UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY OF NEW YORK, INC.

(Indian Point Station, Unit No. 2)

Docket No. 50247

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

Date - November 10, 1971 Pages 3192-3368

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UNITED STATES OF AMERICA

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INC. : DOCKET NO.

(Indian Point Station, Unit No. 2) : 50-247

- -

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

Wednesday, November 10, 1971

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

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

DR. JOHN C. GEYER, Member

MR. R. B. BRIGGS, Member

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MORNING SESSION

CHAIRMAN JENSCH: Please come to order. This public hearing is convened after being adjorned last night in this room.

We will recess to reconvene the public hearing at 9:15. Having recessed the public hearing, we will convene an in camera session of this proceeding. All the members of the public are excluded from the hearing. None is present in the room now.

It is the understanding of the Board that ten or fifteen minutes will permit a presentation of those data, which can be done in the in camera session. We will ask Applicant to assign one of his capable assistants to man the doors. So that at 9:15 we will reconvene the public hearing.

(In camera session follows.)

CHAIRMAN JENSCH: Please come to order. This is the public hearing. We are now reconvening at 9:15. We are ready to proceed with the presentation of evidence in open public hearing. All parties are represented and their witnesses are present. Mr. Moore is resuming the stand. Mr. Moore is on the stand. Are you ready to proceed, Intervenor's interrogator?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Will you proceed.

MR. FORD: The first set of questions that I have this morning concern the FLECHT tests and the methods used there to calculate the heat transfer coefficients and the methods used there to obtain the data that's used in the heat conduction equations.

Is it correct, Mr. Moore, that the temperature of the coolant is a necessary parameter in the heat conduction equations used to calculate heat transfer coefficients from FLECHT data?

MR. MOORE: No. Not the way the data was correlated.

MR. FORD: No. My question is whether the delta T, the difference between an assumed coolant temperature and the cladding temperature, whether that's a main part of the equation used, the heat conduction equation used to calculate the heat transfer coefficient?

MR. MOORE: Yes.

MR. FORD: Right. Now, is it correct that several different methods were used to measure local coolant temperature in the FLECHT tests?

MR. MOORE: Yes.

MR. FORD: Does the following list of methods include all the methods used in FLECHT tests to measure local coolant temperatures. One, bare thermal couples. Two, a steam probe consisting of a thermal couple in a guide tube surrounded by radiation shield, and, three, a redesigned steam probe used in Group 2 blockage tests.

MR. MOORE: Yes, I believe so.

MR. FORD: Is it correct that attempts at measuring local coolant temperatures prior to quench using the bare thermal couples were "unsuccessful" due to the influence of radiation on thermal couple response? My reference here is WCAP-7544, page 4-4A.

MR. MOORE: Yes.

MR. FORD: Is it correct that the original steam probe, the first steam probe defined, No. 2 in my previous list, offered little improvement over the bare thermal couple? My reference here is WCAP-7665, page 3-113.

MR. MOORE: Yes.

MR. FORD: And I believe we discussed the third kind of steam probe yesterday. My question then is, is the reason that you used an assumed saturation temperature in calculating the heat transfer coefficients rather than local fluid temperature the fact that you were unable to obtain through the several different methods accurate local coolant temperature measurements?

MR. MOORE: The redesigned steam probes seemed to give generally reliable and consistent values, but in order to reliably determine a heat transfer coefficient which could be used in the design calculations, the added complication of a detailed following of the temperature of the coolant did not seem to be in order from the standpoint of coming up with a more reliable approach, consistent approach.

MR. FORD: Right. In addition to the possibly greater reliability of the improved steam probe is a further reason for not being able to rely on that the fact that that was only used in the minority of the FLECHT tests?

MR. MOORE: That was also a consideration, yes.

MR. FORD: I'd like to explore with you, if I may, the consequences of sensitivity of the heat transfer calculations to the use of a single saturation temperature over all axial levels rather than the use of local coolant measures for all axial levels.

In our discussion at the calculation of the heat transfer heat coefficient, just so we don't get our signs confused, when I refer to delta T I am referring to the temperature of the cladding minus the temperature of the

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coolant.

Now, can you tell me cladding temperature, as I understand, is one of the pieces of raw data that was put into the heat conduction equations to get heat transfer coefficients, the cladding temperature is the temperature on the inside of the cladding, is that correct?

MR. MOORE: Is that the coolant stream you have going up?

MR. FORD: This is fuel rod 1 and this is fuel rod 2, and this is the coolant.

MR. MOORE: No, that's not correct. The temperature was on the -- well, O.K. You are on the inside of that thickness which represents the cladding?

MR. FORD: Right.

MR. MOORE: Yes, right.

MR. FORD: So the temperature is on the inside wall of the cladding?

MR. MOORE: Correct.

MR. FORD: Now, you use the known transfer, heat transfer properties of the cladding to compute an outside cladding temperature, is that correct?

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MR. MOORE: Yes.

MR. FORD: And since you know what the power distribution of the rod is, you know the local heat generation. In terms of accurately predicting the temperature outside of the cladding, there is no major uncertainty that enters there; is that correct?

MR. MOORE: Yes.

MR. FORD: One thing I am puzzled about is what you consider to be the temperature of the coolant. I know you assume a single temperature. Let us look at the situation itself before we explore the assumption that you make about it. The coolant channel has a certain size. It is filled with coolant of some density. If I stuck a thermometer into the middle of it, I might measure one temperature. If I stuck a thermometer closer to the cladding, I might measure a different temperature. As I got closer to the cladding I would be measuring another temperature.

From a theoretical point of view --

CHAIRMAN JENSCH: Does Mr. Moore agree with that?

MR. MOORE: That is a possibility depending on the coolant conditions.

CHAIRMAN JENSCH: Thank you.

MR. FORD: From a theoretical point of view, which of these possibly different cooling temperatures shouldn't we include in the Delta T or as the T sub-coolant in Delta T?

MR MOORE: That depends on how you are going to use the temperature of the coolant in order to get heat transfer.

MR. FORD: In terms of using the empirical correlation, using the standard heat conduction equations, which of the coolant temperatures would you pick? Because there is no chart on the record, I should label coolant temperature 1 as the coolant in the exact center of the channel. Coolant temperature 2 as the temperature of the coolant halfway between the center of the channel and the rod of reference. Coolant No. 3 as the temperature of the coolant within an episilon of the cladding.

To my question, then. In terms of the empirical way in which you are calculating heat transfer coefficients, if you had accurate measurements of coolant temperature 1, coolant temperature 2, and coolant temperature 3, which of those three coolant temperature measurements will you use?

MR. MOORE: I presume you could use any one as long as you correlated them to a consistent set of temperatures.

MR. FORD: If you systematically used the coolant temperature 1, the temperature of the coolant in the center of the channel, in terms of the possible heat transfers within the coolant, and in terms of the direction of those heat transfers, can you tell me what kind of over-all direction the routine use of one would make in the resulting heat transfer coefficients? What would be the difference in them compared? Would it be greater or larger compared to what you get if you used coolant temperature 2 or coolant temperature 3?

MR. MOORE: Is coolant temperature 1 higher or lower than coolant temperature 2?

MR. FORD: My presumption is if all of them are lower than -- presuming a general case in which the coolant temperature is lower than the temperature of the cladding, I would then further presume that temperature 1 was lower than temperature 2, which was in turn lower than temperature 3, but all of which were lower than the temperature of the cladding.

MR. MOORE: All right. Then the question is if I correlate my heat transfer coefficients to temperature 1 and then I correlate temperature 2, what would be the difference in the correlated heat transfer coefficient?

MR. FORD: Yes.

MR. MOORE: If temperature 1 is a lower temperature than temperature 2, and the heat input and clad temperature are the same in both cases, then using the lower temperature of T1 would give me a higher Delta T and would give me a lower correlated H for the same heat transfer.

MR. FORD: In terms of the calculations that you actually did, you took an axial level. You used the raw data of power generation and conductions through the cladding and so forth to get outside cladding temperature. Then you used an assumed constant value for the temperature of coolant; is that correct?

MR. MOORE: As a function of pressure for any given test, yes.

MR. FORD: You assumed that the nominal pressure of the bundle, the specific flood test b undle from which you are deriving heat transfer coefficients, that is the pressure of reference for determining saturation temperature calculation?

MR. MOORE: No. We knew the pressure in the system. From that pressure we determined saturation.

MR. FORD: That is what I am saying. You used a nominal pressure of the FLECHT bundle?

MR. MOORE: Yes.

MR. FORD: In terms of the state of the coolant at different axial levels, as I understand the FLECHT results, if you have the water level in fairly close proximity to the bottom of the core, you have the steam rising and it is entrained in water droplets; is that correct?

MR. MOORE: Yes.

MR. FORD: In terms of the water droplets that will have the density of them, that is greater for the lower axial levels; is that correct?

MR. MOORE: Yes.

MR. FORD: When we get up to the midplane area, to the fine three areas of coolant, the dense area is on the

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bottom with considerable entrained water; the area at the midplane, with somewhat less entrained water; then there is the eight to ten-foot axial level that may indeed have superheated steam.

Am I correct that the temperature of the part of the coolant that is most densely filled with entrained droplets, that this temperature is lower than saturation temperature assumed at the midplane?

MR. MOORE: Yes, I suppose it is.

MR. FORD: But this varies over all the tests and the period and so forth?

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MR. MOORE: Yes.

MR. FORD: So the general relationship is, as you say, lower?

MR. MOORE: Yes.

MR. FORD: It is also the case, I presume, that the saturation temperature here is, of course, lower by definition than the temperature of the superheated steam; is that correct?

MR. MOORE: Yes.

MR. FORD: Let us talk about the specific case and let us talk about computing heat transfer coefficients for these three axial levels. For the three-foot level, the mid-plane, here the six-foot level and here the 8 to 10 foot level.

Am I correct in appreciating the calculational technique that you used, as explained in WCAP 7435, page 3.1, that when you take the FLECHT data -- say this is FLECHT bundle 109, which is not a FLECHT bundle. It is perfectly hypothetical. If you took the data from this FLECHT bundle and it had a pressure of 60 psi, then you would go and find the saturation temperature. Then is it correct that irrespective of whatever the local cooling temperature was, you used that saturation temperature?

MR. MOORE: Irrespective of the local?

MR. FORD: Yes.

MR. MOORE: Yes.

MR. FORD: So the over-all effect here then is that assuming the midplane, the local coolant is indeed at saturation temperature. Is it correct to say that the over-all effect of your heat transfer calculation, and in this specific assumption, single local coolant temperature, is to underestimate the heat transfer at higher axial levels, and overestimate the axial levels, and presumably hit it on the nose at the midplane?

MR. MOORE: Are we still speaking of the FLECHT test?

MR. FORD: Yes, and the empirical derivation you make of heat transfer coefficients?

MR. MOORE: The FLECHT test, we have the heat transfer at any given indication. That is one of the inputs.

MR. FORD: You calculated the heat flux. I am talking about the heat transfer coefficient.

MR. MOORE: The coefficient itself.

MR. FORD: Yes.

MR. MOORE: Let's go back to the question. When you said heat transfer, I wasn't sure you didn't mean the amount of heat transfer.

MR. FORD: No.

MR. MOORE: Let's go back.

MR. FORD: I might explain that the reason I am talking in terms of your equasion only in terms of Delta T is that the only thing is linear as the ready simplification of the equasion. Since the heat flux and cladding temperature and all of this are just fixed for an axial level, the only thing you are talking about varying is the Delta T, am I correct?

CHAIRMAN JENSCH: There are two questions pending. He has interjected, let us go back to the question. I wonder if you would go back to the question that you overestimated in the upper level and underestimated in the lower level, or vice versa.

MR. FORD: The problem, Mr. Chairman, is a definition of terms. We got slightly on the wrong wave length as to what we were talking about.

CHAIRMAN JENSCH: Withdraw the first questin and ask a second one.

MR. FORD: We are specifically talking about heat transfer coefficients. Am I correct, first of all, that in terms of the basic equasion that is used here, just considering the transfer in the radial direction, disregarding properly the axial conductor, and in terms of the simple equasion that describes the heat transfer, that once you are given the power generation rate and inside cladding temperature, that everything else in the equasion is fixed except the Delta T?

MR. MOORE: Yes. Proceed.

MR. FORD: My question is, when you assume, in computing Delta T, you assume that the temperature of the coolant is constant, and that it is indeed the temperature at saturation. When you make this assumption, you make it, of course, in situations when there is really superheat and when there is really much cooler coolant. My question is, is it correct that when you fix the temperature of the coolant at saturation temperature, that this results in, A, an under-estimate of the heat transfer coefficients at higher axial levels, and, B, an overestimate of heat transfer at the lower axial level?

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MR. MOORE: That's an overstatement. I agree with the direction you have, underestimate and overestimate, with respect to a heat transfer coefficient which is predicted on the basis of the actual temperatures, yes.

MR. FORD: Yes. So that now specifically here I wonder if you recall our discussion of the other day of the comparison between the empirical method of calculating heat transfer coefficients and the more detailed thermodynamic model that the Idaho Nuclear was talking about, considering in your calculations in detail the termal conditions of the coolant in its equilibrium and non-equilibrium states and so forth.

Now in terms of that discussion is it clear that the differences between the two methods, the more complicated coupled thermohydraulic model proposed and discussed by Idaho Nuclear and the emperical correlation also discussed by them, is it clear that the actual differences between these two models are precisely the direction of misestimation, if you will, of underestimating the heat transfer at higher axial levels and overestimating it at lower axial levels?

MR. MOORE: No. We are back to terms again. Underestimating a specific heat transfer coefficient. The heat transfer, the way the empirical correlation is used, is equal to that observed in the tests.

MR. FORD: But am I correct that when you take this FLECHT test data you want to now go and analyze say Indian

Point 2 and you wanted a heat transfer coefficient, that you would take not--you wouldn't even take the heat transfer coefficient you calculated here at the high axial level on the basis of an assumed coefficient. You wouldn't even take that one. That what you would take is simply the heat transfer coefficient that you calculated for the midplane and apply that in your analysis of Indian Point 2 in the computer code to all axial levels?

MR. MOORE: Yes. That's an approximation, because the differences aren't significant with respect to what we are calculating.

MR. FORD: In terms of your ability to judge what the differences are between the two methods of calculation, am I correct that in order to make a firm quantatitative judgment that simply what you have to do is compute the heat transfer coefficients in your own way, then compute it in a way which varies the local coolant temperature with local coolant temperature and compare the two results? Is that the correct methodology for determining the difference your way of calculating it and the somewhat more detailed way of calculating it?

MR. MOORE: No.

MR. FORD: No. Well, you can answer the direct and general question about the differences in your method of calculation versus the more detailed method of treating the coolant. Can you show me how you can answer this question directly and generally in a simple way that simply isn't by referring to the fact that the calculations come out the same?

MR. MOORE: Yes. I didn't say the calculations come out the same. We got onto the question of what would be the effect instead of the simplification that's made in our analysis of assuming the heat transfer coefficient that exists at the hot spot exists over the whole rod. The way to determine the sensitivity to that heat transfer is to use the heat transfer obtained from FLECHT, transfer coefficients obtained from FLECHT at the different elevations, and apply them instead of using the hot spot heat transfer coefficient over the whole rod. Then we have applied it in a consistent manner and can determine what the effect on the temperature is.

MR. FORD: I don't quite follow what the different way of applying it is that you are talking about.

MR. MOORE: You understand that the calculation, when we talk about the hot spot, which the temperature of interest, we want to know what the temperature of the cladding is at the hot spot and we want a good calculation of the heat that is removed from the cladding.

The heat that is removed is a heat transfer coefficient times a Delta T. Now this I would call a reference Delta T because we have assumed the saturation temperature of the coolant in determining this Delta T. Now at the hot spot we take that FLECHT data, which is derived the same way with this reference Delta T and then we get a consistent heat transfer coefficient for that reference Delta T. So we get the correct heat release at the hot spot.

Your question was when we do the analysis of the whole channel we put a simplification in that the heat transfer coefficient, the h that we use at this location, was equal to the h we used here at the hot spot.

Now in order to quantify the importance of that you can take the FLECHT test now for this location and take its heat transfer coefficient from FLECHT test now for this location and take its heat transfer coefficient from FLECHT which determined from this same reference temperature approach, and calculate the heat released at this elevation from the FLECHT results rather than the h that is derived from the heat released at this elevation and then calculate the effect on the peak temperature or the temperature at this elevation.

MR. FORD: But you are still not able to, when you look to FLECHT data, to actually take a heat transfer coefficient that is defined by the coolant temperature of reference for this axial level. You are still using a temperature of reference for the hot spot, is that correct?

MR. MOORE: No, no. We have a temperature at this elevation.

MR. FORD: Yes. You have the temperature of the cladding. By temperature of reference I mean the thing that we have been talking about for the last half hour, the temperature of the coolant.

Am I correct that there is no use that you can make of FLECHT heat transfer coefficients because of the way they are calculated, assuming the saturation temperature of the coolant, that you can make no reference, no use of that data, without carrying along implicit in the coefficient you are using the fact that it was calculated at a non-Loca coolant condition?

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MR. MOORE: No. The key point is I have done it in a consistent way. I have put a certain flooding rate into a certain bundle and I am trying to calculate the heat transfer along this bundle. I have chosen to use an arbitrary coolant temperature in both cases, in both cases, the FLECT and the reactor calculation, so I am on a consistent basis. And so whatever the temperature here is in the FLECHT bundle, that's the same temperature in the reactor for the same power and flooding rate.

So therefore the fact that I have assumed some other temperature really isn't important. The important thing is I have simulated the conditions along the channel in the FLECHT test.

MR. FORD: But I am not trying to ask you or challenge your consistency. Indeed, it's the consistency itself. It's realism that I am wondering about. So that as I understand you what your defense of the heat transfer calculation is is that from a mathematical point of view the equations and coefficients and so forth are consistent, they are defined in a consistent coolant temperature of reference. Is that the prime reason for believing that even when you use this coefficient at different axial levels from a mathematical point of view you are still consistent in all of this?

MR. MOORE: And from a physical point of view we are consistent, in that the temperatures at this elevation for the FLECHT test and for the reactor are the same.

MR. FORD: Now, in the FLECHT test at which negative heat transfer coefficients or reverse heat transfer was observed can you tell me in the reactor, in the actual reactor or in your computer analysis of it, if you are using the midplane heat transfer coefficients which were negative how do you ever simulate the actual reverse heat transfer that was observed in the FLECHT test in your computer analysis of say Indian Point 2?

MR. MOORE: We don't. But then you asked how could I determine what that effect was and I said I would use the heat transfer coefficient at that location, which would show this reverse heat transfer.

MR. FORD: I see. Let me go into some other questions that I have prepared on the whole reverse heat transfer problem and see whether at the end of those questions we might be able to come back here and assess what the degree of realism is.

Now, is it correct that the temperature of the steam is a function of the heat transfer to the steam from the cladding as it travels up the channel?

MR. MOORE: Yes.

MR. BRIGGS: Could I interrupt here just a minute. I'd like to get something a little bit straight. You make the point that down at the bottom of the channel you have steam with a fairly large amount of water in it. Let's say that the steam quality is ten per cent. And up in the center of the channel you make the point that the temperature is higher, well, let's say first you make the point that the steam has less water in it, and so let's say that the quality is 60 per cent at the center of the channel.

Now, you further make the point that the temperature of the steam is lower where the quality is ten per cent than it is where the quality is 60 per cent. Is this right?

MR. FORD: This is a question that I put to Mr. Moore and which he answered in the affirmative.

MR. BRIGGS: Well, he seemed to be unusually agreeable early this morning and I wondered if that was so.

MR. FORD: I appreciate the fact, Dr. Briggs, that inanswer to a question of yours yesterday that I believe he answered the other way around, and I am very interested in that and I am going to pursue it.

MR. BRIGGS: Well, I'd like to get this point straightened out. Do we all agree that there is a significant difference in temperature of the steam where the quality is ten per cent and where it's 60 per cent?

MR. MOORE: Heavens, no!

MR. BRIGGS: Well, does the Intervenor suggest that there is a significant difference, an appreciable difference in the temperature of the steam where the quality is ten per cent and where it's 60 per cent?

MR. FORD: No.

MR. BRIGGS: So then we can pretty much agree where the quality of the steam is less than one that the temperatures are about the same, but where the steam is superheated then the temperature is higher, is that correct?

MR. FORD: Well, I think the problem here, which is why I say no, as a direct and general answer to the question, is that the assumption that I make simply when we talk about the quality of the steam is that we are talking about the coolant as I put it in a specific volume here. We are talking about the coolant being homogeneous within this volume. My impression of where the temperature differences come with the steam is that this assumption of homogeneity in an equilibrium is wrong and that you do get within the coolant channel as you progress from top to bottom, you do get steam that's increasing in temperature, but it isn't in equilibrium with the remainder of the coolant in the volume.

MR. BRIGGS: Well, I don't question that, that there certainly is an equilibrium that exists. The question, of course, is how much difference in temperature exists as a result of this, and I'd like that to be kept in mind when one talks about how the steam temperature rises when the quality is less than one; of course, again when the quality becomes one I don't have any problem.

MR. FORD: Yes.

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MR. BRIGGS: Thank you.

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MR. FORD: In terms also of the isotherms that we can use -- I am simply talking here about an isotherm with saturation temperature -- and in answering the question I was just noting how you can have different qualities across here along the isotherm.

MR. BRIGGS: Yes.

MR. FORD: Now one of the concerns that I have, I am trying to hold my direct answer to that somewhat in abeyance, because in terms of the appropriate isotherms to use in analyzing various phenomena, especially blowdown here, I don't want to give the impression that I am endorsing this particular shape of it.

MR. BRIGGS: We are not talking about blowdown, are we? We are talking about reflood.

MR. FORD: Yes. But I am just pointing out, Mr. Briggs, that in terms of my own consistency in the positions that I am going to take I would like to be able at a different point to say that the relevant isotherms to be used in blowdown shouldn't be this kind of isotherm familiar in engineering, but it should be something calculated using Van der Waal's equasion of state.

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MR. BRIGGS: That's during blowdown.

MR. FORD: Yes.

MR. BRIGGS: Are we going to talk about different isotherms for blowdown and for the reflood?

MR. FORD: I think it may indeed be the case, that we should be talking about Van der Waal's isotherms to account for various anamolous coolant properties. However, in terms of the extent to which we are going to get into this in our time I don't know. But I simply want to reserve the right at some point to say that whereas in this context I am happy to use a standard isotherm, later on I am going to say that it's this standard isotherm which is a big problem in the analysis.

MR. BRIGGS: Thank you.

MR. FORD: If we were discussing an early portion of reflood before we actually had a lot of entrainment of water here, when this was mostly steam at the bottom, the water level was still low, then we could state fairly clearly, am I correct, that then the temperature of this steam is less than the temperature of the steam at the midplane, which is less than the temperature of the steam at the higher axial level because of the heat transferred to it by the cladding.

MR. MOORE: Until we have entrainment we don't really have any heat transfer at the cladding. In fact, in the calculations of the reactor it's assumed to be zero heat transfer.

MR. FORD: Yes, I appreciate that. But in terms of the fact that -- before entrainment am I correct that there is some steam rising in this channel, is that correct?

MR. MOORE: No.

transient.

MR. FORD: In terms of the length of this point in the transient I believe we noted on Monday that in tests with flooding rates of less than two inches or so, the low flooding rate test, that in these flooding rates am I correct that this situation could persist for on the order of a hundred seconds after the beginning of reflooding?

MR. MOORE: Yes.

MR. FORD: So that throughout this period it would be possible for the steam that's absorbing the heat at lower levels to be taking this heat and transferring it to the higher axial level?

MR. MOORE: Yes.

MR. FORD: Now, the general concern that I have here, and I am going to ask my general question, is that isn't it possible that the mechanism of negative heat transfer observed in the FLECHT tests presents us the following insight about the nature of heat transfer during reflood, namely that it may be possible, because of the absorption of heat by the moving coolant, that the net effect of the reflood period may be simply to move the hot spot from the midplane to an area above it, which, because of its own low decay heat power generation wasn't very hot in the first place, but because it acts as a sink for the superheated steam that we get an axial conductance in a very sophisticated

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MR. FORD: There is no steam in the system at all before there is actually entrainment here?

MR. MOORE: There is essentially no steam movement within the bundle.

MR. FORD: But there is steam?

MR. MOORE: Yes.

MR. FORD: You contend its velocity is small?

MR. MOORE: Yes.

MR. FORD: Now if it's heated it's also expanding; is it simply that expansion that counts for its small velocity?

MR. MOORE: Well, I believe the test was run. We come to an essentially eq ilibrium condition with steam and essentially no heat transfer, then start to flood from the bottom.

MR. FORD: But by essentially no, you mean all that you are really doing is qualifying a magnitude of the heat transfer at this point?

MR. MOORE: Yes.

MR. FORD: As the steam in the course of the flooding, as the steam picks up heat, is it correct that as observed in the FLECHT test when the steam that's absorbed heat from lower levels gets to the higher levels it may be possible that the temperature of that steam is higher than the temperature of the cladding?

MR. MOORE: Yes. At a certain point in the way by means of the coolant from the lower levels to the higher levels?

MR. TROSTEN: Mr. Chairman, I ask the reporter to reread that question, please.

(The pending question is read by the reporter.)

MR. TROSTEN: Mr. Chairman, I confess I don't understand the question and don't know whether to object to it. If Mr. Moore understands it, fine.

CHAIRMAN JENSCH: I think that's a pretty good guide. If a technical man doesn't understand, I'm sure he will say he doesn't understand it. If you have a legal objection, please state it. Proceed, please.

MR. MOORE: I understand the question. That is precisely one of the things that was determined in the FLECHT test. In all the cases of the FLECHT test, the peak temperature always occurred at the midplane. These temperatures further up the channel were always lower than the peak, even with the effect of superheated steam that you postulate.

CHAIRMAN JENSCH: Then your answer to the question is no?

MR. MOORE: That's right.

MR. FORD: I am talking about a kind of result of the FLECHT test. My understanding was not anticipated before the FLECHT tests were run. In this regard can you tell me whether Westinghouse had ever performed any previous analysis of the possibility of moving the hot spot because of this kind of heat transfer mechanism through the coolant that produces axial conductants of the heat?

MR. MOORE: This mechanism is no surprise. It is straightforward and energy balance and heat transfer phenomena.

There is nothing magic or surprising about the phenomena that we have been discussing.

CHAIRMAN JENSCH: I think the question was, have you ever conducted experiments in that regard.

MR. MOORE: That's fundamental heat transfer, sir. It is straightforward.

CHAIRMAN JENSCH: You took it from the book and let it go at that; is that right?

MR. MOORE: Pardon me?

CHAIRMAN JENSCH: You took it from the book and let it go at that; is that right?

MR. MOORE: Yes.

CHAIRMAN JENSCH: No experiments?

MR. FORD: In terms of the relative power distribution of the eight to ten-foot axial levels versus the midplane, we get a relative power distribution of, I believe, something on the order of the factor of 50 per cent; is that in terms of the mean -- am I correct that the midplane's power was 1.6 versus the eight to ten-foot level that had a power of approximately .8 of the mean. So there is a factor of two in difference between the power?

MR. MOORE: Yes.

MR. FORD: During the test was it the case that the relative temperatures of the eight to ten-foot elevation and midplane elevation were of that same proportion?

MR. MOORE: I don't know.

MR. FORD: Isn't it indeed the case that the proportion changed significantly; that this axial conductance mechanism by way of the coolant was at least observed in the FLECHT test to narrow the gap significantly in relative temperatures between the so-called hot spot and the presumably cool higher axial level?

MR. MOORE: No.

MR. FORD: Can you tell me the exact analysis and cite this in the FLECHT test that you performed with the question of this axial conductance in mind, to show that in terms of relative temperatures you didn't vary from the relative temperatures you would expect simply on the basis of power distribution?

MR. MOORE: That isn't what I said. That's a different statement.

MR. FORD: Let me get it clear, then. My question was whether the relative temperatures between the eight to ten-foot level and the six-foot axial level, whether those relative temperatures in the FLECHT test or in the transient, whether they stayed in the same proportion, same ratio as you would expect from their relative power?

MR. MOORE: That's a different question.

CHAIRMAN JENSCH: Whatever it be, can you answer it?

CHAIRMAN JENSCH: What have you handed the witness now?

MR. FORD: I am handing him the Idaho Nuclear overview document on the FLECHT test. This is IN-1386, Table --

MR. TROSTEN: Mr. Chairman --

CHAIRMAN JENSCH: Let him finish.

MR. FORD: II, which indicates the flooding rates for all of the tests and gives all the other parameters that I asked for as well.

MR. MOORE: No. That table is Table 3-1 in WCAP 7665.

MR. FORD: This table here isn't giving the same data?

MR. MOORE: No. That's proposed test sequence.

The actual test sequence is described in Table 3-1.

MR. FORD: Excuse me. As this is written after most of these tests were done?

MR. MOORE: What's the title of the table?

MR. FORD: PWR FLECHT test sequence. It says nothing about being just a proposed sequence.

MR. MOORE: Table 2 is a listing of the actual test sequence being followed.

MR. FORD: Yes.

MR. MOORE: I am pointing out that if you really want to know exactly what the test conditions were, you should dismissal of this mechanism as a possibly significant thing in a loss of coolant accident?

MR. MOORE: Yes.

MR. FORD: Can you tell me how many low flooding rate tests were there?

MR. MOORE: 23 or 24.

MR. FORD: Of the 23 or 24 tests that you conducted with low flooding rates, can you tell me what the variation was, the range of initial conditions and pressures and so forth?

MR. MOORE: These are in the report. I would have to go back and dig them back out. All of them were done with the peak power, 1.24 kilowatts per foot. What parameters are you specifically interested in?

MR. FORD: I am interested in their initial temperature, flooding rate, power density, inlet cooling temperature and cladding material in bundle size.

CHAIRMAN JENSCH: That seemed like a pretty long list to me. Do you want to take a break in order to get these data?

MR. MOORE: They are all in the report that Mr. Ford has.

MR. FORD: My summary of the FLECHT test series has given me -- I have been able to answer my own question. If you will agree, we can put this in the record as the answer.

MR. FORD: Whatever it was.

MR. MOORE: I am trying to make the record clear that that's the third question on the same topic, and each one is different. I will answer that question.

MR. FORD: Fine.

MR. MOORE: There was an effect of the ten-foot elevation of getting a higher temperature than just from the relative differences in power, because of the differences in heat transfer, yes.

MR. FORD: So that the mechanism of axial conductance by way of the coolant that I am talking about, that the FLECHT data is at least consistent with the possibility that this mechanism may redistribute the relationship or change the relationship between the hot spot and different axial levels?

MR. MOORE: Yes, it is a total integrated heat transfer phenomena.

MR. FORD: The point I am wondering about is, simply in terms of the fact that we have only a certain number of tests under a certain number of conditions, whether or not we have a sufficient basis to say that this mechanism with the responsibility of changing hot spot location relative to the rest of the rod, that this mechanism really isn't something that we have to worry about. Do you think that we have a significant number of tests on which to base such a

CHAIRMAN JENSCH: Let us proceed. Mr. Moore has resumed the stand.

Are you ready to proceed, Intervenors' interrogator?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Will you proceed, please.

MR. FORD: Is it correct that the values of the parameters in the FLECHT tests represent a nonrandom selection of the possible parameter combinations?

MR. MOORE: I don't believe so. We discussed that yesterday, I believe.

MR. FORD: If I decided to take a criterion such as popularity, and applied it to a group of people, a set of people, and if I picked a sample of ten people out of one hundred thousand people, and I chose them on the basis of whether or not they were well known to me, would that be a random selection of people from that sample?

MR. MOORE: I guess so.

CHAIRMAN JENSCH: What is the random sample?

MR. FORD: I was trying to give the definition in terms of -- shall I ask Mr. Moore?

CHAIRMAN JENSCH: He says he guesses. Maybe he has a different definition than you have.

MR. MOORE: Repeat the question, please.

MR. FORD: The case?

MR. MOORE: Yes.

CHAIRMAN JENSCH: This is something that could be conveniently done at the break? Is this a convenient time to interrupt your examination for this purpose?

MR. FORD: Yes. I think if I asked the question that I am concerned with in this data, then we can do it.

CHAIRMAN JENSCH: Proceed, if you need a little further identification.

MR. FORD: The question that I am asking is what are the parameters for the low flooding rate tests, the parameters in terms of heat rate pressure, peak power, inlet coolant temperature, and so forth?

CHAIRMAN JENSCH: Are all those shown on the Table 3-1 which is referred to?

MR. MOORE: Yes, sir.

CHAIRMAN JENSCH: At this time let us recess and reconvene in this room at 10:30.

(A short recess is taken.)

MR. FORD: Fine. I think we have two sources for the same data.

In terms of the tests with low flooding rates, am I correct that the initial temperatures here, with perhaps two or three exceptions, was 1600 degrees Fahrenheit or less?

MR. MOORE: No.

MR. FORD: No?

MR. MOORE: No.

MR. FORD: Let me see if we have the same numbers o these,the numbers of your tests?

MR. MOORE: No.

MR. FORD: You don't have numbers like that?

MR. MOORE: We have our own test numbers as indicated in Table 3-1 in the report.

MR. FORD: Let me get it.

MR. MOORE: Please.

MR. FORD: Which is that?

MR. MOORE: Table 3-1 of WCAP 7665.

MR. FORD: What page is that on?

MR. MOORE: 3-4, 5 and 6.

MR. TROSTEN: Mr. Ford, have you been furnished this document previously?

MR. FORD: Yes, sir. I have been relying on the Idaho Nuclear. I will be happy to transform my test numbering to yours.

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go to Table 3-1 which exactly describes the test conditions as measured.

MR. FORD: The case was, I have a sample of one hundred thousand people and I want to select twelve of them to be on a jury. If I went through this list of one hundred thousand people and rejected or selected only people who had blond hair as my first round of selection, would that be a random or nonrandom selection of potential jurors from a sample of one hundred thousand people?

MR. MOORE: Nonrandom.

G1Bt1

MR. FORD: Now as I went through the various parameter combinations that we could use in an experiment, and if I decided that my criteria for choosing parameters was that I was mainly concerned with representing a situation which I thought was typical, would my choice of parameters based on the criterion of whether I was on the whole choosing typical values, would that be random or non-random?

MR. MOORE: I am afraid I don't understand that. It got a little involved. Try again.

MR. FORD: I have a set of 30,000 parameter combinations. Now for a possible experiment like FLECHT tests I want to choose, because it's expensive every time I make a run, I want to choose only a small number, say seventy-three parameter combinations. Now if I chose seventy-three parameters, using the criteria applied to all of the possible parameter combinations, as to what was a typical parameter combination or typical parameter values within a range, would I be making a random or non-random choice of parameter combinations?

MR. MOORE: I guess I would characterize that as a more non-random.

How do you define typical?

MR. FORD: I just decided, I took for each parameter range and underlined, as for example the FLECHT report does in its table in Dn-1300 of parameters, as was done on this table in the FLECHT test, if I selected certain parameters and

G1Bt3

MR. FORD: Can you tell me specifically with regard to the problem of superheated steam or I should say the phenomenon of superheated steam and reverse heat transfer, when you selected the parameters for the FLECHT test did you specifically say, "We want to select parameters which will be favorable to the presence of superheated steam, such that given the favorable conditions for its occurrence we can settle the question, the most favorable conditions for its occurrence we can settle the question of whether or not it will occur.

MR. MOORE: The most favorable conditions with respect to what? What bounds have you put on most favorable conditions?

MR. FORD: Well, in terms of some general facts we know.

For example, we know, I suppose, that the higher the temperature of the entire rod, this means the higher the temperature of the higher axial levels. So that in terms of their heating up in a disproportionate way, the possibility for this is increased if we begin with a higher temperature, is that correct?

MR. MOORE: No. I would argue the reverse.

end

GLBt2

labeled them typical and decided to do most of my experiments with them as the value of the parameter range that I was using, would that be a non-random process?

MR. MOORE: No. As I understand you have picked a typical set, and that is what you are looking at. You have defined a typical set. You just didn't pick it at random.

MR. FORD: Yes. It is. The answer is that it's non-random.

MR. MOORE: Thank you.

MR. FORD: When you selected parameters with some criteria, when you selected parameters for the FLECHT test, can you tell me whether or not you did it in this fashion; you said there are so many possible bad events that could occur, there is, say, six of them, now the parameters that would, you know, really contribute to each of these events can be identified. So in order to really determine whether or not this bad phenomenon would occur in a real situation you decide, well, let's actually choose the paramaters that would make this bad phenomenon, about. If it's true that our <u>a priori</u> relationship between these parameters and the bad event is true.

Did you actually do this? Did you choose parameters with specific problems that you wanted to investigate?

MR. MOORE: Yes. I believe there was some engineering judgment involved along those lines.

MR. FORD: The reverse?

MR. MOORE: Yes.

MR. FORD: So that if you wanted to study the question of superheat you would study very low temperatures?

MR. MOORE: I am just thinking about the problem that the postulation is that the steam temperatures at the exit become higher than the cladding temperatures and there is a reverse heat transfer. It would seem to me that if the cladding temperatures are higher all along the channel that my likelihood of getting an even higher steam temperature is reduced, since the amount of power I am putting into the channel is the same in all cases.

MR. FORD: Yes. But is it the case that if you had a higher high elevation temperature then it was closer to the dangerous levels at which embrittlement or melting takes place, such that if you began with a higher high elevation temperature and then it received heat into itself from the coolant, that then you would be in a much worse situation in terms of the thresholds above which events which we try to preclude might happen?

MR. MOORE: Your question was events favorable to reverse heat transfer. You have now added an additional factor, which is not reverse heat transfer per se, but is also the corresponding maximum level that is reached during the transient. You have different considerations.

MR. FORD: Yes, that's correct. My questions relate to if we wanted to find out whether reverse heat transfer were a problem, isn't it correct that what we would do is try to set up the parameters in such a way that we might expect them to aggravate the problem and so that then having set up the most favorable conditions for the occurrence of the problem, if it didn't occur we'd have a very strong indication that it wasn't a problem, whereas, of course, on the other hand if it did occur then the problem would be well-established.

MR. MOORE: I'm having difficulty again with what your interpretation is of most favorable conditions.

CHAIRMAN JENSCH: And may I go back to the question that kind of raised that question. I think the question was propounded, "If you wanted to really settle the question about superheated steam would you stick to parameters more favorable to superheated steam?"

And you said, "No. I would argue just the reverse."

So the question was, would you study the lower --

MR. MOORE: No.

CHAIRMAN JENSCH: And the question, I think, that was outstanding was, "You would study the lower?"

And I don't think you answered that.

MR. MOORE: No, I am sorry. The discussion was I asked Mr. Ford, "What do you consider most favorable?"

And then he said for example, "If you have higher temperatures, initial temperatures, wouldn't that be most favorable to this reverse heat transfer?"

And I said, "No, I would argue the reverse."

CHAIRMAN JENSCH: You said, "We would study the lower"

MR. MOORE: And I explained to him why I would consider that higher initial temperatures would not necessarily be conducive to reverse heat transfer.

CHAIRMAN JENSCH: Well, would you study the lower temperatures?

MR. MOORE: I would study a range of temperatures.

MR. FORD: Yes. Well, it's correct, I take it, from both your analysis of one set of parameters which would aggravate reverse heat transfer problems and from my own analysis of another set of parameters, a higher temprature that would aggravate the problem, that there are clearly a number of different parameter combinations that might aggravate reverse heat transfer, is this correct?

MR. MOORE: Yes.

MR. FORD: So that if, for example, we took, let's say, a given fixed temperature somewhere in the midrange that temperatures are expected to rise to in a loss of coolant accident and we made that our initial condition, is it then the case in terms of the analysis that you have given that it may be that if we really wanted to aggravate the reverse heat transfer we'd set a much lower temperature?

MR. MOORE: That was my contention, a lower heat transfer.

MR. FORD: I am just trying to --

MR. MOORE: Yes.

MR. FORD: -- get it all straightened out.

Now is it also correct that if you wanted to increase the probability for reverse heat transfer to lead to, say, melting or embrittlement, that you would pick a temperature higher than the midrange in order to see whether this would result?

MR. MOORE: That's not only a function of looking at effects such as reverse heat transfer, which, by the way, to put in proper context, were observed at essentially the 10-foot elevation, not below that.

MR. FORD: Yes.

Now, my question was as Mr. Chairman appreciates --

CHAIRMAN JENSCH: I have trouble keeping my notes, if you will excuse me for interrupting, but I understood your answer to be, there may be other factors involved too. But I wonder if before you get to describing what the other factors are if you would answer the question whether you would pick a higher temperature such as might lead to embrittlement or melting? Let the question be reread, please, Miss Reporter.

(The previous question is read by the reporter.)

MR. MOORE: Not necessarily.

CHAIRMAN JENSCH: Proceed.

MR. FORD: In combination with other factors would this higher temperature aggravate reverse heat transfer?

MR. MOORE: Not necessarily.

MR. FORD: I am trying to get you to be more precise about "not necessarily." I mean if we took -- if we increased the temperature and changed what other parameters would we then have reason to expect that the negative heat transfer problem would be aggravated?

G3Bt1

MR. MOORE: The high temperature per se I don't feel aggravates reverse heat transfer. The aggravation is at low flooding rates.

MR. FORD: Now if the ten-foot elevation had a temperature of 1600 degrees and through reverse heat transfer it received 500 degrees worth of incremental temperature, it would be under the interim criteria, is that correct?

MR. MOORE: If you are asking me is 1600 and 500 less than 2300, the answer is yes.

MR. FORD: Yes. Now if the cladding at the ten-foot elevation were at a temperature of 2,000 degrees and through reverse heat transfer it received another 500 degrees in temperature, would this be above the interim criteria?

MR. MOORE: 2500 is above the interim criteria.

MR. FORD: So that the significance of reverse heat transfer in our example, is it correct that it is a function of what the temperature before heat treansfer at the axial ten-foot level was?

MR. MOORE: Would you repeat that, please.

CHAIRMAN JENSCH: Let the reporter reread it, please.

(The pending question is readby the reporter.)

MR. MOORE: No. You have grossly simplified the discussion. It's the function of what happens up to that point also.

MR. FORD: Yes. But all other things being equal is simply the fact that it's at 1800 degrees versus the fact that it's at 1600 degrees, does that fact imply that in that situation negative heat transfer has more potential for contributing to problems than if it were at a lower temperature?

MR. MOORE: Yes. That's a very academic hypothesized situation and I am sure you realize has nothing to do with the reactor situation.

MR. FORD: Yes. Now can you tell me--

MR. MOORE: Excuse me. My comments are directed toward the application of this data and its use in the Indian Point analysis, you understand.

MR. FORD: Yes. Now I am concerned, my point of view is whether or not this data, the experimental program, was set up in such a way that we could expect from it an answer to the question is negative heat transfer a problem.

Now with this prospective in mind, can you tell me in terms of the parameter combinations and how they are described, the main descriptions that I have indicated, that it was felt that such-and-such water temperature range and such-and-such within it was the typical value and so therefore most of the tests, all of the tests were run within the range and most were run at this typical value, can you tell me is there any place in which you make further elaboration of your reasons for choosing certain test parameters as the test parameters?

G3Bt2

MR. TROSTEN: Mr. Chairman, I object to the question for the reason that it appears that Mr. Ford is trying to find out whether the Westinghouse test program was set up in such a way as to get as much data as possible or to prove some general point that Mr. Ford is interested in proving.

Let me finish, please.

And whereas the purpose of the test was to prove certain points related to the safety of the Indian Point 2 reactor and the PWR reactors in general, and it seems to me that the thrust of his question is leading into an irrelevancy.

MR. FORD: Mr. Chairman, I am not trying to ascertain, as Mr. Trosten pointed out, the most general facts about the Westinghouse program. The specific point that I am trying to ascertain--I have studied the purposes of the FLECHT test and so forth and the specific investigation and the problem of negative heat transfer is not given. And I am trying to ascertain whether in setting up the parameters, in choosing the parameters and setting up the tests, whether Westinghouse was concerned with this problem such that they could say or could not say that "We have investigated the specific issue," rather than just taking tests done for other purposes and trying to say, "Well, it doesn't show negative heat transfer."

CHAIRMAN JENSCH: Can you pose the question as you have now stated the problem?

MR. FORD: Yes.

G3Bt4

CHAIRMAN JENSCH: The objection is overruled.

MR. FORD: Can you tell me did Westinghouse in setting up the FLECHT tests and any of these tests specifically choose parameter combinations with the specific purpose in mind of investigating whether negative heat transfer would cause serious problems in a loss of coolant situation?

MR. MOORE: Yes. In the sense that we were developing a set of conditions to run that incorporated the conditions expected to exist in Indian Point 2 reactor for this transient with the express purpose of determining the heat transfer behavior of the entire rod, fuel rod bundle, in order to determine peak clad temperatures. These conditions assumed for the test covered a range of parameters which encompassed the particular parameters of interest that are described in our testimony on the calculations for Indian Point 2.

And as I indicated under all of these conditions, including all of the low flooding rate conditions, in no case did the peak temperature occur anywhere other than at the hot spot. And where you have overemphasized I am afraid the negative heat transfer or reverse heat transfer is a more correct statement alluded to in the report at the ten-foot elevation, which was intended to indicate to the reader why particular temperature behavior at the ten-foot elevation existed.

end

MR. FORD: Mr. Chairman, as I understand Mr. Moore's answer to my question, whether they specifically set up things to investigate negative heat transfer, as I understand the question, he has told me that the general purpose of these tests was to investigate heat transfer in general. He has told me, secondly, that he does not think that negative heat transfer is a big problem.

I don't feel that that is responsive to my question. There is really a direct answer as to whether they directly investigate set-up things to investigate negative heat transfer. And I ask your judgment as to whether that was directly responsible or not.

CHAIRMAN JENSCH: I understood he gave a specific yes, and then he explained what he did. So I infer that he has given a direct answer. That doesn't prevent you from pressing his explanation, perhaps, or any other approach you desire.

MR. FORD: Your answer was, as I understand it, that you indirectly investigated negative heat transfer because you investigated all of heat transfer, is that your position?

MR. MOORE: I investigated -- no. I investigated the heat transfer characteristics under a loss of coolant situation, although heat transfer characteristics under those specific loss of coolant situations.

MR. FORD: So that as a matter of the focus, you were concerned, you know, with the general questions of heat transfer and you didn't set for yourself the goal at the beginning of these tests of agreeing whether negative heat transfer at high axial levels was the problem or not?

MR. MOORE: No, I didn't say -- I don't know what you characterize as general heat transfer. I was specifically setting out to determine the heat transfer conditions in a reactor in a loss of coolant conditions.

MR. FORD: Let us discuss the specific parameters for the FLECHT test that had low flooding rates. I am referring to Table 3-1 in WCAP 7665. I believe all of the low flooding rate tests with flooding rates less than 1.1 inch per second, this class of low flooding rate test, and all of them are contained in a section of the table on page 3-6; is that correct?

MR. MOORE: All the tests less than 1.1. Low flooding rates would also include the 1.9 or 2, also.

MR. FORD: Let us stick, for the moment, to the criterion of low flooding rates here of 1.1 inch per second.

Am I correct, as I go down the initial temperatures for these low flooding rate tests, that the initial temperatures for the 13 low flooding rate tests were 1632 degree Fahrenheit, 1795 degree Fahrenheit, 2012 degree Fahrenheit, 1605, 1603, 1602, 1063, 1580, 1600, 1600 and 2028?

MR. MOORE: Yes.

MR. FORD: In terms of the mean temperature here, is it correct that most of the tests were done at the initial temperature of 1600 degrees, approximately?

MR. MOORE: I point out there are tests at 1.9 inches per second which is defined as a low flooding rate on page 340, which started us on this discussion several days ago.

CHAIRMAN JENSCH: I know. He said, let us confine ourselves to 1.1. Let us stay with that because we are getting diverted in something else.

MR. MOORE: Mr. Chairman, I disagree with the characterization of the low flooding rate of 1.1 or less. May I make that disagreement, please?

CHAIRMAN JENSCH: Yes.

MR. MOORE: Thank you.

CHAIRMAN JENSCH: But confine your answers to whether it would be 1. 1, higher or lower. Let us not get to 1.9.

end

MR. FORD: We will discuss the question of what is a low flooding rate with Mr. Novak tomorrow.

In the low flooding rate less than 1.1 inch per second range, is it correct that the pressure in all of those bundles was at the high end of the pressure spectrum of the FLECHT test?

MR. MOORE: Yes.

MR. FORD: Is it correct that the peak power was at a constant value for all of these tests?

MR. MOORE: Yes.

MR. FORD: Is it correct that the inlet subcooling, in degrees Fahrenheit, was at approximately a constant temperature of 135 to 140 degrees?

MR. MOORE: 250 degrees.

MR. FORD: In terms of the range of various parameters, is it correct that the deviation from the standard initial temperature, that the standard deviation in these tests compared to the mean is quite small?

MR. MOORE: You haven't included all the variable in the test.

MR. FORD: I am talking about the initial temperature range. Is it correct that the standard deviation as a fraction of the mean is quite small in all of these tests?

MR. MOORE: Yes, I suppose.

MR. FORD: Is it correct that the low temperature range of the FLECHT tests in general, that the temperature range went from 800 degrees Fahrenheit to 2400 degrees Fahrenheit?

MR. MOORE: The total range?

MR. FORD: Yes.

MR. MOORE: Yes.

MR. FORD: In the range tested for these low flooding rate tests, less than 1.1 inch per second flooding rate, is it correct that the lowest temperature, initial temperature for these tests was approximately 1600 degrees?

MR. MOORE: The lowest was at 1580.

CHAIRMAN JENSCH: Have you finished?

MR. MOORE: Yes.

CHAIRMAN JENSCH: Thank you.

MR. FORD: So that referring to our previous discussion of how you would choose an initial temperature parameter with the point of view in mind of aggravating the negative heat transfer problem, we indicated that it was your suggestion that what we would do would be to choose something somewhat lower than the mean in the lower part of the spectrum.

Is it correct that in the low flooding rate tests reported on page 3-6 here, of WCAP 7665, that in point of fact, you did not choose temperatures off the mean for these tests, but the lowest temperature was only 20 degrees lower than the mean?

MR. MOORE: Of the test you have picked out, yes.

MR. FORD: Of the low flooding range test with flooding rates of less than 1.1 inch per second?

MR. MOORE: Yes.

MR. FORD: I will add that all of the time.

MR. MOORE: Thank you.

MR. FORD: So that in terms of your own criteria, these particular 13 tests were therefore not set up in such a way as to aggravate the negative heat transfer problem?

MR. MOORE: Yes. They were all done at low flooding rates.

MR. FORD: In terms of the further time of initial temperature, that a low initial temperature would further aggravate the formation of superheated steam was not chosen?

MR. MOORE: The lower temperature does not further aggravate superheated steam. I don't get more superheated steam because of a lower temperature.

MR. FORD: It further aggravates the negative heat transfer from superheated steam?

MR. MOORE: Yes. That is the direction I feel the temperature effect would have.

MR. FORD: Is it correct that the formation of superheated steam would increase as the pressure in the system decreases?

MR. MOORE: All of the things being equal, I guess I would agree with that.

MR. FORD: Is it correct that in terms of the pressure range of the FLECHT tests, that none of the low flooding rate tests with flooding rates less than one inch per second had low pressures? That is pressures in the low range of FLECHT test pressures.

MR. MOORE: That's correct.

MR. FORD: Is it correct that a lower peak power density would reduce the temperature difference between the higher axial elevations and midplane?

MR. MOORE: Which lower peak power density?

MR. FORD: The power densities for the FLECHT test. As you indicated earlier when we discussed the relationship of power midplane in relation to the mean versus power as the eight to ten-foot elevation, that there is a factor of two in power density in the mean as compared to power density of the higher elevation.

What I am asking is, if we change the distribution power density in the rod, would we be able -- if we changed it, of course, in the proper direction, would we be able to reduce the maximum clad difference between the midplane and every other axial level?

MR. MOORE: What's your definition of reducing in a proper direction?

MR. FORD: Reducing in the direction that reduced the cladding temperature the difference between the midplane and the higher level.

MR. MOORE: I can only do that by reducing the midplane, also.

MR. FORD: What I am talking about is in terms of the curve that you have given for power density as a function of axial location. If we wanted, can we do something to flatten out this curve, to lower the relative power densities of the midplane and the eight to ten-foot elevation?

MR. MOORE: Yes. But that is obtained by lowering the peak. I don't raise the elevation. I don't do that, no. You have to lower the peak, since the peak average is never higher in value that is assumed for the test.

MR. FORD: But nevertheless you could lower the peak and make the --

MR. MOORE: By lowering the peak, yes.

MR. FORD: Was this done in any of the tests with flooding rates less than 1.1 inches per second?

MR. MOORE: No, because we are so far away from any temperature limit.

MR. FORD: Does the inlet temperature of the coolant influence the direction, the formation of superheated steam?

MR. MOORE: As a function of pressure, yes.

MR. FORD: Is it correct that the coolant inlet temperature in the FLECHT tests was in the range of 70 degrees Fahrenheit to 270 degrees Fahrenheit?

MR. MOORE: The coolant temperature, you say?

MR. FORD: Yes, the coolant inlet temperature.

MR. MOORE: No. I have temperature indicated as inlet subcoolant. Are you speaking of inlet subcoolant?

MR. FORD: I believe that's the same thing.

MR. MOORE: No, it is not.

MR. FORD: It isn't?

MR. MOORE: No.

MR. FORD: Let me see. On the FLECHT tests of parameters, they have the parameter entitled, "Coolant Inlet Temperature." It has the same values as what is called here "Inlet Subcoolant."

MR. MOORE: They are mistaken.

MR. FORD: For the parameter called, "Inlet Subcoolant," does this have a range of 70 degrees to 270 degrees Fahrenheit?

MR.MOORE: Not according to Table 3-1.

MR. FORD: As I look at 3-1, it goes to much lower temperatures than to around 16 or 17; is that correct?

MR. MOORE: Yes.

MR. FORD: Would variations in this inlet subcoolant influence the formation of superheated steam?

MR. MOORE: Yes.

MR. FORD: In the tests which were run with the flooding rate of less than an inch per second, was this inlet subcooling temperature varied over the range of all the FLECHT tests, inlet subcooling temperatures?

MR. MOORE: No, because inlet subcooling was determined to have a very small over-all effect on heat transfer temperatures.

MR. FORD: Was it determined experimentally?

MR. MOORE: Yes.

MR. FORD: In these very experiments?

MR. MOORE: Yes.

MR. FORD: Was it determined in a situation in which you had a low flooding rate?

MR. MOORE: Two inches a second was plotted. I'm not sure what other data was obtained.

CHAIRMAN JENSCH: The record doesn't show anything for 1.1 inch flooding rate; is that correct?

MR. MOORE: Yes, that's correct, for the figure that we plotted up, which shows a sensitivity to inlet temperature.

MR. FORD: In terms of the kind of testing that could be done varying the initial temperatures? Varying pressure of the system and so forth, can you state that the FLECHT test of these low flooding rates of less than 1.1 inch per second, can you say that they have done all that can be done simply with the variations of those parameters to determine nd

the sensitivity of negative heat transfer phenomena to the other influential variables in heat transfer?

MR. MOORE: All that can be done?

MR. FORD: I mean are there a number of obvious things that can be done with those parameter combinations that we have been discussing for the less than 1.1 inch flooding rate test. Are there a number of things that could be done, like extending the temperature range to the lower region which we are talking about, like putting the pressure from its highest constant point throughout those tests to a lower pressure? Can all those things be done to determine, as additional evidence pertaining to the role of negative heat transfer in loss of coolant accident?

MR. MOORE: Yes. We had observed any difficulty with respect to reverse heat transfer.

MR. FORD: Now, for the moment can you tell me in terms of the computer code that you use, whether the computer codes that predict the cladding temperature at seven axial levels, whether if we put into those computer codes low flooding rates, low pressure, so forth, the conditions that aggravate negative heat transfer occurrence, can you tell me if we put those assumptions into the code would they predict negative heat transfer?

MR. MOORE: Yes, they would show the effective heat transfer at the various elevations, completely consistent with the FLECHT test which also showed at certain parts of the transient reverse heat transfer.

MR. FORD: Now, do the codes that you use have as a variable the temperature of the coolant?

MR. MOORE: As I said before, no.

MR. FORD: So that in terms of their explicit simulation we realize that the negative heat transfer results because the temperature of the coolant is higher than the temperature of the cladding, but the codes themselves in their mathematical logic, it's not perfectly isomorphic with our view of the thing, is that correct?

MR. MOORE: No. That's not correct. The code will, using the FLECHT data consistently and directly compute the heat release from any elevation, if you are using the derived heat transfer coefficients for elevation. That's what I started out some time ago this morning on. That if I apply my correlation in a consistent way I will get the consistent heat release.

MR. FORD: What I am really talking about is sort of the directness or the indirectness with which the codes do this, whether they predict negative heat transfer under certain conditions, because they continuously simulate the physical phenomena that contribute to this, whether they have a variable that is local coolant temperature, whether they have a variable which is the change in the coolant temperature as it goes up the channel, whether they have a variable that's the velocity by which the coolant is going up the channel and so forth.

Do they explicitly simulate all of these different phenomena, and then as a result in the synthesis of this simulation predict the negative heat transfer coefficient?

MR. MOORE: No.

MR. FORD: Now is it correct that simply conceptually that if we postulated a high enough superheat temperature at a high axial level, is it correct then conceptually with that postulation that we could bring about a higher cladding temperature at that higher axial level that actually obtained at the same time with the same assumption that the core mid-plane so-called hot spot?

MR. MOORE: I don't follow that. Do you want to try again?

MR. FORD: Well, let me try again. I want to ask you what the implications are of various postulates we can make about the temperature of the superheat steam. Now we recognize, of course, that it's the temperature of the steam relative to the temperature of the cladding that determines whether or not there will be this reverse heat transfer.

MR. MOORE: Yes.

MR. FORD: My question is for a sufficiently high temperature of the superheat steam could we in effect produce a cladding temperature at this 10-foot elevation that is higher than the cladding temperature at the so-called hot spot at the midplane?

MR. MOORE: I doubt it even conceptually.

MR. FORD: You mean if I postulated some superheat temperature for the steam of 5000 degrees, is it possible that the heat transfer from that into the cladding at the 10-foot level would produce a higher cladding temperature there than at the midplane?

MR. MOORE: Yes. But let's not be confused here. The main effect of having higher temperatures, superheat temperatures in the steam, is not so much that you are getting heat from the steam. I hope you are not hung up on that, because the main effect is that you are not transferring heat from the clad to the steam.

MR. FORD: Well --

MR. MOORE: And so the temperature does rise. I just wanted to get it in perspective though. I am continually putting heat into the cladding, you understand that. So if the sink temperature is higher then I will not transfer heat from the cladding until the cladding temperature gets higher than that. So if I have 5000 degrees steam temperature, the cladding, before it will transfer any heat will heat up to 5000 degrees.

MR. FORD: Yes. But now the negative heat transfer coefficient means that it's going just the other way around, namely that the steam is a good bit higher or higher in general than the cladding, and the heat is being transferred the other way. That's the meaning of negative heat transfer coefficient or of reverse heat transfer.

MR. MOORE: Yes, reverse heat transfer.

MR. FORD: So it is correct then that for a given 10-foot elevation initial cladding temperature there is a temperature of the superheated steam such that it could raise the initial 10-foot cladding to a point higher than the hot spot?

MR. MOORE: Not under loss of coolant conditions.

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MR. FORD: Not under loss of coolant conditions as you have been able to analyze them for us.

MR. MOORE: Not under loss of coolant conditions. I didn't qualify it.

MR. FORD: Well, that's the hypothesis now.

Now let's discuss steam. Have you performed calculations with your computer code that give us a nice sharp upperbound that tells us the temperature of the superheated steam at which we woul really have serious problems at this higher level?

MR. MOORE: No, I don't rely on codes. I have got experimental data that indicate I don't have a problem.

MR. FORD: I see. Now I am talking about the fact that we may want to use the analytical models in situations not covered by the experiments. For example, the experiments simply had superheated steam, is that correct. It had whatever temperature it had.

MR. MOORE: For the correct conditions.

MR. FORD: For the direct conditions.

MR. MOORE: Correct conditions.

MR. FORD: For the conditions that were used in the test. Can you tell me am I correct in recalling from our previous discussion that you did not actually measure the temperature of the superheat steam, is that correct, in the FLECHT tests?

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MR. MOORE: That's my understanding, yes, sir.

MR. FORD: So that that FLECHT test data, am I correct because the superheated steam's temperature wasn't measured it doesn't tell me really what the bounds are for this superheated steam parameter relative to real negative heat transfer problems. It doesn't tell me that the threshold defining the onset of a substantial problem for negative heat transfer is such-and-such a value of the steam temperature, is that correct?

MR. MOORE: That's fairly involved, but I believe the answer is yes. What the FLECHT is telling you is the amount of heat that is transferred from the rod, which is of course what is of interest.

MR. FORD: Now I am concerned with, I believe, what nuclear engineers call scoping calculations. You do a calculation, you say, for example, there are a number of unknowns here. Our question is, for example, if a small amount of the core melts and drops into water what is the maximum steam explosion that that could create per pound, say, of zirconium that is melted. So that they haven't performed experiments with large pounds of zirconium, and so forth. So they say, "Well, let's do a calculation and let's get the T.N.T. equivalent of all this." That kind of scoping calculation.

Now I am concerned with in terms of applying this kind of calculation for hypothetical situations, such as the melt-down, I am concerned with applying that same kind, or finding out whether you made the same kind of calculation here, whether you studied the question, for example, in terms of your calculations, whether you have calculated the superheat steam temperature at which the negative heat transfer problem would be such as to move the core hot spot from the midplane to the higher elevation.

MR. MOORE: No.

MR. FORD: Is it a correct interpretation of the reverse heat transfer observed in the FLECHT tests that instead of heat being added to the coolant all the way along, as it goes up the channel, that at the point where reverse heat transfer is taking place that heat is being transferred to the rod rather than vice versa?

MR. MOORE: At the ends of the bundles.

MR. FORD: Can you tell me in terms of the estimating procedures used for heat transfer coefficients, can you tell me what, for the physical variables here, can you tell me what measurement uncertainties were involved in the parameters that you actually measured versus, say, the temperature of the coolant, which you assume to be at a given pressure.

MR. MOORE: The thermocouple measurements, I believe, were accurate to better than about six or seven degrees Fahrenheit.

MR. FORD: And the measure of axial power level?

MR. MOORE: I don't recall that number specifically. The overall accuracies on heat transfer quoted are in the range of plus or minus ten per cent.

MR. FORD: Now, when you take, for example, the mid-plane heat transfer coefficient from FLECHT data and to use in your computer code for evaluating the heat-up in a loss of coolant accident, which particular FLECHT data do you go to?

MR. MOORE: Directly to the heat transfer data as derived from the DATAR code, starting from the rod A.

MR. FORD: There are a number of FLECHT runs under a number of different initial conditions and pressure and inlet coolant temperature and so forth. How do you pick the specific heat transfer coefficients that you are going to use?

MR. MOORE: It's a function of primarily the flooding rate.

MR. FORD: I see. Now is it correct that heat transfer coefficients are insensitive to all of the other parameters?

MR. MOORE: Some more than others, but we use flooding rate, inlet temperature and temperature of the clad.

MR. FORD: That's to calculate. I am talking about the fact that these are calculated from tests run at different inlet temperatures, different pressures and so forth. Now, when you want to find a relevant heat transfer coefficient to use in your accident analysis, how do you take all these different considerations into account? I mean, isn't it the case that the pressure inside the reactor, whatever it turns out to be, that it's somewhere in the range of the FLECHT test data? You know, that the inlet coolant temperature is somewhere in the range of the FLECHT test data? How do you know, because the data was evolved under all these different conditions -- not all, there is only one condition in the reactor. There are various different conditions in the test. How do you pick among the various FLECHT data the right heat transfer coefficient?

MR. MOORE: You interpolate the conditions.

MR. FORD: Can you describe a little more fully what that means?

MR. MOORE: If I have a flooding rate of 1½ inches a second I have data for 1 inch a second and 2 inches a second, I interpolate the heat transfer coefficient in between the two data points.

MR. FORD: Now, the thing that I am interested in is that that's all very transient with regard to flooding rate, but when we look at the FLECHT data there are FLECHT tests that have the same flooding rate or similar flooding rate, but the cladding was at a different temperature, the pressure was different in the test. So, all right. Now, that you have narrowed it down that you choose FLECHT data with roughly the same flooding rate as what you are predicting in the reactor, now how do you go from there to choose among all the other parameter ranges? Which specific tests with which parameter combination are you going to take the heat transfer coefficient from?

MR. MOORE: There are two methods. I can search for the data directly, if we are close to those conditions. You find the most closely appropriate set of conditions, or I can use the correlation that was derived as part of the FLECHT test, either way.

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MR. FORD: I am concerned about the mechanics of this. The code that calculates the flooding rate, what is its name?

MR. MOORE: It doesn't have an acronym name.

MR. FORD: Would you like to coin one?

MR. MOORE: Not today.

MR. FORD: I will call it Code 1. It isn't the first code, though, you used.

MR. MOORE: No.

MR. FORD: Is it in the sequence some place that could help us?

MR. MOORE: The flooding rate code?

MR. FORD: Yes.

MR. MOORE: It's the code that's used after blowdown during the reflood, so it's used after the SATAN code.

MR. FORD: Post-SATAN code.

Now in terms of the way the codes are nested together, one following the other, receiving input from the other, contributing output to the next, the post-SATAN code, it predicts what the flooding rate is. And now you tell me from this you go to the FLECHT data as summed up in the DATAR code, is that correct?

MR. MOORE: As predicted, as determined by the DATAR code. It's a lot of print-out.

MR. FORD: Now what data output from the post-SATAN

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code is handed to the DATAR code? Does it simply say, "Well, here is the flooding rate. Now you tell me what the heat transfer coefficients are going to be?"

MR. MOORE: DATAR is merely the code that was used to process the data to derive heat transfer coefficients.

MR. FORD: I see. So that it is not a process of plugging this code into a further code.

MR. MOORE: No.

MR. FORD: I see. Is it a process of now incorporating into this code heat transfer coefficients?

MR. MOORE: The code you are describing now merely determines flooding rate.

MR. FORD: I see. Now, what code do we incorporate the heat transfer coefficients into?

MR. MOORE: The LOCTA code.

MR. FORD: Now LOCTA receives from the post-SATAN code. Is the only output it receives the flooding rate?

MR. MOORE: No. Flooding rate, initial temperature, pressure, and then subcooling. That's used to determine the heat transfer coefficient to be input into LOCTA.

MR. FORD: I see. Now after you have computed the flooding rate now you go to the output of the FLECT test, is this correct?

MR. MOORE: That's correct.

end

MR. FORD: Do you do this mechanically? Is it set up that it has in its programming questions that it addresses to the FLECHT data scored in it and it goes and asks its question and then goes on, or is it simply in terms of the sequence of computations? You get a flooding rate and then you go to the FLECHT data and look it up and use your criteria for suggesting heat transfer coefficients?

MR. MOORE: It is done both ways. You can do it with the raw data and go into the data itself, or you can use the correlations that were derived for each transfer co-efficients, and input parameters to those.

MR. FORD: If it is done by a machine algorithm, does the machine algorithm take the same heat transfer coefficients that the man picks? Do they both have the same criteria?

MR. MOORE: Essentially, yes.

MR. FORD: Can you explain what the precise criteria are? Obviously it goes to FLECHT tests that have more or less the same flooding rate within an interpolation, so it looks for that. It predicts initial temperature. Does it go among data with this flooding rate to tests that were run with this initial temperature?

MR. MOORE: I have difficulty in answering that question because I believe the initial temperature was not a strong influence. Each parameter is evaluated and you pick the set of FLECHT data which approximates or equals that particular condition. I have difficulty right now quantifying the effect of the initial temperature. The cooling primary effect is the flooding rate itself.

MR. FORD: Are there any figures in the FLECHT report on the sensitivity of heat transfer coefficients to initial temperature?

MR. MOORE: I believe so. I will find a reference. It is page 3-23, the figure 3-10. You can see from the bottom figure there at lower flooding rates, which are rates of interest. There is very little variation with heat transfer as a function of initial clad temperature.

MR. FORD: I'm not too pleased with reading off differences like this without any statistical analysis.

As I understand the difference here -- if you can check me in the figure, if you prefer to -- 180 seconds as the time after flood. If you compared the bottom curve for temperature of approximately 1602 degrees Fahrenheit and the top curve for temperature of 200 degrees Fahrenheit greater at that point than the accident, isn't it correct that there is a difference in heat transfer coefficients about 25 per cent, the difference between heat transfer coefficient of 20 and one of 25? Doesn't that difference seem to persist all the rest of the way after that point?

MR. MOORE: Yes, but that's well after turnaround.

The coefficients of interest are in the earlier times. That is when the peak clad temperatures obtain.

MR. FORD: Can you tell me if a statistical analysis has been performed? This is simply the comparison of three tests out of the 73 tests for their own parameter combinations. Can you tell me if a rigorous statistical analysis has been performed to indicate that there's nonsensitivity to initial clad temperature is in general the case?

MR. MOORE: I can't tell you what other individual plots were made. As an engineer, I look at that, and in my judgment, certainly the range of interest, there is no real difference between those curves.

MR. FORD: In terms of the way I look at it as a statistician, these are three curves, sample of 73 curves. Is it your view on statistics that you can simply take three curves out of 73 and conclude, without any further statistical analysis, that the relationship shown in these three curves is the relationship typical of the entire sample of 73 observations?

MR. MOORE: No. I am sure there is other data that could have been similarly plotted. It just was not published in this report.

MR. FORD: The second factor, the effect of inlet subcooling on the heat transfer coefficients.

MR. MOORE: Yes.

MR. FORD: I believe this is shown in Figure 3-15 on page 3-29. Is this correct?

MR. MOORE: Yes.

MR. FORD: In this data, is it clear to you that there is a very substantial difference after 30 seconds after flood, a very substantial difference in the heat transfer coefficient depending on what the assumed inlet cooling or subcooling temperature is?

MR. MOORE: I would argue about the substantial. As shown in the curve, there is a deviation with lower inlet subcooling showing lower heat transfer coefficients observable on the curve.

MR. FORD: When you choose heat transfer coefficients on the basis of this data coming out of the post SATAN code, do you make sure that you are using the heat transfer co-efficient as a comparable temperature in the prediction of the SATAN code and in the observation in the FLECHT test?

MR. MOORE: Yes.

MR. FORD: Can you tell me, does the post SATAN code set up any other parameters to be used when you choose heat transfer coefficients? Does it mention whether the pressure is, predict what the pressure is inside of the reactor?

MR. MOORE: You are talking about the code to calculate flooding rate?

MR. FORD: Yes.

MR. MOORE: Yes. Pressure is an input to that code, containment pressure?

MR. FORD: Containment pressure.

MR. MOORE: Yes.

MR. FORD: From that containment pressure does this post SATAN code calculate what the reactor pressure is, the pressure inside the core?

MR. MOORE: Not directly. As we indicated in testimony yesterday, the difference in pressure within the reactor and the containment is quite small.

MR. FORD: My question is, when you go now looking for a germane heat transfer coefficient, is pressure something that post SATAN code asks to be considered in the choice of heat transfer coefficient? Does it say, I have calculated all I have assumed that the pressure is such-and-such and I want you to look at the FLECHT data with that pressure?

MR. MOORE: Yes, that's considered in the development of the heat transfer data.

CHAIRMAN JENSCH: While the interrogator is putting some more work on the easel, I wonder if I could ask Mr. Moore about the similarity or the small difference between the containment pressure and the reactor pressure. Can you tell me if that condition was found in the semi-state scale test at Idaho after blowdown?

MR. MOORE: I can't speak directly to that. I believe there was a small pressure difference. The pressure in the semi-scale is slightly higher than -- well, they had no containment. It's higher than ambients. It wasn't a large difference, as I recall.

end

CHAIRMAN JENSCH: Thank you. Proceed. Excuse me.

MR. FORD: The FLECHT tests, when they set the ranges for the different parameters, they presumed the initial cladding would go from here to here and pressure at inlet subcooling would go from here to here and so forth. Is it correct that the ranges that they use, sort of the outer-bounds that might occur in accident situation?

MR. MOORE: Yes.

MR. FORD: They also intended to set what is expected to exist at the end of blowdown; is that correct?

MR. MOORE: I'd have to look at the reference. It, is out of context.

MR. FORD: The reference is immaterial.

MR. MOORE: That's an Idaho Nuclear report, not a Westinghouse report.

MR. FORD: I will frame it in other terms, if you like. The question is, was it in terms of parameter selection in the flood test? Was it the intention to have tests over the whole range of parameters, but to really concentrate tests, you know, in a midrange which is considered to be most likely conditions to exist at the end of blowdown?

MR. MOORE: I believe that was the intent, yes.

MR. FORD: Let's talk about these conditions at the end of blowdown as coming out of the code in our criteria for selecting heat transfer coefficients.

MR. MOORE: If the initial cladding temperatures and subcooling and so forth, and pressure that come out of the code, if these are typical midrange values, am I correct that we would have no trouble finding heat transfer coefficients for them because we did a lot of work on all these typical midrange values? So most of our FLECHT data is right in the form that we want it; is that correct?

MR. FORD: The result came out of the code were such that it was in the midrange of the FLECHT data?

MR. MOORE: Yes.

MR. FORD: In terms of the statistical significance of FLECHT data, is it the case of a particular range of parameters, particular parameter combination within the midrange, that in the FLECHT test there would have been a number of tests that are pretty close to that parameter combination? Let us talk about the midrange of initial cladding temperatures as 1600 degrees, the midrange of inlet subcooling as 150 degrees, and the assumed pressure, I believe, didn't really have a midrange but mostly around 60. Are these correct midranges?

MR. MOORE: What is the third one down?

MR. FORD: Inlet subcooling.

MR. MOORE: I don't recall that number specifically.

MR. FORD: Is it correct that in terms of the statistical significance, since these are the main parameter combinations and since most of the work was done, you know, within, say, a Delta of those midrange values, is it correct that the result we get for combination of 1600, 60, 150, that it's statistical significance is increased for it is consistent with the results we get for 1602, 58 and 148?

MR. MOORE: Is what statistical significance increased?

MR. FORD: If I got a particular heat transfer coefficient as a function of a 1600 degree initial clad temperature, a 60 degree pressure and a 150 degree inlet subcooling, say I got a specific heat transfer coefficient for the midrange here that is such and such a value. Let us say I did another test in which I derived a heat transfer coefficient for 1650 degrees, initial temperature of 55 pounds pressure, and of 141 degrees inlet subcooling. If I perform the first test, I get a certain heat transfer coefficient. If I perform the second test, I get another heat transfer coefficient. If I varied the parameters a little bit more still within a Delta of the initial one, I get a third. If it turns out that the three heat transfer coefficients that I get from these three close variations of the parameters, if it turns out that these three heat transfer coefficients are pretty much the same, is the statistical significance of any of them greater than if it turns out that the three of them are wildly different?

MR. MOORE: I say not necessarily.

MR. FORD: Is the whole point of using our statistical analysis, or is the general point to try and see whether if you make an estimate, if you re-estimate it with pretty much the same conditions, you would get pretty close to the same original estimate? Is that the whole idea behind statistical significance?

MR. MOORE: I thought we were discussing data points here, not predictions.

MR. FORD: We are. I am trying to move it to certain statistical problems associated with the use of the FLECHT data. I am simply trying to see whether or not you agree with me, that in the area in which you do a lot more testing, your results have a lot more statistical significance than in areas where you don't do very much testing.

MR. MOORE: I agree with that.

MR. FORD: You agree with that?

MR. MOORE: Yes.

MR. FORD: So that this means, once we start moving over beyond the midrange into areas in which we only did a small amount of tests, that the statistical significance is less than what it is in the midrange? The statistical significance of our heat transfer coefficients is less.

My question concerns what happens to the statistical significance of your computer code when, for not just one parameter, but for a couple of parameters, we move outside of the midrange of parameter values used in the FLECHT tests. Am I correct that what would happen is if we moved outside of the midrange of initial cladding temperatures, outside of the midrange of pressures, and outside the midrange of inlet sub-cooling, that what we do is, the results that we have there are one of two problems: Either there are no FLECHT test data for this parameter combination or what FLECHT test data there exists has accumulated the statistical insignificance of a statistical problem of three observations?

MR. MOORE: You have ignored one fundamental problem that statisticians occasionally have, and that is to ignore the physical significance of or the physical phenomena of the data that they are treating statistically. In the engineering profession you don't blindly take data points and play around with the statistics solely on a statistical, for a statistical effect. You look at the data that you obtain and you look at engineering judgment and does it agree or disagree with the results that you have obtained. In the case of many of these parameters the specific intent of the FLECHT program was to determine sensitivity. There was not to be expected large sensitivities in many of these parameters and such sensitivities were in fact not observed. So your theoretical argument here as to be tempered by an understanding and a judgment of the actual physical phenomena as well.

MR. FORD: Well, I appreciate the comments.

CHAIRMAN JENSCH: Excuse me. I want to get the question answered nevertheless. I think you are trying to give an explanation before you give the answer. Now if you give the answer you agree with it then you say you don't believe it should be applied, that's a good argument if you can present it.

Could you go back to the question, please.

Let me see if we can take the two instances up there. If you have the h1660-150 and you run another analysis at

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1655 and 141 and you find the heat transfer coefficients come out alike, do you feel more confirmed as to the h1660-150 in view of the fact that your next one came out alike?

MR. MOORE: Do I feel more what?

CHAIRMAN JENSCH: Convinced that it is valid. Does the second calculation confirm the validity of the first?

MR. MOORE: Not just looking at the two calculations.

MR. FORD: Well, if you do look at the two calculations, however, do you not feel convinced that the second one comes out like the first?

MR. MOORE: The postulation was that the coefficient for those two sets of conditions came out to be about the same?

CHAIRMAN JENSCH: Yes.

MR. MOORE: I conclude from that that the effects of the variations in those parameters that occurred between the two cases didn't have much of an effect. You get the same h. That is comforting. Is that the question?

CHAIRMAN JENSCH: Well, you are convinced as to the validity. I don't care about your comfort. You are convinced of the validity of the first calculation, are you not?

MR. MOORE: I don't see how I can conclude that.

MR. FORD: Mr. Chairman, I think that this particular question was answered by Mr. Moore when he indicated that in the points in the midrange we would have more experiments, that the statistical significance of each result is higher than

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the points on the different ends. The question outstanding was whether if the computer code predicted, the post-SATAN code predicted these various parameters outside of the midrange in which they were tested, the question was in two parts: first, is there some probability that you will not have tested the parameter combination because it's outside of the range in which most of the work was concentrated.

MR. BRIGGS: Could we stop right there?

CHAIRMAN JENSCH: And ask him to answer that?

MR. BRIGGS: Yes, answer the questions one at a time.

MR. FORD: Find.

MR. MOORE: Yes.

MR. FORD: Is there the second possibility that what estimates there are, their statistical quality is diminished because they have accumulated the uncertainties of three uncertain estimates?

MR. MOORE: That's a possibility.

MR. FORD: Yes. Now in terms of the ability of the computer code to use the FLECHT data am I correct that if a statistical point of view the premise has to be that the loss of coolant accident situation must be pretty well confined to the midrange, or else the statistical validity of the heat transfer coefficients you incorporate into the subsequent codes is diminished greatly.

MR. MOORE: I disagree.

MR. FORD: Can you tell me whether Westinghouse has prepared a statistical analysis of its use of FLECHT data in the codes that supports your position?

MR. MOORE: No statistical analyses have been performed.

MR. FORD: Can you tell me as a practical matter, let's say, we were running this code to do scoping calculations and we predicted initial temperatures and initial subcooling and flooding rates, which were a good bit out of the midrange of the FLECHT tests, and you wanted to go on with the scoping calculations but there were no observations in the FLECHT test under those conditions, can you tell me what would you do? This is to get heat transfer coefficient.

MR. MOORE: Yes. I would not focus on any specific combination of parameters in the midrange. I would look at the sensitivity of the heat transfer coefficient to the parameters of interest, looking at my data. Then I would determine what are the more critical parameters that affect heat transfer. I would want to be reasonably sure that of the more critical heat transfer parameters that I am in the range of interest when I do the analysis. In applying this overall review I also want to know what the sensitivity of the answer is to the coefficient and also whether assumptions have gone into the analysis.

For example, with respect to the determination of

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the flooding rate itself and it's the overall analysis we then evaluate for applicability.

MR. FORD: Now in terms of your diagram here let me try and see if I can make clear how you proceed when you are outside of the midrange. Now let's suppose that this is the, something we call the critical parameter. It has a clear influence in this case on the heat transfer coefficient. Now you have collected data in a specific range of this parameter and let's suppose that for this specific range the data corresponds very well to this curve, you know, the R^2 let's say, is a super respectable .99. Now the thing is in terms of going out of this midrange, you can continue the curve simply by direct extrapolation and you can decide that it takes off here, you can decide that it crashes here, you can decide that it reaches a plateau and so forth.

Can you tell me from a statistical point of view how you can translate statistical validity of this correlation, youw you can transfer it to whatever you decide to estimate outside of the range?

MR. MOORE: If you had been listening to my answer before I said I would determine the critical parameters and I wanted to be sure that with the critical parameters I was within the range of the data.

end

MR. FORD: I see. So that because of your implicit awareness of the statistical insignificance of bootstrap extrapolation, that you simply don't go into this matter. If you have to, you know, for a critical parameter, go outside of your range, you just wouldn't. There would be no point because if the statistical --

MR. MOORE: I'd be very cautious and then I would be involved in a more rigorous statistical evaluation, that's right.

MR. FORD: I see. So that for critical parameters am I correct that your use of the code is pretty well limited to the midrange of the FLECHT data?

MR. MOORE: No.

MR. FORD: No. Now --

MR. MOORE: I have FLECHT data for the critical parameter over a wide range.

MR. FORD: Yes. But I think our previous point was -- let's go into this point more carefully. Our previous point is, let's call this a critical parameter. This C means critical parameter rather than cladding as it was before on our chart. Let's call it a critical parameter, and we previously acknowledged that the statistical validity of points outside of the midrange was less than the statistical validity of the points in the midrange. Now are you saying that if this is a critical parameter and this is not, that for the critical parameter the relative statistical validity of points outside two points inside the midrange for critical parameters is higher than the relative statistical validity for noncritical parameters?

MR. MOORE: No. I don't think I said that. I just said we did not focus -- you indicated that the intent was to develop data around a given expected midrange. That was the intent.

MR. FORD: Right.

MR. MOORE: You failed to recognize that the tests were expanded and in the area of flooding rate, which was to be determined by tests to be the critical parameter tests were done at very low flooding rates and under conditions that very closely represent the conditions expected for the Indian Point plant.

MR. FORD: Yes. But whatever your accomplishments might have been with regard to flooding rates as a critical parameter are you saying that there are no other critical parameters and thus it wasn't necessary to have the range you did for flooding rates?

MR. MOORE: As observed in the report we did determine sensitivity to parameters such as initial clad temperature.

MR. FORD: Yes.

MR. MOORE: And found out that the results were fairly insensitive in a region of interest.

MR. FORD: I see. Now, are there any other parameters besides flooding rate which you label critical parameters?

MR. MOORE: No.

MR. FORD: Can you tell me when you use the heat transfer coefficients you have acknowledge that their uncertainty is plus or minus ten per cent, now after I have gone to the data, found the heat transfer coefficient that is suitable to the flooding rate, the initial temperature, the inlet subcooling, and maybe even the pressure that I have just predicted with my code, when I find the heat transfer coefficient that's germane to that do you just use that as it is or do you multiply it by plus or minus ten per cent?

MR. MOORE: I use it as it is.

MR. FORD: I see. Now, if you used it, if you used it on the side of the plus or minus ten per cent that makes the cladding temperature worse, makes it higher, would you be in that kind of conservative framework, conservative estimating framework, would you then take minus ten per cent of the heat transfer coefficient as the thing of use, rather than the raw coefficient?

MR. MOORE: No, because I have to consider the basis for the flooding rate, the independent variable itself, how was that calculated. That's where the conservatism is applied.

MR. FORD: Independent of the conservatism applied to the flooding rate calculation is it correct that you do not apply a conservatism to the heat transfer coefficient itself?

MR. MOORE: Not per se, that's correct.

MR. FORD: Have you performed a statistical analysis for which you took the expected values of all of your parameters, you took flooding rate and it said that it has an uncertainty of plus or minus f per cent. You took initial temperature of plus or minus f per cent and so forth. Have you performed a statistical analysis to indicate how these various uncertainties accumulate as you go through the process of calculating and calculating, and putting in parameters and putting in parameters?

MR. MOORE: I don't understand the question with respect to cumulative calculating and calculating and calculating.

MR. FORD: Let's talk about a baseball game and let's say that I make a prediction of the number of hits by the Red Sox in the first inning and I predict that the Red Sox will have five hits in the first inning. O.K. And now I go on and I predict that the Red Sox will have two hits in the second inning. And let's suppose that the game is canceled after two innings. Now, let's further suppose that

I had data on what the uncertainty of my prediction was for each prediction. I knew that my prediction of first inning -- let's say it's the same uncertainty all the time. I knew that, you know, I might be plus or minus two runs in predicting every inning. Now, if we translate my total prediction into ranges of uncertainty there could be between three and seven hits in the first inning, that could be between zero and four hits in the second inning. Now, if I neglected all of this uncertainty as it accumulates in my estimate and somebody said, "Well, what is your over-all estimate for the number of hits in both innings," well, I'd say seven. But if somebody asked me, "Well, take the uncertainty into account," I know my uncertainty, I am always off, my evidence has said, by plus or minus two hits. And somebody said to me, "Well, take your uncertainty into account and give us, you know, what is your best judgment." I know from a statistical point of view that 99 per cent of the time I am going to be right if I adjust my estimates for uncertainty. So with a confidence of 99 per cent I can say that the number of runs hit by the Red Sox in both innings will be between three and eleven. Now, is it clear to you in terms of this example how uncertainties accumulate in estimating procedures as the estimating procedures are combined?

MR. MOORE: No, it's not clear to me as to how you are applying this to the reactor situation.

MR. FORD: I am trying to just see whether we can find ground for discussion of the reactor case by asking you whether it's clear to you that the statistical validity of the estimated range is considerably higher than the statistical validity of just the estimate without uncertainty considerations.

MR. MOORE: I still fail to see the analogy. I understand your ballgame.

CHAIRMAN JENSCH: Well, I guess the question is do you understand there could be uncertainty in the ballgame as he has depicted it?

MR. MOORE: For his ballgame, yes, sir.

MR. FORD: Right. Now, in terms of the uncertainty we can even quantify it. I have indicated that because of my knowledge of the firmness of this error of plus or minus 2, because that's so high, because that's .999 I know that -- well, let's even say it's absolutely certain, that I will be within plus or minus 2. We know then that it's absolutely certain or at least .999 probable that I will be in the range, but the ballgame will end up, the score, in the range of 3 to 11 hits.

Now, once we start talking about where it will be in the range, the number 7 is one of eight numbers of possible hits that could be had so that the probability of seven hits is only 12 per cent, twelve and a half per cent, whereas the probability of it being in the range in the first place of 3 to 11 is 99 per cent.

So is it clear to you that a cumulative statistical estimate that systematically and rigorously incorporates uncertainty estimates in its procedures and in its symthesizing of all of the data, that the statistical validity of results predicted with uncertainty analysis is vastly higher than the statistical validity of results with no uncertainty analysis at all.

end

*** MR. MOORE: Isn't that a function of the The differences we are talking about, isn't that a function of the uncertainties? I mean if you had a prediction of 27 runs plus or minus 1 run my variations are not as critical *** as my differences can be as large?

MR. FORD: Well, it's a function of the uncertainty in this sense, namely, that if there were no uncertainty, you know there would be none of this analysis. But the point is once there is some amount of uncertainty then my hypothesis for which I am asking your confirmance, is it correct that once there is any amount of uncertainty from a statistical point of view the results of an analysis that rigorously analyze these uncertainties has a higher statistical validity and a statistical significance than the results of an analysis which does rigorously study the uncertainty of the different parameters?

MR. MOORE: I guess you understand that in a statistical sense that's not the approach that's been taken in these analyses. That's where I think we have a communication gap here.

MR. FORD: Well, I took to heart your suggestion earlier that abstract statistical analysis needs the perspective of the engineers, not just enough to talk about ranges of the variables without considering their statistical significance. I also hope that you will take to heart the fact that the engineers' use of data could also be informed by some statistical analysis and statistical rigor.

Applying all of this to the predicted maximum clad temperature as predicted by your codes, do I understand it correctly that when you predict 2300 degrees Fahrenheit that in the process of preparing that prediction with your code you do not step by step perform uncertainty analysis and cumulate this analysis all through the comparison such that you can associate with your final prediction a clear, well-grounded statistical statement by its uncertainty?

MR. MOORE: That's correct.

MR. FORD: Can you tell me where this figure plus or minus 10 per cent for the heat transfer coefficient came from as its uncertainty?

MR. MOORE: That was derived from an evaluation of the measurement uncertainties in the specific FLECHT tests.

MR. FORD: Can you tell me what the measure of uncertainty was?

MR. MOORE: You mean on the individual components?

MR. FORD: Well, I am looking for the mechanism by which somehow or other from the FLECHT data it was decided that FLECHT heat transfer coefficients were plus or minus 10 per cent accurate?

MR. MOORE: By the tolerance or the measurement uncertainty of the thermocouples and the power input, which

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formed the two specific parameters that were measured to get heat transfer.

MR. FORD: I see. Now, can you point out to me where this statistical analysis is presented in the FLECHT reports?

MR. MOORE: No.

MR. FORD: Do you know that it is presented in the FLECHT reports?

CHAIRMAN JENSCH: The witness has been on the stand for a considerable time. He is looking up something in the record. Maybe this would be a good time during the noon hour to give him a chance to get rested in the meantime.

Is this a convenient place to interrupt the examination?

MR. FORD: I have one final question for a moment. on this plus or minus 10 per cent uncertainty.

CHAIRMAN JENSCH: Proceed.

MR. FORD: Is it correct that if we computed the maximum clad temperature over the range of uncertainty of plus or minus 10 per cent for the FLECHT heat transfer coefficients that we would compute a range of maximum cladding temperatures, half of which exceed the interim criteria of 2300 degrees Fahrenheit?

MR. MOORE: No. That's a misapplication of the statement on the 10 per cent error. What you don't understand, it is indicated in the information we have given, is that you asked me the uncertainty in the FLECHT measurement, measured heat transfer.

I indicated based on the measurement accuracy it's plus or minus 10 per cent. There are several beneficial effects on heat transfer that are completely ignored with respect to FLECHT results, and furthermore there is the other aspect which we have not explored, which is that we calculate a flooding rate, which is the primary variable of interest, in a manner which ensures that the flooding rate is less than expected by the assumptions made for uncertainties in the flooding rate. So you just can't take a plus or minus, a stated plus or minus 10 per cent accuracy for a specific FLECHT test.

You must understand, we did not include in doing the analysis the effects of fallback, which were indicated in the FLECHT report as beneficial. We did not include the effects of having borated coolant, which had some beneficial effect, as indicated in the FLECHT report. And also the FLECHT tests were done without mixing vane grids and the effect of mixing vane grids which exists in the Indian Point Plant will be to improve or enhance reflood heat transfer. The total effect of those three is estimated to be at least 50 to 60 per cent increase in heat transfer during reflooding. So I want to make sure that the plus or minus 10 per cent is put in the proper ·erspective.

MR. FORD: Well, I am sure that when you give us the precise references that indicate how this plus or minus 10 per cent was arrived at that we will be in a much better position to determine whether or not your analysis is correct.

CHAIRMAN JENSCH: And do I understand that he has some references to the 50 or 60 per cent benefits in the FLECHT reports too?

MR. MOORE: There are references to two of the three benefits, yes.

CHAIRMAN JENSCH: At this time let us recess, reconvene in this room this afternoon at two o'clock.

(Luncheon recess.)

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

CHAIRMAN JENSCH: Please come to order.

Is Witness Moore available?

MR. TROSTEN: He is temporarily unavailable, Mr. Chairman. He will be right back.

CHAIRMAN JENSCH: Very well.

Witness Moore has resumed the stand, but before proceeding with interrogation by the Intervenors I wonder if I could check my notes and see if I have a correct impression of your last answer to the last question. I wondered if it was a "Yes, but" type of answer.

As I recall it, the question was, "If you computed the maximum clad temperatures in with these other components of I believe pressure and inlet coolant and midrange of cladding temperature, that half of the calculations would exceed 2300 degrees."

And as I understood your answer you said that that was a misapplication because you wanted to have credit for 50 to 60 per cent of some items, such as borate coolant and mixing vane grids and fallback. And I wondered if I could ask a clarification of your answer. Did you agree that the statistical analysis was correct but you wanted to have the benefit of these other credits, is that correct?

MR. MOORE: No. I didn't agree to any correctness of any statistical analysis. What Mr. Ford asked was, as I

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remember the question, "If you used a heat transfer co-efficient which was ten per cent lower in calculating peak clad temperature would the temperature be higher than 2300 degrees Fahrenheit? And all other things being constant it's obvious that if I input a lower heat transfer coefficient in the analysis the temperature will go up."

So the answer is yes. But certainly a question that is not in context at all.

CHAIRMAN JENSCH: Yes. For the reasons that you indicated.

MR. MOORE: Yes.

CHAIRMAN JENSCH: Thank you very much.

Will you proceed, Intervenor.

MR. FORD: Mr. Moore, you have indicated that there are assumptions which you do not make that if they were made they would reduce the maximum clad temperature. For the purpose of this record, the assumptions that you have listed, do you or do you not make these assumptions in calculating the maximum clad temperature during the loss of coolant accident for Indian Point 2?

MR. MOORE: The assumptions that I referenced earlier?

MR. FORD: Yes.

MR. MOORE: They are not inclided in the analysis.

MR. FORD: Can you differentiate of the assumptions that you indicated would reduce max clad temperature, can you

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differentiate for us between those which you want to make with which the Atomic Energy Commission will not allow you to make under the interim policy statement and those assumptions which you want to make which for some other reason are not made?

MR. MOORE: We are specifically referring to assumptions on FLECHT heat transfer at this point?

MR. FORD: Well, I am referring to the list of assumptions which Mr. Jensch was referring to, namely the assumptions that you feel in some sense could be made, and if they were made would reduce the calculated max clad temperature but which nevertheless have not been made.

Perhaps you could clarify matters by simply listing all of the assumptions which you feel could be made but were not made and which have this influence on the max clad temperature.

MR. MOORE: Yes. The three specific ones I mentioned were in order of effect mixing grids. Second was the effect of borated coolant and the third small effect in this case is of fallback.

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CHAIRMAN JENSCH: What is a fallback?

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MR. MOORE: That's the phenomena where the entrained water -- the exit of the core hits the core support structures and falls back into the core and provides additional flooding mechanism in the heat transfer.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Can you tell me, these three assumptions that are not made in calculating the mass clad temperature for Indian Point 2, are they not made because the Atomic Energy Commission will not accept them under the interim policy statement?

MR. MOORE: No.

MR. FORD: Are they not made because sufficient data has not been evolved to justify these assumptions?

MR. MOORE: In the case of the mixing vane grid, that's correct, to quantify the improvement.

MR. FORD: The effect of the borated coolant, can you explain why this is not considered in your calculation?

MR. MOORE: It is just a conservatism in the calculation.

MR. FORD: Have you performed a sensitivity analysis which indicates the magnitude of conservatism here?

MR. MOORE: It is indicated in the FLECHT report, comparison of heat transfer.

MR. FORD: In terms of after you have taken it from the FLECHT report and incorporated it in the different codes and gone through the calculation, can you tell me if the calculated temperature of 2300 degrees would be increased or decreased if you considered the effect of borated coolant?

MR. MOORE: If I considered the effect of borated coolant, would the peak temperature be increased or decreased?

MR. FORD: Yes, if you took credit for it.

MR. MOORE: The peak temperature would be decreased.

MR. FORD: Can you tell me how much it would be decreased by?

MR. MOORE: Not offhand, no.

MR. FORD: In any of the analyses that you submitted to the Atomic Energy Commission on emergency core cooling to assist them in their formulation of the interim policy criteria, did you supply them with calculations in support of the assumption that the boration of the coolant allows you to take credit for a lower calculated temperature?

MR. TROSTEN: Mr. Chairman, I object to that question.

CHAIRMAN JENSCH: Why?

MR. TROSTEN: I don't see that the matter of what Westinghouse Electric Corporation told the Atomic Energy Commission in connection with the formulation of the Commission's interim acceptance criteria is relevant to the line of questioning being directed to this witness.

CHAIRMAN JENSCH: You say you don't see any relevancy?

MR. TROSTEN: No, sir.

CHAIRMAN JENSCH: Therefore, you say it is irrelevant?

MR. TROSTEN: Yes, I object to the question as being irrelevant.

CHAIRMAN JENSCH: Can you establish your position a little more than that?

MR. TROSTEN: I will try, Mr. Chairman. I believe that by stating that I feel the question is irrelevant, I think, sir, it is incumbent upon Mr. Ford -- and stating it as I already have -- it is now incumbent on Mr. Ford to show that it is relevant. I will be glad to amplify.

CHAIRMAN JENSCH: I don't want you to proceed on an improper premise. Because somebody jumps up and says it is irrelevant, even the statement of irrelevancy may be not proper. So proceed with your statement.

MR. TROSTEN: I will proceed.

I feel that there is no issue, it seems to me, here, sir, as to what has Westinghouse Electric Corporation told the Atomic Energy Commission. That isn't one of the issues to be determined in this proceeding. I think that a line of questioning that is addressed to the question of what has Westinghouse Electric Corporation told the Atomic Energy Commission is irrelevant.

That is the reason why I am objecting to this question.

CHAIRMAN JENSCH: I think your statement brings into issue a matter that we probably should discuss. That is the scope of the interim criteria in emergency core cooling.

Do those criteria exclude these matters, such as the borated water and mixing vane grid, and water hitting the support grids and getting more water for coolant?

MR. TROSTEN: I didn't interpret the question as being directed to the question of what does the interim acceptance criteria say. If I had intrpreted Mr. Ford's question relating to the issue of what do the interim acceptance criteria say, I wouldn't have objected, Mr. Chairman.

CHAIRMAN JENSCH: If you will just discuss it with me, if you will, regardless of what the question is, because I think your statement brings in an issue of the question of the criteria. Do you believe that the interim policy statement that emergency core cooling systems exclude credit for mixing vane grids, borated coolant and fallback of the water on the support grids?

MR. TROSTEN: I'd have to go back and reread carefully Part 3 of the criteria in order to answer your question. I am not able to answer it at this time. I would just have to go back and restudy Part 3.

CHAIRMAN JENSCH: That's my problem. It is not necessarily excluded. It may be a factor in the judgments that were made to the recommendation made by the Staff concerning this matter. If it is excluded, definitely, then I think the objection is well taken and likewise these credits that the witness is talking about should be excluded in consideration. So it is either one or the other.

MR. TROSTEN: Mr. Ford had explained to me, in response to my objection, that what he was trying to show was that he was trying to decide what paragraph 7 of this section meant. Perhaps I would have understood his point. I don't know the answer to your question, Mr. Chairman. I'd have to go back and look at it.

CHAIRMAN JENSCH: Will you do that and discuss it later?

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: We will overrule the objection for the time being. The witness may answer.

MR. MOORE: No.

MR. FORD: I haven't adjusted myself to listening to your tone of voice after listening to Mr. Trosten's.

MR. MOORE: The answer was no.

MR. FORD: Is there anywhere among the documents described in your code calculations or in any of your analytical reports and so forth, is there anywhere the sensitivity analysis for the assumptions of borated coolant and in relation to maximum clad temperature calculations presented?

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MR. MOORE: No. It is just the comparison of heat transfer coefficient in the FLECHT report.

MR. FORD: We discussed the various sources of uncertainty in the procedure relied upon to come up with the max clad temperature of 2300 degrees Fahrenheit. One second, please.

MR. ROISMAN: Mr. Moore, I have a question on this previous set of considerations regarding the three factors that you said would reduce the maximum clad temperature that we are not taking into account. Were those factors capable of occurring in the FLECHT tests? In other words, is the design of the FLECHT tests such that those particular factors would have been present if they occur at all?

MR. MOORE: Two of the three directly, yes.

MR. ROISMAN: Which two are those?

MR. MOORE: The borated water and the fallback.

MR. ROISMAN: How did you exclude their effect on maximum clad temperatures for FLECHT in determining what should be the heat transfer coefficients?

MR. MOORE: We did not use the data obtained for borated water or the data obtained under the test-up which simulated fallback. Neither of those data was used in the calculation of peak temperature.

MR. ROISMAN: In other words, you ran a couple of experiments, a number which isn't important at this point, where you had borated water in them. And you have not in any way included the results of the borated water experiments in coming up with the heat transfer coefficients from the FLECHT data, is that correct?

MR. MOORE: That's correct.

MR. ROISMAN: And equally true of the mechanism that would simulate fallback, not every test had that mechanism in it, and the tests that did have the mechanism of fallback in it, all the results of those tests were excluded.

MR. MOORE: That's correct.

MR. ROISMAN: Will it be clear if we look at the FLECHT tests, will it show us which set of test results were ones in which the borated water was used or the fallback mechanism was used?

MR. MOORE: Yes, I believe so.

MR. ROISMAN: Is that on there? Is it Table 3.1 that lists all the various parameters?

MR. MOORE: Yes. Fallback is indicated in the remarks column.

MR. ROISMAN: Yes. I see that now. On the table. What about the first effect of mixing grids? Were tests run in FLECHT which had that effect in them?

MR. MOORE: Not directly. The flow blockage tests were interpreted with respect to expected performance of mixing vane grids. Mixing vane grids were not directly simulated in FLECHT.

MR. FORD: As I study Table 3.2 of FLECHT data

SUMMARY OF STAINLESS STEEL CLAD VARIABLE FLOW TESTS, almost all of these, it's indicated in the remarks, this is page 3-7 of WCAP 7665 -- all but one of the items, two of the items on this table, have fallback. Am I to understand that these tests which were the variable flow tests, that you used no heat transfer coefficients from these tests?

MR. MOORE: That's correct.

MR. FORD: Am I to understand then that the heat transfer coefficients you used derive from tests that have constant flooding rates?

MR. MOORE: Yes.

MR. FORD: Now, to refer here with this additional complication back to our original discussion of how the post-SATAN code predicted what flow, what the flooding rate was, and from whence you picked a heat transfer coefficient, is the output of this code a single flooding rate or is it a variable flooding rate?

MR. MOORE: It's a variable flooding rate.

MR. FORD: So that do I understand it correctly that you take the variable flooding rate from the post-SATAN code and when you go to the FLECHT data you don't go to the variable flooding rate FLECHT data but to the constant flooding rate FLECHT data.

MR. MOORE: That's correct.

MR. FORD: Can you tell me what statistical analysis you have performed on the sensitivity of maximum clad temperature quench time and so forth in the FLECHT tests to the use of constant versus variable flooding rate?

MR. MOORE: No. The constant -- you are perhaps misinterpreting the use of the information. The constant flooding rate is used in a manner that you can effectively obtain the effects of variable flooding rate. This is described in the June 1, 1971 report.

MR. FORD: Do we have that report, Mr. Moore, do you know?

MR. MOORE: Yes, you do.

MR. FORD: Is that one of the proprietary documents?

MR. MOORE: Yes, it is.

MR. FORD: Now, in terms of taking the prediction of variable flooding rate that comes out of the post-SATAN code and putting together heat transfer coefficients from different constant flooding rate tests, can you tell me what you do with the parameters of these different tests? Are they simply disregarded and just talk about it in mean terms, about the, you know, over a range of different pressures and initial temperatures and inlet cooling temperatures? You simply talk about what the mean heat transfer coefficient is at a given flooding rate, at a given point in time?

MR. MOORE: No. As I understand the question, we, in using constant flooding rate information, we use that constant flooding rate information that is closest to the actual conditions for the specific calculation.

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MR. FORD: Yes. But if the variable flooding rate predicted by the code, for example, predicted nine inches per second for a certain number of seconds and 6.7 inches per second for a certain number of seconds and then one inch per second for a certain number of seconds, do you go to FLECHT data that had nine-inch per second flooding rate and the specific pressure and inlet temperature that are predicted by the code that predicts the variable flooding rate and use that specific data?

MR. MOORE: We take into account the effect of the coolant flow that would have come in during the transient as a function of the flooding rate during transient. This correction or correlation is described in the 6-1 report I referenced earlier.

MR. FORD: Can you tell me precisely where it's described in that 6-1-71 report?

MR. MOORE: Page 58.

MR. FORD: Now I presume that you are referring to the equation on the bottom of Page 58, is that correct?

MR. MOORE: That's correct.

MR. FORD: Now does that equation specify the other parameters of the different FLECHT tests besides its flooding rate and heat transfer coefficient?

MR. MOORE: No. At this point this equation is merely reflecting the total coolant inlet flow, the actual

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look at a certain amount of time on the FLECHT data."

So that code says, "Well, this is the point in time that we should have as the time of reference," and then we can find the heat transfer coefficient responding to the time in the data?

MR. MOORE: That's correct.

MR. FORD: What I am concerned with is that this particular FLECHT bundle was run under certain conditions. It had an inlet coolant temperature condition. It had a pressure condition. It had an initial cladding condition and so forth. And of course it also had flooding rate, a constant flooding rate. We are not using variable flooding rate, FLECHT test data. What I don't understand, and it isn't covered in the equation on Page 58, is how--all right. Once you have gone to the FLECHT data at the time specified by that equation, well how you then combine--all right. At that same time does the heat transfer coefficient, does the heat transfer coefficient, and there are any number of other heat transfer coefficients at that time after reflood, can you tell me how the algorithm presented on Page 59 tells us how to put all these together? All I can see that algorithm is telling me is a point of time on the standard FLECHT test scale.

MR. MOORE: Yes. As I indicated the main effect on heat transfer is the flooding rate itself. The parameters you are talking about are the parameters such as pressure, inlet mass of water in the core.

MR. FORD: Now can you tell me, the equation I am looking for is the one that tells me how you take the heat transfer coefficients from tests with different flooding rates and use them in a situation that has a certain pressure and a certain cooling temperature without in some way, some statistical way, incorporation these parameters of the FLECHT test heat transfer coefficients.

MR. MOORE: This relationship defined on Page 58 determines where in time you go for the heat transfer coefficient for any given flooding rate. As you observe in the FLECHT curves, flooding rate varies with time. With a constant flooding rate you get variable heat transfer. This correlation tells you where to go for the heat transfer at a given point in time and that then we incorporate the pressure-temperature effects. These two figures that I have drawn here, I'd like these to represent the form of the FLECHT data.

FLECHT tells you for different times after the beginning of reflooding what the heat transfer coefficient is, and I represent here two hypothetical heat bundles which have different heat transfer coefficients as a function of time. Do I interpret the equation on Page 59 correctly in that what it tells us is it says, "Now we want a heat transfer coefficient. We want it for a certain flooding rate, variable flooding rate, which means that with that amount of flood behind us we should

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temperature, clad temperature, which we observed in the FLECHT results weren't significant effects on flooding rate when we saw the curve, for example, for a one-inch-a-second flooding rate. And you could lay in this region the curves, one on top of another, and then later on intended to diverge.

So the first point is that the parameters that we are discussing here that may change the heat transfer coefficient do not change it significantly. The approach to use would be to take a curve for a--if I am looking for a flooding rate of one inch a second, I have a curve for constant flood rate of heat transfer at a given temperature-pressure and clad temperature, I would pick the curve which most nearly represented the conditions in the reactor.

But the initial clad temperature at this point in time is 1650 degrees and I have a run at 1620, I take a run at 1620 degrees. The results show that a run for a higher or lower temperature really isn't much different. I picked the one closest to it similarly for pressure.

And the correlation says that the heat transfer at any given flooding rate is a function of how much water you put in the core and that is what this time shift is doing. It's saying, "I'm equivalent to that point in time of this flooding rate because I have put a certain amount of water."

You can observe the effects of this by taking a different curve of heat transfer versus time at the same flooding rate, at 2,000 degrees. If you go into this correlation, if you go into the data then at 2,000 degrees, you will find very little difference in heat transfer coefficients because that was not a significant parameter.

A second test of this would be to use the correlation of the data which attempted to empirically include all these parameters even though they weren't significant and go directly into that and generate through the correlation a constant flooding rate heat transfer. Versus time and then go into that curve.

MR. FORD: It is fairly clear to me what the options are. There are a variety of things like this that we can do to get the right heat transfer coefficient. For the record, the first thing I am concerned with is exactly which of the various methods do you use for the calculations for Indian Point?

MR. MOORE: The method used for Indian Point 2 was to go into the data for conditions that most closely represent the conditions predicted for the reactor. So we will go into the temperature pressure conditions data set which mostly are represented as Indian Point.

MR. FORD: The alternative of relying on the general correlation analysis for all of the FLECHT test data, that is the main alternative, and that isn't what you did?

MR. MOORE: Not specifically for Indian Point. We have used it on others. There really isn't much difference between them in the result.

MR. FORD: So that really, in order to assess what the quality of this procedure is, we have to know two things: First of all, we have to have some firm statistical evidence on the hypothesis that these curbs are similar enough in general so that you don't introduce any great errors in doing that.

The second thing we have to know is that in terms of the generally usefulness of the code, that there is enough data for the different parameter combinations such as when we looked for something, we will find it. The second question is an academic one. I wonder if you can answer that first, simply, and then we will go into exactly the more substantive one, the Indian Point 2.

MR. MOORE: In my opinion, as I understood the question, there was data closely representative of the Indian Point situation. We weren't out of bounds on the parameters used.

MR. FORD: The first part of my question was, what firm statistical evidence do we have that indicates, putting together all of the factors that influence the heat transfer coefficient, what statistical evidence do we have that even after putting them all together, if you only do the analysis in terms of flooding rate, you won't be making more than a few per cent error? The rest of the factors are irrelevant.

MR. MOORE: Just a comparison of the sensitivity of the heat transfer in the region of interest. I want to stress the region of interest.

If you look at the variations of that heat transfer with pressure, temperature, you find it insensitive for all the conditions that were examined in the FLECHT program. That is the basis.

MR. FORD: I can think of a very simple way to do that, namely, if you take the heat transfer coefficient and you set it equal to given points in time, equal to flooding rate plus an error term, and you do a regression analysis in this form, it will give you a coefficient for how much flooding rate explains all this. It will tell you what the statistical significance of the relation is, and it will answer clearly and precisely whether or not the error term, everything else is important relative to the flooding term.

My question is, is it correct that this kind of simple regression analysis would easily answer the question, is flooding rate the main determinant and such a large determinant that everything else may be forgotten?

CHAIRMAN JENSCH: Are you asking him to agree to that?

MR. FORD: Yes. I am asking him to agree whether this simple regression analysis is a simple straightforward method by which we can answer the question as to whether or not his hypothesis is that if you just put your mind on flooding rate, you have a high probability of getting the thing right, and if you bother to consider everything else, you are not going to improve probability of getting things right significantly.

CHAIRMAN JENSCH: Do you agree with that approach?

MR. MOORE: In theory, yes. One has to be careful about where in time we are now with respect to the parameter selected.

CHAIRMAN JENSCH: Thank you.

MR. FORD: Put in, as you remember, as I was writing this, the assumption that all of this is at time t. So given that assumption, we agree on the method.

Now the question is, did you actually use the simple straightforward method, and can you give us the results?

MR. MOORE: No. The simple straightforward method we used was to look at the data directly, and by inspection ascertain the sensitivity.

MR. FORD: So in terms of the application of statistical techniques, simple straightforward application -- of statistical techniques, that you did not perform other FLECHT data?

CHAIRMAN JENSCH: Is that correct? Is that your question?

MR. FORD: Yes, is that correct.

MR. MOORE: That's correct.

CHAIRMAN JENSCH: If you are running out of paper, you can turn the whole mat over and use the back side.

MR. FORD: I have discovered that this has been done before. We are already on the back side. I found a moderately clean one.

MR. TROSTEN: Mr. Ford, if you need more paper, we will undertake to see if we can find some more.

MR. FORD: I appreciate it.

I am concerned to find out what the statistical validity is of the calculated maximum clad temperature of 2300 degrees Fahrenheit. I am trying to indicate on my diagram that the 2300-degree figure is a product of a variety of computer codes working together.

Is it correct simply in terms of the construction of a computer code, that there are essentially four possible areas for error, the first being errors in measurement of the data going into the computer code; is that correct?

MR.MOORE: Yes.

MR. FORD: The second possibility of error is error involved in the derivation of empirical correlations between the data; is that correct?

MR. MOORE: If used, yes.

MR. FORD: The third possibility of error involves the pitfalls of model building. Is it correct that the uncertainties involved here relate, first of all, to the possibility of there being total areas in which there is a lack of general basic knowledge?

MR. MOORE: Speaking in general terms, yes.

MR. FORD: Is it the case that another aspect of the uncertainty of model building is the uncertainty involved in simplifying the phenomena for calculational purposes?

MR. MOORE: That's another possibility of error.

MR. FORD: Finally, is the over-all use of the model subject to calculational errors due to the nature of the approximations that have to be made to put these models on large computers?

MR. MOORE: I wouldn't want to nitpick, but I guess I could consider that as part of the errors in the models, yes.

MR. FORD: Can you tell me, in terms of the data that Westinghouse Corporation has put on the record here, whether in each of these areas we can go and find documents where you explicitly evaluate all of these different areas of uncertainty as they relate to the calculations you performed of maximum clad temperature for Indian Point 2?

MR. MOORE: That's the basis for the assumptions, and the analyses are described in our reports for the key parameters. They are indicated.

MR. FORD: I am concerned with uncertainty analysis of these different possible areas of uncertainty and the whole process. I know you present your assumptions and say this is our assumption, and we believe it. But do you perform along the way rigorous statistical analyses of the uncertainties involved along the way?

MR. MOORE: For example, what rigorous statistical analyses would you make of the assumption that we throw away the accumulator water during blowdown?

MR. FORD: I think what would be involved there would be a sensitivity analysis such that you would have your model predict the correlation between accumulator water in general and all of the other parameters of interest, and these kind of calculations which show you the uncertainties involved in max clad temperature. For example, the question of throwing away the accumulator water. There is a point of conservatism involved there in that it delays the time to flood.

There's a time of nonconservatism involved there in the sense that the water thrown away is water which could be generating steam and causing metal-water reaction to steam binding at an earlier phase of the accident.

Irrespective of whether you accept the specific premises that I am talking about and making, that the accumulator actually cuts both ways as a conservative or non-conservative assumption, that is the kind of reasoning I am talking about with regard to that assumption that could be made to determine the uncertainties in your calculation.

MR. MOORE: I don't see how that has given me a handle on the uncertainty in the calculation. That is a sensitivity study.

end

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MR. FORD: Oh, yes. I am not trying to say that all of the uncertainty analysis is analysis of F statistics and T statistics. The uncertainty analysis there is this: What you do in one case is, you compute what the max clad temperature is under assumption 1 about accumulator water. You can compute what the max clad temperatures are about assumption 2 of the accumulator water. So if it is sort of arbitrary how much accumulator water you keep and how much accumulator water you throw away, you know from these calculations how you are influencing the maximum clad temperature by this process. So it gives you a range of maximum clad temperatures associated with the range of uncertainty and how much accumulator water you will assume.

MR. MOORE: That seems to me a question with respect to sensitivity studies. Sensitivity studies are performed. Another example of an assumption that goes in the code is, during the reflood phase of the accident, the reactor coolant pump rotor is assumed to be locked. What uncertainty should I apply to the assumption that the rotor is locked?

MR. FORD: Obviously not.

MR. MOORE: In fact, I don't expect the rotor to be locked. If I do a calculation with an unlocked rotor, I get a lower clad temperature. That is a sensitivity study. But you are asking me then to quantify a statistical uncertainty with respect to 2300 degrees. I am having difficulty with the assumptions that one is forced to make in the engineering sense, that that is not a pure delineation or an easily obtainable parameter.

MR. FORD: I think the nature of my delineation in the various areas of uncertainty are -- I am not trying to indicate that all of the uncertainty is resolved by a single tool. The uncertainties with regard to empirical correlation there analyzed by the variety of statistical indices. With regard to assumptions, not with regard to -- with regard to whether or not the assumption is influential or important. So you put a handle on that by way of sensitivity analysis.

My question concerns the extent to which you have taken the various areas of uncertainty and rigorously analyzed them by whatever means are necessary. What I am wondering is, can we go, for example, through your various analyses of uncertainty, and instead of taking 2300 as a single scale comes out of the computer at some point, can we use your uncertainty and sensitivity analyses to predict a statistically more valid range for the expected value of maximum clad temperature?

MR. MOORE: I don't know.

MR. FORD: But is it your understanding that in terms of the materials that Westinghouse has put on the record in support of its calculations of maximum clad temperature, it has never put them in the form of a range such as this that reflects the result of comprehensive and systematic analyses of the accumulation of uncertainty in the entire model building and calculational procedure?

MR. MOORE: I don't know if there is any one place where this is pulled together. What you normally see are the parameters which must directly affect the temperature, and a justification to ourselves and to the AEC that we have properly selected those parameters that we do not expect the clad temperature to be higher with respect to those parameters. You will see in the documentation of the loss of coolant analysis various discussions in support of the assumptions made of the particular parameters, with the intent to show that the parameters we have selected are in the direction of increasing clad temperature when we are considering possible uncertainties.

MR. FORD: I am aware of the fact that you make attempts to justify parameters by a variety of arguments. What I am looking for is something that parallels our analysis earlier of the uncertainty involved in predicting a baseball game. Was it clear to you from that analysis that a result of the uncertainty analysis had a much higher probability than the result that you get simply by adding up separate predictions with no consideration of the uncertainties involved in the things that you are adding?

MR. MOORE: If I knew the uncertainties in your ballgame discussion and it was important to me to determine the maximum number of runs that would have been scored, then I can add up the expected plus the uncertainty and I get the maximum number of runs; is that correct?

MR. FORD: Yes.

MR. MOORE: That is a correlary kind of approach that is used here. If a specific parameter can be selected or if there is an uncertainty in a parameter such as accumulator bypass during blowdown, then where a situation has to be made as to how much water will be bypassed, I take the five runs plus two rather than plus or minus 2 for an assumption. So the approach is not one of trying to determine a best estimate temperature and then looking at uncertainties to either side of this best estimate temperature.

MR. TROSTEN: Mr. Chairman, may I confer with Mr. Ford for a moment?

CHAIRMAN JENSCH: Surely. It is almost three o'clock and it is halfway to the time that we are considering terminating the public hearing today at four o'clock. Maybe this will be a convenient time for a recess.

MR. TROSTEN: That will be fine.

CHAIRMAN JENSCH: At this time let us recess and reconvene in this room at 3:05.

end

(A short recess is taken.)

P1Bt1

CHAIRMAN JENSCH: Please come to order.

I understand that you were having a conference at the close just before the recess.

MR. TROSTEN: Yes. We have concluded the conference and Mr. Ford is prepared to proceed.

CHAIRMAN JENSCH: Very well. Mr. Witness Moore has resumed the stand. Are you ready to proceed?

Please do.

MR. FORD: In several of the models that you used to analyze the emergency core cooling situation, the models themselves are built up from experimental data, the experimental data is reduced into empirical correlations and then these correlations are used for calculational purposes.

Now let's suppose in what I call here Model 1, computer code 1, now let us suppose that we looked at the empirical correlation that you used and let's suppose that the R^2 was equal to .333. Now is it correct that the R^2's value of .33 is an index of the per cent of the variance of your dependant variable that can be explained by the factors you propose?

MR. MOORE: Yes, statistically, just by the factors you happen to have selected.

MR. FORD: So this means that roughly speaking if you have your set of data, you draw this regression line through it, that 33 per cent of the time you hit on the nose and that

P1Bt2

66 per cent of the time the points lie at different distances away from the line you have predicted, away from the predictions that you have made, is that correct?

MR. MOORE: Yes.

MR. FORD: Now let's suppose further that we wanted to get straight on all of the empirical correlations that we use and go from model to model, we look at the data that the model relies on, we look at the empirical correlation and we ascertain what its statistical validity is.

Here I have indicated that in Model 2 the correlation coefficient here is .98, which is very good.

MR. MOORE: Could I ask a question?

If I have an empirical correlation I may not be interested in fitting that correlation to get a best estimate or an exact prediction of the parameter. Let us suppose I just want to determine an upperbound for the parameter, that the particular parameter at those given set of conditions will not be exceeded. I don't believe in that case the R^2 has any significance at all, because the R^2 is a measure of my prediction for an exact value, compared to what the data shows, is that correct?

MR. FORD: I think that that is correct in the sense that there are a variety of things you can do to your data besides correlate it. You might simply want to say, "Let me look at the range of the data and I want to find out what was the largest pressure I observed under all the experiments I

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tested."

You say, "Fine. You look at the data. The largest pressure was such-and-such."

So sure, you can reduce data without doing correlational analysis. I am simply talking that in the main in terms of then say putting together FLECHT test data, putting together burst test data and so forth, what you are mainly doing, as I understand it, is trying to put it together statistically and in a number of cases you do report R^2's and so forth.

MR. MOORE: In very few cases.

MR. FORD: But in other cases--let's talk about the other case that you are discussing. You simply have a set of data and you want to find out what the bounds are and you simply, you know, just to find out what the point is furthest out in one radius, one axis, and furthest out on the other. So that gives you the bounds.

Now is it not correct that the uncertainty analysis is a question of trying to figure out whether you have enough points, enough observations to justify the bounds as the bounds? Is that correct?

MR. MOORE: Yes.

MR. FORD: So that even though there are situations in which you are judging your data, in the other situation which you proposed there is still statistical questions about the size of the sample and its sufficiency, and so forth, is that correct?

MR. MOORE: Yes.

MR. FORD: Now let's say that we went through all of your models and we collected all of this data on the various forms of uncertainty of your empirical correlations. It would then be possible, would it not, simply as a function of the empirical correlation, to determine what the uncertainty was of the final result of all of these models, forgetting about the uncertainty in the assumptions and calculational procedures and so forth.

We could simply figure out we have ranges here, we have all the data on the size of the standard deviation as related to the mean. The influence of specific variant F and T tests and all of this. So that we could go through and using the ranges, the probability ranges indicated by these statistical tests, we could go through, could we not, and do something in the same spirit as we did with the ballgame and come up with a range of predictions for the maximum clad temperature and assign to it a probability based on analysis of the probability data and the various empirical correlations, is this correct?

MR. MOORE: I suppose theoretically that sounds like a very complicated process.

MR. FORD: Can you tell me whether this process, however complicated you regard this statistical analysis, can you

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tell me whether you have done this and whether, therefore, you can give us a probability, a range of predictions for max clad temperature, and sign a probability number to it?

MR. MOORE: I indicated earlier a range for the sensitivity I expected, and that was I didn't expect the temperature to go above 2300 degrees, and in fact it could be as much as 800 degrees lower. I would not be in a position to define a probability for that.

One of the difficulties is you have apples and oranges here as you go through your analysis and that the procedure used in doing the safety analyses is to apply, taking basic data that's required in order to make an assumption, and apply this data in a manner that will, based on sensitivity studies, tend to increase the clad temperature and then set reasonable conservatisms on there.

The approach that you are talking about, I think, would be more applicable and perhaps more direct if I were to take each of my parameters and assumptions and determine a best estimate for that, with a variance around that best estimate, and combine all these statistically and get a best estimate temperature with plus or minus uncertainties.

end

MR. FORD: I see.

Is the logic of your argument following: That since whenever there is uncertainty you always assume the worst with respect to max clad temperature, therefore all the uncertainty that does result is in the form such that it's all -- the only direction in which you'd correct your prediction would be downward?

MR. MOORE: Well, I will quarrel with your characterization of the word worst. We assign assumptions for the parameters in the direction which will increase the clad temperature. As an example, the discharge coefficient assumed.

MR. FORD: As an example that we mentioned earlier, the heat transfer coefficient, you indicated earlier that it had an uncertainty of plus or minus 10 per cent. Yet on the other hand you don't, in terms of the range of that variable, the highly probable of between 90 per cent and a hundred per cent of what the actual number is on the graph -- am I correct that you do not make the assumption here when you plug that heat transfer coefficient into your calculations, you don't use the lower end of the uncertainty range. You don't say, "Well, it may be .9 instead of 1 and therefore to be conservative we will use .9." You don't do that in this case, do you?

MR. MOORE: That's a yes and no answer. Perhaps you may argue my semantics, but I look at the reflood heat transfer part of the transient as an integrated evaluation of flooding rate and heat transfer associated with the flooding rate. So that I have selected the parameters that go into the analysis to minimize the flooding rate, which in itself minimizes heat transfer, and as you indicated I have gone directly to the FLECHT data without an additional, an additional now uncertainty.

And one of the reasons I feel confident about that is, as we mentioned earlier, the more significant effects, the effects of mixing vane grids, which I cannot truly quantify, so that that's why that's not incorporated. But I hope you understand the philosophy that's used.

MR. FORD: Well, as I understand it, you are trying to say that -- first you indicated that the reason that we don't have to do this uncertainty analysis is that we always make the assumption when we have a choice of assumptions that gives us the highest max clad temperature. And now you are telling me, as I understand it, that, well, there are a number of parameters relevant to this. One of them is flooding rate. One of them is heat transfer, and so forth. And they say, "You make the conservative assumption with regard to flooding rate, presumably with regard to a number of other things,"but now we come along to the heat transfer coefficient and you say, "Well, we have already been conservative enough, terribly conservative every place else." So that we don't make it here.

Now, my concern is that in terms of things you refer to such as the effect between mixing grids, whatever term you used --

MR. MOORE: Mixing vane grids.

MR. FORD: Mixing vane grids. You refer to this term and you say that's a credit we could take, but we don't, yet in answer to my question you indicate that there was insufficient data. That's why you don't take the credit.

MR. MOORE: That's not what I said. Perhaps I can clarify it. I said we did not have data from the FLECHT tests for mixing vane grids per se in order to precisely quantify the benefit. The fact that there is a benefit, I believe, is beyond dispute. That it is determined by the flow blockage tests that have been run, both in FLECHT and other facilities, the point being that the mixing vane grids have an effect of a reduced area at the grid, which is like a flow blockage effect, and that is my basis for making the point.

MR. FORD: Now, you contend that it's undisputed, you know, by professional nuclear engineers, it's undisputed that such mixing vane grids improve heat transfer.

MR. MOORE: Is that what I said?

MR. FORD: I believe so.

MR. TROSTEN: Do you wish to have your statement reread, Mr. Moore?

CHAIRMAN JENSCH: I think the question is he says it's without doubt. I suppose he should identify without doubt to whom. I think that would answer the question.

To whom did you refer when you said "everybody's in agreement about this mixing vane grid?" Everybody in Westinghouse or everybody in Westinghouse and General Electric and Babcock and Wilcox and Western Engineering? How many come in on this?

MR. MOORE: I was referring to the test results that people in the field have observed which indicate that with blockage there is an improvement in heat transfer due to breaking of the water droplets. That's my reference. It's an observed effect. And that same effect would come into play with the mixing vane grids.

end

in the reactor would have the same kind of an effect in the reactor, in the Indian Point reactor. The FLECHT tests were performed without mixing vanes. That's my basis for the conservatism.

CHAIRMAN JENSCH: When did you learn that the flow blockage was working so well that you didn't have time to work it into the FLECHT tests?

MR. MOORE: That's right. The flow blockage tests were done late in the FLECHT program.

CHAIRMAN JENSCH: Very well. Thank you.

MR. FORD: I think we are going to move on to some other areas. There are a few small questions that I had in a few dissimilar areas, that I just wanted to go to my miscellaneous collection.

Is hypoiodous acid, that is HIO formed in significant quantity within the reactor for containment during the loss of coolant accident?

MR. MOORE: I pass.

MR. TROSTEN: Mr. Ford, I believe, I think Mr. Roisman will confirm this, that we went into this at some length with Mr. Fletcher, perhaps, and Mr. McAdoo.

MR. FORD: I am not talking about the formation of methyl iodide.

MR. TROSTEN: Am I correct, Mr. Roisman?

MR. ROISMAN: Yes. I am explaining to Mr. Ford so

MR. FORD: Is the grid that you used in the flow blockage test, that's an example of a mixing vane grid?

MR. MOORE: It's an example of a reduction in flow area at a specified limitation.

MR. FORD: I see. And it is similar enough to a mixing vane grid such that the improvements in the heat transfer that accompany the atomization of the coolant from that blockage grid, would you also expect that from the mixing vane grid itself?

MR. MOORE: If you look at a picture of a mixing vane grid you will see how the mixing vane fingers protrude into the coolant stream. We are talking about the same kind of effect with respect to breaking off the droplets and causing improved heat transfer.

MR. FORD: Can you tell me if such grids have such improvements on heat transfer why they aren't in the reactor as normal equipment?

MR. MOORE: They are.

MR. FORD: The kind of grids you use to simulate flow blockage?

MR. MOORE: You are confused. I said the flow blockage simulation indicated the effect of a reduction in flow area. This effect was to break up the water droplets and improve heat transfer.

I then indicated that a mixing vane grid as used that I make sure that he and I are after the same thing.

MR. FORD: I withdraw my miscellaneous question. I am informed that everything I want to know about it is on the record.

I'd like to turn then to some questions about blowdown. If I understand your blowdown calculations, you used two models to understand and to compute max flow, is that correct?

MR. MOORE: Yes. Maximum flow through the break?

MR. FORD: Yes.

MR. MOORE: Yes.

MR. FORD: Now these two models apply to different assumptions you made about the nature of the coolant at that stage of the accident. Can you tell me in terms of time your assumption about the saturated coolant and your assumption about subcooled stage? Can you tell me what fraction of the blowdown from the guillotine break, what fraction of the blowdown involves subcooled liquid and what fraction involves saturated liquid?

MR. MOORE: In general, yes. The subcooled phase of the blowdown occurs over a range of time in milliseconds. I don't know if it's important, but 30, 40 something milliseconds. Various fractions of the second. Then you are into saturated blowdown and you are into rather high quality saturated blowdown, again in the order of milliseconds, and that gives you a scope of the time involved. The majority of the blowdown is obviously saturated blowdown.

MR. FORD: Can you tell me in terms of the experiments that have been performed to analyze the quality of discharge from blowdown, can you tell me whether these experiments have confirmed that analysis or whether anomalous discharge conditions have been observed?

MR. MOORE: Well, in the application of the reactor calculation I think the experimental data confirms the correlations used. There have been some experiments which indicate higher blowdown rates. For example, compared to the Moody correlation. But these higher rates occur over a very low quality. The quality is less than 10, 20 per cent. These qualities, we are beyond that quality. We have higher quality than that in a very short period of time. Again, millisecond kind of times. So that I believe the approach we used, particularly with the discharge coefficient in the analysis, will overpredict the discharge.

end

MR. FORD: Is it correct, in terms of the equation you use for mass flow per unit area, that the pressures in the system that determine this, these are all fixed? The primary determinant -- I mean fixed at the instant of blowdown. The primary determinant of the mass flow is therefore the density of the fluid; is that correct?

MR. MOORE: It is a function of temperature. Density is a function of temperature, yes.

MR. FORD: Is it correct that the density of the subcooled liquid is two or three times the density of the saturated flow?

MR. MOORE: Yes.

MR. FORD: So that in terms of the standard mass flow equation, if you wanted to relate mass flow to the assumption you make about the quality of fluid, the main thing you would be doing is changing the density parameter of the mass flow equation so that the mass flow itself would vary as the square root of whatever your adjustment factor is for density?

MR. MOORE: I am lost in the question. In what direction are we going?

MR. FORD: I have written up the equations. Maybe I can show them to you. It might help.

MR. MOORE: Yes.

MR. FORD: We will let everyone see what we are agreeing to. I should put this up. We are talking in a simplified case here about discharge from a pressure vessel to a rupture. The pressure inside the vessel is inside, and the pressure in the containment here, of course, is p outside. The equation for the conservation of energy we use here is this, which gives us a mass flow equation of density times velocity. The point being, as Mr. Moore and I have agreed, that as you change the density, as you increase it from, say, the density of the saturated discharge to the density of the subcooled discharge, the entire mass flow here increased , by the square root of whatever factor you plug in as the difference between the density of the discharge.

In terms of any assumptions we might make about the nature of the discharge -- we assume that most of it is saturated and a tiny bit of it is subcooled.

MR. MOORE: That's not assumed. We calculate that.

MR. FORD: You calculate that?

MR. MOORE: Yes.

MR. FORD: Am I correct that if we had more subcooled discharge and less saturated, that we would be talking about an increase in the mass flow rate by the factors indicated here?

MR. MOORE: With one complication, the fact that for the geometry indicated and the situation of the reactor, we have critical flow existing at that discharge.

MR. FORD: I am trying to simplify it to this simple model without all of the complexities of the actual blowdown.

MR. MOORE: If the question is, do the subcooled blowdown rates, are they greater than the saturated blowdown rates, the answer is yes.

MR. FORD: Are you aware of the various observations that have been made in blowdown experiments of the presence at times when two phase flow was expected of a metastable equilibrium of the discharge that behaved as a subcooled liquid and had the density of a subcooled liquid?

MR. MOORE: Yes, in general.

MR. FORD: Can you tell me, in terms of the thermodynamic analysis that you have performed, whether this is anomalous or whether this is predicted?

MR. MOORE: The analyses we perform assume homogeneous conditions in equilibrium.

MR. FORD: Is it correct that if you used non-equilibrium assumptions, that you would predict this kind of metastable state of the liquid with the higher density than a two-phase flow and thus a higher mass flow rate?

MR. MOORE: I understand there is a lot of discussion in that field by those who specialize in that field as to the true significance and relevance of this metastable situation. As I understand, it can be involved in discussions of nucleation rates and bubble formation and even just temperature gradients in a pipe. In any event, it is not clear that a nonequilibrium model -- attempts have been made with a nonequilibrium model to simulate this. I believe in one particular case it did not correctly simulate the effect. In any event, it is not a significant effect with respect to our application, such calculations.

MR. FORD: I am interested in a few things that you said. I realize it is a very open kind of question as to the precise significance of this metastable equilibrium. What I am concerned with is, whether you have prepared or Westinghouse has prepared any analysis which, in terms of the available data on this, would justify the specific assumptions that you make. You make the assumption it does not occur. I am wondering whether you can justify that.

MR. MOORE: I didn't make the assumption it doesn't occur. I said the occurrence of it is not of significance in the analyses performed. Comparisons of our analyses have been made against blowdown experiments with reasonably good agreement.

MR. FORD: What I am concerned with is, you say you assume it doesn't occur. It isn't explicitly disregarded, this particular state, is it? I am wondering whether you have specifically been able to justify, in terms relating the not just general conformance of the models for blowdown experiments, but whether specifically in terms of the anomalous data in those experiments, whether you specifically related your models to that?

MR. MOORE: No. It is more of a purist problem than an engineering problem.

MR. FORD: Isn't it an engineer's problem in the sense that you can get a significant increase in the mass flow rate?

MR. MOORE: No, not of import during the subcooled stage.

end

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MR. FORD: Can you tell me where you presented your analysis of this phenomenon?

MR. MOORE: You mean blowdown analysis?

MR. FORD: Your analysis of the metastable states of the liquid and why you can make assumptions you make and why it is insensitive to interference from such phenomena.

MR. MOORE: Yes. In the WCAP that I referenced--I don't know when it was, maybe last week. That was 7015, I believe. It is 7401. Excuse me.

MR. FORD: In terms of the isotherm that I had put on there earlier this morning, do you recall that? Do you still have the picture?

MR. MOORE: The isotherm?

MR. FORD: Yes. I will show you the picture.

MR. MOORE: I think I have taken your paper away.

CHAIRMAN JENSCH: It is on the floor.

MR. FORD: As a matter of fact, it is a lot easier to redraw it.

In terms of the isotherm represented here as the ideal gas, is this the shape of the isotherm that you assumed governs the behavior of the coolant during blowdown?

MR. MOORE: No. The behavior is determined through the empirical correlations that are presented in the report.

MR. FORD: I am talking theoretically in terms of the discharge equations that you use, what assumptions do they

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make about--my understanding of the assumptions that you make is that as you reach saturation temperature here, you can simply decrease the quality at the same temperature at the same pressure. Is that an assumption behind your calculation?

MR. MOORE: It is a transient calculation. We are decreasing temperatures and pressures throughout the transient.

MR. FORD: Do you understand, in terms of Van der Wall's equation, do you understand how this region here describes the metastable equilibrium wherein the coolant is below this saturation temperature here. It still has the density of the subcooled liquid rather than the density of the two-phase mixture?

MR. MOORE: I'm not familiar with that aspect, no.

MR. FORD: Am I correct that in terms of the analysis that you present of blowdown, that your main point is that your calculations of blowdown agree with blowdown experiments, and that whatever the theory is about the nature of metastable states of the coolant, that is irrelevant?

Is that the level on which, you know, you are supporting your position? Is it simply that the empirical correlation of your model to the blowdown experiments supposedly works, and therefore what the theory in depth is and what assumptions you have to make for negative pressures here, that all is irrelevant?

MR. MOORE: I don't know if I should even respond directly to that kind of a question, but I will anyway.

Number one, our blowdown analysis do not in fact correlate to blowdown tests because our blowdown analysis indicate higher discharge flows than any blowdown tests that have been performed using our discharge coefficients of 1.0.

Point number two, we have experts back at Westinghouse who spend a lot of time looking at these correlations. They have evaluated them. They are aware of the theoretical experts. We are big boys. We got a big company. We know what we are doing here. The upshot of it is that when you look at the data that is presented in the June 1 report beginning on Page number 35, there is a detailed discussion of the blowdown assumptions made both subcooled and saturated.

MR. FORD: I don't mean to offend you--

MR. MOORE: I'm not offended.

MR. FORD: It may indeed be the case that one can prescind from all of these compexities and simply say, well, it works. But my concern is--I just want to understand that that's the position and that's the position, and then we simply go on--

MR. MOORE: Perhaps if I can understand your concern, I could be more responsive.

MR. FORD: Can you tell me in terms of your experiment confirmation of your blowdown model, that I note that you list in the references in the June 1971 report, the report

Q2Wt4

of Battelle's blowdown experiment study as your first reference. The document is entitled, "Experimental high enthalpy water blowdown from a simple vessel through a bottom outlet."

Is it your submission that the experimental data presented here presents confirmation of the blowdown analysis that you rely on at Indian Point 2?

MR. MOORE: Yes, as described on Page 36 for different blowdown cases. We compare the measured discharge rate to the discharge rate that would be predicted by the Moody correlation that we use, and show that in order to properly or accurately predict the discharge rate in the test, you have to multiply the Moody rate by a factor of .48 or .37.

MR. FORD: Can you tell me, in terms of the experiments reported as part of the Battelle containment systems experiment that are contained in this document referred to in your June 1971 statement on emergency core coolant performance, can you tell me what the scale is of the blowdown experiments and how they relate to the system size at Indian Point 2?

MR. MOORE: I believe that the vessel used was about one-fifth the size of the Indian Point reactor vessel. The area of the blowdown hole over the area of the vessel was a factor of 4.5×10^{-2}, which in terms of relative area to volume, area orifice to area of the vessel for Indian Point would be equivalent to a four and a half-square foot break.

Q2Wt5

MR. FORD: Can you tell me, in terms of that scale of blowdown experiment, whether you can scale this data to Indian Point 2 with no fear that the results could be reversed or altered?

MR. MOORE: Yes, I believe so.

MR. FORD: It is indicated here that there was some considerable problem in estimating the mass flow rates because of subcooled blowdown data, that they don't quite understand what happened to it. Are you aware of this problem in the experiments reported in your reference?

MR. MOORE: May I see the reference?

MR. FORD: Yes.

MR. MOORE: Where was your reference? Where is the statement you are referring to?

MR. FORD: I am referring to the general problem throughout the report of this anomalous behavior in a subcooled discharge. This is a general problem that they consider throughout the document.

My question was whether or not you were familiar with this problem.

MR. MOORE: Perhaps we have characterized the problem. This was a test which performed blowdown which had both subcooled and saturated blowdown.

end

MR. FORD: Perhaps the abstract may sum up a good bit of their discussion. This is page 1 of BNWL-1411. It says, and I quote:

"Comparison of the data with theoretical prediction was made. The mass flow rates were less than predicted.for all but the smallest orifices and were shown to be proportional to orifice area raised to the minus zero .165 power. The subcool power data gathered here and elsewhere using gas, such as nitrogen pressurization without special precautions will not be applicable to a typical reactor loss of coolant design basis accident because of the pronounced effect demonstrated by nitrogen solubility on blowdown in the subcooled regime."

Does that refresh your memory as to the general problem they were concerned with?

MR. MOORE: Yes, in general.

MR. FORD: Can you tell me, in terms of using that data to relate to the subcooled portion of blowdown, when you say that the references given in this July 1971 study provide experimental confirmation of your blowdown model, in terms of the fact that the blowdown model does a number of things, one of which predicts subcooled discharge -- does that experimental data provide confirmation of your prediction of subcooled discharge for Indian Point 2.

MR. MOORE: I don't know the answer to that immediately. I have to refresh my memory with respect to the subcooled aspects of this test on discharge rates. The reference I made, of course, on page 36 is in comparison to Moody and applies to the saturated blowdown. It is not inconsistent with the statements made in the report.

MR. TROSTEN: Mr. Ford, could you use your microphone. I believe the people in the back of the room cannot hear you.

CHAIRMAN JENSCH: Page 36 was the July 1971 report?

MR. MOORE: Yes. I'm sorry.

MR. FORD: That was June.

MR. MOORE: Yes, June.

MR. FORD: Can you tell me, have you prepared any analysis on the special precautions that you take in applying the Battelle experimental data to Indian Point 2 as confirmation of your blowdown calculations?

MR. MOORE: We don't have the nitrogen situation they describe in the reactor.

MR. FORD: I realize that. It is in their mind that because you don't have the nitrogen situation that there is difficulty in applying their data to the large reactor loss of coolant. I am asking, where have you indicated these special precautions that you took pursuant to their caution?

MR. MOORE: I am just looking for a specific reference. Those tests are discussed in the previously referenced WCAP 7401.

MR. FORD: Well, can you tell us in a general way, and we can check there for the precise way in which you evaluate the data, but can you tell me in a general way what special precautions you took in using this data from the Battelle tests as confirmation of your own blowdown calculations pursuant to their caution that this must be done to apply the data to a large reactor?

MR. MOORE: No, I cannot at this point.

MR. FORD: Do you rely on experimental data evolved in the Idaho semi-scale tests to confirm the blowdown calculations, calculational models that you used for Indian Point 2?

MR. TROSTEN: Mr. Ford, I'd like to have you clarify that question. Are you referring to specific data?

MR. FORD: My question was a general question relating to the 800 series of tests. The tests at Idaho, semi-scale tests, have numbers on them. The 800 series involved blowdown tests. I believe they are reported in the Idaho Nuclear document IN-1384; the same semi-scale model was used in the accumulator tests was used in some of the more general blowdown tests. I believe that all PWR vendors have used the semi-scale result. Idaho Nuclear indicates in its June 29th, 1971 document on the semi-scale results that their purpose was for code development and so that the vendors could check their codes against Idaho semi-scale blowdown, such as trying to ascertain whether in point of fact part of the confirmation of the blowdown calculations for Indian Point 2 is based on experiments involving the small semi-scale model.

MR. TROSTEN: I appreciate the explanation and I would like to ask you another question in determining whether or not I should object, and that is whether you are asking a question related to reliance on specific data in connection with design of the facility, because this reactor was designed prior to the time that these tests were performed. I don't really understand at this point your question.

MR. FORD: No. I am concerned with the code used to evaluate blowdown during loss of coolant accident and whether the code was checked on the Idaho loss of coolant accident blowdown tests.

MR. TROSTEN: I understand your last question. I don't object to your last question.

MR. MOORE: Yes. We have made comparisons of our blowdown codes to the semi-scale tests.

CHAIRMAN JENSCH: I think the question is do you rely upon them.

MR. MOORE: Well, in the sense that, as the data becomes available we do check against our correlations that are used to an additional confirmation. We don't rely on them per se.

MR. FORD: In addition to the Battelle work and the Idaho semi-scale work is there any other experimental data that you use in some way as confirmation of your blowdown calculations?

MR. MOORE: Well, yes. There have been tests performed at the Illinois Institute of Technology Research Institute on the blowdown of pipes, vessels, and this has also been compared. I indicated some of this in previous testimony with Mr. Roisman.

MR. FORD: Now, the specific question I have -- I have noted previously testimony generally related to this area. But the question I have specifically in mind is whether the assumption you make about the portion of the blowdown and subcooled regime versus the powers of the blow-down in the two-phase regime, whether that specific section has been verified in either Battelle, Idaho or your recently-cited Illinois experiments.

MR. MOORE: Yes. In the sense that we trace the time history of the transient from the beginning through the end of blowdown.

CHAIRMAN JENSCH: Is this a convenient place to interrupt your examination? It's just about four o'clock or a couple minutes thereafter, and as we discussed this morning we planned to recess this hearing now until nine o'clock tomorrow morning. Is this a convenient place to interrupt your examination?

MR. FORD: Yes, to begin the in camera session.

CHAIRMAN JENSCH: Yes.

MR. FORD: Yes.

CHAIRMAN JENSCH: We are planning to have a further session of your in camera session proceedings so we will recess in order that that in camera session may convene at 4:15.

At this time the public hearing of this proceeding is recessed.

MR. TROSTEN: I am sorry to interrupt you, Mr. Chairman, but I'd like to clarify if the interrogation in open session of the Witness Moore is ended. I understand that it is on the basis of what has been said.

CHAIRMAN JENSCH: I thought he was halfway through something. But what is the situation?

MR. TROSTEN: That's why I wanted to clarify it, because of the fact that the interrogation of Witness Moore in open session was supposed to end at or about four o'clock this afternoon.

CHAIRMAN JENSCH: That's where those predictions and calculations give us some sort of -- blowdown.

MR. TROSTEN: Pardon?

CHAIRMAN JENSCH: There is a little blowdown.

MR. TROSTEN: What I was about to suggest, Mr. Chairman, was if Mr. Ford had another question or two, rather than asking Mr. Moore to hold over for that question or two until tomorrow morning that we continue him until he completes that question or two.

CHAIRMAN JENSCH: What is the convenience of the interrogator and the other parties? Are you agreeable?

MR. FORD: I just have a question or two.

CHAIRMAN JENSCH: Well, let's proceed then.

MR. FORD: To finish off this discussion of blowdown.

Now, you relate to these experiments as having indirectly confirmed the assumption that you make about the proportion of blowdown that's in different regimes. Now, what I'd like to know is whether these experiments specifically provided data on that proportion. I mean I know they predicted over-all -- you observed a mass flow rate and so forth and you predict one and you compare that. Now, what I was more concerned with was whether the specific assumption that you make about the proportion of blowdown that takes place in subcooled regimes versus the proportion of blowdown that takes place in a two-phased regime was specifically confirmed by virtue of being observed in any of the tests that you studied from Battelle, Idaho or Illinois.

end

MR. MOORE: Well, I guess we are arguing about what is specific. There are correlations which individually have been compared and developed by other experimenters and then we apply these to the results and we get a comparison in time to the result. No specific measurements were made, to my knowledge, of the conditions at the discharge to determine whether in fact at one instant in time we switched from subcooled to saturated blowdown, if that is the thrust of your question.

MR. FORD: No. I am not concerned with whether or not these experiments identified the instant in time. What I am concerned with is whether they verified the proportion of time that you assumed the system is in subcooled discharge versus two-phase discharge.

MR. MOORE: I guess I would just have to say if we have been able to predict the pressures at the flows in the system then we'd probably have a good check on the correlations.

MR. FORD: But in terms of the fact that you make a clear assumption about the proportions of time that the system spends in the different regimes, was there a clear observation in these experiments that the system actually spent that proportion of time in the two regimes?

MR. MOORE: I believe when you look at the results there is a definite break point in flows that can be related back to the interphase between subcooled at saturated blowdown, yes. I think the answer is yes, in comparing that to show this occurs in very short time periods.

CHAIRMAN JENSCH: You have concluded your examination of Mr. Moore?

MR. FORD: Yes, sir.

CHAIRMAN JENSCH: Is there any redirect?

MR. TROSTEN: We have no redirect of Mr. Moore at this time, Mr. Chairman.

As I indicated previously we may well have some redirect on the subject of the emergency core cooling.

MR. MOORE: Mr. Chairman, excuse me. May I consult with Mr. Trosten a moment.

CHAIRMAN JENSCH: Yes, surely.

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MR. TROSTEN: Mr. Chairman, Witness Moore points out that there was a question raised by Mr. Ford on transcript pages 1893, 1894. I presume that is the old number. In any event, it was an unanswered question which he would like to answer at this time.

CHAIRMAN JENSCH: Very well. You will summarize the question as well as you can, Mr. Moore.

MR. MOORE: The problem is to find the answer.

MR. ROISMAN: Mr. Chairman, while he is looking I just want to be clear that there are several places in the transcript where data was requested, the answers to which have not yet been received, and whether it was Witness Moore or the Applicants' other witnesses who are going to provide it orally or in writing to be submitted as though it had been provided orally, I want to make it understood that we weren't waiving any right to that subsequent data. I can't list it all right now, but I know that there are some -- there is still some information yet to be received.

CHAIRMAN JENSCH: Well, will you take that up with Applicant's counsel.

MR. ROISMAN: Yes.

CHAIRMAN JENSCH: And if there is some later arrangements you want to make about the submission of data we can consider it later.

MR. TROSTEN: Yes. I'd like to say that except for today's information we were under the impression that this was it. But I will be glad to talk to you, Mr. Roisman.

MR. ROISMAN: One thing that I remember was -- I believe this is correct -- a rod temperature census through the core.

MR. MOORE: That was answered the following day.

MR. ROISMAN: I think all that we got were percentages, didn't we?

MR. MOORE: Percentages of the cladding that were above certain temperatures, yes.

MR. ROISMAN: My recollection was that the question was more than that. But I will check and I will do what the Chairman suggests, talk with Mr. Trosten.

CHAIRMAN JENSCH: Very well.

Are you planning to go home, Mr. Moore?

MR. MOORE: Where is that?

MR. TROSTEN: Yes, Mr. Chairman. The answer to your question is he was planning on going home this evening. Of course he could be subject to recall later.

CHAIRMAN JENSCH: Well, we are going to be here in the morning, but if he is planning to leave tonight --

MR. BRIGGS: Is it the thought that the Board won't have any more questions of Mr. Moore?

MR. TROSTEN: No, sir. That wasn't it. It was just a matter of tomorrow's session being devoted to the Staff, Friday, and so on.

CHAIRMAN JENSCH: Well, if you have redirect undoubtedly Mr. Moore will be back.

MR. TROSTEN: Yes. That is exactly the thought.

CHAIRMAN JENSCH: The Board's questions can be propounded at that time.

By the way, does the Staff have any questions?

MR. KARMAN: We would like to waive at this time, Mr. Chairman, the right to cross-examine until after redirect. There are certain matters which Mr. Trosten has indicated they would like to straighten out or clarify and some of these may very well be some of the problems which we have encountered.

CHAIRMAN JENSCH: Very well.

Do you have your question and answer, Mr. Moore?

MR. MOORE: Yes.

CHAIRMAN JENSCH: Will you proceed, please.

MR. MOORE: The question related to the assumptions made with respect to the heat transfer coefficient, the gap conductance in the fuel rod for the loss of coolant accident and the fraction by which the coefficient used is lower than the normal heat transfer coefficient, and the figures used in the analysis. The gap coefficient, the importance of the gap coefficient is primarily one of initial gap conductance, which determines the amount of stored energy in the fuel, which then must be subsequently transferred to the cladding during blowdown and during the heat-up phase.

The gap conductance that is used is a value of 2,000, compared to an expected value of approximately 2800. This then provides about a 120 degree Fahrenheit increase in equivalent stored energy at the hot spot and were we to use the expected gap coefficient we would expect the peak clad temperature to be lower by a 100 to a 150 degrees Fahrenheit.

MR. FORD: Could you explain when you say it provides an increase in equivalent stored energy such that you get a clad temperature of a 150 degrees higher---lower, rather?

end

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MR. MOORE: Lower, no. Let me clarify that. The gap conductants, because we use a lower value of conductants than expected, this has the effect, then, for the same heat transfer, giving us a higher fuel temperature. Therefore, we have more stored energy in the fuel at the beginning of the blowdown transient. So this is additional heat that must be transferred to the cladding and ultimately then to the cooling. And I was indicating the reverse, that if we used the expected gap conductants the fuel temperature would be reduced, we would have less stored energy and the clad temperature would have been reduced, because we have less stored energy during the transient to transfer.

MR. FORD: Am I correct that whether or not this is conservative depends on what kind of assumptions you make about the heat transfer mechanisms during blowdown and reflood, such that when you assume that when the fuel is hotter and you save the stored energy for later, that implies, of course, that you have a lower clad temperature at the beginning of reflood, is that correct?

MR. MOORE: Oh, I am sorry. You are talking about the--once I have got the stored energy in there. Now if I get the stored energy out too soon, that by the time I get to reflood the temperature could be lower?

MR. FORD: Well, I am talking about the trade-off involved. The stored energy is heat which may cause you

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problems in blowdown. Now by increasing the cladding temperature in the blowdown, now this assumption that you make decreases the cladding temperature of the blowdown and it saves the heat for later and then it says, well now during reflood, now this is conservative with regard to the heat that has to be dealt with there, but it's not conservative with regard to the heat that has to be dealt with on the caldding during blowdown.

So I am trying to clarify or asking you in precisely what sense is this conservative assumption, or if it's a conservative assumption with regard to one of the two phenomena is it a non-conservative assumption with regard to the other?

MR. MOORE: I see. No.

I was referring to the initial gap conductants which is the primary effect. There is an effect, as you indicate, that if I were to maintain that kind of a gap conductance this would, during the transient tend to get the heat out into the cladding and out into the coolant during blowdown. The assumption is made in the analysis that in a tenth of a second the gap conductant reduces to a thousand and in three seconds it reduces to five hundred, using the same units, and so this is always below the expected gap conductants during blowdown.

So we have tried to keep the heat in a conservative way from going to the cladding and into the coolant during

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blowdown and saving it, essentially, for reflood.

MR. FORD: I see. But is it correct in the method that you calculated you predict a higher clad temperature at the beginning of blowdown, at the beginning of reflood, than you would have--you can predict a lower clad temperature at the beginning of reflood than you would have if you had actually gone along with your expected values rather than the one that you assumed?

MR. MOORE: If I used expected value this reduces the stored energy initially and also gets it out faster, both of which are beneficial effects. So if I use the expected value the temperature at the end of blowdown should be lower, so that the total peak clad temperature is reduced.

With the values I have used I have increased the temperature at the end of blowdown.

MR. FORD: But you have saved a good part of the stored thermal energy to go out during reflood rather than having that cause problems during blowdown.

MR. MOORE: Yes, yes.

MR. FORD: So that there is some price to pay either way. In making an assumption about gap conductants here is it correct you simply can't make one that's conservative all the way around, and if you make it in one way you are going to have that heat around later and then vice versa, if you get it out quicker you won't have the heat around later.

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MR. MOORE: Yes, I would agree, I guess. It's the total, overall transient you have to look at, that's right.

MR. FORD: Right.

Now can you tell me in terms of the procedures you used to estimate this, in any experiments that you have done, has gap conductance been measured? Have you simulated gaps?

MR. MOORE: Yes. There have been some experiments performed. They are referenced in the FSAR.

MR. FORD: Now these are the presumed experiments or data collected during normal operation. I mean the gap conductance is well-known for that. I am talking about under the loss of coolant accident situation, whether in that kind of a situation you experimentally studied gap conductance?

MR. MOORE: I can't speak specifically for any calculations that directly simulates the loss of coolant. The behavior of the gap during the transient is such that as the cladding expands, as it heats up and the fuel contracts as it cools down the gap increases and this reduces the gap conductance. And one can then, knowing the constituents of the material in the gap, calculate as a function of this distance what the heat transfer would be.

We take a much lower value for conductance during blowdown than you would predict by this differential expansion, that kind of a straightforward calculation.

MR. FORD: I see. Now have you actually taken

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honest-to-goodness fuel rods with zircalloy cladding and UO_2 pellets and an honest-to-goodness gap and heated them up to LOCA transient temperatures and from that determined the magnitude of the various swelling and contraction that determines what changes in gap conductants will be?

MR. MOORE: No, not directly.

MR. FORD: I think the rest of the questions in this section covered my concerns.

CHAIRMAN JENSCH: Very well. Thank you, Mr. Moore. You are temporarily excused, subject to call for redirect and possibly recross examination.

If there is nothing further the public hearing in this proceeding is now recessed to reconvene tomorrow morning in this room at nine o'clock and we will reconvene here at 4:35 for an in camera session.

(Public hearing recessed.)

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