sure.

CHAIRMAN JENSCH: He doesn't have to read it again for the record. We have had it once.

MR. MOORE: We have answered the question.

MR. TROSTEN: May I interrupt, Mr. Chairman. Question No. 32 is a question that was asked of Mr. Moore yesterday, as I recall it.

CHAIRMAN JENSCH: What page?

MR. ROISMAN: 34.

MR. TROSTEN: I'm sorry. The particular question, what was the number of the question you just read?

MR. ROISMAN: 34.

DR. FORD: 34.

MR. TROSTEN: 34, I am sorry.

CHAIRMAN JENSCH: What page does the witness have before him?

MR. WIESEMANN: 1679.

MR. MOORE: The answer is on page 1679.

MR. TROSTEN: The answer is on page 1679, and what is unclear to me, Mr. Chairman, is when the question was asked of Mr. Moore and Mr. Moore answered the question why the interrogator is now somehow posing the same question to Dr. Roll. Would he please explain to me, or why somebody is asking this question of Dr. Roll? The question has been asked and answered.

DR. FORD: I see. This is in the context of today's discussion.

MR. TROSTEN: I know.

DR. FORD: But --

MR. TROSTEN: But the point is --

DR. FORD: It is the development of the analysis that we have been doing of fuel rod migration.

MR. TROSTEN: I understand that, Mr. Ford.

DR. FORD: Dr. Roll earlier raised the issue of the Oak Ridge test in connection with fuel rod migration. He thought it had to do with filling of the balloon rather than ejection and propagation. So in terms of both continuing the development of this discussion of mechanical behavior of the fuel elements during cladding and in terms of if Dr. Dr. Roll cares to go further than the answer of Dr. Moore, that's acceptable.

MR. TROSTEN: I would suggest that if there is a question that relates to the answer raised by witness Moore, if the question be directed to witness Moore and if witness Moore feels that somehow he is not capable of answering the question he can then defer the answer to someone else.

DR. ROLL: I wonder if I could set the record straight. You asked the question somewhere earlier was I familiar with any data at all having to do with pellet chip movement, et cetera.

DR. FORD: Yes.

DR. ROLL: I responded that the only thing I knew of was the Oak Ridge document having to do with that test. I did not raise the question of this. I did not mean to offer concurrence in the Oak Ridge conclusions or to mean that it was a significant phenomena. You asked a question, was there any data related to movement of pellet chips in the core. My answer was the only thing I was aware of at all was this document. That's all I meant to infer by that answer.

DR. FORD: I am asking you now that since you are by your confirmation aware of this data, I am asking you for your judgment on it as to whether or not the Oak Ridge data demonstrates that propagating fuel element failure modes would take place when rods burst in the LOCA transient.

DR. ROLL: I have not reviewed the Oak Ridge report in detail, nor have I talked with the authors of the Oak Ridge report to know in detail what they did and why they have reached this conclusion. Therefore, I don't think I should offer merely conjecture and opinion based on a previous review of this report, and one on which you are apparently placing great weight.

DR. FORD: I will respect that and if I could direct questions to Mr. Moore pertaining to his answer of yesterday.

You indicated that your analysis doesn't consider propagating failure modes. I was wondering whether you can define for us the problems that could ensue, the significance of propagating fuel element failure modes, in your judgment. You indicated it cryptically here yesterday. I am wondering if you could elaborate on that.

MR. MOORE: Yes.

The point I was making in my answer yesterday is that if one postulates a possible propagation of fuel failures due to this mechanism, then you would get potentially more fuel failures as a result of this.

I indicated in the analysis that we performed and under the conservative assumptions that we make we feel a large fraction of the fuel rods in the core anyway. Even without any consideration of fuel failure propagation. So these rods have in fact already failed and have relieved the internal pressure through bursting.

So that any additional contribution of failures which may under this hypothesized mechanism occur would be small with respect to the total number, which are assumed to occur with design calculation.

DR. FORD: Now you assumed that the rods in the vicinity of the instigator, you presume that they have already failed. Do you presume that they have already failed in the same place, I mean in the same axial levels?

MR. MOORE: They have failed at -- Some rods have failed at the same axial level as determined by the multi-rod burst testing, and this assumption has been incorporated into an analysis which I described yesterday which determined the effect of adjacent failures and rods which would be in contact.

DR. FORD: Isn't the real problem of propagating fuel element failure modes that it would concentrate the failures in one place rather than just letting each rod fail at its own will?

MR. MOORE: That could be a concern if the adjacent rods had not already failed at some other location.

DR. FORD: I see. But in terms of your indication this morning at a given level, axial level, the temperature difference within an assembly is only on the order, I believe you said, of twenty degrees or thirty degrees, doesn't this indicate -- And I presume that because the whole bundle is moved around during core loading and enrichment, by moving around that their internal fuel, that their internal gas pressure is pretty much the same, too. So that we would expect that in terms of the forces acting on all the rods in the bundle, they are all pretty much the same. Is that a reasonable interpretation of the data that you have given us?

MR. MOORE: They are very close within an assembly here.

DR. FORD: I see. So that we could expect if such and such a combination of internal pressure and heating rate, if it was for a given combination of internal pressure and heating rate, we'd associate with this a predicted burst temperature which you do need predict, so that we would predict for this, for any given assembly, or certainly for the assemblies in the middle of the core, that they would all have the rods in the assembly, they would all have pretty must the same predicted burst temperatures, is that correct?

MR. MOORE: Yes.

DR. FORD: So that in terms of the role of propagating fuel elements, that in terms of the dymanics of the situation wouldn't it be significant if they were propagating fuel elements because of the rods in the group everybody was already set to burst, one burst and started each other, the rest of them were already ready to go, this extramechanical effect, wouldn't we expect from this,from the data on temperature distribution and conditions in the rod, thatthis is the problem with propagating fuel element failure modes, that the heat-up has got to a point where it's just waiting for something to happen to cause a real local blockage problem?

MR. MOORE: No. In fact, I would use the same argument in the reverse, to support my contention that it's properly not a problem, and that the rods are very similar and they are very similar then to the multi-rod burst configuration, and rods failed at nearly the same time, and in differently randomly located spots along the rod.

So that I would expect the neighboring rods to have failed about the same time some place else. So any failure propagation due to fuel would not add to the problem in any one location.

DR. FORD: Well, let me ask the big question.

Of course, since we are talking about the mechanical effect of the uranium dioxide pellets as the mechanism for propagating fuel, they are not in the tests that you are talking about. They are electrically heated rods, isn't that correct?

MR. MOORE: I am speaking of the failures that would occur independent of propagation. And you postulated an assumbly where you postulated that all the rods were very close to failure because they were all behaving similarly. This is identical to the situation in the multi-rod burst tests. They are all ready to fail. I haven't failed any yet. I haven't propagated any fuel yet. And then suddenly practically all the rods in that assembly fail at the same time in a random way, and if you were to hypothesize that subsequent to that that some fuel came out it's not going to affect the neighboring rod because it's already failed.

DR. FORD: No. But I think the point is that the main mechanism that we are concerned with, the mechanical effect of the fuel rods, of the release ejected pellet, is it correct to say that that is not simulated at all in these tests which you are referring to?

MR. MOORE: That's correct.

DR. FORD: So that basically these tests because they don't in any way simulate the mechanism for propagating fuel element mode, they can't be used, or can they, to say that there will be no propagating fuel element failure modes?

MR. MOORE: My point is the neighboring rod will already have failed at basically the same time due to its own mechanical failure as exhibited in the multi-rod burst test before I would get into a problem of fuel failure propagation.

DR. FORD: Then let us talk one test that has been done to date, using live fuel rods, with fuel pellets, namely, the Oak Ridge 4635 document. Is it clear from that that this phenomenon as the authors asssert, is it clear from that that indeed this ejected uranium pellet did propagate the fuel element failure modes? You didn't have random failure in the area; you had all local failures.

MR. MOORE: Absolutely not. The report in fact only indicated one single rod which may have had premature failure because of possible fuel ejection from a ruptured rod. One rod may have had a premature failure. The basis for deciding whether it was premature failure was a calculation that said detailed stress time temperature calculations, which said it should have failed a little later than it actually failed. It is my understanding looking at the report that that is the conjecture of the author, is that possibly this rod, this one rod failed prematurely due to the effect you are talking about. This has nothing to do with the fact that all the failures occurred over a small breech in the axial length of the core of a few inches.

As I indicated yesterday, clearly that nonrandonness of failures in that test is associated with the power distribution in the axial direction which tended to concentrate the power over a two-inch region.

DR. FORD: Well, I see now what. You have given us your impression as to what the tests wer and what the author has concluded. Can you substantiate that interpretation in terms of the report itself?

MR. MOORE: No, I cannot. I have cursorily read the report. I have not contacted the authors, but I am merely pointing out the presumption as stated by the authors on how they ascertained that this could have been the case.

CHAIRMAN JENSCH: Is this a convenient place to interrupt your examination?

DR. FORD: Yes.

CHAIRMAN JENSCH: At this time let us recess, reconvene in this room at 3:45.

(Recess.)

CHAIRMAN JENSCH: Please come to order.

Before we proceed, Board has been giving consideration to various schedule possibilities. In view of the uniform expression to move the case along to have an extra day for preparation with these new data which we have been submitted and which are anticipated to be presented, the Board accepts the suggestion of Applicant's Counsel that we recess Thursday night and reconvene Monday morning at nine o'clock.

Proceed.

DR. FORD: I'd like to return to the question of the Oak Ridge 635 tests related to propagated fuel element failing modes. I'd like to read the analysis given by the Oak Ridge authors. This was on the page references which we previously pointed out to the Applicant's witnesses.

It states on page 32, and I quote, "Calculations based on hoop stress show the center rod should have ruptured several seconds before rod H." That is an adjacent rod. "The pellets in the center irradiated rod were cracked into small pieces, and the pieces may have settled inside the rod as the cladding swelled, thus making a somewhat concentrated heat source. The direct effect on rod H was probably the spilling out of UO_2 chips that heated and caused local stress concentrations where the expanding cladding of rod H pressed against UO_2 pieces. This form of fuel rod failure propagation would not occur in out-of-reactor fuel rod failure tests and apparently would not have occurred in the treat experiments with unirradiated rods since the unirradiated pellets remained intact."

Directing Mr. Moore's attention to this analysis, I would like to ask him to identify the aspects of this analysis with which he disagrees. I will give him -- You have your own copy.

MR. MOORE: I have my own copy here.

DR. FORD: To identify the aspects of this analysis with which you disagree and to set forth the experimental data that confirms your alternative hypothesis.

MR. MOORE: As I indicated in testimony prior to the recess, I merely pointed out that results of this test were somewhat conjectural on the part of the author, as indicated by his words, where he says, "The pellets in the center irradiated rod may have settled inside the rod as the cladding swelled. The direct effect on rod H was probably a spilling out of UO_2 chips that heated and caused the stress concentrations."

The whole analysis was predicated upon a calculation of what the stress should have been in this rod H compared to the stress in the center rod, and the fact that rod H failed sooner was because of this conjecture.

DR. FORD: I see. Do you find the use of terms like "probably" in this analysis uncommon and general in experimental interpretation to indicate that this is a kind of footloose conjecture that is not really an intimate part of the heuristic interpretation of research results?

MR. MOORE: I guess I don't understand that question.

DR. FORD: I will repeat it and perhaps clarify it.

Do you contend that the language, the qualifications that were being given is in a style that is clearly typical of experimental reports in general, or don't you agree that one of the main functions of such research is a heuristic one and the evolution of hypotheses, and in terms of collating, assembling the presumptive evidence of fuel evidence failure modes, do you contend that this is something unsatisfactory from a scientific point of view, and for that reason is not worthy of our consideration?

MR. TROSTEN: Mr. Chairman, I would merely like to observe that I really believe that that question is not really a proper question to direct to Mr. Moore. If Mr. Moore wants to answer it, since he is a qualified engineer and technical expert, he may do so. I really don't think the question is a proper one.

MR. MOORE: I would say it is a rather esoteric one. I'd rather not get into it. My point is, the fact that this author has identified a possible situation, I don't find fault with that. I don't think he or we should take this implied situation, inferred situation. There were no direct measurements to show that this in fact was the case. We shouldn't take this isolated single experiment at this point and draw any significant conclusions.

MR. ROISMAN: Mr. Chairman, in terms of Mr. Trosten's suggestion which was never articulated, nor do I think the witness articulated objection at all -- The question was asked by Mr. Ford was perfectly proper. He wants to know whether this kind of language in the usage of the terms as to what was probably the case is so unusual in experiments that it would justify the conclusion that has been drawn from it by Mr. Moore, namely that it showed that we should disregard this propagation effect that the author was talking about, or on the other hand, is the use of the terms that it probably was so, a fairly standard procedure used in experiments; and he can't draw all the support he attempts to draw from that language. He is trying to find out the basis for this judgment that that language supports his conclusion about the possibility of propagating fuel failures. I don't see anything improper about asking him to give us that basis, if he has one.

CHAIRMAN JENSCH: There was no objection. The comment was made by Applicant's Counsel. The witness proceeded to answer. I suppose there are several ways to approach it. If you ran parallel columns on all the experimental reports that come out of Oak Ridge or Idaho Nuclear, you will find a lot of probablies and maybes and sometimes and so forths. Whether you would infer from that that that's kind of standard experimental report language, I don't know. I infer from the witness' answer that he felt there was an initial approach to this problem. In view of the fact that there were some probabilities expressed, he didn't think it was proper. This is his opinion to attribute any great significance to the matter, not that it is something worthy of further consideration. But at the present stage, he doesn't feel that it's reached that point.

Is that your position, Mr. Witness?

MR. MOORE: Yes, sir.

CHAIRMAN JENSCH: Very well. Proceed.

DR. FORD: Can you tell me, given that position, are we to now review Westinghouse reports and to reject all non-directly measured conclusions as to the values of particular variables?

MR. MOORE: I thought you had been doing that all day.

CHAIRMAN JENSCH: Take a shot at the answer and your comment about what he was doing.

DR. FORD: Are you now then conceding one of the basic points that I have been trying to make?

MR. MOORE: No. I am saying each case we have to look at one at a time.

DR. FORD: Back to the specific language that is used. Let's see what clear contradictory evidence that you could offer us as a corrective to the purported speculation that is going on here. Going back to the main section of the report, page 32, I quoted the paragraph as beginning, "Calculations based on hoop stress show that the center rod should have ruptured several seconds before rod H."

Do you have any criticisms of the way in which this report calculated the hoop stress of the center rod such that the inference it draws from the result of that calculation can be invalidated with your evidence?

MR. MOORE: Since the report does not tell me how they calculated this hoop stress, I really can't comment.

DR. FORD: Does Westinghouse have, for the temperature and pressure of this center rod, any calculation which they could supply which would indicate a different result from what you indicate is not a documented calculation? At least it is not documented as I see it on page 32.

MR. MOORE: We have calculated burst conditions as a function of temperature and internal pressure. But not in the sense and to the degree of accuracy that might be implied necessary for this kind of a calculation to draw this conclusion. It depends on for what purpose you are trying to make the calculation. So I would not apply our calculation to this experiment and I certainly cannot do it without any information regarding the actual experiment.

DR. FORD: One thing that is given here very clearly is a description of the heat-up rate, the internal pressure of the rod, rod diameter and so forth. Is there any additional information that you would need to calculate the hoop stress to check out their calculation? They give the entire time temperature of the rod, the cladding, thickness, inside and outside diameter and so forth.

MR. MOORE: I would require uncertainties of these parameters, properties of the specific cladding.

DR. FORD: Is there any other data?

MR. MOORE: I can't recall or think of any right offhand.

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DR. FORD: In the Westinghouse reports I have yet to see, in any table that we have, any indication that all of the uncertainties in any of the parameters that influence hoop stress. Are you asking for kind of an analysis which you yourself do not practice?

MR. MOORE: When I do an analysis within Westinghouse, it is not incumbent to indicate all the uncertainties in any particular report.

DR. FORD: You contend that in order to calculate hoop stress, you would need certain variables and statistical information pertaining to these variables, whether probability is at certain deviations. I am saying, in all of the data that you have presented, I am asking, is my impression correct that you have in no case included all of the statistical uncertainty information that you require now to do any calculation of hoop stress?

MR. MOORE: And in no case do I do a hoop stress calculation in support of the application for the Indian Point plant.

DR. FORD: I see. In none of the topical reports that predict time to burst is there any hoop stress calculation?

MR. MOORE: Predictions of time to burst are not significant or important with respect to our analysis.

DR. FORD: I see. Now, as part of your--

CHAIRMAN JENSCH: I wonder if we could get that answer again. I think you asked him are there any of these analyses of hoop stress or something about it. Could you try it again? Do you have any analyses of hoop stress, yes or no?

MR. MOORE: Yes.

CHAIRMAN JENSCH: Proceed.

DR. FORD: Is the hoop stress analysis and the per cent of rods perforating, is this used to calculate the release of fission products in the containment or the accident?

MR. MOORE: Not at all.

DR. FORD: Do you mean that fission product release into the containment is not a function of the hoop stress that would take place?

MR. MOORE: It is a function of that, but we assume that all of the rods failed when we determined the releases as indicated in testimony yesterday by Mr. Wiesemann.

DR. FORD: Can you tell me, in the time to burst data in which you do present, because you don't present the uncertainties in the hoop stress parameters, that we should dismiss them or regard them as providing no relevant information at all?

MR. MOORE: I really can't comment on that.

DR. FORD: You seem to consider acceptable practice in this area because it is your practice to present results of hoop stress calculations and burst predictions. I find no footnote saying to forget this column, it is irrelevant, an irresponsible way of doing things.

MR. MOORE: These calculations that you are referring to that we may perform were not intended to determine the kind of effect this presented in the specific report periods. So you must understand for what purpose you are making these calculations in order to determine with what accuracy it is important that they remain.

DR. FORD: I see. Now, if you were given data pertaining to the accuracy of a specific parameter, is your experimental data sufficient to provide the basis for sensitivity analysis; that the range of possible values of this parameter would result in ranges and possible values of hoop stress? Are you prepared to do a sensitivity analysis even if you have the uncertain data?

MR. MOORE: For what case? I am confused.

DR. FORD: For calculating the hoop stress on the center rod and as a test.

MR. MOORE: What is the question?

DR. FORD: You want uncertainty data for the parameters and hoop stress calculations. What I am asking you is, when you get these uncertainty parameters, what are you going to do with them? Are you prepared and do you have a sufficient base of experimental data relating these parameters to hoop stress that you can perform some kind of sensitivity analysis?

MR. MOORE: Experimental data with respect to what?

DR. FORD: With respect to the determinates of hoop stress. Could you say, for example, if I told you that my estimate of cladding thickness was accurate to within one per cent, can you go from this uncertainty estimate to tell me what difference this would make to calculate at hoop stress?

MR. MOORE: I would think so, yes.

DR. FORD: What experimental data have you at all on that particular sensitivity analysis involving hoop stress at bursting to this parameter?

MR. MOORE: Clad thickness, was that the parameter?

DR. FORD: Yes.

MR. MOORE: It is the fact that the stress is proportional to the area. That's a fairly fundamental parameter.

DR. FORD: For all of the uncertainty parameters that you are concerned with, are you prepared to do this kind of analysis?

MR. MOORE: No, I am not. I don't understand quite where the questioning is going. I indicated that I could not comment on the calculations performed by Oak Ridge, but obviously they are important in developing their hypothesis.

DR. FORD: But what I'm trying to obtain is some indication from you of whether or not the calculations that were made were reasonable ones and whether you are able, with your analysis of the situation, to provide us with information that says that is clearly wrong and therefore this whole concern with propagating fuel element failure should be forgotten about completely.

MR. MOORE: I have no basis to make any judgment on that at all.

CHAIRMAN JENSCH: I wonder, Mr. Wiesemann, if you find it crowded there, if you want to sit back at the table with Mr. Cahill, and if some concern comes up for your interrogation, you will be able to come forward. Will you be able to do that? I think sometimes we get into a conference and it may interfere with a witness in answering the question. I think it might be better if Mr. Wiesemann sat down at the table and then the conferences won't be so interfering for the answer.

MR. TROSTEN: It is agreeable to me, Mr. Chairman.

CHAIRMAN JENSCH: Will you step down with Mr. Cahill, Mr. Wiesemann.

Proceed with your interrogation, please.

DR. FORD: Could you tell me, if I had the general question as a topic for safety water reactor research, if I had the general question, what is the possibility that propagating fuel element failure modes would significantly degrade emergency core cooling system performance? If I had that general question, could you describe to me the kind of experimental data that would be sufficient to resolve that matter in your judgment?

MR. MOORE: I certainly would have to have information as to how the rods were expected to fail without the propagation factor. That is what are the failure configurations.

DR. FORD: What specific data do you require on this, about using which variables?

MR. MOORE: That determines the geometry of the rod burst and gives me an indication of what possibility I have for expulsion of fuel or collection of fuel.

DR. FORD: What variables are these that are relevant to this consideration?

MR. MOORE: Geometry of the failure.

DR. FORD: There are no other variables relevant to determining what failure rate is without propagation? You don't want to know the relationships, for example, between internal pressure and the phenomenon of swelling and so forth?

MR. MOORE: Are we starting from the point that I would have to understand how the rod would fail from internal pressure initially in order to see wherher the geometries of the failure would be such that I could get gross dispersal of fuel. For example, if I had large gross splits in a rod with large areas exposed, this would be one situation. The actual situation obtained in the multi-rod burst test shows very small openings associated with the burst.

DR. FORD: I am talking in terms of how you would set up an experimental research program to determine whether fuel rod propagations was a phenomenon that would degrade emergency core cooling system performance significantly or not.

In terms of your response, I don't get anything that you should do these kinds of tests with these parameters with such and such a kind of rod, and so forth. That is the kind of answer I am looking for and that which would be responsive to that interest.

MR. MOORE: You would have to propose some kind of a test which will simulate the behavior of UO2. Under the circumstances which simulate the loss of coolant accident, that is. If there were a way to do this out of pile, it would certainly be preferrable from the standpoint of the experimental aspects. Short of that, you will do in-pile tests such as the one Oak Ridge has performed.

DR. FORD: Was there a kind of in-pile testing do you think would contribute to a resolution of the uncertainties you find in the Oak Ridge office's analysis of the data?

MR. MOORE: Well certainly some more tests would be in order. In fact, they had run a subsequent test FRS 2 and I am not sure whether they had any of this situation in that test or not.

DR. FORD: Well, are you aware that the maximum blockage in FRS 2 was ninety-one percent as opposed to the forty-eight percent in the report you have in hand?

MR. MOORE: No, I am not. Is that reported somewhere?

DR. FORD: Yes. I can give you the reference to that.

MR. MOORE: I'd appreciate it.

DR. FORD: Tell me, the further testing you pinpointed specific speculations in the Oak Ridge authors' analysis of propagating fuel element failure modes. Addressing yourself --

MR. MOORE: Excuse me. You pinpointed specific speculations.

DR. FORD: I mean in terms of your quoting of their use of the term and so forth, you found those sentences to be significantly speculative. Addressing yourself to those sentences which you were quoting earlier, I believe on page 69 of the Oak Ridge report, can you talk turkey about an experimental program that would resolve that speculation either in favor of the position that you are taking or in further support of the significance of propagating fuel element failures?

MR. MOORE: I have no specific test program to propose at this time. I note that the failure of propagation was not one of the conclusions of the authors of this report.

DR. FORD: It is not included in the abstract. Isn't there a whole separate section?

MR. MOORE: I am looking at the section entitled Conclusions.

DR. FORD: So that to sum up your view of the Oak Ridge test, is it correct to say that in your opinion the role of propagating fuel element failure modes is not definitely established in that test, and on the other hand that you can suggest no route to obtain such information?

MR. MOORE: Yes. I am not in a position to suggest any alternative test.

DR. FORD: Now in terms of the *a priori* analysis that the authors made, the logical analysis about the significance of propagating fuel element failure modes, do you accept that? I mean logically speaking even if you don't find any basis for the calculation of hoop stress, do you criticize the logic by which they use the calculated hoop stress to relate it to the other data from the experiment and conclude that there was therefore a propagated fuel element failure mode?

MR. MOORE: I am not sure I understood the question.

DR. FORD: Well, they have made certain factual contentions, for example, that the hoop stress is reliably calculated as a certain value. They have used that calculated value to relate it to other data at times of failure and so forth to conclude that there was a propagating fuel element failure mode.

What I am asking you is from a logical point of view is there anything suspect about their reasoning? You mean assume in answering your position that the premise is false. But is the argument false itself? Simply from a logical point of view if the premise were true the conclusion would be true?

MR. MOORE: I guess I understand the question. I would say that given data which seems to refute your prediction one then searches for possible alternative situations or mechanisms or what possible alternative mechanism is fuel failure propagation.

So I guess I would agree that there could be some logic.

DR. FORD: I see. Well, could you suggest to the Oak Ridge authors or to us other hypotheses that they could consider that would explain what they calculate but which would not be the hypothesis that there was a propagated fuel element failure mode?

MR. MOORE: I already have. I said that there is a possibility that their stress calculations could be sufficiently in error or uncertain, that they had drawn the wrong conclusion.

DR. FORD: But aside from their calculation all errors, I mean simply from a logical point of view interpreting the data that they have, can you disatrophy in our imaginations to show what else besides propagating fuel element failure modes could have been responsible for that behavior?

MR. MOORE: I am not sure I understand disatrophy. You would not need to come up with any other solution or postulation if your stress calculations showed you that the rod in fact did not fail prematurely. So the data -- I look at the results of the experiment and I wouldn't conclude anything other than the fact that the total blockage was on the order of forty-eight percent.

DR. FORD: I see. But if we accepted, you know, just from in terms of trying to see what the possibilities are for interpreting this data, if we accept it, the calculations that we are talking about, if we accepted them then is there any other phenomenon besides heat propagating fuel element failure modes that could contribute, that would have, you know, degenerated the situation?

MR. MOORE: Well one possibility certainly is that a rod burst and just the cladding itself contacted an adjacent rod and caused the adjacent rod to burst.

DR. FORD: That's another kind of propagating fuel element failure mode, not via the fuel pellets themselves, but via cladding impingement.

MR. MOORE: Yes.

DR. FORD: Are there any others?

MR. MOORE: I can't think of any others.

DR. FORD: So that in terms of a null hypothesis other than just calculational errors you can't provide a plausible alternative to their interpretation simply on logical grounds?

MR. MOORE: I just did.

DR. FORD: Yes. But that's another kind of -- I mean they do mention that and the pressure with which the cladding itself, the one in which the cladding itself, the center rod had touched all other rods, as they explained, with sufficient pressure to have some influence on the swelling of the other rods, so that is part of the --

MR. MOORE: Could you give me the reference to that?

DR. FORD: I think we will go on. I had just read that during the break, but we will inform you and bring it up.

In terms of a further contributing factor to local cladding blockage, this is a phenomenon which both relates to metal-water reactions -- This is specifically to Dr. Roll -- namely the presence of grid spacers and springs and supporting structures and the influence that they have on fuel element failures. I wonder if you could just get us in this area, whether you could tell us what is the number of supports on the bundles, what their spacing is and what the metals involved are.

DR. ROLL: In this particular --

MR. TROSTEN: Excuse me, Dr. Roll. I believe you were not here yesterday but I am informed that this question was asked and answered yesterday. We are searching to get the transcript reference.

MR. ROISMAN: Mr. Wiesemann was kind enough to show me the modes of contact and so forth, all off the record. I think what we wanted to do was just get it on the record. Let me state what Mr. Wiesemann told me, if that is correct, and we will simply go from that premise.

I understood the spring supports are at seven different locations along the length of the rod, that at each location there are six contact points, four springs, two of which have two contact points and two of which have one contact point. So that there is a total of forty-two contact points on each rod of the iconel spring with the rod. Mr. Wiesemann, is that correct?

MR. WIESEMANN: I think also you were referred to the safety analysis report for a description of the location of those spacers.

MR. TROSTEN: That is correct. I don't have the transcript reference here. I'm sorry, Mr. Chairman. There is a page in the transcript where the reference to the FSAR is contained. I don't have it before me.

CHAIRMAN JENSCH: While we have paused a moment, Applicant's Counsel, you stated that witness Roll had some commitments that required his presence when back at the office?

MR. TROSTEN: Excuse me. I will have to confer with the Westinghouse representative to find out exactly how soon he has to be back. Will you wait just a moment, please.

DR. FORD: While the Applicant is discussing that matter I might report that we found the reference in the ORNL document that I promised earlier.

CHAIRMAN JENSCH: Just a minute.

MR. TROSTEN: Mr. Roll is preparing for work on an entirely unrelated matter, Mr. Chairman, which would make it highly desirable if his examination could be completed today.

On the other hand, if it is necessary that he be here tomorrow he could be here tomorrow. We would just hope that his testimony could be completed today.

CHAIRMAN JENSCH: Can you indicate if you would be able to when you will make complete your interrogation of the witness Roll.

DR. FORD: I didn't realize it was so late. I am sure that we will require him tomorrow. I can't go at a great pace. I will try.

CHAIRMAN JENSCH: You will require him tomorrow?

DR. FORD: I would expect as much.

MR. ROISMAN: Yes. This is on the assumption that we are going to recess the hearing today at the usual time.

CHAIRMAN JENSCH: Five o'clock.

MR. TROSTEN: Would it be feasible to extend it for a non unreasonable time to complete the examination of the witness?

DR. FORD: The questions I have, the further questions on metal-water reactions I think, are a good bit simpler than the more complicated interpretations of experimental results that we have been concerned with much of the day. So we can decide. I can have some indication from my own idea what I am doing at five o'clock, so I can indicate how much longer I'd require him.

MR. TROSTEN: As far as tomorrow is concerned, if we could have some assurance that he could leave by noon this is really what we are after.

DR. FORD: Oh, I would definitely expect that that would be possible.

CHAIRMAN JENSCH: Let's proceed upon that basis. We will go to five o'clock today and try to release Dr. Roll by noon tomorrow. Proceed.

DR. FORD: Do you know what the total mass is of iconel in the reactor in the bundle?

DR. ROLL: It is known. I don't have the number on the top of my head.

DR. FORD: I see. Have you performed calculations to put pounds on the total amount of heat that could be generated if the liquid metal eutectic iconel and zircalloy were formed and reacted with unlimited supplies of water during the transient? Have you performed that kind of calculation?

DR. ROLL: We have not performed a calculation to determine the heat given off or absorbed in the event of total iconel reaction with zircalloy.

DR. FORD: I see. Do you --

DR. ROLL: It's not clear that it's an exothermic reaction.

DR. FORD: Do you have data pertaining to this kind of reaction that clarifies whether it releases or absorbs heat?

DR. ROLL: I am not prepared right now to quote a reference with the data.

DR. FORD: I see. Are you familiar with the BWR-FLECHT report by the Idaho Nuclear Corporation, IN-1453, called A Metallurgical Evaluation of the Simulated BWR Fuel Bundle?

DR. ROLL: Not in detail.

DR. FORD: The general facts of that test, namely that this unexpected reaction between the springs and the cladding produced a liquid metal eutectic that melted at 1760 degrees, and that liquid metal reacted with water and caused local cladding temperature spikes of up to 2940 degrees, at which point the thermocouples, relevant thermocouples failed.

Are you familiar with that general data as being presented by this Idaho Nuclear test?

DR. ROLL: Very generally.

DR. FORD: Do you have any very general basis for extracting that there is any misanalysis there of this eutectic formation or of its responsibility for the temperature spikes and gross cladding damage?

T2Bt1

DR. ROLL: We have not done a detailed analysis of that report and hence cannot comment on its validity or the validity of the conclusions. Our reasons for rejecting this as a significant contributor to the total energy release in the accident is in fact our own test which shows very little extent of zircalloy iconel reaction.

DR. FORD: I see. Can you give us, set forth the test that you have run in this area?

DR. ROLL: These are once again summarized in 7379, Volume 1. There were eight tests run. Four tests were run by placing iconel springs in direct contact with zirconium cladding and running this test specimen up to temperatures ranging from 1800 to 2390 degrees Fahrenheit. Times were five minutes for three of the tests, three minutes for the test at the 2390. The other four tests were run with pressurized rod specimens in which we wired small pieces of iconel onto the side of the rod and then ran it through our single rod burst procedures at, in two cases, pressures of 100 psi and five degrees F. per second. And in the other two cases 200 psi, the same heating rate.

The burst temperatures ranged from 1750 to 2115 degrees F. The results of these tests are as follows.

The four tests which were run up to high temperatures and held showed very small evidence of any bimetallic reaction. The spring pieces, iconel pieces, did not disintegrate or contort or lose their basic configuration, there was no evidence of any penetration into the tubing wall, and hence the degree of iconel formation was extremely slight, and then I believe all but the one or two higher temperature tests, and those two high temperature tests it was noticeable. In the lower temperature tests it was not. And the four tests that are run on pressurized rods they were intentionally run-off at a low heating rate and a low internal pressure to give as high a time as practical, as possible, in the range of above 1760, and the rods burst essentially as epxected in terms of a qualitative perusal of the configuration of the break and the time of the break and the location of the break. They did not break underneath the iconel springs.

And based on these tests we reached the conclusion that, number one, the presence of the iconel did not prejudice the location of the burst; and, number two, the extent of iconel and zircalloy reaction is indeed very, very slight.

DR. FORD: Now iconel is, as I understand it, an alloy of chromium and nickel.

DR. ROLL: Nickel primarily.

DR. FORD: Is the iconel that you used in your tests the same alloy that is in the reactor?

DR. ROLL: That's correct.

DR. FORD: You don't have any date on whether it's

T2Bt3

the same alloy used in the BWR FLECHT tests?

MR. MOORE: I do not personally know if it's the same alloy.

DR. FORD: Now can you explain to us what the theory is that governs the eutectic melting formation in terms of the fact that the different crystalline structures of different forms of an alloy have the effect that you may get of melting of the entire alloy at lower temperatures than melting of any of the components? Can you enlighten us on the theory behind this kind of reaction, behind eutectic melting?

DR. ROLL: No, I cannot. That's not really my area of competence.

DR. FORD: I see. Has Westinghouse, to your knowledge, research personnel for whom this is the area of competence?

DR. ROLL: I believe the area of why eutectics form and what the phase diagrams look like and why eutectic is formed is basically metallurgy.

DR. FORD: Well, I will translate my question. Does Westinghouse have a metallurgist?

DR. ROLL: Of course we have a metallurgist.

DR. FORD: Then in terms of this particular metal-water reaction you are not--

DR. ROLL: You are talking eutectic formation.

DR. FORD: Right. And the metal-water reaction

T2 Bt4

between this liquid metal eutectic which melts at 1760 degrees Fahrenheit, according to the General Electric data, in terms of the reaction between this and water or metal-water reaction, that's why the whole area is addressed to you, but are you saying that it's mainly a metallurgical phenomenon, that you are not the cognizant person to discuss this matter with?

DR. ROLL: The formation of the eutectic per se given intimate contact and zirconium at 1760 degrees will happen. That's a property of the material combination. Why it didn't happen in this particular test, the source of why it didn't happen in our particular test is twofold.

Number one, we had a point contact. That is in our design the spring is in contact with the rod at only a very small total area.

And, number two, is during the heat-up of the bimetallic test specimen and also as will happen in the reactor there is a coating of zirconium oxide on the surface of the rod and this would effectively separate the bare zirconium metal and the bare nickel metal from each other and preclude, for the time of interest at least, the formation of more than a very, almost insignificant quantity of eutectic.

DR. FORD: I see. Now you mentioned that there is only one point of contact.

TBt5

DR. ROLL: I didn't say that.

DR. FORD: Oh, excuse me. What was your point?

DR. ROLL: I said the point of contactbetween the spring and the rod is very small.

DR. FORD: I see. But for a given rod how many contacts are there that are small?

DR. ROLL: Well, it's six points per grid per rod and there are seven grids along the rod and I believe there are nine grids total on the two on the end, so that nine times six would be fifty-four points of contact per rod. We can carry that on to times approximately 40,000 rods to get the total number of points of contact in the reactor.

DR. FORD: The question is concerning the explanation for eutectic formation as related to the experiments that you performed on eutectic formation and what I'd like to ask is whether or not you have sufficient confidence of the mechanisms of eutectic formation to say, to give us a qualified opinion as to whether or not this particular test was done in cognizance with the whole theory of the eutectic formation, such that it really gave eutectic formation a fair trial?

DR. ROLL: We believe, and this was not a unilateral decision on my part, but we did consult with the metallurgical group at the time, we believe that the test is an adequate demonstration of our set of conditions with our geometries.

TBt6

DR. FORD: I see. But this is not something which you are presonally testifying to, that you have reviewed the relevant data pertaining to eutectic formation and therefore can say that the tests that were conducted gave sophisticated consideration to this relevant area of eutectic formation, therefore constituted an adequate test. This isn't your own personal affirmation as an expert witness, is it?

DR. ROLL: The tests that were conducted gave adequate demonstration for our conditions. The objective was not to learn all there is to know about the formation of eutectics.

DR. FORD: But in terms of your own conditions are you sure that there is no phenomenon that would be expected to occur in loss of coolant accident or no interaction between this phenomenon which would encourage eutectic formation, but which was not considered in your test?

DR. ROLL: I can think of none now with your conditions or relevance which we are not adequately simulating in these tests.

DR. FORD: But I'm talking specifically in terms of the fact that you seem to be saying that the basic theory as to why eutectics form and so forth is not something which you will testify to as an expert witness.

DR. ROLL: That's correct.

DR. FORD: So that if I asked you for example to

TBt7

set forth all of the conditions postulated for a loss of coolant accident that influence eutectic formation you would decline to answer that question.

DR. ROLL: I would decline to answer that question at this very minute.

DR. FORD: So that if I follow that question by asking you to explain to me which of those conditions which should be considered were considered in the test that you could not give an expert answer on that, is that correct?

DR. ROLL: No, I don't think so. I think you are asking am I prepared to go into a dissertation on eutectic formation and then obtain as a result of this a list of all the variables which might affect the presence or absence of a eutectic and I'm prepared to say that at this time. But you are asking by inference have we missed something of major importance in the test which we ran which had been presented in the reference document. My answer to that is I believe not.

DR. FORD: I think my concern here is simply to find out why in one of a recent series of important tests we got eutectic formation which contributed in a very significant way to local cladding damage, and why in another test we didn't get it, and I'm trying to find what is the depth of the analysis that you are prepared to offer to resolve this kind of basic question. And it's in this regard that--well, do you regard the General Electric test result which showed the temperature spikes due to eutectics, is that an anomaly or is your test result that didn't include eutectic melting an anomaly?

DR. ROLL: We believe that our tests which did not show eutectic melting is not an anomaly I believe that it's perhaps imprudent of us to conjecture on the details of the GE test, and how one might interpret the results in light of our own set of conditions.

DR. FORD: You are called upon to support your position that your result is not an anomaly am I to take it from your previous remarks that you are not able to argue in terms of the theory of eutectic formation and the conditions which influence it, that you are not able to argue on that basis to support your position?

DR. ROLL: That's correct.

DR. FORD: So that basically your test results pertaining to the lack of eutectic melting, you can offer no theoretical support for that?

DR. ROLL: I can offer the fact that we didn't see any eutectic formation. That's not theoretical, that's data.

DR. ROLL: Right. I am asking the further question as to whether we should regard that data as exclusive in light of the theory that we have developed, if we have developed any, related to eutecticformation, and do you think

TBt9

you can be responsive to this concern?

DR. ROLL: I believe we have conducted eight tests which adequately cover the conditions of concern. The tests which have been summarized in the document and presented for that purpose.

DR. FORD: Can you give a definition of the term "adequately covered"?

DR. ROLL: We have answered to our satisfaction the concern that has been raised and our satisfaction as measured by our willingness to put the data and the interpretation of this data in a document, which is generally available.

DR. FORD: When you used the term "our," you are not relying on your own judgment and analysis? You are relying on the judgment and analysis of somebody back in the shop, that they did this test right and there is no question at all about it?

DR. ROLL: And that others that are using the results of this test feel that this provides adequate demonstration of the condition of concern.

DR. FORD: Can you tell me who else is using this test?

DR. ROLL: Westinghouse Electric is using this test.

DR. FORD: They are the ones that developed it?

DR. ROLL: That's right.

DR. FORD: Is there someone else? Is your test result something relied upon by people who use eutectics in general?

DR. ROLL: I'm not a student of loss of coolant analyses in literature. Therefore, I can't comment on whether somebody else is using this or not.

DR. FORD: So basically you are saying that you yourself are not prepared as an expert witness to affirm this test as a valid one? You are simply relying on the fact that Westinghouse did it?

DR. ROLL: What are you inferring? Maybe you are stating something that I am totally missing.

DR. FORD: I am trying to find out whether this is a valid test. You say it is. But when asked to give in terms of what we know about this metallurgical phenomenon in terms that it is a metallurgical question and outside of your sphere of technology -- At any rate, you are saying that you don't have the relevant background and experience. You are still affirming the test. I am trying to figure out how this is at all reasonable.

MR. TROSTEN: Mr. Chairman, I suggest what is happening here is that Mr. Ford has asked the question. He has actually asked it several times in several different ways. The witness has answered the question several times in several different ways. Mr. Ford doesn't like the answer that the witness has given and is in a sense arguing with the witness. I think he is going on like this. I would request that you order that it cease at this point.

CHAIRMAN JENSCH: I take it your statement is an objection?

MR. TROSTEN: Yes, sir.

CHAIRMAN JENSCH: I think there is a great deal of repetition in the questioning. I think the witness has indicated the scope of his familiarity. He doesn't know the work in detail. Maybe you are leading up to a request that some other witness be presented. But I think it should be as far as this witness is concerned concerning those tests that have been pretty adequately covered. Do you not think so?

DR. FORD: I was just about to conclude.

CHAIRMAN JENSCH: We will sustain the objection and make it certain.

DR. FORD: Additional metal-water reaction that we are concerned with is the metal-water reaction between uranium dioxide fuel and the steam once the fuel rod has burst. Can you set forth the experimental work, theoretical work that Westinghouse has done in this area?

DR. ROLL: Did you say did I or could I?

DR. FORD: Would you please.

DR. ROLL: I believe I answered earlier that the reaction of UO_2 and water to a higher oxide of UO_2 is thermodynamically not what happens at these temperatures, and that we have done no experimental work to confirm the rate, the nature, the extent of this reaction or lack of reaction.

DR. FORD: So that you would contest the

conclusion of the Idaho Nuclear metallurgical analysts -- I am quoting from IN 1453; that another possibility is the two perforations resulting from eutectic melting caused by the lantern springs or resulting from other causes would allow steam to come in contact with the uranium dioxide. The uranium dioxide may be converted to U_3O_8 which would thermodynamically be extected to react with the zircalloy. These possibilities bear further investigation and further work.

You disagree with their analysis of the possibilities for this conversion; is that correct?

CHAIRMAN JENSCH: Hand the document to the witness.

DR. FORD: Yes.

DR. ROLL: The Idaho people from which you have quoted this statement presumably are saying, gee, there are several materials here which are in contact under certain conditions and which one might be concerned about the degree of heat release or heat evolution in the event they react. That, as a flat statement, I can agree with. However, one might also want to take the next step and review the thermodynamics of some of these reactions without a significant amount of work, and say, number one, has work been done; if not been done explicitly, what are the indications on the extent or rate or heat evolution in the event that these reactions do take place; and reach some early indications of what ought to be done. In developing our calculational procedures which are loss of coolant accident and analysis in toto, these kinds of reactions had been reviewed as potentials and rejected as possible or significant or even notable contributors to the total heat load which the analysis must carry.

U2Wt1

DR. FORD: Can you provide us with the analysis that was made when this review was made? Did you personally make it?

DR. ROLL: I did not.

DR. FORD: Are you familiar with what the thermodynamic argument was?

DR. ROLL: The thermodynamic argument based on free energies of formation of the compounds and thermodynamics will tell you whether or not a reaction will go or not under the conditions of interest. It won't tell you that it definitely will, but it can tell you that it definitely won't or will to a slight extent. It is this type of an approach that was used.

DR. FORD: What I am asking you for is whether you are familiar with the actual instance of this approach that was used?

DR. ROLL: No, I'm not. This was the approach that was used and this was the approach that is used in reporting the data. Do we have in hand a document that can be brought out that summarizes this calculation and process, no. I wouldn't know where to go to find such a document.

DR. FORD: You know that some theoretical analysis was done, although it is not in any form accessible to inspection, and you also--I believe you said before that there was no experimental work done to confirm this?

U2Wt2

DR. ROLL: I said that we have no experimental work.

DR. FORD: You have no experimental work?

DR. ROLL: No. I believe that in some cases there are some experiments which could be partly if not totally germane to the specific conditions.

DR. FORD: In terms of the seriousness with which the Idaho Nuclear people take this matter--I looked to see if it says this will happen. I don't find any. Isn't it correct that in this test, what they were responding to is an anomaly occurring in a very late stage, in our experience with nuclear power, that they are responding to metallurgical performance anomalies that were not anticipated at all. Is that the context in which you regard them as having made this suggestion for further work?

DR. ROLL: Perhaps.

DR. FORD: In terms of anomalous metal-water reaction that has been noted in the FLECHT test--for example, the alumina reaction--I presume you are familiar with that?

DR. ROLL: Yes, sir.

DR. FORD: Prior to the actual observation of this reactor in the FLECHT test, have any analyses that you or Westinghouse had performed, basic general thermodynamic considerations, and so forth, had that predicted this kind of reaction?

DR. ROLL: Again, I'm unaware of such calculations.

DR. FORD: I see. So that the reaction which took place between the alumina, which you use as a filter, and the zircalloy itself, that that was something that was not thought of before and not anticipated, obviously, because it wouldn't have set up such a funny refractory if they expected to use something like this; is that correct?

DR. ROLL: Wait one moment. In what context were these alumina zirconium reactors?

DR. FORD: The alumina was used as filter rods as they were in your own tests. You had the hollow piece of zircalloy tubing with a molybdenum filament, and that in order to hold the filament in place they packed alumina, AL_2O_3, into the void in the tube, and what they observed was that in temperatures in the order of 2100, 2200 degrees, that the alumina filter reacted with the zircalloy cladding which melted and reacted with water which caused extensive local cladding damage. That's in answer to your question to me. That's the frame in reference in which I am talking about alumina.

I believe it is correct that you used, in your rods involving filaments, nichrome.

DR. ROLL: I wonder if I could quote for the record--

CHAIRMAN JENSCH: Let him finish.

DR. FORD: I am correct that in these tests or in all of the tests that you use these filaments, heating

U2 Wt4

filaments, inside the rod, you do use alumina? That's not correct?

DR. ROLL: That's not correct.

DR. FORD: Do you use an alternative filter?

DR. ROLL: Could I quote from the final report on the FLECHT tests? It is WCAP 7665, Page A-3. This may be responsive to your question.

"Boron nitride was selected as the insulation material for FLECHT heaters because it has a substantially higher thermo conductivity than alternate materials such as alumina or magnesia. Thus, for a given clad temperature, the resistance element temperature is lower with Boron mitride than with the other materials which were considered. In addition, concern about zircalloy--ceramic reactions as a possible cause of Group III heater failure at elevated temperature was a factor in the preference for Boron nitride. Subsequent BWR FLECHT experience was shown that reactions between zircalloy and alumina can take place at temperatures as low as 1500 degrees F. and can result in heater rod failure."

DR. FORD: Let me repeat an earlier question. You have used alumina in some of the tests.

DR. ROLL: Perhaps, but with stainless steel rods. The concern was for the Group III rods in that quote.

DR. FORD: So you used no alumina with zircalloy rods; is that correct?

DR. ROLL: That apparently is correct. That's what it says here.

DR. FORD: This is WCAP-7379, Volume II. It says on page 2, "Twenty tests were performed of these irradiated tubes. These are tubes from Yankee Row E." It says, and I quote with reference to the Yankee Row E fuel, "Twenty tests were performed on these irradiated tubes. Sixteen were tested for failure at various internal, pressure and heating rate conditions (these tests were run with Al_2O_3 pellets alumina) --"

So that you do, in fact, use alumina contrary to your statement that --

DR. ROLL: Not in contrary to the statement. Those weren't FLECHT heater rod tests. Those were burst tests.

DR. FORD: My question was not with regard to FLECHT. I simply wanted to know, tests that you conducted with electric filaments, did you use alumina?

DR. ROLL: The answer is no. I believe those tests were not filament heated.

DR. FORD: It is parenthetical. Filament heated or not, you nevertheless in these tests used alumina?

DR. ROLL: As simulated UO_2 pellets inside the rods.

DR. FORD: One of the things in the longer statement you read, you footnote the Idaho Nuclear metallurgical evaluation pertaining to alumina. What I'd like you to explain about that is the quotation here says that zircalloy and alumina -- That reactions between zircalloy and alumina can take place at temperatures as low as 1500 degrees Fahrenheit and can result in heater rod failure. This was not known to you before these BWR-FLECHT tests; is that correct?

DR. ROLL: On the contrary. The development work certainly took place earlier than February of 1971, if the final report is documented in April of '71.

DR. FORD: Let me understand. Before the anomalous reaction occurred in the BWR-FLECHT tests, had you been aware that this type of reaction between alumina and the cladding could take place at temperatures of 1500 degrees?

DR. ROLL: I believe that statement is made in the paragraph which we just quoted.

CHAIRMAN JENSCH: Did you understand that it occurred?

DR. ROLL: Yes. The concern about that reaction was the reason for not selecting -- Was the reason for selecting boron nitride for our own heater rods for the zircalloy class.

DR. FORD: I am asking you before the reaction was observed in the Idaho test, was this something that you had previously expected?

DR. ROLL: Yes.

DR. FORD: There seems to me to be some great difference in your understanding of alumina's potential reactions and the Idaho people. Isn't it clear in the statements from Idaho that this is completely unexpected reaction, that they state in their conclusions?

DR. ROLL: To answer that question, we would have to go into the context of all the reports that are being referenced here.

DR. FORD: You obviously take it into account after it happened.

DR. ROLL: No, not obviously. Obviously it was taken into account before it happened, because it was taken into account during the heater rod development for the FLECHT zirc clad heaters.

DR. FORD: In your taking account of this, the suggestion that the Idaho people have made subsequent to its occurrence, was that the time at which it was reviewed, or was it decided at some earlier point that uranium transformation to U_3O_8 reaction of the cladding, something that you didn't have to worry about? When did this review take place relative to the BWR-FLECHT results?

DR. ROLL: :I'm not completely up to date on all the chronology in our FLECHT tests, the Idaho reviews and the BWR-FLECHT tests. However, the statement that is made here is that the subsequent BWR experience confirmed this concern.

DR. FORD: My question really pertained to your analysis of the uranium reaction. What I am trying to ascertain is when was the analysis that you rely upon done with regard to uranium transformation to U_3O_8?

DR. ROLL: I don't know.

DR. FORD: So that in terms of its up-to-dateness and review in terms of these matters, is it a matter of months or years or whenever this was decided?

DR. ROLL: It is not a matter of months.

CHAIRMAN JENSCH: Is this a convenient time for you to interrupt your examination?

DR. FORD: I think so.

CHAIRMAN JENSCH: Is there anything before we recess?

MR. TROSTEN: Yes, there is something I would like to offer. Mr. Chairman, as I indicated previously in the hearing, the Applicant and three of the Intervenors have entered into a stipulation generally reflected on the record at the hearing on October 5th concerning the further conduct of proceedings. I refer to the further conduct of the radiological safety proceedings as well as the environmental proceedings. I won't bother summarizing these matters because of what was said on the record on October 5th.

Pursuant to 10 CFR 2.7433 and also 2.753, I would like to offer this stipulation among Applicant and Intervenor's Citizen's Committee for the Protection of the Environment, Environmental Defense Fund and Hudson River Fishermen's Association, concerning further conduct of this proceeding, as a joint exhibit in this proceeding on the part of the Applicant and the three named Intervenors. Copies will be served at this point on the other parties which are present in the room at this time, and copies will be served in accordance with the regulations on the other parties at the proceeding.

CHAIRMAN JENSCH: May we see a copy?

MR. TROSTEN: Yes.

Although we are prepared to offer it as an exhibit, Mr. Chairman, it is entirely satisfactory to us to have it incorporated in the transcript of the proceeding, if you wish.

CHAIRMAN JENSCH: What is the purpose of the offer?

MR. TROSTEN: The purpose of the offer, Mr. Chairman, is to set forth the understanding of the Applicant and these three Intervenors as to the further procedure to be followed, and particularly with respect to the timing of procedure for the conduct of the radiological safety hearings, and also for the conduct of the environmental hearings.

As contemplated by the Commission's regulations, the Applicant and these three Intervenors have stipulated as to the procedural aspects of this proceeding. We wanted to offer this as an exhibit in this proceeding to set forth and record for the record what our understanding is. It is also our purpose in doing this, Mr. Chairman, to further advise the Board as to the intentions of these parties to the proceedings with respect to actions which we are hopeful can be taken by the Regulatory Staff and also by the Board.

CHAIRMAN JENSCH: Let us review the matter. I notice on page 7, just scanning the pages, "The stipulating parties agree that the Board should conclude any hearings that the Board considers necessary concerning the issuance of such license and -- " You refer to four days of continuous session. I'm sure the Board will not make any commitment as to how long it should receive evidence in this matter nor exhaust its concerns respecting the matter. I'm sure the Commission is anxious that a full and complete record be made.

I think time schedules get more limiting so that if any suggestions given to the Board that thou shalt complete in four days, I just feel it might be somewhat beyond even what the Administrative Procedure Act contemplates for a yearing.

MR. TROSTEN: No, Mr. Chairman, I certainly hope the Board does not interpret that way. That was not, of course, the intent at all of the stipulating parties to suggest to the Board that the Board is in any sense bound by this stipulation. This represents an agreement among the stipulating parties as to what those parties felt ought to be done. The Board will of course determine the order of the proceedings here.

In the event that we wish to make a motion pursuant to the Commission's regulations for an expedited proceeding, we will, of course, submit a motion in accordance with the Commission's regulations.

CHAIRMAN JENSCH: We will give consideration to this matter. If there is nothing further at this time --

MR. ROISMAN: Mr. Chairman, excuse me. Just to keep the record straight, I would like to say that the participation of the Citizen's Committee for the Protection of the Environment, as reflected in the stipulation, is a statement on their part that they continue to oppose the issuance of any license for this plant. We are included in there because we are willing to have the Board consider the issuance of such license subject to our objections based upon radiological safety matters at the conclusion of these radiological safety hearings.

Less there be any confusion -- And I'm not going to be so concerned with the Board being confused, but rather any members of the public who are here at the hearing confusing it, the Citizen's Committee for the Protection of the Environment is unalterably opposed to any kind of a license, testing, operating, limited or otherwise. That stipulation makes clear that we have thatopposition.

CHAIRMAN JENSCH: Very well.

MR. MACBETH: One last matter. As you know, the Hudson River Fishermen's Association have not been cross-examining on the radiological case. I would like to be excused until the end of the radiological case. I have been here for the last two days partially working out this stipulation with other parties. I would prefer if I could return when we reach the environmental matters.

CHAIRMAN JENSCH: You could be excused, but you are obligated to ascertain when that aspect of the case comes up. No notice will be given by the Board to you for your time to return.

At this time we will recess and reconvene in this room tomorrow morning at nine o'clock.

(Hearing adjourned.)

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