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IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY OF NEW YORK, INC.

(Indian Point Station, Unit No. 2)

Docket No. 50-247

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Date - November 4, 1971

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UNITED STATES OF AMERICA

ATOMIC ENERGY COMMISSION

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CONSOLIDATED EDISON COMPANY OF NEW YORK, INC. : DOCKET NO. 50-247

(Indian Point Station, Unit No. 2 :

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

Thursday, November 4, 1971

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

BEFORE:

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

DR. JOHN C. GEYER, Member.

MR. R. B. BRIGGS, Member.

I N D E X

CHAIRMAN JENSCH: Please come to order.

Before we proceed with the evidence this morning, the Board would like to provide an addendum to its statement with reference to proposed findings and conclusions. The Board discussed that with the parties yesterday.

In connection with the proposed findings of fact and conclusions, the Board will appreciate the Applicant and the Intervenors submitting a brief in connection with their proposed findings of fact and conclusions. The Staff, we would appreciate if not a brief, a statement of comments, possibly, in connection with the proposals which have been submitted by both the Applicant and the Intervenors, which would mean that probably after the Intervenors have filed, the Staff will at a later time submit its comments.

Very well.

MR. TROSTEN: Excuse me, Mr. Chairman. We understand, of course, that there are a number of legal questions that will have to be briefed. Were you preparing to identify at this point any particular items?

CHAIRMAN JENSCH: No. We would like to have a brief in connection with the proposals made by the Applicant and the Intervenors so they will make a selection of those items which they think are of greatest importance as well as all the legal issues involved.

With that, if there is nothing further, are we ready to proceed with the further interrogation of the witnesses? I see an absence of witnesses in the front row. I notice that they are getting farther and farther in back of the room as the hearing goes on. Mr. Moore is coming from the last row now.

MR. TROSTEN: They are trying to get away, Mr. Chairman.

CHAIRMAN JENSCH: I take it that is no necessary reflection on the character of the interrogation either.

MR. ROISMAN: Mr. Chairman, I understand that the Staff was going to make a formal submittal of a document into evidence. Mr. Karman, are you going to do that now?

MR. KARMAN: Yes.

On Monday, Mr. Chairman, I offered into evidence the Supplement No. 3 on Staff safety evaluation, the pressure vessel report of the AEC Regulator Staff in response to questions of the Atomic Safety and Licensing Board, and responses to the Board by the Regulator Staff related to questions asked by the Board on October 5, 1971 session of this proceeding.

I have, Mr. Chairman, copies of the corrections which were read by Mr. Novak with respect to Supplement No. 3

to the Safety Evaluation. Mr. Novak will now distribute those corrections to the Board and to the parties. I have one correction to make in the pressure vessel report, which, too, has been distributed to all the parties.

On page 30 of said report, on the third line of the last page, strike the words "installation in the plant" and substitute in place thereof the word "hydrotest."

That's the only correction to that report, Mr. Chairman.

CHAIRMAN JENSCH: Very well.

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MR. KARMAN: I again offer these documents in evidence. I will say at this time I am not certain we have sufficient copies for the stenographer, but we will have them when we return on Monday.

CHAIRMAN JENSCH: Is there any objection to the offer by Staff counsel? Applicant?

MR. TROSTEN: No objection, Mr. Chairman.

CHAIRMAN JENSCH: Intervenors?

MR. ROISMAN: No objection.

CHAIRMAN JENSCH: Very well. The offer in evidence as identified by Staff counsel is accepted and the documentary material to which Staff counsel referred may be incorporated in the transcript as reflecting evidence from the Regulatory Staff.

Does that complete the offer by the Staff?

MR. KARMAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Very well. Are we now ready to proceed?

MR. ROISMAN: Mr. Chairman, just one question so that I will be able to do my scheduling appropriately. Will the Board be planning to do a cross-examination of the Applicant or Staff witnesses on the Reactor Pressure Vessel subject or do you know at this time? I just want to make sure that I am not scheduling things for a time when the Board would want to be doing it.

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MR. KARMAN: As I indicated I believe on Monday, Mr. Chairman, we would be extremely grateful if the Board decides that it does need further clarification on the Pressure Vessel Report that we be given some time to have the appropriate witness present.

CHAIRMAN JENSCH: Yes. There will be some questioning but we cannot indicate how much.

MR. KARMAN: Thank you.

MR. TROSTEN: Mr. Chairman, because of the fact that we have a panel of several witnesses as I mentioned we need, we would like to have twenty-four hours' notice if we could in order to have these people present. So if you would bear that in mind we'd be very appreciative of that.

CHAIRMAN JENSCH: Yes. If you will be of assistance to the Board and if you will indicate when you are completing your other cross-examination we will know how near the subject of Pressure Vessel will be arising.

MR. TROSTEN: Well, the only direct examination, Mr. Chairman, that we presently know that we are going to-- you say complete our cross-examination?

CHAIRMAN JENSCH: On other subjects. As we move along from subject to subject so that we will know when it will be convenient to the parties to have the witnesses here on the vessel.

MR. TROSTEN: I can comment on this point.

Bl Bt3

CHAIRMAN JENSCH: Please do.

MR. TROSTEN: I have been talking to Mr. Roisman about the scheduling of further ECCS and other matters. We certainly are going to be continuing on ECCS matters today. I understand that Mr. Roisman will want to continue on ECCS matters in the early part of next week. We are tentatively discussing possibly Tuesday as a time for the State of New York witnesses to be cross-examined, although Mr. Roisman has not agreed to that at this point because of his schedule problems.

MR. TROSTEN: Applicant does intend to offer a limited amount of direct examination on plant security matters for the <u>in camera</u> session which should take a very brief period of time. We will be able to advise the Board no later than Monday morning as to the extent, if any, on the matter of ECCS insofar as the testimony to date is concerned. It's possible that we will have some redirect on that. We will certainly advise you immediately on Monday morning.

So that is about the situation at the present time, Mr. Chairman.

CHAIRMAN JENSCH: Well, the Board will better be able to indicate its position by Monday, and in any event it looks like Wednesday would be the first opportunity, but we should be able to indicate to you on Monday.

MR. TROSTEN: Thank you, Mr. Chairman.

MR. KARMAN: Mr. Chairman, I would also like for the Board and for Mr. Roisman, if it's at all possible, we are contemplating bringing with us several additional witnesses to respond to the cross-examination on ECCS. Of course, the Board has been extremely cooperative as have the parties, with other parties to this proceeding, and I would certainly expect the same treatment. And if we can get some idea when these witnesses are going to be up here, because for me to bring three or four men and for them to sit two or three days here would be, I believe would be rather wasteful for the taxpayer as well as everybody else.

We are extremely anxious to cooperate with this Board in every possible way. However, I feel we should have some definitive time for these witnesses to be aware of the fact as to when they will be on the stand.

CHAIRMAN JENSCH: Well, did I understand from a statement by Applicant's Counsel there will be some interrogation even on Monday the 8th on ECCS, Tuesday the State of New York, and I infer the plant security evidence might come in on Tuesday, too.

MR. TROSTEN: Certainly Tuesday would be fine for plant security if its satisfactory to Mr. Roisman. I gather it is not.

The only thing that I can't give you a definitive statement on at this point, Mr. Chairman, is the extent, if any, of redirect on ECCS as far as the transcript to date is concerned. But I will know that first thing Monday morning.

CHAIRMAN JENSCH: Well, could this be done. You, the Applicant and the Staff decide what the situation will be in reference to your possible redirect on ECCS and as soon as we have accommodated the State of New York, which as I understand has made a request for a specific time --

MR. TROSTEN: Yes.

CHAIRMAN JENSCH: As soon as the Tuesday performance by the State of New York is over we will take up either the redirect and ECCS or proceed immediately to the Staff ECCS.

So in any event, it would start sometime perhaps on Tuesday with the Staff evidence. Would that be agreeable to the Staff?

MR. KARMAN: That would be fine. Mr. Roisman is going to be the one who is doing most of the interrogation on this.

MR. ROISMAN: Mr. Chairman, it has been our planning and we have not yet been able to settle upon a time with the State of New York for their witnesses to come, that we would take the first four days of next week on ECCS, probably the first three with the Applicant's witnesses and the fourth with the Staff witnesses. Part of the difficulty, this is something that I am trying to get into a well-enough written form to show Mr. Karman, may be a legal problem in terms of us wanted to find out what happened at the ECCS task force meetings and Mr. Karman not wanting us to find out.

MR. KARMAN: I take exception, Mr. Chairman.

MR. ROISMAN: Maybe I understated his position.

In any case, our questions to the Staff are not the same questions that we asked to the Applicant, but primarily questions related to the manner in which the interim criteria were established, the evidence that was relied upon in setting those up and also some idea of the application of the interim policy statement to this specific case as the Staff understands it so that we will have a better idea of what these interim policy statements mean.

It is not an exceedingly lucid document, at least it doesn't appear to be to us, but Mr.Ford's availability to us is on a limited basis and that is why I wanted to do only ECCS during the first four days of next week and then on Friday move out of the ECCS into the other areas. But I understand that that may have some difficulty for the State of New York. I am going to talk to their attorney, who I understand is here, is going to be here shortly, and talk to him about that and see if we can work that out.

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CHAIRMAN JENSCH: Well, the Board has indicated before, the scheduling of witnesses, the availability of their personnel is a problem that can better be resolved among themselves. The Board is amenable to any agreeable arrangement that all the parties work out. So we will be ready and here and will be available for the presentations that are made. It will be up to the parties then to schedule these things among themselves. What changes they make will be agreeable to the Board.

The objective the Board has in mind is to have witnesses here when there is time to hear their presentations.

MR. TROSTEN: Thank you, Mr. Chairman. We will try to work it out.

CHAIRMAN JENSCH: Very well. Let's proceed.

MR. ROISMAN: Mr. Moore, I'd like to begin this morning, if you will, with some evaluation material. If we can, let us get a cataloguing of things so we will know what we are talking about later.

Can you tell me by designation the codes that are used for evaluation of the performance of the Emergency Core Cooling System and the subject area that that particular code covers for the plant? In other words, if you have one that covers your blowdown and one that covers the performance of the rods, and so forth, I will make sure I will use the right code labels. I just want a one-line description of it.

MR. MOORE: Yes. The first code is a blowdown code, our SATAN code. This code calculates the thermal hydraulic performance, the blowdown of the system. It carries out to the end of the blowdown. It carries out the transients to the end of the blowdown.

Then there is a reflood calculation which is performed for the post blowdown part of the transient as the accumulator of water, additional accumulator water is intorduced to the system and is used to calculate the flooding rate into the core.

Then there is a heat transient--

MR. ROISMAN: What is the name of that?

MR. MOORE: There is no specific name for that particular one.

MR. ROISMAN: Thank you.

MR. MOORE: Then the temperature transients are calculated with the LOCTA code. This calculates the thermal behavior of the fuel rod, using input from the previous codes.

MR. ROISMAN: With reference to the LOCTA code, this is the code that divides the core into seven regions; is that right? We have discussed it previously, I believe.

MR. MOORE: That's right.

MR. ROISMAN: Can you explain to me how precise the measurements are of the code in terms of what actually happens? Let me give you a couple of specific examples.

First, as I understand it, the regions are not geographic regions in the sense that the upper left-hand corner is one region. But rather, the regions defined by the temperature or the power distribution is what defines what the region is; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: And these are power distributions computations made on the basis of how power distribution is prior to the time of any accident. All the rods are at a certain power level at a certain point and are in a single region; is that right?

MR. MOORE: Yes.

MR. ROISMAN: The power regions, how are they? Are they even throughout? In other words, is the power distribution within a region on a specific rod--is the power distribution identical throughout the whole portion of the rod that is included in the region?

MR. MOORE: In a specific region?

MR. ROISMAN: Yes.

MR. MOORE: Yes. The power level in a specific region is constant, the same.

MR. ROISMAN: I think I didn't make it clear. Let me state it again.

It is assumed that it is the same. In point of fact, do the rods come out having power distribution exactly the same within the region? In other words, is the manufacturing, and so forth, such that you can say with certainty that the power will be exactly the same every inch of that particular rod that is within the power distribution region?

MR. MOORE: I'm sorry. Are we talking about in the reactor or in the calculation?

MR. ROISMAN: First I want to find out what the code simulates. Then I wanted to find out how closely that simulation is to what is actually true in the reactor. I am now asking the other half, what's actually true in the reactor.

MR. MOORE: I see. In the reactor there are variations in power levels between rods even within a fuel assembly. Of course, there are variations from assembly to assembly. In the reactor there are differences within any given assembly of rods.

MR. ROISMAN: In the code do you have any designation by which you identify the regions, Region 1 or Region A or something like that?

MR. MOORE: No. These are not representative of physical regions, as I indicated earlier.

MR. ROISMAN: I just wanted to get a basis so we can talk about a particular region based upon the characteristics that are simulated for the code. Can you call them Region 1?

MR. MOORE: Fine.

MR. ROISMAN: In Region 1, let's assume it is the one that has the highest power density. What is the power density for all of the portions of rods in Region 1?

MR. MOORE: For Indian Point 2 it is 17.4 kilowatts per foot.

MR. ROISMAN: Is that a power density that exists over the entire length of a rod or only over a portion?

MR. MOORE: That exists over the one-seventh axial portion in the calculation.

MR. ROISMAN: In turning from the code to the rod itself, focusing your attention on any one rod in the core that has a section with 17.4 kilowatts per foot power density.

MR. MOORE: Yes.

MR. ROISMAN: In the one-seventh of the rod is the power density exactly 17.4 kilowatts per foot for the entire one-seventh?

MR. MOORE: No, it would not be exactly.

MR. ROISMAN: Can you give me an idea of what the range is?

MR. MOORE: I would say it could vary several percent along that length in power. What we do, we have really simulated the hottest spot in the whole core and arbitrarily assumed that that hottest spot which we don't physically expect to exist does in fact exist over that whole length, that one-seventh length.

MR. ROISMAN: I know there are seven regions. Are they actually equal in length?

MR. MOORE: In the axial simulation they are equal in length.

MR. ROISMAN: So one-seventh of the rod has two or three percent variance?

MR. MOORE: I'd say a few percent, two or three, along that length probably.

MR. ROISMAN: Those variances are inherent -- Manufacturing processes can't provide you with anything more precise than that; is that correct? Or is it something that happens during burn-up that changes it?

MR. MOORE: These are just typical variations associated with the power distribution in the core, the neutron flux distribution.

MR. ROISMAN: Does anything happen during the operation of the core which would alter this, during normal operation of the core?

MR. MOORE: That would alter these kinds of variances, for example?

MR. ROISMAN: The power density of the one-seventh portion of the rod.

MR. MOORE: Yes, there are variations in power density with operation, if that's your question.

MR. ROISMAN: Just taking the rod that started off with 17.4 plus or minus a few percent of its one-seventh length, six months later what would the power density be expected to be for that same one-seventh length? The same, higher, lower? Would a variation within those seven lengths be different?

MR. MOORE: All of the above. It could be any specific part of the core that may have a different power level depending on, of course, what power level you are operating at, full power, reduced power, and depending where control rods are located at the time. So we take the maximum condition that could occur any time in the operation, any time in the life of the core.

MR. ROISMAN: Nothing happens to the rods during the course of their use such that the maximum could be any higher?

MR. MOORE: That's correct. That's how we determine the 17.4

MR. ROISMAN: When you are computing under the code the temperature, you compute the temperature for this one-seventh region, is that correct?

MR. MOORE: That's correct.

MR. ROISMAN: And again, you assume that the entire region has whatever the temperature would be at the hottest pinpoint in the region; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: You also add into the heat of the rod the heat of the metal-water reactions that are considered; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: How is that heat added in? Is all of the metal-water reaction heat assumed to affect the single point, or do you take the metal-water reaction heat from a point and spread it over the entire one-seventh length of the rod?

MR. MOORE: Well, the metal-water reaction energy would be a per unit length basis. So it would be distributed evenly through that whole region as each gram of zirc would react. That gram at that location would give up so much energy.

MR. ROISMAN: Let's talk about the real rod rather than the simulated rod for a moment.

MR. MOORE: Yes.

MR. ROISMAN: We will focus on that portion of it that is one-seventh that has the hottest spot in it. There is a spot on there which may well be hotter than the others. It is that hot spot that you use to determine the heat for the whole rod for calculational purposes, but in fact, if you could go into the core with a little thermometer, you will find there will be variations within the one-seventh, and you have tried to pick the highest; is that right?

MR. MOORE: Yes.

MR. ROISMAN: I'd like to focus your attention on the highest point in reality that is actually there in the rod. If a metal-water reaction should occur at that point, does your code show how that temperature increases based upon the temperature increase from the metal-water reaction on the point, or does the code take the metal-water reaction and take its heat and spread the heat over the entire one-seventh? In other words, if you will dilute the heat contribution.

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MR. MOORE: I understand. No, it does not. The heat that's generated at the hot spot is all added to the cladding at the hot spot.

MR. ROISMAN: And then the code assumes that that same metal water reaction has happened for the entire one-seventh.

MR. MOORE: That's correct. That's the reason we overpredict the metal-water reaction for the calculation, because we are really assuming the hot spot actually exists over one-seventh of the rod rather than at a LOCA point.

MR. ROISMAN: In the code itself is the calculation able to say what the temperature is at a specific point within the region, or is the code geared to only tell you what the temperature is at what you calculate will be the hottest point in the region, and you assume it for the whole region?

MR. MOORE: Well, the code is calculating the temperature of a total region. Then as I have indicated earlier, I think, the temperature of all the cladding in that region willbe the same because the assumptions for that region are the same. The power generation over that whole length of rod is the same. So you will get the same temperature in that particular region.

MR. ROISMAN: If in the region there is evident which at one point along one-seventh of the rod, at that point would be more severe than it would be at another point along the same one-seventh of the rod, how does the code take a count of the fact that the events may actually differ in reality from the assumption? What do you do to take a count of that?

MR. MOORE: Well, for example, for the hot spot that's the reason we assume the power level for the hot spot to exist throughout the region so that there would not be any point in the region that would be at a higher power level than we have assumed.

As I mentioned, the power level may be less, will be less on either side of the hot spot. No credit was taken for that. We assume the whole region is at a hot spot.

MR. ROISMAN: Is it always conservative to assume the highest power density?

MR. MOORE: With respect to the limits that we are talking about, peak clad temperatures, yes.

MR. ROISMAN: Nothing, for instance, in terms of rod bowing or swelling or bursting which would be in any way changed by having it be at a lower power density and have it be more severe in terms of more flow blockage or something of that nature?

MR. MOORE: No. We are talking about the calculation of peak temperatures. If we discuss effects on deformation of blockage and so forth, then there are effects of heating rate and power levels and so forth, and that is a separate topic, and which was really covered through the testing we did in the multi-rod burst tests, which had different power levels and rates.

The code we are talking about is not a code that calculates deformation or blockage.

MR. ROISMAN: Which code is the one that does not?

MR. MOORE: As I have indicated in earlier testimony, we don't calculate deformation and blockage. We rely on actual experimental data.

MR. ROISMAN: Well you mean you simply add in the effect of the, is it fifty per cent flow of blockage that you assume in the hot regions?

MR. MOORE: Yes. As indicated earlier for the fuel assembly, you take the hot assembly and assume fifty per cent flow blockage for that analysis.

MR. ROISMAN: And then that just becomes a data point or a reference point in your reflood calculations? Is that where it comes into the whole picture?

MR. MOORE: Where what comes in?

MR. ROISMAN: The amount of flow blockage.

MR. MOORE: No. The calculations that we perform, the design calculations, are performed without consideration of clad deformation. We have done a separate calculation which is reported in Volume 2 of, I forget the reference, 7495, I believe, the multi-rod burst Volume 2, shows a calculation of the temperature effect of distortion, and there the analysis shows the maximum increase in temperature that is expected or that was calculated in that case was 70 degrees Fahrenheit. And that is where we indicated that we fully expect this effective deformation to be less than 100 degrees.

MR. ROISMAN: What I was asking was when you do your analysis of the emergency core cooling system performance where does the 70 or up to a hundred per cent temperature increase come into the analysis. For instance, you come up with a figure of 2300 or I think it is on one 2300 degrees Fahrenheit. Where in computing the 2300 degrees Fahrenheit did the 70 or 100 degree increase in temperature due to flow blockage get added into that whole formula?

MR. MOORE: It is not in directly.

MR. ROISMAN: Does that mean that if when you add flow blockage in the maximum clad temperatures would be 2370 degrees to 2400 degrees Fahrenheit?

MR. MOORE: I would have to say a qualified yes, because it depends on how you calculate the effects of blockage.

As indicated earlier, I believe it was in Monday's testimony, the analysis presented in the Volume 2 was a very conservative one in the fact that we calculated the effects of blockage and rod distortion. But we did not incorporate the beneficial effects of blockage with respect to heat transfer. That was obtained in the FLECHT test that we discussed earlier with blockage.

I am qualifying this on the basis of a very conservative approach. The temperature could be 70 degrees higher.

MR. ROISMAN: Let me just see if I understand the mechanism that's been done. Without regard to the question of flow blockage, youhave computed what the maximum temperature would be at the hot spot and found in the worst case that it would be 2300 degrees Fahrenheit. Then in some experimental tests, multi-rod burst tests, you attempted to find out how whatever maximum clad temperature you come up with would be affected by the problem of flow blockage. Those tests showed that the worst situation would be a 70 to a hundred-degree temperature increase, disregarding any beneficial effects that might come from flow blockage, and based on that you simply said, "well, this is small enough that we don't have to even go into the question of considering the beneficial side of it. We just sort of scope the worst parameter and we consider that to be something that we can live with," and then in a sense it doesn't actually show up in the 2300 degree Fahrenheit figure. It just increased your confidence with regard to the 2300 degree Fahrenheit figure.

Is that an accurate statement of what went on?

MR. MOORE: You said that very well, yes.

MR. ROISMAN: Thank you. I just wanted to make sure that I understood what happened.

So that we don't have a situation in which the very same one-seventh portion of the rod in making your calculation of the 2300 degree Fahrenheit temperature has conflicting assumptions made about it in different portions of different codes.

For instance, you said for one kind of assumption it might be the worst case to assume that the power density was the highest. For another kind of assumption it might be the worst case to assume that the power density was lower. In your analysis you don't add together all the worst assumptions without regard to whether they contradict each other and apply them to this one-seventh region in order to see what would happen to it, is that correct?

MR. MOORE: No. In fact quite the opposite. That's what we did do. We discussed earlier that the blockage can be affected by internal pressures and heating rates and so forth. The maximum blockage tends to be at low internal pressures and low heating rates as observed from the tests. This would not be a characteristic, for example of the hot spot. However, we took the blockage that we obtained, the maximum blockage that we obtained, and the maximum rod-to-rod contact we obtained from the multi-rod test independent of the specific power density. We just took the worst that we obtained and we used that and assumed that that was the case for the hot spot.

So that the calculation performed in Volume 2 of the multi-rod burst test calculates the effects of the worst blockage, independent of heating rate, power level, et cetera, applied directly to the hot spot. So we have a contradiction here in the way that we have got a conservative approach.

MR. ROISMAN: Can you explain to me how you take account of the interaction that flow blockage would have on the temperature of the core for the purposes of computing in the LOCTA code, what the temperatures would be? For instance, blockage over on assembly here with an assembly adjacent to it that has less or more blockage, and how the presence of blockage in the one assembly affects what happens in the other assembly. Where does that enter into your computations?

MR. MOORE: That was also calculated as part of the same analysis where we were calculating the effect of blockage on peak temperature. And as indicated we take the maximum blockage obtained from the test and apply this to a whole assembly, one fuel assembly, which contains the hot spot. So we take the hot fuel assembly and then we take the adjacent assemblies and assume that they are not blocked at all. And what this does then is overpredicts the amount of flow redistribution that you will get from the blocked assembly, because the neighboring assemblies are unblocked.

So we calculate the flow redistribution now that occurs in the blocked assembly because it is blocked with reference to its neighbors.

MR. ROISMAN: You say you calculated it. How do you know how much will flow into the unblocked adjacent assembly and how much will flow through the blocked assembly?

MR. MOORE: This is a thermal hydraulic calculation. It's performed with our thermal hydraulic code called the THINC code. That's T-H-I-N-C. This is also referenced in Volume II.

MR. ROISMAN: Those are Westinghouse codes, is that correct, the THINC 1 and 2 codes?

MR. MOORE: That's correct.

MR. ROISMAN: Now do those codes have any experimental background? In other words, there are a set of experiments that have been run to verify the calculations and so forth used in the codes.

MR. MOORE: Yes.

MR. ROISMAN: Where are those reported?

MR. MOORE: They are reported in various Westinghouse topical reports.

MR. ROISMAN: Would you be able to give me the ones they are?

MR. MOORE: Yes, I could. I couldn't give you the references right now, but I certainly could.

MR. ROISMAN: Maybe you could give them to me at the break, if you would, so that I could have a listing of those.

Without the numbers, can you discuss at all a little bit about what kinds of experiments were done? I mean are you familiar with the experiments generally even though we can't pin them down to a WCAP number?

MR. MOORE: Yes. These were experiments of blocking flow tests through fuel assembly rod bundles. These bundles had -- We had, for example, several fuel assemblies next to each other and then predict the kinds of -- These were with different power levels for the assemblies, and then predict the kind of flow redistribution that you would expect within or from assembly to assembly and also within assemblies, and then experimentally measure these and compare them to the calculations. There is also some degree of in-reactor evidence, too, to support the calculations in that we measure with thermocouples in a reactor the exit temperatures in various fuel assemblies and these exit temperatures vary because of power distribution within the core. But also because the full redistribution that takes place as from the hot assembly to the cold assemblies, and we calculate these with this code and then can confirm this with the actual temperature measurements in the reactor.

MR. ROISMAN: What you were just talking about, the experiments that were run in actual operating reactors, those were run without any flood redistribution. This was merely finding out how much you get flow redistribution merely because of the power density differences, is that correct?

MR. MOORE: Yes. That's correct. What we are confirming though is the ability of these codes, of this particular code, to calculate the flow redistribution that occurs due to differing pressure drops from assembly to assembly. And also the out-of-pile test had to again show you the flow distribution to the different pressure drops from one assembly to another.

I believe there were some tests performed where we actually forced different pressure drops within assemblies not just by power changes but by physically having higher resistances and then predict, have the code predict what this mass transfer will be as a function of the pressure drop. So you are confirming the basic equations in the code.

MR. ROISMAN: In terms of your understanding of what the data shows would the flow redistribution be affected in a situation which you have a group of rods all roughly the same power density, roughly the same temperature and pressure, except that for five or six in the center of a larger group at one point halfway up they were all 500 degrees hotter at that one point than was the rest of the rod above or below them. In other words, does the concentration of the temperature difference at a specific point as opposed to a broader range for the same, for a temperature difference, affect the flow redistribution?

MR. MOORE: Not directly. This would be through the heating up of the water in the vicinity of this hot location that you have postulated. Actually heating the water up at that point would then create a higher pressure drop, tend to create a higher pressure drop as the water expands, and the fundamental point here is we have an open lattice core which tends to have a similar pressure over any given axial, or excuse me, any given plane in the core. So that the pressure drop would try to increase in the assembly where you put the hot spots and then this would tend to push flow into the other assemblies. But it's in through the heating up of the water.

MR. ROISMAN: Did the experiments that were done in-core, were they able to simulate the kind of temperature difference that one -- Temperature and pressure differences, the range of differences and types of concentrations that would occur in the event of a loss of coolant accident?

MR. MOORE: Yes, in the sense that there were varying temperatures along the lengths of the rods associated with the power distributions that occurred in the reactor which are then typical of the power distributions that occurred in the core during blowdown. I don't want to overemphasize the applicability of the inter-reactor tests to the loss of coolant situation.

Clearly, we don't have the same kinds of quality conditions in the core under normal operations. There is experimental data out of pile taken for high void fractions which are representative of the loss of coolant situation to confirm the calculations in that range.

MR. ROISMAN: Can you briefly describe those?

MR. MOORE: Well, these were just cases where we heated up, and we had high power assembly and low subcooling -- High temperature water injected into the bottom of the assembly so we got a significant amount of steam generation and void fraction along the length of the assembly. So this gave us larger flow rate distributions. They were checked by the actual code itself.

MR. ROISMAN: But did they give you what we call localized hot spots along the rods?

MR. MOORE: They were power distributions typical of the reactor. They were not all uniform power distributions.

MR. ROISMAN: But typical of what you have in the reactor during its normal operation?

MR. MOORE: That's also typical of what I have in the reactor during the loss of coolant.

MR. ROISMAN: For the purposes of power distribution?

MR. MOORE: Yes.

MR. ROISMAN: But what about in terms of temperature? Do you find, for instance, if you have a rod that is divided into seven regions, and if the power distribution in the rod under normal operations, just for our purposes ranges from 16.3 to 17.4, gradually moving from the end to the middle and then back down to the other end again, do you find that same level of gradient in the course of a loss of coolant accident? Not in terms of power distribution but in terms of actually in the rod. Some of the portions of that rod because of various events that occur in the loss of coolant accident, tend to get hotter than other portions such that the curve would be more peaked at some point and not as gradual as it is in normal operation.

MR. MOORE: No. The temperature distribution within the rod follows very closely the power distribution.

MR. ROISMAN: In other words, the fact that some portions of the rod will reach critical temperatures for swelling or bursting or for metal-water reaction won't, as the result in the swelling case, of contact with an adjacent rod or in the case of metal-water reaction, the addition of heat won't cause the temperature gradient along the rod to be more peaked than it would be if you just followed what the power density was along the rod?

MR. MOORE: No. There could be some peaks in temperature gradients associated with rod-to-rod contact, as we calculate in the report.

CHAIRMAN JENSCH: Excuse me. Maybe that last statement does answer the question. There was a question given, did you get localized hot spots under rod. I think you may have anticipated with might be an inference if you answered yes or no. You gave the explanation. Give us which way it was. Do you get localized hot spots on the rod? Do you recall that question?

MR. ROISMAN: Yes.

MR. MOORE: My jumping back and forth is with respect to the calculation without distortion, the calculation with distortion and the reactor with distortion.

MR. ROISMAN: I had meant it in the case of the axial situation.

MR. MOORE: The answer was yes.

CHAIRMAN JENSCH: Thank you.

MR. ROISMAN: During the flow redistribution analysis, did that flow redistribution analysis -- The experiments. Did those experiments have these temperature peaks in them?

MR. MOORE: I believe we simulated conditions that would be representative of that. I would have to, in this case, check the specific data.

MR. ROISMAN: Would you do that maybe when you come back with the WCAP reports that verify the codes you can discuss in some more detail with the experiments of flow redistribution in that event?

MR. MOORE: Yes.

MR. ROISMAN: I believe it is the SATAN code that you said is the one that predicts what happens during the blowdown; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: Does it include an analysis of the effect of the blowdown on the rods themselves? That is to what extent they are deformed or disturbed or anything by the blowdown forces. Is that part of what is in the safety code?

MR. MOORE: No.

MR. ROISMAN: Is there an analysis done of that code or calculations done of that code?

MR. MOORE: Yes.

MR. ROISMAN: Which one is that?

MR. MOORE: That's with the BLODWN Code.

MR. ROISMAN: Are you responsible for that, sir? Let's start at the very beginning. What holds the rods into fuel assemblies in normal operation? Are they bolted into the assembly?

MR. MOORE: No. They are held in place with the grids through the springs on the grids.

MR. ROISMAN: How much force are the springs applying against the side of the rod? I don't need an exact number, just a rough idea.

MR. MOORE: The total force on the rod is due to the springs, holding the springs, about 125 pounds.

MR. ROISMAN: What holds the rod to the top or bottom?

MR. MOORE: They rest on the bottom and are held by the springs.

MR. ROISMAN: Is there anything above them?

MR. MOORE: No, not holding them.

MR. ROISMAN: In the BLODWN Code, what experiments have been done to determine what the value should be of the pressure in force on those rods during the course of blowdown?

MR. MOORE: Well, we use the BLODWN Code, which has been checked against several different blowdown experiments, specifically some of the early semi-scale experiments, run in Idaho, also some experiments that have been run at the containment systems experiment out at Battelle Northwest Laboratories.

MR. ROISMAN: In these tests that were run, you mention the semi-scale test. Was that the same reactor that was involved in the semi-scale tests run 845 to 851 that dealt with the ECCS performance?

MR. MOORE: Yes, it was the same arrangement.

MR. ROISMAN: Basically the same piece of instrument, just a different test was run using that instrument?

MR. MOORE: It is all part of the same series of tests. I just said we applied the BLODWN Code and predicted the responses using blowdown as an experimental check on the BLODWN Code.

MR. ROISMAN: Can you briefly describe to me -- For instance, I gather that the reactor has smaller rods than the rods that are used in this --

MR. MOORE: I'm sorry. I was going to interrupt. There is a misunderstanding. I said the code itself was checked against these blowdown experiments, not the forces on individual rods in the semi-scale experiments.

MR. ROISMAN: What was it that was being checked of the code?

MR. MOORE: Just the thermohydraulic behavior and predictions of pressure versus time, and flows versus time, for example.

MR. ROISMAN: Were the semi-scale tests run with rods in the core?

MR. MOORE: There were some tests run with heated rods, yes, but there were no measurements of forces on those rods, to my knowledge.

MR. ROISMAN: But I was curious about it in terms of predicting how much force there would be, what account was taken of a filled core with rods in it as opposed to just an empty void.

MR. MOORE: The analysis has been performed. In both cases they ran some tests without a core and some tests with a core.

MR. ROISMAN: When they ran it with a core, did it simulate the outer configuration of the westinghouse core in the sense that it was the ratio of space between the last rod or the wall of the reactor, and between the top rod or the top of the reactor and the bottom rod, the bottom of the reactor, and the distances between the rods scaled to what you have in the Indian Point reactor?

MR. MOORE: No.

MR. ROISMAN: Were the rods themselves scaled in size so that the size of the reactors -- If the reactor was one-tenth the size of your reactor, the rods were one-tenth as round in circumference as your rods?

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

MR. ROISMAN: What about the length?

MR. MOORE: Well, no. Obviously it wasn't.

MR. ROISMAN: I meant the ratio.

MR. MOORE: No.

MR. ROISMAN: How about the method by which the rods were held?

MR. MOORE: No, no specific correlation to a reactor.

MR. ROISMAN: In the code -- I'm sorry.

MR. MOORE: I was going to comment that the other series of tests I mentioned were the containment systems experiment systems at Battelle Northwest Laboratory where they had a vessel containing a simulated reactor core again just to simulate the loadings on a core. This was not an exact representation of a core, but it was similar kind of geometry that you get in a core. They did blowdown experiments there and actually measured the forces on these internals.

MR. ROISMAN: You mean they measured how much force was applied to a specific rod at a specific point?

MR. MOORE: I'm not sure now whether they measured them specifically on rods, how they simulated the core. They measured them on various internals, core barrel and plates and that sort of thing.

MR. ROISMAN: The thing of interest to me is, at least as I understand it, the rods are standing vertical in the core. They aren't bolted to anything but are held by the pressure of the inconel spring, and the bottom of the rod is supported against some kind of a plate or something like that, and the top is open. I am trying to figure out how you go about determining whether or not that rod can be ripped out of there by force. I take it that assuming the head were off the reactor, that one could, with equipment with the appropriate type of grapple, reach down and physically pull the rod up without actually undoing any bolt or screw or anything like that, and just slide it up between the springs; is that right?

MR. MOORE: You have to pull pretty hard.

MR. ROISMAN: I understand. What I meant, it is a fraction rather than a bolt that would have to break before one could pull it up. I am just trying to understand how the forces during blowdown might operate to dislodge or dislocate that rod. That's why I am talking about the experiments that were run.

You mention the ones at Battelle. Did they have rods that were held in the same fashion that the Westinghouse rods are held in their core?

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

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MR. ROISMAN: In determining the kinds of forces, what were they looking at? In other words, were they just trying to see how much force would hit the rod or do they concentrate on the direction in which the force would hit the rod or the point at which the force would hit the rod? How was the measurement done?

MR. MOORE: Well, the purpose of the test was to determine whether you could accurately reliably calculate the decompression forces that are obtained after a loss of coolant early in the transient, and the second time period, and also the subsequent loads to the flow effect, and see whether you could reliable determine pressures on different physical geometries under this particular kind of a transient. What they did is run these blowdown tests and compare then the predicted loadings on these components using, in their case--in their case they used the WHAM code, which is really the early version of the blowdown code and so that in fact the predictions of the forces obtained--prediction of the forces were higher than what they actually obtained.

So my point is that this is a confirmation of the ability to calculate these decompression forces. The actual application of these forces to a fuel rod is not very exotic. It is merely taking these forces that you calculate with code and applying them to the rod itself.

MR. ROISMAN: But doesn't it make a difference of

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the direction of the force? For instance, I assume your rods are capable of taking substantially more force from the top down since they are supported on the bottom by a plate which itself is welded or screwed or something into a larger grid which is attached, and so forth, then they are too if you take the same force and if somehow or other you push it straight up. I'm trying to find out, how do you know the direction the force is going to take?

MR. MOORE: You calculate the direction of the forces. This is all part of the analysis, that the main forces that act on the rod are just the differential pressures across the rod, along the length of the core, acting on the rod in the axial direction. Those forces in fact are not enough during blowdown to override the friction forces of these springs of the grids.

MR. ROISMAN: In terms of calculating the force directions, what experiments, of the ones we have talked about, or others, were there to verify what would be the direction of the force?

MR. MOORE: Any of these calculations are forces on components involved determining the directionality of these forces. When you make a measurement, you have to know what the direction of the forces you predict are, and how they are applied, and actually measure the force. So this is a check on getting directionality of the forces.

MR. ROISMAN: How do you know the direction of the

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force for a reactor such as we have in Indian Point No. 2 if you tested the direction of the force on a reactor that was constructed differently both inside and outside?

MR. MOORE: Because I know the direction of the flow.

MR. ROISMAN: It doesn't matter how large the reactor is? If you know the flow is going from the lower right-hand corner to the upper corner or what is in between, the forces don't turn corners, or anything like that?

MR. MOORE: The forces are in the direction of the flow.

MR. ROISMAN: In terms of the forces that would operate on the rods in this reactor, what is the calculated force in the worst situation that is predicted against a rod?

MR. MOORE: I believe the maximum force on the fuel rod itself is in the order of 2230 pounds total for the rod, for each rod.

MR. ROISMAN: In what direction is that?

MR. MOORE: The limiting situation is the hot leg break which is causing it to tending to lift the rod in the fuel assembly. So they are in the upward direction.

MR. ROISMAN: Are all the rods getting 2230 or is there a worse rod in the whoe core that gets 2230?

MR. MOORE: Essentially all the rods are getting 2230 pounds, total force.

MR. ROISMAN: Does it all come on the rod at essentially the same moment? In other words, it doesn't get buffeted by a total of 2230 pounds of force?

MR. MOORE: This, what I am giving you, is the peak force.

MR. ROISMAN: For how long a time are the rods subjected?

MR. MOORE: This is in the millisecond.

MR. ROISMAN: Is the force direction vertical? You said it tends to be upward.

MR. MOORE: Yes.

MR. ROISMAN: Is it actually vertical or a little off to one side or the other?

MR. MOORE: It is vertical.

MR. ROISMAN: So the rods, in your analysis, there is no analysis, if any, of horizontal force hitting these rods? I mean that might tend to bend them as opposed to lifting them out of their assemblies.

MR. MOORE: The lateral forces are also calculated on the rod and these are shown to be quite small. The effects on a possible damage or distortion of the rods in the later direction, cross-flow, is very small.

MR. ROISMAN: I have some difficulty in conceptualizing what force looks like, but I want to ask you some questions based on the assumption that we can see what it looks like. I take it that the force direction is subject to change. For instance, if you had a steel plate in the channel where the force was coming up in a vertical direction and the force hit that steel plate, could the force's direction be altered? Is that correct? Then the force would be applied sideways rather than vertically.

MR. MOORE: The effect of water hitting the plate, water impinging on a plate vertically, would be a force in a vertical direction.

MR. ROISMAN: Is there a secondary force? Instead of going through the plate it goes someplace. Instead of going through the plate where does it go or where would it go in that kind of effect?

MR. MOORE: There is a vector contribution as the flow changes direction, which could give you other forces in a non-vertical direction.

MR. ROISMAN: As I understood from the drawings that were up on the board on previous days the inconel spring angles up to the point of contact with the rod and then angles away from it above. Have you computed what the forces on the rod from the -- Did you call it vector? I called it the ricochet, just so I -- I'm seeing this as billiard balls, you understand.

MR. MOORE: I see.

MR. ROISMAN: The ricochet off of the inconel spring to the rod itself, is that computed in the code or have experiments been made to test what that force would be?

MR. MOORE: That's computed as a force through the -- A resistance effect, and the flow going through the grid would create a force acting on the grid. Yes, that's incorporated. It's incorporated as an empirical relationship based on experiment.

MR. ROISMAN: Well in particular now, let's take the specific rod and it's got its little spring over here on one side and a force of twenty to thirty pounds I believe you said is coming up the channel and it strikes the lower end of that spring and then it tends to ricochet along the spring to the point where the spring contacts the rod. We already know that the spring contacts that rod with, I believe you said sixty is it?

MR. MOORE: 125 pounds total per --

MR. ROISMAN: How much is it at that point for that one spring roughly? The reason I mentioned sixty, I think Mr. Wiesemann had told me that he thought it was sixty. I don't mean he told me on the record, but off the record.

All right. In any case, whatever the figure is can you tell me how much more the pressure will be at that point as the result of the ricochet of this force off of the side of the inconel spring?

MR. MOORE: Well, no. That's not the -- That's really not the significant effect we are talking about. The force that I indicated acting on the rod in the axial direction is one that's created by the differential pressure from the bottom of the rod as the coolant's coming in to the top of the rod for the hot leg break. And that force when you look at the total rod was the number of twenty to thirty pounds acting on the fuel rod itself.

Now the grids are holding the rod at each elevation and the total holding force on the rod is this 125 pounds. That's just looking at the rod.

So the blowdown forces on the rod then indicate the rod will not slip in the grid, because all these grids are holding it for the force much greater than that acting on the rod itself.

Now when you talk about the forces do you, due to the grids, this is a force acting on the total fuel assembly, and you have to calculate what happens to the fuel assembly itself now where the grids contain these fuel rods. And we do calculate then the forces acting on the total assembly which are tending to lift this assembly during the loss of coolant, the hot leg break.

MR. ROISMAN: I'm trying to look at one rod. What I am trying to find out is, if you know, what happens at that point on the rod. How much pressure is applied to that point?

MR. MOORE: The frictional forces along the length of the rod are trivial. I mean they are very small.

MR. ROISMAN: No. This isn't a frictional force. I am talking about the ricochet of the twenty to thirty pounds of force that was going in a vertical direction and now hit.

MR. MOORE: It's acting on the grid, the grid itself, and it will tend to lift the grid which is carrying the rods. There is no force on the rod per se associated with that.

MR. ROISMAN: In other words, nothing slides along the inconel spring like a droplet of water or something like that at a certain rate of speed and strikes the rod?

MR. MOORE: No, no.

MR. ROISMAN: If there were BB's in the bottom of the core, just assume you have got a bunch of BB's down there and they just lie down in the bottom of the lower plenum and now we have a loss of coolant accident, would the BB's be thrown up if it was the hot leg break and would they go flying through the core on the way out where the break occurs?

MR. MOORE: Yes. Depending on the size and the mass of the BB's.

MR. ROISMAN: Right, okey. We are talking about little BB's here.

Now if one of those BB's were to hit an inconel spring on its way up would the BB ricochet off the inconel spring and could it hit the rod?

MR. MOORE: Yes.

MR. ROISMAN: Why aren't the water droplets similar or is it that there are no water droplets?

MR. MOORE: This is a continuum of water in the core. There is no water droplets per se that we are talking about.

MR. ROISMAN: During normal operation of the core does the inconel spring and the rod tend to swell slightly as a result of heat? Is there any expansion at all, either or both?

MR. MOORE: Well, during normal operation there are heat effects in expansion of the rod, yes.

MR. ROISMAN: What about in the inconel spring?

MR. MOORE: This is absorbed by variations in the spring force.

MR. ROISMAN: But all I am saying is that the inconel spring also is affected by the heat.

MR. MOORE: Yes.

MR. ROISMAN: It expands.

MR. MOORE: Yes.

MR. ROISMAN: Is the coefficient of expansion for the material in the spring and the coefficient of expansion for the material in the rod the same?

MR. MOORE: I don't recall the specific values for the coefficients of expansion.

MR. ROISMAN: In computing the amount of force that's holding the rod in place in the event of a blowdown is there any computation that takes account of varying coefficients of expansion, in this case coefficients of contraction, if the temperature of the rod is cooling or heating up during the blowdown period, whichever way it's going? Is there anything that takes account of those?

MR. MOORE: No, because I guess maybe I didn't make it clear, the maximum loads that occur during loss of coolant on the rods occur in a time period of less than fifty milliseconds. There is no heat variation or temperature variation off the rods at all during this period of time. They are at their full power conditions.

MR. ROISMAN: At that point the power of the rods does not cease to operate?

MR. MOORE: No, no. Milliseconds we are talking about.

MR. ROISMAN: I understand that.

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MR. ROISMAN: You have run tests like the FLECHT test designed to determine what the heat transfer would be for the rods in the event of a loss of coolant accident, is that correct?

MR. MOORE: Yes, sir.

MR. ROISMAN: That goes with the primary tests that were run in order to determine--

MR. MOORE: Those were the primary tests that were run to determine heat transfer during the reflood part of the transient, right?

MR. ROISMAN: Now when those tests were run did they have tacked onto the front of them a simulated blowdown?

MR. MOORE: No.

MR. ROISMAN: So that in the case of a FLECHT test we begin with what you presume the conditions of the rods would be after blowdown and in terms of the amount of heat they got, the physical condition which they were in, and begin computing the heat transfer at that point, is that correct?

MR. MOORE: That's correct.

MR. ROISMAN: I think yesterday you talked about the rods that were used in there. Are those the only stainless steel rods that were used in the FLECHT test or were there also zircalloy?

MR. MOORE: No. We also ran zircalloy.

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MR. ROISMAN: Which group of the tests were the zircalloy?

MR. MOORE: The later group were the zircalloy.

MR. ROISMAN: Three.

MR. MOORE: The two reports that we have had up until now were the Group 1 and Group 2 tests.

Well, you should have the WCAP 665.

MR. ROISMAN: No, we did not have it. I mean we are getting it.

MR. MOORE: That's the report that Mr. Ford was--

MR.ROISMAN: It's Mr. Novak's report.

MR. MOORE: I'm sorry.

MR. ROISMAN: He was kind enough to let us use it, but we haven't had a chance to study it.

MR. MOORE: Yes. In that report the Group 3 zircalloy tests are reported.

MR. ROISMAN: Okay. Now go back a second to the blowdown situation. Are there different kinds of forces that are operating inside of the core when blowdown occurs. When I say different kinds, in other words I take it there is a force associated with the water trying to get out of there. Is that a different force than the force associated with the water flashing to steam and other varying forces associated with the blowdown?

MR. MOORE: These are all hydraulic loadings associated with the acceleration of the fluid in the core and it's being expanded and expelled out through the break.

MR. ROISMAN: Maybe you can help me. I have heard the term "water hammer". What does that refer to? Is that one sort of label for one of the forces?

MR. MOORE: There is an initial decompression force which acts during the initial blowdown or when the coolant is sub cooled, that is sometimes--that is a different kind of a loading than the actual hydraulic loading of the flow effects themselves.

MR. ROISMAN: Can you tell me what kind of a loading is it? What actually happens?

MR. MOORE: The loading that occurs, this decompression occurs in this millisecond. And the differential pressure associated with that decompression is the main contributor to the loading in the fuel rod. The maximum loading on the fuel rod. The effects on the fuel assembly primarily associated with the flow forces which act on the, mainly on the grids, as I was discussing earlier, there are two kinds of loadings and it's the decompression loading which gives the primary load on the fuel rod itself.

That's again in the hot leg break, it's in the direction, the axial, along the length of the rod, the difference in pressure across the core.

MR. ROISMAN: That's a term that I hear and my problem is that phrase "difference of pressure across the core" and I'm afraid I am not visualizing it right. By across do you mean--

MR. MOORE: From top to bottom.

MR. ROISMAN: Okay. Thank you. I don't know why, but I think of across as being that way.

You say one is the force that acts primarily on the grids and one is the force that acts primarily on the rods?

MR. MOORE: Yes.

MR. ROISMAN: Can you explain to me how do you mean, it acts on the rods but doesn't act on the grid or it acts on the grid and it doesn't act on the rods?

MR. MOORE: It's a differential pressure across the core which acts on the cross-sectional acrea of a rod. Okay. Pushing on the rod, the cross section of the rod.

Now the forces on the grids are associated with the flow passing the grid and the friction forces on the grid and the flow is passing by drained by the grids tends to push the grids up. These friction forces on the rod itself are very small.

MR. ROISMAN: You mean that the force, the first force, the one that's on the rod, is a force that takes place inside the rod?

MR. MOORE: It's just acting on the rod. It's a difference in pressure at the bottom of the rod and the top of the rod.

MR. ROISMAN: On its outside you mean?

MR. MOORE: Yes.

MR. ROISMAN: Okay, all right.

And what is that force?

MR. MOORE: That force--

MR. ROISMAN: I mean what is it in pounds?

MR. MOORE: That's the one, the 2230 pounds force.

MR. ROISMAN: I see. And the other force is the one that has very little friction effect on the rods but effects the grids, how much is that?

MR. MOORE: That's a total of 500 to 600 pounds per assembly. I am talking about the whole assembly.

MR. ROISMAN: Has it been computed for specific springs in the assembly?

MR. MOORE: Yes. The resistance of the grids are very well known, using our grids.

MR. ROISMAN: No, I am sorry. That's a force. You said it's 500 pounds on the whole assembly.

MR. MOORE: That's adding up the effect on each spring.

MR. ROISMAN: In other words, you do know what it is as to each spring, so to speak?

MR. MOORE: Oh, yes, yes.

MR. ROISMAN: Was what I call the water hammer, the force that operates on the rod itself, was that also tested in

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the experiments at Battelle or in the semi-scale tests?

MR. MOORE: Yes. That was the primary purpose of those experiments.

MR. ROISMAN: Was that a particular part of it?

MR. MOORE: Yes.

MR. ROISMAN: Is the computation of those forces affected by the relative size of the rate to the size of the area of the reactor? In other words, do you get different forces when you assume the break of a five-inch line or assume the break of a double-ended pipe?

By what means is the test from a non-identical reactor to the Indian Point reactor translated into the computation of the forces here? Is it assumed to be just a straight line difference, that that core was one-half the size of the other one, then you will get twice as much force here as you would there?

MR. MOORE: No. You specifically calculate the conditions for whatever system you are analyzing. What we are doing is checking basic models and theory in a code. So the calculations are performed for the geometries and conditions of the test and then the same calculations are then performed for the geometries that exist in a reactor.

MR. ROISMAN: How do you know that as you change the size of the various things that the relationship between them will be equally represented by the same formula? For instance, did you run the test on a variety of different although not identical to the Indian Point reactor, variety of different reactor cores so that you could prove whether the relationship changed between the size of the core and the size of the reactor vessel and the size of the rod and the size of the break and so forth, the code still continued to predict accurately, and you could expect that when the size was changed to the Indian Point 2 size that it would continue to predict accurately? Was something like that done?

MR. MOORE: Yes. In the sense that there were tests at Idaho and tests at Battelle which were on entirely different systems of different sizes. So in that sense that's a check of two different geometries. There are scaling laws that would apply here that were confirmed by the different sized tests.

MR. ROISMAN: Have the scaling laws been confirmed with regard to -- Well let me ask you this. Can you give me some idea of the difference in scale between the largest of the two tests, whether it was the Idaho or the Battelle one, and what we have at Indian Point?

MR. MOORE: Well, I'm speaking from memory now, but the Battelle test, I believe their vessel was about one-fifth the size of the Indian Point vessel. So it was a pretty large-scale test.

MR. ROISMAN: And the rods that were used in that test, were they one-fifth the size of the rods in this reactor?

MR. MOORE: I don't recall what the core simulation actually was. That's really not the key point here, because it's not the frictional loading on the rods per se that is of interest. It's your ability to calculate the total pressure difference across the -- along the core.

MR. ROISMAN: Thank you.

MR. MOORE: Rather than the specific pressures on the rod itself.

MR. ROISMAN: I understand that. But aren't those affected by what is inside the core? I mean if your core had a steel plate welded from all the way around that went clear across the middle of the core and had two small two-inch diameter holes in it for the flow removed from the bottom to the top you'd certainly get some different kinds of forces than you would have if you had had an open lattice such as you had here. I am trying to find out how you take account of the variables in design of the core in order to predict from the one-fifth size reactor vessel to the Indian Point vessel in terms of blowdowns.

MR. MOORE: I don't recall the specific configuration or geometry in the Battelle test. It was an attempt to simulate the complexity of a typical reactor internals, which is what we are speaking of here.

MR. ROISMAN: But like the semi-scale tests, and I gather the word semi means it is not exactly scaled?

MR. MOORE: Certainly not.

MR. ROISMAN: These were also semi-scaled tests, that is the Battelle reactor was not an exact scale model of the Indian Point reactor.

MR. MOORE: That's correct.

CHAIRMAN JENSCH: Excuse me. I wonder if we could get an answer to that last question. I think the answer that was given was that he said he didn't know exactly what the Battelle tests included as to variations, but I think your question was "Don't you get different variations in forces with the different sizes of reactor?"

Now can you answer that aside from what the Battelle situation was?

MR. MOORE: Yes.

CHAIRMAN JENSCH: Proceed. Thank you.

MR. ROISMAN: Is the ratio of the size of the pipe that was assumed to be broken in the Battelle test to the reactor the same as the ratio of the size of the largest pipe in this reactor to the size of the reactor?

MR. MOORE: I don't recall.

MR. ROISMAN: Is the distance from the top of the rods to the point at which the exit would occur the same ratio in the Battelle as it is in this reactor?

MR. MOORE: I don't recall the specific geometry.

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MR. ROISMAN: Is the distance from the top of the rods to the point at which the exit would occur the same ratio in the Battelle as it is in this reactor?

MR. MOORE: I don't recall the specific geometry.

MR. ROISMAN: Is it your contention that it really isn't relevant, that we could assume that they weren't the same ratio and it wouldn't affect the validity of the Battelle test as a confirmation of the blowdown code?

MR. MOORE: That's right. As long as you have correctly applied the specific geometry in your analysis.

You understand you don't take a calculation of a reactor and then run the Battelle test and see if it looks the same.

MR. ROISMAN: I know.

MR. MOORE: You take a calculation of the Battelle test and run it and see if it looks the same.

MR. ROISMAN: Yes. I understand you take your code and say, "We are going to predict what will happen at Indian Point. Now we are going to prove it will predict well because we are going to predict what happens in some other reactor and if we predict what happens correctly in this other reactor then we can assume that we are right here.

MR. MOORE: That's correct.

CHAIRMAN JENSCH: Excuse me. Is there any possibility of getting the Battelle test data?

MR. MOORE: I have a reference here. I have a reference. I don't have it with me. Would you care to have the reference?

MR. ROISMAN: Yes, please.

MR. MOORE: BNWL-1524 dated June, 1971, by Allemann, et al and several other authors. The title of the reference, I believe, is Coolant Blowdown Studies of Reactor-Simulated Vessel Containing Simulated Reactor Core.

MR. ROISMAN: When the FSAR in this case was originally filed it was before the report date of this test. Did you already have available the data?

MR. MOORE: No. The original report was performed using the same code that was confirmed or corroborated by this test. In fact, in the FSAR there is a comparison of a semi-scale test with this particular code.

MR. ROISMAN: Are there other tests which are scheduled to be run that you know of that will be further verification of the BLODWN Code?

MR. MOORE: Of the BLODWN Code?

MR. ROISMAN: Yes.

MR. MOORE: Yes. Any blowdown experiments are applied to these codes as another check and there are additional tests planned on the semi-scale program. These are not specifically related to forces.

MR. ROISMAN: Is there any test that Westinghouse is planning to run for the specific purpose of further verification of the BLODWN Code?

MR. MOORE: None.

MR. ROISMAN: Was the Battelle test one which Westinghouse was planning to, was looking to for specific verification of the BLODWN Code?

MR. MOORE: Only in the sense that we follow these tests and take the data and apply them to assure ourselves that our codes are adequate.

MR. ROISMAN: But you were satisfied that the semi-scale test in terms of it being able to demonstrate the reliability of the BLODWN Code?

MR. MOORE: Semi-scale and other blowdown tests on vessels. There have been several blowdown studies that have been referenced in the literature.

CHAIRMAN JENSCH: Just a minute. What was the answer to the question? I think he said were you satisfied with the semi-scale tests.

MR. MOORE: The answer is no, and there were other tests, too.

MR. ROISMAN: Are those referenced in the ECCS analysis? In other words, have you listed in the ECCS analysis all blowdown experiments upon which you rely in determining that the BLODWN Code is reliable?

MR. MOORE: No.

MR. ROISMAN: Could you give us what those are, either now or, you know, after the break or something like that, the same as we talked about for the THINC 1 and 2 Code?

MR. MOORE: Yes, I will try.

CHAIRMAN JENSCH: Well, in view of the fact that it's almost the time of our usual recess and he has two codes to examine, at this time let us recess to reconvene in this room at 10:55.

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CHAIRMAN JENSCH: Please come to order. The witness has resumed the stand. Is counsel ready to proceed?

MR. ROISMAN: Yes, Mr. Chairman.

CHAIRMAN JENSCH: Please do so.

MR. ROISMAN: Mr. Moore, as to the two questions that were outstanding from the earlier cross-examination, can you give us the answers to those now? One was what WCAP reports verified the one and two codes. The second was, were experiments verified blowdown code, or do you rely upon for verification of them?

MR. MOORE: The WCAP that discusses the verification of the THINC code is WCAP 7015 entitled "Sub Channel Thermal Analysis of Rod Bundle Core."

MR. ROISMAN: That is the only one; is that correct?

MR. MOORE: No. That's a Class 3 report which is available, a non-proprietary report that I feel addresses itself to your question.

MR. ROISMAN: Let me check my list. I think that is a report that we have. I just want to make sure that if we go and look at WACP 7015 and we come back next week and start talking to you about it, will we have all of the basic material upon which you are relying that is in written form for the validity of the THINC 1 and 2 codes?

MR. MOORE: Probably not. I'm sure there is more written material. I would look at any secondary references in t2

that report. I will be prepared to discuss these next week.

MR. ROISMAN: The WCAP report, does it describe various experiments?

MR. MOORE: Yes.

MR. ROISMAN: There are no experiments that verify the THINC 1 and 2 codes that aren't at least described in the WCAP document?

MR. MOORE: I'm not sure. I didn't happen to have the WCAP here. I just got the reference. There is one experiment that may not be there that I think is germane and should be discussed if it is not. That's a one-seventh scale hydraulics test using the Indian Point 2 vessel internals. That is one-seventh scale.

MR. ROSIMAN: Who conducted that?

MR. MOORE: Westinghouse.

MR. ROISMAN: Is it reported in a report? Not in that one. Was there a separate little report done on it to get a number?

MR. MOORE: I'm not sure. I will have to check that back home.

MR. ROISMAN: That certainly sounds like it would be fairly relevant.

MR. MOORE: The reason I bring it up is, it had a condition where we blocked the inlet assembly by ninety per cent and then calculated or measured the flow redistribution with that blockage and confirmed it with the THINC code.

MR. ROISMAN: When was this test run, roughly?

MR. MOORE: I'm guessing. Four, five years ago. Maybe four years ago.

MR. ROISMAN: Do you have a copy of it that we could look at, also, at the first of next week?

MR. MOORE: Yes, I will get what information I can.

MR. ROISMAN: Thank you.

What about the blowdown code, will you be able to get a list of the experimental data?

MR. MOORE: I don't have the data per se, but I have several different experiments that have been used, have been compared against blowdown. There were some pipe blowdown experiments done at the Illinois Institute of Technology, Research Institute.

MR. ROISMAN: Where are those reports?

MR. MOORE: The comparisons of these tests are reported in WCAP 7401, entitled, "Topical Report, Loss of Coolant Accident Analysis, Comparison of Blowdown 2, and Tests."

MR. ROISMAN: Does that summarize all of the experiments upon which you relied for verification of the BLODWN CODE?

MR. MOORE: Yes, I believe so.

MR. ROISMAN: Is that a proprietary document? Do you know that?

MR. MOORE: That is not a proprietary document.

MR. ROISMAN: In the BLODWN code, is there an analysis of vibrations, if any, that will occur during the course of the blowdown on the rod to the grids?

MR. MOORE: The forces are calculated. The answer would be yes.

MR. ROISMAN: What I'm getting at is, vibrations as opposed to--I didn't understand from the earlier discussion. You talked about flow, the forces flow. I saw something more, like a steady pressure against the rod. What I'm trying to find out, is the rod subjected or the grid subjected to varying pressures all along that would cause them to vibrate, and is that computed in the course of the code?

MR. MOORE: Yes. The forces do vary on the rod. These are computed in the code.

MR. ROISMAN: So that the code does simulate flow induced vibrations; is that correct? That is what I am asking.

MR. MOORE: In the context of the blowdown transient, yes. I'm not sure what you mean by flow induced vibrations.

MR. ROISMAN: Vibrations induced by flow.

MR. MOORE: I have the forces induced by flow. Whatever vibrations that are associated with that are calculated.

MR. ROISMAN: Let me see if I could take a gross example. If you have a certain amount of water coming out of a pipe and you wanted to know what the maximum force was of

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that water coming out of the pipe, you could measure and come up with a figure. That would be a single figure as to what the maximum force was.

If you wanted to know all of the effects that that flow of water might have on a piece of tubing that was stuck in the midst of the flow, there would be variations of pressure that that piece of tubing would be subjected to virtually instant by instant that could cause it if they were regular variations to begin to vibrate. What I'm trying to find out, in making your computations, have you taken account of the way in which the rods would vibrate as the result of variations in pressure that it is subjected to on an instant by instant basis, or is it a grosser calculation than that?

MR. MOORE: No. The variation is continuously with time and are calculated and applied to the rod.

MR. ROISMAN: And the experiments that were run to verify the codes, were those experiments able to measure these instant by instant variations in pressure in determinations about the vibrations of the rods?

MR. MOORE: Yes.

MR. ROISMAN: During the course of normal operation, does water get into those rods? Does that ever happen? Like when the reactor is shut down, the rods are sitting there and a rod, in refueling, could get water in the rod.

MR. MOORE: That's a possibility, yes.

MR. ROISMAN: During the course of the loss of coolant accident, have you done an analysis of the pressure inside the rod as a result of the expansion of steam in the rod if the rod had this little leak of water into it?

MR. MOORE: There is no water in the rod at full power.

MR. ROISMAN: What happens to the water?

MR. MOORE: It is driven out as the rod heats up as you go to full power.

MR. ROISMAN: So at the time of any loss of coolant accident, the rod is completely clear of water under all circumstances?

MR. MOORE: Yes.

MR. ROISMAN: Have you verified that fact, that there won't be any water remaining in there?

MR. MOORE: You can calculate the temperatures that exist within the rod. You are well above saturation temperature. So there is no possibility for water to stay in the rod.

MR. ROISMAN: What about steam?

MR. MOORE: You can have some steam.

MR. ROISMAN: Will the steam during the loss of coolant accident expand more rapidly? In other words, will it begin to expand rapidly? Will it be one of the things which will add to the pressure in the rod?

MR. MOORE: I don't see that that's a significant effect, no.

MR. ROISMAN: What do you mean, not significant?

MR. MOORE: You would not get any large rapid expansion of steam in conjunction with loss of coolant. The amount of steam that might be in that small plenum is just not sufficient to create large additional forces during the loss of coolant.

MR. ROISMAN: Have experiments been run to determine how much would be the maximum steam that could be in there and how much would be the maximum that would expand in the loss of coolant accident?

MR. MOORE: It seems to me it would be a fairly straightforward thermodynamic calculation.

MR. ROISMAN: Have there been experiments?

MR. MOORE: There have been straightforward thermodynamic experiments, yes.

MR. ROISMAN: Have there been experiments of that particular event in the rod? In other words, a rod has got a certain amount of plenum, a certain amount of gap and a certain amount of fuel and so forth. There is a maximum that we could conservatively predict as to the amount of steam that might be in there. Has an experiment been done to determine how much that steam would expand and how quickly under the most conservative assumption regarding heat-up rate of the rod?

MR. MOORE: I'm not aware of any experiment directly applicable to the situation you are describing.

MR. ROISMAN: What about an analysis of it? In other words, a computation of what that would be with a given amount of steam and a given amount of space in the plenum and so forth. What would you expect expansion to be? Has there been such an analysis conducted?

MR. MOORE: I believe so.

MR. ROISMAN: Do you know who conducted it?

MR. MOORE: Not specifically.

MR. ROISMAN: Or where it is reported?

MR. MOORE: Not specifically, no.

MR. ROISMAN: Is it something that's outside your area of expertise, or is it something that you could check and tell me later?

MR. MOORE: Outside my immediate area of information. It is something I could check.

MR. ROISMAN: If you would, please.

MR. MOORE: Yes.

MR. ROISMAN: This is to identify a report or place where there is a report of the calculated expansion of the rod and the steam in the rod during the loss of coolant accident.

MR. MOORE: Yes.

MR. ROISMAN: In the blowdown calculational method, is the fluid in the calculation assumed to be in a thermodynamic equilibrium or non-equilibrium condition?

MR. MOORE: It is in an equilibrium condition.

MR. ROISMAN: We talked earlier about these experiments on the blowdown forces. Perhaps the information is in the WCAP. Does it describe the variation in the equipment that was used in the experiment and the equipment that is in this plant? For instance, can we tell how the support structure differed, the channel wall material, the thickness of it. those kinds of things? Are they explained adequately in the WCAP report? That's all I have to ask you about it. Then I can just look at the report and come back to it at another time.

MR. MOORE: No. I would recommend the report that most clearly or closely simulates those effects. It would be the Battelle Northwest report reference. I would recommend that.

MR. ROISMAN: You mean that report clearly describes these differences or it comes closest to simulating the ones that exist on this reactor?

MR. MOORE: As I understand it, it was the one that was the closest simulation of internals in the reactor, and it will explain what their simulation was.

MR. ROISMAN: Are you familiar with the interim policy statement on the performance of the emergency core cooling system?

MR. MOORE: Yes.

MR. ROISMAN: In that statement reference is made to the fact, I think, condition No. 3, that the core must remain amenable to cooling. That's one of the conditions. Are you familiar with that particular condition?

MR. MOORE: Yes.

MR. ROISMAN: Can you explain to me what your understanding is of what would be a core that is not amenable to cooling? Does it have a certain geometry to it that you can describe that would indicate one that is not, at the first point at which it becomes not amenable to cooling?

MR. MOORE: It is rather difficult. I think the concern is one in which the fuel is not contained into a reasonably well-defined geometry such that you can treat the heat transfer effects in removing this residual heat from the fuel. So a configuration which caused the cladding to no longer effectively contain and support the fuel creates unknowns that make it difficult to further calculate what's going on.

MR. ROISMAN: For instance, if the various rods in the core were to become embrittled at a single point along the way -- Let's say at midpoint. -- to the extent that it wants to refill, and reflooding began, all of those rods split right at that point and the top of the rod dropped down into the channel adjacent to the rod. Would that be the kind of destruction of the core geometry that would, in your opinion, make the core no longer amenable to cooling, or would it be able to be cooled in that condition?

MR. MOORE: I'm not sure I understand the configuration we have.

MR. ROISMAN: We now have all of the core rods cut in half, if you will.

MR. MOORE: Cut in half?

MR. ROISMAN: Yes. The rod itself is still intact except for the place that it broke.

MR. TROSTEN: Excuse me. Mr. Roisman, may I ask you a question about the thrust of your question?

Do I understand that you are asking a question of this witness as to what his professional opinion is concerning when the core would be amenable to cooling or what that regulation means?

MR. ROISMAN: Not the second question, only the first question.

He already testified that he has about as much knowledge as I do as to what it means.

MR. MOORE: If I have just split the rod, it is split at one point. But the rod, the top part of the rod is still there and the bottom part of the rod is still there. It is just split. I don't that's necessarily an uncoolable geometry.

MR. ROISMAN: In the case of a loss of coolant accident, if a rod should slip like that as a practical matter, would the top stay sitting on the bottom, or would it tend to begin to float around or get knocked around or something? What could we expect to happen to it?

MR. MOORE: Just looking at the loads and so forth, I'd expect it to stay there because it is constrained by the grids.

MR. ROISMAN: What about the fuel pellets or the fragments of pellets, what would you expect to happen to those if the rods were to split?

MR. MOORE: Again, it depends on the split configuration. If a significant amount of fuel could be expelled from the split--I have no longer my fairly well defined geometry.

MR. ROISMAN: That's what I was getting at. Is the critical thing here where the fuel is as opposed to more than as to where the rods are?

MR. MOORE: Certainly. That's what we are cooling.

MR. ROISMAN: In other words, if the rods remained in place, but all the fuel came out of them, that would be a non-coolable geometry, although technically the geometry, if you are looking at the outside of the rods, is roughly exactly what it was before this whole event began?

CHAIRMAN JENSCH: Is that correct?

MR. ROISMAN: Yes, is that correct.

MR. MOORE: Not necessarily, but the potential is there, yes.

MR. ROISMAN: Is there a percentage of the core which can be non amenable to cooling and still the entire core would be, in your opinion, generally amenable to cooling? In other words, we have talked about the fact that two rods might fuse together at a point where they both had swollen. If that occurred, let's say, for fifty rods, they have fused together for the full length of the hot region of the fifty rods so that they formed a solid block at that point in the one-seventh region.

Would that substantially interfere with the cooling of the core as a whole?

MR. MOORE: Certainly, no, not the cooling of the core as a whole. It would certainly affect the cooling of that particular region substantially.

MR. ROISMAN: I understand that. By the way, when you used "region" there, you meant a geographic area of the core and not the regions of the code?

MR. MOORE: Yes.

MR. ROISMAN: We talked, I think, yesterday or the day before about the point where rod-to-rod contact occurs. You have explained that at the point of rod-to-rod contact no heat transfer is assumed at that point. Then, instead, the heat goes around the rod and goes out the other side.

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I believe I asked you--if you want, I can give you the transcript reference. It is transcript 1692. When the two rods touch, which do you assume gets the other's temperature? Are both rods assumed to be the hottest, or do you get an average temperature for the two rods now that they have touched, or do you assume they are the coolest of the two. I remember you said you wanted to check and you thought it was assumed it was as hot as the hottest.

MR. MOORE: Yes. I checked. The assumption is made that there is no heat transfer cross inferface, and the temperature we calculated was the temperature for the hottest rod.

MR. ROISMAN: Now what happens at the point of contact? Is there for any period of time a period which that point of contact, because it's going to cool more slowly than it would cool if the water were running around it, would begin to rise in temperature, or its rise in temperature would continue longer than it would have continued otherwise, or is the time that it takes to heat to that point just as quick as it would be circumferentially around the rod as it would have been if it had been picked up by water or steam flow proceeding around the point?

MR. MOORE: There is a time transient associated with that which is indicated in that report which is in Volume II of the report describing that analysis.

MR. ROISMAN: Time transient associated you said with that? You mean with --

MR. MOORE: With the temperature of that portion of the rod which is in contact.

MR. ROISMAN: In computing a 2300 degree Fahrenheit with a hot spot in the core is that a spot where two rods have come in contact? In other words, is that the hot spot in the core?

MR. MOORE: No. As I said before, the hot spot is calculated on the basis of without distortion, without contact.

MR. ROISMAN: All right. I thought you meant something different. I thought we were talking now about the water flow itself when you said that it wasn't computed. But it also does not compute the physical contact of rods and how the physical contact affects the heat at that point.

MR. MOORE: The design calculation that gives us 2300 degrees does not.

MR. ROISMAN: Thank you.

Turning to the reflooding calculations I believe you testified on the first day you assume that there is essentially no water remaining in the lower plenum following the loss of coolant accident. Is that correct? Is there essentially no water remaining in the lower plenum following a loss of coolant accident?

MR. MOORE: That's correct.

MR. ROISMAN: Now by essentially none do you mean that when you compute the amount of water that is in the core you, that is in the vessel, you assume that there is none or it's just a very little bit? By figuring your refill rate you have to determine how much you have got to start with.

MR. MOORE: That's correct.

MR. ROISMAN: Do you start with zero?

MR. MOORE: We start with zero.

MR. ROISMAN: In determining heat transfer during the pre-reflooding period and the pre-refilling period, do you take credit for any amount of water being at the bottom that flashes to steam and assists heat transfer during that period?

MR. MOORE: No, no.

MR. ROISMAN: Is there a period in the computation of the heat transfer, is there a period during which the core is dry, the plenum, the lower plenum is dry, and in effect we have got nothing in there, no liquid at all?

MR. MOORE: Yes.

MR. ROISMAN: How long is that period?

MR. MOORE: I'm sorry. I guess I should have that question repeated. Were we talking about water in the core and the plenum or just in the core or --

MR. ROISMAN: Okay. If it makes it easier to answer we will do first the core, then the plenum and then we can have them together.

MR. MOORE: All right.

There is a period of time when there is no water in the core. During this time we are filling the plenum with accumulator water.

MR. ROISMAN: Maybe I should start by asking you to sort of describe to me what is happening here. The core, before we have the loss of coolant accident, including the plenum and everything, is all filled up with water. Bam! We have a loss of coolant accident and the water begins to leave the core. You have got steam flashing and so forth. Now is the last place that the water that's in the core -- That's the last place that that water goes that's in the vessel, is it through the core? In other words, is there a time when there is water in the core but there is no water in the lower plenum?

MR. MOORE: No, I don't think that exists.

There is a blowdown transient. The total system is blowing down to the containment. At the end of blowdown there is essentially no water in the system by our conservative calculations.

MR. ROISMAN: I understand.

MR. MOORE: And at that point in time then there is no water in the core and now we are filling up the system again with accumulator water.

MR. ROISMAN: But when you say that there is no water in the core you seem to be saying that carefully, not trying to say that there is also at that time no water in the plenum.

MR. MOORE: I am not purposely trying to say it carefully. There is basically no water in the system, by the assumption and the analysis, and now we are pumping or pushing accumulator water in from that point on.

MR. ROISMAN: The accumulator water to get to the lower plenum comes down the downcomer, is that right?

MR. MOORE: That's correct.

MR. ROISMAN: Is that the only way that you assume for purposes of the calculation that the accumulator water gets to the lower plenum, is down through the downcomer?

MR. MOORE: That's correct.

MR. ROISMAN: And during the course of the blowdown the water that is in other loops, is that assumed to pass through the core on its way out the break or -- Sorry.

MR. MOORE: Some does, some doesn't.

MR. ROISMAN: The route that that loop water takes, is it the same route that the lost accumulator water is assumed to take?

MR. MOORE: No.

MR. ROISMAN: What is there about that water -- I take it the accumulator water is coming into the same loops that we are talking about, the loop water being in. Why don't they go essentially to the same place? What is the mechanism that's occurring?

MR. MOORE: Well, there is discharge from the loops in two different directions toward the break, the intact loops. The hot legs -- We are talking about the double-ended cold leg break?

MR. ROISMAN: Yes.

MR. MOORE: The hot legs are discharging partially through the core up the downcomer and out through the break and part of the water is going through the steam generators through the cold legs into the downcomer and out the break.

MR. ROISMAN: And part of it is going into the cold leg that has the break and isn't getting to the core at all.

MR. MOORE: All that, all of it has to go out through the broken cold leg. Some of it bypasses the core.

MR. ROISMAN: Now the accumulator water that's in the same loops, it does not go through the core on its way out, is that correct?

MR. MOORE: That's correct.

MR. ROISMAN: What I am trying to understand is why does the water that's essentially in the same loop -- Some of it was in the loop to start with and some of it was in the accumulator -- why doesn't it all follow the same path? Why doesn't some of that accumulator water come through the core on its way out, just as the water that is replacing it in the loop, some of it, came through the core on its way out?

MR. MOORE: Because during most of the transient period when the accumulators are injecting, the flow through the core is in the reverse direction. So that it is flowing from the hot leg down through the core and up the accumulator and out the break.

MR. ROISMAN: You said up the accumulator and out the break.

MR. MOORE: Up the downcomer, excuse me.

So the direction of the flow is such that the accumulator flow would not go through the core.

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MR. ROISMAN: You mean in other words everything that's in the core in the vessel itself is tending to go out, whether it's out the hot legs, around, and finally through the break, or out the cold legs that broke and directly to the break. It's all going away from the center of the core, is that right?

MR. MOORE: No.

The flow from the steam generators to the hot leg piping during a large part of the blowdown, particularly at the later stages of the blowdown, is going back through the core and there is a stagnation point where flow can flow in the other direction in that intact route through its cold leg into the annulus and outbreak. If you look at the testimony we presented for the loss coolant analysis you can look at the core flow and see the direction of the core flow in that over a large part of the blowdown transient we are getting reverse flow through the core. The accumulator water is coming in the cold legs, is spilling into the down, and will tend to fall down the down, and fill up the lower plenum while the steam is discharging up the down, and out the break.

MR. ROISMAN: Is the water that comes into the core from the loops, not the accumulators now, from the loops, does it go through the middle of the core or does it stay to the side of the core? As it comes through what is the tendency?

MR. MOORE: It goes through the whole core.

MR. ROISMAN: In other words, there is no pressure or anything that's occurring near the center of the core which would tend to force that water to the side?

MR. MOORE: No.

MR. ROISMAN: Is the center of the core hotter than the edges of the core?

MR. MOORE: Somewhat.

MR. ROISMAN: Is it causing the production of steam at a hotter rate than the edges of the core?

MR. MOORE: Not significantly to--

MR. ROISMAN: Well--go ahead.

MR. MOORE: Not significantly to cause any flow redistribution.

MR. ROISMAN: Can you "not significantly" means in a figure?

MR. MOORE: Well, analysis would say ten to fifteen per cent flow redistribution might occur in a calculation. We assume a twenty per cent flow redistribution, just to be conservative.

MR. ROISMAN: What is it? There are experiments that have been run to determine that.

MR. MOORE: Again, this is a calculation using the THINC codes that have been verified by experiments as discussed earlier.

MR. ROISMAN: My question is as to the specific--

when you have got the kind of power distribution that you have in this reactor, have experiments been run to find out how much steam will be generated in the center compared to the edge and to what extent there will be forces keeping water, during the blowdown stage, away from the center of the core? Or the hot part of the core, and assuming it is.

MR. MOORE: There are no specific experiments directly related to that condition.

MR. ROISMAN: And the experiments that are not directly but that are indirectly and you feel reliable enough are the ones that are outlined in WCAP 7015?

MR. MOORE: Yes.

MR. ROISMAN: And this one-seventh scale hydraulic test that was run for a scale of this reactor, is that correct?

MR. MOORE: Yes.

MR. ROISMAN: Just in general do these tests have the same pressure in the reactor core as the pressure that would exist in this reactor core prior to the loss of coolant accident?

I think it's--what is it, 20--

MR. MOORE: 2250.

MR. ROISMAN: Yes. Is the operating pressure?

MR. MOORE: No.

MR. ROISMAN: Did the ratio of the amount of water in the loops to ratio of water in the core, was it the same ratio as was true in this reactor?

MR. MOORE: No. What is significant is the void fraction and the amount of boiling flashing you have. That's what is important in calculating the redistribution of flow.

MR. ROISMAN: Well maybe I am misunderstanding, but I thought it would be equally relevant to know how much pressure the additional water is coming into that core with in order to see what back pressure would deter it from entering a certain area.

In other words, assuming the back pressure difference were a hundred, the water was coming in at a pressure of thirty-five, that it wouldn't ever get into the place where the difference was a hundred?

MR. MOORE: Yes. We were talking about redistribution effects which don't have an effect on total pressure in the inlet and outlet core. The comparison to the total pressure, which is driving the flow through the core, is obtained in blowdown type experiments.

MR. ROISMAN: We are now trying to figure out--you have got water coming from the various intact loops passing through the core on its way out of the system. As it passes through the core it comes through at a certain pressure and at this point we aren't--I'm not discussing with you what that pressure is, but it has one, and I assume that has been some computations as to what its pressure is.

MR. MOORE: Yes.

MR. ROISMAN: Unless it's going to pass through the core and the core has got different pressures in it. My question is as it flows through that core if there is an area in which the pressure of the core is substantially greater than the pressure of the incoming water, then the water isn't going to get to that part of it at all.

MR. MOORE: That's not the case. The pressure across any cross-section of the core is essentially uniform because it's an open lattice core.

MR. ROISMAN: Excuse me?

MR. MOORE: The pressure is uniform.

MR. ROISMAN: We are back to this across thing.

MR. MOORE: Now you are across.

MR. ROISMAN: Oh. Now you mean horizontally across?

MR. MOORE: Right. The pressure is basically uniform and that is why in order to maintain that uniformity in pressure there is a slow redistribution.

MR. ROISMAN: You say that it's uniform. You mean that at the same instant it's the same or you mean that the tendency will be for it to get uniform?

MR. MOORE: If the flow redistributes such that it does become uniform.

MR. ROISMAN: We are not talking about--when you first start with a hot center the steam begins to expand more quickly than it does at the cooler areas. There is a pressure

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difference as the steam in the middle is still at the middle. Now it's moving to the lower pressure areas. I understand that.

But my question is, is there a point at which that will be a cross core, if you will? There is more pressure at that center hot spot than there is at the edge until we get a redistribution of that pressure.

MR. MOORE: It's all a continuous transient. You don't suddenly have a large pressure in the center to be relieved. As the water comes through and it's heated up, the water in the center as it's heated up more will flow toward the other assemblies, in order to maintain the same pressure drop between assemblies.

MR. ROISMAN: We start with water in the core already. Now we have a loss of coolant accident. Things are starting to get hot very fast. In the center they are starting to get hot faster than they are on the edges because it's hotter there to begin with. You have got a higher power density. Isn't there a tendency at that time to start already pushing water away from that center as that steam begins to expand more quickly than the steam on the edges expanding.

MR. MOORE: There is a tendency for this--yes. There is a tendency for the water in the hotter regions of the core to expand to the polar regions of the core. I think we have got a misimpression here about the power distribution in the core. I don't have a very local hot spot or region in the core.

The hotter regions, the hottest assembly, occurs in say four locations in the core, symmetric locations. These are not all together in one volume as we were portraying it here in the discussion.

MR. ROISMAN: I understand that. All that does then is it increases the number of places from which higher pressure is being generated.

MR. MOORE: My point was it's not large volume of high pressure surrounded by a low pressure volume.

MR. ROISMAN: You can look at only the section of the core that has the hot region in it, if we can see it that way.

MR. MOORE: It's one assembly surrounded by cooler assemblies.

MR. ROISMAN: Yes. I am just trying to understand the mechanism of the pressure moving from the hotter to the cooler thing and now what I am saying is as you have pointed out it's a continuous event. It's going on all the time. So the water is coming in the bottom of the core now and it's trying to figure out what is the best way to get the hell out of this core, and as it's making that decision it sees through the core places where the steam is expanding faster than it is on the edges.

What I'm asking is how do you know what experiments you could run to determine how much of it will get into the hot region, whether there are five of them scattered through the core, or one of them in the middle of the core, and we started talking then about experiments that had been run and you said "Well, it doesn't matter what the pressure is of the incoming water," and I am trying to find out why that doesn't matter if you have got some areas that have higher pressures than others.

And when I reached that point you said, "Well, the pressure is essentially even at the same region of the core," and I understand that it's moving in that direction. But at any given moment it is not at that point. We are now trying to find out where the water goes when it gets through.

MR. MOORE: You understand the large pressure difference is the pressure difference from the top to the bottom of the core. That's what is driving the flow through the core.

MR. ROISMAN: I understand that.

MR. MOORE: Okay. And these are small pressure differences, a few psi, or tenths of psi, that can occur in getting the flow redistribution between the assemblies. It doesn't take -- As soon as the pressure difference starts to build up in a hotter assembly the flow immediately redistributes to ensure that the pressure is uniform across the core. As it's coming through the core and as long as the total pressure drop from the top to the bottom of the core is in a direction to force the flow through the core that's where the flow goes. We are talking about a second-order effect in terms of the flow redistribution within the core.

MR. ROISMAN: Don't misunderstand me. I am not doubting your conclusion, at least not at this point, that the water doesn't go through the core, that it goes from the bottom to the top. What I am trying to find out is whether or not the twenty percent redistribution is an appropriately conservative figure in figuring out the route that it takes in going through there. I mean if I assume that if you have in the middle of the core a huge open hole with no rods in it at all, that that would very much change your question as to what the redistribution is, particularly if it was surrounded by a curtain of something that tended to be an insulating material and made it cooler there. You'd have a tendency for the water to go to that point rather than for it to go to hotter places. What I am trying to find out is you don't have it quite mathematically but we do have cooler places, hotter places, lower pressure and higher pressure places. How do you compute how much would go through which portions of the core?

MR. MOORE: You compute it by calculating for each region of the core the equations of --

MR. ROISMAN: Excuse me. Are we talking about geographic regions or the code regions?

MR. MOORE: I am talking about geographic regions when you look at a hot assembly adjacent to a cooler assembly.

MR. ROISMAN: Oh.

MR. MOORE: And you calculate the amount of flow that goes into each of these assemblies. You do an energy and a mass balance on the flow going to each assembly and you do a pressure drop calculation on each of these assemblies and you calculate what the pressure drop will be as that fluid expands in each assembly and you find out that the pressure drop in the hotter assembly wants to be higher than the pressure drop in the cooler assembly.

So the conservative way to calculate how much water goes out of the hot assembly in order to get an equal pressure drop is to assume that it's immediately redistributed with no pressure drop in the radial direction now across the core, as you would say, and it can immediately redistribute.

So this would tend to always push water out of the hotter assemblies into the cooler assemblies and will in fact overpredict the amount that will go out of the hot assembly.

So you can calculate this by just calculating along the length of the assembly the pressure drops in each assembly and take the flow out of the hot assembly which is required to keep the pressure drops equal.

MR. ROISMAN: I understand the mechanism that you are talking about.

MR. MOORE: That's how we calculate it.

MR. ROISMAN: Did you do experiments to find out whether it was correct to say what the rate of flow, radial flow now, would be as a result of the power distribution differences?

MR. MOORE: Yes. These are the experiments that I discussed in this WCAP 7015 where you have assemblies, adjacent assemblies with different power levels, and you heat the water up more in one assembly than you do in the other assembly and you measure the flow in the two assemblies and compare them to what you would predict. Yes, those experiments have been run.

MR. ROISMAN: Were they run with -- Did that water turn to steam in the course of the experiment?

MR. MOORE: There were some experiments where there were void fractions up to eighty, ninety percent, that's with a considerable amount of steam which is representative of the loss of coolant conditions. The answer is yes.

MR. ROISMAN: In the situation where the hot assembly is surrounded by cooler assemblies the pressure difference between -- Well, let's go back. You are computing it using this one-seventh of the rod, right? The hot spot is a seventh of the rod's length long.

MR. MOORE: We are talking about a different calculation now. It's a flow redistribution calculation.

MR. ROISMAN: What is the height of the area in which the hotter spot is assumed to exist? As percentage of rod. I don't mean in inches.

MR. MOORE: I don't recall offhand. There are probably at least twenty such axial sections, perhaps more. The rod was split up into much smaller increments than the one-seventh that's used for the temperature calculation.

Is that a less conservative or I should say is that a more realistic or a more conservative method of doing it than if you merely divided the rods into seven sections and treated one-seventh as having the same power density, the same amount of heat and so forth?

MR. MOORE: With respect to calculating the detailed flow phenomena and the redistribution, you want a finer measure, a finer distribution of regions in your analysis. The number of regions you choose for any particular analysis is a function of the kind of analysis you are doing and the kind of results , the application of the result.

MR. ROISMAN: I understand that. But now we are talking about whether a region in the core is hotter or not than an adjacent region. As I understand it it's power distribution that determines whether it will be hotter, all other things being equal, assuming that we don't have metal-water reaction, something like that, which we are not talking about at this stage in the loss of coolant accident, is that correct? It's the power distribution which makes one area hotter than another.

MR. MOORE: Yes.

MR. ROISMAN: I thought you had said that in this reactor one-seventh of the reaction portion of the rod represents this same relative power distribution. Now in the hot rod that was 17.4 kw per foot, is that right?

MR. MOORE: Yes, sir.

MR. ROISMAN: Then in the hot rod a seventh of that rod has a power distribution which is the same, namely 17.4 kw per foot.

MR. MOORE: That's right, for the temperature calculation.

MR. ROISMAN: Go ahead. I am sorry.

MR. MOORE: To wit, my main purpose for that calculation is to calculate the peak clad temperature.

MR. ROISMAN: Isn't it that temperature that will determine how much steam is generated and how fast it will be generated at that point, assuming you have got water there?

MR. MOORE: It's more the power in the assembly than the temperature itself. The heat is transferred from the cladding to the core.

MR. ROISMAN: Power is what defines the one-seventh area, isn't it? The one-seventh area has the power.

MR. MOORE: Yes.

MR. ROISMAN: But what I don't understand is why isn't one-seventh then used as the region of interest when you are looking at the hot test section? It's got the same power.

MR. MOORE: Because we are looking at apples and oranges. We are talking about the temperature calculation, the LOCTA Code in one case. In the other case we are looking at what possible flow redistributions can I have using the THINC Code. These are two entirely different calculations. The LOCTA calculation assumes that there is a twenty percent flow degree distribution.

MR. ROISMAN: Right. And that assumption is based upon the computations in the THINC 1 and 2 Codes, which in turn are based on the experiments in WCAP 7015, among others, correct? They are apples and oranges but they are all going to end up on the same table, and that is what we are talking about. They all get added together at the end of figuring out what the peak clad temperature in the loss of coolant is, loss of coolant accident is, which is 2300 degrees Fahrenheit or 2700 degrees Fahrenheit.

MR. MOORE: Yes.

MR. ROISMAN: Okay. What I am trying to find out is when you are figuring out how much radial flow redistribution there is going to be in the core as a result of hot spots, why isn't the area of interest the whole area that has the same power density for that rod, namely one-seventh of the rod. Aren't you going to get the same amount of steam, assuming that the one-seventh area represents a food and a half from the point that is three inches, six inches, nine inches, twelve inches, fifteen inches, eighteen inches, aren't they all going to be producing the same amount of steam at the same temperature provided that they have got -- I mean they start off with the same amount of water around them, don't they?

MR. MOORE: Yes.

MR. ROISMAN: What is the point of splitting that up into thirds? What difference do you get in each third when you split it up into twenty regions as you said or roughly twenty for purposes of the THINC Code variations?

MR. MOORE: Well, I want to properly simulate the effects now in the case of the THINC Code. I am interested in the effects along the length of the fuel center from the inlet to the outlet and the effects of the previous regions are important in determining the thermohydraulic effects of regions downstream from those for thermohydraulic analysis. Therefore I need a rather detailed continuous representation of the hydraulic channel from the inlet to the outlet.

MR. ROISMAN: I see.

MR. MOORE: In the case of the temperature calculation where we had one-seventh, a one-seventh length, there is basically no effect of that length on adjacent lengths. So it is not as important, the actual physical length that I am using, with respect to the temperature calculation. I have just been overly conservative in picking the total one-seventh to be at the hot spot. It doesn't matter what the one above it or below it is really doing with respect to temperature.

MR. ROISMAN: Can you explain to me how it matters in the hydraulics situation? For instance, let me see if I understand this.

If we were talking about a core in which the water was not moving, but for the time being, was just standing still, and you had a rod that had the same power distribution over an area of one-seventh of that rod, a foot and a half or so, would the amount of heat transfer to the water at the full length along that one-seventh be the same, or should it be?

MR. MOORE: Yes.

MR. ROISMAN: What you are saying is, if the water is moving, it now makes a difference in figuring out how it is going to move, what is happening along the length? That is, as you move up the rod, even though the same amount of heat is being transferred out of the rod, what that heat does when it gets out of the rod would be affected by what has happened to the section of water a moment before as it passed by an earlier portion of the rod; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: What happens? What is going on with this water as it is passing along the same section of the rod that has the same power distribution, the same amount of heat coming out of it, what is going on, for practical matters, with the steam as it is going up that way?

MR. MOORE: When I simulate the temperature calculation with one-seventh of the rod, where each seventh of the rod has a certain constant power, can you visualize the power along the length ofthe assembly is in finite steps where each interface there is basically a ten per cent increase in power? We have the hottest or the hot spot and then we had .9 times the hot spot and .8 times the hot spot. If I do a thermal hydraulic calculation where the power distribution takes a sudden ten per cent step at each of these areas, I won't get correct representative results at the interfaces between the regions. The interface is important. I want a more reasonable representation of the slope of the power distribution along the total channel, not just in the region of the hot spot.

That's the reason you do a more detailed axial analysis for the thermal hydraulics.

MR. ROISMAN: But how does it affect the flow? That's what I was trying to find out.

MR. MOORE: How does what affect the flow?

MR. ROISMAN: How is the flow affected, if you will, by the history of the drop or the steam as it passes along the length of the rod?

MR. MOORE: As the water heats up, its density is changing and its pressure drop characteristics. There is friction pressure drop that is changing. So--

MR. ROISMAN: Go ahead. I'm sorry.

MR. MOORE: So you have to properly account for the frictional pressure drops and the density variations in pressure drop along the length of the channel. So I am accumulating a pressure drop between the inlet of the core and the outlet of the core, and I am specifying how that occurs along the length of the channel.

MR. ROISMAN: On the bottom end of the region, let's talk about a point a third of the way up the rod.

MR. MOORE: Yes.

MR. ROISMAN: Will the outward pressure--as we are moving up from the bottom of the rod, the water at this point is being exposed to progressively hotter portion of the rod; is that correct? The rods go from lower power density to higher power density as they move from the ends toward the center; is that right?

MR. MOORE: Yes.

MR. ROISMAN: So as the water is coming up from the bottom, it is being heated progressively more and more as the steam has been heated progressively more and more; is that right?

MR. MOORE: Yes.

MR. ROISMAN: In a given area, as it gets to the bottom of the hottest region, which is a seventh of the rod's length, the hottest region, is there a tendency for the pressure being generated by the expanding steam to push down to the sides and up? In other words, does it tend to expand in all directions? Not equally, but does it have a tendency to expand in all directions?

MR. MOORE: Yes, there is a certain pressure drop associated with that water going through that particular region.

MR. ROISMAN: As it continues to go out through the hot region, it continues to have a tendency to expand in all directions; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: At the bottom of the hot region, is the percentage of it that's going to expand downward, sideways and up, the same as it is at the top of the hot region?

MR. MOORE: No.

MR. ROISMAN: How does it change?

MR. MOORE: By the pressure drop in the radial direction, across the core, is much lower than the pressure drop along the length of the fuel assembly.

MR. ROISMAN: Does that difference between the radial pressure and the vertical pressure change as the water passes by the hot spot along the one-seventh recorded as the hottest power distribution, highest power distribution?

MR. MOORE: Yes. In the hotter regions of the core, the tendency is for more flow to go out of the hotter regions across the core.

MR. ROISMAN: Why is it the tendency for more of it to go up the core? You testified, I thought, that a difference is between the rods.

MR. MOORE: I am comparing the flow in one cooler assembly to a hotter assembly. The pressure drop at the inlet to a cool assembly, the pressure at the inlet of the cool assembly and the hot assembly are equal. The pressure drop at the exit of the cool assembly and the hot assembly, the pressure there is also equal. There is a larger pressure drop at a given flow than the hot assembly. So that flow must flow out of that assembly into the cooler assembly in order to maintain equal pressure drops between the assemblies. The pressure drop at any given plane in the core is essentially the same.

MR. ROISMAN: If we look at the bottom of the rods for several assemblies, what would be the variation in the power density at the bottom of those rods? Assume that one of the assemblies is what we would call a hot assembly surrounded by cooler assemblies. What is the power density difference in the bottom of those rods?

MR. MOORE: Between a hot assembly?

MR. ROISMAN: And an adjacent cooler assembly, a typical difference.

MR. MOORE: Maybe five, ten per cent reduced in the cooler assembly.

MR. ROISMAN: If we started with a dry core, assuming it was warm, and had recently lost its water, would the water tend to go up the cool assembly more than the hot assembly? That is where the bottom was cooler?

MR. MOORE: Yes.

MR. ROISMAN: Would that create a pressure difference at that point between the two?

MR. MOORE: In which direction?

MR. ROISMAN: Horizontally.

MR. MOORE: Yes, it would tend to create a pressure difference. If you assume there is no pressure difference impeding the redistribution of flow, you would get more flow from the hot region to the cooler region.

MR. ROISMAN: Then how is it that you assume that the pressure at the inlet and at the exit to the core is the same?

MR. MOORE: I didn't say the pressure at the inlet is the same as the exit.

MR. ROISMAN: For the all the inlet you said it is assumed to be the same. You said for all the exits it is assumed to be the same. How is it that that it true?

MR. MOORE: That's a conservative assumption with respect to calculating the flow which will redistribute from the hotter regions to the colder regions.

MR. ROISMAN: Before we label what it is, it is inaccurate; is that correct? It is not the case that the pressure is the same all across the inlet to all of the rods, or that it is the same all across the exit to all of the rods?

MR. MOORE: If the bottom of the core is the same because there is no heat generation -- That is at the very bottom of the core.

MR. ROISMAN: It is the same because of what?

MR. MOORE: We have not generated any heat to cause any flow redistribution. We aren't into the flow assembly yet, correct? The same pressure at the bottom of fuel assemblies, we are at that.

MR. ROISMAN: As soon as you start at the bottom of the rods is what I should have been saying, at that point. You assume that the pressure is the same horizontally across the bottom of all the rods? In fact, that is not the case?

MR. MOORE: Once we start to get into the assembly there will be no distribution and will tend to be a pressure difference between the hot assembly and the adjacent assemblies in the radial direction.

MR. ROISMAN: Right at the tip end is there any heat coming off the tip end going down?

MR. MOORE: Wherever you want to start adding the heat.

MR. ROISMAN: Taking this to a loss of coolant accident situation, the rods are hot. They are hot on the tip, the portion that points to the bottom of the core.

As water starts coming into the bottom of the core and begins to move up through the core, at the very bottom, the first time the water gets to a point where it is horizontally even with the bottom of the rods, at that point is there a pressure difference between some of the bottom of the rods and other of the bottom of the rods?

MR. MOORE: Only because -- Yes, because flow is trying to come out of the hotter assembly into the cooler assembly.

MR. ROISMAN: But you assume that there is no pressure difference there; is that right?

MR. MOORE: Yes.

MR. ROISMAN: In the calculations?

MR. MOORE: Yes, and that gives me the highest flow out of that assembly. There is no retarding pressure drop in the flow redistributing from the hot regions into the cooler regions.

MR. ROISMAN: I'm sure we are getting into a level of hydraulics which is well beyond my ability to fully understand.

Just from the standpoint of common sense, the first time that water gets to the bottom of the core, it is already beginning to diverge away from the center where it is warmer, to the sides. That seems to me to be a more conservative way of figuring out how much water got into the channel for the hot rods than it is to assume that all of the rods got the same amount of water moving up through the bottom, and later, as they got further up, they began to do their diversion.

MR. MOORE: I think I follow what you are saying. I think it comes back around to my statement earlier that I want to calculate these effects over small axial increments for that very reason. So that I do get the effect, increment by increment, as I traverse from the inlet to the outlet.

MR. ROISMAN: I like Mr. Ford's knack with the pen. Maybe we can get something here.

Assuming this is a line that represents the bottoms of the rods. All the bottoms of the rods are right here. As I understand it, if we are going to look at a geographic region -- And in this geographic region of the core a hot assembly is present. Let us say it sits here. In this region, which is a two-dimensional representation of what is a three-dimensional situation, there are other regions out here and here cooler. Water is coming up like this.

I gather in your assumption you are assuming it is coming up equally everywhere, the same amount of it.

CHAIRMAN JENSCH: When he agrees, it will help us.

MR. MOORE: Yes.

CHAIRMAN JENSCH: Proceed.

MR. ROISMAN: As it is coming up here and these assemblies go up -- And here is the top like that and it runs through the middle like that. This, for our purposes, will represent the hottest region of the hot assembly which, if we are merely defining it in terms of power distribution, represents roughly one-seventh of it. So far this is accurate. Will you say yes?

MR. MOORE: That's yes for the temperature.

MR. ROISMAN: I understand. Whether it is temperature calculations or anything else, the power distribution is the same for one-seventh of the length of the rod; is that right?

MR. MOORE: Not in the core. We discussed earlier that there is a variation along that length of a few percent on either side.

MR. ROISMAN: Other than that few percent, it is roughly, if this is the hottest, 17.4 kw per feet; is that right?

MR. MOORE: Yes.

MR. ROISMAN: What we are trying to figure out, what the THINC Code is trying to give us information about is how much water is going to get here, and once we know that, you can make heat transfer determinations, and you can tell us how hot that's going to get.

As I understand it, you assume that the water, as it comes up here, does make the first infinitesimal break through the bottom of these rods equally for all of the assemblies, and then it begins to diverge and you divided it up into twenty seconds to determine how much it diverges as it goes out. Is that correct or not correct?

MR. MOORE: Let me try to explain. I'm not sure about the twenty seconds.

MR. ROISMAN: I understand.

MR. MOORE: We divide it up into certain sections. Here is the cooler assembly and here is the hotter assembly. I take, within that little section, and I say the pressure P_0 here is the same.

MR. ROISMAN: That's at the beginning?

MR. MOORE: At the beginning. Then I say the pressure, P_1 is going to be the same at the exit of these two. Then I calculate the amount of water coming in here, coming in here based on that pressure difference. Then I've got heat coming in here and heat coming in here. But the heat coming in here in the hot assembly is higher and more than the heat coming into the cooler assembly, this assembly. So the coolant in here will expand and it will tend to give me ahigher pressure difference for this incoming flow in this region from here to here, the hot assembly, and in this region from here to here.

I have said that the pressure should be the same at the exit. So I calculate this pressure drop across here for both of these, and I do this by taking flow from the hot section here, this hot section here into the cooler section. So I am taking flow out of this hot assembly. I take enough out so that the pressure drop from here to here in the cold one and from here to here in the hot one are the same.

Now, that gave me a certain flow coming out of here and here. The flow is lower in the hot assembly than it is in the cold assembly, right?

You asked me, is there a pressure difference between the hot assembly and the cold assembly. I say, yes, there is a pressure difference to be expected that will tend to impede the amount of flow that goes from the hot assembly to the cold assembly. I say, forget about that. I won't assume I am impeding the flow and let it all go. That gives me more flow out of the hot assembly. Are we agreed?

MR. ROISMAN: My question was, down here you have heat radiating downward. There is more of it radiating downward here than is over here. Why don't you assume that the flow that enters this assembly at the beginning is less than the flow that enters that assembly at the beginning because of the higher heat that's down here at the bottom?

MR. MOORE: Is that what started all this? There is that much difference in heat that's going off the bottom of either one of these rods. The heat transfers from the bottom. There is no real difference between those two.

MR. ROISMAN: Is the power density different at the bottom of this rod than at the bottom of the rods in the cooler assemblies, than it is at the bottom of the rods at the hotter assemblies?

MR. MOORE: Yes, mainly measured in -- In a radial direction I'm talking about power density. You understand the power density down here is very, very low.

MR. ROISMAN: You mean the power density for any rod is very, very low?

MR. MOORE: Yes.

MR. ROISMAN: But what's the difference between them? That is what I want to know.

MR. MOORE: I don't recall the number offhand. The power difference between -- The level of power at this bottom of the core is so small that differences in power between the assemblies aren't really important. That's when you get up to higher powers.

MR. ROISMAN: Do you disregard it? Whatever the difference is, you disregard the difference?

MR. MOORE: Yes.

MR. ROISMAN: Therefore, is it fair to say, without giving a qualitative term, you overpredict the amount of water that enters the hot channel?

MR. MOORE: Well, yes, without any qualitative--

MR. ROISMAN: I understand that. Put your qualitative thing on it. I want to make sure we understand that. You say it is an insignificant amount of overprediction.

MR. MOORE: Yes.

MR. ROISMAN: That's all I wanted to find out about first.

MR. BRIGGS: May I ask a question, please? I don't want to break up the trend of thought.

In this vertical element that you have here that you calculated the flow through, is the flow entering the element and leaving the element the same, or do you actually calculate the radial flow, the element in your calculation?

MR. MOORE: Yes, we calculate the radial flow, and that's the whole purpose of this analysis. The flow here, W_1, if this is the hotter assembly, is W_0 and up the channel.

MR. BRIGGS: There are two ways of doing it. One wouldn't involve a radial flow calculation but a pressure drop calculation along the point, and having uniform pressure drops along the section. You would assume in and the flow out was the same for each element, but not the same for each--let's put it another way.

The flow in and out the section of center elements would be the same, and the flow in and out a side element would be the same, but not necessary equal to the center. You do calculate the actual radial flow as a function of position in each element; is that correct?

MR. MOORE: That's correct. As I understand the point you are making, if I calculated the flow in this element independent of the flow in that element, it is as if I had closed the channel. My assumption is that this is a completely open channel between the two, in fact open to the extent that I don't even take credit for a resistance to flow from this region to this region.

MR. BRIGGS: I guess we are not speaking the same language.

If you did actually calculate the radial flow, then you calculate a radial pressured distribution; is that correct?

MR. MOORE: I assumed that whatever flow is required out of this assembly in order to make the pressure drops between these assemblies is the same, and it does go out.

MR. BRIGGS: I understand that. The question I am asking is, let's say you have a section that is a foot long. Do you calculate the radial flow from the first inch, the second

Wt3

inch, the third inch, the fourth inch of that?

MR. MOORE: I calculate it for this total volume, no matter how many increments I split the core up into.

MR. BRIGGS: I am talking about the first length in the core. Let's say that's a foot long.

MR. MOORE: Yes.

MR. BRIGGS: Now, I am asking do you break that up further and calculate the amount that flows radially in the first inch and the amount that flows radially in the second inch, and the third inch?

MR. MOORE: No.

MR. BRIGGS: You don't do that?

MR. MOORE: No.

MR. BRIGGS: Let's assume that the radial flow is ten per cent of the flow that goes into the bottom of that element. So then you would have the flow going into the bottom of the element ten per cent larger than the flow going out the top of that one-foot section; is that right?

MR. MOORE: No. The flow in here is the same as the flow in here.

MR. BRIGGS: Let's take that central high power element. Let's assume, now, that as you make your calculation, that the amount that flows radially is ten per cent of the flow in. Then you end with a flow out of that section being ten per cent less than the flow in; is that right?

MR. MOORE: That's right.

MR. BRIGGS: So that you do actually have a difference in flow from the inlet to the outlet of that section; is that right?

MR. MOORE: Yes.

MR. BRIGGS: Okay.

MR. ROISMAN: I ask you along the line that Dr. Briggs was talking. As the flow comes up through this channel and--and this section is divided into three of your regions, roughly--and you figure the flow in this area comes in--let's use a figure so we will have some idea. It comes in at ten and gets out at nine. When you are computing heat transfer in the region, do you assume that it is the ten or the nine for this whole region in determining how much water is actually there?

In other words I think--but I'm not going to commit myself on it.--that an integral equation would be able to tell you at every instantaneous point along the line here how much of the ten was gone as you were going from ten to nine, and the total amount of water available at this point. Do you do that or do you only divide this, if it is twenty, into twenty segments and the segment is assumed to have the ten as flow for its entire length, and then jump to nine for the next one? I don't know if that's clear.

MR. MOORE: The calculation of the flow, the flow calculation that we did showed that under loss of coolant conditions, the flow at the hot spot, at the hot spot--and I am probably, if I recall, talking about an average of the region. The flow at the hot spot was reduced by ten plus per cent in the thermal hydraulic calculation. So in calculating the--now I am at the temperature calculation, which needs a flow input with respect to heat transfer assumptions. I arbitrarily assumed it was now down by twenty per cent. The two calculations are not done at the same time.

MR. ROISMAN: I understand that. If this is the rod on which the heat calculation is to be done, and it has here in the center a spot where the power density is all very close to 17.4 kw per foot, and it in turn is divided, we assume, into something like three regions for purposes of figuring out the flow of liquid around there, and I say to you, how much water is right there at a point one-third of the way up in the middle ofthis entire region, this one-seventh region divided by power density only, can you tell me what is there or can you only tell me how much entered that little area and how much left that little area?

MR. MOORE: I can tell you the flow rate at that point was the average between the two. You are talking about flow rate?

MR. ROISMAN: I am talking about how much water was there. We talked about water in and water out.

MR. MOORE: Flow rate we are talking about, pounds

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per second going in and pounds per second going out. If it steady state, then pounds per second--if it is steady state, pounds per second going in, and this is the hotter region, there is slightly less coming out. You can figure out then at any given instant time. There is only a certain amount of water in there which is based on the density of the water in the volume of the region.

MR. ROISMAN: In your computation of what the temperature would be at the hottest spot and the hottest rod in the worst loss of coolant accident, do you break it down into a point by point analysis of the amount of water available at a given point for purposes of determining heat transfer?

MR. MOORE: No. For purposes of heat transfer, we look at the temperature. What is important is the temperature of the water in this region for which the cladding is conducting heat. That is the temperature for which the cladding is conducting heat. The amount of water there is not important. It is only important in terms of the temperature of the water, as the water goes through the core.

In loss of coolant accident, the main concern is turning the temperature around during the reflood part of the transient, in which case there is no water in this particular elevation until the entrained steam and water that hit the bottom of the core reaches this hot spot.

MR. ROISMAN: We started off talking about a certain amount of water that is down here and coming up through the core. We were discussing the fact that radial flow, as a result of the fact that at the hotterspots it is going to push the flow out. You have a computation by which you figure how much water comes through here and how much of it is going to go around. You divided it up in order to make that computation into twenty, we assumed, subject to your correction as to what the real number is, X number of regions.

I believe that you said in figuring out the X number of regions, you figured the flow in and then, assuming the pressure is equal, you figured the flow out based upon how much water do I have to lose in order to get the flow out to have equal pressure at the beginning and the end.

What I am asking is, along that little section of X, that little piece that you look at, do you know at each given point what the flow is? I shouldn't say do you know. Do you use it in the computation of how much flow there is for purposes of determining, eventually, the temperature of the water and temperature of the rod?

MR. MOORE: For the calculation of--no, I don't go through a point-by-point calculation of flow in determining the temperature of the rod with the LOCTA code.

MR. ROISMAN: Do you for some other codes? You mention LOCTA. Is there another code?

MR. MOORE: The confusion here is, I did a detailed thermodynamic analysis with the THINC Code to see whether or not I could get large flow redistributions. So my assumptions of average flow in the core would be correct. The detailed thermodynamic calculation said I might have ten percent-plus flow redistribution. So I am not going through a detailed thermodynamic calculation here. Because I am not, in order to be properly conservative, I arbitrarily assume that the flows that I do calculate -- And they are calculated through the SATAN Code, which is calculating flows for the average assembly of the core. I properly reduce that flow in a conservative way to make sure that I am not going to underpredict a peak temperature. That's the confusion. There are two different codes. One was a benchmark to tell me what to use on the other.

MR. ROISMAN: You say that you reduce it by twenty percent. If you reduced it by 20.01 percent what would the peak clad temperature be then in your calculation? Would it be over 2300 degrees Fahrenheit?

MR. MOORE: Trivially, yes.

MR. ROISMAN: It would be?

MR. MOORE: Yes.

MR. ROISMAN: Can you tell me how you determine that this twenty percent only was the appropriate margin of safety?

MR. MOORE: I did the detailed thermodynamic calculation and saw that the real number was more like ten percent. So I used twenty percent.

MR. ROISMAN: Why didn't you use eighteen percent or twenty-two percent? That is what I am trying to find out. Twenty came out right on the nose. Did you know in advance that it would be 2300 degrees Fahrenheit if you used twenty percent?

MR. MOORE: Yes.

MR. ROISMAN: In other words, it was just a very lucky break for this applicant and Westinghouse that the twenty percent figure that you picked showed exactly 2300 degrees Fahrenheit as the peak temperature, and that you, in figuring how much margin over ten-plus percent you should use, if you picked twenty-one percent, we would be having some other kind of hearing altogether?

MR. MOORE: No. Look on the margins of many of the inputs that have been described in the calculation. That's just one of them.

MR. ROISMAN: I understand.

MR. MOORE: Take any given --

MR. ROISMAN: How did you figure to do it at twenty? Why didn't you set it at fifteen or eighteen?

MR. MOORE: My judgment of twenty was an adequate number representing the maximum possible flow redistribution.

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

MR. ROISMAN: Yes. I'd like to come back to it.

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

(The luncheon recess is taken at 12:35 p.m.)

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

CHAIRMAN JENSCH: Please come to order.

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

MR. ROISMAN: Yes.

Mr. Moore, before the recess we were discussing this twenty percent conservatism figure that's used on radial flow, and I was trying to find out what kind of factors went into your determination that that was sufficiently conservative. I wondered if you could tell me what they are now and let me just see if I have got my facts straight. You did not know when you determined that twenty percent was conservative and conservative enough that if it had been twenty-one percent you would have exceeded the 2300 degree Fahrenheit for fuel clad; is that correct?

MR. MOORE: That's correct. And perhaps I can elaborate on that so it's going to confuse you none.

The fact that the 2300 degrees is not a fortuitous happenstance of all things coming together and just equaling the criteria, we actually set the maximum peak kilowatts per foot for this plant on the basis of maintaining the 2300 degrees Fahrenheit. In other words, we did the calculation with all these assumptions and then determined the maximum allowable kilowatt per foot to maintain 2300 degrees.

MR. ROISMAN: Now getting to the twenty percent figure itself --

MR. MOORE: That is as I indicated earlier just a judgment factor that we have applied on the basis of the calculations of possible flow redistribution under these conditions and we have taken, in our opinion, a reasonable conservative factor above that.

MR. ROISMAN: Now the ten-plus figure is a figure that comes from experiments which in turn verified the THINC Code, or does it come from the THINC Code itself?

MR. MOORE: From the THINC Code itself.

MR. ROISMAN: And the THINC Code came up with ten plus what? What was the percentage, do you know?

MR. MOORE: I am not sure if it was ten to fifteen or less than fifteen.

MR. ROISMAN: Now do you know of any experiments which would have warranted thinking that it would have been eighteen or nineteen?

MR. MOORE: No.

MR. ROISMAN: Did you know of any experiments that would have warranted thinking that it would have been sixteen or seventeen?

MR. MOORE: No.

MR. ROISMAN: Did you do some sort of a scoping analysis to figure out that twenty percent was the worst possible case?

MR. MOORE: Only insofar as the analysis which gave us the ten percent value was in our opinion a conservative analysis, which would give us a large flow distribution.

MR. ROISMAN: But in other words, there were no specific tests, experiments that you relied upon in determining the twenty percent had the appropriate margin of conservatism in it?

MR. MOORE: Only those that we had to verify the adequacy or accuracy of the THINC analysis.

MR. ROISMAN: The THINC analysis didn't come up with the twenty percent. It came up with a lesser figure, is that right?

MR. MOORE: That's correct. What was your original question?

MR. ROISMAN: My original question was what experiments do you rely upon in saying that the increment over what the THINC Code predicted was an appropriately conservative increment?

MR. MOORE: And again my response is that the THINC Code for this analysis overpredicts the flow redistribution. So I don't expect the flow redistribution to be more than what the THINC Code would calculate. Therefore in assuming twenty percent I have additional conservatism.

MR. ROISMAN: Why did you do that?

MR. MOORE: Why?

MR. ROISMAN: Yes.

MR. MOORE: It's just our judgment as to what the appropriate assumptions should be for the analysis.

MR. ROISMAN: But I mean for every single figure that adds into the analysis of the emergency core cooling system performance did you in every single case add the same margin of conservatism as you added to this figure on radial flow?

MR. MOORE: No.

MR. ROISMAN: Well then, there must have been some factors to determine what the margin of conservatism was that was warranted, and I was asking why did you add it at all? Why didn't you just go with the THINC Code figure?

MR. MOORE: Primarily because I did not -- We did not want to have to be dependent on a very detailed calculation and wanted to assure ourselves that there is no question as to the margin we had for this particular parameter.

MR. ROISMAN: Well, is there some measure of imprecieness in the THINC Code that this was designed to compensate for?

MR. MOORE: No.

MR. ROISMAN: It's in your opinion a very precise accurate measurement?

MR. MOORE: I said before the THINC Code will overpredict flow redistribution.

MR. ROISMAN: So it's precise or better for your purposes.

MR. MOORE: Yes.

MR. ROISMAN: Then why did you go even further, if there is already conservatism built into the THINC Code that gives you a margin over what the "realistic" figure would be? Why did you add any additional margin? What was your motive in doing that?

MR. MOORE: Just arbitrary additional conservatism which we assigned to that particular parameter.

MR. ROISMAN: Are all your conservative figures equally arbitrary?

MR. MOORE: No.

MR. ROISMAN: Why don't we take the arbitrary twenty-five percent figure?

MR. MOORE: Because in my judgment twenty is fine.

MR. ROISMAN: But what enters into your judgment? You said it was arbitrary, which is an antithesis of judgment in my mind. Twenty-five is an arbitrary figure too and I just picked it. I don't have any judgment about that. What entered into your judgment that twenty percent was the right figure?

MR. MOORE: Just the several facts that come into this. I want to be assured that there is no question in someone's mind about the fact that the number is, the twenty percent, that the number will not be higher than that. That's my first consideration.

I have based --

MR. ROISMAN: Can I stop you? Can we talk about each consideration rather than have you list them all, or would you rather list them all and then go back and talk about it?

MR. MOORE: I'd rather give you the general discussion.

MR. ROISMAN: Okay.

MR. MOORE: We have a specific calculation which indicates the number is closer to ten percent. I choose not to perform this specific calculation ad nauseam, so I take a conservative low number to apply to the overall analysis.

MR. ROISMAN: What does that mean?

MR. MOORE: That means if I use the output of the THINC Code directly then I must in going through all the assumptions for the THINC Code, then I have to be sure that I have covered the proper conditions and assess each independent variable in the analysis. So I took the approach of calculating it under what we considered worst case conditions and applying an extra margin on top of that, to be sure that there was no additional concern. I wouldn't give the difference any more credence than that.

MR. ROISMAN: Well, what was it in the THINC Code analysis, to the extent that you used it to come up with your ten-plus figure, that the margin of conservatism was designed to correct, if you will, or compensate for, or leave you without having to do the specific calculation <u>ad nauseam</u>, as I think you said?

MR. MOORE: As far as the calculation, the specific calculation was concerned, we feel we have the upperbound and that we did not have to apply any additional margin.

MR. ROISMAN: If you feel that you didn't have to, who made you do it?

MR. MOORE: We voluntarily did it.

MR. TROSTEN: Mr. Roisman, excuse me.

We have discussed this in the past and I hope that you are not resuming the line of inquiry with Mr. Moore that you took with Mr. Wiesemann earlier in the hearing about this basic question of, you know, how conservative is conservatism. I assume that you are not trying to do that.

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MR. ROISMAN: My recollection was that Mr. Wiesemann was at least prepared to identify some factors that entered into the judgment, and the impression that I got from Mr. Wiesemann's testimony was that factors make up what constitutes conservatism. This witness says, as I understand it, that the THINC code is good or better than could be expected. That is, it already overpredicts. But he added something else in there first, because he didn't want anybody to raise any questions, that didn't work. I am raising questions. So it didn't fulfill the first purpose.

The second was that he didn't want to keep doing specific calculations ad nauseum. That's the one I don't understand. I am not at that issue, but obviously that issue underlies virtually every bit of cross-examination we do.

MR. TROSTEN: Sure. I am not quarreling with your questioning. I am just trying to figure out where you are going so I can know whether I should form an objection to it. That's all.

CHAIRMAN JENSCH: Let the cross-examination proceed, please.

MR. ROISMAN: I am going toward no.

The specific calculation that you said you have to do ad nauseum, what did that mean? To make the THINC code applicable to the Indian Point 2 reactor, would you have to recalculate in some way, do the calculations again for each spot in the core or each point on the rod or something like that that you didn't do?

MR. MOORE: Yes. If I used the exact analysis and exact THINC code analysis for each calculation, that's correct.

MR. ROISMAN: Did you not use the exact THINC code analysis for each calculation?

MR. MOORE: I used an upper bound calculation of redistribution and added an additional margin on top of that.

MR. ROISMAN: Why wasn't just doing the upper bounds sufficient?

MR. MOORE: I felt it was not.

MR. ROISMAN: What made you feel that way?

MR. MOORE: Just an additional conservatism. It does not--it was not based on any specific concern with the initial THINC calculation.

MR. ROISMAN: Well, here is my problem that comes in. I assume that when you get the 2200-degree Fahrenheit figure for the cladding temperature that at some place along the line you added a simple mathematical addition, X + Y, and it's well-established that we wouldn't have any argument about the fact that if just assuming that the numbers were 2100 degrees and 200 degrees, you'd add them together and come up with 2300 degrees, you do not, in that case, as an additional margin of conservatism add another twenty-five degrees. You don't just say, "Well, you can never tell when you add 2100 and 200 together that it's always going to come up 2300. So let's put an additional margin of twenty-five degrees."

In short, there are certain things, as I understand it, that are sufficiently well-established that it would be absurd to say that you were adding any margin of conservatism.

Now the THINC code came up with the calculation which you based upon a "upper bound" and you say it already had in it a tremendous amount of conservatism. There must have been, I think, some reason why you went ahead and added an additional margin of conservatism, and what I'm trying to ask you to do is just very frankly to tell us what is it about the THINC code that isn't as good as 2100 plus 200 equals 2300?

MR. MOORE: Nothing. If I calculate the flow redistribution it is ten to fifteen per cent in the THINC code, and I want to make sure under all conditions that I don't have any problem with flow redistribution. I don't pick 17,18, 21, 23. I pick 20. I think 25 is excessive. I think 20 is reasonable and that is my judgment and that is the judgment of my people.

MR. ROISMAN: But you said all conditions. Are there some conditions that the THINC code doesn't analyze?

MR. MOORE: None that I am aware of.

MR. ROISMAN: Are there some that you are able to imagine?

MR. MOORE: None that I was able to imagine.

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MR. ROISMAN: Then how did you know that the twenty per cent covered all the conditions?

MR. MOORE: I don't, but I know of none for which it doesn't cover.

MR. ROISMAN: Do you know any that nineteen per cent doesn't cover?

MR. MOORE: Absolutely none.

MR. ROISMAN: As Groucho Marx used to say, "This is your last chance to beat the other couple." Have you got any further rational explanation as to why if twenty per cent is the figure which you have used, that that has the appropriate margin of conservatism? Now is the time to say it. Otherwise--

MR. TROSTEN: Forever after hold your peace.

MR. ROISMAN: That's it, forever after hold your peace, right.

MR. MOORE: I have nothing else to add.

MR. ROISMAN: Okay. In answers to questions which we submitted to the Applicant on the 16th of September dealing with the Emergency Core Cooling System and the answers were provided and served on the parties and the Board on the 12th of October and the answers or the questions are referred to as Section C questions, a designation of the original letter of transmittal.

I'd like to direct your attention to that.

MR. ROISMAN: Just for the record it's in answer to question C.3 and I will read the question so that you can give it some thought.

"In what manner was the SATAN 5 Code compared to the semi-scale tests A 4.5-A 5.1 and what differences in results as to any reported phenomena occurred in those tests than what was predicted by the SATAN 5 Code?"

MR. TROSTEN: Would you wait just a minute, please.

MR. ROISMAN: Yes.

MR. MOORE: I am sorry. Which question was it again now?

MR. ROISMAN: C.3.

MR. MOORE: Yes, I have it.

MR. ROISMAN: Would you mind just reading over quickly the answer there.

MR. MOORE: Yes.

MR. ROISMAN: Now directing your attention to the second sentence it says, "The comparison of the test results, meaning the test results from semi-scale test 848 and the SATAN Code comparison of the test results with regard to the SATAN Code calculations indicates good agreement between the measured and calculated pressure and flow transients."

Can you tell me what the agreement was between the measured calculated pressure and flow transients?

MR. MOORE: As I recall the pressure transients were within 100 psi and better than that over much of the transient. The flow comparisons, I don't recall the number offhand. We will have to get the number.

MR. ROISMAN: Is it reasonably handy or not?

MR. MOORE: One minute please. Apparently not reasonably handy.

I think I have it. The flows over most of the transient look like they were within three or four percent. There was one point in the transient where they were about fifteen, twenty percent apart, it looks like.

MR. ROISMAN: Can you tell me what pressures were being measured, what flow transients were being measured?

MR. MOORE: These were pressures at various points around the system that we were comparing.

MR. ROISMAN: Do you mean during blowdown, after blowdown?

MR. MOORE: During, yes. This is a function of time. During blowdown.

MR. ROISMAN: Okay.

MR. MOORE: And also flow rates were measured at the various points in the semi-scale compared to what the code predicted at that point.

MR. ROISMAN: Were the flow transients the same kind of flow things that we were discussing this morning, that is flow through the core?

MR. MOORE: Yes. Some of the flows represented flow through the core.

MR. ROISMAN: Can you tell me in what portion of the flow transients the difference was something like fifteen to twenty percent?

MR. MOORE: Pardon?

MR. ROISMAN: Which portion of the measurement of flow transients, in other words which particular flow transients did the data code predict a difference from the semi-scale 848 of from fifty to 100 percent?

MR. MOORE: Station 35 in the Loft Tests, and I'd have to go back to the -- If you have the semi-scale report you can find Station 35.

I do have it. It's Station 35.

MR. ROISMAN: Is the prediction of the SATAN Code more conservative than the observed or does the observed present a worse condition than the SATAN condition on these flow transients?

MR. MOORE: I can't tell until I know where the station specifically is.

MR. ROISMAN: I take it that the answer to this question wasn't actually written by you?

MR. MOORE: No, it was written by me based on looking at and analyzing the comparisons between the measurements and the test.

MR. ROISMAN: You just don't remember now what Station 35 was?

MR. MOORE: That's right.

MR. ROISMAN: Oh,

In your opinion is good agreement something that you feel means a certain percentage difference between the predicted and the observed results?

MR. MOORE: Not necessarily. An important consideration is that one is able to get the behavior of the pressure and flow together throughout the course of the transient. That means you get the right general characteristics of the blowdown, shape of the curves and so forth. Determining a specific required accuracy is really difficult to do.

MR. ROISMAN: By the way, is the SATAN Code that the test was run with, was the SATAN 5?

MR. MOORE: Yes.

MR. ROISMAN: The code is designed to simulate the event and as I understand it, that portion of the loss of coolant accident is scheduled to the blowdown portion that's being simulated and figures and predictions that are based upon the code to determine how the emergency core cooling system is going to operate. From time to time you run or someone else runs and you have a chance to compare an experiment that's designed to prove out the code, either an experiment like semi-scale, which was designed to prove out its ability to predict, or an experiment that tries to directly represent what has happened in the -- What will happen in this reactor. And then you get a double thing. You cannot only check the predictions of the code, but actually find out or get an idea of what is going to happen in this reactor if the event occurs.

I am trying to find out since it's the codes that we see usually when you run these tests what is the requirement that you put in for the measure that the code must correspond with the prediction? Does it have to have a certain percentage of accuracy?

MR. MOORE: You don't really assign a specific percentage of accuracy. What you look at is to be sure there are no anomalous effects between the code and the specific experiment. For example, if the code said the pressure went up and the experiment said the pressure went down, at a given point in time, and there was no explanation as to why the abrupt change in direction of pressure, there would be cause for the code and you would try to determine that. If both the pressures go down at the same time and one happens to be different than the other by a certain amount, that in itself is not a critical situation. You have to look at other variables, too. You look at flows for the same kind of a judgment. It is a total transient that is of interest and its effects on the result, not whether the specific pressure at any given time is exactly identical between the code and the experiment.

MR. ROISMAN: Do I understand that the code's function is not to exactly predict what will happen?

MR. MOORE: In the test?

MR. ROISMAN: In this reactor if the accident occurs.

MR. MOORE: The code's intention is not to exactly predict in this particular case, that's right.

MR. ROISMAN: It is your feeling that it is to always overpredict; is that correct?

MR. MOORE: That is the intent, yes.

MR. ROISMAN: Would a single data point where it underpredicted be a flaw in the code that would require a correction? Would that be the kind of thing that would cause a modification in the code?

MR. MOORE: Not necessarily, but that's the kind of thing you look at to see if there is anything different with that specific test.

MR. ROISMAN: In comparing the SATAN Code to the result in the semi-scale test 848, it says, in a further portion of this answer, "The results of the SATAN Code calculations confirm the test results that indicate that the water injected in the lower plenum was discharged and none of the accumulator water was stored in the lower plenum of the vessel."

Do I understand that to say that SATAN 5 Code had always predicted that the accumulator water would be lost during the course of blowdown in a reactor such as the semi-scale reactor?

MR. MOORE: No.

MR. ROISMAN: Could you explain to me -- Was this a modified SATAN Code that was then applied after the fact to what happened in the semi-scale to see if it, in its new form, would accurately predict what happened?

MR. MOORE: No.

MR. ROISMAN: Will you explain it, please.

MR. MOORE: Yes.

MR. ROISMAN: Please.

MR. MOORE: The reference here is to a prediction of what would happen in semi-scale. It was using the semi-scale geometries, not the geometries of a reactor system. So the semi-scale geometry was such that the injected water was injected into the lower plenum, and the lower plenum was very, very much smaller than the lower plenum in the reactor. So the code would put the water into the lower plenum, but at the same time there is calculated steam flow rever sed through the core which swept this water right out. The code predicted this. It is merely because of a specific geometry on semi- scale test, and we would have expected. The code said that this water was in fact swept out.

This is not the situation in a reactor.

MR. ROISMAN: That was the first question I asked. Had the SATAN code always, if you had compared it to the semi-scale 848 test,had the SATAN Code in its form 5, before 848 was ever run, had it had within it the capability to predict that the semi-scale reactor, the one that was actually used, had water injected during blowdown, that the water would be lost?

MR. MOORE: Let me be sure I understand the question. The SATAN 5, the code you use, in its form was used to check the semi-scale test and it predicts that the water was lost in semi-scale?

MR. ROISMAN: Not based upon any modification, based on the SATAN Code after the interim policy statement or anything like that.

MR. MOORE: No, no modifications were required.

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MR. ROISMAN: Had you ever had the occasion to run the station code and make a prediction as to what would happen in semi-scale test before the test was actually run?

MR. MOORE: No, not in the case of the test with ECCS injection. In fact, I don't think we, during the time, had pre-predicted any semi-scale tests. We had been comparing results of various semi-scale tests as they came out with the station code.

MR. ROISMAN: So Westinghouse was not at all surprised to find out that all the accumulator water was lost during blowdown of the semi-scale tests?

MR. MOORE: Well, we were surprised--when the test results came out, then we looked into what the test situation was with ECCS, how they had simulated ECCS. When we reviewed the manner in which the ECCS was injected with the geometry, it was not a surprise. But that was after the fact.

MR. ROISMAN: Okay. I understand that.

In other words, you just hadn't done any -- you hadn't predicted it in advance?

MR. MOORE: I hadn't gone into detail at the time.

MR. ROISMAN: Let me direct your attention to Question C.5. Can you just quickly look over the question and the answer?

MR. MOORE: Yes.

MR. ROISMAN: In the answer you make the statement, "upon the completion of the blowdown part of the transient, the amount of water that was injected by the accumulators during the blowdown transient is totaled and subtracted from the bottom plenum and downwater inventory."

I think when I read that answer I had gotten the impression that there was some water left in the bottom plenum following blowdown. I thought this morning you had testified that there was not.

Am I misreading the sentence?

MR. MOORE: No. That's a general statement as to how we handled the injection of accumulated water. At the end of blowdown we determined the amount of water remaining in the system. We subtract the amount of water that's coming in from the accumulators. I said the result for Indian Point leaves us with essentially an emptyvessel.

MR. ROISMAN: You came up with a minus figure?

MR. MOORE: We never get a minus figure. You throw all the water away or--that's it.

MR. ROISMAN: I am just trying to understand the computation.

MR. MOORE: Yes.

MR. ROISMAN: You had 40,000 gallons of water that was the inventory in the bottom plenum in the downcomer. The amount from the accumulators was 41,000. Therefore, zero. You keep using the water essentially. It has me puzzled.

MR. MOORE: It is a few cubic feet out of a thousand downcomer volume.

MR. ROISMAN: I just want to know whether essentially zero is different from zero.

MR. MOORE: No. One or two cubic feet, as I remember, out of a thousand.

MR. ROISMAN: But in any event, as you testified earlier, no credit is taken for any heat transfer as a result of the flashing of those few cubic feet of water?

MR. MOORE: That's right, no credit.

MR. ROISMAN: During the blowdown, is the main factor which produces steam forces that might be expected to keep the accumulator water out of the core, out of the vessel, are those the forces associated with the initial pressure that was in the system at 2250 psi?

MR. MOORE: No. The forces assumed to act on the accumulator water to prevent it from coming in is the steam-- water flowing through the core in the reverse direction into the lower plenum and up the downcomer annulus. The accumulator water comes in the cold leg nozzles into the downcomer annulus. Now there is steam flowing in the annulus and out the break.

It is postulated that the accumulator water spilling into the downcomer could be entrained by the steam flowing up the downcomer and carried out the break rather than falling down and accumulating in the bottom of the vessel.

MR. ROISMAN: Thus, at the end of blow downcomer is it assumed that the downcomer is empty?

MR. MOORE: Yes.

MR. ROISMAN: So we are starting at the end of the blowdown. With the water at the edge of the downcomer for the first time that pressure now is sufficiently low that the water can start to fall down; is that correct?

MR. MOORE: The flows are low, yes.

MR. ROISMAN: When the new water comes into the core, into the bottom of the vessel and refills and then refloods, there is a new steam generation that begins; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: What is it that prevents that steam pressure from continuing to push in the downcomer direction and retard or prevent new water from coming in once you get some water coming down and the steam starts to generate again?

MR. MOORE: Because the preferable relieving path for that steam is through the core into the hot leg and back around and out the break.

MR. ROISMAN: And that path didn't exist before because the hot legs had the water in them that would be in there during normal operations?

MR. MOORE: No. I don't understand. It didn't exist when?

MR. ROISMAN: Well, the steam didn't take that preferable route during the time during blowdown. It took a different route.

MR. MOORE: Because during blowdown we are blowing down the water that existed in the system.

MR. ROISMAN: It is because the water is no longer in those hot loops that the preferential direction can now go out there; is that correct?

MR. MOORE: Yes.

MR. ROISMAN: You will have to help me conceptualize this. As I understand the loops, hot leg, cold leg tells us which side of the generator we are on; is that right?

MR. MOORE: Yes.

MR. ROISMAN: And the accumulator is on each one of the four sides of the loops?

MR. MOORE: Yes.

MR. ROISMAN: When the accumulator water comes out, what prevents it from going toward the generator instead of toward the reactor?

MR. MOORE: Well, the pressure -- The accumulators are injecting in the lines -- Let me think. The accumulator water, as it enters the pipe, tends to go the other way, is impeded by the reactor coolant pumps and will flow toward the vessel.

MR. ROISMAN: Those coolant pumps are located between the generator and the accumulator?

MR. MOORE: Yes.

MR. ROISMAN: That is their inlet to the pipe is between the accumulator and the generator; is that right?

MR. MOORE: The accumulator is on the discharge side.

MR. ROISMAN: And the pump is between the generator and the accumulator?

MR. MOORE: That's right.

MR. ROISMAN: When do those pumps go on? Will you refresh my memory on that?

MR. MOORE: These are the reactor coolant pumps that are running during normal operation and now assume to be coasting down.

MR. ROISMAN: So that the pressure from them is slowly going down during the blowdown?

MR. MOORE: Yes.

MR. ROISMAN: At what point is the pressure from them going to get to essentially zero?

MR. MOORE: There is still spinning at the time I am injecting the accumulator water. When the total system pressure -- The difference in pressure around the system is very small because I have completed blowdown. So they are still spinning.

MR. ROISMAN: Is there water coming in from them?

MR. MOORE: Not from the sunction side, no.

MR. ROISMAN: I am trying to picture it. I have almost a cartoon in my head here of water leaving the accumulator and coming to the pipe. It now has two ways to go: I am trying to understand what's pushing it toward the reactor vessel which is hot. Why doesn't it go toward the generator which is presumably cooler?

MR. MOORE: There really isn't any real difference in temperature or pressure at this point in time.

MR. ROISMAN: The temperature in the whole system is the same as the temperature in the reactor vessel?

MR. MOORE: The temperature of the metal, the walls, which was a function of the initial temperature of the primary system, which is essentially the same all throughout the system.

MR. ROISMAN: The radiation of heat from the reactor rods is not moving down the line at all?

MR. MOORE: No.

MR. ROISMAN: It is still right at the rod?

MR. MOORE: Yes. The vessel wall doesn't see that kind of heat, no.

MR. ROISMAN: What is happening to it?

MR. MOORE: To what?

MR. ROISMAN: To the heat that's being --

MR. MOORE: It is heating up the rods. That's what's happening to it.

MR. ROISMAN: And not going away at that time?

MR. MOORE: No. It is adiabatic heat-up.

MR. ROISMAN: That's what I wanted to know. That's the adiabatic heat-up?

MR. MOORE: Yes.

MR. ROISMAN: If it weren't adiabatic and the heat instead were being taken away by air, now that the steam and water have gone, would there be a tendency for that air to keep the accumulator water from coming toward the core, and would there be some of the accumulator water post-blowdown, moving away from the core and out the breaks? In other words, not getting at the downcomer.

MR. MOORE: No. The direction of any steam generation -- Well, there isn't any air in the system. Any steam generation is from the core to the hot leg into the cold leg and back into the annulus, which is the direction the accumulator flow is going. It is the same direction.

MR. ROISMAN: Isn't that based upon the fact that the accumulator water is pushing from one end? In other words, doesn't the direction that you predict that the steam or hot air or whatever it is moves out of the core following the blowdown depends upon the assumption that there is accumulator water in the cold leg moving toward the vessel? Isn't it that that makes that direction a non-preferential direction for the steam?

MR. MOORE: It is just the fact that any accumulator water getting into the downcomer is acting to push any steam in the core back out the system, to the cold leg and out the break.

MR. ROISMAN: Let's go back to the pump again. The pump is still spinning. You said that pressure coming from that pump tends to get the accumulator water moving toward the vessel instead of away from it?

MR. MOORE: Not the pressure. The fact that the rotor is spinning and tends to splash the water and force it toward the core.

MR. ROISMAN: Do you predict the coast-down time on that?

MR. MOORE: Yes.

MR. ROISMAN: What is it, roughly?

MR. MOORE: Oh, it is, I guess, at least forty, fifty seconds. It got inertia that is added to the pump in the form of a flywheel. So zero fly, I would guess is forty, fifty seconds.

MR. ROISMAN: Is there any way in which you could run in the opposite direction?

MR. MOORE: No.

MR. ROISMAN: That's a design feature on it?

MR. MOORE: Yes.

MR. ROISMAN: It can only go in the direction of --

MR. MOORE: That's correct.

MR. ROISMAN: Once the blowdown period is over and the accumulator water enters the downcomer, is there any resistance at all that is assumed, or is the water entering the downcomer and the lower plenum and moving up toward the core at the rate at which it is leaving the accumulator, subtracting friction?

MR. MOORE: That's right. It is the rate at which it leaves the accumulator. It is filling now the downcomer.

MR. ROISMAN: In other words, there is no steam at all that is assumed to come in the direction of the downcomer? I understood you to say the preferential route is the route out the other way. There is no back pressure, whatever the proper term is?

MR. MOORE: That's right, there is none.

MR. ROISMAN: That is the way the code analyzes it?

MR. MOORE: That's right.

MR. ROISMAN: Is there experimental data that demonstrates that the steam never pushes back?

MR. MOORE: What steam are we talking about?

MR. ROISMAN: The second steam, the post-blowdown steam that is generated in the refill and reflood period.

MR. MOORE: There isn't any significant steam generated during post-blowdown until we reach the bottom of the core. Once we reach the bottom of the core, that steam that is generated by hitting the core must be relieved through the system and out through the break.

MR. ROISMAN: And it doesn't in any way put any pressure against the incoming water?

MR. MOORE: Of course, it does, but the driving function to push this steam out the system is the height of water in the downcomer.

MR. ROISMAN: That's what I thought. That is what I didn't understand. In other words, there is a pressure and a counterpressure. The steam, if it had its choice, would like to go the downcomer route as well as the other routes, and it is the water pressure in the downcomer that keeps it from coming in that direction; is that correct?

MR. MOORE: That's right. Steam will go the path of least resistance. The driving head is in the downcomer itself.

MR. ROISMAN: Is the driving head calculated at all times to be higher than steam pressure during the post-blowdown period?

MR. MOORE: The flow of steam out of the system that is generating the core, by ability to get into the core, is fixed by the downcomer. That is what is driving the steam out. If I were to generate more steam than that, then the pressure drop would increase and the pressure acting on the downcomer would tend to push the downcomer back up. So, in fact, you don't generate that much steam.

You only generate as much steam as the neight of the downcomer can push through the system. That determines the flooding rate.

MR. ROISMAN: In computing the blowdown period, do you consider that the conservative direction is to assume that the blowdown occurs as quickly as possible? If you were just playing with the time of blowdown and you wanted to make it more conservative than it is now, would you make the blowdown occur more quickly?

MR. MOORE: Yes.

MR. ROISMAN: And therefore, the conservative assumptions that are built into the whole variety of factors that go into determining how soon blowdown will occur are all directed toward making the blowdown occur more rapidly; is that correct?

MR. MOORE: Yes, we want to maximize flow from the system to maximize blowdown.

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MR. ROISMAN: Is it thirteen seconds?

MR. MOORE: I think it is about fifteen or sixteen seconds in this case.

MR. ROISMAN: Do I understand that under the design criteria for ECCS, the amount of accumulator water that is assumed to be lost is the amount that is injected during blow-down, whatever that amount is, that you simply take your cal-culation as to when the accumulator water begins, one blowdown is over, and figure out what the pressure is of the accumulator water coming out and subtract the appropriate amount of water? In your case, I think twenty-six per cent was the figure. I have seen that figure.

MR. MOORE: It is about that. That's right, yes.

MR. ROISMAN: If blowdown were longer, would more accumulator water be lost?

MR. MOORE: Yes.

MR. ROISMAN: If you lost an additional, say, twenty per cent of the accumulator water for purposes of calculating the cold leg double-ended pipe break, how would that affect the peak cladding temperature?

MR. MOORE: I have a case where I have a longer blow-down and I have lost more accumulator water.

MR. ROISMAN: Just for the moment let's hold aside the time of the blowdown. Assume the blowdown is as long as you have predicted it here, but for some other reason now the accumulator water is reduced by an additional twenty per cent.

MR. MOORE: That's strictly a postulation, right?

MR. ROISMAN: Yes.

MR. MOORE: It wasn't involved with time of blowdown, was it?

MR. ROISMAN: No. I am just asking if there were a twenty per cent reduction in the amount of accumulated water, what would its effect be on the peak clad temperature?

MR. MOORE: I don't have a quantitative number for that.

MR. ROISMAN: I understand.

MR. MOORE: What is does is delay the time at which you reach the bottom of the core and start to reflood. So it extends the period of time under adiabatic heat-up. So the temperature increases at a rate of, let's say, thirty degrees a second. So the temperature rise you get as a function of time, the delay in time to start to reflood.

MR. ROISMAN: I take it, since you asked me earlier, if we extend the blowdown period, that won't necessarily be the case? I take it the way we get a loss of additional twenty per cent of accumulator water is the blowdown period is extended longer. Would your answer still be that the effect would always be lay the temperature of the fuel rods, the peak temperature?

MR. MOORE: No, that wouldn't be the answer now

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because there is a compensating effect of the longer build-up period.

MR. ROISMAN: What is that compensating effect of the longer build-up?

MR. MOORE: You can get an indication of that by looking at the result in Table 1 in the description of the Loss of Coolant Analysis for Indian Point where the show the effect of a double-ended break, eight-tenths times the double-ended break, which is a smaller hole and a longer blowdown. The peak temperature occurs for the largest break, the fastest blowdown.

MR. ROISMAN: As I understand it, those analyses involve the assumption--I should say, they demonstrate how temperature is affected by the length of the blowdown time assuming the same--that the flow from the system is limited by the size of the break, and all other things are equal. Then it does not assume that you begin with a double-ended pipe break with the largest pipe, and then three-quarters of the way through the blowdown something happens to limit the amount of water that goes out that double-ended pipe. You have no analysis of that situation?

MR. MOORE: That's right.

MR. ROISMAN: At least not in Table 1?

MR. MOORE: That's correct.

MR. ROISMAN: Assuming now if something happened

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nine seconds into the blowdown, ten seconds into the blowdown, which had been for those nine or ten or seconds moved at the rate of a double-ended cold leg pipe break, and now for the tenth second it starts to blow down at the rate of .5 foot pipe break, and the stretch the blowdown time as a result of these slower blowdowns in the latter stage of the blowdown, would it still be the case that the rods would not heat to a higher clad temperature?

MR. MOORE: I really can't say. I have to go through a detailed calculation on the heat transfer effects during blowdown.

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MR. ROISMAN: But all I am saying is that it is possible that at someplace along the line of the double-ended pipe break, that the flow from the break, if it were altered to slow it down, would result in more accumulator discharge, using the criteria now? Because they say you assume that whatever accumulator water is ejected during blowdown is lost and it might or might not have the compensating effect that you spoke of, namely that the water remains in the core for a longer period of time. Is that true, that it's possible that such an event would produce higher peak clad temperatures?

MR. MOORE: In theory.

MR. ROISMAN: And you haven't done an analysis in which you have postulated such an event?

MR. MOORE: No, because there isn't any physical mechanism for such.

MR. ROISMAN: Let me mention, and I am not trying to raise this up to the probable stage, okay, but if the pipe that broke, the double-ended pipe that broke, the other end is assumed to have severed off completely, if the forces during blowdown, one of the pieces of that pipe that broke was higher than the other, it now falls and blocks so that the two pipes tend to overlap a little bit and we don't have at the latter end of the blowdown the double-ended severance, wouldn't that be a situation which, if it happened, which the rate of blowdown would now be slow, and it's physically possible in a sense that that is a way in which it could be slowed? I'm not asking you to say it is physically possible for the two pipes to fall back together again or in any sense fall back together.

MR. MOORE: That's a condition that would change the flow, I agree, but in that case if that is the postulated mechanism I would expect that to occur very early in blowdown. I think it's pretty clear we'd be better off if the blowdown loads occur early, the highest loads occur early in the transients, if that happened early I think we'd definitely be better off.

MR. ROISMAN: The earlier in the blowdown period that the slowdown occurs, the better it is in terms of what the ultimate cladding temperature will be. The later in the blowdown period that it occurs, the worse it will be.

MR. MOORE: That's right.

Now we are again back using the arbitrary postulation of the interim criteria as well. We can't lose sight of that. We are not talking physically now. We are talking of the model which throws accumulator water.

MR. ROISMAN: That's right. I wasn't trying to get away from that.

Is the rate of blowdown predicted using the Moody calculation?

MR. MOORE: Yes. In the saturated phase, yes.

Saturated blowdown.

MR. ROISMAN: Mr. Chairman, I have no further questions at this time that don't involve opening up extended areas, but there are a couple of things.

I have a note here from Mr. Ford that would be relevant to how we schedule the hearings next week. As you know, one of his reasons for not being here today was to take an opportunity to study the proprietary documents which we received the day before yesterday on the rod performance. And he has sent me a note saying that based upon his analysis of those documents it appears clear that we will have to get into an extensive discussion of the proprietary material. And it's my understanding that in that circumstance the Applicant will want it to be done in an <u>in camera</u> proceeding. And we respect the Applicant's request with regard to that.

Mr. Ford has suggested that we schedule it on Monday and has asked me to ask the Applicant in addition to being prepared for a full discussion of those documents to please provide, bring with them, copies of all the regression analysis data on the fuel rod failure tests. I hope nobody asks me what that means.

MR. TROSTEN: Mr. Chairman, Mr. Roisman's announcement on the record of this of course is the first understanding that the Applicant has that such an in camera session will be required. I don't know any more than Mr. Roisman does, I guess, what is involved.

MR. ROISMAN: No.

MR. TROSTEN: I would like to suggest that there be, that it is satisfactory to the Board that there be a brief recess in which I can discuss this with Mr. Roisman and discuss it with the witnesses from Westinghouse and try to figure out what has happened.

CHAIRMAN JENSCH: Let us take a fifteen minute recess. Then we will spend fifteen minutes more with a recess again at 2:30.

MR. TROSTEN: Thank you.

CHAIRMAN JENSCH: At this time let us recess to reconvene in this room at 2:20.

(A brief recess is taken.)

CHAIRMAN JENSCH: Please come to order.

Have we some further report on the release of these documents?

MR. TROSTEN: Yes, Mr. Chairman.

We are prepared for an in camera session for the discussion of the documents which we understand to be the following documents: WCAP 7495 L, Volume I and

Volume II which are the multi-rod burst tests and WCAP 7379 L, Volume I, which is the single rod burst test volume. And we are prepared for an in camera session on those volumes.

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CHAIRMAN JENSCH: Very well. I hope that the parties will bring with them some briefing of cases I am sure are developing in the several jurisdictions about the exact scope and breadth of proprietary information and the necessity of the classification and the maintenance of that classification which may or may not be affected by dissemination of the same information or publication of the same items and the different authorships. I think that probably has arisen in several situations where it may have been discovered that many of these things were dealt with in other areas and, the claim of proprietorship may have been an attempt to maybe utilize data that others had in fact developed. I don't know what the situation is here but I think there is a growing case law about this type of thing and we'd be glad to have the reference to the authors on Monday.

MR. ROISMAN: Mr. Chairman, will this be all right if it's oral at this time as opposed to a written brief?

MR. CHAIRMAN: Oh, yes. Any presentation to indicate to us that--

MR. TROSTEN: Mr. Chairman, do I understand that you are requesting that a presentation--that we be prepared with respect to questions that may arise on that subject or that you are asking that a presentation be made on the subject?

CHAIRMAN JENSCH: I understand there is a claim being made that that certain data are of a proprietary nature, MR. TROSTEN: Yes, sir. These are Westinghouse's designated proprietary reports.

CHAIRMAN JENSCH: We'd like to have that claim shown to be substantiated by case law as well as any other contention you have in that regard.

MR. TROSTEN: In connection with the discussion on the record at the hearing on Monday.

CHAIRMAN JENSCH: Yes.

MR. TROSTEN: I understand it.

CHAIRMAN JENSCH: Oh, yes.

MR. TROSTEN: All right, Mr. Chairman.

CHAIRMAN JENSCH: I think the proprietary law has developed a growing body of law in several respects about disclosure of material.

MR. TROSTEN: I'd like to just note for the record, Mr. Chairman, that the Intervenor has these documents. We have made these documents available to the Intervenors.

Mr. Chairman, may I ask this? I have been discussing with Mr. Roisman the possibility, since we are going to be having an in camera session with reference to the proprietary information, that we might be able to schedule an in camera session on plant security at the same time. Would you give consideration to that as a possibility?

CHAIRMAN JENSCH: Whatever is convenient to the parties and the Board except that all the parties will have to be considered.

MR. KARMAN: This may very well affect when we have our witness up here, Mr. Trosten. At the moment we were planning to have Mr. Thompson come up on Friday.

MR. TROSTEN: I am sorry. Excuse me, Mr. Karman, I had forgotten that point.

CHAIRMAN JENSCH: We will shift to whatever is convenient to the parties.

Is there anything further we can consider at this time before we recess.

MR. ROISMAN: I just want to make sure the staff has copies of the three proprietary documents also.

CHAIRMAN JENSCH: Of course, there is a possibility, I would imagine, that even if the material is shown to be proprietary we might have an in camera session with reference to the discussion of the proprietary information. I think that that was done. I am trying to recall whether Applicant's counsel was a participant with a former staff counsel when it came up.

MR. TROSTEN: I am aware of the case, Mr. Chairman.

CHAIRMAN JENSCH: And I think the Staff made the contention there that the data sought to be preserved as proprietary had been published in every technical journal that has been issued in the last two or three years. We will be glad to proceed with the matter at a later time.

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Is there anything further we can consider at this time?

MR. TROSTEN: Yes, Mr. Chairman. I have one announcement which Mr. Bruce Martin, who is the counsel for the New York State Atomic Energy Council, asked me to pass on to you, sir.

Mr. Martin is ill and he is unable to attend the session today because he is ill. He asked me to tell that to you.

CHAIRMAN JENSCH: We regret hearing that he is ill but we will let his presentation await arrangements among the parties here.

MR. TROSTEN: The other point that I wish to mention is that I have discussed the matter of the New York State presentation further with representatives of the State of New York and if Friday is satisfactory with Mr. Roisman this would be satisfactory, I am advised, by New York State.

MR. ROISMAN: That's fine, Mr. Chairman. We would be prepared.

MR. KARMAN: It's fine with us, Mr. Chairman.

MR. ROISMAN: To deal with it Friday of next week.

CHAIRMAN JENSCH: We will plan it that way.

One other item, the board wasn't clear as to what the parties had fully determined in reference to the schedule for the following week, and I take it that the subject matter, 12 Bt5

I believe its enumeration in the proposed agenda was such that we had better meet on Monday and probably utilize the entire of that third week to continue the session, is that correct?

MR. TROSTEN: We certainly anticipate meeting Monday of this coming week and utilizing the entire week and then moving into the environmental hearing one would hope, Mr. Chairman, in the third week.

CHAIRMAN JENSCH: Starting on Monday of the third week?

MR. TROSTEN: Yes, sir, starting on the Monday of the third week.

CHAIRMAN JENSCH: Very well. Just so we can be sure about it. Very well.

Is there anything at this time. If not, let us recess and reconvene in this room on Monday, November 8th at 9:00 a.m.

(Hearing recessed.)

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