UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY OF NEW YORK, INC.,

(Indian Point Station, Unit No. 2)

Docket No. 50-247

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

Date - November 8, 1971 Pages 2885 - 3000

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

Monday, November 8, 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.

APPEARANCES:

On Behalf of the Applicant:

LEONARD M. TROSTEN, Esq., LEX K. LARSON, Esq., 1821 Jefferson Place, N.W., Washington, D.C. 20036.

On behalf of the Regulatory Staff:

MYRON KARMAN, Esq., and KARL KNIEL, Esq., Office of General Counsel, U. S. Atomic Energy Commission, Bethesda, Maryland.

APPEARANCES (Continued):

On behalf of Intervenor, Citizens' Committee for the Protection of the Environment, and on behalf of the Environmental Defense Fund:

ANTHONY B. ROISMAN, Esq., 1910 N Street, N. W., Washington, D.C.

On behalf of Intervenor, Hudson River Fisherm:n's Association:

AGNUS MACBETH, Esq., 36 West 44th Street, New York, N.Y. 10036.

* * * * *

Witness	Cross
James S. Moore	2891

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MORNING SESSION

CHAIRMAN JENSCH: Please come to order.

Are the parties ready to proceed? Are we going to consider the further evidence from Mr. Moore or are we ready to receive the evidence from the State of New York?

MR. TROSTEN: No, we are ready for further cross-examination of the witness Moore by the Citizens' Committee for the Protection of the Environment.

CHAIRMAN JENSCH: Are you ready to proceed with this proceeding?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Mr. Moore, will you resume the stand, please.

MR. TROSTEN: Mr. Chairman, before Mr. Moore resumes the stand, may I take just a moment to comment on a matter that was reported in the press on Friday, which I felt I ought to inform the Board about, and this concerns a fire which occurred in an auxiliary building for the Indian Point 2 plant which occurred on Thursday evening. This was reported in a press release which Con Edison published on November 5th and it was also reported in the press.

Essentially what happened, Mr. Chairman, was that there was a fire which occurred in a construction toolshed which is located in a steel frame structure which is 300 feet to the rear of the building which houses the Indian Point 2 turbine

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generator.

Now the fire did not involve any radioactive release. The fire started about seven p.m. and was put out about nine-thirty p.m. As I say, this was a matter which was reported in the press. The AEC Staff, of course, is fully aware of this. I mentioned this to Mr. Roisman, mentioned it to him Friday evening, and I thought it appropriate for me to mention this to the Board this morning before we get started with the cross-examination of Mr. Moore.

CHAIRMAN JENSCH: Very well.

One other matter that perhaps might be mentioned before we proceed.

On Friday last I received a further telephone call from Congressman Dow who indicated that Dr. Sternglass was ready to proceed to attend this hearing and make some statement. I informed him that since Congressman Dow had been here in the proceeding that schedules had been arranged in an endeavor to compress into two weeks a pretty long list of witnesses and examination. I endeavored to indicate to Congressman Dow that the readiness of Dr. Sternglass to appear did not necessarily mean that there was convenience for his appearance and necessarily the demands of this case and the necessity of moving forward with the presentation of evidence would have to take priority in any consideration for an appearance.

I thereafter telephoned Staff counsel and gave him

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the substance of this conversation that I have now reported to you.

If there is nothing further--

MR. KARMAN: Mr. Chairman.

CHAIRMAN JENSCH: Yes.

MR. KARMAN: Continuing in that vein, I spoke with Congressman Dow Friday afternoon after having received your telephone message. Congressman Dow indicated that he was not at that time aware when Dr. Sternglass would care to appear and exactly what might be entailed in any appearance he makes, and I tried to explain to him what I thought would be involved in Dr. Sternglass either submitting a limited appearance statement or if he was desirous of doing so, and the Board so desired to have him read into the record a limited appearance statement which I thought which would not take up too much time, so that we might get on with the evidentiary part of the hearing which had been scheduled.

Congressman Dow then indicated to me that he was going to speak with Dr. Sternglass and that he would communicate again either with the Chairman of the Board or with me at the hearing in Croton.

CHAIRMAN JENSCH: Very well. I think that the suggestion about any presentation by Dr. Sternglass was really an endeavor to see if the further data could be supplied to Congressman Dow himself. Maybe that's a method by which that

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could be done, so that any further questions that may be in Congressman Dow's mind may be answered some other way.

But as to that I will leave it to arrangements by those.

(Discussion off the record.)

MR. TROSTEN: Mr. Chairman, Applicant's position with regard to Dr. Sternglass' making a statement in connection with this proceeding is as stated in the last time we discussed this. We will simply have to await developments to see what our position is with regard to this matter.

CHAIRMAN JENSCH: Very well. Let us proceed.

Mr. Moore, will you resume the stand, please.

(Witness Moore resumes the stand.)

MR. ROISMAN: Mr. Chairman, the cross-examination this morning will begin with material which is contained in certain reports that have been designated by the Westinghouse Corporation as proprietary. We have no suggestion to the Board as to what the procedure should be for doing this, but we'd be glad to accommodate whatever the Board thinks is appropriate.

CHAIRMAN JENSCH: You have discussed this with the Applicant, have you?

MR. ROISMAN: Yes, we have discussed the subject matter of the cross-examination.

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CHAIRMAN JENSCH: Before we proceed --

MR. TROSTEN: Mr. Chairman, we have discussed this matter with Mr. Roisman in the past. The Citizens' Committee for the Protection of the Environment, an applicant, are agreeable to an in camera cross-examination by the Citizens' Committee for the Protection of the Environment of Mr. Moore with regard to the proprietary documents involved.

As far as the Intervenor and the Applicant are concerned, this is entirely acceptable procedure. At the last hearing session the Chairman indicated an interest in some further presentation with regard to this matter. We are prepared to go forward with such a presentation, if you wish. If you feel it is necessary before we may have an in camera session, that is.

CHAIRMAN JENSCH: What is the contention about the proprietary matter? Is it anything we can discuss on the public hearing record?

MR. TROSTEN: Yes. The contention, fundamentally, Mr. Chairman, is that the three or four reports which Mr. Roisman, assisted by Mr. Ford, wished to cross-examine Mr. Moore about our Westinghouse proprietary reports. The cross-examination on the public record with regard to these reports would result in the divulgence of Westinghouse propriety information. Hence, it is the Applicant's position that the Westinghouse proprietary information should be protected by not being disclosed in a public hearing.

I want to emphasize, Mr. Chairman, that the Applicant and Westinghouse have furnished to the Citizens' Committee for the Proection of the Environment -- that is to Mr. Roisman as attorney for the Citizens' Committee. -- and to two consultants to the Citizens' Committee under a suitable protective agreement of the proprietary reports involved. They have these documents. They have examined them fully. These documents have, I might add for the record, been furnished to the Board. Of course, they have been furnished to the AEC Staff.

So there is no question here about the willingness of the Applicant and the Westinghouse Electric Corporation to furnish this information to the Intervenor for purposes of trial preparation.

In accordance with the agreement between the Westinghouse Electric Corporation and the Intervenor, the Intervenor has agreed that any proceeding in which these documents are discussed will be held in camera. That is the point at which we find ourselves today, Mr. Chairman.

CHAIRMAN JENSCH: Is there any question among the parties respecting the adequacy of the showing concerning the proprietary character of the material?

MR. TROSTEN: I can answer that for the Applicant, and I suppose Mr. Roisman will have to answer for himself.

CHAIRMAN JENSCH: Are these all data developed solely by the proprietor without any funding of the research by the Atomic Energy Commission and that sort of thing?

MR. TROSTEN: As far as the Applicant and the Intervenor are concerned -- and I am stating this correctly for Mr. Roisman. -- there is no disagreement by us as to the proprietary character. We see no disagreement. Mr. Roisman has not raised any question about the proprietary character of this.

We are prepared, Mr. Chairman, if you wish, to introduce testimony today, if you desire it, to further explain why these reports are proprietary, the reports involved.

CHAIRMAN JENSCH: I think any change in procedures would require some sort of a showing on the record as to why we need to go into an in camera session. I think the public are entitled to have those data.

MR. TROSTEN: Let me try to explain this on the record and maybe it won't be necessary to have testimony in this respect. In addition to the explanaation I have already given to regard to why it is we feel an in camera session it is appropriate here, I should add that the Applicant's position is that all of the necessary data for the evaluation of the safety of this plant are contained in nonproprietary documents which are fully available to the public, have been available throughout the course of this proceeding. Hence, the particular proprietary information which is contained in these reports is not necessary to be divulged to the public because there is already adequate information in the record for the safety evaluation to be made. So the public has access to all of the necessary safety information as does the Board and the parties.

CHAIRMAN JENSCH: Those conclusions I think are the ones that need be supported by fact. As to what the public has adequate data or not, they may disagree. I think the important issue here is were the data which are claimed to be proprietary developed solely by the alleged proprietor of the information? So it constitutes the solely the information and data of the alleged proprietor thereof.

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MR. TROSTEN: Good morning.

Mr. Chairman, in view of your suggestion that we need a factual presentation here of the testimony, I think it would be appropriate for me to offer a witness now to offer testimony in this respect.

CHAIRMAN JENSCH: I think that will be well. Will you do that.

Mr. Moore, step down temporarily. Thank you.

MR. TROSTEN: Mr. Chairman, with regard to this, our interest of course, was attempting to get the cross-examination in. It is our intention it will complete the radiological safety proceeding. We had thought perhaps that a procedure that could be used -- although we hadn't spoken with Applicant about it, that if the session is closed, if we have it in camera, that the Applicant, after a short period of time after the transcript is prepared, advise the Board which portions of that transcript could be taken and turn the in camera session into at least a part of the public session. It is very difficult to sort the material out in advance. We did not want to have a long period of time in which that would be done. That might at least get onto the record all of the nonproprietary discussions on the record.

There will be a substantial portion of it that will be nonproprietary. We have a proprietary version of one of the reports. The major thing that is missing are the numbers. The discussions virtually are the same, but all the numbers are missing.

So there may be the same sort of deletions from the transcript, by making the transcript available to the general public and the discussions contained in there will be fully available to the public.

But we would hope that this wouldn't get into a question that would require a delay in the presentation of our case, if at all possible.

CHAIRMAN JENSCH: We share that view and we hope this matter will be expedited. Before we close this hearing to the public, we will have to have something on the record first.

MR. TROSTEN: As Mr. Roisman has pointed out, we really have not had an opportunity to discuss the precise procedures for this. We are, of course, exceedingly anxious to expedite the presentation of the Citizens' Committee case.

CHAIRMAN JENSCH: Everybody has the same premise. Let's go from there.

MR. TROSTEN: May I suggest that after the testimony is offered, assuming the Board agrees, we will have an in camera session, that the Board could determine after that session whether any portions of the --

CHAIRMAN JENSCH: Yes, that's perfectly all right. Call your first witness, please.

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MR. TROSTEN: I would like to call Mr. Robert Wiesemann to the stand.

CHAIRMAN JENSCH: He has been previously sworn and need not be sworn again. Will you proceed, please.

MR. TROSTEN: Mr. Wiesemann, are you familiar with the procedures utilized by Westinghouse Electric Corporation, and in particular, by Westinghouse Nuclear Energy Systems, for classification of certain information as proprietary?

MR. WIESEMANN: Yes, I am.

MR. TROSTEN: Are you generally familiar with the documents classified as proprietary to which we are referring? That is the ones to which cross-examination is to be directed. I am referring to the following documents: WCAP 7379-L, Volume 1; WCAP 7495-L, Volume 1; WCAP 7495-L, Volume 2; and unnumbered document dated June 1, 1971, which is entitled, "Emergency Core Cooling Performance"?

MR. WIESEMANN: Yes, I am.

MR. TROSTEN: Mr. Wiesemann, does Westinghouse have procedures which it utilizes in classifying such documents as proprietary?

MR. WIESEMANN: Yes, we do.

MR. TROSTEN: Would you describe these procedures, please.

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MR. WIESEMANN: Yes. An initial determination is made by the author of the particular report as being the person most knowledgeable with respect to the context, nature of the sensitivity of the information concerned, the state of the art, and the knowledge in the industry of the particular subject matter and the usefulness or the potential usefulness of that information to people who are in competition with Westinghouse, or the extent to which that information would give Westinghouse a competitive advantage over its competitors.

This preliminary determination is reviewed by the management level supervisors of the person originating the material and then if that preliminary determination is agreed to by the management level supervisors, that determination holds if the determination is that is should not be proprietary or if the original determination by the author is that the material should be non-proprietary, the report then is reviewed by management up to and including the level of the general manager of the division involved in order to determine whether or not the material is indeed non-proprietary.

And if it is found that the report is in fact proprietary in nature, the report is then returned to the author, either to make the report proprietary or to make changes necessary in order to make the report non-proprietary in nature.

MR. TROSTEN: Now in determining whether information is to be made proprietary what criteria and standards are

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utilized by Westinghouse?

MR. WIESEMANN: The questions that have to be investigated in determining whether or not a report is proprietary or not are these following questions:

Does the report reveal distinguishing aspects of a process whose exclusive use constitutes a competitive advantage to Westinghouse?

Does it consist of supporting data relative to the process constituting competitive advantage?

Does it contain information, the use of which by our competitors, would reduce his expenditures of resources?

Does the report reveal cost or price information, production capabilities, budget levels or commercial strategies?

Does the report reveal aspects of past, present or future or customer-funded development plans and programs of commercial value?

And, does the report contain patentable ideas?

MR. TROSTEN: Mr. Wiesemann, does each of the documents to which I referred earlier contain information in one or more of the categories you have mentioned?

MR. WIESEMANN: Yes.

MR. TROSTEN: With regard to their proprietary nature, what is the status of these documents?

MR. WIESEMANN: They are presently deemed to be proprietary by Westinghouse.

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MR. WIESEMANN: Yes. An initial determination is made by the author of the particular report as being the person most knowledgeable with respect to the context, nature of the sensitivity of the information concerned, the state of the art, and the knowledge in the industry of the particular subject matter and the usefulness or the potential usefulness of that information to people who are in competition with Westinghouse, or the extent to which that information would give Westinghouse a competitive advantage over its competitors.

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MR. TROSTEN: Would you summarize why the documents are considered proprietary by Westinghouse?

MR. WIESEMANN: Each document involved is considered proprietary because it contains information which is customarily held proprietary by Westinghouse. Each document reports on research and the development programs, including experiments, tests, analyses and development of analytical techniques, and each document sets forth in detail equipment, procedures, results and/or conclusions of such experiments conducted with Westinghouse funds for its exclusive benefit.

MR. TROSTEN: Are those documents customarily held in confidence by Westinghouse?

MR. WIESEMANN: Yes.

MR. TROSTEN: Are those documents customarily made available to the public by Westinghouse?

MR. WIESEMANN: No.

MR. TROSTEN: Have those documents previously been transmitted by Westinghouse to the Atomic Energy Commission?

MR. WIESEMANN: Yes.

MR. TROSTEN: In connection with such transmittal what, if anything, was requested of the Atomic Energy Commission?

MR. WIESEMANN: We requested that the Atomic Energy Commission withhold the documents from public disclosure.

MR. TROSTEN: Have those documents previously been transmitted to representatives of the Citizens' Committee for

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the Protection of the Environment, an Intervenor in this proceeding?

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MR. WIESEMANN: Yes.

MR. TROSTEN: Can you tell us what procedure was used with respect to such transmittal?

MR. WIESEMANN: Basically the reports were transmitted to the persons you identified under the terms of an agreement. When the question initially came up regarding the production of proprietary documents we entered into discussions with the Intervenor in question and developed an agreement which limited the use of the document and provided protection for the proprietary material contained in the documents, and this agreement was agreeable to all parties and was subsequently signed by the parties to whom the documents were produced.

MR. TROSTEN: Mr. Chairman, I have no further questions of this witness.

CHAIRMAN JENSCH: I wonder if I could see the documents.

MR. TROSTEN: The Board, of course, already has copies of these documents. Mr. Wiesemann has in his possession the documents involved.

MR. WIESEMANN: Those are the three.

Is there a copy of the 6-1-71?

MR. FORD: I have the copy.

MR. WIESEMANN: Oh, here it is.

MR. TROSTEN: Mr. Wiesemann has the 6-1-71 document.

CHAIRMAN JENSCH: Does anybody desire to interrogate the witness in reference to this matter? Intervenors?

MR. TROSTEN: No, Mr. Chairman.

CHAIRMAN JENSCH: Staff?

MR. KARMAN: No, we have no questions.

CHAIRMAN JENSCH: Is it your view, Mr. Wiesemann, that none of the funds from the Atomic Energy Commission have been utilized in any respect pertaining to the development of the data revealed in these four documents to which you have referred?

MR. WIESEMANN: Yes, sir. If you would look on the cover page, title page of any one of those three documents that are bound, just past the blue sheet, you will see a line about one-third or two-thirds, I am sorry, down the page, which says "Work was performed under DGR." The DGR in that letter series refers to Division General Research funds which are moneys exclusively Westinghouse funds.

CHAIRMAN JENSCH: And none of the information revealed in these documents is related in any respect to the previous R & D work undertaken by Westinghouse for the Atomic Energy Commission, is that correct?

MR. WIESEMANN: I am not sure I understand the nature of your question. They certainly are related in the sense that they relate to the same -- to some of the same problems for which other work has been performed. But the work itself is separate and independent.

CHAIRMAN JENSCH: I notice in the acknowledgment that you acknowledge receipt of some data from General Electric Company. Is this something that's shared with General Electric Company?

MR. WIESEMANN; No, sir.

CHAIRMAN JENSCH: What was shared with General Electric Company?

MR. WIESEMANN: To my knowledge only the data that was obtained from General Electric Company is the data that was obtained as the result of performing -- I believe we discussed earlier in the record the fact that there were certain tests performed in order to get a comparison of the results from our test facility with the General Electric test facility, and to my knowledge that is the only data that was, if you want to call it shared.

CHAIRMAN JENSCH: It says "We wish to acknowledge the assistance of" so-and-so, and then goes on to Messrs. J-u-e-n-k-e and J. F. White of the General Electric Company who directed the experimental program reported in Volume 2.

Are these data shared with General Electric, do you know, or did they shut off their transmission of data from the experimental program as far as submitting it to General Electric, do you know?

MR. WIESEMANN: No. I think you are talking about the program where we performed tests on certain General Electrc

Company fuel rods to get comparison data, and the General Electric Company directed the tests of that portion, since it involved their fuel rods.

MR. TROSTEN: Mr. Chairman, I am informed by a representative of Westinghouse that perhaps your question refers to Volume 2 of 7379.

MR. WIESEMANN: Yes, it does.

MR. TROSTEN: Which is not proprietary, I am informed.

CHAIRMAN JENSCH: There is no interrelationship between the two experimental programs reflected in Volume 1 or 2, is that correct?

MR. WIESEMANN: That's correct.

CHAIRMAN JENSCH: In your opinion is this a legally sufficient test to determine the proprietary character of these exhibits, Applicant's counsel?

MR. TROSTEN: Mr. Chairman, in our opinion the testimony which has been given is legally sufficient to establish that this proceeding wherein Mr. Moore is to cross-examined with respect to the documents involved should be held in camera.

CHAIRMAN JENSCH: I didn't hear any testimony as to whether or not these data have been of a kind that have had general distribution.

MR. TROSTEN: Mr. Chairman, in response to that point it is our view that the testimony which has been offered by Mr. Wiesemann establishes that this information is proprietary and is of a sort which is entitled to protection through the means of an in camera proceeding. It is Applicant's position that under the applicable statutes and regulations that the testimony that has been offered is sufficient to establish that point.

CHAIRMAN JENSCH: Yes. I think that is what you just stated a moment ago. I understand you have now repeated it.

My question, however, was what general distribution of these data have been made, or have these data had general distribution either in this country or abroad, do you know?

MR. WIESEMANN: This data has not had general distribution, Mr. Jensch. It is classified as proprietary, and having been classified as such it is only disseminated where there is need and also where there is protection in the form of an agreement between the receiving party and Westinghouse to protect the proprietary nature of the material.

CHAIRMAN JENSCH: And none of these data have appeared in technical journals that have had general public distribution, is that correct?

MR. WIESEMANN: Of the data that we are holding proprietary.

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As Mr. Roisman mentioned earlier, we have identified the material which is proprietary and the report which sonsists primarily of data and the description of photographs describing the tests and description of test set-ups.

CHAIRMAN JENSCH: I think my question was have these data been contained in any technical journals that have had general public distribution?

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MR. WIESEMANN: No.

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CHAIRMAN JENSCH: I will return these documents to Mr. Wiesemann. The Board will be giving some further consideration to this matter. Before doing that, the Board has some questions on pressure vessel integrity they would like to more or less propound at this time for which you will perhaps be desirous of securing some additional information or presentation within the next few days. At the end of this presentation of these further concerns on pressure vessel integrity, the Board will make its determination respecting the proprietary contentions which we have discussed.

MR. WIESEMANN: Excuse me, Mr. Jensch. Did you intend to return the fourth copy?

CHAIRMAN JENSCH: I will, but just a moment. I do intend to do it. Dr. Geyer was reviewing it. He has now completed his review. It is received thereof. Will you acknowledge receipt for the record.

MR. WIESEMANN: I received all four proprietary reports.

MR. TROSTEN: Did you say you were going to defer your ruling until when, sir?

CHAIRMAN JENSCH: In just a few minutes. You may step down, Mr. Wiesemann.

MR. ROISMAN: Just so the record is clear, as we have indicated before, we have no objection to the closed session. We will want to have an opportunity to argue, based upon the closed session. the portions of the session are all that should be made public based upon the nature of the evidence.

Just for the record, I would like to direct the Board's attention to the two rulings of the Atomic Safety and Licensing Board in the matter of Midland Nuclear Power Station, Units 1 and 2, 50-329, 50-330, decided September 2 and September 22nd of this year, which dealt with the question of when documents which are proprietary still are to be released to the general public. We are not after release of the documents per se, but we may wish to argue at a subsequent date that the transcript or portions thereof should be released. That will be after filing of our brief and proposed findings of fact and conclusions on law.

CHAIRMAN JENSCH: What prevents you from making that argument now without having the specific figures in mind? I think the proprietary character, to some extent, should be determined before we start the interrogation. If it should later be determined that a portion of these data should be indicated to the public, they should hear it now.

MR. ROISMAN: Our argument is not that the documents are not proprietary. The decisions here that I have just referred to, in the matter of Consumers Power Company, indicate that even if the material is proprietary, it may be that it should be released because it would be in the public interest.

We are not prepared to argue and it would not be our position that the release of the documents would necessarily be in the public interest, but we wish to reserve the right to argue that release of all or a portion of the transcript will be in the public interest. We think that it would be difficult now to establish the public interest's allegations in the context of documents until the cross-examination has taken place.

Again, we are anxious to have the hearing go ahead. We did not feel it was necessary to tie it up on a technical point. There are problems, if there is a disagreement on that issue, about certification to the Appeals Board and so forth. And frankly, we have our expert here now and are anxious to proceed with it.

MR. TROSTEN: Mr. Chairman, as Mr. Roisman has called attention to the Atomic Safety and Licensing Appeal Board decision, I feel that I should also call the Board's attention to the decision of the Atomic Safety and Licensing Appeals Board in the matter of Northern States Power Company, Docket No. 50-263, dated August 20, 1970, and specifically pages 19 and 20 thereof, in connection with what we believe is the propriety of the course of action that is being proposed here. That is an in camera session. Of course, this procedure is also, we believe, Mr. Chairman, entirely consistent with procedure that has been adopted in two other proceedings with which the Chairman, I am sure, is familiar. One involves the Jersey Central Power and Light Company, Oyster Creek Reactor, and the other involving the Commonwealth Edison's Company, Dresden-1 Facility.

CHAIRMAN JENSCH: We will go forth and proceed with the questioning of the Board.

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MR. BRIGGS: In the interest of expediting the testimony later in the week, I believe the Applicant's counsel asked that the Board indicate what witnesses we might like to have here. I don't believe we can indicate what witnesses, but may be able to indicate some of the questions that we would like to ask to discuss, and some information that might be provided ahead of time. This will be to the Staff and to the Applicant both. Some of the questions are concerned with the Staff's proposed testimony, and some are concerned with the Applicant's testimony.

The first one has to deal with the Staff testimony. In that it is indicated that ninety-five nuclear pressure vessels of commercial pressurized and boiling water reactor plants have been operated, and talks about the hours of operation and the lack of problems.

The Board would like to be provided with a list of those ninety-five nuclear pressure vessels, roughly the number of hours that each has operated, indicating in the list where the vessels have received more than a superficial volumetric in-service inspection, and then, at the time of the discussion, be prepared to indicate any problems anticipated or unanticipated that have been experienced with those vessels.

In the Staff testimony there is also discussion of burst stresses and pressures. We would like to have these compared with the numbers that Mr. Wiesemann indicated for the

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Indian Point 2 vessel, to be sure that there is no significant difference.

Great stress is put on the point that the reactor vessel will be operated under ductile conditions but there are temperatures at which the vessel material will not be ductile. Could the Applicant or the Staff provide some discussion about what causes the nil ductility transition in steel, nil ductility temperature in steel; and why does radiation cause this temperature to change. Also, how certainly it is known that the change found in the surveillance specimens will be duplicated in the thick wall of the reactor vessel.

Now I would like for there to be a discussion in some detail about the pre and the post hydro test ultrasonic inspection of the Indian Point 2 vessel. In other words, prior to the hydrostatic tests was the ultrasonic testing done on a grid that where a test was run here and then two or three feet away over here, just how fine was the grid on which the vessel was inspected both before and after the hydrostatic test?

If it is at all possible, we would like to be provided with information concerning the ten largest indications of flaws in the vessel. That is, what was the nature on the indication, how were these indications interpreted? These are the ten largest flaws in the vicinity of Wells, and the ten largest in the base material. Also, how these locations compare with locations that will be examined during the in-service

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testing of the reactor or in-service inspection, I should say.

It is pointed out in several places that the vessel, if it were to develop flaws, could be expected that leaks would appear before the flaws became large enough to cause failure of the vessel. This is also indicated for piping and other places.

With regard to the vessel, how certain is it that flaws that are foreign in the carbon steel will penetrate the cladding before the flaws are large enough to cause failure of the vessel? The cladding is of a different material, has different characteristics. How certain is it that these flaws will penetrate the cladding?

I would like for some agreement to be reached during the discussion of the failure of the vessel, on the consequences of such failure if it did occur. I would like some agreement on what the likely consequences of a melt-down of the core would be, however remote the possibility is, if such melt-down were to occur.

Finally, I believe during the previous testimony, testimony on October 5th, a question was asked concerning whether there had been an experimental stress analysis of the reactor vessel during the hydrostatic testing, whether the stresses were measured during the hydrostatic testing on a reactor vessel. The thought seemed to be that combustion had done such a test, and I believe it was agreed that if it had

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been done, the Applicant would provide a reference, if such reference existed, and would like to have that reference if there is one available.

There certainly will be other questions that will be asked. I hope that these will give some idea of the kinds of questions and the people that ought to be here to reply to them.

MR. KARMAN: Mr. Briggs, I was wondering whether it would be possible, if after receiving a copy of the transcript this evening and carefully noting the several questions which you propounded this morning, whether it would be possible for us to communicate with some of our technical staff in Bethesda, the ones who are primarily responsible for submitting the response which we did distribute to the Board and parties on pressure vessel integrity. If it is at all possible, we can then come back to the Board with an additional statement clearly and comprehensively responding these inquiries without the necessity, if that is possible, of bringing an additional witness to the hearing.

Of course, we would leave this to your judgment. I indicated to our people back home that if it is at all possible and if they can respond to your questions by way of a written statement, it will be much appreciated.

MR. BRIGGS: I think certainly I would have no objections to the answers provided in writing. I don't know whether the full answers can be provided, and I would hope that

2Wt5

the questions would considerably limit the number of people that you would want to bring here.

MR. KARMAN: I will discuss this with my people back at the office. If there is any problem, I will get back to you, sir.

MR. BRIGGS: If it means that you have to bring one witness or two witnesses, I think this could be expected. I would hope that it wouldn't require that you would have to bring a half dozen.

MR. KARMAN: I did not anticipate doing so. This week is ACRS week.

CHAIRMAN JENSCH: Maybe the ACRS could come and help us.

MR. KARMAN: I have very little control over the ACRS

E Bul

CHAIRMAN JENSCH: In view of the information that was given about the attendance of other witnesses, I hope we can give it further consideration to this matter at a later time.

The Board recognizes the rules of the Atomic Energy Commission provide that proprietary information developed by an inventor or manufacturer is entitled to protection. I think the language in some of the legal journals in dealing with proprietary information and the necessity of the protection thereof is to avoid and maybe this won't apply to this case, but to avoid parasitic use of the information, that people who have not undertaken research and development would be inclined to utilize the benefits of others' labor to the disadvantage of those who have exercised initiative, ingenuity, in developing data.

And just perhaps not in the same extent or scope, but just as the patent system endeavors to protect ideas which are developed to permit the abuse of new and novel techniques of manufacturers, likewise the proprietary information is entitled to a protection of perhaps a different kind, but of the same general character.

The Atomic Energy Commission will respect proprietary information of any party to the proceeding, whether it be the Applicant, the Intervenors. It is the Board's determination that upon the basis of the record presented here that the data described and identified by Applicant's counsel is within the scope of the protection intended by the rules of the Atomic Energy Commission. Further consideration will be given to the release of this information or a portion thereof at later sessions but at this time the Board will recess the proceeding to reconvene within ten minutes at which time only the parties, that is the Intervenors and the State of New York and the Hudson River Fishermen's Association and the Staff, of course, and their attorneys and their technical assistants will be permitted to attend. This transcript on the matter will be prepared in what is known as an in camera session and the data developed at that session will be the subject of consideration as to the extent of its release at a later time before recessing, however, inquiry was made as to how long do you think this in camera session will last so that we may now inform the public to return to the public hearing? Can the Intervenors indicate?

MR. ROISMAN: Mr. Chairman, it appears it will surely run until lunch. It would be possible for us to make a more definitive judgment at 12:30 as to whether it will be running after lunch. We will be starting with the proprietary documents and try to dispose of them in the course of the discussion, but as was apparent last week it's difficult to predict exactly. My guess is --

CHAIRMAN JENSCH: Well, we don't have to stop at 12:30;

if you can go through to 1:30 or something without lunch and finish up the proprietary character the Board would prefer to indicate now to the public when the public session of the hearings will be available.

MR. ROISMAN: Perhaps why don't we agree now that we will go for a certain number of hours. If that isn't enough, we can pick it up later, like the first thing in the morning, or something like that.

CHAIRMAN JENSCH: We will perhaps have to issue further public notices to the public. If you are not done with the proprietary interrogation I think it would be better to continue with that now and if necessary issue a further public notice. I was hoping to avoid any inconvenience to the public, but at this time let us state that we will recess this public hearing, to reconvene at 3 o'clock this afternoon. We expect from what indications have been given that the proprietary data will be developed by that time and we will resume the public hearings. And if that is not correct, we will have to formally convene a public hearing and then recess the public hearing to a time later to be determined.

At this time let us recess this public hearing to reconvene this afternoon at 3:00 o'clock in this room. And the in camera session will begin at 10:05 this morning.

(Hearing recessed.)

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

CHAIRMAN JENSCH: Please come to order.

Mr. Moore, would you resume the stand, please.

J A M E S   S.   M O O R E, resumed.

CHAIRMAN JENSCH: Intervenors, are you ready to proceed?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Will you proceed.

MR. FORD: Mr. Moore, is it correct that in the BWR FLECHT tests negative heat transfer coefficients were observed at axial levels in a number of different instances?

MR. MOORE: They were recorded as negative heat transfer coefficients. What they actually indicate is reverse heat transfer from the coolant to the cladding.

MR. FORD: For purpose of this discussion and since they are plotted as heat transfer coefficients, would you just accept the definition of terms, that is a negative heat transfer coefficient?

MR. MOORE: I guess I'd prefer reverse heat transfer, which is more descriptive.

MR. FORD: I see. It is correct, though, that the reverse heat transfer coefficients are represented in your data as negative heat transfer coefficients, is that correct?

PBm2

MR. MOORE: Yes, yes.

MR. FORD: Thank you.

Do you agree that if you passed a saturated vapor, saturated steam through a furnace that you'd create superheated steam?

MR. MOORE: If I pass saturated steam through a furnace I create superheated steam?

MR. FORD: Yes.

end

MR. MOORE: Yes.

QWt1

MR. FORD: Do the codes that you use for analyzing the loss of coolant accidents explicitly consider the formation of superheated steam or do they regard the coolant at different axial levels being simply liquid entrained in steam, period?

MR. MOORE: It depends on which calculation you are talking about.

MR. FORD: In the calculations that you have used for Indian Point 2 to calculate the maximum clad temperature, have you separately considered the role of superheated steam in precipitating, or yielded the maximum clad temperature?

MR. MOORE: In terms of reflooding, yes.

MR. FORD: In terms of the code analysis that you have done, do you use negative heat transfer coefficients under any assumptions of flooding rate or pressure?

MR. MOORE: If they would exist, yes. For the hot spot calculation, such a condition never does exist.

MR. FORD: I see. In terms of the negative heat transfer coefficients that were observed, can you tell me at what axial levels these were observed?

MR. MOORE: They were well above the hot spot. That is specifically the point. They were where the temperature was quite low of the cladding.

MR. FORD: Have you done any calculations which guarantee that the superheated steam, a negative heat transfer coefficient would always occur above the mid point?

QWT2

MR. MOORE: Yes.

MR. FORD: Where are those calculations presented?

MR. MOORE: Any one of these core cooling analyses were computed with the hot spot temperature. You can see the temperature itself is much greater than any saturated or even superheated condition that could exist.

MR. FORD: Those are the calculations that you have presented. What I am asking is whether you have performed parametric calculations that indicate under no circumstances, that is, under no combination of parameters, which you get superheated steam at lower than the ten-foot elevations that it was observed at in the FLECHT test?

MR. MOORE: Yes.

MR. FORD: You performed these parametric calculations specifically for the point of determining the axial location of superheated steam?

MR. MOORE: To determine the energy balance, yes.

MR. FORD: I am trying to ascertain whether or not you have paid particular attention to the phenomena of superheated steam and whether you have performed calculations specifically on that question.

MR. MOORE: Yes.

MR. FORD: Can you indicate where these calculations are reported?

MR. MOORE: I don't know that there is any specific

QWt3

reporting of temperatures of steam in the core during the course of the transient. I know of no specific report.

MR. FORD: And am I to interpret that as an indication that the calculations which you said had been performed on superheated steam--you change your opinion to say they have not been performed.

My question was for a specific reference.

MR. MOORE: I said there was no specific reference. That's a different question.

MR. FORD: There is no specific reference--let me get this straight--in which you specifically focused on the question of superheated steam and determined the extent of superheating and its role in loss of coolant accident?

MR. MOORE: Can we go back to the fundamentals and the way we calculated the heat transfer? That is what you are really getting at.

MR. FORD: Can you answer that question directly?

MR. MOORE: I can't directly, no. We have to understand how the analysis is performed.

MR. FORD: Can you tell me, in your calculations of the metal-water reaction rate, that the reaction is the function of the cladding temperature? Is that correct?

MR. MOORE: Yes.

MR. FORD: Have you performed any calculations defining the role of steam temperature in rate of reaction?

MR. MOORE: We just determined that the rate of reaction is a function of cladding temperature.

MR. FORD: Yes. That's what I said. That's the way in which you do calculate?

MR. MOORE: No. That is the basis for the rate equation.

MR. FORD: I understand that. What I am asking is whether you have performed calculations that indicate the extent to which that rate equation would have to be modified if you considered variations in the temperature of the steam.

MR. MOORE: It would not be modified. The temperature of the cladding in terms of the rate.

MR. FORD: What I am asking for is the evidence for that hypothesis.

MR. MOORE: I am afraid we are back to our previous discussions on the metal water reaction kinetics where I thought we ascertained that the metal water reaction was the function of cladding temperature, not steam temperature.

MR. FORD: Yes. I am clear that that is the position that you have taken. Can you tell me what experiments you have performed with different temperature steam to verify the fact that the metal water reaction rate is not a function, in addition to your cladding temperature, of the temperature of the steam?

MR. MOORE: We are back to the previous discussions on the FLECHT tests where we had steam, we had temperature of cladding. The temperature of the cladding was determined and the reaction rate was determined on the basis of that temperature and we fell below the Baker-Just relationship.

MR. FORD: Could the reporter read my question back again, please.

(The last previous question is read by the reporter.)

MR. FORD: Now, in the test that you referred to was the temperature of the steam varied to a range including superheat?

MR. MOORE: We are talking about the FLECHT tests now? That's the test I referred to.

MR. FORD: Yes.

MR. MOORE: As it said in the FLECHT Report, there was.

MR. FORD: Did you in specifically ascertaining the data in order to do sensitivity analysis on the superheat steam -- is it correct that you'd have to measure the temperature of the superheat steam?

MR. MOORE: In order to determine what?

MR. FORD: The sensitivity of steam temperature to the metal water reaction rate.

MR. MOORE: To get a precise quantity for sensitivity, yes.

MR. FORD: Did you measure the temperature of superheat steam?

MR. MOORE: I believe there were some measurements made.

MR. FORD: I have looked. Can you cite any measurements here and explain how they were made?

MR.MOORE: No. We were not specifically measuring the temperature steam. I believe there were some measurements made in some of the tests. I don't even know if they are reported in the report.

MR. FORD: Can you explain to me or can you document any experiments that have been done to determine the capability of the thermocouple used to measure accurately the temperature of superheat steam?

MR. MOORE: No.

MR. FORD: Is it possible that the thermal inertia or the mass of the thermocouples could interfere with their ability to measure temperature of superheat steam?

MR. MOORE: Yes.

MR. FORD: Is it correct that the initial steam temperature in the FLECHT test was uncontrolled?

MR. MOORE: Could you give me the reference, please?

end

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MR. FORD: The reference is Page 4.2 of the final FLECHT report WCAP 7665. I will read the paragraph that I am concerned with. It says, "Although prescribed reactor conditions were accurate when produced, it is worth noting that initial steam temperatures within the bundle were not controlled in the PWR FLECHT tests. Reactor losses coolant accident calculations indicate that in some cases steam temperatures at the start of reflooding may be within 100 degrees of the clad temperature. As discussed in Section 3.6 PWR FLECHT steam temperatures were in the superheat range at the start of flooding but were generally several hundred degrees below the clad temperature. This could be expected to have some effect on the value of the initial heat transfer coefficient. However, it is not believed to have had a significant effect on subsequent test behavior."

With regard to this statement it indicates here reference to Westinghouse calculations that steam temperatures at the start of reflooding may be within 100 degrees of the clad temperature. Can you tell me for the FLECHT tests what was the relationship of the temperature of steam to the temperature of the clad, initial temperature of the clad?

MR. MOORE: As stated in the report they were several hundred degrees below the clad temperatures.

MR. FORD: But do you know exactly how many hundred degrees below the clad temperature?

RØBt2

MR. MOORE: No. If we look at Section 3.6 which is referenced.

The temperature of the steam is indicated in Figure 373 and 374 for a couple of runs.

MR. FORD: Yes. I am aware of the fact that it's measured for a couple of runs, but in view of the fact as asserted on Page 4.2 that this has a potential significant influence on initial heat transfer, I am wondering if you can find for me further data in addition just for a couple of runs on what the initial steam temperature was during the FLECHT tests.

MR. MOORE: No, I believe not.

MR. FORD: Do you believe that this--I mean that this data is available, that you don't know whether it's available? Because as I look over all of the FLECHT charts, and there are dozens and dozens of heat transfer coefficients, I don't find listed the specific parameter at all.

MR. MOORE: The specific parameter being the--

MR. FORD: Initial steam temperature.

MR. MOORE: --temperature at the beginning of the test. I believe the data is available but not in this report.

MR. FORD: In terms of the argument that is advanced on Page 4.2, the first premise that steam temperatures at the start of reflooding may be within only a hundred degrees of the clad temperature, do you know what specific calculations the

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document is referring to?

MR. MOORE: I don't know of the specific cases to which this is referenced, no.

MR. FORD: Do you know in terms of calculations in general the relationship between initial steam temperature to initial clad temperature? Do you know whether the specific relationship here is to be considered statistically representative of a whole population of relationships or is this aberrant or what?

MR. MOORE: I really can't comment. I guess I would have to determine at what point in time this particular calculation was made and by what type of analysis.

MR. FORD: Assuming that it was at the defined end of blowdown, could you give an answer in that case?

MR. MOORE: Well, the way we do the calculation at the end of blowdown we assume that the situation is adiabatic in the core and so there is no heat transferred to the steam at all and so all the heat goes into heating up the rod. So it's very difficult to then comment on what the temperature of the steam is, because I am not taking any heat transfer into the steam. That's the confusion I have here.

MR. FORD: I see. Now if I made the question, if I put the parameter in terms of distance of the accumulator or flooding water, if I put the parameter in terms of distance between that water level and the bottom of the core, now that

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I am starting to provide for steam to be available, to be heated up by the furnace, would you under those conditions given distance of the water level from the bottom of the core, with that as a parameter could you make the calculations we have been discussing?

MR. MOORE: Yes. As soon as you reach the bottom of the core with accumulator water you get intrainment of water. Entrainment of water.

MR. FORD: Yes.

MR. MOORE: With the steam. This entrained water is carried up through the core and the calculations show that at the hot spot, which is at the core mid-plane, there is enough heat transferred to the entrained water to boil it away.

So we always have a quality at the hot spot. We don't have superheated steam at the hot spot during any part of the transient, which is of course where we are looking for the peak clad temperature.

MR. FORD: Is it possible that the quality of the steam changes below and above the hot spot such that you would have superheated steam below the hot spot changing the phase at the hot spot and then turn to superheat at higher levels?

MR. MOORE: No.

MR. FORD: Have you any experimental observations or any attempts to simulate, to make superheated steam below it and watch what happens to it?

R2Bt5

MR. MOORE: Well, that's just a physical effect, that the power level as I rise in the core is always increasing with respect to the hot spot. In other words, adding heat on the way up. I am not taking heat away and there is always more and more heat being added per unit length as I go up the core.

So you can't have a situation where I would get superheated steam and then revert back to a quality situation, adding heat all the time.

MR. FORD: Yes.

MR. MOORE: It's just a physical impossibility.

MR. FORD: Can you tell me in terms of the steam probes that you use whether the information from them is sufficient to confirm your hypothesis, namely, that there is no superheated steam below mid-plane during the FLECHT test?

MR. MOORE: I am not sure. I would have to check the data. I would be doing it just on the basis of a heat balance, a physical heat transfer relationship. We calculate the heat transfer coefficients along the rods and, you know, the heat is going out of the rod.

MR. FORD: In terms of the heat transfer calculations that you do, have you ever predicted negative heat transfer coefficients?

MR. MOORE: Which calculations are we speaking about?

MR. FORD: I am talking in terms of any of the analysis that you do on the loss of coolant accident situation, have you predicted, in terms of theoretical models that you have available, any of them, are they negative heat transfer coefficients?

MR. MOORE: Do you understand what negative heat transfer coefficients are? It is really a misnomer. It is a calculation of the cladding and the temperature of the coolant. If the coolant temperature is higher than the cladding, we have heat transfer in the reverse direction. I am not aware of any loss of coolant situation where that obtained.

MR. FORD: I am talking in more general theoretical sense.

MR. MOORE: So am I.

MR. FORD: I am talking about the extent to which the models that you have available for analyzing behavior and loss of coolant accident, whether they have given you an indication that this reverse heat flow, negative heat transfer coefficient, that it would be a phenomenon occurring in a loss of coolant accident.

MR. MOORE: Yes. I don't expect, under the conditions of the loss of coolant accident, except perhaps at the very upper parts of the core, which are very low power regions, to have any kind of a reverse heat transfer.

MR. FORD: What I was asking was not what your expectation was.

MR. MOORE: Based on the analyses performed.

MR. FORD: What I am asking is whether the analytical models that you have, whether you have taken them and you assigned them the task of analyzing the sign of heat transfer coefficients during an accident. Have you ever taken the models and explicitly analyzed the question and the answer somewhere we can study it?

MR. MOORE: Certainly any heat transfer calculation performs part of the loss of coolant accident analysis. It takes the heat and the cladding and calculates the heat removed at the surface of the cladding. If the temperature gradient is in the reverse direction, heat will come back into the cladding in the analysis.

CHAIRMAN JENSCH: The question is, have you made any calculations that give you the prediction?

MR. MOORE: At some time or another I am sure we have had those kind of calculations. I think it is an academic question.

CHAIRMAN JENSCH: Academic, theoretical, realistic or anything. It is just a question. Do you have calculations that predict it?

MR. MOORE: Do I have calculations that predict reverse heat transfer during a loss of coolant, is that the question?

CHAIRMAN JENSCH: That is what I understand is the question.

MR. MOORE: No, I don't predict reverse heat transfer during loss of coolant.

MR. BRIGGS: May I ask a question, too?

MR. FORD: Certainly.

MR. BRIGGS: What is the quality of the steam leaving the highest power bundle during the loss of coolant accident? Just what is the quality of the steam? Do you know?

MR. MOORE: It is very close to one.

MR. BRIGGS: So it is less than one, is it?

MR. MOORE: If I recall, it just about reaches one as a maximum.

MR. BRIGGS: So on that basis the steam temperature is pretty much constant all the way through the bundle; is that correct?

MR. MOORE: Yes.

MR. FORD: Mr. Briggs' question anticipates a question of mine with regard to a statement on page 3-126 of

WCAP-7665, which explicitly talks about the steam that is leaving the top of the bundle in which it states, and I quote: "An unreasonably high effluent steam temperature was required from the amount of carry-over reported in Reference 3 for similar conditions."

In terms of your answer to Mr. Briggs' question, does that data on steam effluent temperature referred to here provide a basis for your answer, a contradiction of your answer or what?

MR. MOORE: Well, it is a basis for my answer. On page 3-126, "10 per cent of the coolant supplied to the inlet was carried over so that there was a steam-water mixture coming out of the bundle."

end

That's my point.

MR. FORD: What about the point about the unreasonably high effluent steam temperature? Is the analysis that you have sufficient, since you weren't specifically looking for it, and sufficiently able to distinguish in terms of the quality of mixture coming out, whether part of it is saturated steam and so forth?

MR. MOORE: I believe the reference in context, reading from page 3-123 on, is to try to correlate the amount of carry-over that was measured with the carry-over measurement system to a heat balance to see whether they made sense. There was some question about the previous carry-over measurements. When the improved carry-over measurements were made, they checked reasonably well with the heat balance. In order to get the carry-over that was previously measured and check it, you had to add, I gather from the statement, your heat balance effectively required and much higher steam temperature than you would have had expected.

MR. FORD: What I am concerned with is the question of whether or not this problem that occurs in estimating the carry-over water, whether this problem is due to the fact that this unrecognized heat condition in the effluent is possible. Is that possible?

MR. MOORE: I find it difficult to have superheated steam in contact with saturated or subcooled water entrained. Now, I don't think that condition existed.

MR. FORD: In terms of the thermal dynamic here and in terms of the time it would take to get, instead of superheated steam and jets of two-phase fluid ,in terms of the time it would take to get a homogeneous mixture there, is it unreasonable to have both of them existing as effluent from tops of bundles?

MR. MOORE: Yes, I believe that's unreasonable

MR. FORD: In terms of the thermodynamics here and in terms of the time it would take to get, instead of superheated steam and jets of two-phase fluid, in terms of the time it would take to get a homogeneous mixture there, is it unreasonable to have both of them existing as effluent from tops of bundles?

MR. MOORE: Yes, I believe that's unreasonable.

MR. FORD: Can you set forth experimental data pertaining to the time history of mixing between superheated steam of whatever the temperature is here, and the two-phase liquid of whatever the temperature is. Can you set forth experiments that explicitly address themselves to the time it would take to reach an amount of equilibrium for that flow?

MR. MOORE: I'm sure there must be experiments in the field of thermodynamics and heat transfer related to that, yes.

MR. FORD: The question was, can you set them forth?

MR. MOORE: No. I don't understand the relevance either to the line of questioning.

MR. FORD: I'm sorry.

Back to the line of questioning.

The temperature of the superheat steam, do you have any upper bound for that, what it could possibly be?

MR. MOORE: It won't be any higher than the peak clad temperature.

MR. FORD: In terms of simply the thermodynamics of superheated steam, what is the maximum possible temperature that you have as a temperature in superheated conditions?

MR. MOORE: Under what conditions?

MR. FORD: Simply under the conditions of the higher part of the bundle during loss of coolant accident. Is there any limit up -- I'm sure there is. What is the limit to the temperature that could occur?

MR. MOORE: It is a function of the heat transfer from the fuel rods themselves.

MR. FORD: I appreciate that. What I am asking is, as a cunction of the heat transfer from the fuel rods, the time involved and the velocity of the superheated steam, I am asking what is the upper bound for the temperature of the superheated steam?

MR. MOORE: As I said, we are very close to saturation at low flooding rates at the exit of the core. I don't have a number for how much above saturation I could be.

I think you have to be more specific under the conditions you are asking me to answer the question.

MR. FORD: Under the conditions that are listed, given your hottest rod, given the axial co-sign power distribution, given the velocity of the superheated steam, under those conditions, those aren't enough conditions, is that your problem?

MR. MOORE: If I look at the FLECHT results under those conditions, I still have steam - water discharging. Under the conditions of the hottest rod, the water, the heat transfer --

MR. FORD: In terms of the FLECHT results for specific bundle measurements of the quality of the fluid, can you cite in the FLECHT reports the question of whether or not there is superheated steam there that has been explicitly answered? Do they say they looked in every case and measured the quality of the effluent and made sure there was no superheated steam and used such and such a technique to decide that?

MR. MOORE: I don't believe I could find a reference to that directly. I can't directly right now.

MR. FORD: Just to have the record have some actual examples of negative heat transfer coefficient, I'd like to ask you simply just about a number of figures in the report, whether they show the presence of these negative heat transfer coefficients.

On WCAP 7665, I refer to page C-43. Does the data from that FLECHT run, does that indicate between zero and 30 seconds after flooding? Does that indicate the presence of a negative heat transfer situation?

MR. MOORE: Yes.

MR. FORD: The FLECHT then reported on page C-39, does that represent that situation? Is there a negative heat transfer coefficient there?

MR. MOORE: No.

MR. FORD: You don't think so?

MR. MOORE: No.

MR. FORD: On page C-21, that is, I take it, a clear negative heat transfer coefficient?

MR. MOORE: Which one?

MR. FORD: Page C-21.

MR. MOORE: Yes.

MR. FORD: In terms of the directions on the heat transfer coefficient indicated on the quotation earlier on page 42, it indicates that the superheated steam could have some effect on the value of the initial heat transfer coefficient. For the record, can you clarify the direction of the effect?

MR. MOORE: I believe that would give us a somewhat larger heat transfer coefficient. It would be an additional

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90 degrees or so below the clad temperature.

MR. FORD: You mean the superheat would contribute to a positive heat transfer coefficient rather than a negative one?

MR. MOORE: Yes. In this case, if the steam temperature is built below the clad temperature, the heat is coming from the clad to the steam.

MR. FORD: That is how you are interpreting it. The 90 degree difference, does that refer to the difference they are talking about between superheated steam in the clad or the difference at the start of the reflooding simply between clad and the initial temperature of the steam?

end

MR. MOORE: This is at the start of the reflooding where the steam temperature is below the cladding temperature.

MR. FORD: Right. Now this doesn't say here, am I correct, that the superheat temperature relative to cladding is in constant difference of a hundred? This is just initial conditions for this test.

MR. MOORE: That's right.

MR. FORD: Without reference to superheat.

Now, my question is in terms of superheat steam is it correct that if we had superheat steam in the accident as opposed to steam at lower temperature, that the effect of this would be to decrease the initial heat transfer coefficient as stated here.

MR. MOORE: Not the way we have used the data. I think that is where you are probably confused.

MR. FORD: Excuse me. Not the way you used the FLECHT data?

MR. MOORE: That's right.

MR. FORD: I see. So that the negative heat transfer coefficients here, you never actually plug these into your computer codes, is that correct?

MR. MOORE: Yes. Let me explain. We take the FLECHT heat transfer where we determine the heat transfer from a backward calculation of the heat balance. So that we know the heat into the rod. We know the cladding temperature and we assume for purposes of obtaining the data that the temperature around the rod is saturation temperature, not superheat. O.K.?

Now, we calculate then based on the heat input into the rod and FLECHT and its temperatures, the equivalent H or heat transfer coefficient to transfer that heat with a temperature gradient equivalent to the clad temperature minus the saturation temperature of the steam.

Now, when we do the analysis for the reactor we do it in a completely consistent way. We again take the hot channel, hot spot. We assume that the sink temperature is saturation and get the equivalent heat transfer coefficient in order to transfer the heat. This way we have obviated any consideration with respect to superheat per se.

MR. FORD: I see. Then in terms of the computer code then before you put in heat transfer coefficients am I correct you have already calculated what the sink temperature is?

MR. MOORE: Yes. For the calculation of the peak clad temperature we assume the sink temperature is saturated, that's right.

MR. FORD: No. That is what I am trying to get through. You say you assumed that the sink temperature is saturated.

MR. MOORE: Just that we assumed it was saturated for the FLECHT test in deriving the basic heat transfer coefficient in the first place. So we are consistent.

MR. FORD: I see. Yes. I appreciate the consistency. But the thing that interests me is to get clear on what it means to assume that the sink has a saturation temperature.

MR.MOORE: Oh.

MR. FORD: I mean does the computer code go through and before you put inheat transfer coefficients, before you do heat up does the computer code go through and compute what the sink temperature is or do you, as you seem to be indicating, at some point in the accident, assume such-and-such a sink temperature, take the heat transfer coefficients from FLECHT data pertaining to that sink temperature and then go on with the calculations? I am giving you a dichotomy.

MR. MOORE: It's the latter, in that we take the temperature according to the pressure that exists at the end of blowdown and during the reflood. We take that pressure and the saturation temperature associated with that pressure to get the heat transfer.

MR. FORD: Could you repeat that? You take the temperature associated with the pressure predicted, this average core pressure.

MR. MOORE: Yes. At the end of blowdown. It's

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effectively containment pressure.

MR. FORD: Do you assume the entire core is in thermodynamic equilibrium?

MR. MOORE: You are talking about the fuel at the cladding and everything?

MR. FORD: Yes.

MR. MOORE: No.

MR. FORD: I am talking about the state of the fluid.

MR. MOORE: The fluid is assumed to be saturated throughout, yes.

MR. FORD: When you compute the relationship between the temperature and the core pressure do you consider the subsaturation temperature metastable states, as Van der waal's equation of state?

MR. MOORE: Would you repeat the question, please?

MR. FORD: When you relate temperature to core pressure, you know, at the subsequent move before plugging a heat transfer coefficient do you in terms of the way in which you regard a thermodynamic state of the coolant, do you regard it as being in at the subsaturation temperatures as being in the metastable equilibrium that you get from Van der waal's isotherms?

MR. MOORE: No. For the analysis of the peak clad temperatures we assume it's as saturated.

MR. FORD: Yes. But do you understand what

TBm5

Van der waal's isotherm is?

MR. MOORE: We just follow the steam tables of temperature and pressure and thermo equilibrium.

MR. FORD: Yes. Do you understand what Van der waal's isotherm is?

end

T2Btl

MR. TROSTEN: Object to the question. The witness has been asked the question. He has answered it. Mr. Ford is asking the same question again.

CHAIRMAN JENSCH: Yes. I suppose it would be more direct if he'd answer yes or no.

The question was do you understand Van der wall's isotherms, and he purported to explain something. I don't know whether it had anything to do with Van der wall or not.

Do you understand Van der wall's isotherms or not? Yes or no?

MR. MOORE: No.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Thank you.

Now, can you tell me in general terms what the relationship is between core pressure and the formation of superheated steam.

MR. MOORE: We bring water in, sub-cooled. We heat it up. It flashes. It entrains water. You add additional heat. If you have enough heat to boil all the entrained water off and continue to add heat it will superheat the steam.

MR. FORD: My question was what is the general relationship between the quantity of superheat steam formed and the core pressure?

MR. MOORE: The core pressure determines where my saturation temperature is.

12Bt2

MR. FORD: So then what is the general relationship between superheat formation and core pressure?

MR. MOORE: I don't understand the question.

MR. FORD: Well, you have the logic for it, but I was just trying to make you go all the way.

Is it correct that the lower the pressure there is the great quantity of superheat steam, all other things being equal?

MR. MOORE: All other things being equal, yes.

MR. FORD: Thank you.

Now in terms of the range of pressures expected in containment, I think we got into this a little bit earlier, can you tell me what is the lowest pressure in the vessel during brief flood. What is the lowest pressure in the vessel that is consistent with all the experimental and theoretical data you have on that subject?

MR. MOORE: During reflood?

MR. FORD: Yes.

MR. MOORE: The lowest pressure in the vessel would be equal to the containment pressure plus the driving head associated with the hydrid downcomer which is about fifteen or sixteen feet of water. So it's within a few psi of the containment pressure. It's above the containment pressure. About seven psi.

MR. FORD: So in order to answer this you have to

T2Bt3

tell me or I might as well as what is the lowest containment pressure that would occur under variations in all the parameters which influence it?

MR. MOORE: Again, during the refloods? The reflood phase?

MR. FORD: Yes.

MR. MOORE: We consider that the containment pressure would not be below 80 per cent of the calculated rise in pressure due to the blowdown.

In other words, we take the peak pressure, calculated due to the blowdown and the pressure during the reflood will be a rise of 80 per cent of that additional rise. So that you will drop the pressure by that much.

MR. FORD: Now in terms of variations in parameters of blowdown, extremely slow blowdowns, extremely fast ones, and so forth, I understand the standard is sort of a medium speed blowdown. Now can you tell me in terms of the range of all of the various factors influencing blowdowns what is the lowest pressure psi that you will get in the containment during this period? I realize you say it will be 80 per cent of whatever the peak was, peak pressure was during blowdown itself.

Now given all the variations in that what is the lowest pressure that we will get to in the containment?

MR. MOORE: Given all the variations in what? I am having trouble following the question.

T2Bt4

MR. FORD: Given all of the variations and factors influencing the rate of blowdown, influencing the maximum pressure, at the highest pressure during blowdown.

MR. MOORE: Well, the peak pressure in the containment is not a very strong function of the blowdown time, because the blowdown time is relatively short with respect to any heat treasfer mechanisms in the containment itself, so if our blowdown in fifteen seconds versus eighteen seconds versus thirteen seconds, I don't expect to get a significant change of containment pressure.

MR. FORD: Well, let's suppose that we calculate containment pressure, assuming, for example, that all of the energy, all of the heat in the containment, is dissipated readily, just to put a lower bound or at least upper bound, I guess, on it. You have this lower bound on containment. If you create this lower bound on containment pressure do you have anyscoping calculations which would indicate what that greatest lower bound would be?

MR. MOORE: We have determined that containing pressure is a function of the heat sinks and to some extent then early in the transient the engineers' safeguards come into play and the number I have quoted for the 80 per cent factor for containment pressure assumes greater heat transfer capability in the containment than is normally provided or normally taken credit for for the design.

T2Bt5

In other words, we have overestimated the amount of heat removal in the containment to do just what you are saying, determine a lower bound for pressure, and that is the basis for the 80 per cent.

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MR.FORD: Now what does this come out to in psi? What is the lowest value of this lower bound out of all calculations that you have done of different accident situations?

MR. MOORE: It was no lower than the 80 per cent number.

MR. FORD: Yes, I realize 80 per cent of another pressure, but what is the psi?

MR. MOORE: In psi? I'd have to check the containment design pressure. I could get the number if you want to take the time. It's there.

MR. FORD: We will be happy to just let you give us the number during the break or later.

But in terms of the general relationship between superheat formation and core pressure is it clear that the lower the pressure the more superheat you would be expected to have, all other things being equal?

MR. MOORE: Yes.

MR. FORD: In terms of the possible eutectic formation during the course of a loss of coolant accident did the tests that you performed on eutectic formations involve study of superheat effects?

MR. MOORE: No. As described in the report they were heated up in air and it was an attempt to get a larger than expected time at peak temperature. Larger than expected during loss of fluid accident.

MR. FORD: In terms of the ability of superheated steam liquid droplets to exist in the channel, would you refer to page 3-117 of W 7665. Is it correct as it's stated here that, "The amount of initial superheat increased with initial clad temperature but lacked the clad temperature by several hundred degrees. During the run superheated steam was found to be present for long periods of time, indicating that the coolant was a non-equilibrium mixture of steam and liquid droplets."

Do you agree with that? Is there a qualification you would make to that?

MR. MOORE: I have no reason to dispute that. I agree with that.

MR. FORD: Is it correct that as observed on page 3-123 'that the presence of superheated steam during the run was consistent with the negative heat transfer coefficient calculated by the datar code to the 10-foot elevation in some runs? The peak steam temperature and the time of superheat observed tended to increase with decreasing flow rate and increasing blockage." Excuse me. That was decreasing flooding rate and increasing blockage.

MR. MOORE: Yes.

MR. FORD: Have you analyzed in the flow blockage tests that you have performed, have you specifically analyzed the relationship between the increase in blockage and the superheat observed?

MR. MOORE: Have we related a lot of superheat to amount of blockage?

MR. FORD: Yes.

MR. MOORE: Not directly, to my knowledge. We were mainly interested in the heat transfer coefficient.

MR. FORD: Now this date indicates that superheat increases would increase in blockage. Can you tell me do you have any data pertaining to the flow channel blockage that disputes this statement?

MR. MOORE: No. I think the pertinent information is the heat transfer you get at the hot spot with blockage, which was measured directly. As I said before, we have not wanted to get into a complication with respect to superheat, so we have produced the data consistently and used consistently for cooling analyses just for that very reason.

MR. FORD: Am I correct that in terms of the multi-rod burst tests that you have done that the blockages and the swelling, ruptures, that that would be expected along the one-seventh of the axial length of the rod located near the middle of the hottest section?

Is that correct?

MR. MOORE: Well, I can't tell whether you are asking a broader question. We expect the blockage to occur over a one to two feet section because of the power distribution.

MR. FORD: Right. At the highest parallel of the rod.

MR. MOORE: Yes.

MR. FORD: So that the statement here that increasing blockage increases the amount of superheat formation, that means that there can be, therefore, superheat formation near the maximum temperature point because that's where the blockage is.

MR. MOORE: Perhaps. But apparently the results show this was an improvement in heat transfer.

MR. FORD: I see. Now, in terms of blockage being described here is the datar code talking about the particular orifice that you used to simulate blockage or are they talking about blockage in general resulting actually from different geometry of rod swelling and rupturing?

MR. MOORE: Any references to the datar code would be the specific data obtained in our own blockage test.

I think the answer is yes, if that was the question.

end

MR. FORD: In terms of the thermodynamics of the conversion of uranium dioxide to U308 during course of loss of coolant accident, with superheated steam, would that increase that reaction rate or decrease it?

MR. MOORE: I really can't comment on that. That is a subject that you went into at some length with Dr. Roll earlier.

MR. FORD: Thank you.

Does the flooding rate vary with time and actual loss of coolant accident situation?

MR. MOORE: Yes.

MR. FORD: In terms of the variable flooding rates that were used during the FLECHT test, is it correct that in only seven of the 73 tests that variable flooding as opposed to constant rate flooding was used?

MR. MOORE: I presume your numbers are correct. They are all reported in the report.

MR. FORD: Is it correct that the flooding rate in the data here has been indicated as influential in the formation of superheat?

MR. MOORE: There is a tendency to form superheat at low flooding rates because of just the heat input along the channel.

MR. FORD: Is it correct in this regard as analyzed by page 340 of WCAP-7665, that, "Negative heat transfer coefficients were generally found at the ten-foot elevation below flooding rate runs two inches per second or less at early times from around five to a maximum of around 120 seconds after the flood"?

MR. MOORE: Yes.

MR. FORD: Can you give us the equations that were used to predict the variable flooding rates used in the FLECHT test?

MR. MOORE: Which predictions are you referring to?

MR. FORD: In the Idaho nuclear report, their overview report of the FLECHT test, Document IN-1386 entitled, "Pressurized water reactor full length emergency cooling heat transfer,BWR test FLECHT project, April 1970." They gave a figure 5 predicting the variable flooding rate versus time. I have two questions:

First, what is the basis of this prediction? Secondly, is this the variable flooding rate used in the test, because the data that I have just says flooding rate variable. It doesn't give it.

MR. MOORE: May I see the reference, please?

MR. FORD: Sure.

MR. MOORE: You are referring to figure 5 which is a predicted reactive variable flooding rate versus time?

MR. FORD: Yes.

MR. MOORE: I don't really know who predicted that particular curve.

MR. FORD: I don't know who did. The thing I am concerned with is, I cannot find, in your own FLECHT reports, any clear indication as to either how, A, you predict flooding rates during the accident, or, B, how you predict flooding rates for the FLECHT test. I am looking for data in this area.

MR. MOORE: I see.

MR. FORD: Can you respond in a general way to that concern?

MR. MOORE: Yes. The flooding rate prediction for the reactor is described in some detail in the July 13th submittal. What we do is calculate the head generated in the downcomer, the height of water. The height of water in the downcomer acts as a force to drive steam-water through the core up through the hot legs, through the steam generators, back through the pumps, in the annulus and out through the break. So there is a relationship between the driving head generated by the downcomer and the amount of mass or steam flow that you can push through with that pressure drop. So we calculate using the assumptions indicated in the report, a low value or a lower value than expected for the flooding rate into the core. From that we determine the heat transfer coefficients using the basic FLECHT data. The FLECHT data was determined on a parametric basis and was not intended to be any specific representative flooding rate versus time transient.

MR. FORD: So that when you use the FLECHT data, the main determinant of which FLECHT heat transfer coefficient you use is what core pressure that you predict for reflood; is that correct?

MR. MOORE: No. Pressure per se is not the significant parameter with respect to FLECHT transfer. It has a related effect in that the pressure determines the velocities, and therefore the pressure drops around the system. So that the pressure you have in the coolant system determines the density of the steam. For a given mass flow, determines the velocity of the steam. That's mainly the effect of pressure on flooding.

MR. FORD: As I understand your calculations, is it correct that when you compute core pressure at the beginning of reflood in order to select the temperature and maximum cladding and in order to select the heat transfer coefficients, that is the pressure that you compute for the hot spot and is the same pressure along the entire length of the rod; is that correct?

MR. MOORE: Yes.

MR. FORD: So when you choose the heat transfer coefficients, do you use the same heat transfer coefficient over the entire length of the rod?

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MR. MOORE: Yes.

MR. FORD: So that the heat transfer coefficients that you derive for different axial levels in the FLECHT data, you don't actually heat them in the computer code; is that correct?

MR. MOORE: That's correct, we do not.

MR. FORD: I see.

MR. TROSTEN: Mr. Chairman, while Mr. Ford is preparing the next question, I wonder if perhaps under this type of highly technical questioning it wouldn't be preferable if we could have a break for the witness every hour or so for five minutes.

CHAIRMAN JENSCH: Certainly, anything that will be a revival activity for lawyers as well as the witness will be appreciated. If you can tell us what the computer time is in your code, we will be glad to run through our code.

At the moment, let us recess and reconvene in this room at 4:25.

MR. TROSTEN: Thank you very much, Mr. Chairman.

MR. MOORE: Thank you, Mr. Chairman.

end

(Recess.)

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CHAIRMAN JENSCH: Please come to order.

Mr. Moore has resumed the stand. Are you ready to proceed?

MR. FORD: I will be ready in just a second. Excuse me. I am making a diagram.

I am ready to proceed.

I have reproduced on the paper here Figure 3 from the Idaho Nuclear Report 1386, Pressurized Water Reactor for Full Length Emergency Core Cooling Heat Transfer Tests Project Report. The caption for Figure 3 is Heat Transfer Coefficient for PWR FLECHT Test Conditions Representative of Those in Large Pressurized Water Reactors During A Loss of Coolant Accident, and I will give the figure to Mr. Moore to ask him to confirm whether more or less I have properly drawn and have not misrepresented the figure in any ghastly way.

MR. MOORE: No. I agree.

MR. FORD: Fine.

CHAIRMAN JENSCH: In case there is any doubt, use the one in the book, of course.

MR. FORD: Right. I am sure Mr. Moore would use the one in the book. This is so everyone else can follow what we are discussing, and of course, if I were to expand this chart to take into consideration negative heat transfer coefficients we would be going in this direction here.

Now, I would refer to the FLECHT Test Report, page 340, which I have quoted a number of times.

end

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MR. MOORE: Yes.

MR. FORD: It mentions that negative heat transfer coefficients were generally found at the ten-foot elevation for low flooding rate run, two inches per second or less at early times, from around five up to a maximum of about 120 seconds after theflood.

This means that, in terms of this graph, in terms of point of reference of typical values for the heat transfer coefficients--am I correct that this means that this curve here, about, I believe, five seconds after the flood to up to a maximum of 120, that this entire curve would be below the zero point for the entire range given at Idaho Nuclear of typical heat transfer coefficient values code?

MR. MOORE: The question is if we had a negative heat transfer coefficient as defined in the report?

MR. FORD: Yes. My question is, in terms of how I represent this negative heat transfer coefficient, in terms of the range of typical coefficients in typical conditions that have been given at Idaho Nuclear summary, PWR FLECHT data, if I wanted to represent this typical heat transfer coefficient that is cited in the document, is this an accurate representation of approximately the way it would look on the curve? I mean its relationship to the rest of the data.

Is that correct?

MR. MOORE: I really don't know. It is in the right

WWt2

sign. I don't know about the magnitude compared to the others.

MR. FORD: Fine. Simply in terms of the fact that is it clear that in terms of the definition of the maximum length of time over which this negative heat transfer coefficient was reported, is it clear that that length of time goes the entire length and more of the typical way in which heat transfer coefficients are reported?

DR. GEYER: Is the bottom dotted curve for the upper portion of the core, the top of a rod?

MR. FORD: Yes, sir. No. It is for the ten-foot elevation. In terms of the claims in which the core is divided, as indicated on Page 340 here, this is the ten-foot elevation for a specific FLECHT test.

DR. GEYER: Thank you.

MR. MOORE: I refer to the figure on Page 342 of the FLECHT report.

MR. FORD: Yes.

MR. MOORE: The top figure, the flooding rate of one inch per second. That should be the ten-foot elevation as shown there with a very slightly negative coefficient. Then it rises, as you can see.

MR. FORD: That's true. It is negative until the point of, approximately, am I correct, one hundred seconds after the time of reflooding?

MR. MOORE: Yes.

WW3

MR. FORD: My point is, that is another case in which the coefficient is negative for one hundred seconds. Does that contradict this at all?

MR. MOORE: Yes. I'm trying to get this in the right perspective on the scale, that's all.

MR. FORD: Your test here is negative for one hundred seconds; is that correct? I am talking about one hundred and twenty.

MR. MOORE: You are drawing a curve with respect to the Idaho report which is a flooding rate of six inches per second. You see,their ten-foot elevation never had negative coefficients. You are comparing that to--you see, they did not have a negative coefficient.

MR. FORD: Excuse me. If I am referring to the chart, can I point here and ask you whether or not that is below--

MR. MOORE: That is what I am talking about. The Idaho report that you have sketched up there--

MR. FORD: That's true. Excuse me. Can I explain my point in putting on this Idaho chart? My point is to show that when we get to negative heat transfer coefficients, it is into just a tiny dip that is reported in the pages I read you earlier for negative heat transfer coefficients; that you can get negative heat transfer coefficients all the way across the criticial initial one hundred seconds or so after reflooding; is that correct? Your additional case, I believe, is a second

WWt5

instant of this negative heat transfer coefficient for a long time, the case on Page 342.

MR. MOORE: Yes.

MR. FORD: Thank you.

CHAIRMAN JENSCH: Which question did he answer? You gave him two. I think he may have had a second question in mind. The first question was, would you get a line all across for the time scale depicted with a negative coefficient.

MR. MOORE: The answer is yes. Instead of using the freehand curve, let's use the one where it actually exists in the test.

CHAIRMAN JENSCH: Very well.

MR. FORD: Fine.

In terms of our discussion before the break, in which you explained how you selected these transfer coefficients, is my understanding correct that you, first of all, compute average core pressure; is that correct?

MR. MOORE: Yes.

MR. FORD: Then the model assumes that that same core pressure is the same along the entire length of the rod; is that correct?

MR. MOORE: It assumes it is and it also is very similar across the length of the core.

MR. FORD: Then the model goes and chooses a heat transfer coefficient; is that correct?

WWt5

MR. MOORE: Based on a flooding rate, yes.

MR. FORD: And based on the temperature that exists at the hot spot with the pressure that you have calculated; is that correct?

MR. MOORE: I get a heat transfer coefficient from the flooding rate. I have the saturation temperature at the hot spot.

MR. FORD: Is it correct that in the sequence you compute the pressure and then you compute what temperature would exist with that pressure, and then knowing that saturation temperature, you go and get a heat transfer coefficient; is that correct?

MR. MOORE: Yes.

MR. FORD: And the heat transfer coefficient you pick since you are concerned with the maximum temperature, and that is the coefficients along the six foot elevation; is that correct?

MR. MOORE: Yes, that is associated with the hot spot.

MR. FORD: You assume, when you pick the temperature coefficient, do you not--when you pick the heat transfer coefficient, rather, that you apply the same heat transfer coefficient over the entire axial length of the rod; is that correct?

MR. MOORE: Yes.

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MR. FORD: So that what you do, referring to these heat transfer coefficients here, you pick, at a different time after the beginning of reflooding, that at all lengths of the rod you would be at whatever heat transfer coefficient on the six-foot elevation that corresponds to that time after reflood; is that correct?

MR. MOORE: I'm sorry. Would you repeat that?

MR. FORD: Yes.

My question is, when you go and chose a heat transfer coefficient, you go and you look at what time after the flooding is for a chart like this computed for a given flooding rate. If it is thirty seconds after the flood, it would have a heat transfer coefficient of about thirty-three; is that correct? That is just in terms of using this chart.

MR. MOORE: Yes. If you had the exact flooding rate condition, that's right.

MR. FORD: If you had the exact flooding rate?

MR. MOORE: Yes.

MR. FORD: According to the FLECHT tests, is it correct that at some elevations there would be much higher heat transfer coefficients than the one you are actually using in the calculations; is that correct?

MR. MOORE: Yes.

MR. FORD: Is it also correct that at higher elevations there will be much lower heat transfer coefficients than

WWt7

the one you actually use?

MR. MOORE: Yes, they could be lower.

MR. FORD: Is it possible, under the flooding rate assumptions, the low flooding rate assumptions of about two inches a second, is it possible that you are using a positive six-foot elevation heat transfer coefficient when in fact it is predicted from the FLECHT data that the heat transfer coefficient for that axial level would be negative?

MR. MOORE: It is possible in the very low power regions of the core, yes.

MR. FORD: Is it a correct interpretation of this data and of the more accurate data that you have in front of you, that heat transfer coefficient is a clear function of axial level; that as you increase the axial level you greatly decrease the heat transfter coefficient?

MR. MOORE: I guess I quarrel with the representation of "greatly decreased," but it is decreased.

MR. FORD: In terms, specifically, of the more accurate data that we have here--let us read it together. Is it clear that the heat transfer coefficient at the two-foot elevation, approximately ten seconds after the accident, is around sixty Btu's per hour per square foot? Is that correct?

MR. MOORE: That's correct. We have quenched.

MR. FORD: Is it correct that the heat transfer coefficients two feet up are only ten Btu, or a fact of six lower

WWt8

than the heat transfer coefficients for the second foot elevation?

MR. MOORE: At that point in time, yes.

MR. FORD: Is it also possible, from the data that we have reviewed on superheat, that as you get up to the ten-foot elevation, that you're actually talking about potentially negative heat transfer coefficient?

MR. MOORE: We are talking about potentially reverse heat transfer over a part of the transient, yes.

MR. FORD: So in terms of this curve, the ten-foot elevation would be represented on the other size of zero?

MR. MOORE: As shown on the curve on Page 342.

MR. FORD: Can you explain what calculations you have done to determine the effects and sensitivity of maximum clad temperature to this use of an intermediate hea transfer coefficient along the entire axial length rather than to use specific heat transfer coefficients for all the specific axial lengths.

MR. MOORE: The primary concern is the behavior of the peak cladding temperature as a function of time. So that is the calculation that we want to get closely represented, representing the transfer period.

The regions at the higher elevations are at very low power levels so we are not concerned about the specific temperatures of the cladding in that region. I think it's of interest to look at the figures on page 343 of the FLECHT report.

MR. FORD: Yes.

MR. MOORE: The top figure shows the temperature rise that occurs during the reflood. If you will look at the triangles depicted there, the triangles are for a low flooding rate, one inch a second, which is the only kind of flooding rate where you may get this reverse heat transfer effect. You notice that the peak rise in temperature at the ten-foot elevation is about 650 degrees Fahrenheit, whereas the peak rise at the six-foot elevation where the peak power occurs is about 450 degrees Fahrenheit. So we have picked up an additional 200 degrees or so increase in temperature, which is insignificant with respect to the initial temperature of the cladding in that region because it's at a low power level.

MR. FORD: Yes. Now, you indicated in describing the data that it's only at a flooding rate of an inch a second that we get the superheat phenomenon that I am discussing. Is that what you just said?

MR. MOORE: Well, I am just looking at this data in front of me. If you look at the squares on the same curve and the temperature rise you notice that the ten-foot elevation, the rise is less than the rise.

MR. FORD: I see.

CHAIRMAN JENSCH: Wait a minute. I wonder if we could have the question answered. Is it true that the one-inch per cent reflooding rate is the only time that you are going to get the superheat phenomena?

MR. MOORE: Loading flood rates like one inch a second. Not exactly one inch a second and only one inch a second, but low flooding rates.

My point, Mr. Chairman, was we have curves here for 1.9 inches a second, which show a very small effect, and it looks like we probably didn't have this reverse heat transfer.

MR. FORD: Isn't it correct in the previous quotation at page 340, we were talking about a flooding rate of two inches per second or less and we had negative heat transfer coefficients for 120 seconds there?

MR. MOORE: It's not clear from that statement that the 120 seconds applies for two inches. It could apply for the "or less" flooding rates. As would be seen in looking at the curves on 343.

MR. FORD: I see. Now, the codes that you have from a mathematical point of view, are they capable of giving us the simplification of using simply one average heat transfer coefficient? Do they have the capability of incorporating all of the heat transfer coefficients from the FLECHT data?

MR. MOORE: Yes.

MR. FORD: Have you performed any calculations that used all of the heat transfer data from FLECHT rather than just from the six-foot elevation data?

MR. MOORE: Not that I am aware of.

MR. FORD: Now, in terms of the proprietary report that we were discussing this morning, is it correct that Point 3, your change in the design basis calculation, your proposed changes in the design basis calculation, based on the use of FLECHT heat transfer, am I correct in that this proposal of yours was not accepted?

MR. MOORE: You are incorrect.

MR. FORD: This proposal of yours was accepted?

MR. MOORE: Yes.

MR. FORD: Can you explain to me the computation performed on FLECHT data to derive the heat transfer correlation, the heat transfer coefficient, rather?

MR. MOORE: I am sorry. Which computations?

MR. FORD: I believe that in order to get the heat transfer coefficients from all of the measurement equipment readings during the tests that you calculated the heat transfer coefficient as a function of, a) given rod power, and, d), measured clad temperature, is that correct?

MR. MOORE: That's correct.

MR. FORD: Is the characterization of these computations given in the Idaho nuclear report IN-1386? Is the characterization given on page 22 accurate? I will both read it and allow you to study it. And by the subcontractor PWR FLECHT, they are referring to Westinghouse Electric Corporation. It says, and I quote, "The subcontractor for the PWR FLECHT project is processing the experimental data and analyzing the test results for a given set of experimental conditions, broad data in the form of rod power and temperature measurements at the insulation cladding interface is used in standard heat conduction equations to determine the heat flux of the insulation cladding interface, and to describe the temperature distribution of the heater rod. By solving the heat reduction equations for the cladding, the temperature heat flux and coefficient of heat transfer at the outer surface of the cladding are studied."

Would you care to study this or is it clear?

MR. MOORE: Yes.

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MR. FORD: Now, the Idaho Nuclear proposes another method or discusses another method, I should indicate more precisely, for calculating heat transfer coefficients in their later document IN-1390 titled EXPERIMENTAL RESULTS OF FUEL HEATUP SIMULATION TESTS TO EMERGENCY CORE COOLING TESTS SERIES on page 24. And they make the following statement as the alternative method of calculating heat transfer coefficient, and I quote.

"The transient nature of this experiment makes the heat transfer analysis very complex. A detailed analysis would require the coupled thermal hydraulic models that are capable of handling the heat thermal transient, the transient cooling thermal properties, and the nonequilibrium which exists between the coolant phases, steam and water. Another type of analysis that could be used is the empirical correlation of surface heat transfer coefficients or heat flux as a function of systems parameters such as initial temperature, inlet water temperature, inlet flow rate, rod power output and axial position."

end

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MR. TROSTEN: While Mr. Moore is studying that, what is the outstanding question? You have read an excerpt from this Idaho Nuclear Report.

CHAIRMAN JENSCH: I don't think there is any. I think he is waiting till he studies it.

MR. FORD: No, no. I am waiting till he studies it.

MR. MOORE: Go on.

MR. FORD: My question is if we compared the two different ways of computing heat transfer coefficients from experimental data, the empirical correlation that was used, given the broad data observation versus the coupled thermo-hydraulic models capable of handling the heat thermo transient, transient cooling thermal properties and the nonequilibrium which exists between the coolant phases from a theoretical point of view, which of these two methods of calculating heat transfer coefficients could we expect to be more accurate and to more closely simulate the conditions?

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on the grounds of no showing of relevance, Mr. Chairman.

MR. FORD: The relevance, I believe, is clear. There are two methods of calculating heat transfer coefficient. One was used, one wasn't, and I'd like to ascertain the justification for the specific technique that was used, as my first question here concerns simply from a theoretical point of view which of the two techniques simulates more closely the phenomenom that we are concerned with?

CHAIRMAN JENSCH: I don't think that was quite the question that was propounded, however. May we have the last question previous read.

(The last previous question is read by the reporter.)

CHAIRMAN JENSCH: Well, the Board is having some difficulty with the form of the question and I think it's incumbent upon this witness to support the method he has used, and presumably his data has been directed to that thesis. There is a little complication in this situation, because as I understand some of the statements just made by the interrogator, Westinghouse is a subcontractor to Idaho Nuclear, which is the main contractor, and it isn't as if Idaho Nuclear were some distant stranger to the transaction in relationship to Westinghouse. So there is some relationship between the two theories, presumably.

But let me before the Board makes a ruling _voir dire_, if I may, the witness a bit.

Is it your view, Mr. Moore, that the method of computing the heat transfer coefficient more closely simulates the conditions likely to be encountered in the loss of coolant accident than other theories might be?

MR. MOORE: Yes.

CHAIRMAN JENSCH: And without analyzing somebody else's theory, unless you have, you would prefer your theory to other theories, as far as you know them, is that correct?

MR. MOORE: Yes. In the context of getting something usable and that can be applied in an engineering sense.

CHAIRMAN JENSCH: On that basis the Board will sustain the objection, but believes that the inquiry within the range of the voir dire would be appropriate inquiry.

MR. FORD: Can you tell me does the model that you use to calculate the heat transfer coefficient consider the transient coolant thermal properties?

MR. MOORE: No.

MR. FORD: And does it consider the non-equilibrium that exists between the coolant phases?

MR. MOORE: No.

MR. FORD: Does it consider the non-equilibrium which exists between saturated steam on the one hand and a combination of steam with entrained water droplets on the other?

MR. MOORE: As I told you before, it's derived, the heat transfer coefficient is derived on the basis of a saturation temperature. These effects are not included.

MR. FORD: Can you tell me in terms of the variety of thermal dynamic complexities which are not simulated in the empirical computation method that you used, can you tell me what estimate is there, what well-supported estimate is there of the difference in a method which considers these non-equilibrium thermal dynamic conditions and your method which does not?

MR. MOORE: One must consider the use of the data, its intended use. The purpose of these experiments is not to derive a theoretical derivation of this complicated heat transfer phenomena. It is to derive appropriate heat transfer relationships for the use in the loss of coolant analysis. The results have been derived on the basis of saturation temperature assumptions and have been applied consistently on the same assumption to the reactor situation, and we feel this most closely then represents the expected condition in a reactor.

end

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MR. FORD: Is this scenario correct: The data that we have from the FLECHT tests consist of the raw data at Idaho Nuclear Hoted rod power under one hand, and measurements of cladding temperature on the other? Is that the data that you have when your FLECHT tests are finished?

MR. MOORE: And in the flow rate, yes.

MR. FORD: And the flow rate?

MR. MOORE: Yes.

MR. FORD: So in the data, the raw data that comes out of the FLECHT tests are heat transfer coefficients part of the raw data?

MR. MOORE: In my opinion, I would say yes.

MR. FORD: When I asked you for raw data, I think there were three pieces of data listed. Was one of them heat transfer coefficient?

MR. MOORE: We are arguing about semantics now. The heat transfer coefficient is directly calculated from raw data, directly calculated.

MR. FORD: Directly calculated from raw data?

MR. MOORE: Yes.

MR. FORD: Is it correct that between the raw data and the final heat transfer coefficients that we get, that assumptions with thermal dynamic consequences or thermal dynamic assumptions are involved in intermediate steps between the raw FLECHT test data and final heat transfer coefficients that we get?

MR. MOORE: Which assumptions are you referring to?

MR. FORD: I mean, for example, assumptions with regard to the coolant thermal properties in the non-equilibrium that exists. Do you have to make some assumption on those parameters in order to calculate heat transfer coefficients?

MR. MOORE: Yes. As I said before, we use saturation conditions.

MR. FORD: So that whereas you would say, for example, that under certain flooding rates at the 10-foot elevation, there will be superheated steam. Nevertheless, when you calculate the heat transfer coefficient there, is it correct that you pretend as if it were a saturated liquid?

MR. MOORE: Yes, and as we would also apply it to a reactor calculation.

MR. FORD: Can you tell me, when you make this assumption that you have a situation where you have a superheated steam and instead you assume that it is a saturated liquid, can you tell me what kind of error you introduce into your heat transfer coefficient?

MR. MOORE: With respect to my use of the correlation, I only have my measurement. I am using the correlation consistently.

MR. FORD: In terms of the assumption that you have a measurement of the rod power under one hand and a measurement of the interface of the rod and the cooling sink, and the third thing you do is make an assumption about the thermal dynamic stage of the coolant, of the sink, are you saying

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that calculation of heat transfer coefficient you come up with is insensitive to whatever assumption you make about the thermal dynamic state of the coolant sink?

MR. MOORE: No. It is sensitive to the assumptions for the sink.

MR. FORD: So if you made the assumption that it was superheated steam versus the assumption that it was saturated liquid, you would compute a different heat transfer coefficient; is that correct?

MR. MOORE: That's correct, but that's not relevant to our situation.

MR. FORD: I'm simply talking in terms of general terms of how this algorithm translate raw data into heat transfer coefficients. Relative to our situation, is it correct that the heat transfer coefficient that you gave us at the ten-foot elevation, the negative one, is it correct that they were computed assuming that there was a saturated liquid at that level rather than superheated steam?

MR. MOORE: Yes.

MR. FORD: If you were to assume that it was superheated steam, what would the heat transfer coefficient have been? What direction would it have changed in? Would it have been more negative?

MR. MOORE: It would have been improved heat transfer, higher coefficient.

MR. FORD: Can you explain that to me, please.

MR. MOORE: Certainly. If the temperature of the cladding in the test is at, say, one thousand degrees Fahrenheit, if the assumption is made that the steam temperature is saturation at 600 degrees Fahrenheit, then the way the calculation is performed, we will get a heat transfer coefficient which will be based on the power going into the rod with that delta T of 400 degrees Fahrenheit. Whereas, if in fact the delta T, in that the steam temperature was 1200 degrees and transferring heat to one thousand degrees cladding, you would have to get a higher heat transfer coefficient in order to transfer the same amount of heat because the delta T has been reduced.

MR. FORD: So that in terms of the assumption that you make regarding the thermodynamic state of the coolant, it is possible for you to compute negative heat transfer coefficients and indicate the superheated steam when in fact there is no superheated steam?

MR. MOORE: I don't think I follow that. I don't think that's the case, no. If we don't have superheated steam and the temperature of the clad is higher than the temperature of the steam, we don't get this negative, quote, heat transfer coefficient.

MR. FORD: Excuse me.

Can you tell me, is there any thermodynamic assumption that you could make that would increase the heat transfer coefficient? You assumed it is saturated liquid. If you assumed it is subcooled liquid, what happens to the heat transfer coefficient?

MR. MOORE: The way we compute it from the data?

MR. FORD: Yes.

MR. MOORE: The heat transfer coefficient, to transfer the same amount of heat would be smaller if you had subcooled liquid than if it was saturated.

MR. FORD: So that, am I correct, according to your analysis, that assuming two pieces of raw data constant, namely, the axial rod power given at axial level, and its temperature at axial level, the changing thermodynamic assumptions from subcooled through saturated through superheated changes as you progress through those thermodynamic assumptions, and you progress from smaller to larger heat transfer coefficients; is that correct?

MR. MOORE: That's correct, to transfer the same amount of heat.

MR. FORD: A model that considered the coolant thermal properties at nonequilibrium, is it correct that that model will compute more accurate transfer coefficients than your model which assumes one thermodynamic state and applies to all of the raw data?

MR. MOORE: More accurate with respect to what?

MR. FORD: That the coefficients, the large coefficients that you compute, assuming saturated liquid, that would not be computed if you had assumed subcooled liquid.

MR. MOORE: If we are talking about just a pure number, that's right. That's not relevant on two counts. We don't have saturated liquid or subcooled liquid at the hot spot. We use the same assumption to derive the heat transfer in both cases, the reactor and the FLECHT test. So we are not looking for a pure scientific number here.

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MR. FORD: I understand that you have geared the algorithm to the hot spot. But I am concerned with the significance of all of the rest of heat transfer coefficients for different axial levels. I am correct that in all of the different axial levels you assume the same thermal properties and same equilibrium stated in the coolant as you do in the hot spot for the 6-foot elevation calculation?

MR. TROSTEN: I object on the grounds of lack of materiality, lack of relevancy, and notwithstanding the scope of the voir dire.

CHAIRMAN JENSCH: May we have that question reread, please.

(The last question was read by the reporter.)

CHAIRMAN JENSCH: The objection is overruled. While there is a pause for this purpose, let me inquire.

*** I was wondering whether this step in Idaho Nuclear 1386 -- or is it Idaho Nuclear 1390? Does that constitute a report in some sort of sequence from the work that was done by Westinghouse? so that the Idaho Nuclear Report really represents the final determination of the work that was done by Westinghouse, and thereby would in a sense precede it? Is there some contention of that fact by the Intervenors?

MR. FORD: I'd like to take a look to answer the question in detail. I might explain there are a number of complex relationships between the Idaho Nuclear Report and the actual PWR FLECHT report. One relationship seems to be in terms of time. The final FLECHT report seems to come out a good bit after the actual tests that are conducted. The Idaho Nuclear tests seem to come out in closer time to the actual tests.

As I understand the relationship between Idaho Nuclear and the FLECHT program, that the over-all design responsibilities for the program rest with Idaho Nuclear as the main contractor.

CHAIRMAN JENSCH: What I had in mind was, does Westinghouse have a worker in the field, in a sense, and Idaho Nuclear, kind of the master designer of the thing, and whether Westinghouse likes it or not, Idaho Nuclear has determined how it should be handled and that thereby gives its conclusion, i.e., Idaho Nuclear's conclusions as to what theory should be applied to the data. I don't know whether these factors are reflected in any other documents or not.

If Westinghouse is kind of a worker in the field and gathering data but going off on a frolic of its own, if I use the term, to say that the theories it is going to concoct are more applicable than the fellow doing the work, then you kind of wonder if the worker in the field was having his own revolution against the establishment and that sort of thing. I don't know whether there is some necessity of Westinghouse complying with the directions of the group for which it was doing its work.

MR. FORD: I think there is a definite hierarchy in terms of theoretical responsibility between Idaho Nuclear and Westinghouse, and there is either a hierarchy above that, namely that the Idaho Nuclear work is part of the Atomic Energy Commission's water reactor safety program, its full relation in reactor development technology.

I read you the paragraph at the beginning of report, IN-1386, which may answer some of your questions. This is page 1. It says, "The PWR FLECHT PROJECT is an experimental project designed to provide data necessary to determine emergency core cooling system performance following a loss of coolant accident. The project, which is part of the water reactor safety program of the Atomic Energy Commission, will provide data from which heat transfer core correlations can be developed to predict the thermal response of PWR cores from the time the emergency core coolant fills in lower core plenum until the core is reflooded. Westinghouse Electric Corporation is conducting the test program under subcontract to the Idaho Nuclear Corporation."

And there is a further use for the FLECHT test. The Idaho Nuclear Corporation has a program which is called "Technical Assistance in Reactor Safety Analysis." It is reported in the document, IN-1383. The purpose of this program is for the Idaho Nuclear Corporation to provide, and

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I quote on page 1, "Analytical assistance to the AEC and its Regularity Agencies."

It goes on to describe in this test how part of the purpose of the technical consulting that Idaho Nuclear performs is to review data from a variety of programs, which include semi-scale blowdown and emergency core cooling, the pressurized water reactor, full length emergency cooling, heat transfer fests, the PWR FLECHT test, the Carolina - Virginia tube reactor in plant testing, and so forth.

So that part of the responsibility that Idaho Nuclear, as I understand it, from a specific document, is to review the data that becomes available from the water reactor safety program, to analyze it from a theoretical point of view and to try to conclude, in terms of its advice to the Regulatory Staff, to conclude whether or not data is sufficient to provide the requisite information required for the Staff to evaluate.

I intend, in my cross-examination of the Staff, to discuss this entire relationship and to discuss the manner in which Idaho Nuclear, when and after reviewing all of the safety data, comes to the conclusion that information on heat phenomena associated with the loss of coolant accident is not yet available.

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CHAIRMAN JENSCH: Excuse me for interrupting. Could we go back and have the question read.

MR. MOORE: Mr. Chairman, I would like to clarify the record. Mr. Ford is perhaps somewhat misled. The report we referred to--we started on this discussion on the approach to be used to reduce the data from the FLECHT test. Our method of data reduction was performed with the complete cognizance and agreement of the Idaho Nuclear people. In fact, the paragraph that Mr. Ford read that said there are other ways of correlating the data, at the end of that paragraph the author from Idaho Nuclear states that the approach being used for FLECHT is the emperical approach.

MR. FORD: Excuse me. Is it correct that the approach that you used is that it creates an imperical computation?

MR. MOORE: As described in that report that you referenced earlier.

MR. FORD: The quotation that I read earlier from Page 24 of IN-1390, the last sentence says, "The latter approach is expected to be used," the latter being the approach that you did use?

MR. MOORE: Yes.

MR. FORD: Can you tell me whether Idaho Nuclear has given us a judgment on the matter as to whether or not there is any superiority or whther they simply select the latter method because that is the only one that we are presently

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capable of using?

MR. MOORE: The discussions with Idaho Nuclear involved trying to determine what was the best way and the most practical way to evaluate the data. I think it was one of practicality in getting an applicable useful correlation as opposed to a generally applicable theoretical evaluation.

CHAIRMAN JENSCH: Is it your interpretation from that statement that was read that since the Idaho Nuclear report says that method X will be used, you interpret that as being some endorsement of it, or is that just a statement of what is going to occur?

MR. MOORE: I know he agreed with the approach that we used to process the data.

CHAIRMAN JENSCH: They didn't disagree with what you did?

MR. MOORE: That's right.

CHAIRMAN JENSCH: That's about as far as it went, isn't it?

MR. MOORE: Well, we performed the tests under subcontract to them and as part of our final report that is the way we treated the data with their concurrence.

CHAIRMAN JENSCH: And you collected their money?

MR. MOORE: And we collected their money.

CHAIRMAN JENSCH: Very well.

Mr. Reporter, please don't go away. Will you come back and reread your question.

MR. WAGA: Yes.

(The previous question is read by the reporter.)

MR. MOORE: Yes.

CHAIRMAN JENSCH: I think it's about 5:30. As I understand it, we have got the question answered and this is the time of a previous commitment. Is there anything further we can take up before we recess?

MR. FORD: I think simply with regard to Idaho Nuclear perspective on the superiority of the coupled thermal hydraulic model it takes into detailed consideration the coolant thermal transient properties and its thermodynamic equilibrium. I think that's about as clear from their over-all June 1970 statement from a loss of coolant analysis program, I think it's clear that they regard the coupled set of thermal hydraulic equations as clearly the more preferable way of doing it, because they announce that that is what they wanted to develop, and I'd like to ask Mr. Moore to consider their state-of-the-art summary and solicit from it his judgment as to whether or not it indicates their preference for the much more detailed thermal hydraulic model versus the empirical correlation. The statement that I read is from the document LOSS OF COOLANT ANALYSIS PROGRAM I.N.-1382, in June of 1970, page 3, and I will give this to Mr. Moore to study. It says, and I quote, "The purpose of the loss of coolant accidents analysis program is to provide broadly applicable analytical tools for predicting the thermodynamic, hydrodynamic, and mechanical events which result from a loss of coolant accident, including the subsequent action of various engineered safety systems. The analytical and experimental results to date have clearly demonstrated that this task is unique, because unlike other accident situations, such as boiler explosions or reactivity excursions, the loss of coolant accident involves the simultaneous interaction of several mass and energy redistribution processes, including nuclear, ZBm3

thermal-hydraulic, chemical and structural processes. The task is complicated by the need to represent the accident process dynamically and at many positions in space for certain of the processes such as the sonic decompression subcooled liquid in a PWR system analysis techniques dealing with a distributed rather than a lumped system are required to define energy and mass redistribution. The spatial representation to date has generally been in one dimension. The apparent need now is to extend certain of the representations to two and three dimensions in order to establish sufficient precision to assure that fluid availability to a core is properly taken into account. The task also is complicated by the need to incorporate a considerable number of improved two-phase heat transfer relationships and two-component fluid-flow relationships. Many thermal hydraulic energy transfer processes are currently represented by empirical correlations because of the difficulty in describing the process on a purely theoretical basis. Further, these empirical correlations are based primarily on steady state data from tubes and annuli rather than the transient data from rod bundles."

And they go on to list in the area of core thermal response the specific recommendations that they make, including a thermohydraulic code to predict a localized and total system fluid response during a LOCA.

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MR. TROSTEN: Mr. Chairman, I object to the question which preceded the long reading from the Idaho Nuclear Corporation report.

CHAIRMAN JENSCH: What is the question?

MR. TROSTEN: He asked the question in which he wanted Mr. Moore to comment on the approach as I heard his point, asked Mr. Moore to comment on the approach and the relative merits of one system versus another system. I object to his question. I object to the long reading into the record of that excerpt from an Idaho Nuclear Corporation report which is time-consuming and a burden upon the time of the Board and the parties and I believe is an improper procedure to follow in a hearing of this sort, Mr. Chairman. We have covered this ground before with this interrogator , and I reiterate my objection, and as I say I object to the question on the grounds of no showing of relevance

CHAIRMAN JENSCH: Will you show the document to the witness.

MR. FORD: Yes.

CHAIRMAN JENSCH: Miss Reporter, can you goback to the portion prior to the quotation.

(The previous question by the interrogator is read by the reporter.)

CHAIRMAN JENSCH: Let me ask you have you finished reading that, Mr. Moore?

MR. MOORE: Yes.

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CHAIRMAN JENSCH: Areyou able to discern from that what is the judgment of the Idaho Nuclear people in this regard?

MR. MOORE: No. I think it's very difficult from just what is stated there.

CHAIRMAN JENSCH: You don't discern any preference in that statement as to the selection by Idaho Nuclear, is that correct?

MR. MOORE: That's correct. They refer to an apparent need. They seem to imply there is something wrong with emperical correlations, which I don't understand but--

CHAIRMAN JENSCH: On that basis the objection is sustained.

At this time is there anything further before we recess?

MR. TROSTEN: Just one point, Mr. Chairman. I told the Board last Thursday that we would advise the Board today whether we planned to have any redirect testimony with respect to the ECCS cross-examination that had occurred to date. It appears we will require some redirect. We will be able to advise the Board shortly as to its scope and of the amount of time that will be required for this.

I would also like to inquire of Mr. Roisman at this time as to the general scope of the cross-examination tomorrow, if that would be satisfactory to you, Mr. Chairman.

CHAIRMAN JENSCH: I think that kind of thing you can

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take up with him off the record. We want to save the record.

Is there anything further we can take up?

MR. ROISMAN: Mr. Chairman, I would just like to get the record straight, because the pauses, of course, are not on the record, but I believe that the Chair requested that we stop at what was on my watch twenty-five after five. It was at that time that the question asked by Mr. Ford was read, which I think took about a minute and a half, and I calculated. It will not show in the transcript since it's read.

Mr. Trosten then raised an objection which included a statement that Mr. Ford's reading of that half page of the Idaho Nuclear report was burdensome on the record. I merely want the record to show that we spent seven minutes dealing with Mr. Trosten's objection. If we are interested in speed I suggest those with the kind of technical excess which had nothing to do with the Board's ruling, as I understood it, on the question of whether the question should be answered or not is more likely to delay the hearing. Mr. Ford merely read the paragraph so that all of the parties, including Mr. Trosten and the Board, had an opportunity to hear what it is the witness and the interrogator had read.

MR. TROSTEN: Mr. Roisman, as you are aware--

CHAIRMAN JENSCH: I think when you propound a question to a witness seeking opinion evidence you have to lay the foundation for the question. Reading something from a document as to

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which you are seeking an opinion is not any burden on the record or burden on the Board.

At this time let's recess to reconvene in this room tomorrow morning at nine o'clock.

(Hearing adjourned to Tuesday, November 9, 1971, at nine a.m.)

Regulatory Docket File

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