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 9, 1971 Pages 3001-3191

Regulatory Docket File

Telephone:
(Code 202) 547-6222

ACE - FEDERAL REPORTERS, INC.

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UNITED STATES OF AMERICA

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

Tuesday, November 9, 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.

Witness	Cross
JAMES S. MOORE	3003

MORNING SESSION

CHAIRMAN JENSCH: Please come to order.

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

JAMES S. MOORE resumed.

CHAIRMAN JENSCH: Intervenor's counsel, are you ready to proceed?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Will you proceed.

MR. FORD: Mr. Moore, is it correct that a loss of coolant accident involves a simultaneous interaction of several mass and energy redistribution processes, including nuclear , thermal, hydraulic, chemical, and structural processes?

MR. MOORE: Yes.

MR. FORD: Is it correct that the analytical path is complicated by the means to represent these accident processes dynamically and at many positions in space?

MR. MOORE: Yes.

MR. TROSTEN: Mr. Chairman, I ask that Mr. Ford inform the Applicant's counsel of the document from which he is reading, and also inform the witness.

CHAIRMAN JENSCH: Why? Maybe it's his own notes.

What difference does it make?

MR. TROSTEN: Well, I think it's significant, Mr. Chairman.

CHAIRMAN JENSCH: Well, maybe he has prepared it so well he has it in writing. I don't know that it makes any difference.

MR. TROSTEN: Perhaps if he would say that maybe that would be the answer to it. I just asked the Board if you will direct the interrogator to inform him --

CHAIRMAN JENSCH: I think if the witness has any difficulty with the problem we can take that matter up. I think if he has his own notes or he has typed them out or something, those are matters personal to his own interrogation. I don't know what he has.

If the witness has any trouble with the question I think he should so indicate. Will you proceed.

MR. FORD: Is it correct that many thermalhydraulic energy transfer processes are currently represented by empirical correlations because of the difficulty in describing the processes on a purely theoretical basis?

end

MR. MOORE: Yes, as described in the Idaho report, which you are reading.

MR. FORD: Is it correct that these empirical correlations are based primarily on steady state data from tubes and annuli rather than on transit data from rod bundles?

MR. MOORE: No.

MR. FORD: Are you familiar with the code THETA-1B that was recommended by the Atomic Energy Commission in its interim policy statement of June 29, 1971, for analyzing behavior of a nuclear reactor core during a loss of coolant accident?

MR. MOORE: Not in detail.

MR. FORD: Are you familiar with the general structure of this code so that you could compare its structure and the nature of its simulation of accident situation with that of codes of Westinghouse?

MR. MOORE: Not in detail.

MR. FORD: Can you tell me, in terms of simply the complexity with which the code is able to deal, whether or not under one hand it is correct that to accurately determine the response of the fuel rod, the complete set of conservation equasions must be solved in detail throughout the primary system?

MR. TROSTEN: I object to the question on the grounds of no showing of relevance, Mr. Chairman.

MR. FORD: If Mr. Trosten understood the nature of the previous questions, it would be clear that this is again concerning the ability of the codes to represent complex thermodynamic situations.

MR. TROSTEN: I reiterate my objection, Mr. Chairman.

CHAIRMAN JENSCH: Is it your thought that the code does not have that compass?

MR. TROSTEN: I feel Mr. Ford has not shown the relevance of this particular question to the issues and to the line of questioning that he is propounding to Mr. Moore.

CHAIRMAN JENSCH: I understand that is your view. I am trying to find out what you think about the codes. I understand the inquiries are related to the codes. You may think something about the codes. Just to declare it is not relevant doesn't help us.

MR. TROSTEN: If I make an objection to a question of relevancy or if I object to the relevancy of a line of questions with a person of alleged technical or technical qualifications who is propounding the question to the witness, it seems to me it is incumbent on the interrogator to explain the relevance of it to the Board so you may rule on it.

MR. FORD: My first question contains the processes. In connection with that, I am asking you a question about how the complete set of conservation equasions relates to these processes involved.

MR. TROSTEN: It seems to me, Mr. Chairman --

MR. FORD: The answer to my first question, I might point out, Mr. Trosten, was yes. A nuclear reactor accident does involve the specific phenomena about with I am asking now the further question, about the code's representation.

MR. TROSTEN: As I understand the line of questioning propounded by Mr. Ford, the thrust of it, Mr. Chairman -- it has a number of elements as I hear him describing it. The thrust of it is seeking information from Mr. Moore as to a comparison of the Westinghouse and the Atomic Energy Commission code. I don't think that he has made any showing of relevance of this. That is why I have objected to his question.

CHAIRMAN JENSCH: The objection is overruled. Do you have the question in mind?

MR. MOORE: I'd like to hear the question again, please.

CHAIRMAN JENSCH: Reread the question.

MR. FORD: Is it correct that the cost of the complexity of the fluid dynamic and heat transfer processes occurring in a nuclear reactor cooler during loss of coolant accident, that to accurately determine the response of the fuel rods, the complete set of conservation equasions must be solved in detail throughout the entire primary system?

MR. MOORE: No. I guess I would disagree with that as stated.

MR. FORD: Can you give me the basis of your disagreement, please?

MR. MOORE: I don't quite understand what they mean, throughout the system. We are talking about a rod and then we are talking about a system. So it is not clear what they are discussing.

end

CBT1

MR. FORD: In terms of the interdependence between the various mass and energy redistribution processes, in terms of this complicated interdependence is it required that this kind of complete set of conservation equations be solved, or is it satisfactory simply to determine the fluid behavior using not very detailed solutions of these conservation equations?

MR. MOORE: That all depends on the type of analysis you are doing and the alternate use of the answer.

MR. FORD: Now in terms, as the question was originally set, of accurately determining the response of fuel rods to the accident situation is it necessary to solve the complete set of conservation equations in detail?

MR. MOORE: Where the attempt is to exactly determine such temperatures I would say the answer is yes.

MR. FORD: Can you tell me what topical report or major reference or minor reference that the Westinghouse Corporation has put together discussing its view, (a) on the necessity of solving these complete set of conservation equations for the accurate predictions that you indicated, and, (b) setting forth its theoretical justification and assessing the error involved in solving these things not in great detail?

MR. MOORE: Yes. The reference is to the LOCTA report, the discussion of the FLECHT heat transfer, FLECHT report, the overall calculations performed as described in the July 13th testimony.

BT2

MR. FORD: I see. Now can you tell me in terms of your use of the FLECHT data or your use of the mid-plane FLECHT data and non-use of the other heat transfer coefficients in terms of the detailed calculations of mass and energy redistribution, can you tell me whether your use simply of the mid-plane co-efficients and not of any of the others, whether that is an approach consistent with the--an approach that would involve a solution of the complete set of conservation equations?

MR. MOORE: No. As I described earlier, the purpose of the analysis is to determine the peak temperature that occurs in the core, and the result of the FLECHT program at the mid-plane are directly applicable to peak temperature calculation.

MR. FORD: So the answer is no, this approach is not consistent with the more elaborate approach that I have described?

MR. MOORE: And not necessary.

MR. FORD: Now in the Idaho Nuclear Corporation report on THETA-1B code the document IN-1445, February, 1971, entitled "A Computer Code for Nuclear Reactor Core Thermal Analysis," they indicate, they give some indication of the process of development that's going on in the code and of their own code they say that it was, on Page 2, it was designed more as a development tool than as a production code. Could you tell me in--

CHAIRMAN JENSCH: I wonder if you would hand the document to the witness and point out the place to which you referred.

MR. FORD: It's a typed sheet, sir, with my note, that I'd be happy to let him see that.

CHAIRMAN JENSCH: You don't have to show him your note. Show him the document.

MR. FORD: I have simply on this particular document three typed sheets of direct quotations from it.

CHAIRMAN JENSCH: That's all you have on your typed sheet?

MR. FORD: Yes.

CHAIRMAN JENSCH: Show it to Applicants' counsel and then it's subject to introduction as a document.

Is the witness familiar at all with the Document IN-1445?

MR. MOORE: No, not in detail.

MR. FORD: I had previously ascertained from the witness, Mr. Chairman, that he was familiar in a general way with THETA-1B, which is the code that's described in this document. I don't intend to get into the minutiae of the code. It's the more general characteristics of it that are of interest to me.

CHAIRMAN JENSCH: Show the document to the witness.

MR. TROSTEN: Mr. Chairman, I think that--

CHAIRMAN JENSCH: Wait a minute. Let's let the witness finish reading and see what the question is.

Have you finished reading?

MR. MOORE: Yes.

CHAIRMAN JENSCH: What is the question?

MR. FORD: I need my sheet.

My question is whether the Westinghouse codes for reactor core thermal analysis can be similiarly considered as development tools in the field of accident analysis, rather than as production codes?

MR. TROSTEN: Mr. Chairman, the interrogator has shown the witness a piece of paper that purports to be a copy of excerpts from a document that the witness has said he is not fully familiar with. The witness does not have the document before him. There is no showing in the record, in the evidence, that the document that the interrogator is questioning about actually says what it says, what that sheet of paper shows, and I think under these circumstances that this question is improper.

CHAIRMAN JENSCH: Well, I think ordinarily I think the document from which the excerpt has been taken should be produced. However, as I understand this question neither the document nor the excerpts are important to the question. As I understand the query is whether the Westinghouse code can be considered as a developmental code or as a production code. It doesn't make any difference what Idaho says about their own code.

MR. TROSTEN: I understand the Chairman's construction. I don't quite understand why if neither the document nor the excerpt are important why Mr. Ford felt it necessary to read it.

CHAIRMAN JENSCH: In cross-examination I think we are getting to a different field. I think we will all take a look at the question as it is and the objection is overruled.

The witness may answer.

MR. MOORE: Our LOCTA code I would consider a production code.

MR. FORD: Is further development work on the LOCTA code planned, or underway?

MR. MOORE: None specifically, no.

MR. FORD: Does your code in its calculations--do you have to put in boiling data for the water and rod bundles at high pressure?

MR. MOORE: We use heat transfer correlations. No. We use heat transfer correlations that are explained in the material which describes our analysis.

MR. FORD: Have any computations of the maximum cladding temperature that would be experienced during design basis accident at Indian Point 2 then performed, to your knowledge, with the THETA-1D and RELAP-3 codes at the Idaho Nuclear Corporation?

MR. MOORE: Not to my knowledge.

MR. FORD: Did you talk to any members of the Idaho Nuclear Corporation who were acting under their technical assistance in reactor safety analysis program to the AEC Staff in reviewing the Indian Point 2 power station?

MR. MOORE: In specifically reviewing the Indian Point 2 power station?

MR. FORD: Yes.

MR. MOORE: No, I don't believe so.

CHAIRMAN JENSCH: There seems to be a pause. I wonder if I understand the question.

Idaho Nuclear was particularly reviewing the Indian Point proceeding, was that the basis of your question? You wanted to know whether this witness had talked to somebody of the Idaho Nuclear group respecting their review of the Indian Point 2 proceeding?

MR. FORD: Yes, sir. It is my understanding the Idaho Nuclear report on the technical assistance consulting program for the AEC Staff, that that involved specific licensing proceedings that they performed in a review of the plant. I want to know how this proceeds and I want to know in particular if they talked to Westinghouse people.

CHAIRMAN JENSCH: Thank you.

MR. FORD: In the PWR FLECHT tests, am I correct that the parameters in this test were initial heater surface temperature, power density, coolant inlet temperature, coolant pressure at the outset, number of rods in the bundle, the cold flooding rate in inches per second and the cladding temperature?

MR. MOORE: Yes.

MR. FORD: Have I omitted any main parameters?

MR. MOORE: I don't believe so, as you read the list off.

MR. FORD: Is it correct that the following two parameters were not included in that list and were not parameters of the FLECHT test: One, the internal gas pressure of the fuel rods, simulated fuel rods; two, the heating rate in Fahrenheit degrees per second by which the rods were raised to their initial temperature?

MR. MOORE: I believe that's true, yes.

MR. FORD: Is it correct for the test parameters, the following ranges? I will ask you yes or no after I read the range. For initial heater surface temperature between 800 degrees Fahrenheit and 2400 degrees Fahrenheit.

MR. MOORE: I am looking at the summary table in the report, the FLECHT report, Table 3-1. I was looking at just all the temperatures that are indicated there. I was searching for the maximum. I guess I see a temperature of 2300 degrees.

MR. FORD: 2300 degrees is your maximum?

end

MR. MOORE: On the summary of the data, yes.

MR. FORD: I am reading the FLECHT parameters, the Idaho Nuclear overview report on FLECHT, Document IN-1386 which you have referred to several times yesterday. For the sake of efficiency, may I show you the table and ask you whether the parameter ranges as indicated here are accurate, to your knowledge. Secondly, whether the specific values of the parameters within the range that is indicated here that we are actually testing, whether that also is accurate?

MR. MOORE: You want me to go down through each one?

MR. FORD: I think rather than checking every number, my impression is that Idaho Nuclear is a close enough contact that wouldn't have made a ghastly error. Simply by looking at the list, whether the ranges indicated there and specific values of the parameters, that they indicate they were used. Specific tests, and whether that is, to your judgment, an accurate representation of parameters of the FLECHT test?

MR. MOORE: Yes. These were the conditions. As indicated in this report, they were expected to be investigated. I believe we covered the range intended in the summary.

MR. FORD: In this table of FLECHT test parameters, it indicates that tests were to be conducted at eight initial heater surface temperatures, at three initial power density, at seven coolant inlet temperatures, at five coolant outlet pressures, and so forth. Is it correct that if we combined --

we wanted to make the parameter combinations. Or the possible ways you could put together these eight different initial temperatures with three different power densities and so forth, is it correct that the number of parameter combinations that we get is slightly greater than 30,000?

MR. MOORE: I have no number for that.

MR. FORD: The calculation is by multiplying the number of selected values for each parameter by the number of selected values for all the other parameters so that the total number of parameter combinations you figure out is slightly greater than 30,000. Would you dispute that or would you like to multiply eight times three times seven times five times two times eight times two?

MR. MOORE: I will trust yor arithmetic.

MR. FORD: In terms of the actual FLECHT tests that were conducted, can you tell me how many parameter combinations off of this table are parameters you actually tested?

MR. MOORE: They are indicated in the report you have, which is Table 3.1.

MR. FORD: Is it correct that you tested 73 parameter combinations out of a possible 30,000?

MR. MOORE: Did you obtain that by comparing the numbers in that table?

MR. FORD: That's right. There were 73 tests, a maximum possible 73 parameter combinations, assuming that you didn't repeat the same thing over and over again. I am looking at the FLECHT Table 2, which is in the Idaho Nuclear report. This is just a summary table of all of the FLECHT tests.

MR. MOORE: Is this the same report I was looking at before?

MR. FORD: That's right. That is IN-1386.

MR. MOORE: May I see the page you are referring to?

MR. FORD: Sure.

MR. MOORE: The table referred to here was the proposed test sequence. We essentially followed that test sequence. There may be some differences within the sequence on specific tests. The specific tests are indicated in Table 3-1 of the FLECHT report.

MR. MOORE: No. It's as indicated in the reports. You can see the predicted versus measured correlations are presented to you.

MR. FORD: Can you tell me what section of the report correlation analysis is presented in?

MR. MOORE: Yes. Beginning at 3-80, page 3-80.

CHAIRMAN JENSCH: Is this of IN-1386?

MR. FORD: No, sir. This is --

MR. MOORE: I am sorry. This is WCAP-7665.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Can you tell me whether for purposes of these empirical correlations the statistical indices that we discussed yesterday, the R^2 T and F statistic, whether they were evolved?

MR. MOORE: I don't believe they were.

MR. FORD: Does the statistical analysis that you present offer any other method of doing what the R^2 does, namely, explaining the per cent of variance in heat transfer coefficients that can be explained by the factors that you propose, by the independent variables you propose?

MR. MOORE: The correlation was able to predict the data within plus or minus ten per cent.

MR. FORD: But is it correct that you under-standing of the R^2 index is that it's possible, since R^2 measures the distance between points in a line, that if everything falls within plus or minus ten per cent of the line the R^2, the per cent of that variance explained, you know, may nevertheless be exceedingly low?

MR. MOORE: Yes. I believe I understand that. I would still refer you to the figure 355 on page 3-89 which indicates the ability of the correlation to predict the measured coefficients.

MR. FORD: Can you tell me whether the errors involved here, not just in the entire range but the specific points, were measured such that a statistical analysis could be performed to decide whether or not these errors were randomly or not randomly distributed?

MR. MOORE: I don't believe such was done, no.

MR. FORD: Is it correct that in the statistical analysis relied upon here that if the errors were nonrandomly distributed that that would substantially invalidate the correlation, because in that case another factor was systematically altering the data, but which was not accounted in a sample of independent variables?

MR. MOORE: I believe all the independent variables were incorporated into the correlation.

CHAIRMAN JENSCH: Is that the question?

MR. FORD: No, sir.

CHAIRMAN JENSCH: Reread the question, please.

(The pending question is read by the reporter.)

MR. MOORE: What kind of errors, for example, are we discussing?

MR. FORD: We are talking -- the definition of error here is distance between the point and the predicted line. That is the error. And of the statistical analysis we are talking about the furthest step. We are checking whether or not the errors are randomly or nonrandomly distributed.

MR. MOORE: Could I hear the question again, please?

MR. FORD: Will the reporter reread the question that was previously reread.

end

(The pending question is reread by the reporter.)

MR. MOORE: Yes. Non-random effects could affect the correlation. I guess I would argue with the word "substantially" alter. It's a function of the errors.

MR. FORD: In precise statistical terms can you qualify your answer that way?

MR. MOORE: No.

CHAIRMAN JENSCH: I wonder if I just could go back to that question. I think the answer was, it could affect the correlation. I think the question was would it invalidate it.

MR. MOORE: Not necessarily. It's a function of the magnitude.

CHAIRMAN JENSCH: Proceed.

MR. FORD: In terms of the magnitude that could be associated with a plus or minus 10 per cent error do you have statistical analysis that rules out the possibility that your correlation could not be invalidated in this case by a clearly non-random error within the range that you are talking about, plus or minus 10 per cent?

MR. MOORE: I confess I don't understand the question. I am looking at the figures which shows errors of plus or minus within a 10 per cent span. They don't look non-random to me as plotted there. I guess I don't understand. I already indicated I do not have the statistical evaluation.

MR. FORD: I see. Now, this is a --

CHAIRMAN JENSCH: Let him finish. Excuse me.

Have you finished?

MR. MOORE: I am finished, yes.

MR. FORD: Now since this is the result of a multi-variate analysis is it correct that in order to measure the errors involved you simply can't tell from a diagram on a page that you are talking about much higher spaces?

MR. MOORE: Much higher spaces?

MR. FORD: Yes. Dimensions, many more dimensions than the two represented here. I mean, my point is since you are dealing with multi-variate analysis are you contending it's possible to look and see the errors or is this because it involves higher spaces and something which requires calculation rather than inspection?

MR. MOORE: No. My point was that this reference is forty -- the coefficients predicted for over forty typical runs, which included many variations of the variables that we are discussing. We started upon this line of questioning on the basis of the fact that we didn't have 30,000 combinations of parameters. I indicated we have done a reasonable job of determining the effect of various parameters, which did not require performing 30,000 combinations. In that we were able to correlate the data for runs that had different variations in these parameters with the correlation. We were able to predict this.

MR. FORD: In terms of the justification for the number of parameter combinations used is it correct that the justification is mainly a financial one, that in a sense that investigating more than a few of the parameter combinations which could conceivably occur would be unacceptably expensive?

MR. TROSTEN: I object to that question, Mr. Chairman. I don't see the relevance of that to this inquiry.

CHAIRMAN JENSCH: Would you care to speak to that matter?

MR. ROISMAN: I will, Mr. Chairman. I think this relates to a subject which we have discussed at an earlier time which relates to this question of the cost benefit analysis. One of our contentions in the proceeding is going to be that it would appear, whether made explicit or implicit in the way that various things were done, that cost benefit was a factor in determining the safety of this reactor. The question here with this witness on the stand is to find out to what extent cost versus benefit was a determination in deciding how to test in the FLECHT tests and how reliable the results are, and it's all part of that same discussion.

MR. FORD: I might point out specifically in this context my question was prompted by the witness's previous answer in terms of his remark to the effect how good a sample was, and I am asking in terms of the virtue of the sample whether its virtue minimizes the cost of the testing rather than from a statistical point of view it is to be found to be a high quality sample.

MR. TROSTEN: Mr. Chairman, the purpose of the question should be to elicit information from the witness as to whether or not the analysis that was presented here was adequate. Asking questions extraneous to the issue of whether the analysis was adequate for the purposes of the safety evaluation serves no useful purpose in this hearing from Applicants' point of view, and a question of that sort is irrelevant in Applicants' view.

CHAIRMAN JENSCH: Certainly the premise you have established doesn't make it relevant. I think the question is extraneous. I understood that the questioning was trying to find a basis for the selection of the number of tests that were undertaken, and he has offered one, for instance, that there was a reasonable correlation. He was able to predict within a reasonable margin. In cross-examination, I don't think the party is necessarily limited to the answer that's given. I think he may press to see are there other factors that may affect the selection of these tests.

CHAIRMAN JENSCH: The Board will overrule the objection to the extent that unacceptability is in the question.

And will you restate it without the word unacceptability. Can the reporter reread it.

(The pending question is read by the reporter.)

MR. TROSTEN: Mr. Chairman, I ask that the question be clarified also to distinguish whether Mr. Ford is referring to Idaho Nuclear Corporation selecting parameters or Westinghouse Corporation selecting parameters.

F1Wt1

CHAIRMAN JENSCH: Maybe that hasn't been established. I understood this was a selection made by Westinghouse out of a possible 30,000 parameters that Idaho Nuclear suggested. Did Westinghouse make the selection of the tests that it would undertake, or were they given direction in this regard by Idaho Nuclear? Did they tell you to just investigate seventy-three tests?

MR. MOORE: This was jointly determined between Idaho Nuclear and Westinghouse.

CHAIRMAN JENSCH: Proceed with the question.

MR. MOORE: The answer is no. Cost was not the only or main consideration.

MR. FORD: Can you tell me, in terms of your discussions with the Idaho Nuclear Corporation, did you see eye to eye on the number of parameters that should be tested, or did you want to test more parameters than they wanted to test, or did they want to test a lot more parameters than you wanted to test?

MR. MOORE: I don't recall the detailed discussion. We came to an agreement on the specific number of tests to be performed.

CHAIRMAN JENSCH: With whom did you talk and make this joint determination? Can you recall that?

MR. MOORE: I personally was not involved in these discussions. There was a representative, the project director or program director from the Idaho Nuclear Corporation that talked with our people.

CHAIRMAN JENSCH: What is his name?

MR. MOORE: I don't recall.

MR. FORD: Was it--

CHAIRMAN JENSCH: Excuse me.

MR. MOORE: I don't recall his name specifically, sir.

CHAIRMAN JENSCH: How did you learn it was jointly determined?

MR. MOORE: Because I knew that Westinghouse, as a subcontractor to Idaho Nuclear, sat down with them to agree on the manner in which these tests were to be performed, including the parameters to be evaluated. That's the basis for the report that Mr. Ford is referring to, which indicates what the proposed tests would be. That is an Idaho Nuclear report.

CHAIRMAN JENSCH: Who are the representatives of Westinghouse that made the agreement?

MR. MOORE: We had Dr. L. S. Tong who was involved with the program, and also Dr. Cermak.

CHAIRMAN JENSCH: And they made the agreement with Idaho Nuclear; is that correct?

MR. MOORE: Yes.

CHAIRMAN JENSCH: Proceed.

MR. FORD: Is Mr. Cermak here?

F1Wt3

MR. MOORE: No.

MR. FORD: Does the following description of the criteria for selecting parameters, selecting the number of parameter combinations given by Idaho Nuclear refresh your memory from the document IN-1386 that we have referred to before?

It says on Page 10, in the section called "Parameter Description of the PWR FLECHT test plan," and I quote: "A test program investigating more than a few of the parameter combinations which could conceivably occur in a LOCA would be unacceptably expensive"?

MR. TROSTEN: Could the reporter reread the question, please?

(The last question was read by the reporter.)

MR. MOORE: Was the question, is my memory refreshed?

CHAIRMAN JENSCH: Yes.

MR. MOORE: Is that the question?

MR. FORD: Yes.

MR. MOORE: I don't think that's in conflict with what I said before. I said money was not the only variable that determined the number of tests.

CHAIRMAN JENSCH: I think the only question is, does that refresh your recollection?

MR. MOORE: I think my recollection still stands.

CHAIRMAN JENSCH: Very well. Proceed.

F1Wt4

MR. FORD: Is it correct that flow blockage in the FLECHT tests was simulated by placing a perforated steel plate bundle mid-plane?

MR. MOORE: Yes.

MR. FORD: Are you familiar with the Atomic Energy Commission's programs investigating the effects of flow blockage on Emergency Core Cooling System Performance?

MR. MOORE: I believe so, yes.

MR. FORD: Are you familiar with the main body of this work conducted at the Oak Ridge National Laboratory for the Atomic Energy Commission?

MR. MOORE: With respect to what?

MR. FORD: With respect to zircalloy cladding failure modes and the effect on Emergency Core Cooling System Performance?

MR. MOORE: I am familiar with tests being run at Oak Ridge, yes.

MR. FORD: Is Oak Ridge the main center for research in this area, main judge, suppose, in terms of percentage of the people involved, percentage of the resources expended and percentage of the tests done, and so forth?

MR. MOORE: I don't have any exact information on that, but I would say no. It was work being done in several places.

MR. FORD: Who are the other main flow blockage investigators?

MR. MOORE: Other than the reactor manufacturers, the Idaho Nuclear Corporation.

MR. FORD: Can you tell me, in terms of the orifice plate that you used to simulate blockage, is this the manner in which the Oak Ridge people, to your knowledge, simulate flow blockage?

MR. MOORE: Simulate flow blockage in what kind of tests?

MR. FORD: In bundle tests, in in-pile tests, and so forth.

MR. MOORE: What kind of tests? Heat transfer, or what?

MR. FORD: Tests of the effects of flow blockage on emergency core cooling system performance.

MR. MOORE: Again, what kind of tests?

MR. FORD: Tests such as the in-pile treat tests reported in ORNL-4635.

MR. MOORE: Those were not tests to simulate blockage.

MR. FORD: By test to simulate blockage I mean tests which were run with the expectation that the flow blockage would rise, and with the intention to measure the degree of blockage and measure the degree of coolant flow diversion and so forth. Those are the kinds of tests I am talking about.

MR. MOORE: Those are different kinds of tests, and they are not all run at the same time.

MR. FORD: What I am simply trying to ascertain is whether the method that you use to simulate flow blockage in heat transfer tests, whether that method of putting a steel grid with uniform orifice holds in it, whether that is the method that other people use when they want to simulate flow blockage.

MR. MOORE: With respect to heat transfer, I don't recall any specific tests at Oak Ridge of the effects of blockage on heat transfer. There were tests performed at Idaho Nuclear on flow blockage and assemblies. We went over this in some detail with Mr. Roisman, I believe, one day when you were not here.

MR. FORD: Can you tell me, in terms of your simulated blockage, is your use of this data with blockage simply in a heat transfer context, or do you use this blockage data to demonstrate that the emergency core cooling system performance would not be degraded by the flow around this blockage?

MR. MOORE: The answer is yes to both questions because it is an effect on heat transfer.

MR. FORD: In terms of the work at Oak Ridge, are you familiar with the large number of writings of the Director of that flow blockage work, Mr. P. L. Rittenhouse?

MR. MOORE: Yes.

MR. FORD: Are you familiar with his document, "Progress in Zircalloy Cladding Failure Modes Research"? This is a document, ORNL-TM-3188.

CHAIRMAN JENSCH: Can you show him the document?

MR. FORD: Yes. I will do that in a second after I identify the specific section that I am concerned with. I am concerned with a section on pages 24 and 25 which relates to the differences between methods of simulating flow blockage. It states, and I quote: "In the PWR-FLECHT Project, tests of flow blockage were performed with 7 x 7 arrays (49-rod bundles) of stainless-steel-clad rods. Flow was blcoked by a 3/8 inch-thick plate at the middle of the bundle length. Blockages of 50 and 75 per cent of the flow area in the 5 x 5 center array of the 7 x 7 bundle were not found detrimental to the effectiveness of bottom-flooding heat transfer. In fact, these blockages seemed to improve cooling capability. Much the same thing was observed by Idaho Nuclear Corporation in smaller scale experiments. This effect is explained in terms of atomization of the coolant as it is forced through the openings in the blockage plate. Real blockages, however, will probably not provide the simple, uniform orifice configuration that gives rise to this mechanism (coolant atomization) for improved cooling. Also, it seems likely that in a reactor core the coolant would bypass the blockage zones instead of being forced through them. Tests with more severe blockages were recently performed, but the results of these experiments are not yet available in the literature."

CHAIRMAN JENSCH: Would you let the witness see the document?

MR. FORD: Yes, sir.

MR. MOORE: Is there a date on this report?

MR. FORD: I believe the date of it is January 1970.

MR. MOORE: I don't see any.

MR. FORD: It is one that was provided to us by the AEC in that form. I believe, if you study the initial footnotes, it is a progress report and it gives the date of the previous reports. I believe that is January 1970. I can check that on the way. Let me do that.

CHAIRMAN JENSCH: Are you familiar with the report or the material reflected therein?

MR. MOORE: Not the specific report, sir, but the material reflected in here, yes.

CHAIRMAN JENSCH: Thank you.

MR. FORD: The footnote on the page we are directly referring to is a paper given July 2, 1970. It is somewhat later than my previous estimate.

Mr. Rittenhouse's evaluation here of the use of blockage plates, can we go through this piece by piece? Is Mr. Rittenhouse correct that real blockages will probably not provide the simple uniform orifice configuration of the blockage plate?

MR. TROSTEN: Mr. Chairman, I believe Mr. Ford is going over the same grounds that was covered in questions 26 and 27 that Mr. Roisman asked of Mr. Moore. This appears on pages 1672 and 1673 of the uncorrected version of the transcript. I suggest that Mr. Ford refer to that question and answer.

CHAIRMAN JENSCH: I think that reference should be undertaken. If there is some difference, point out the difference.

MR. FORD: I believe I have looked at the transcript last night. It does ascertain that the tests were performed in using these blockage plates. I don't think the discussion here at all -- aside from my initial question, I don't think the discussion will be repetitious.

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MR. TROSTEN: Before I determine whether I should object to this as repetitious, I would like to know how the question and how the answer that was given is not responsive.

CHAIRMAN JENSCH: As I understand, may I see the reference? Our transcripts are in our adjacent room. Intervenor's counsel is handing to me a transcript.

end

CHAIRMAN JENSCH: I will return this document to you, to the Intervenor's counsel.

May we have the question reread.

(The previous question is read by the reporter.)

CHAIRMAN JENSCH: Yes. It is the understanding of the Board that the previous questions were answered in a way which Westinghouse indicated the reason they used what they did. This pending question now is to compare it with what Rittenhouse says and an evaluation of Rittenhouse's approach.

The witness may answer it.

MR. FORD: Mr. Chairman, may I direct your attention to the transcript page 1790, where Mr. Roisman asked me further questions in some detail on the simulations of blockage, not only of Westinghouse tests but Idaho tests.

CHAIRMAN JENSCH: Well, now, are we back to Oak Ridge? I think we went over Oak Ridge. Now we have different analyses, some from Oak Ridge, some from Idaho, and some of yours. I think the question is related to yours. In your judgment, what are Rittenhouse's suggestions and the results of his work? Can you approach it from that point of view?

MR. TROSTEN: What is the Chairman's question? I am sorry.

CHAIRMAN JENSCH: I am not asking any questions, but I understand the question propounded to the witness requests his judgment of Rittenhouse's analysis.

Can you approach it from that point of view?

MR. MOORE: Yes. As I understand the question, it's does the flat plate simulate the kind of geometry expected under the actual conditions? The answer is no. And the detailed development of that has already been followed in the transcript on page 1790.

MR. FORD: The precise question was whether Mr. Rittenhouse was correct, that real blockages will probably not provide the simpler uniform orifice configuration that's represented by the blockage plate.

MR. MOORE: That's right, no.

MR. FORD: Is Mr. Rittenhouse correct that it is likely that in a reactor core the coolant would bypass the blockage zones instead of being forced through them?

MR. MOORE: Yes. As demonstrated in our tests where we had 100 per cent blockage.

MR. FORD: By blockage zones here is he, as I understand him, talking in general terms of blockages? Has he specifically qualified himself? He is only talking about the extreme case of a 100 per cent blockage.

MR. MOORE: I don't know.

MR. FORD: I can give you the reference, and let me ask you if you can point out how it's not clear, that he is simply talking about a generic blockage rather than the extreme case?

Now, from your study of his statement is there any indication that he switched in the point I was asking you about from talking about blockage in general to simply talking about an extreme case of a 100 per cent blockage?

MR. MOORE: I believe so, because he is referring in the end of that paragraph to tests with more severe blockage being performed. Those were the tests I referred to.

MR. FORD: Is it not correct that the talk of more severe blockage follows his talk about the blockage of zones causing radial flow? And are you still referring to 50 or 75 per cent blockage that was simulated by the orifice grids that he was talking about?

MR. TROSTEN: Mr. Chairman, I object, because what appears to be happening here is that Mr. Ford is reading a document and interpreting it in a certain way and he seems to be asking Mr. Moore to read the same document and then the two of them are going to discuss what it is that some third person meant, and I don't think that has anything to do with what we are trying to establish here.

CHAIRMAN JENSCH: I think the objection is well taken. I think we have a great deal of difficulty here with semantics. I think it's a difficult way to approach it, to take a document and try to ascertain the meaning of a writer or what he really was saying and so forth. I think if there is some contest as to whether what Westinghouse did or the kind of steel plates they used were adequate or inadequate, I think that is the way to approach it. And, "Would you believe that Joe Blow should have used a sieve or a screen or something else?"

I mean an analysis of the mechanism itself rather than what somebody else said about a mechanism. The objection is sustained.

MR. FORD: The geometry of the orifice, is it simply a round hole in the plate or is it irregular in any way?

MR. MOORE: These were round holes in plates.

Let me correct that. We have both types. We had round holes and we also had some square holes.

MR. FORD: Did both give the same coolant flow area? Was the area of the hole the area of the circle?

MR. MOORE: Yes. We had tests with the same area blocked.

MR. FORD: Did you perform any sensitivity analysis on the differences, on how the differences in orifice geometry related to differences in flow and coolant atomization and so forth?

MR. MOORE: Yes. We had the heat transfer coefficients obtained from both geometries.

MR. FORD: Yes. Now, I am asking did you do any statistical comparisons of the heat transfer coefficients with the cooled atomization you got with one geometry versus the heat transfer coefficients that you got from the other geometry to determine whether orifice geometry was the influential statistically significant way with heat transfer flow blockage, et cetera?

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MR. MOORE: No. We just observed that in all cases heat transfer was improved.

MR. FORD: In all cases of having the plate?

MR. MOORE: That's correct.

MR. FORD: But my question was whether you differentiated in your studies of data between the plate with the square holes and the plate with the round holes in terms of direct analysis of the data or sensitivity to the geometry?

MR. MOORE: No.

MR. FORD: Have you performed calculations that lead you to expect that there would be no difference in heat transfer if you used a square orifice or the use of a round orifice?

MR. MOORE: No. We have been over this ground before. We observed with movies of the FLECHT results the atomization behavior with both geometries. We also observed with movies of the Idaho tests with different smooth-tapered geometries the same effect. They had the same kind of improvement in heat transfer.

MR. FORD: So that you contend from the data that you have available that the magnitude of radial flow that accompanies flow blockage is well-established and small?

MR. MOORE: No. We observed the effects of blockage, including one hundred per cent blockage in the sixteen channels. We observed that there was atomization due to blockage and turbulence even downstream of blockage, one hundred per cent

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blockage, such that the effective heat transfer was greater than the no block case.

MR. FORD: In terms of the radial flow around the blockage, as I take it, except for the most extreme case, am I correct that you didn't observe radial flow? Instead the coolant, in a turbulent way, it was nevertheless forced up through the channels in which it was already traveling?

MR. MOORE: Essentially, yes.

MR. FORD: So that does that provide data on the non-existence for all practical purposes of radial flow as the result of flow blockage?

MR. MOORE: Insofar as we observed no radial flow in the bundle, the reservations, of course, of geometry in the bundle still obtained. The fundamental point is that it's the water droplets which are being carried by the steam which are traveling in the upward direction are broken up to improve the heat transfer.

That's the phenomena which will not be very sensitive to radial flow.

MR. FORD: Now in terms of the extent to which your data says something about the magnitude of radial flow and establishes a fact about the magnitude of radial flow, given changes in a given flow blockage do you dispute the judgment of Idaho Nuclear Corporation in this report IN-1387, technical description of the assimulated emergency flow effects, SEFET

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project? Their judgment was stated on Page 5 concerning radial flow, and I quote, "No known data are available which establishes the magnitude of radial flow in rod arrays for single and two-phase fluids."

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

MR. FORD: The question was whether you dispute this statement.

MR. MOORE: As indicated in the testimony last week we had some measure of radial flow through the FLECHT tests. Also what the pressure drop situation was in the core and how the velocity contribution to pressure drop was very small, that the main pressure drop was an elevation pressure drop, and I gave you the reasons why we consider radial flow not to be a serious consideration in the evaluation of the peak clad temperature.

MR. FORD: Now is it your contention that the data which you have evolved in the FLECHT tests refutes the assertion of Idaho Nuclear contractors of the FLECHT test that no known data are available which establish the magnitude of radial flow in rod arrays of single and two phase fluids?

MR. MOORE: No. The FLECHT test was a rod array.

MR. FORD: Is it no, that data does not provide reputation of this? Is that what you mean by no?

MR. MOORE: I disagree. There is data.

MR. FORD: Yes.

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MR. MOORE: That was carefully weighted, but there is data with respect to a radial flow in a rod bundle array.

MR. FORD: Now in terms of the relevance of the FLECHT data to the radial flow question, may I refer you to the final report on FLECHT WCAP 7665, Section 4, concerned with The Relation of the FLECHT Test to the Reactor LOCA Analysis, the title of Section 4, Section 4.1, The Assumptions and Limitations of the FLECHT Tests, and in particular to Page 4-5 which says at the top, and I quote--do you have the document there, Mr. Moore?

MR. MOORE: Yes.

MR. FORD: Fine. Which says, and I quote, "It should be noted that no attempt was made to simulate corewide radial flow effect in the PWR FLECHT tests. Typical reactor loss of coolant accident calculations indicate that the coolant flow at the mid-plane at the hot assembly with fifty per cent flow blockage would be approximately seventy-five of the core average. Therefore, it is important to recognize the need to take the radial flow distribution into account inusing FLECHT data for reactor loss of coolant accident analysis."

MR. MOORE: Yes.

MR. FORD: With this statement of the limitation of FLECHT tests as far as their applicability of the question of corewide radial flow goes, do you still contend that FLECT data is a reputation of the Idaho Nuclear statement that there

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is no known data available which establishes the magnitude of radial flow in rod arrays for single or two-phase fluids?

MR. MOORE: That's not inconsistent with that I said before. Properly weigh the data you have and what it represents and do not ignore all the other conditions that exist in a loss of coolant.

MR. FORD: You refer in the Westinghouse report to calculations indicating that in an accident fifty per cent flow blockage there will be approximately twenty-five per cent radial flow.

MR. TROSTEN: Mr. Chairman, I suggest that Mr. Ford is becoming repetitious of material that was gone over at length, starting on Page 1834 of the transcript.

MR. FORD: I appreciate the fact that the specific quotation from 465 of WCAP 7665, which we were just discussing, was previously discussed. My suggestion is that I am using this material or attempting to use it in a very specific new context, namely relating the qualifications of FLECHT test data with regard to radial flow, relating those qualifications specifically to the strong inclusion of Idaho Nuclear, namely that no known data of the magnitudes of radial flow are available.

MR. ROISMAN: Mr. Chairman, I should also like to point out that at the request of the Applicant, Mr. Ford interrupted his examination on radial flow which began on pages of the transcript that he is referring to, and turned instead to the metal-water reaction question because of Mr. Roll's availability. I think it was made clear -- I'm sorry, I don't have that transcript here in front of me. There might be a little bit of overlapping. We got back into the area again of radial flow. We did it as a courtesy to the Applicant. I think it is unreasonable of Mr. Trosten to take an overtechnical attitude here with regard to what is probably not a repetition in any case.

CHAIRMAN JENSCH: As I understand it, you are now seeking to use this information as the basis of a different inquiry; is that correct?

MR. FORD: That's correct, sir.

CHAIRMAN JENSCH: The witness may answer.

MR. MOORE: Could you reread the question, please?

MR. FORD: I'm afraid Mr. Trosten interrupted my line of thought.

CHAIRMAN JENSCH: I think you started out to say, would you take this calculation of the Westinghouse report on the 50 per cent flow and 25 per cent radial -- something like that.

MR. FORD: What I'd like to ask now is that you compute with 50 per cent flow blockage, 25 per cent radial flow. Is that correct?

MR. MOORE: That's correct, as I indicated in the previous testimony. We calculate a 25 per cent flow reduction due to the effects of blockage. I indicated how that is calculated.

MR. FORD: That's assuming a 50 per cent blockage. Have you performed calculations of the magnitude of radial flow assuming a 60 per cent blockage?

MR. MOORE: I have such a calculation. I don't have it in my notes right in front of me. You're asking the effect of blockage on redistribution, correct?

MR. FORD: Yes.

MR. MOORE: I have that. I don't have those notes directly in front of me.

MR. FORD: In terms of the general relationship between flow blockage and radial flow, is it correct that as we increased the per cent of flow blockage from 50 to 60 per cent, it would be an increase in the per cent of radial flow from 25 to a higher per cent?

MR. MOORE: Yes, more blockage and more redistribution as we calculate it. It is a very conservative calculation.

MR. FORD: Is the Idaho Nuclear Corporation correctly describing the FLECHT project, the BWR FLECHT project, when they say, "It has provided the experimental information required to analyze the reflooding portion of the loss of coolant accident in BWR reactors under the assumed conditions that radial flow is negligible"?

This is from page 2 of the report IN-1387.

MR. MOORE: No. That's an overstatement. It doesn't recognize the way the over-all analysis is performed.

MR. FORD: Would the following qualification make it less of an overstatement? They continue. It says, and I quote again from page 2: "However, several conditions occurred for and during core reflooding that provide the potential for radial flow and subsequent coolant variation in the hottest core regions."

MR. MOORE: Perhaps I'd better see the reference to get it into context.

CHAIRMAN JENSCH: While the witness is examining the document, I wonder if any of the parties can indicate to us whether the Board has been provided with the Idaho Nuclear documents, 1386 and 1389. Has any of the parties sent those documents to the Board? We don't seem to have our files.

MR. TROSTEN: Mr. Chairman, to the best of our knowledge, Applicant has not furnished these to the Board.

MR. KARMAN: Mr. Chairman, I am checking now. My recollection was we sent copies only to attorney for the Intervenors. I am going to check my records.

CHAIRMAN JENSCH: At your convenience, would you send a copy of these Idaho Nuclear reports to Dr. Geyer?

MR. KARMAN: Certainly.

MR. FORD: I can provide the Staff with a list of approximately a dozen Idaho Nuclear Corporation reports that I have or will be using. I also point out that the main reports I am using have also been listed by the Hanauer Task Force on emergency core cooling systems as documents that they consulted in their review of emergency core cooling systems.

CHAIRMAN JENSCH: Let me amend that statement. If the transmittal may include all of the documents which have been submitted in reference to the Idaho Nuclear reports, may I have a copy as well?

MR. KARMAN: We will so endeavor to do, Mr. Chairman.

MR. TROSTEN: I wonder if it would be possible to take a five-minute break for the witness, consistent with what we did yesterday.

CHAIRMAN JENSCH: I was trying to make a long distance call at 10:45. I was hoping to hang on. Mr. Witness, are you able to hang on for another few minutes?

MR. MOORE: Yes, sir.

MR. KARMAN: Mr. Briggs, are you desirous of having copies of these reports as well?

MR. BRIGGS: I think we have them at Oak Ridge.

MR. KARMAN: Thank you very much.

CHAIRMAN JENSCH: Thank you for your inquiry.

MR. FORD: The question was whether Mr. Moore disagrees with the analysis presented here, or the hypothesis presented here in this section of the Idaho Nuclear report, that, "Several conditions occur before and during core reflooding that provide potential for radial flow and subsequent coolant variation in the hottest core regions."

MR. MOORE: There are specific items referenced there that I don't feel are germane. My point earlier was that taking a specific phenomena out of context with the over-all calculation is misleading. The references to potential redistribution I would concur with.

MR. FORD: In terms of taking this Idaho Nuclear document on radial flow in context, do you think it might help simply to give a summary of the report in a brief ten-line abstract? Would it help you to determine your over-all position versus their over-all position if I ask you whether or not you agree with the summary of the substance here in the abstract? Let me read it, if I may. It is only ten lines. Then I will let you study it.

This is a description, a technical description of the simulated emergency flow effects test project on page III.

"This report describes the technical program to the SEFET Project, the simulated emergency flow effects tests project that is designed to determine the magnitude of the radial flow and coolant bypass problem following a loss of coolant accident. Calculations indicate that this problem may cancel the margin of safety previously thought to exist in emergency core coolant systems.

"Since the primary loop pressure drop controls coolant bypass, full length heater rods and full length steam generator tubes will be used in SEFET to obtain the same amount of superheating and loop pressure drop as will occur in pressurized water reactor. The magnitude on radial flow will be controlled by core temperature gradients and the ratio of hot to cold assembly volumes.

"SEFET rods will be placed in a wedge-shaped array and the power to the rods will be controlled to provide as much or more radial flow as will occur in a reactor."

MR. TROSTEN: What is the precise question?

CHAIRMAN JENSCH: I think if the reporter can reread it, as I recall, it is what is his over-all review of the tests in comparison with the Idaho statement.

(The last question was read by the reporter.)

MR. MOORE: I don't agree with the summary. Specifically what is not recognized in the summary is the manner in which the calculations are performed. We take the hot assembly, and in determining the flooding rate into the core, which is the primary parameter with respect to heat transfer during reflooding, we assume the whole core acts as a hot assembly; that all assemblies are hot assemblies. This

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CHAIRMAN JENSCH: Please come to order.

The witness has resumed the stand. Are you ready to proceed, Intervenors?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Please do so.

MR. FORD: Mr. Moore, in the abstract here the kind of wedge-shaped array with full-length heater rods, full-length steam generator tubes and so forth that have been used in these tests to study radial flow, are you familiar with the basic apparatus that's being referred to here, the 300 rod configuration of 45-degree wedge with a six-foot radius of the PWR core?

MR. MOORE: In general, yes.

MR. FORD: Can you tell me does Westinghouse in their evaluation of the role of radial flow in an accident, do you at present have any data from any large-scale cross-sectional geometry of the core?

MR. MOORE: Not specifically for that condition.

MR. FORD: So that the specific kind of tests that are being talked about or are planned under the simulated and emergency flow effects test project, these would be very different kinds of experimental results pertaining to radial flow than the ones that have been formed to date, is that correct?

MR. MOORE: Yes. Although I understand those tests are not planned.

MR. FORD: Is it correct or is your last statement has the effect then of giving us a larger corewise entrainment and a greater mass flow through the system which they reference is an important consideration. The system loop pressure drop is what determines the flooding rate. So we have underpredicted the flooding rate and therefore unpredicted the effect of heat transfer at the hot spot. So that we already have taken into account this variation that they are talking about with respect to the loop.

CHAIRMAN JENSCH: Maybe this would be a convenient time to interrupt your examination. Would it, Intervenors' interrogator?

MR. FORD: Yes.

CHAIRMAN JENSCH: At this time let us recess and reconvene in this room at 11 o'clock.

(A recess is taken.)

IBt2

disputing the fact that the SEFET projects part of the water reactor safety program of the Atomic Energy Commission?

MR. MOORE: It is my understanding it is not.

MR. FORD: I see. Now I refer here to Page 1 of the IN-1387 report which states, and I quote, "The SEFET project is part of a water reactor safety program with the Atomic Energy Commission."

MR. MOORE: It's my understanding funding was not allocated for that particular test.

MR. FORD: You mean it hasn't been allocated yet or--

MR. TROSTEN: Mr. Chairman, I don't understand the purpose of this line of questions. Mr. Moore has been asked a question whether the SEFET program is part of the Commission's program. He has answered it. Now Mr. Ford for some reason keeps asking him the same question, which is not apparent to me.

CHAIRMAN JENSCH: I think in addition, of course, what is the AEC program has to be determined by the AEC. He has given his understanding and the objection is sustained.

MR. FORD: You mentioned before the break that in your analysis you assumed that all assemblies are hot assemblies. Does that mean that if we looked at the radial temperature profile during normal and the same profile for abnormal operation that it's flat across the core or that it's assumed to be flat across the core?

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MR. MOORE: Yes. In the context of calculating flooding rate.

MR. FORD: Now in the context of analyzing radial flow, isn't one of the important potential contributors to radial flow the temperature gradient, the temperature differences radially across the core?

MR. MOORE: Heat flux differences, yes.

MR. FORD: Is it correct in terms of my understanding of the fuel configuration in Indian Point 2 that there are different concentric enrichment zones, such that if we plotted the radial temperature profile at the very midpoint of the mid-plane, it would have certain height. As you go out along the radius it declines within the center enrichment zone and then as you get into the outer enrichment zone it goes up again slightly and then declines again. Is that an accurate general description of the mid-plane radial temperature profile in Indian Point 2?

MR. MOORE: Basically, yes. There is a variation across the radial dimension.

MR. FORD: The variation across the radial dimension, is it correct that these temperature gradients provide the potential for radial flow and subsequent coolant starvations in the hot channels during reflooding?

MR. MOORE: No, not so much during reflooding, because as I indicated earlier during reflooding the main

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differential pressure across the core, as I reflood, is an elevation effect, not a friction effect.

MR. FORD: So that do you, and I will give you the statement to read, do you then wish to contradict the statement of Idaho Nuclear on Page 2 of the report IN-1387, "The temperature gradients and channel blockage expected in the large PWR cores following blowdown provide potential for radial flow and subsequent coolant starvation in the hot channels during reflooding. This potential for radial--" let me just stick to that sentence. Do you wish to contradict that statement of Idaho Nuclear?

MR. MOORE: I wish to read it in contect.

Where are we in the statement? Thank you.

Well, I would agree with the statement with respect to channel blockage in that as I indicated in earlier testimony we calculated the effects of channel blockage in a very conservative way in that we ignored any resistance to possible flow redistribution in the crossflow direction as indicated in previous testimony. We took the reduction in flow, the twenty-five per cent reduction in flow due to blockage as indicated earlier in testimony and calculated its effect on the peak temperature in the blockage region.

The point to remember here is that we never in the calculation of the temperature--we did not take credit for the improved heat transfer due to atomization. So taking a statement

IBt5

out of context in the sense that one argues channel blockage can cause coolant starvation in the hot channels during re-flooding, yes, channel blockage can. Yes, we have evaluated it. We have even evaluated it in beneficial effects and have ascertained the upper bound. So my point is you must take all these statements in context and many of the discussions that may appear in Idaho Nuclear reports don't really reflect the way the actual analyses are performed on a reactor.

MR. FORD: Well now, let me discuss the specific mechanisms which they suggest in that report would cause radial flow.

May I have the report back.

MR. MOORE: Yes.

MR. FORD: Now in your analysis of the mechanisms of cooling that take place during the reflood is it correct that steam-entrained water droplets that would be in the channels several feet above the actual flooding level, that these steam entrained water droplets are essential to the entire heat transferring and cooling process?

MR. MOORE: Yes.

MR. FORD: Is it correct that there are Liedenfrost forces exerted on these droplets?

MR. MOORE: Yes.

end

MR. FORD: Is it correct that Liedenfrost forces cause the droplets to be forcibly ejected from hot surfaces by a build-up of steam pressure between the surface and the droplet?

MR. MOORE: Yes, I believe this is an agitation phenomena.

MR. FORD: Is it correct that as these droplets bounce back and forth in a random fashion within the channel, then they find their way into adjacent flow channels?

MR. MOORE: It is a possibily, yes.

MR. FORD: Is it correct that Liedenfrost forces exerted on droplets in cool channels are not as great as the force on the droplets in hot channels?

MR. MOORE: Yes.

MR. FORD: Is it therefore a possibility that the net result of Liedenfrost forces on the steam entrained water droplets that are important to rod cooling is a tendency for droplets to migrate from the hot central core regions to the cooler periphery?

MR. MOORE: No. I think Liedenfrost effects, as referenced here, occurred short distances as indicated bouncing from rod to rod. I submit that the FLECHT analysis where we had 100 rods in a bundle with a variation in power level from rod to rod, a total radiant of, I believe, 10, 15 per cent -- it is in the FLECHT report -- that these effects, if they were significant, did exist in the FLECHT tests, and therefore our heat transfer coefficients that are derived from the FLECHT tests will probably account for this particular phenomena.

MR. FORD: Is the open lattice characteristic of the pressurized water reactor core simulated in the FLECHT test bundles?

MR. MOORE: The bundles were open bundles, if that's what you mean.

MR. FORD: My obvious point, Mr. Moore, are the walls that enclose the bundle against which the droplets can bounce back into the bundle, and so forth, do these walls simulate walls in the PWR core?

MR. MOORE: No.

MR. FORD: Let us talk about the distance over which the migration takes place. In terms of the general direction of migration due to the Liedenfrost forces, it is correct that the direction will be from hot channels to cool channels; is that correct?

MR. MOORE: Yes, I believe so.

MR. FORD: In terms of the time over which this occurs, is it the case that even though the distance traveled per millisecond is small, that given the times involved are before quenching on the midplane, is it possible that the cumulative effect of these small movements and the allowable time would cause substantial radial flow of the order of several inches or feet from the core hot spot?

MR. TROSTEN: That is a long question, Mr. Ford. Would you mind reading that back to me, please.

(The last question was read by the reporter.)

MR. TROSTEN: Thank you.

MR. FORD: Are you going to answer the question, Mr. Trosten?

MR. TROSTEN: No. I was trying to determine whether I should object to it.

MR. MOORE: No, I don't think so.

MR. FORD: Can you tell me, under the low flooding rates, that is two inches per second or less, what is the time between the occurrence of steam entrained water droplets at the midplane and the final clenching of the midplane?

MR. MOORE: No. Perhaps you don't understand the phenomena. The heat transfer is not turned around by quenching phenomena.

MR. FORD: There is no misunderstanding.

MR. MOORE: O.K. I don't have a particular value for the quench time and the hot spot in mind.

MR. FORD: Let me draw a diagram to help us.

(Diagram drawn by Mr. Ford.)

MR. TROSTEN: Mr. Ford, excuse me. Could you stand in such a way so that all persons in the room and Applicant's counsel could see that diagram?

CHAIRMAN JENSCH: I don't know whether it is going to be possible with that grand piano behind there. Mr. Cahill, I notice, the other day, moved over. I wonder if you all would join him.

MR. TROSTEN: Thank you.

MR. FORD: Can you see it?

MR. MOORE: Yes.

CHAIRMAN JENSCH: The witness, after all, has to have an eye on it, too.

MR. FORD: I mean to represent by this diagram, the core here. By this curve I am trying to indicate what temperature gradient is across the core. This is the curve I earlier described to Mr. Moore. He has agreed with me that this generally represents the curve. I don't claim that the peaks here are in the same proximity as the peaks in the outer regions. But basically the core is hotter here, cooler here, gets hot again and again cooler on the periphery. This is the central enrichment zone and concentric is the outer enrichment zone.

As the flood level reaches the bottom of the core, we are describing, in terms of the steam entrained water droplets, flood level is here bu the steam entrained water droplets are up here bouncing again in the midplane region.

For the purposes of our simple discussion, is this

end

an accurate description?

MR. MOORE: Proceed.

MR. FORD: This phenomenon of steam entrained water droplets cooling the midplane, this is going to take place for some period of time, is it not, until the water level rises such to restore convective heat transfer with the subcooled liquid; is that correct?

MR. MOORE: Yes.

MR. FORD: During the period of time there is the tendency, is there not, for these droplets to randomly migrate into other channels; is that correct?

MR. MOORE: Yes.

MR. FORD: In terms of the time it takes -- if we are out here, say, on the periphery --

CHAIRMAN JENSCH: I am afraid you are going to have to move over to the other side. The witness, I don't think, will be able to follow you until you back off of it. If counsel is having difficulty, join him. I see he has two other seats there. He doesn't mind joining the Intervenors.

MR. ROISMAN: It is we who mind. (Laughter.)

MR. FORD: So is it correct that as the temperature gradient decreases as you go out on any axis from the core, that Liedenfrost migration, the tendency that you expect is that these droplets go in the radial direction? Is that correct?

MR. MOORE: Yes. Except there are variations within the assembly itself, also.

MR. FORD: In terms of the total variation in the assembly and so forth, it is nevertheless possible for this migration to take place in these directions? The difference is not such that the gradient is so small between individual assemblies that you never get, you know, enough differential Liedenfrost force to have the migration; is that correct?

MR. MOORE: There are other forces acting upon the entrained water, mainly the steam which is carrying this entrained water in the upward direction. This is the predominant force, obviously.

MR. FORD: So that when we add the two vectors of the Liedenfrost migration going sideways, and the steam pushing it upward, is it correct that we will be modifying radial flow in directions such as this?

MR. MOORE: But they can bounce back within the individual assembly because they will hit on the hotter surfaces. I cannot comment on the net effect in a quantitative sense as shown there.

MR. FORD: Of course they can bounce back in the same random fashion.

Isn't the point of Liedenfrost migrations that there is a natural selection to the course things follow, namely, even though some of the ones from the cool area might, for random reasons, bounce back into the hot area, nevertheless, the over-all effect of justaposing the hot and cold areas is such that in the main the direction will be from hot to less hot?

end

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MR. MOORE: The point was you can go from a hot to a less hot back to a hot and don't you agree if I went back to a hot it would tend to go the other way?

MR. FORD: That's correct.

MR. MOORE: That situation also exists in the core.

MR. FORD: Is it therefore possible that in terms of the dynamics of the situation that drawing the outer enrichment section is it possible that when things bounce back that the preferential--that they will bounce back into the hot region, that the preferential region for coolant flow will be the low point of the enrichment zone between enrichment circles in the reactor, such that if we wanted to move to the equilibrium picture of all of this the main flows that we would be seeing would be the preferential flow, would be in between enrichment zones?

MR. MOORE: Not due to the Liedenfrost effects. They just aren't large effects.

MR. FORD: Oh. Now let me talk, if I may, in terms of dimensionlist Liedenfrost effects and let me talk, I suppose, in terms here of a value that Liedenfrost effects may have to induce this phenomenon. Not postulating significant Liedenfrost forces is this the flow distribution that you'd expect, namely if you went in the accident and measured the flow in every bundle, that the measurement would be exactly this temperature gradient upside down, that there would be

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little flow here, little flow in the middle and that most of the flow would be in the inter enrichment zone areas?

MR. MOORE: Are you asking me if there is a strong effect which can cause a flow to go from the hot regions to the cool regions, that when I look at the flow from the cool regions with respect to the hot regions I would expect to have less flow in the hot regions?

MR. FORD: Yes.

MR. MOORE: Of course.

MR. FORD: Now the precise question then becomes in terms of whether or not this kind of behavior would cause starvation of coolant here because of preferential flow between the zones. Now the precise question then becomes what is the magnitude of Liedenfrost forces that would occur under the loss of coolant accident conditions.

MR. MOORE: I didn't recognize that as a question.

MR. FORD: It is a question. What is the magnitude of Liedenfrost forces that would exist during a loss of coolant accident?

MR. MOORE: Very small with-respect to the momentum effects of the droplet promoting flow in the upward direction as the droplets are accelerated by the steam.

MR. FORD: Can you set forth the experimental data that Westinghouse has evolved pertaining to measurements of the magnitude of corewide Liedenfrost migration forces?

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MR. MOORE: I refer again to the FLECHT tests where there were one hundred rods with different powers between rods simulating the exact situation we are talking about there, hot rods and cold rods. And there were no significant effects observed from rod to rod that would be postulated in the Liedenfrost.

MR. FORD: I see.

Now let me superimpose on this diagram the test apparatus. Now as I understand the non-random choice of parameters in the FLECHT test, is it correct that you were trying to choose--that in choosing parameters you were trying to represent the central bundle in the core, is that correct?

MR. MOORE: Not essential bundle. The hot assembly.

MR. FORD: The hot assembly?

MR. MOORE: That is not the central bundle.

MR. FORD: It's in reasonable proximity to the center, is that correct?

MR. MOORE: It's usually in the central region of the core, yes, sir.

MR. FORD: So that let's for the sake of locating this hot bundle and central region have a black marker.

First of all, is it correct in terms of the temperature gradient across the mid-plane of the bundle that simply because of the fact that it's one among many bundles that to represent this core as representing within itself a very small temperature gradient compared to the overall temperature gradients in the core?

MR. MOORE: No.

MR. FORD: No. Can you tell me in terms of the peak here relative to the trough what is the relationship between the temperature in this region of the core and temperature in between the enrichment zones?

MR. MOORE: The maximum to average is about 1.3 or 1.4.

MR. FORD: Yes. Now in terms of the FLECHT tests as I understand them, given the mean of one, you were talking about a difference in power from .95 to 1.05. Is that approximately correct?

MR. MOORE: 1.1.

MR. FORD: To 1.1. So that is somewhat smaller. It's a 10 per cent gradient versus a 40 per cent gradient, is that correct?

MR. MOORE: It's a 15 per cent gradient from the cold test to the hot test.

MR. FORD: Well, all right. Additionally is it correct that the radius of this bundle, the maximum radius, is approximately a six or seven-inch area, is that correct?

MR. MOORE: Yes. I think that is about the size of it.

MR. FORD: Yes. Now, in terms of the random behavior of the steam entrained water droplets, in terms of the velocity with which they would be ejected from a rod, can you discuss what the probability is that simply from bouncing back and forth within a six or seven-inch area and the time that you observed it what the probability is that you would observe no radial effects simply because of the velocity of bouncing with the wall there?

MR. MOORE: I don't understand what effect the wall has with respect to the Liedenfrost.

MR. FORD: Well, now is it --

MR. MOORE: You are postulating that Liedenfrost was an effect which caused droplets to migrate from the hotter pins to the colder pins. It would appear to me with the gradients that we had in power in the FLECHT tests where we had one side of the bundle was all rods which were at .95 and rods further into the bundle were 1.1, that your postulation of Liedenfrost effects should also obtain there and cause the flow all to migrate toward the .95 side of the bundle.

MR. FORD: Now is it correct as we discussed the Liedenfrost forces within this bundle, is it correct that in the real reactors some of the steam-entrained water droplets that bounce off the hot rod in this bundle end up in an adjacent bundle?

MR. MOORE: Would you repeat that, please.

MR. FORD: Is it correct that in the random rejection of steam-entrained water droplets from the hot surface of the cladding, is it correct that in that random ejection process some of this, some of the water droplets from here are rejected into adjacent channels?

MR. MOORE: Yes, it's possible.

MR. FORD: Now, is it correct the wall stops that progress into an adjacent channel? It can't go into another channel, is that correct?

MR. MOORE: You are now speaking of the test?

MR. FORD: Yes.

MR. MOORE: Yes.

MR. FORD: So that statistically with this random behavior is it correct to say that the presence of the wall means that there are more water droplets in the test bundle

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than there would be in the accident situation where they could randomly migrate to adjacent bundles?

MR. MOORE: No. I would, I guess, argue that if this Liedenfrost were a significant effect the Liedenfrost would tend to direct the droplets toward the outside of the bundle, hit the wall and cause a collection of water more in the vicinity of the lower power rods. This was not observed.

MR. FORD: The bundle walls are heated as well, is that correct?

MR. MOORE: Yes.

MR. FORD: Now, is the bundle a significant enough heat sink to absorb, to cause the condensation of some of these bouncing water droplets?

MR. MOORE: You said the bundle.

MR. FORD: The wall.

MR. MOORE: As I recall tests were run at different wall temperatures.

MR. FORD: Yes.

MR. MOORE: So that it would depend on the temperature of the wall.

MR. FORD: Yes. Now, over the range of wall temperatures tested were the wall temperatures such that they would provide for the condensation of the randomly moving water droplets?

MR. MOORE: An attempt was made in setting the housing temperature to try to simulate the effects of heat transfer and absorption of adjacent rods. That was the intent of varying the housing. So it was to represent more like the adjacent row of rods.

CHAIRMAN JENSCH: I wonder if the question was something about condensation. Will there be condensation?

MR. MOORE: As simulated in tests it should have been equivalent to the expected behavior of the adjacent row of rods which don't differ dramatically in power to the row of rods in the hot assembly. Recognizing that in our core geometry there are no such phenomena as water spots or things like this which could drastically deter flux from assembly to assembly.

CHAIRMAN JENSCH: Will there be condensation?

MR. MOORE: No. That's a heat --

Yes, there could be some condensation, because it was a heat sink, right.

MR. FORD: Am I correct that in the situations that it's a heat sink it means that the wall of the bundle is much cooler than the adjacent rods would be in core?

end

MR. MOORE: Not much cooler. There were cases where the housing temperature was as high as 540 degrees, as shown in the report.

MR. FORD: My question was, if there was condensation, would the temperature of the wall have to be much cooler than what the temperature of adjacent rods to the bundle would be?

MR. MOORE: Cooler, yes, to get the heat transfer.

MR. FORD: It is cooler than what the adjacent rods would be?

MR. MOORE: Yes.

MR. FORD: So that to the extent that there was condensation of these water droplets, that defines the extent to which the simulation by means of the cooling temperature of the wall, simulation of the bundles outside didn't work?

MR. TROSTEN: Mr. Chairman, may I have the reporter read that question back, please?

CHAIRMAN JENSCH: Reread the question, please.

(The last question was read by the reporter.)

MR. FORD: Is the question clear?

MR. BRIGGS: The question is not quite clear to me. Will you please define to me what you mean by condensation.

MR. FORD: I am referring to Mr. Moore's point that as the --

MR. BRIGGS: Just what do you mean by condensation?

MR. FORD: That the droplets of water come together to form larger molecules, and they fall into the pool.

MR. BRIGGS: Form larger molecules?

MR. FORD: Right. We are talking about small steam entrained water droplets. The condensation is increasing the quality of this fluid, increasing it in the subcool direction.

MR. BRIGGS: You mean condensation of steam on the water droplets?

MR. FORD: I am saying that when the steam entrained water droplets hit the wall, the droplets themselves will come together and collect.

MR. BRIGGS: You mean conglomerate and make bigger droplets or make a film on the wall?

MR. FORD: Right, exactly.

MR. BRIGGS: Thank you.

MR. FORD: My question is, to clarify the question that was reread, is it correct that the temperature of the bundle wall is supposed to simulate the temperature of the adjacent rods if this were an in-pile test?

MR. MOORE: That was the intent.

MR. FORD: Is the temperature on the adjacent rods, the rods adjacent to the full power bundle, is that the temperature such that you would expect, when these droplets hit those adjacent bundles, they will form a film and conglomerate?

MR. MOORE: No. The temperature of the adjacent wall was always above the temperature of the water droplets. So heat was being transferred from the walls to the water droplets.

MR. FORD: My question was, in the accident situation, if the water droplets ejected from what we define as a bundle, and if they hit rods in the adjacent bundle, would there be condensation on the adjacent bundle, the forming of a film or conglomeration of the particles?

MR. MOORE: No. There is steam flowing by and entrained water carrying it on up the bundle.

MR. FORD: You did not make a suggestion earlier that there was condensation of these droplets as they hit the wall.

MR. MOORE: No, I did not. My point was that the wall itself, as you recognize, would impede further migration of any droplets outside the test assembly. My point was that the gradient of temperature power in the bundle, assuming Liedenfrost effects, would tend to migrate the water toward the wall. It could go no further and would be carried further up the wall. So if I observed a large amount of water in the outer regions of the bundle, I could attribute this to possible Liedenfrost. This did not occur.

CHAIRMAN JENSCH: While you are drawing another chart, where would the condensation occur that you referred to earlier in your testimony?

MR. MOORE: I would have no condensation in either the reactor or the test assembly as long as the temperature of the metal was higher than the temperature of the water droplets passing by.

MR. JENSCH: I understood your statement. I think previously you said, yes, there could be condensation. Where would that be?

MR. MOORE: I misunderstood the question, sir.

MR. JENSCH: Well, proceed.

MR. FORD: What I wish to represent here is a rod or a set of rods in the FLECHT test bundle, and the wall of the bundle. In terms of the ejection of the steam entering the water droplets, in terms of the angles and velocities involved, if a water droplet were to be ejected from the hot surface and hit the wall, is the energy of this ejection sufficient that if it is ejected and it hits the wall, will it bounce back in?

MR. JENSCH: I think you better wait until he finishes reading the book he is using there. When he finishes, if you will direct his attention to what you are doing --

MR. FORD: I wasn't asking about any book.

CHAIRMAN JENSCH: He wants to read it, I guess, anyway. When he is finished reading, you can go on with your sketching.

Do you have the question in mind?

MR. MOORE: Yes, I do.

CHAIRMAN JENSCH: Will you answer it, please.

MR. FORD: Is it possible, in terms of the energies involved, that when the water droplet by the Liedenfrost forces is ejected by this hot surface -- this is the fuel rod. -- and it hits the wall, is it possible that this will bounce back into the bundle? It depends on what angle. If it is ejected to here, do we expect the thing to be ejected back into the bundle here? If it is ejected at an angle like this, will it come back into the bundle like this? Are these directions of droplet travel due to Liedenfrost migration? Are these all possible circumstances?

MR. MOORE: No.

MR. FORD: They are not?

MR. MOORE: No.

MR. FORD: Can you indicate which of these circumstances is not possible?

MR. MOORE: It was indicated earlier that the predominant forces are the flow of steam entraining the water.

MR. FORD: Yes.

MR. MOORE: And this is always in the upward direction. So there is no tendency for Liedenfrost to cause any droplets to go in the downward direction.

MR. FORD: The steam is coming up in this direction, is that correct?

MR. MOORE: Yes.

MR. FORD: Assuming this direction is the case, is the migration in the downward direction possible given sufficient energy and angle of the ejection? Obviously it is not exactly what would take place if this vector of steam flow were not here. But is it still possible? For example, instead of having this curve, to have the droplet come back like this because of the upward force of the steam?

MR. MOORE: It depends on the steam flow, that's right.

MR. FORD: Pardon me.

MR. MOORE: Yes, it depends on the steam flow.

MR. FORD: It depends, am I correct, initially, on the energy involved in this ejection, the energy of the Liedenfrost force?

MR. MOORE: Yes.

MR. FORD: So that it is possible, just in terms of the general scenario of Liedenfrost forces, it is possible- if you have a wall, to have the bundle reflected off of the wall and back into the test bundle?

MR. MOORE: I don't understand the relevance of a general scenario. I would presume we would be taking about the issue at hand, which is the FLECHT test and also the loss of coolant situation.

MR. FORD: What I was trying to do was to put a specific quantification of a Liedenfrost force. What I am saying, simply in terms of what we know about the random behavior and ejection phenomena and such, is it possible that given sufficient energy for the Liedenfrost force, that you will have the particles bouncing back off the wall into the bundle?

MR. MOORE: I guess, yes. It is a function of all the forces acting upon the water.

MR. FORD: Let us, for the moment, postulate that Liedenfrost forces are significant enough to overcome the steam flow and to bounce water droplets back into the bundle. If this were the case, if Liedenfrost forces were that significant then there would be -- in the FLECHT test with these walls, is it correct that there would be considerable bouncing back and forth into the test bundle of the Liedenfrost droplets?

MR. MOORE: It is my understanding it was not observed in the test.

MR. FORD: Is it correct that if you looked at this phenomena --

CHAIRMAN JENSCH: Excuse me a minute. I wonder if you would go back to a question. He said it wasn't observed in the FLECHT test. I think the question was, isn't it possible for it to occur.

MR. MOORE: Mr. Chairman, I am having difficulty in following the scenario, as Mr. Ford puts it, and jumping back from FLECHT test to loss of coolant to general aspects. I am having difficulty following the questions along those lines.

MR. TROSTEN: Mr. Chairman, I suggest that question be reread. There was an assumption in there which was very important in the answer.

CHAIRMAN JENSCH: Maybe you can tell us what the assumption is. He said he is having trouble with the question. I am going to have the question restated so the gentleman understands it. If you find the assumption, you may tell us, please.

Will you restate the question. Apparently the word "scenario" threw him in your question. He used it in his answer. Let us go back to the question.

MR. FORD: In terms of the actual FLECHT test situation, is it possible that Liedenfrost forces could be such as to produce the behavior that the ejected droplets would be deflected from the wall and back into the bundle?

MR. MOORE: Apparently not.

MR. FORD: My question was, is it possible that a certain phenomenon did occur?

MR. MOORE: I said apparently not.

MR. FORD: You mean you didn't observe it, and therefore, on the basis of that, you say it is not possible for it to occur?

MR. MOORE: In the specific test that we were observing.

MR. FORD: Do you understand the difference between a question which asks you, is such-and-such possible, and a question which asks you, was such-and-such observed?

MR. TROSTEN: I object. I move to -- I object to that question being asked of this witness.

CHAIRMAN JENSCH: The objection is overruled.

MR. MOORE: Yes.

MR. FORD: In terms of your understanding of the difference between those two forms of questions, I am asking the question, is it possible for the phenomenon of reflection of the ejected particles back into the bundle to take place during the FLECHT test?

MR. MOORE: At the conditions for which the FLECHT test was run? I am asking you. At the conditions for which the FLECHT test was run?

MR. FORD: At the range of conditions for all of the FLECHT tests, yes, and the parameter of the wall in particular.

MR. MOORE: Apparently not for the conditions in which the FLECHT test was run. We are talking about a specific test.

MR. BRIGGS: Mr. Ford, I have a problem here. Did you say, it is possible that this phenomenon did happen in the FLECHT test, or is it possible that it might happen in the tests?

MR. FORD: I am concerned first with whether it could happen in FLECHT-like tests.

MR. BRIGGS: I understood you to say, is it possible that it did happen. You say, is it possible that it could happen in such tests; is that correct?

MR. FORD: With your clarification, let me re-ask the question.

Is it possible that with the FLECHT test kind of setup with a bundle wall surrounding the bundle, is it possible that you could get, with this general setup, reflection of ejected particles back into the bundle?

MR. MOORE: Yes.

MR. FORD: If this did happen inside the small bundles used, if this did happen, when you actually looked at the bundle to see whether or not there was a preponderance of particles here or a preponderance of particles there, checking to see whether there was clear migration, is it possible that if there were reflection, then the average equilibrium view you get at looking at the bundles, is such that the particle distribution is pretty much the same every place?

MR. MOORE: No. I would feel that the reflection phenomena would be limited to the outer region or possibly just inside the channel from the outer tubes. I would characterize it as a total -- a large area averaging -- I guess I would not expect that.

end

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MR. FORD: Now is it possible just in terms of the geometry of this small rod, is it possible for rods other than the rods in the very parameter, is it possible for them to get a straight shot at the wall of an ejected particle?

MR. MOORE: With some difficulty I guess, yes.

MR. FORD: Is it possible for a particle at the center through recoil reflection off of another tube in the FLECHT test assembly, is it possible through that intermediate reflection that they get a shot at the wall?

MR. MOORE: I'm afraid I don't understand that. Could you draw the geometry for me?

MR. FORD: Sure.

Is it possible, for example, that we shoot from here and we hit here and then we hit the wall over here? This representing a corner of the assembly.

MR. MOORE: Geometrically and to scale that looks very difficult to do.

MR. FORD: I see. Now simply let me make it easier. Is it possible to hit a rod at a 45-degree angle and go out and then hit the wall out here?

MR. MOORE: That's a function of the spacing. You'd have to do it in a geometrical way.

MR. FORD: Yes. I am looking at the diagrams on Page 2-8 and 2-9 of WCAP 7665.

MR. MOORE: Right.

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MR. FORD: Now doesn't the entire spacing--it simply scales down. Instead of having this giant region here we are just dealing with the smaller one. But in terms of the angles and the relationships of the rods isn't it still the same?

MR. MOORE: I could pack these at various channel spacings.

MR. FORD: Oh, yes.

MR. MOORE: And make it more or less probable that a droplet could be ejected out to the wall.

MR. FORD: Oh, yes. Now my question is in terms of the channel spacing that's used in the FLECHT tests it's the same spacing between all of the rods, as I understand it.

MR. MOORE: Could you draw me the exact channel spacing which you are postulating for possible direction of the droplet?

MR. FORD: Sure.

I don't want to do it with a black pen but I'd be happy to do it with a pencil.

MR. MOORE: All right. I will grant you that's a possible situation.

CHAIRMAN JENSCH: You have made a mark on WCAP--

MR. MOORE: 7665, Page 2-9.

CHAIRMAN JENSCH: Thank you.

MR. FORD: I demonstrated by the mark how a particle ejected by a rod internal to the bundle could, through recoil,

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find its way outside of the bundle and hit the wall.

MR. TROSTEN: Is that a correct statement, Mr. Moore, the statement that Mr. Ford just made?

MR. MOORE: Yes, I believe so. Lookingat his geometries. Of course, I haven't considered all the vector forces and the action of the rod itself, but I would grant the postulation.

MR. FORD: So that it is possible to have, in terms of the distribution of water droplets across the bundle, it's possible for central rods to be ejected in the same way, in the same way rods in the side are ejected, is that correct?

MR. MOORE: Yes.

MR. FORD: Now if this happened during a test with FELCHT-like apparatus, is it possible that this could reach an equilibrium such that the distribution of particles across the radius of the bundle was fairly constant, as I draw here?

MR. TROSTEN: Mr. Chairman, I object to that question because I do not see the direction in which Mr. Ford is going now. We have been talking about FLECHT tests.

I ask for him to demonstrate the relevance of this question.

CHAIRMAN JENSCH: That's an interesting inquiry you have. Sometimes the cross-examiners don't pattern their projection and each question is taken directly. I take it that this question is related to the preceding one as to the

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angular recoil operation.

Objection overruled.

What is the answer?

MR. MOORE: Could I have the question repeated then?

CHAIRMAN JENSCH: Read the question.

(The previous question is read by the reporter.)

MR. MOORE: No.

MR. FORD: Is it the case that the presence of the wall would make the density of the droplets more uniform across the radius than if the walls were not there, and some of these particles would move on to other bundles?

MR. MOORE: Well, we are in an area of conjecture here, but not conjecture with respect to that is that if we are postulating Liedenfrost effects with large forces due to heated surfaces, the effect of the Liedenfrost, the large effect would tend to, as you are suggesting here, cause the droplets to impact the channel, the wall. I would expect an aglomeration of droplets being carried up along the channel if in fact Liedenfrost forces were large, as you have postulated.

MR. FORD: Do you expect condensation now?

MR. MOORE: Not condensation.

MR. FORD: Aglomeration?

MR. MOORE: Aglomeration. They are hitting, recalling back, still in the channel, being carried upward by the steam.

MR. FORD: Are you saying that the energy with which they hit the wall is not sufficient to bring them back into the bundle?

MR. MOORE: The postulation was that the Liedenfrost effects were strong and were ejecting or expelling the droplets toward the wall, and I would expect the change in momentum associated with turning the droplets to be a smaller force, since the wall is not as hot a surface as you indicated as the rod itself. We are looking at the relevant forces.

MR. FORD: Yes. Now, my question is I'd like to stop down from the postulation of Liedenfrost forces in which we don't talk about their magnitude. The specific discussion of the magnitude of these forces, the question is, is the magnitude of the force such that it would provide enough force, given even the loss of momentum when it hits the wall, would give enough force to the particle to be recoiled back into the bundle?

MR. MOORE: I don't know.

MR. FORD: Can you explain to me -- I don't want to have you go over ground that we began on last Tuesday and repeat it all, but can you explain to me basically how you used pressure measurements from the two pressure tabs to determine that there is no radial flow?

MR. MOORE: We measured the pressure inside the bundle and outside the bundle and found a very small or negligible pressure gradient across the bundle.

MR. FORD: Does that measurement of radial flow, is that measurement germane to this specific mechanism of radial flow, namely, Liedenfrost migration?

MR. MOORE: I believe so.

MR. FORD: Now, to get ahead of ourselves slightly, one of the other mechanisms of Liedenfrost migration is the pressure differences from differential steam expansion within the bundle. Now, if that were the mechanism, your measurement of pressure differences would have some clear applicability, is that correct?

MR. MOORE: You characterized that as a Liedenfrost effect.

MR. FORD: No, no. I am saying that is a different mechanism for radial flow than Liedenfrost migration. That's a mechanism because of pressure differences. This is not a mechanism because of pressure differences.

MR. MOORE: Quite the contrary, you are expelling the droplets through the transfer and you are creating higher pressure regions which cause a force on a liquid and postulating that it then goes from one part of the bundle to another part of the bundle. The flow has to occur as a result of a force imposed and you can measure this force imposed and you can measure this force imposed by measuring the resultant pressure drop.

MR. FORD: I see. Now, in your analysis of pressure here are you assuming that the coolant here is in a thermodynamic equilibrium such that you would talk about one pressure for the coolant within a well-defined bound?

MR. MOORE: Speaking of the pressure at a specific pressure transducer.

CHAIRMAN JENSCH: What is the answer, yes or no? Are you assuming coolant is an equilibrium?

MR. MOORE: That's not a question that can be answered yes or no.

CHAIRMAN JENSCH: Oh.

MR. MOORE: I answered that the pressure is pressured at a specific point.

CHAIRMAN JENSCH: Very well. Proceed.

MR. FORD: Well, I am not too happy with the preceding.

Do you assume, when you measure pressure, that the measured pressure represents the average pressure over a volume of the bundle?

MR. MOORE: No. I assume -- I measure the pressure at a pdnt in the bundle.

MR. FORD: So that you measured it at two specific points in the bundle, is that correct?

MR. MOORE: That's correct.

MR. FORD: So is it possible that in other points of the bundle there ae different pressures but which you are not measuring on those two points?

MR. MOORE: Yes.

MR. FORD: So that for example the specific pressures associated, you know, with the forces of ejection, the pressure exerted by the hot surface on the droplet, that is not a pressure that you are measuring, is that correct?

MR. MOORE: It's correct.

MR. FORD: So that it's possible for there to be pressure differences within the core intrinsically associated with Liedenfrost migration that are not recorded by your two pressure tabs?

MR. MOORE: Of an individual droplet?

MR. FORD: Yes.

MR. MOORE: Of course.

MR. FORD: Now of the whole, in terms of the location of the pressure tabs can you in terms of this diagram tell me if these are full-length cores, can you tell me where the pressure is measured by your two tabs?

MR. MOORE: It will take some time. I'd have to refer to the report.

CHAIRMAN JENSCH: Maybe this would be a convenient time to take just a few minutes' recess while you are looking that up. We will recess and reconvene in this room at 12:25.

(Brief recess.)

NWt1

CHAIRMAN JENSCH: Please come to order.

MR. TROSTEN: Mr. Chairman, I have been conferring with the witness during the recess. I understand that Mr. Moore requires some additional time in order to check some data in order to respond to this question. Is that correct, Mr. Moore?

MR. MOORE: Yes.

MR. TROSTEN: Under these circumstances, Mr. Chairman, in view of the time--I believe it is approximately 12:30 now and the witness has been on the stand, with brief recesses, for three and a half hours. I would suggest to the Board that we consider or have a lunch break at this time.

MR. FORD: Mr. Chairman, during the break I took the opportunity to see if I could clarify the matters that are presently in our minds with some improved diagrams. I'd be happy to take the break simply after we go through these diagrams while the whole thing is very fresh in our minds.

MR. TROSTEN: I reiterate my suggestion, Mr. Chairman.

CHAIRMAN JENSCH: Can you hang on, Mr. Moore?

MR. MOORE: I'll hang on, sir.

CHAIRMAN JENSCH: Good of you. Thank you very much. Will you proceed.

MR. FORD: I have tried to draw two cross sectional diagrams here at the mid-point, let's suppose, of the core. I'm going to talk about the ejections of three droplets.

NWt2

In the diagram representing the presence of a wall around it, I roughly trace what the possible path would be for the particles. Of course, in the actual situation when there is no wall such as this, deflecting particles back into the core--I have tried to show that as well.

I take one particle here that travels a small distance and it ends up, in my drawing, between the rods and the wall. If the same phenomenon occurred when there was no wall, of course, the particle would be ejected outside of the wall and it could have a different kind of geometry than a wall at some point here, and it may have some further direction into the bundle, it is more than likely that it will continue outside of the bundle.

The probability here is that there is some question which way it will go, in or out. Here there is no question which way it will go. It is going to stay in the bundle.

I have done this for a second particle showing a possibly more extensive zigzagging, and for a third particle, to show how it might bounce around the core.

For the corresponding particle, whose ejection initiated the corresponding rods, how it would go outside of the bundle, and its potential to get back into the bundle is much less with these things coming back into the bundle.

My question is, in terms of these diagrams, if we had a camera that could get in and take a cross sectional

NWt3

picture of the FLECHT bundle and have a bundle, or a collection of rods in the core without a wall surrounding them, if we were just taking pictures of these three particles, is this a fair representation of what this special camera would show?

MR. MOORE: Not necessarily. You must indicate what assumptions you are making. You have drawn the vectors. you have assumed the magnitude of the forces and you have assumed how these things are randomized and bounce from rod to rod.

MR. FORD: I appreciate that.

CHAIRMAN JENSCH: Have you infished?

MR. MOORE: Yes, I have finished.

MR. FORD: Let us assume that I gave you that these were corresponding forces, that they were in corresponding directions from corresponding rods. The only difference in a situation, simply the presence of this wall in the former case, in the absence of the wall in the outer case. In terms of what this picture shows of the relative density of water droplets in the bundle with the wall, compared to what it shows about the relative density of particles in a situation without the wall, is that indication of relative density reasonable?

MR. MOORE: No, not necessarily.

MR. FORD: Can you explain why this relative density

AWt4

is not correct?

MR. MOORE: This is looking at a cross section, right?

MR. FORD: Right.

MR. MOORE: You assumed the only force acting on this droplet is a force to eject it out in this direction. For some reason it goes all the way out there, although we recognize there are rods all around.

Depending on the force with which the vector ejects this, there is a steam velocity going in this direction, which would cause that droplet to stay in that particular region right there. Giving an upward lift and not letting the droplet go all the way out, that is.

MR. FORD: If we adjusted both situations here, and changing the angle of this vector, we would be changing the angle of this particular force; is that correct?

MR. MOORE: Yes.

MR. FORD: When you draw the cross section of this, and reflect it all down on the axial midplane, is it correct that this would be the representation?

end

MR. MOORE: No. If the velocity, the relative forces, are such that this droplet were being impinged and ejected in this direction, it's very possible that that droplet will never reach that particular rod, depending on the velocities in which --

MR. FORD: Well, now, the rod --

MR. MOORE: In which case the same rod, if a droplet were doing the same thing here, would also be raised up.

MR. FORD: I see. Now, the direction of the force as you indicated before is unidirectional. It's up.

MR. MOORE: That's correct.

MR. FORD: Straight up. So that this rod comes out straight up and it wouldn't have at this point. The force would raise the vector as we indicated before. It would just hit it at a higher point, is that correct?

MR. MOORE: That's possible, yes.

MR. FORD: So that in terms of the cross section reflecting all of this down in the axial midplane, then this density comparison is correct.

CHAIRMAN JENSCH: Let him answer that one.

MR.MOORE: I don't see this as a density comparison. I see this as a measure of trajectories of various rods of droplets, assuming that there is no component flow in the axial direction. At an instant in time certainly I have traced the path of a droplet. It's not the one axial low, right?

MR. MOORE: Yes. No. But do you understand what I am suggesting when I talk about since the additional force is unidirectional and coming through the bundle like this, do you understand that it's therefore possible simply to take and project the vectors? Do you understand what I mean, to project the vectors on a plane?

MR. MOORE: Yes. But you aren't asking me about a projection of the vectors. You are asking me about a projection of density of drops at a given plane.

MR. FORD: Yes. Now, what I am talking about, once we have the projection, the point is that is it clear from the projection that this particle with this force will hit this rod? That was correct that all the projection doesn't show is the axial point at which it hits the rod?

MR. MOORE: It would hit at a different axial location.

MR. FORD: But in reference to the particle it still will be in the sequence indicated here. It still will be within this bundle and it still will be impingent upon this rod at some level.

MR. MOORE: At some level.

MR. FORD: Right.

MR. MOORE: But if I am looking at a cross section here I have got droplets here that are coming from other levels. You haven't indicated those at all.

MR. FORD: Right. I'm only talking about three droplets, just so that we can study them separately.

Now, my point is if we step back from the projection and if we specify a specific volume is it correct that the number of particles in the volume with this wall or the number of these three particles that stay within this wall, assuming that there are only -- let me step back. Let's assume that there are only three water droplets in the whole bundle, in both bundles, all right? We will just discuss there are only three droplets. Is it clear that in this situation as you go up as these things are increasing in axial height here, is it clear that the density of this bundle with a wall will be higher than the density of this bundle without a wall simply because there is no other way, other things for these particles to do than stay inside this volume, whereas these things can go outside?

MR. MOORE: Lest we forget, we may have some coming this way, right?

MR. FORD: That's correct. Now, in terms of what we expect, this being the hottest rod and this being cooler, we expect, do we not, as we agreed earlier, that the preponderance of migration will be that way rather than this way?

MR. MOORE: Not necessarily. I indicated that the assembly next to the hot assembly was an assembly very close in power level and could have rods within that assembly at higher power levels than the rods at the edge of this assembly.

end

MR. FORD: Yes. But on the average when you talk about the temperature gradient across the core, if we took a locus of points within an epsilon of each other in power versus the next concentric circle within two epsilons of that, is it correct that we would expect the flow to be from the center to the outer?

MR. MOORE: That's very difficult to answer. There are a lot of effects you are ignoring here. It becomes a very hypothetical question. I think you will admit that I have droplets which can be ejected toward this assembly because there are hotter rods next to the cooler rods in other assemblies. So if I take your postulation of these forces being large I have got interaction of droplets and it's just a very hypothetical situation that we are talking about.

MR. FORD: Now is it correct that in the situation with the wall, in the bundle, that the only directions available to the little droplet are within the bundle up until the point when they are falling out the top?

MR. MOORE: Yes.

MR. FORD: And here are there more than that direction available? Can they go outside of their bundle?

MR. MOORE: Yes.

MR. FORD: Thank you.

CHAIRMAN JENSCH: Is this a convenient place to interrupt your examination?

MR. FORD: There is one final question that I would like to ask on this mechanism for radial flow.

Mainly, do you agree with the Idaho Nuclear analysis of Liedenfrost migration as presented in IN 1387, page 5, which I will give you to read, and I will also read out loud the first part of it which is a definition of Liedenfrost migration. The second part is their hypothesis as to its effect.

It says, "Liedenfrost migration.

"During reflooding, steam-entrained droplets greatly aid rod cooling many feet above the water level in the reactor core. The Liedenfrost forces exerted in these water droplets cause the droplets to be forcibly ejected from hot surfaces by a built-up of steam pressure between the surface and the droplet. As these droplets bounce back and forth in random fashion within a flow channel, many find their way into adjacent flow channels. The Liedenfrost forces exerted on droplets in cool channels are not as great as the force on droplets in hot channels. The net result is a tendency for droplets to migrate from the hot central core regions to the cooler periphery. Thus Liedenfrost migration results in hot spot flow starvation."

The question is whether you agree with that statement?

CHAIRMAN JENSCH: You may sit down, Mr. Moore, if you find it more convenient, bearing in mind that you have had a long session this morning. I think Applicants' counsel underestimates your ability in thinking that you are about to be fatigued. You are standing.

MR. MOORE: I will sit.

I would agree with the characterization of the physical phenomena and I am at the same problem I had with many such reports and statements such as this, that there is a tendency of droplets to migrate from the hot central core regions to the cooler periphery, yes, and thus Liedenfrost migration results in hot spot flow starvation possibly. But we have the age-old problem which is, does it make any difference and is it a real effect? And I can only agree with the statement with respect to the phenomena represented.

MR. BRIGGS: Mr. Ford, could I ask a question. I am not familiar with that document and apparently you studied it.

MR. FORD: Yes, sir.

MR. BRIGGS: Is there any attempt in the document to quantify tendency in starvation?

MR. FORD: Yes, sir. The document presents a variety of data on the radial temperature profile at mid-plane after a loss of coolant accident, and in terms of that data it analyzes the magnitude of the flow effects that could occur.

MR. BRIGGS: The magnitude of the Liedenfrost effects?

MR. FORD: Yes, sir.

MR. BRIGGS: Thank you.

MR. FORD: The document number is IN-1387, it's entitled, Technical Description Simulated Emergency Flow Effects Tests Project; this is published in June 1970.

MR. BRIGGS: Thank you.

MR. MOORE: Mr. Ford, could you state where the reference to the Liedenfrost force is? You mention they calculated the magnitude of the Liedenfrost effects, forces.

MR. FORD: I have to use the document to prepare my-- there are a variety of other mechanisms for radial flow and I have to use the document during the break, but this evening if you'd like to study the document I'd be happy to lend it to you.

CHAIRMAN JENSCH: Or if you can at some time give him the specific data that he is seeking.

MR. MOORE: In reference to the specific calculation you mentioned they did of the Liedenfrost forces.

MR. FORD: It's not clear to me as I look at this document that all of the calculations they were concerned with are presented in a self-contained form here. So I am not sure. I say, as I indicate, I can give you the document. It has it all there, references.

MR. BRIGGS: Does the Staff have a copy of the document that I can borrow? That would be fine.

CHAIRMAN JENSCH: Very well. At this time let us recess to reconvene in this room this afternoon at 2:00 o'clock.

(Luncheon recess.)

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

CHAIRMAN JENSCH: Please come to order.

The witness has resumed the stand. Are you ready to proceed?

MR. FORD: Mr. Chairman, before we proceed, I would like to return to my answer to Mr. Briggs' question concerning the Idaho Nuclear report, IN-1387. During the break I have studied the report carefully with Mr. Briggs' question in mind. I have also talked to the technical contacts at the Idaho Nuclear Corporation listed on page II of the report, namely, Mr. R. W. Shumway.

The direct question of Mr. Briggs, as I understand it, is whether or not these specific forces associated with Liedenfrost migration had been quantified in this report.

The answer to that question is no.

I can explain, if you were interested in why this particular computation was not done.

MR. BRIGGS: No, I don't think so. I think part of the problem is, the report used the words "tendency" and "starvation," and these really aren't very quantitative. Starvation implies tome that all the flow is gone, everything bad happens, and sometimes people use it that way and sometimes people don't. But part of the question really was, is there any indication of the importance of the Liedenfrost effect to the radial flow.

MR. FORD: I think the analysis in the document establishes a hierarchy of forces determining radial flow. Table 1 of the document, page 15, a table of reactive pressure drops across the core under normal situations and under loss of coolant accident situations, I think. Then the argument in the report is to the effect that the pressure drops here are clearly being main driving force behind the radial flow, and the Liedenfrost migration is something that is argued on the basis of presumptive evidence, and is no indication that while they believe it will get you a few bundles away from the given bundle, they are subsuming this effect in an over-all effect of pressure drops that get you completely across the core.

I think in their analysis they set up this hierarchy here between factors influencing radial flow, and pressure gradients are much more influential determinant or force than the Liedenfrost migration is.

end

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THE CHAIRMAN: Very well. Thank you. Will you proceed.

MR. FORD: For the purpose of comparing the cross-examination of Mr. Moore on matters and diagrams contained in the document IN-1387, I am wondering if we could ask the Staff if they could loan their copy to Mr. Moore to save shuffling about.

MR. MOORE: Excuse me a minute. I have a copy.

MR. FORD: Oh, you have a copy. Fine.

I might make one final prefatory note, that the question of steam expansion is a mechanism for radial flow, that there was some interrogation of Mr. Moore on this question beginning on the transcript Page 2342, and there will be a slight repetition of some of the questions there, as I have tried to ask them specifically in the context of the data presented by Idaho Nuclear. But it is not careless repetition. It's with due consideration of what is in the transcript.

MR. TROSTEN: Is that of the old or the new transcript, please?

MR. FORD: That's in the old pagination.

MR. TROSTEN: Thank you.

MR. FORD: Mr. Moore, is the following comparison of the steam expansion as a mechanism of radio flow with Liedenfrost migration accurate, and I am reading from Page 5 of the Idaho Nuclear document 1387. It says:

"Radial flow due to steam expansion is not a random process, as is the Liedenfrost migration. Radial pressure differences caused by radial steam expansion are continuously communicated over wide distances, whereas the process of Liedenfrost migration is a random, channel-to-channel process."

MR. MOORE: Yes.

MR. FORD: Is it correct as it is further stated that "During the radial steam expansion process steam flows continuously toward lower pressure regions."

MR. MOORE: Yes. Steam always flows toward lower pressure regions.

MR. FORD: Is the conclusion that's drawn here correct that "Radial pressure gradients caused by the differences in radial steam generation is thought to be a more important factor in radial flow than Liedenfrost migration"?

MR. MOORE: Yes.

MR. FORD: Referring to Table 1 on Page 15 of IN-1387, it presents the results of Idaho Nuclear calculations on reactor pressure drop across the core, including nozzles, across the hot leg, across the steam generator, across the pump sumption leg, across the pump discharge leg, does the Applicant contend or would the Applicant deny, rather, that assuming for the moment that the calculations are accurate, does the applicant deny that the calculations in Table 1 demonstrate that a high probability of emergency core coolant

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system bypass exists with the pressure drop situations reported there?

MR. MOORE: Yes, I deny that.

MR. FORD: Do you deny that the data in the table is an accurate representation of pressure drops across the core?

MR. MOORE: Excuse me. An accurate representation?

MR. FORD: Yes. Do you deny that it's an accurate representation?

MR. TROSTEN: Mr. Chairman, I'd like to have the question clarified. Across which core?

MR. FORD: Across the core of a large, pressurized water reactor in a loss of coolant accident situation.

MR. TROSTEN: Well then I object to that question, Mr. Chairman. I don't see a showing of relevance there.

MR. FORD: The study is concerned, as it indicates, with large pressurized water reactors. It's not concerned in specific--it does not mention Indian Point 2. I am trying to find out whether, first of all, as the information relating to this generic class of systems, whether it's correct.

MR. TROSTEN: I don't think that's an adequate showing of relevance, Mr. Chairman.

MR. FORD: I would suggest that it's standard procedure here to be relying on data that isn't specifically designed to represent the Indian Point 2 core. For example, the data that the Applicant has submitted pertaining to single rod and multi rod tests, this is not data designed on cores specifically Indian Point 2.

CHAIRMAN JENSCH: Do I understand there are two tables on this page, or two columns, one of which is referring to a specific reactor, and another is referring to PWRs in general; is that correct?

MR. FORD: Yes. The table concerns pressure drop data from normal operation that was taken from the Sequoia Preliminary Safety Analysis Report. There is a reference for that here.

CHAIRMAN JENSCH: And the other column?

MR. FORD: I believe the other column also relates to Sequoia.

CHAIRMAN JENSCH: What is your objection?

MR. TROSTEN: My objection, Mr. Chairman, is that the question appears to me to be addressed to the general subject of PWR technology and not sufficiently related to this particular proceeding and the inquiry addressed to this particular witness to be a proper question. That is my objection.

CHAIRMAN JENSCH: I think ultimately these things must be related in some way to Indian Point 2. As I understand the FSAR, they have relied upon PWR experiences and operations and designs and developments and tests and experiments in general. Whether or not Indian Point 2 -- as I understand the data, they are used to indicate a degree of confidence that the Indian Point Station is reasonable and proper. I think it is very difficult to compartmentalize a technology on PWRs.

This gentleman on the stand is an expert witness. He is representing the manufacturer here, and has had a great deal of experience with many kinds of PWRs. I'm sure he won't be confused if it is wholly unrelated to Indian Point 2. I think within the concept of having all PWR technology available for analysis of Indian Point 2, then it becomes relevant.

MR. TROSTEN: May I make a general observation about what you are saying?

CHAIRMAN JENSCH: Yes.

MR. TROSTEN: I take it you are overruling my objection.

CHAIRMAN JENSCH: I was getting to that.

MR. TROSTEN: I agree with the general point you are making, but the objection I am making to this question is as follows: This is an example of the kind of question that Mr. Ford has propounded on several occasions. Sometimes I have objected to it and sometimes, in an effort to expedite the proceeding and not hold things up, I have refrained from objecting. He would ask a question related to the technology generally, and it isn't sufficiently related to the Indian Point proceeding to really be a proper question.

Another thing Mr. Ford does, what I believe can confuse a witness who is up there for a long time, is to go back and forth from this test to this reactor, to the technology, and so forth. I think it is difficult for a witness to always perceive those things. I object to the question when Mr. Ford does that.

CHAIRMAN JENSCH: You certainly are showing an objection that we haven't discerned in the witness. I think the witness is showing capability to discuss the PWR technology in general and in specifics and in theory and in practice. I think he has indicated that sometimes questions aren't clear to him, which he should do. I don't think, with an expert witness, we have the concern of confusion that might otherwise be for a general witness.

Your objection is overruled.

Do you have a question in mind or can you restate the question?

MR. FORD: I willrestate the question, sir.

The question concerns Table 1 of the report, IN-1387, which are reactor pressure drops in normal reactor operation and in situations as they would exist when the emergency core coolant system is operated. The data is for a large pressurized water reactor taken from the Sequoia Preliminary Safety Analysis Report.

My question is, whether the relationship here between normal pressure drops and pressure drops in the emergency core cooling operation situation, whether this rWm4

relationship as shown in Table 1 is an accurate representation of the relationship that would be expected in a large pressurized water reactor with emergency core cooling situations as compared to normal operations.

MR. MOORE: No, they don't look correct.

MR. FORD: Do they look correct, if not for reactors in general, for the Indian Point 2 reactor?

MR. MOORE: No.

MR. FORD: Have you performed, for the Indian Point 2 reactor, or taken the equation that has been used and performed the same kind of calculation?

MR. MOORE: Yes.

MR. FORD: Have you reported these to us earlier?

MR. MOORE: I don't recall. I was checking a --

MR. TROSTEN: What do you mean by reported to us, Mr. Ford?

MR. FORD: Somewhere on the record in previous testimony in documents supplied to the Board or the parties, has this information been provided?

MR. MOORE: Excuse me one minute. I want to check one more reference. In the June 1, 1971 report we indicated some pressure drop numbers in the system for another reactor that was not Indian Point 2. The reason for my statement that I question the calculations here is that in the reflood part of the analysis to which they were referring, the pressure drop across the reactor coolant pump is by far the largest pressure drop, and that doesn't appear to be the case for the numbers they have there. That's the main difference.

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MR. FORD: Can you give me the precise reference in this 6-1-71 confidential document?

MR. MOORE: Page 59.

MR. FORD: Now, as I study page 59 of the 6-1-71 document and compare it with page 15 of the Idaho Nuclear report, am I correct that you are computing pressure drops over the different core regions? For example --

Oh, yes. Are these figures proprietary? Do we have any trouble at the moment?

MR. MOORE: No.

MR. FORD: Fine. Then my question was the pressure drops computed are computed for different regions of the core, as I can tell. There is one pressure drop that seems to be the same, am I correct, that is the pressure drop across the steam generator.

MR. MOORE: It's about the same, correct.

MR. FORD: It's both calculation for the same pressure drop and approximately the same value, is this correct, 32.0 psi, for the 6-1-71 document? And 32.3 psi for the Idaho Nuclear calculation.

MR. MOORE: That's correct.

Excuse me. That is not an Idaho Nuclear calculation

MR. FORD: Well, for your other calculation, is that what you mean?

MR. MOORE: The 32.3 is not an Idaho Nuclear number.

MR. FORD: Yes. 32.3, that is your number in the Sequoia PSAR.

MR. MOORE: That's correct.

MR. FORD: Are there other directly comparable temperature drops? It says here, for example, "A computer pressure drop across the core, including nozzles."

Now, do you have an across the core including nozzles pressure drop that's computed from 52.0 psi?

MR. MOORE: No. My --

MR. FORD: Yes or no, please.

MR. MOORE: I cannot tell from the numbers stated on page 59.

MR. FORD: Yes. Well, that's --

MR. MOORE: I thought I explained the differences in the pressure drop during the ECC operation. That is not consistent with the analyses that are performed for the reflood calculation.

MR. FORD: Yes. Now, I want to get straight there are two pressure drops here, one for normal operation and one if the emergency core cooling system is operating. I want to get clear first whether for the pressure drops for normal operations you challenge this part of Table 1.

MR. MOORE: No. I said, do they look reasonable?

MR. FORD: I see. Now, on page 59 of the 6-1-71 report on emergency core cooling performance our pressure drops during emergency core cooling operation calculated?

MR. MOORE: The key parameter is the pump resistance, which is indicated there, which is not a normal operating pump resistance but for a locked rotor.

MR. FORD: Yes. My question is in the 6-1-1971 document do you calculate the pressure drops across the core, including nozzles, and across the hot leg, across the steam generator and so forth for the emergency core cooling system operating conditions?

MR. MOORE: The conditions -- no. The conditions under emergency core cooling are not specifically spelled out.

MR. FORD: Since the data for normal operation here is correct, then do we assume or do you agree that the method that they use for going from normal pressure drops to pressure drops under emergency core cooling system operation are correct?

MR. MOORE: Yes. With the exception of the treatment of the reactor coolant problem.

MR. FORD: So that to get this clearly on the record, is it correct that the equasion given on page 13 for the pressure drop in each section of the loop, that equasion is the correct way of calculating pressure drop?

MR. MOORE: Yes.

MR. FORD: How have the computations of pressure drops during loss of coolant accident conditions been made, say for Indian Point 2, the same ones that are made here for Sequoia?

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MR. MOORE: Have the calculations--

MR. FORD: Yes. Have you taken the equation which you said is the correct equation to use and have you implemented the analysis of pressure drops for Indian Point 2 the way in which it's implemented here for Sequoia?

MR. MOORE: Yes. With the exception of the treatment of the reactor coolant problem.

MR. FORD: I see. Now can you tell me where these calculations are reported, what testimony that has been provided by you, what documents you have submitted to the Board are these? What part of the FSAR, et cetera, contains these computations of pressure drop?

MR. MOORE: Yes. The reference in the 6-1-71 report indicates that we used the normal pressure drops for the system as a basis for the loss coefficients for the pressure drop under the loss of coolant, with the added conservative assumption that we have a locked rotor, which increases the resistance in the loop to such a degree as to reduce the flooding rate. And that is described quantitatively in the 6-1-71 report, I believe, just for Turkey Point, a three-loop plant, but is also described qualitatively in the July 13 testimony for Indian Point 2.

MR. FORD: So that the calculations which you performed do not use the equation on Page 13, is this correct?

MR. MOORE: As I said before, with the exception of

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the reactor coolant pump we determined a new loss coefficient for the reactor coolant pump, which is different than one would obtain from normal operating conditions. This is an additional conservatism.

CHAIRMAN JENSCH: But otherwise you used the same equation?

MR. MOORE: Yes, that's what I said before.

MR. FORD: Now in your equation do you get smaller pressure drops than the pressure drops computed for Turkey Point? Excuse me. Computed for Sequoia.

MR. MOORE: The pressure drop around the system varies in time and it's equal to the height of water in the downcomer during the transient period. This particular analysis appears to be a parameter analysis with an assumed inlet flow. We don't assume an inlet flow. We calculate the flooding rate during transient.

MR. FORD: One of the parameters of the equation on Page 13, the parameters defined on Page 15, is the mass flow rate. So that are you saying that in addition to a different loss coefficient called K in the equation that you also use a different fluid mass flow rate?

MR. MOORE: I haven't calculated the fluid mass flow rate.

MR. FORD: But once you have performed that calculation, when you have put that variable into the equation on

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page 13 you are using a different fluid mass flow rate than was used here, is that what your point is?

MR. MOORE: It's like calculating a different fluid mass flow than this arbitrarily assumed particular calculation that's referred to in this report.

CHAIRMAN JENSCH: Is this a change in the equation?

MR. MOORE: There certainly has been generated a lot of confusion here.

The Idaho report is depicting a calculation of the pressure drop that would exist in the system at a given assumed flow rate. That's what they are doing. Now in the loss of coolant analysis we calculate the flow rate, knowing the driving head and downcomer and this resistance in the system. The approach is the same. I am pointing out that there are two things different between the Idaho calculation and the reactor calculation. The Idaho calculation is an assumed mass flow in just calculates the pressure drop and it also does not assume apparently from the pressure drops indicated a locked rotary resistance for the reactor coolant pumps.

CHAIRMAN JENSCH: Are there any other changes in the equation that you calculated or multiplied?

end

MR. MOORE: Basically no, no.

MR. FORD: The Idaho report indicates that in computing the radial pressure differences, they use the PWI FLECHT test data. I'd like to refer to page 7 of the Idaho report. They state, and I quote:

"Radial pressure differences will be established by axial and radial flow resistances, radial differences and steam generation and superheating, and the relative volumes of hot and cool assemblies."

In the code which you used --

MR. MOORE: Excuse me. That was on page 7?

MR. FORD: That's right.

MR. MOORE: Whereabouts?

MR. FORD: It is the very first line, first sentence on page 7, of Idaho Nuclear.

MR. MOORE: Thank you.

MR. FORD: Am I correct recalling on previous discussions of core pressure, that the Westinghouse computer codes assume that the pressure in the channel is the same over the entire length of the channel?

MR. MOORE: Which codes are you referring to now?

MR. FORD: The codes that you used to compute core heat-up. Is this LOCTA now?

MR. TROSTEN: I want to ask a question in order to determine whether or not I should object to what Mr. Ford has asked.

Are you asking the witness a question as to what the Westinghouse computer codes do? Is that the thrust of your question? It appears that way to me, but I am not sure.

MR. FORD: I would like to compare what the Westinghouse codes do in terms of computing pressure with the discussion of radial pressure differences and their significance in the Idaho Nuclear documentary?

MR. TROSTEN: My point is, I don't think a comparison for the sake of comparison is relevant. It appears to me, from what I now understand to be the question, that he seems to be asking the witness a question as to what the Westinghouse computer codes do, and I accordingly record my objection.

CHAIRMAN JENSCH: Proceed.

MR. FORD: The Westinghouse code, the codes that compute the pressure in channels, am I correct that the computer pressure which is assumed to exist in the entire axial length of the channel?

MR. MOORE: That's correct, for the heat transfer analysis.

MR. FORD: Is it correct that steam generation ratios depend on the temperature of the cladding that is in contact with the water, or the entrained water?

MR. MOORE: Yes.

MR. FORD: So that is it correct that given the axial differences in temperature, there would be differences in axial steam generation rates?

MR. MOORE: I think the reference that you read -- let me explain.

MR. FORD: Could I have that question answered first? I will refer to the references at a later point.

MR. MOORE: I cannot answer the question with a simple yes or no.

CHAIRMAN JENSCH: Let me see if I understand the question correctly. If there are differences, actual differences in temperature, will there be differences in actual steam generation rate? You don't know if you can answer that question yes or no; is that correct?

MR. MOORE: If there are differences in axial temperatures, there are differences in steam generation rates?

CHAIRMAN JENSCH: Yes.

MR. MOORE: Yes.

CHAIRMAN JENSCH: Thank you. Proceed.

MR. FORD: The difference in steam generation rates, will this account for some differences, axial differences in pressure?

MR. MOORE: Very slightly.

MR. FORD: Are these axial differences in pressure related to the flow of steam from the bottom of the core to the top?

MR. MOORE: It depends on where you are in the core.

MR. FORD: In your analysis of the transfer mechanisms, you assume that as the velocity of the steam increases, the amount of entrained water going up through the channel increases so that the velocity of the steam is an important factor, is this not correct, in heat transfer during the accident?

MR. MOORE: Yes, in the effect that it entrains water. The effect of heat transfer is mainly through the water.

MR. FORD: Inasmuch as the different steam generation rates between the lower axial level and the higher axial level, and the subsequent differential pressures in the lower axial levels and the higher axial levels are responsible for this important phenomenon, meaning the velocity of the internal cooling, am I therefore correct in assuming that there is a nontrivial -- in terms of the total analysis. -- a nontrivial pressure gradient between the lower axial levels and the higher axial levels?

MR. MOORE: In the axial direction?

MR. FORD: Yes.

MR. MOORE: It depends on what you consider nontrivial. It is about a one p.s.i. difference in pressure.

MR. FORD: In the FLECHT test data, did you evolve axial pressure drop estimate?

MR. MOORE: I believe there were measurements taken in the axial direction, yes.

MR. FORD: Can you tell me whether there is sufficient data, whether this was taken with sufficient regularity and sufficiently precise techniques to provide data that will simply settle the question of this pressure drop, or are there just a few measurements?

MR. MOORE: What is the question with respect to pressure drop?

MR. FORD: Whether or not there is a significant, in some sense, axial pressure drop.

MR. MOORE: I don't understand. I said the pressure drop was about 1 psi. Is that to be considered significant or not?

MR. FORD: That was, as I understand it -- that figure was supposed to have some relationship to FLECHT data. What I am really trying to find out is whether it is in fact 1 psi, whether -- I don't want to say anything prejudicial. Is that just a guess that you haven't had an opportunity to substantiate yet?

MR. MOORE: It is certainly not a guess. It is a calculated pressure drop given the mass flow through the core.

MR. FORD: I am talking in terms of your memory, if you remember the right number.

MR. TROSTEN: Mr. Ford, are you suggesting that the witness has not remembered the right number, and if so, is there another number that he should remember?

MR. FORD: I am suggesting that the witness estimated a one per cent in a sort of offhand way. I'd like to get it straight, whether it is one per cent or not.

CHAIRMAN JENSCH: I think it would be helpful if you do not have an objection and to rely upon the witness handling the situation. If you have any other suggestions, we can take it up on your redirect.

MR. MOORE: I did not say one per cent. I said one p.s.i.

MR. FORD: That's right.

MR. MOORE: There are axial pressure drop measurements shown in the FLECHT report on 3-112, which indicated a range of one to two p.s.i.

MR. FORD: There were two figures on page 3-112. The lower figure shows axial pressure that drops at one to two p.s.i. The upper figure, as I look, it goes from two to between three and four p.s.i.; is that correct?

MR. MOORE: Yes.

MR. FORD: Right.

MR. MOORE: Yes.

.Wm2

MR. FORD: These are only two indications. I am wondering whether this trend is continued in further charts to get us to five or six p.s.i. My point was in asking whether or not one p.s.i. was an offhand number or whether it was actually founded. It was simply whether the entire question had been, you know, studied well enough so that we could, instead of going now from one to two p.s.i., and now to three or four, whether we can find out where it all ends.

My question is, is there a final word on the subject of axial pressure drops, or do we just have three not terribly consistent estimates.

MR. MOORE: Mr. Ford, I don't think you understand at all the reflooding transient. The axial pressure drop, as I indicated earlier in the testimony, is primarily one of elevation. The height of water in the core, the pressure drop due to friction, is very small. The reason the pressure drop as shown in the curves is high for the 5.9 inches per second flooding rate is that we are filling up the core. As we fill up the core, the pressure drop along the core increases because we got more water there.

MR. FORD: I understand the data that you indicate that is given on the chart. My question is whether in a way -- whether the data on axial pressure drops is put together someplace so that we can settle the question rather than having to rely on these various not very well consistent estimates. Is this all the data that exists on axial pressure drops?

MR. MOORE: What do you mean by data?

MR. FORD: The data applied to the heater represents a few FLECHT runs, six to be exact. Whereas, as I understand the 73 FLECHT runs were had. I am asking whether or not, are these the only FLECHT runs to which axial pressure drop was measured?

MR. MOORE: No. I believe they were measured for more runs than that.

MR. FORD: Is this data available any place?

MR. MOORE: Yes.

MR. FORD: Do you know where?

MR. MOORE: Back in Pittsburgh, Pennsylvania.

MR. FORD: Thank you.

CHAIRMAN JENSCH: Why was it excluded here? Do you know?

MR. MOORE: The point of the report was to indicate that the axial pressure drop was primarily a function of the elevation head and the density of mixture above. It was not a function of friction and momentum effects. These were relatively small. So we applied some of the data to ascertain whether that in fact was the case. That did turn out to be the case, that you could look at the pressure drops as they were measured and relate them to the elevation head wm 4

and the density of the fluid and the much smaller effect due to friction and momentum.

CHAIRMAN JENSCH: And the selection was representative data?

MR. MOORE: Yes, at various flooding rates.

CHAIRMAN JENSCH: The other data won't mitigate or lessen the value of that showing there in any respect?

MR. MOORE: Not at all.

CHAIRMAN JENSCH: Thank you. Proceed.

While there is a pause, did you run these tests on the other 67 of your FLECHT experiments or tests?

MR. MOORE: As the measurements?

CHAIRMAN JENSCH: Yes.

MR. MOORE: Yes, I believe so.

MR. FORD: Mr. Moore, was any regression analysis, the data reduction technique, was it performed to relate the variable axial pressure drop to the factors that you think influence it?

end

MR. MOORE: No.

U1Bt1

MR. FORD: Thank you.

Have you performed any experiments in which you were studying a full simulation of the reactor core and loops and in which you directly measured the corewide pressure gradients?

MR. MOORE: Can I hear the question again, please?

MR. FORD: Would you repeat the question, please.

(The pending question is read by the reporter.)

MR. MOORE: No. I assumed we meant radial pressure readings.

MR. FORD: That's correct.

Now is it correct that in the hot central region of the core the steam generation rate will be many times the steam generation rate in the cool regions, cooler regions of the reactor core?

MR. MOORE: Under what conditions?

MR. FORD: Under loss of coolant accident conditions.

MR. MOORE: Yes.

MR. FORD: On Page 8 of the Idaho Nuclear report they provide a table called Steam Generation Rate Obtained From PWR FLECHT Data. Are the steam generation rates that are presented there, that are presented as a function of the temperature of the core region and of the temperature of the inlet water an accurate representation of steam generation rates that would be obtained from PWR FLECHT data?

U1Bt2

MR. MOORE: I really can't say just looking at the figures. They do not look unreasonable if that is the question.

MR. FORD: Yes

Is Idaho Nuclear correct in their assertion on Page 7 that similar steam generation rates in hot and cool regions of a reactor core would be observed during a loss of coolant accident similar to the steam generation rates presented in Figure 3?

MR. MOORE: Where are you reading that reference, please?

MR. FORD: This is again the first paragraph on Page 7. It says, and I quote, "Similar temperature--" referring to Figure 3--"Similar temperature differences and steam generation rate differences will exist in the reactor core."

Or perhaps for everyone's understanding I should read the previous part which both you and I can see. It says, and I quote, "Radial pressure differences will be established by axial and radial flow resistance, radial flow differences and steam generation and superheating, and the relative volumes of hot and cool assemblies. Calculations of steam generation rates for three PWR FLECHT tests are shown in Figure 3. Hot assemblies (1600 degrees Fahrenheit) attain high steam generation rates earlier than cooler assemblies (800 degrees Fahrenheit) after ten seconds. Test 2 was generating five

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if the premises that are discussed here and which you agreed to, what factors in your mind intervene between these premises and the conclusion that they have drawn, such that you don't think that you can draw the same conclusion?

MR. MOORE: Well, we have to look at the effects of what we are talking about now are the effects of adjacent assemblies where we have a hot assembly and a cooler assembly adjacent to it. As we flood into the core in the flooding region of the core where we just have water coming up into the core, the main pressure difference in that region is due to the height of the water. There is very little frictional pressure drop at all in that region of the core.

So we have here a thermal siphon effect where maybe a hotter region will generate more steam as indicated in the test. This steam will rise and entrain water and it will tend to avoid the hotter regions more than the colder regions.

Now in this case the frictional pressure drops are very small, relative to the elevation losses. So what we have here is a situation where I have a higher level of cold water in the adjacent assembly, which then drives this denser water into the hot assembly and tends to generate more steam.

So when one is looking at the effects of the inlet flow rising in the care we can get, we can sustain a higher steam generation rate and flow in the hot assembly, which is

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giving us effective heat transfer. That's one point.

The second point is the one I made earlier, that the pressure drop calculation and the system are calculated on the basis of the higher steam flow, higher steam generation and entrainment conditions that obtain for the hot assembly, assuming these are corewide. This has the effect of a larger pressure resistance in the loop and reduces the incoming flow and therefore tends to cause higher temperatures.

So just from the statements that are made in the paragraph on Page 7 one does not necessarily conclude that the temperature turnaround time of the hot region is delayed.

ULBt3

times more steam per second than Test 5. Similar temperature differences and steam generation rate differences will exist in a reactor core."

The question was is the assertion by Idaho Nuclear that similar steam generation rate differences between hot and cool regions will exist in a reactor core that is similar to those presented in this Figure 3?

MR. MOORE: I would agree, I guess, that similar steam generation differences would exist, since they are generated by the cold water heating hot regions at the bottom, yes.

MR. FORD: Now the conclusion of Idaho Nuclear from this is given in the statement for which you have concurrence or disagreement? I am asking.

It states, "Because of differences in the steam generation rates in the hot and cool regions, steam will rush into the cool regions, thereby delaying the temperature turn-around time of the hot region within a reactor core."

Do you concur or disagree with that statement?

MR. MOORE: That's not a clear conclusion to draw.

CHAIRMAN JENSCH: Do you agree with it or disagree with it?

MR. MOORE: I guess I disagree. It's not a clear conclusion.

MR. FORD: Can you tell me what factors would exist

MR. FORD: Now, the analysis that you are giving me, water level in the hot bundles versus water level in the colder adjacent bundles, can you tell me is this analysis based on experiments that Westinghouse has done with large numbers of bundles with different power levels and so forth, so that the difference in water levels you are talking about is something that you have actually measured?

MR. MOORE: No, that's an argument on the basis that cold water, denser water, tends to flow toward a region of less dense conditions.

MR. FORD: So that in terms of direct response to the question there is no experimental mock-up of the flow distribution conditions and water level differences that you can cite as support for your position?

MR. MOORE: On the contrary. The discussion here is with respect to what are the pressure drops that exist in the bundles and the core, and I indicated earlier that the main pressure drop is an elevation pressure drop, and I showed you FLECHT data that showed also that for different initial clad temperatures where we are getting higher steam rates, steaming rates as you indicated earlier, that the pressure drop was very similar, this is again for the same flooding rate, indicating that it is primarily an elevation effect and the frictional effects are small.

Given that situation I would hope we could agree that when we only have elevation effects that the colder water will tend to fill the channel which has been voided of water.

MR. FORD: As I understand your analysis you are talking about different levels of the flooding water in each channel, is that correct, in the hot channel versus the one next to it, is that correct?

MR. MOORE: That's correct.

MR. FORD: Now for the hot channel and the channels immediately adjacent to it, in inches what will be the difference between the water level in the central channel and the water levels on either side? We are referring to the hottest bundle in the core.

MR. MOORE: Because of the effect I just described I expect to see fractions of an inch difference between a hot and a cooler assembly.

MR. FORD: Yes. I know between a hot and a cooler assembly. I mean specifically focusing your attention on the hot assembly and the assembly immediately next to it, what will be the difference in water level in inches?

MR. MOORE: Fractions of an inch.

MR. FORD: What kind of fractions are you talking about? A hundredth of an inch, a thousandth of an inch? Half an inch?

MR. MOORE: Excuse me a moment.

About two-tenths of an inch.

MR. FORD: Now, what were the relative powers of the rods that you assumed in your calculation, relative power levels?

MR. MOORE: I assumed that the total frictional pressure drop that existed could be offset by a height of water -- this is the total frictional pressure drop associated with the hottest assembly, that that total frictional pressure drop could be offset by the difference in heat between the two, and adjacent assembly. That's at upperbound situation.

MR. FORD: Oh. So that the greatest difference that there could be in water levels is two-tenths of an inch, is that correct?

MR. MOORE: Yes. Again remembering now as indicated in previous testimony that I am talking about the case where I'm in the early stages of reflooding and I have a pressure drop of about one p.s.i. in the core.

MR. FORD: I see. So that the greatest difference under those functions which you want to make is two-tenths of an inch.

Now, in terms of the phenomenon that we are discussing, referring to this chart which indicates the temperature gradient across the core, it's correct that the absolute middle of the core, it's the radial and axial hot spot, is that correct?

MR. MOORE: Under which calculation are we talking about now?

MR. FORD: In terms of the radial and axial temperature differences in the core during the accident situation, that the six-foot elevation of the hot bundles in the middle, that is the core hot spot, the hottest region in the core for which you compute 2300 degrees Fahrenheit and so forth.

MR. MOORE: Yes.

MR. FORD: Now, the phenomenon that Idaho Nuclear is talking about relates to steam generation within the locus of this hot spot as a macro object. It's some number of bundles, you know, and it's in a radius that you measure in feet. Now they are talking about the steam generation versus the steam generation in the interenrichment zone in the core. Is that a correct analysis of the way in which the Idaho analysis of steam expansion relates to the whole core? Is that situating it properly?

MR. MOORE: I believe so, yes.

MR. FORD: Now, can you tell me is their analysis correct in the sense that the steam generation rates around this hot area, using the FLECHT data, will be greater than the steam generation rates at the same axial levels in the interzone, interenrichment zone area? Is that correct?

MR. MOORE: Yes.

MR. FORD: So that the question now is if a unit of steam is formed, let us say, at the four-foot elevation, or if a unit of two-phase fluid is present there, is it correct that in terms of the velocity, pushing it up, the axial velocity, that that fluid -- let me talk about my quantity of fluid here -- that that fluid is going through and as it's going in an upward direction immediately above it is an area where the steam generation rate may be five or six times, according to the FLECHT data, what the steam generation is a few feet away, is that correct?

MR. MOORE: No. I think those numbers are certainly excessive.

MR. FORD: Which numbers, the factor of five or six between steam expansion here and steam expansion there?

MR. MOORE: Yes.

end

MR. FORD: Well, now I am referring here to the Figure 3 which we previously discussed which is steam generation rate given as a function of the temperature of the core region, and it was earlier indicated here that the Idaho statement on page 7 that steam generation was five times more in the hotter assembly than it was in the cooler assembly, is that correct?

MR. MOORE: Yes.

MR. FORD: Now, in terms of the pressures involved with the greater steam generation and the greater heat in the hot spot is it clear to you that the bundle of two-phase coolant has an option, its existential option, between passing through the area of higher pressure and higher steam generation or going around its perimeter to an area of lower pressure and lower steam generation?

MR. MOORE: Can I correct an assumption you seem to have made here, that the regions you depicted there have a difference in steam generation of five to six times?

MR. FORD: Yes. Well, what is the factor?

MR. MOORE: Well, I am pointing out that the cladding, which is 800 degrees, is cladding out at the periphery of the core, not in the region anywhere near the hot spot.

MR. FORD: Now, in terms of the power factor here, the mean power somewhere across here, the mean is a 100 and you indicated that the peak goes up to a 140, is that correct?

MR. MOORE: That's the peak hot channel, not hot assembly.

MR. FORD: Right. Now, in terms of the trough here, what does the 100 go down to? What is the temperature gradient in this area?

MR. MOORE: Well --

MR. FORD: I mean the mean temperature is a 100. If temperature for the hot channel is 140 what is temperature for the cool channel?

MR. MOORE: The cool channel is not where you have the trough. The cool channel is on the edges of the core.

MR. FORD: Now isn't this in terms of our original agreement on this as the temperature gradient across the core?

MR. MOORE: That was not an original agreement. I said that it was typical or representative that we could have varying distributions.

MR. FORD: I see. Now, can you tell me in more specific terms what is the lowest bundle temperature in the interenrichment zone region where I put a trough where something else is more accurate? What is the lowest temperature in the interenrichment zone region?

MR. MOORE: I don't have that number right now. I could get you that number.

MR. FORD: Well, in terms of your understanding, it's less than the mean, I presume.

MR. MOORE: Certainly.

MR. FORD: So that let's simply call it less than a hundred.

MR. MOORE: My point is it was not five or six times less.

MR. FORD: Oh, not the temperature. Oh, I see what your point is.

Now temperature difference between the hot assembly and the cool assembly in the Idaho statement on page 7, that is a temperature difference of a factor of two between a 1600 degree region and an 800 degree region, is that correct? A 1600 to 1800 factor of two?

MR. MOORE: I agree with that.

MR. FORD: Thank goodness.

MR. MOORE: Thank you.

These are all apparently tests done at a constant power level as indicated in the curve. The description we are showing now in the core is power level, not initial temperature.

MR. FORD: I am referring to Figure 3.

MR. MOORE: So am I.

MR. FORD: Now let me get clear here. The Idaho people say that, and we agreed it was the same for the real core and so forth before, that in this particular region where there is a difference of a factor of two in temperature, there is a difference of a factor of five in steam generation. Now is that correct?

MR. MOORE: That's correct for apparently the series of tests they have here, which are a constant power rod, but they have allowed it to heat up to different temperatures. The case you are talking about is a different situation with a different power rod at different temperatures.

MR. FORD: Well, let's just assume for the moment that simply the difference in steam generation is proportional to the difference in temperature. Forget this possibly much greater steam generation.

MR. MOORE: Let's clarify it some more and say it's proportional to the difference in power generation.

MR. FORD: O.K. So that given the difference in power generation, the clear difference responsible for some clear gradient here, given that power difference is it clear that at the periphery of the intemenrichment zone and its interface with the periphery of the outer enrichment zone that steam generation rates at the same axial level will be substantially less than they are in the hot area, the hottest area on that axial level?

MR. MOORE: Yes. Steam generation rates are less in the lower power area as compared to the hotter power areas.

MR. FORD: Now given the temperature gradient here is it also correct that we are talking about a pressure gradient between the hottest area on an axial plane and the area, the interzone, interfuel enrichment zone area?

MR. MOORE: That'is a function of the resistance to flow of the steam from one assembly to the next and it's a function of the total pressure drop in the given assembly.

CHAIRMAN JENSCH: Could I have that question read again, please?

(The pending question is read by the reporter.)

CHAIRMAN JENSCH: Thank you. Will you proceed.

MR. FORD: Now, assuming that there is a pressure gradient, a radial pressure gradient, under those circumstances would this quantity of two-phase fluid that is moving in the hot channel up to the midpoint, is there the tendency then, assuming this pressure gradient for this quantity of fluid, to go out the path of least resistance as instead of going through the area of greater pressure to go through the area of lesser pressure?

MR. MOORE: Yes.

MR. FORD: Mr. Chairman, I'd like to put another diagram, somewhat more intricate than the ones we have had, and I am wondering if this would be a good time for a break.

CHAIRMAN JENSCH: Very well. At this time let us recess to reconvene in this room at 3:35.

(Brief recess.)

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CHAIRMAN JENSCH: Please come to order.

We will note for the record that Dr. Geyer had committed himself and won't be here the balance of the week.

Will you proceed with your interrogation.

MR. FORD: The document that I'd like to refer you to to ask a question pursuant to the line of questioning that we have been concerned with on flow maldistribution during the loss of coolant accident is here. The quotation which I'd like to refer to is from the document entitled, "Water Cooled Reactor Safety - An Assessment Prepared for the Committee on the Reactor Safety Technology of the European Nuclear Energy Agency," published by the Organization for Economic Cooperation and Development, OECD, in May 1970.

On Page 38 of the document, which I will show the witness in a moment, it says, and I quote: "No experimental measurements or flow distribution through a reactor core have been reported for flooding systems."

This is, of course, concerned with the loss of coolant accident situation.

My question is, do you agree with this statement that no experimental measurement of flow distribution to a reactor core have been reported for flooding systems?

MR. MOORE: Yes, I know of no experimental measurements.

MR. FORD: I'd like to discuss another phenomenon

V1Wt2

associated with flow maldistribution. This phenomenon relates to not flow maldistribution within the core, but to flow maldistribution within the entire system. One problem is, of course, is that coolant will be maldistributed within the core.

The second problem is that the coolant will never reach the core because of maldistribution in the system.

I'd like to ask Mr. Moore whether this latter form of flow maldistribution is properly defined in the following statement of Idaho Nuclear. It says, and I quote: "Steam binding refers to a situation in which steam generated during a loss of coolant accident may create a pressure build-up in the reactor vessel outlet or upper plenum in the large PWR of sufficient magnitude to preclude or retard the injection of emergency cooling water into the core region."

That is a quoatation from the Idaho Nuclear document, IN-1383, entitled, "Technical Assistance in Reactor Safety Analysis".

The question was whether their definition of steam binding is one which can form the basis of a discussion.

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

MR. FORD: Sure.

MR. MOORE: At least you get some exercise this way.

CHAIRMAN JENSCH: I know you wouldn't want to say that you seemed to get more out of him than he does out of you.

V1Wt3

MR. MOORE: Not at all.

Yes, I agree to the definition of a possible steam binding situation.

MR. FORD: The document further lists four possible energy sources that could be attributed to the steam binding effect. I am wondering if you can review that part of the document immediately following the quotation we just referred to, and indicate whether you agree with the analysis of these four factors as related to steam binding, or potential steam binding.

MR. MOORE: Yes. I have read the four factors. What did you desire?

MR. FORD: Whether you find them to be indeed four factors that could potentially contribute to steam binding.

MR. MOORE: Yes. It characterizes potential sources of steam generation.

MR. FORD: Are there any additional sources of steam generation that are not characterized there?

MR. MOORE: None that I can think of.

MR. FORD: For the record and for everyone to understand, could you just list the four factors if they are listed there?

MR. MOORE: Yes. Core decay heating, stored energy contained in the core, energy contained in the reactor vessel and primary system components, which are in contact with liquid water, leakage or rupture of steam generator tubes.

MR. FORD: I have tried to put a diagram up here which willhelp s discuss the steam binding phenomena. In terms of the diagram, I'd like now to incorporate the parameters that we find relevant or we can agree that are relevant. I think the first parameter that we would include here would be the pressure in containment; is that correct?

MR. MOORE: Yes.

MR. FORD: The second factor that we would include here would be measures for the pressure in the upper plenum pressure, in the core itself; is that correct?

MR. MOORE: Yes.

MR. FORD: The further pressure drop that we have to consider is the function of this downcomer distance; is that correct?

MR. MOORE: Yes.

MR. FORD: If we discussed the addition or injection of emergency core coolants through the accumulator, when the flooding level begins to rise, is it correct that what will determine the direction of the coolant into the cooler or around the cooler must be computed in terms of the pressures in the cooler and the upper plenum versus the combination of pressure in the containment and the downcomer factor here; is that correct?

MR. MOORE: Yes.

MR. FORD: Have any experiments been done which simulated a primary loop such as this, injected emergency core coolant and evolved results or whether the coolant in fact went into the core or whether the coolant was also projected from the system through the cold leg break?

MR. MOORE: Could you help me by defining the mechanism for the injection? I don't understand.

MR. FORD: The mechanism being that the pressure in the core is much greater than the pressure in the containment plus the factor for the height of the downcomer.

MR. MOORE: Excuse me. Are we in the reflood phase of the transient now, after the completion of blowdown?

MR. FORD: Yes.

MR. MOORE: Before I answer your question directly, there is really no source of pressure in the core because I have an empty core. In answer to your question, I know of no specific experiments which properly simulated the loop resistances and the driving force in a reactor configuration.

MR. FORD: The Idaho semi-scale test 845 through 851, in that it is correct that that simulation involved a whole primary loop; is that correct?

MR. MOORE: One single loop, yes.

MR. FORD: A single loop?

MR. MOORE: Yes.

MR. FORD: Let us assume for the moment that the pressure drop in the loop, in the Idaho semi-scale model, and are realistic. Is it correct that during the blowdown injection of the accumulator water, that the apparent occurrence in the Idaho semi-scale test was of a steam pressure head inside the core, such that the emergency coolant bypassed the core entirely and was ejected through the cold leg break?

MR. MOORE: I don't know that I need to assume anything about pressure drops. The results of the experiment showed that the water did not go through the core, bypass the core.

MR. FORD: Is it your understanding of the Idaho semi-scale test results, that the apparent cause of the bypass of the core was steam slug, steam pressure inside the simulated core?

MR. MOORE: There were two effects of which that was one.

MR. FORD: Can you note the other effect?

MR. MOORE: The fact that the accumulator water was injected into the bottom of a very small vessel which had very close proximity to the break, the broken pipe. So that the preferential path in the containment was not simulated. So that there continued to be blowdown through the entire course of accumulator injection. With the geometry effects of the very small plenum and vessel, there was a sweeping action to sweep this water out through the break, also.

MR. FORD: Is it correct that in the Idaho semi-scale test, that in different tests they tried to alter geometry to see whether, for example, if they injected emergency coolant directly into the core rather than into the anulus between the flow skirt and the vessel wall, whether that geometrical change would make a difference?

MR. MOORE: That's not quite correct. They never injected water directly in the core. They did look at different injection points in the outside and through the skirt. But they were constrained by a very small geometry situation. So they weren't really effectively changing the geometry with the changes they could make.

MR. FORD: Can you tell me, for a large pressurized water reactor, have any experiments been done on a large scale, not necessarily full scale, but a scale such that the purported scale and problems with the Idaho nine-inch core wouldn't be operative?

MR. MOORE: No, not the vessel effect.

end

MR. FORD: So that in terms of the way in which we can approach the problem of steam binding, the only approach that we have available at the moment is an analytical one? We have to see what our models will tell us.

MR. MOORE: Excuse me. The vessel effect we were talking about was not a steam binding effect. It was a bypass effect during the accumulator injection.

MR. FORD: Yes, that's correct. My question is whether the bypass, either due to the situation of blowdown or due to post-blowdown pressures in the core -- I am trying to analyze the two situations together. My question is in terms of our analysis of those two related problems, do we have any experiments on this matter with a reasonably scaled primary loop, or do we have to rely on our analytical models to tell us whether bypass or steam binding will recur in an actual loss of coolant accident.

WBt1

MR. MOORE: Well, the effects of bypass due to geometry of the vessel, the assumption is made in the analysis in a way that no test information is required. That is, we bypass all of the water during blowdown. So I would not characterize that as a requiring a dependency experimentally or analytically.

MR. FORD: Right. I see the conservative assumption you have made with regard to one of the mechanisms by which the coolant would be ejected, and clearly there is nothing that you can do more conservative than assuming that it all goes out.

Now what I am concerned with is the possibility of steam binding subsequent to that point at which you have made the conservative estimate of the loss of portions of the accumulator water.

Now for this post-blowdown steam binding effect do we have any correctly-scaled experiments with a primary loop that directly and generally answers the question as to whether or not steam binding effect post-blowdown would cause ejection or retardation of the coolant, of the emergency coolant?

MR. MOORE: Insofar as the analyses are involved with a pressure drop analysis through the system, the resistance characteristics of piping, of a steam generator, and a pump, for example, are known, both under single phase and two-phase conditions. So these are really--these have been experimentally determined by various experimenters in the general literature

WBt2

on pressure drop. So we are applying these generally well-known relationships, together into a combined analytical model.

MR. FORD: Right.

MR. MOORE: And continuing, there is no specific overall test that I'm aware of that ties all of these together in the geometry of a reactor.

MR. FORD: Right. So that simply put, your answer to my question about experimental data is that we don't have experimental data from a full simulation of the whole phenomenon on a large scale, but what we do is to use experimental data we have on various factors of the phenomena and combine this in an analytical model and determine from that whether steam binding post-blowdown would take place.

MR. MOORE: That's correct.

MR. FORD: Now the Battelle Institute has analyzed this problem in their model. It's reported in their document BMI-1-8-71, by W. A. Carbiner (phonetic) and R. L. Ritzman (phonetic) entitled An Evaluation of the Applicability of Existing Data to the Analytical Description of a Nuclear Reactor Accident and further the Idaho Nuclear Corporation in their document IN-1383, which we referred to earlier, they have indicated that they are under contract with the Atomic Energy Commission making a model with their RELAP 3 code of whether or not steam binding will pose problems in an emergency core cooling situation. In terms of work being done these are

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the two model-building efforts that I know of, specifically investigating the question of steam binding.

Can you tell me whether any other analytical work has been done in this area with which you are familiar?

MR. MOORE: I would refer to our own analyses of the situation that obtains in a reactor.

MR. FORD: Can you give me a reference to the document in which you direct yourself or the section of document, of course, in which you address yourself specifically to the phenomenon of steam binding and provide with your model a direct and a general answer to the questions?

MR. MOORE: Yes. On Page 13 of the July 13th testimony we summarize the key assumptions in the analysis of the steam binding or reflood phase of the accident.

MR. FORD: Now in your analysis did you relate your model-building analysis of steam binding to a Battelle effort or to the Idaho Nuclear effort?

MR. MOORE: Not directly, to my knowledge.

MR. FORD: Now as reported in IN-1387 the Battelle model indicated that in a loss of coolant accident the steam binding effect was present to cause rejection or retardation of the coolant. Now are you familiar with this Battelle work?

MR. MOORE: Could you indicate the reference in this report? I have a copy of that 1387.

MR. FORD: Right. The Idaho Nuclear people present

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Battelle's computations in the form of a figure. This is Figure 11 on Page 17. They plot the different parameters here and indicate there at this point in the accident forty seconds after the break that we have we still don't have the core reflood progressing unhindered. Instead we have, in this prediction, steam binding that's causing ejection of the emergency coolant.

Is that a correct reading of Figure 11? It's described on Page 16.

MR. MOORE: Excuse me a minute.

MR. FORD: Surely.

MR. MOORE: I believe that's a correct description of what they did.

MR. FORD: So that in terms of the relationship of your model to their model, is it clear to you that there is a discrepancy or a difference, that they seem to think steam binding is occurring forty seconds after the accident, whereas I presume that you don't think steam binding is occurring then, if ever.

MR. MOORE: Well, I think there is a misnomer. The analysis they have shown here is not steam binding in the sense that you would interpret no flow.

MR. FORD: What?

MR. MOORE: No flow. In other words, there is flow in their analysis going out through the break. There is flow through the core. It's been reduced by the steam generation within the core during reflood.

MR. FORD: Yes. Do I correctly interpret their arrows here indicating the direction of a flow as indicating that there is one whole channel area in the cooler area that seems to be filled with water, almost up to the top, and that the other channels have a little water up to perhaps a foot in the bottom, but their arrow seems to indicate the channel that's full is becoming unfull, that the flow is going out of the channel?

MR. MOORE: I really don't know what that particular representation is. The arrow is in a downward direction. I don't know where that is described in the report. The characteristic of a differential in level between the water level rising in the core and the level on the downcomer is one to be expected under this condition.

MR. FORD: Could you repeat that, please.

MR. MOORE: Characterization of a small rising level in the core with a large level in the downcomer is a characteristic of this stage of the transient.

MR. FORD: Yes.

MR. MOORE: In our calculations as well.

MR. FORD: Now, is it a further characteristic of your calculations that actually some of the emergency coolant is flowing out the break?

MR. MOORE: No. That gets a little more complicated. With the assumptions we have used for the interim criteria in the AEC, we have thrown so much water away to begin with during the blowdown phase that we effectively just about fill the downcomer and we have no water spilling over in this part of the transient.

MR. FORD: Yes. I realize you treat water as a free commodity here.

MR. MOORE: Very expensive commodity.

MR. FORD: Yes. What I am asking you is whether Battelle's calculation seems to indicate that after the point at which you have stopped throwing water out of the system they are still predicting that water is being ejected?

MR. TROSTEN: Mr. Chairman, I object to that question because I think we are back at the point now where Mr. Ford and Mr. Moore are discussing and trying to determine what it is that some report says. The same problem we had yesterday, sir, and I object to the question.

CHAIRMAN JENSCH: Well, I think it's a little different. They are trying to interpret something that an author may have meant by what he said. But I think as a statistical analysis if the witness can apply the same technique that he has done for his own analysis to this, to give us an interpretation of another presentation, I think it would be helpful, just as the technology from one reactor or the technology from some other operation of pressurized water reactors are informative and helpful.

Objection is overruled.

MR. MOORE: Can you restate the question or reread it?

MR. FORD: I can restate it easily enough.

All that I am really trying to ascertain is whether or not there is a difference of prediction between your model and Battelle's, and my specific question here is simply whether or not in the Battelle model as reported by Idaho Nuclear here, whether or not Battelle regards emergency coolant as being ejected or still being ejected from the primary system at a much later time, when you have stopped, in your model, assuming that you are throwing emergency coolant away.

MR. MOORE: Yes. That's apparently the case. But again this is done obviously without the interim criteria, since the report is dated 1970.

MR. FORD: Yes. So if --

MR. MOORE: It's a different situation.

MR. FORD: Right. So that if they were doing their calculation with the interim criteria they would not only be throwing away forty seconds after the break, they would already previously have a record of having thrown the accumulator water, a good fraction of it, away.

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MR. MOORE: No. I disagree. I think you cannot compare this calculation performed without throwing any water away initially with one where the water is thrown away initially. We don't have any data here to ascertain the effect that would have.

end

MR. MOORE: I see. Now, do you have any data on the sensitivity of steam binding to whatever assumption you made about accumulator water, as the interim model has an assumption with respect to accumulator water that affects steam binding, and that is that during the period of time that accumulator water is being ejected the possible venting pads are plugged. I guess I really don't understand any other significance to your question.

MR. FORD: Now, if I made a chart here that on the X axis has the per cent of the accumulator water that is assumed to be ejected from the system during the blowdown and on the other axis I have the probability of steam binding post blowdown, what I am asking for in terms of asking the relationship between your assumption about accumulator water and your assumption about probability of steam binding, I am asking for a curve here. I mean what is the general nature of the relationships?

MR. MOORE: I don't have any specific relationships derived for that. If I don't have any -- I guess I have got one data point. If I don't have any accumulator water, I have no steam binding, I have a source of steam. You will grant me that.

MR. FORD: Yes.

MR. MOORE: I don't have any specific relationship. It's steam pressure drop compared to the driving head.

MR. FORD: I see. Well, the reason I asked the question was to help us in our -- help you in the judgment that you make about the Battelle study.

You indicated that they don't make the interim criteria's assumption about accumulator water. Now I am asking for this relationship so that knowing the relationship we can then say, "If they did make the interim criteria's assumption about accumulator water then their prediction of steam binding and post blowdown would be different in this specific way from what it is."

MR. MOORE: I am sorry. I really can't comment on that because I have no knowledge for all the assumptions that went into the Battelle calculation for steam binding. That will be a requirement in order to come to such a conclusion. I have no knowledge of what assumptions they used for this particular analysis.

MR. FORD: Right. Then I appreciate it. What I am trying to do is to see whether in terms of your own model, of which you have more knowledge than anyone, of what assumptions went into it. I wanted to ask you in terms of your own model what sensitivity is there between the assumption you make about the per cent of accumulator water loss during blowdown and the probability of steam binding as you would put it with your model? Do you have knowledge on your own home ground there of this sensitivity?

MR. MOORE: Yes. The flooding rate, the one that obtains early in the transient, is a function of the build-up of the level in the downcomer. If I have a lower level in the downcomer at the end of accumulator injection my flooding rate is lower. That's taken into account in the analysis.

MR. FORD: I see. Now, in the analysis do you take account of the various factors that could influence steam binding? I mean in addition to the pressure in the core do you separately compute the pressures in different areas of the core? Do you separately compute -- I mean in the one computation? Do you sort of put together in a simultaneous way all the different pressures in the core, different pressures in the upper plenum and different pressures in the containment and so forth so that you perform an analysis of the steam binding phenomenon that's related to a detailed thermodynamic analysis of the various nonequilibria in the primary system?

MR. MOORE: Well, in answer to that very long question, yes, we do account for the effects which can increase or affect the pressure drop in the system because that, of course, is what determines our ability to flood the core. And as indicated on the previous reference, page 13 of our July 13th testimony, we indicate some six assumptions there that are specifically derived in order to obtain conservative evaluations of the pressure drops that exist in the system.

Xwt1

MR. FORD: What concerns me is the level of detail with which you represent the factors influencing pressure differences within the different parts of the primary system.

It was my understanding that when you computed core pressure, the output of your codes in that area, simply one average core pressure; that the localized core pressures that may be influential here, that you don't compute this in a once-through manner. Is my understanding of that correct?

MR. MOORE: Let me clarify. With respect to the steam binding, I indicated earlier we assume that the mass flowing from the exit of the core is equivalent to the mass discharge we would get assuming the whole core was made up of hot assemblies. We used the FLECHT data which, for hot assemblies, gives us a higher entrainment factor and gives us the greatest mass discharge from the core. So we start with an assumption on flooding rate. We are reiterating here. We have a flooding rate in the core using FLECHT data to determine the discharge mass out of the core without any detail simulation for the core. We are using the FLECHT data directly. Now the analysis traces that particular mass around the system and determines the pressure drop through the system for a given mass flow.

MR. FORD: So that your clarification, I take it, my understanding, as I expressed it, that you simply compute one core pressure that is assumed to be the average of the entire core. You compute one core pressure which you use as an average?

MR. MOORE: We don't need core pressure. I computed the discharge mass as a function of flooding rate. I don't need pressure per se in the core.

MR. FORD: But for the detailed analysis of the steam binding, you do need pressure; is that correct?

MR. MOORE: Yes. I'm sorry. Yes, you do. We have a corewise pressure entracing the pressure drop through the system, that's right.

MR. FORD: Can you tell me whether the pressurized water reactor has any steam release system such that if you wanted to mitigate potential steam binding, you could relieve the pressure in the upper plenum?

MR. MOORE: You are speaking of Westinghouse pressurized water reactor?

MR. FORD: Yes, something comparable to the pressure release system of a boiling water reactor.

MR. MOORE: Yes.

MR. FORD: In terms of the other possibilities of preventing or reducing the effects of potential steam binding, do you have any of these other systems that you can rely on, as indicated by Idaho Nuclear? I will show you the list. This is IN-1383, Page 26. This is a description of the purpose of their analysis. The third purpose is: "To examine various possibilities for preventing or reducing the effects of

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potential steam binding, such as secondary steam blowdown, core barrel, check valves or reactor vessel spray or deluge systems."

Do you have any of these systems that could be of use in reducing the potential for steam binding?

MR. MOORE: No.

MR. FORD: One of the potential sources of steam generation that could contribute to steam binding, as we list it earlier, was, as quoted here, the leakage or rupture of the steam generating or tubes.

MR. MOORE: Yes.

MR. FORD: Is it correct that that is a potential source of steam binding, that if this leakage or rupture took place, it could possibly aggravate a steam binding situation?

MR. MOORE: Yes.

MR. FORD: Can you tell me in your own analysis of steam binding, what assumptions you made about the contribution to the problem from these potential sources?

MR. MOORE: There is none considered in our analysis.

MR. FORD: Of the three other potential sources of steam generation which can contribute to steam binding, could you indicate whether your analysis considered all of them, which might have been excluded?

MR. MOORE: The other three have been considered directly. I have one problem with your nomenclature. Item 4

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was also considered but dismissed.

MR. FORD: I appreciate that. Can you tell me, in terms of your analysis of the probability of steam binding, what kind of margins are involved in terms of the core pressure which you actually compute, you contend that that core pressure is not great enough to cause steam binding. If that core pressure were increased by ten per cent, would steam binding occur?

MR. MOORE: No.

MR. FORD: If it were increased by fifty per cent would steam binding occur?

MR. MOORE: No.

MR. FORD: Can you tell me whether you performed the calculation to determine exactly what the threshold core pressure would be above which steam binding is likely and below which it is not?

MR. MOORE: No, not specifically. The major over 65 per cent of the total pressure drop for steam binding occurs in the reactor coolant pump. That's the basis for a statement that variations of the pressure drop and the core, which during this early stage of reflood, which is important with respect to temperature turnaround, these pressure drops are on the order of one psi. So variations in pressure drop in the core-- large variations are not significant because the major resistance is in the reactor coolant pump.

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MR. FORD: If we performed an analysis in which we were varying different parameter combinations, can you tell me, in terms of the pump resistance factor that you are talking about, the variation in that under given assumptions of that core pressure, or what variations in that would establish a threshold above which or below which you would have steam binding, and the other side of which we do not?

MR. MOORE: No. I have no specific numbers for that. As noted in our reports, the pressure drop in the pump is assuming arbitrarily that the pump rotor is locked to give us the highest pressure drop.

MR. FORD: Can you tell me, in your July 13 testimony, and can you identify the discussion that relates to the steam binding phenomenon?

MR. MOORE: Being on page 9, description of core reflooding model.

MR. FORD: At the defined end of blowdown, when there is zero break flow, what is the containment pressure calculated to be at that time, approximately?

MR. MOORE: I must confess, I still haven't looked up the design number for the containment pressure. It is 90 per cent of the rise in containment pressure.

MR. FORD: Is it approximately 47 psi?

MR. MOORE: I believe so, yes.

MR. FORD: In terms of the analysis which you perform on steam binding, am I correct in reading the July 13 testimony, that the containment pressure which you used in steam binding analysis, that you simply initially fix this at whatever you calculated plus 90 per cent?

MR. MOORE: No, not for the reflood part of it. You asked for the container pressure at the end of blowdown. There is a further reduction in pressure assumed in calculating the reflood part of the transient.

MR. FORD: Can you tell me, do you simply take the curve of containment pressure as a function of time during the reflood, and you simply multiply that curve by 1.9 in order to calculate the containment pressure that you assumed during your steam binding computation?

MR. MOORE: I wouldn't multiply by 1.9.

MR. FORD: You increase it by 90 per cent?

MR. MOORE: As indicated on page 13, the containment back pressure for reflood -- which you started asking about. -- is equal to the initial prebreak pressure plus 80 per cent of the calculated pressure increase for the accident.

MR. FORD: So that the assumptions that you make about containment pressure in the steam binding analysis, in that it is assumed that the containment pressure is independent of whatever else may be happening in the primary system related to steam binding. For example, that containment pressure isn't changed by any additional fluid that may be ejected during the reflood. From steam binding.

MR. MOORE: Yes. The value used for the analysis is purposely picked to be a low value for conservatism in the reflood calculation. The actual pressure transient is higher. The calculated containment pressure during this phase of the action and for a turnaround is actually higher. So we reduce this calculated pressure to apply conservatism.

MR. FORD: Let me understand this. Have you performed calculations of containment pressure at the blowdown in which you made that containment pressure after blowdown a function of, among other factors, the quantity of additionally ejected water?

MR. MOORE: Yes. The containment calculation includes these energy sources.

MR. FORD: In terms of the direction of the influence?

MR. MOORE: Yes.

MR. FORD: If the containment were to receive additional water at the temperatures of the emergency coolant water, if it were to receive that additional water at the reflood, would containment pressure increase or decrease?

MR. MOORE: Compared to what?

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MR. FORD: Compared to what it would be if it were not receiving this additional coolant.

MR. MOORE: Additional coolant to the containment should increase containment pressure.

MR. FORD: Can you tell me whether your calculation of the steam binding phenomenon varied the temperature of the emergency cooling water?

MR. MOORE: No.

MR. FORD: Can you tell me whether there is any sensitivity analysis that has been performed on the inlet temperature of the emergency cooling water relative to steam generation during reflood?

MR. MOORE: Not specifically, I guess.

MR. FORD: Is it correct, in general, the higher the inlet temperature, the greater the steam generation?

MR. MOORE: Yes.

MR. FORD: Is it possible, at the early stages of reflood, is it possible that the superheating phenomenon which occurs at later stages of reflood of high axial levels, that this phenomenon of superheating steam would occur at low axial levels during reflood, during the early part of reflood?

MR. MOORE: I'm not sure I understand the question. Is it possible that the superheating that can occur at the upper levels could also occur--

MR. FORD: Let me--

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MR. MOORE: Why don't you repeat it.

MR. FORD: That's not fair.

MR. MOORE: Try again.

MR. FORD: Let me try again.

We observed the presence of superheated steam for long periods of time in FLECHT tests at high axial levels for periods on the order of 100 or 120 seconds. What I'm asking you here is whether it is possible or if it would be possible for there to be superheated steam at low axial levels during an early part of the reflood?

MR. MOORE: It is possible.

MR. FORD: In tests that have been done--I should point out, with the water levels in the Battelle computations as presented in Figure 11 of 1387--in the tests that have been done with variable flooding rates, whether any attempt was made to vary the flood rate to such a low point that there was practically no increase in the water level, and was to hold the flooding rate or stop the flooding rate and keep a water level; to hold that water level constant while the core began to heat up, to determine whether the retardation in the flooding, whether that aggravated the formation of the superheated steam at low axial levels?

MR. TROSTEN: Mr. Chairman, I'd like to hear that question repeated. I can't tell whether Mr. Ford is asking about Westinghouse tests or Battelle tests.

CHAIRMAN JENSCH: Reread the question, please.

(The last question was read by the reporter.)

MR. FORD: I'd like to withdraw the question for substantially reworking it. Let me go to my diagram.

If the phenomenon of steam binding were to occur, would one aspect of the development of this phenomenon include the fact that you may have reflooding proceeding, but some of the water is being lost, and the water level here would be staying constant at a flooding rate of zero? In terms of the phenomena of steam binding, would that be an abstract of a situation in which steam binding actually occurred?

MR. MOORE: If you had actual steam binding, which we don't have, you could get into the situation that is obtained in the semi-scale tests, effectively as you describe, an inability to get a level into the core.

MR. FORD: One aspect of steam binding, as you point out in the semi-scale test, instead of having a rising water level, the water just flows around the whole core and out the break. That's one aspect.

As the Idaho Nuclear definition earlier pointed out, they were talking about steam binding as involving both retardation of flooding and the course of total ejection of coolants. What I am concerned with at the moment, with the question I am trying to rework, is focussing on the retardation aspects of steam binding. Is it clear to you the aspect

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of the steam binding phenomenon that I am talking about, the particular form of development that I would like to discuss?

MR. MOORE: Yes. Perhaps I could help. The flooding rate, as accumulator water comes in, for flooding in the bottom of the vessel at a fairly high rate, that at the time we hit the bottom of the core and get about twenty inches into the core, we are generating steam now in the core and you find that with time the level in the core rises quite slowly while the level in the downcomer is rising up about that value at the Indian Point Plant.

So that is a retardation in flooding rate associated with the resistance of the loop.

MR. FORD: I am talking, you know, about the situation in which the resistance of the loop is such that the water level is staying constant here; that additional water that is being added by the accumulator is spilling out the break. The water isn't all being ejected, yet it isn't all going in. We are in that margin of pressure that I was trying to determine earlier. We are in the margin that you have a stand-off at this phase of the steam binding problem.

What I am asking about is when you have the water level at the bottom of the core with a zero increase, there is going to be steam forming in here. Even though the water level isn't rising, is it correct that there is still some small amount of coolant going up the channels and creating

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more steam at these lower axial levels? Is that correct?

MR. MOORE: Yes, at the point where we heat the water.

MR. FORD: The question I was trying to ask is whether the fact that the water that is going up here, the velocity is clearly very small. That is what steam binding means. Is it clear that it is possible to have situations in which the steam formed here isn't going any place, but there is heat transfer to the rod so that it is increasing in t emperature?

MR. MOORE: Yes. But let me make you understand situation. The downcomer head is applying a pressure here. Now that's forcing -- there is very little pressure drop, as I indicated in the FLECHT results, a p.s.i. of one and a half p.s.i., in this range. I have got about seven p.s.i. acting through the height of this water in the downcomer. So what happens in the core is not too significant. We get a certain amount of water that comes out here. Questions with respect to superheat or changing the temperature of the water droplets as they are carried out of the core are not significant, in that the major pressure drop is over here and the steam generator is assumed to superheat that water anyway. Even if it comes out as droplets it is superheated in the steam generator as a conservative assumption.

MR. FORD: Right. But what I am concerned with here is that simply in terms of the fact that since the velocity has been, but no pressure drop has been involved here, since you are holding the steam in the same axial location, you are holding it for much longer periods of time, than the time that these temperatures is sufficient, as I believe you are agreeing, to get superheat formed at these low axial levels?

MR. MOORE: No. I doubt it. I can't definitely state what the specific case is. I said it was a possibility.

MR. FORD: Let's investigate the implication of the possibility. If the superheat were formed at this low axial level and then let's suppose in the dynamic situation that we are able to the water starts to rise and the axial pressure gradient changes so that there is much more velocity up the channel, do we get the situation as the possible development of the steam binding accident where the superheated steam formed at the low axial level where that gets up to the high axial levels, where instead of just the amount of superheat that we observe with no steam binding, you know, we get a much aggravated superheat steam situation at the higher axial levels. I mean what I am asking is this development an implication of steam binding -- of superheated steam formation in the steam binding problem?

MR. MOORE: Well, the results from the FLECHT test are used in this calculation and the FLECHT results were obtained for flooding rates less than those which are calculated to assist during steam binding. So that effect, whatever it is, has already been ascertained from the FLECHT tests.

MR. FORD: No, I understand the different substances that have been made, but my question was based on the premise that we developed a few minutes ago, namely, that we are talking about a constant water level, FLECHT. FLECHT had data for low flooding rates. Now I'm talking about the situation where the steam pressure is sufficient in the core to stand off the inflow of emergency coolant. Now, under that situation, which is quite different from the way the flooding rates were varied in FLECHT, in that situation and you get the kind of aggravation of superheated steam formation at higher axial levels because of a possible formation of it at lower axial levels? The premise of the question, the assumption that I ask you to make, is that we do indeed get the superheated lower level. I am trying to figure out what the implication of that is. I am not asking you to concede the point that we in fact do get it. I am simply saying that if we get it at the lower axial level will this aggravate the superheated steam problem at the higher axial level?

MR. MOORE: I guess I would say yes, in that that represents the zero flooding rate situation. That is true in a steam binding situation.

MR. FORD: Right. Now, in terms of the dynamic process in which steam binding phenomena may develop, now if we further suppose that -- all right. After a period in which we have had a zero flooding rate, but now we start to have an increase in the flooding rate, will the incremental superheat steam formed in the previous round, will that tend again to make conditions in the core such to stop the flooding rate again or would it be once the flooding rate started to increase after the initial, or after a temporary zero point, zero flooding rate point, that it would then simply proceed and cover the core, no problems?

MR. MOORE: You have me down a very long, conjectural path, and I am not sure I understand just where I am now with respect to the core. I am certainly nowhere near the real situation. I am sure you realize that. What is the point that you are driving toward with respect -- what is the concern that you have with respect to your conjectural situation?

MR. FORD: Well, I am trying to ascertain whether the understanding that you have of the steam binding situation from your analytical models is developed to the point where with this analytical model we can go in and investigate the implications of steam binding. I mean, for example, you and your analytical model, the contention is made that all of the various thermodynamic factors that influence steam, the steam binding phenomena, all of them are simulated. Now what I am asking is whether in terms of this model and in terms of your familiarity with it, whether that's a well-developed enough analytical tool for you to be able to apply it to different situations?

MR. MOORE: For example, when we'd make an assumption and say a set of calculations we make an assumption that there is a steam binding phenomenon and at some point the flooding rate becomes zero. Now what I am wondering is

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whether or not your model for analyzing steam binding is an analytical tool which in this circumstance can be used to tell us what then will happen, and does that make clear point of my asking questions which you regard as conjectural? I mean they are questions which really ask you not for conjecture, but ask to apply this analytical tool that you have. And now I am trying to see how well-developed the tool is in terms of how well it can be applied to various situations.

end

MR. MOORE: I am just questioning the relevance of that approach. I had indicated that the intent of the model is to describe the reflooding phenomena in a manner that can appropriately develop a conservative low flooding rate, recognizing the parameters that are important with respect to the determination of that flooding rate, and as applied directly to the reactor situation.

MR. FORD: Yes. Now, in terms of the way in which you apply the various analytical models that you have developed, as I understand it, if you have the entire phenomenon or chain of phenomena that's supposed to occur in an accident you can bind up this chain of phenomena, this Model 1, which specifically analyzes this, Model 2, which goes to the next phenomena, Model 3, and so forth, and with each model, you know, Model 2 comments on the scene and it says, "Well, what boundary conditions do I get from the Model 1?"

Model 1 tells me what core pressure is going to be at the end of the blowdown and then I go on to analyze what heat-up there is going to be after blowdown. So that as I understand the nesting of your various models they supposedly have a flexibility that they can be used in a given situation after specifying boundary conditions as calculated from another model.

Now, what I am asking you in terms of these questions about the steam binding phenomenon, is whether the steam binding model you have is such that I can take it, give it certain initial conditions, I can tell to the model, "Well, here I am. I have a flooding rate that has just become zero and it's just stayed at zero for such and such a point and I can define what the pressure is in the core and throughout the system at this point. These are your initial conditions. Now tell me what in the world is going to happen? Will I be able to overcome these various forces? Will the superheat that's formed, will that create a positive feedback mechanism heating of the upper axial levels such that at this point there is no hope of stopping the core heat-up, or will it tell me, "No, even if a tremendous quantity of superheated steam were formed at the low axial levels and went up to the high axial levels and aggravated the superheat phenomena,"it will tell me, "No, there is still no problem. We can proceed from those boundary conditions and that we can overcome this steam binding." So I think that, you know, I have completed the questions I had in this area, but in terms of, you know, making clear to you what the purpose of them was, it's both because of my interest, both because of my interest in steam binding and because of my interest in the level of development of the codes as indicated by whether or not they can say something intelligible about steam binding phenomenon given certain initial conditions.

MR. MOORE: Thank you.

CHAIRMAN JENSCH: I wonder. I wanted to ask you to restate the question. It was something about whether your analytical model can be used in applied analysis to the Indian Point facility as to whether this reflooding will build up superheated steam at the lower axial level and that sort of thing.

MR. MOORE: Yes. The models apply to the reflooding of the Indian Point reactor. It includes those effects.

CHAIRMAN JENSCH: And is there the prediction of superheat in the lower axial levels?

MR. MOORE: Yes. The effects of superheat at lower axial levels is included.

CHAIRMAN JENSCH: Very well.

MR. BRIGGS: Can it take into account a chugging phenomenon, if that were to occur?

MR. MOORE: No.

CHAIRMAN JENSCH: Have we reached the end of the line for today?

MR. FORD: Well, we have reached the end of steam binding for the day.

CHAIRMAN JENSCH: Very well. And you were about to go into another subject. We will start tomorrow, is that correct?

MR. FORD: Yes.

CHAIRMAN JENSCH: Is there any other matter we can take up before we recess this evening? I hear no such suggestions.

At this time let us recess, reconvene in this room tomorrow morning at 9:00 o'clock.

(Hearing recessed to Wednesday, November 10, 1971, at 9:00 a.m.)

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Regulatory Docket File

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