UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

CONSOLIDATED EDISON COMPANY OF NEW YORK, INC.

(Indian Point Station, Unit No. 2)

Docket No. 50-247

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

Date - July 16, 1971 **Pages** 1475 - 1668

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Springvale Inn
Croton-on-Hudson, New York

Friday, July 16, 1971

The above-entitled matter came on for hearing, pursuant to notice, at 9:30 a.m.

BEFORE:

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

DR. JOHN C. GEYER, Member.

MR. R. B. BRIGGS, Member.

APPEARANCES:

(As heretofore noted.)

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CHAIRMAN JENSCH: Please come to order.

Are we ready to proceed with the program as outlined last evening with the interrogation of the Staff witnesses on containment spray?

MR. ROISMAN: Intervenors are ready, Mr. Chairman.

CHAIRMAN JENSCH: Would you indicate the witnesses you desire to interrogate.

MR. ROISMAN: Mr. Karman has advised me he will provide people capable of talking about the entire problem of the spray systems.

MR. KARMAN: Mr. Chairman, we will start with Dr. Burley on the stand. If he needs help, he can call for it.

CHAIRMAN JENSCH: Very well.

Dr. Burley, will you resume the stand.

Having been previously sworn, you need not be sworn again.

Whereupon,

GORDON BURLEY

resumed the stand as a witness and, having been previously duly sworn, was examined and testified as follows:

CROSS-EXAMINATION

BY MR. ROISMAN:

Q Dr. Burley, could you please state what your connection with this plant, Indian Point No. 2 plant, is with respect to the containment spray system?

A I formulated a Staff model for the evaluation of the effectiveness of containment sprays for iodine removal and I applied this model to the Indian Point 2 case.

Q Can you tell me, is the effectiveness of the containment spray system in a sense a mechanical function? That is, the spray only removes iodine to the extent that the spray drops come in contact with iodine?

A Yes.

Q Now, I understand that there have been some analyses done to determine whether or not the drop size is as was originally assumed at an earlier time in the model you developed for the purposes of evaluating this particular plant; can you tell me what data went into determining what would be the drop size?

A The data is based on a statistical analysis furnished by the supplier of the nozzles, corroborated by independent analyses by Oak Ridge National Laboratory and added to by the Westinghouse Corporation, Westinghouse people with respect to an evaluation of the additional increments in size by drop accretion due to steam condensation and also due to drop coalescence.

Q Can you tell me, in the types of tests that were run, what were the conditions that existed during the tests that were run?

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For instance, -- let me give you some of the things I am concerned with. The pressure, the content of the spray, the type of nozzles that were used, the atmosphere into which this spray goes, the height the nozzles were from the bottom, and the technique that was used for determining what size the drops were?

A I am not intimately familiar with all of the experimental details. I can tell you that in general the nozzles were tested under ambient atmospheric conditions and with a water spray and the size determination was made at approximately eight or ten feet below the point of exit.

Q Now, those are the tests that were run, I believe, when we talked yesterday with Mr. McAdoo, those were the tests run by Spraygo, the people who supply the nozzles.

A That is correct.

Q Those were run on the same nozzles that are on the plant, that is a random sampling of the nozzles?

A That is correct.

Q In your opinion, how does the fact that those tests were run, using plain water and not using the atmospheric condition or the pressure that would exist in a loss of coolant accident, affect the conclusions with regard to drop size?

A very little, if at all.

Q Is that because the drop size that was being measured was the drop size as the drop came out of the nozzle?

A Both that and also because the addition of boric acid and sodium hydroxide to the spray changes the viscosity and density of the liquid, essentially negligibly. It is still practically pure water.

Q In terms of the effect of turbulence inside the reactor vessel in the case of a loss of coolant accident, as a result of, let's say, either in the first few minutes or for a subsequent period of time, how would that affect the drop size in the eight to ten feet from the nozzle?

A It may to some extent increase the number of drops which collide.

Q Does that mean that it would increase the size of the drops?

A It would tend to eliminate some of the smaller drops primarily.

Q So we would have more drops -- we would have fewer drops and they would be larger? That would be the tendency I mean?

A That is the tendency.

Q What about the fact that the tests were run, again as I understood from Mr. McAdoo's testimony yesterday, with the pressure, instead of having pressure imposed on the bottom of the nozzle and the nozzle operating at its maximum pressure in order to get the pressure at the loss of coolant accident time, a difference was determined, and the nozzle pressure was assumed to be that? Would there be any effect on the drop size by having, physically having a counterpressure against the nozzle? Would that tend to affect the drops?

A It should not.

Q It should not affect the drops?

A That is right.

Q Have tests been run, do you know of any tests that have been run where the results can be compared to show that drop size under those conditions would not be affected?

A No. However, the design of these nozzles is such that the exit velocity essentially determines the drop size spectrum, and that is the only parameter which is of interest.

Q In other words, the fact that the exit, that the velocity coming through the nozzle, as would be the case in a loss of coolant accident, -- we will just pick some figures here -- let's say that velocity is at 100 pounds per square inch. It comes in contact with something that is 47 pounds per square inch, and pushing. That is, it is not a static velocity of pressure that is building in the reactor, but it can be expanded, maybe moving up from 40 to 47 and down to 43.

That in your opinion does not affect, or should not affect --

A Its differential pressure is what is of importance.

Q Now, in terms of the drop size, what effect does drop size have on the effectiveness of the spray?

A It enters into the theoretical equation to the first power.

Q Can you explain that in terms I understand?

A It is directly proportional to the drop size.

Q So that it becomes more effective --

A I am sorry, may I explain that?

Q Yes.

A There are two ways of analyzing drop effectiveness. One is to look at the drop size spectrum in detail and calculate efficiencies for various groupings of drop sizes. The other one is to take what is a mean drop size derived from the drop size distribution spectrum, and use only a single analysis.

This is the analysis that the Staff has used.

Q The first --

A Using the mean drop size spectrum.

Q Let me go back to the earlier question. As the drop gets larger, its ability to remove iodine increases. Is that what you said?

A No, the per drop, the ability to remove iodine is proportional to the surface area. And for a specific volume of liquid, the smaller the drops, the greater the surface area, and the greater the ability to remove iodine.

Q So that in terms of the effectiveness of this spray system, if the drops could be kept to the small end of the spectrum, that would be preferable for its effectiveness?

A That is correct.

Q To the extent that they are larger, that is bad, when we are talking about a given amount of spray available to be turned into drops?

A That is right.

Q Is there a limit in terms of the effectiveness of the drops as they get larger beyond which, even though they still have some chemical in them which would normally be available for chemical reaction, but the drop is sort of saturated, it can't absorb any more iodine, even though it has the sodium hydroxide in there?

A It is not a question of saturation for the larger drops, it is a question of limitation on the surface area compared to the iodine in the surround atmosphere.

Q No, that is right, I understood that. But I was talking about as an additional aspect.

A For the drops of interest here, there is no practical limitation.

Q Do you mean to suggest that if the drops were falling further, or were to be exposed to iodine for longer periods of time, you might reach this -- I am sure it is not an appropriate term -- saturation point?

A Actually the problem would be at the lower size limit, that one would reach an ultimate saturation limit for the smaller drops, not for the larger drops.

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Q I see. Is that occurring with regard to the very small drops that come out?

A For the absolutely smallest drops, it is conceivable, yes.

Q Now, how does that affect the determination of the effectiveness of the spray? As I understand it in the grossest terms, one way of looking at the effectiveness of the spray, you have to find out how much iodine you have, and how much sodium hydroxide you have, in order to see if you are going to have enough sodium hydroxide there to react with the iodine.

Now, if I understand what you just said, in the case of some drops it is possible that although they still have some sodium hydroxide in there which might be available for reaction with iodine, it may be that it won't happen, because they are saturated?

A A small number of drops may reach the saturation limit. However, this is taken into consideration in calculating the overall effectiveness of the spray for iodine removal.

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Q In what manner? In other words, have some tests been run to determine how many of the small drops should be considered to have a limited ability?

A Well, the easiest way of approaching the calculation --

CHAIRMAN JENSCH: Excuse me, I know you don't intend to, but I wonder if you would help us by answering the question -- if you want to by an explanation -- but answer it directly, yes or no. I think the question was have any tests been made on this?

THE WITNESS: None that I am directly familiar with.

BY MR. ROISMAN:

Q Then go ahead, I would like to hear the explanation.

A The easiest way of compensating for such a phenomeno is just to increase the assumed mean drop diameter in the calculation.

Q In other words, in effect wipe out of your calculations these small drops for which this event might occur?

A That is correct.

Q What tests have been run to determine at what point you need not worry about that problem any more in terms of drop size?

A No good experimental data is available on that point It is a calculational procedure.

Q Can you tell me, in terms of microns, at what drop size you just start disregarding all of the smaller drops, using the calculation?

A The smaller drop size limit is effectively in the 50 micron region or thereabouts.

Q I am a little bothered by that. I think eventually when we get to talking about the effediveness of the spray, when all of the elements are added in, we are going to be talking about, in a sense, how many drops of what size were available at a given time to absorbe iodine.

Now I would assume to do that you would have to have a specific cut-off figure. Is your moving around on the 50 micron size merely that you don't happen to have the figure at your fingertips, or there isn't such a figure?

A The figure is a calculational figure. And the number of drops below 50 microns is quite small in the case of these nozzles used in the Indian Point #2 containment.

Q How was the calculation made that the 50 micron size was the appropriate cut-off size for these small drops?

A One knows from experimental data the capacity of the sodium hydroxide spray liquid for iodine per unit volume. One can also calculate the volume of the individual drops by standard geometrical techniques and then one can compare these two.

Q Now in that sense, as I understood it earlier, the explanation of this saturation point, it is to some extend effected by time, that is the amount of time the drop has to be exposed to iodine?

A That is correct.

Q In the calculation in what manner is the time, the drop time, the time the drop is available to be in contact with iodine taken into effect?

A The only point where saturation limit is of interest is to calculate the infinite time saturation. Anything before that would obviously saturate the drop.

Q I understand that.

A So one calculates saturation for the 100 or so drop height in the containment.

Q How fast do you assume the drops fall?

Let me put it a different way: What factors do you take into account in determining how long the drops will take to fall 100 feet or whatever?

A I am using curves which were derived by Mr. Parsley of the Oak Ridge National Laboratory, which correlate drop size and fall times.

Q Is that based upon tests that were run, or is it a calculational model?

A This is a calculational model, but a calculational model which incorporates the atmosphere which one would find in the containment.

Q You mean its pressure and content?

A Pressure, temperature and steam.

Q What about its turbulence, updrafts, downdrafts, crossdrafts?

A None of that, of course. In general, updrafts, downdrafts, would be compensating. Updrafts would tend to keep, especially the smaller drops, in suspension for a longer time.

Q Do you know -- you say they would be compensating. They would compensate if there were an equal number of updrafts and downdrafts. Do you know if that is true?

A On physical principles, it has to be.

Q Well, where are the fans located physically in the containment?

A I would have to refer you to another member of the staff. I believe they are very close to the perimeter of the structure.

Q Near the top of near the --

A Near the top.

Q Do those fans draw? That is, they are sucking air towards them rather than pushing air in?

A That is right.

Q So would that tend to make the movement of the air go from the bottom of the containment to the top of the containment rather than the reverse?

A That is right.

Q Was that factor considered in the drop time equation by this gentleman?

A It was not.

Q Continuing on with regard to the whole question of how the drops get down, how is it determined what route the drops will take? That is, where they will go after they leave the spray nozzle?

A Again this is a calculational procedure where after the spray pattern has been established, a situation which occurs within about the first 10 or 15 feet after exit from the nozzle, that beyond that point the drops are assumed to fall straight down.

Q Do I understand that is effected by pressure, that is to the extent there is pressure in the containment vessel, that will effect how far out the drop can go from the line directly down the nozzle?

A That is correct.

Q In terms of evaluating the effectiveness of this spray system, what kind of assumptions did you make with regard to the drop trajectory? Did you try to trace it throughout the period of time you were gauging or measuring the change in drop trajectory as pressure in the vessel changed; or did you make a conservative assumption --

A We have not gone into a detailed analysis of drop trajectories. This is incorporated in the overall factor of conservatism which the staff has applied.

Q Can you tell me, then, in terms of the overall conservatism, what did you assume about drop trajectory?

A The drop trajectory was not injected in the analysis. It was incorporated in the composit of drop diameter and fall time.

Q In other words, to the extent the drop trajectory would affect spray effectiveness, you simply took account of the worst possible effect and disregarded the trajectory? Is that an accurate statement?

A I am not sure what one can consider a worst trajectory. All of these trajectories --

Q That is what I was going to ask you next?

A -- keeps drops inside the spray volume of the containment and effectivoly, whether a drop is displaced by an inch or two to the left or right is immaterial.

Q What about in terms of the question of drop coalescence through collision? Is that effected by trajectory?

A It is effected by the trajectory of drops of differing diameter. This is determined by the exit velocity from the nozzle.

Q Could you explain that to me a little bit, please?

A In that by experimental procedures one can determine what the distribution of different drops is in terms of the spray cone and larger drops are on the outside of the cone and smaller drops on the inside; and there is a certain amount of interaction.

Now one can talk about interaction of drops issuing from the same cone and also interaction of drops from an adjacent cone. And it really makes very little difference whether a spray pattern is much larger or much smaller in that the total interaction comes out to make very little difference.

Q Do I understand from what you said a few moments ago that your coalescence of drops through collision occur with drops that have different sizes? In other words, two drops of the same size, if they strike, what happens there? What would be expected to happen?

A The probability of coalesence is greatest with drops of differing size.

Q Now, if the large drops in spray . #1 have only the small drops in spray nozzle #1 to come into contact with, is the probability of collision and the probability of coalescence less than if the large drops in spray ~~nozzle~~ #1 have both their small drops to come in contact with and the small drops of adjacent sprays to come in contact with?

A Yes.

Q Can you explain to me, then, why you say that it isn't really relevant to be concerned with the trajectory of the drops, since if the sprays operate in such a way that no sprays overlap each other, that you have a lower, or would appear to have a lower probability of drop coalescence than you would if sprays do overlap and extend the overlap, as I understand what you just said, would increase the probability of drop coalescence?

A The staff has done a few rough calculations in this respect and we do incorporate drop coalesence from adjacent cones in our evaluation.

Q But how do you decide -- that is what I am trying to find out -- how do you decide whether you put enough drop coalescence into your model? What bounds you in terms of making your assumptions?

A What we have done was essentially mix drops at random from several adjacent cones, which is an almost impossible -- well, I shouldn't have used that term -- situation, let's say, improbable.

Q Maybe you could help me with it if we did it somewhat more mechanically. How far apart are the nozzles in the spray system?

A I don't have the detailed numbers.

CHAIRMAN JENSCH: This came up yesterday. I wonder if it would be helpful to take a look at the FSAR diagram that shows the specifics and while we have interrupted, do you have any of these calculational models or rough calculations that you referred to several times in the testimony, available for review? Not now, but sometime could they be brought into the hearing room?

THE WITNESS: We can make them available.

CHAIRMAN JENSCH: You do have them available, do you?

THE WITNESS: Yes.

CHAIRMAN JENSCH: Very well. Thank you.

In fact, if I counted correctly, I think you had six calculational models so far. And when you review the transcript, if you find any more, if you would include them. And if you have any experimental data, if you could give us references -- perhaps you could give us references?

THE WITNESS: We will endeavor to accommodate you, sir.

CHAIRMAN JENSCH: Thank you, sir.

MR. ROISMAN: Just for the record, can I share, as a recipient of that data, when it is submitted, please?

CHAIRMAN JENSCH: If it is available,perhaps you can review it before we resume whatever next session we have and then you can bring them to the hearing room.

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MR. ROISMAN: If that is possible.

MR. TROSTEN: We should have this information shortly, sir.

CHAIRMAN JENSCH: Well, we seem to be taking a little more time that I thought we would in this regard. I notice Mr. McAdoo and Mr. Grob perusing the books, but they can continue and perhaps we can go ahead.

MR. ROISMAN: All right, Mr. Chairman.

BY MR. ROISMAN:

Q Dr. Burley, the point I am trying to discover is with regard to your conclusions on the effectiveness of the spray. If I understand you correctly, you say that you take account of the drop coalescence that might occur, assuming that the trajectories of the sprays overlap; in other words, under the worst possible condition for coalescence, vis-a-vis spray overlap. Is that correct?

A That is all lumped into our factor of conservatism, yes. May I sort of indicate what our factor of conservatism is? It might help you.

Q All right.

A The mean drop diameter as issued from the nozzles under the worst conditions is of the order of about 950 microns, not including drop coalescence and steam condensation. The drop diameter which the staff has used in the evaluation of the Indian Point #2 spray effectiveness is 2,000 microns.

Q Now, when you picked 2,000 microns, what did you use as a basis for deciding that that was as conservative as you needed to be to take care of these areas that we have been talking about?

A We looked at the general effect of drop coalesence, drop condensation, uncertainties in the determination of the drop size spectrum in the first instance, and decided that there was a bit of uncertainty in all of these numbers.
And we then decided that we wanted some more time to perhaps make a decision in the future that it was easier to grant a larger iodine removal effectiveness at some later date than it was before you have all of the facts.

And we decided that a sizeable favtor of conservatism was warranted at the time the spray model was developed.

Q How come the figure wasn't 2,100 microns?

Let me put it a different way? If you were searching around, now, for the first time, trying to figure our what conservatism should be built in, and one of the people said 2,100, and somebody said 2,000; somebody else said 1,900 -- how would you decide? Or did everybody unanimously agree that the figure should be 2,000?

A I think I was the only one who held out for as large a figure.

Q Good. Then you are the man I want to talk to.

Why didn't you hold out for 2,100 or 2,500?

A It is a matter of judgment.

One look at the available data, and one decides just how much variation one would expect on an outside chance, and at that point one chooses a number. The number we chose was 2,000.

Q In terms of that, in terms of looking at the available data, what, precisely, was the available data that you had with regard to drop coalescence as a result of overlapping of the trajectories of spray nozzles at the time you made the judgment that 2,000 was correct?

And perhaps you could tell me at what time that occurred? That is, not the exact day, but month or year, if possible?

A The model was developed approximately two and a half years ago. At that point we lacked most of the data which we have now, other than the statistical determinations of the drop spectrum in the nozzle. And I had to perform some of these calculations myself. Most of the results which have come in since then have tended to show that the calculations I made earlier were quite conservative.

Q Have any shown that they weren't?

A No.

Q What did you choose to determine the amount, the maximum amount of drop coalescence that could occur as a result of the overlap of the trajectory of sprays? From the spray nozzles?

A The same answer that I gave you a few minutes ago, the mixing of the drops from several adjacent nozzles.

Q Hos did you know that there couldn't be mixing of the drops from more than a number of nozzles?

A Well, obviously there is going to be an infinite mixing of some droplets if one takes a propagation from one nozzle to the next. One has to decide what is a cut-off limit which gives you a reasonable number and also what are the physical principles which govern the falling of the larger droplets which are primarily instrumental in scavenging the small drops.

The small drops, themselves, do not deviate, ordinarily, too much from a vertical trajectory.

Q In that context, did you, in trying to figure out what would be the outer limit of the trajectory, in other words, the limits as far as you could see them, did you assume the worst possible loss of coolant accident conditions; that is, those pressures and atmospheres and heat and so forth?

A For calculating the fall times, yes.

Q How about for purposes of calculating the trajectories? Or for making what you just explained a moment ago, your attempts to figure out how much mixing would occur between the spray nozzles?

A Yes.

Q Did you testify a few moments ago that pressure tends to make the drops stay closer to the nozzle rather than spread out. That is, the higher the pressure, the more likely the drops are to stay near the center of the nozzle from which they come?

A I am not sure I said that. I would have to check which way it goes.

Q Could you do that, please?

CHAIRMAN JENSCH: While Dr. Burley is looking for those data, I wonder if Mr. McAdoo or Mr. Grob has found the distance between the nozzles in the spray system? I gather not yet.

MR. TROSTEN: No, sir.

CHAIRMAN JENSCH: Very well.

Will you continue, Dr. Burley, when you have your data.

BY MR. ROISMAN:

Q Dr. Burley, if it would be of any assistance, I think the document WCAP 7499L has some charts in it that suggest drop trajectory in the case of pressure.

A Yes. The drop cone spectrum is tighter at higher pressures.

Q Then in terms of evaluating the maximum possibility of coalition of large drops in one cone with drops in an adjacent cone, is that with the most conservative assumption with respect to pressure -- would it be the lowest pressure rather than the highest pressure?

A That is correct.

Q Can you tell me in terms of the trajectory of the drops to what extent does the mixing between the cones effect the coverage, thatis the physical coverage, getting the spray to cover every little nook and cranny in the containment?

A Well, first of all, obviously, we are not covering every nook and cranny in the containement, and depending to some extent on mixing and convection to bring unsprayed or iodine from unsprayed regions into the spray pattern. The more overlap there is between adjacent cone patterns, the smaller the unsprayed area or volume would be in the region which is actually accessible to the sprays.

Q Can you tell me, then, in the context of the Indian Point #2 plant, is there a difference in coverage of these sprays, a practical difference, between the time when the pressure is at its highest point and the cone is tighter, as I think you described it, and the time when the pressure is lower and the cones are broader?

A I would say effectively, no.

Q Is that because the trajectory of the cones, even in their tightest state, overlap one another? This may depend on this figure we are looking for.

A I would expect some overlap, yes, but also in the beginning when yourpressure is highest and the cones are smallest, tightest, the turbulence in the containment would be highest, your mass transport of iodine into the spray pattern would be greatest

Q And therefore you wouldn't have to worry that -- you cover, in effect -- I will ask you in a second whether this actually occurs. But in effect, you could have spaces between the trajectories of the outer edge of the cones from the nozzles and still figure that with all of the air moving around, the iodine would be moving from those empty spaces into spaces where spray was actually falling. Is that it?

A Yes.

Q Can you tell me, is there or has there been any analysis done regarding the effect of turbulence in the reactor vessel -- excuse me -- in the containment following the loss of coolant accident?

A In terms of mass transport?

Q Well, let's start in terms of anything and then we will narrow it down to some particulars.

A I am not directly familiar with that. I would have to confer with other staff members, I am afraid.

Q Just so I will be able to frame my questions properly for the period that you are on the witness stand, are you not familiar with the analysis done by the staff with respect to the whole question of physical distribution of drops within the containment vessel, and the effect of mixing of, this diffusion of the gas into where the iodine, into where the spray and vice versa? Is that a subject on which I should be asking a different person?

A The staff has not done a detailed analysis of all those factors. We have relied to some extent on the experimental data of several facilities to aid us in an understanding of the actual iodine removal capability of spray systems under post-accident conditions.

Q Do those experiments include conditions where the post-accident turbulence was simulated?

A They included the steam atmosphere and the quenching of the steam atmosphere by sprays, so turbulence essentially did exist in those experiments, yes.

Q What about the turbulence caused by a loss of coolant accident producing the steam in the first instance?

A There was some steam addition in a number of these experiments, yes.

Q My question was, was that steam addition, was there an attempt to simulate the conditions or the force of the conditions under which steam would be produced in the loss of coolant accident situation?

A Not precisely, no.

Q What about in terms of the turbulence or air currents created by the existence of the fan filter system?

A No.

Q What about the turbulence that would be created by the movement of the liquid on the containment floor to specific places where it has accumulated and taken through the sumps and so forth?

A Yes, I would say so.

Q You mean because these experiments were run with floors that had comparably distributed collection points for liquid that was falling to the floors?

A The Containment Systems Experiment Facility has a collection facility and a recirculation capability, yes.

Q Is the speed with which the collection occurs and the location in the experimental containment building comparable or identical to that which exists in Indian Point #2?

A Obviously not identical. I am not sure I can even say comparable.

Q Would the presence of the various structures that exist near the bottom of the containment in the Indian Point containment and the crane and the other structures that rise up, would their existence and the effect of their existence have been included in the experiments at the Containment Systems Experiment Facility?

A The containment systems experiment is a compartmented facility, so some of the volume of that installation is not directly accessible to the spray, yes.

Q But I was also talking just in terms of this mixing and interaction question, the force that exists following the loss of coolant accident. I assume that again we are talking about a relatively mechanical function, that is spray drops and iodine coming into contact with each other.

I was wondering whether the precise, if you will, baffles, because of the loss of coolant accident -- that is, in effect, as I understand it what all of these structures on the bottom of the containment vessel will be -- the specific location of the baffles with reference to the fans, with reference to where the other sources of turbulence are occurring, would effect this question of mixing.

First, is that correct? And secondly, if it is, has that been simulated in the containment spray experiments?

A Obviously structures in the containment do effect mixing behavior. The exact mixing behavior which would exist in the Indian Point #2 containment has not bee simulated.

Q With reference to the route that the drops take following their exist from the nozzle, would you say the possibility of coalescence of the drops would increase if you had a line of small drops that was crossed perpendicularly at one point and not again by a line of larger drops, or assuming the same line and large drops and small drops, if they fell relatively parallel to each other and tended to sort of knock back and forth between each other, due to turbulence and so forth -- under which condition would you expect greater coalescence of the drops to occur?

A Well, it depends on two factors. One is the proximity of the drops and the other one is the time for interaction. One would have to have a more detailed question which would detail the crossing of the two streams, what are the velocity of these two streams, and so forth.

Q Assuming the velocity of the sprays at the point 10 feet from the nozzle, under the design basis loss of coolant accident situation, and assuming that the time that the drops would be in contact with each other in somewhat parallel fall to be the time the drops are in free fall in the containment, from the time they leave the nozzle, assume that the 10 foot point, they come together and begin to fall in a somewhat parallel fashion in one case, and in the other case at the 10 foot point they cross perpendicularly and don't see each other again.

A If you use that hypothetical case you just mentioned, where the two streams come together and fall together, the probability should be greater.

I would like to point out also that the larger drops do fall at a much greater velocity than the small drops.

Q But they are --

A So two drop sizes which are together at one point don't stay together all of the way down to the bottom of the containment.

Q I understand that.

Can you tell me what effect that has on the question? Would that mean there would be a greater chance of collision, because the large drops have more small drops that they pass by that they might collide with, rather than finding a friendly small drop and staying a precise distance away from it during the whole fall?

A A larger number of large drops would tend to be in the proximity of the smaller drops.

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Q And that would increase the chance --

A Would increase the probability of coalescence.

Q To the extent that the trajectories of the sprays are different, depending on the pressure in the reactor vessel, when the pressure is high, I think you said the trajectory tends to be straight down.

Would that be then, vis-a-vis the large drops and small drops within that cone and with respect to the large drops in adjacent cones, would that tend to be a situation that more closely proximates what I discussed with you about the parallel falling of drops and during the time when the pressure in the vessel is relatively low, would there be a greater tendency for the contact between the drops to come somewhat more perpendicularly?

A There is a greater probability for coalescence of drops within an individual cone when the pressure is higher; there is a slightly higher probability of interaction between adjacent cones when the pressure decreases.

Q Now, in setting this 2,000 micron limit, did you run calculations or experiments to take account of those factors, those impacts upon coalescence in figuring out that the 2,000 micron drop was a maximum boundary, a very conservative figure?

A We did not run a complete drop size distribution analysis and coalescence analysis, no. We looked at the probabilities for coalescence, and decided that this would

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provide a very adequate margin for any existence of coalescence which we might not have included, such as nozzles with misdirected streams, this sort of thing.

Q But specifically in doing that, did you think it ought, did you consider, did you take any account of this factor we have been discussing about the drop trajectory having a greater chance for coalescence with small drops when the large and small drops tend to fall parallel to each other than when they tend to be perpendicular and have only one point at which they can coalesce, and the effect of pressure on increasing or decreasing those probabilities?

A We looked at what I would consider a realistic physical situation.

CHAIRMAN JENSCH: Could you give an answer yes or no. Did you consider what he asked you about?

THE WITNESS: I would like the question repeated.

(The Reporter read the question.)

THE WITNESS: I think my previous answer stands, the realistic physical situation is where drops fall parallel to each other and don't cross once and never see each other again. We looked at the interaction of drops within cones and adjacent cones.

BY MR. ROISMAN:

Q I thought I knew where I was going, now I am not sure.

Would the drops from nozzle No. 1 that come in contact with drops from any adjacent nozzles, the trajectory of the drops from nozzle No. 1, have to get to the points where the trajectory of the drops from nozzle No. 2 occur?

Is that correct?

A That is correct.

Q Now, in figuring out what the chance of coalescence is, I gather that to some extent you took account of the total number of drops that were available and something about their size distribution, because size is important in determining the possibility of coalescence.

Did you in that assumption assume that drops from nozzle No. 1 had crossed into the inner part of the cone from nozzle No. 2? Or did you assume that -- I mean if everything ran parallel -- I assume the AEC is still using geometry, if they all ran parallel, they wouldn't tend to cross each other, at least not within the 100 feet from the top to the bottom of the containment.

How did you compute the number of drops that had any possibility of coming into contact with each other, if you assumed, as I thought you just said, that they were always parallel?

A I think I have to go back to an answer which I gave previsouly, which I thought had clarified that point, I probably didn't get the idea across.

The probability of collision is proportional to the number of drops within a given unit volume. And if one mixes the drops from several different nozzles, this gives you the highest possible number of drops within the unit volume.

And this is the physical situation which we looked at.

So we essentially used a simplified conservative model for modeling our drop coalescence.

Q You assumed that all the drops from several adjacent cones were available for collision, but you assume that they were available for collision in the manner in which they would be falling, parallel to each other, where the maximum possibility of collision could occur.

Is that what you are telling me?

A Parallel, after approximately 10 or 15 feet from the nozzle, yes.

Q Now, in terms of figuring this, did you assume that the drops that were available for collision included only -- let's refer to the middle nozzle as nozzle No. 1 -- and all the nozzles, one nozzle removed from it, but none of the nozzles two nozzles removed from it?

A That is correct.

And calculate the effect on the first nozzle.

Q Yes, right.

CHAIRMAN JENSCH: I notice Mr. Grob just returned

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to the room. Does he have the data on how far the nozzles are apart?

MR. GROB: Mr. McAdoo is working on it right now.

CHAIRMAN JENSCH: All right, just keep on with you calculation. I see you have a pad and paper back there.

Will you proceed.

MR. ROISMAN: Yes, Mr. Chairman.

BY MR. ROISMAN:

Q What effect does the turbulence inside the reactor, or inside the containment have on the question of drop coalescence? Does it tend to increase or decrease the possibility of coalescence of drops?

A My first impulse is to say increase.

Q What sources of turbulence exist inside the containment vessel? In other words, from what places would you normally expect in the loss of coolant accident to have turbulence occur?

A The turbulence, of course, is purely thermal and there is the heat source in the core region and also there is the convection of steam going to the colder surfaces.

Q What about the fans and their operation? Perhaps I am using the word turbulence not scientifically accurate. Air movement in selected directions, I guess.

A I am not the world's expert on the addition of these convection curves. I would say the effect of the fan coolers

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would be very, very small.

Q Who is the Staff expert on that?

A I am not sure I want to give you a name. Maybe the man who knows best could identify himself.

CHAIRMAN JENSCH: Who will take that responsibility?

MR. KNIEL: Would you repeat the question?

You are interested in what the extent of turbulence is due to the fan coolers?

MR. ROISMAN: Yes. That is correct.

Whereupon,

KARL KNIEL

resumed the stand as a witness and, having been previously duly sworn, was examined and testified as follows:

XXXXX

FURTHER CROSS-EXAMINATION

THE WITNESS: Well, the fan coolers mostly at any rate, they are located around the peripheral of the containment, I don't remember the elevation, about one-third of the way from the bottom, something like that, and their function really is to provide, to distribute air to the various parts of the containment, and a lot of air goes to the parts of the containment that are under the deck.

In fact, I think the majority of the air goes to the containment under the deck. So that the air from the fan coolers, the majority of the air from the fan coolers does not affect the turbulence in the area above the deck, where all of the spray drops are falling.

Now, there are two ducts that go up the side of the containment, one on one side, one on the other side, which do carry air from the fan coolers, that leads to the top of the containment. I think the flow from those is 10,000 cubic feet per minute each.

As I recall, that is a small fraction of the total flow from all of the fan coolers.

BY MR. ROISMAN:

Q Can you give me some comparison in terms of the flow at 10,000 cubic feet per minute, how does that compare to the household fan? If you stood in front of it, would you feel a breeze on your face?

A (Mr. Kniel.) Yes, you would.

Q High fan or low fan? You have a little room air conditioner in your room, I assume, like I do. If you had it on fan, how would it compare to high or low fan?

A I don't know what the size of the duct is, so I don't know what the velocity is from the duct exit at the top of the containment.

Q Between the two of you, can I assume from that answer that the effect of the velocity of the air from those ducts has not been taken into account in terms of the turbulence in the containment atmosphere?

A As far as I know it has not. What I am saying is qualitatively that is a small effect on what turbulence there is in the containment atmosphere. I might point out one additional thing and that is there had been some discussion about what turbulence originates from the loss of coolant accident.

The turbulence from the loss of coolant accident results from wherever the break is in the coolant pipe. Most of the coolant pipes are under the deck also and the time for the blowdown is a relatively short time in terms of the time of the operation of the sprays.

In other words, the double-ended break blowdown is ten seconds. And the blowdown from other breaks is of the order of minutes. So that once the blowdown has occurred and the steam has escaped into the containment, there isn't any turbulent effect originating from the break. I would say most of the spray performance is during the time that there is no turbulence from that effect.

The turbulence, as Dr. Burley pointed out, was principally due to thermal convection in the containment.

Q What about turbulence from the reflooding stage? When steam is produced by having the fuel rods come in contact with the cooler emergency core cooling water?

A Again the steam is released under the deck and it gradually escaps into the containment space above the deck and the steam flow there is relatively smaller than it is during the loss of coolant accident.

During the loss of coolant accident, you have a blowdown of the whole primary system which is a high steam flow. Subsequent to that, during the recovery phase, the steam flow is quite modest.

Q Are you the person I should be talking to, if I am interested in further questions on the convection turbulence caused by the location of specifically warmer spots in the containment than others, or Dr. Burley? Are you the one?

A I can only talk in a qualitative way about what kinds of convection we have in the containment. I haven't looked at what the effect on the sprays are, but I would be glad to contribute anything I can.

Q What I am interested in, and perhaps I will go ahead with Dr. Burley and you can be thinking about this and let me know when you are prepared to answer. I am interested in finding out exactly what kind of convection currents are created at the point directly above the reactor, as a result of the fact that the reactor, I assume, is the hottest single space inside the containment, at least for awhile, or at least the place where more heat is being generated than at any other spot.

I would be interested to know the extent to which the Staff has taken account of that specific convection current in determining the effectiveness of the spray.

Dr. Burley, to get back to my discussion with you, you indicated that there were several places in which turbulence does occur in the loss of coolant accident situation. What about the turbulence caused by the contact between the cooler drops and the warmer steam?

Is there turbulence when that occurs also?

A (Dr. Burley.) That is a condensation phenomenon which I would not say would lead to any great degree of turbulence, no.

Q If steam and the drops don't physically contact, but come near each other, is there any turbulence caused -- I am not familiar with what happens when a warm body and a cold body come near each other. I take it the steam will tend to go up and the drop will tend to go down.

Would this tend to divert them from each other or would they be attracted to each other, or what?

A You are getting outside of my area of specialization. I am not sure I can give you an answer which has any great validity.

Q Then I don't want you to have to guess about it.

Can you explain to me, then, whether that factor, whatever the factor is, whether it was taken into account when you did your calculational model for the effectiveness of the spray?

A We did not analyze each degree of turbulence which might possible be introduced into the pathway or the spray droplets individually, no.

Q Is that both for purposes of making your determination as to how much coalescence might occur and also in terms of making your calculations as to the extent of contact between the spray on the one hand and the iodine on the other?

A I think you have crossed the line. The answer to that is no. The calculational model which we used distributed large and small droplets over the whole spray volume and this is effectively the end result of gross turbulence and gross mixing.

Q In other words, you assume uniform mixing?

A Close to it.

Q Is it your testimony that the effect of turbulence is leading always to the creation of fairly uniform mixing?

A Not necessarily.

Q How do you know that the turbulence in this containment will have the effect of making the mixing greater? Is it possible or are there ways in which turbulence could tend to even isolate the sprays on the one hand and the iodine on the other, one from the other?

A I don't see that situation as conceivable.

Q Is that another word for credible?

A Incredible, sir.

CHAIRMAN JENSCH: I think this is about time for a

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recess.

If it is convenient to interrupt your examination here.

MR. ROISMAN: That will be fine, Mr. Chairman.

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

(Recess.)

end 4

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CHAIRMAN JENSCH: Please come to order.

Are you ready to proceed, Mr. Roisman?

MR. ROISMAN: Yes, M.r Chairman.

CHAIRMAN JENSCH: Proceed, please.

BY MR. ROISMAN:

Q Dr. Burley, I would like to concentrate now, if I can, on the individual drop, the mechanism by which it absorbs or has a reaction with the iodine in the containment. Can you describe to me the chemical situation -- I don't mean the precise details -- but just what happens when a drop from the spray nozzle comes in contact with iodine in the reactor containment atmosphere?

A As I said yesterday, the purpose of the sodium hydroxide additive is to convert elemental iodine into a more soluble form. The sodium hydroxide leads to a hydrolysis reaction, which includes an autooxydization of iodine, oxidation and reduction both of the iodine, forming iodine ions and iodate ions, both of which are very soluble in the solution.

The overall process of removing iodine involves transport of the gaseous iodine to the drop surface, a transfer across the surface barriet of the drop, and absorption into the liquid.

Q Does something come out from the drop or does something go into the drop?

A The iodine goes into the drop; nothing comes out.

Q The spot where the iodine comes in contact with the surface of the drop after the chemical reaction is completed, what remains on the surface at that point? Is it a new substance, not sodium hydroxide, not iodine, but something else?

A If you had no internal circulation with the drop itself, you would have a local depletion of sodium hydroxide molecules at that particular spot, yes.

Q Would you have some new element that was replacing it at that point? It is not a void, right?

A The hydrolysis products are the iodine, iodate atoms and water.

Q So, in other words, there is no more sodium hydroxide left at that point. And if again there were no new sodium hydroxide coming up to the surface, a new iodine atom that came in contact with that spot, nothing would happen?

A At exactly that spot, you are right.

Q Now, I assume both from reading and from your being anxious to qualify it, that sodium hydroxide does come up from the inside of the drop; is that correct?

A Well, the general laws of nature say that the movement of material is towards the point of lowest concentration. Therefore, if one has a local depletion of sodium hydroxide, the movement of material is towards that particular spot. Also one has general mixing and circulation within the drop which tends to make the sodium hydroxide concentration both within the volume and at the surface quite uniform.

Q You mean, in other words, that the drop has a uniform distribution of sodium hydroxide when it starts out from the nozzle? Not that as it is falling, assuming it is not coming in contact with any iodine, that the inside of the drop is constantly mixing itself up and the mixture is taking place after it leaves?

A One has constant circulation. If one looks at a particular molecule in the drop, it does not say in the same place, it may appear some place differently a fraction of a second later.

Q Okay.

Now, with respect to this mixing that occurs as a result of trying to keep the concentration at any given point in equilibrium, what rate does that mixing occur at compared to the rate at which the reaction between iodine and sodium hydroxide occurs? In short, is the little blank spot we talked about before instaneously replaced with more sodium hyrroxide, or is there a time lag, and might this item of iodine following immediately on the tail of the one that had the reaction, discover when it gets to the identical spot, there is no sodium hydroxide there for it to react with?

A It is conceivable there would be a short time period, in a few instances, where no sodium hydroxide is at a spot where, let's say -- let's say the sodium hydroxide concentration is depleted at the point of impact with iodine.

Q This diffusion rate, the rate at which the sodium hydroxide comes to the surface of the drop to replace the now fully reacted sodium hydroxide, is that rate the same regardless of the size of the drop, and regardless of the history of the drop as it falls to the containment?

A If it were diffusion, it would be primarily temperature dependent, since it is also a mixing and circulation phenomenon this is dependent on external perameters.

Q For instance, as more and more reactions take place, so that more and more of the drop consists of these iodide and iodate ions, will the sodium hydroxide that is coming to the surface to replace used up sodium hydroxide move to the surface more slowly than it would have moved at the very beginning when the drop first left the nozzle?

A No, the mobility of particular molecules should stay constant.

CHAIRMAN JENSCH: Excuse me.

I appreciate your answer. I am sorry to interrupt, but I wonder if precisely the answer -- it would help me if you could deal with this question: I think he asked you, would the sodium hydroxide move more slowly. You said it should do something. Do you have any data for your conclusion? If the data say one thing and your conclusion is the other, that might be a consideration we would like to have.

THE WITNESS: if one is talking about molecular atomic phenomena, one is getting into a statistical process and one cannot make an unambiguous statement. Physicists know that there are such things as quantum jump, which are statistical phenomena, and on a statistical basis one would say that the probability is great but there is also the probability that something will happen to the contrary.

CHAIRMAN JENSCH: Thank you. I appreciate that philosophy.

I think the question is, will the sodium hydroxide move more slowly in the droplet as the reactions continue?

THE WITNESS: No, but again with that qualification.

CHAIRMAN JENSCH: Very well. Thank you.

Will you proceed?

MR. ROISMAN: Yes.

BY MR. ROISMAN:

Q In other words -- this is something I wasn't clear about in the elaboration you gave of the answer: Is that because the precense of iodide and iodate ions is not the type of substance which would slow down the movement of sodium hydroxide? I mean, if the reaction that were taking place were producing another kind of chemical, or another type of molecule, would there be a possibility of effecting the rate at which the sodium hydroxide mixes?

In other words, is the content of the drop relevant?

A As long as the sodium hydroxide stays as a chemical entity, no. The rate of movement would stay contstant.

Q Now, does the size of the drop increase as the iodine is absorbed into the drop?

A Are we talking on a quantum basis again or on a relative basis? On a relative basis, no, effectively, no.

Q What I am talking about is that earlier we talked for a long time about drop size being important.

A It is not of that magnitude, no, sir, the size of at atom is the order of a thousandth, or so, of a micron; and we are dealing with things of the order of a thousand microns, so we are talking about chemical entities of the order of a millionth, the size of the drop diameter.

Q I understand that. But I take it that during the course of the drop's fall, more than one atom of iodine reacts with a drop?

A Right. And you can add an awful lot of atoms before you would increase the diameter by one micron.

Q I understand that.

I think yesterday in response to a question by Dr. Geyer, you indicated the number of grams -- I think 14,000 grams of iodine would be present in the reactor containment following the assumed situation after the loss of coolant accident.

How much of that 14,000 grams does one full spray cycle eliminate in terms of weight from the reactor containment?

A This, of course, is a changing phenomenom. If you want to refer to the first spray cycle, which is probable the one which removes the greatest portion of the iodine, the total addition of all the water from the refueling tanks, which is of the order of 350,000 gallons, would probably remove of the order of close to 95 percent or so of all the iodine airborne in the containment -- the number is probable higher.

Q The amount you mean that is removed?

A Yes.

Q But despite thathigh percentage, it is not going to show up in terms of an individual drop being any bigger as a result of having --

A It is not going to show up in terms of an individual drop size change. Remember, we are also dealing here with a huge quantity of water in which the sodium hydroxide and the iodine are dissolved. We are talking about 350,000 gallons -- or more than a million liters.

Q I was more comfortable with gallons.

Is the size of the drop effected by condensation on the surface of the drop when it comes in contact with the steam that is in the containment atmosphere?

A To a small extent, yes.

Q Is there a limit on how much condensation a specific drop might have during its path through the reactor containment?

A Yes.

Q What is the limit?

A The limit is the point at which the drop reaches the temperature of the steam.

Q Has a computaiton been made or a test been run to find out how that limit, based on the chemical reaction that will occur, how that limit works out in terms of actual drop size increase? In other words, how much bigger does the drop have to get in terms of condensation before its temperature and the temperature of the surrounding system are in equilibrium and no additional condensation will occur?

A The computations have been done; they have been performed by Westinghouse, and have been reported to the staff.

Q And you have put those computations into your spray effectiveness model for this reactor?

A That has been included in the spray effectiveness calculations, yes.

Q Earlier you indicated that the model was prepared two or three years ago. The report I think which you have in your hand, WCAP7499L, is dated April 1970. Has the staff model been updated?

A We requested assistance from the staff of the Oak

Ridge National Laboratory at the time the spray model was developed on this particular point.

Q Then --

A It is outside my specific area of competence.

Q But I am trying to find out, you were the formulator of the model. Did the test data and the calculations that entered into the portion of the model that includes compensation for condensation on the drop, did it come from Oak Ridge tests rather than from those Westinghouse tests you referred to?

A It is based on calculations.

Q From?

A Presently calculations by Oak Ridge at the time the model was developed, and verified and added to by the information which we received from Westinghouse.

Q Now, let me --

A The two are comparable.

Q Identical?

A I don't recall the exact numerical values we got from Oak Ridge. They are nearly identical, as I recall.

Q Was there something wrong with the Oak Ridge tests or some weakness or gap in the calculations conducted by Oak Ridge that warranted having new tests run, or new calculations?

A There have been no tests run. These are calculations.

Q I am sorry -- what was not in the original calculations that had to be, that required that additional tests be run or was Westinghouse just skeptical of the results from Oak Ridge?

A I can't speak for Westinghouse. I presume they wanted to verify numbers which were given to them by another organization. After all, Westinghouse is also concerned with the safety aspects of the spray systems and also is interested in having a conservative numerical value for the drop diameters.

We take information from wherever we can get it, and in this case two calculations are better than one.

Q In short, they weren't the same; I mean, the same formulas for the calculations weren't used, then? It wasn't just two people taking out the slide rules and starting with the same formula?

A I can't exactly speak for what calculational techniques were used. Obviously one can calculate the amount of steam which would have to be condensed on the surface of a drop in order to raise it to the temperature of the steam starting with an initial temperature which is known.

Q From your knowledge of the calculations, do they take account of these factors: One, that the starting temperature of the spray varies as the sprays continue, since you are recirculating the spray water, and all of the various sources which may effect the starting temperature of the spray; Two, the temperature of the steam is effected also during the course of the accident due to the fact that its generation is coming from different places at different times; and three, the distribution of the drops in terms of their size to the same extent that drop distribution is taken into account in the general analysis of the effectiveness of the spray?

A I know the Chairman wants me to answer yes or no. The worst case is the initial case, where the incoming spray water is cold and the reactor atmosphere is at its peak. This is the situation for which the calculations have been arrived. The later situation, where the spray water has already heated up, and the temperature within the containment has been lowered would result in less steam condensation on each drop. Therefore, the most conservative case, and the case for which the steam accretion on drops has been calculated is for the initial case.

Q And that is then assumed throughout the time the spray operates?

A Yes, sir.

Q Is the steam temperature, is that assumed to be the temperature of the -- of what I guess is in effect the average temperature of the entire containment following the loss of coolant accident, which I think has been testified to yesterday as something in the neighborhood of 270 degrees Fahrenheit, or is it the temperature of the steam measured as it comes out of the pipe at the point of the double ended pipe break?

A Subject to correction, the number is 270 degrees.

Q Yes. So that is not as conservative as it would have been taking the temperature of the steam as it comes out of the pipe at the point of the double ended pipe break?

A That is the temperature essentially as it issues into the containment atmosphere.

Q So that it is more conservative than if you took a thermometer and stuck it in the middle of the containment to see what it was after the steam mixed with the air inside the containment?

A I am not sure I understood the question?

Q Well, there are a variety of ways of figuring out what the post-losst of coolant accident temperature is. One way is to put a thermometer at some appropriate spot inside the containment area and look at it after the accident to find out at various times what the timperature is. Another way would be just to take the temperature of the steam which is the heat source or at least one of the heat sources, in places which the temperature inside the reactor is being effected, and measure the temperature of the steam as it comes out of the break. Another way would be to take the thermometer and put it on the reactor vessel itself and measure the temperature of the reactor vessel.

Now what I am asking is just to make sure, is it your understanding that the temperature was taken at the point where the steam comes from the break?

A No, the temperature is the average air temperature in the containment and obviously this is the type of atmosphere that the spray drops throughout the entire fall.

Q But that average temperature is based upon -- it comes about at what point following the loss of coolant accident? Does it allow for the steam to mix with the cooler air in the containment? In other words, how is it computed?

A That I can't answer. I am afraid you would have to ask some one else.

Q You don't know who?

A I don't know who might want to answer that.

A (Mr. Kniel) The 270 degree Fahrenheit temperature is the temperature after the steam has mixed with the containment atmosphere.

Q At what time following the loss of coolant accident would the temperature be 270 degrees? In other words, how long are you assuming the mixing is going to take?

A The mixing takes place very rapidly, in a matter of seconds or minutes at the most.

Q Thank you.

Dr. Burley, to go back to my earlier question, at least for some period time it is possible that some drops would be subjected or come in contact with air, which is hotter than the 270 degree temperature that is assumed as the starting point for the temperature unless the sprays don't begin until after the temperature mixing has occurred. Is that correct?

A (Dr. Burley) With your last proviso, yes. I am not sure what the spray delay is. Of course, the containment pressure has to reach a pre-set limit before actuation of the spray system. And this may be after 10 or 15 or 20 seconds of mixing required to essentially attain the peak containment temperature.

Q Well, Mr. Kniel, is there a specific figure available to know when this mixing would have occurred? I mean, has a figure been determined or have analyses been made or tests run?

A (Mr. Kniel) We haven't made an analysis of when the 270 degree temperature would be reached, no.

Q Would it be just in general terms, before, after or at the same time that the pressure within the containment would trigger the operation of the spray?

A I think the spray has more than one sensing -- it is initiated by more than one measurement. I think it is initiated by the SIS system, which is low pressure, low level in the pressurizer, and there is an additional redundant initiating, which I think is set at 5 psi, 3 psi, something like that.

Q As you understand it, if the primary system for actuating the spray operates as it is designed, would it cause the sprays to go into operation before the temperature in the containment had reached the 270 degree figure that is used as the basis for concluding that that is what the loss of coolant accident maximum temperature is in the containment?

A I couldn't testify as to exactly what the sequence there is. The sequence is a little bit different for a loss of offsite power, where the sprays would come on 10 or 15 seconds after, if you had on site power, it would come on more rapidly. So I couldn't testify exactly as to what the temperature sequence in the containment is.

Q Do you know, has the staff considered, has someone on the staff taken account of the effectiveness of the containment spray system determined at what point the sprays will come on and related that to the precise temperature in the containement or various part of it at that point?

A We have considered it to the effect that we don't consider that there is any particularly quantitative effect that would result from the containment temperature variation that you are speculating about during the loss of coolant accident, during the very initial phase of the loss of coolant accident.

Q In other words, you consider it by disregarding it?

A Well, in our judgment it is not a signfiicant factor.

Q Do you disregard it?

MR. KARMAN: I believe Mr. Kniel has answered that question, Mr. Chairman.

CHAIRMAN JENSCH: Objection overruled. Proceed.

WITNESS KNIEL: Well, I don't know what you mean by "disregard."

BY MR. ROISMAN:

Q Are there any figures in determining the spray effectiveness which take account of the fact that for a period of time at the beginning, immediately after the loss of coolant accident, the temperature inside the containment is not an evenly mixed 270 degrees, but the sprays may already be on?

A (Mr. Kniel) We don't make any calculations to that effect. I might add that you haven't specified what that temperature is, the temperature could be higher or lower than 270 degrees.

Q Yes. I know I haven't.

CHAIRMAN JENSCH: Since you raised that, I wonder what are the highs and lows on that when you fix your average?

WITNESS KNIEL: We don't calculate a transient temperature for the containment during a blowdown.

CHAIRMAN JENSCH: But my question is, how did you arrive at the average? What is the high and low?

WITNESS KNIEL: There is no high and low. We assume there is a blowdown in which there are very effective convectional forces, in which the steam escaping mixes into the containment atmosphere and we all assume there is an equilibrium there, which there is, and when that equilibrium is achieved, the temperature becomes 270 degrees Fahrenheit.

CHAIRMAN JENSCH: I understand that. In other words, you disgard the high and the low and wait for the equilibrium to be established; is that correct?

WITNESS KNIEL: There is no high and low. There is a transient condition. The steam as it comes out of the break is somewhat hotter than the eventual equilibrium temperature in the containment atmosphere.

CHAIRMAN JENSCH: What is that temperature in numbers when it is somewhat hotter that you referred to?

WITNESS KNIEL: I don't know exactly what that is. It depends on the kinds of break, what the flow from the break is, that kind of thing.

CHAIRMAN JENSCH: Thank you.

Will you proceed with cross-examination.

MR. ROISMAN: Yes, Mr. Chairman.

BY MR. ROISMAN:

Q Dr. Burley, in making an assumption about the amount of condensation on the drops, are all drops assumed to have the same amount of condensation based upon this model we talked about before?

A (Dr. Burley) No. The amount of condensation is of course proportional to the contact area and it affects the smaller drops to a greater extent than the large drops.

Q In other words, there is more moisture condensed on smaller drops than there is on larger drops?

A Proportional to their initial volume, yes.

Q All right.

A In other words, the percentage increase of a small drop by steam condensation is greater than the percentage increase for a larger drop.

Q Are all drops assumed to have -- are all drops of the same size in the spray assumed to have condensation to the same quantity on them in making your calculations as to the effectiveness of the model?

A Yes.

Q Now, can you tell me what effect does condensation have on the ability of the drop to absorb iodine? Does it increase it or decrease it?

Let's start simply with that and then go on.

A No effect.

Q It has no effect whatsoever.

Does it in any way effect the ability of the sodium hydroxide to move to the surface where reaction can occur?

A The phenomena occur -- the answer is no. The phenomena occur simultaneously, mixing within the drop and slow condensation on the drop surface. You don't condense all of the steam which is going to condense on the drop in a fraction of a second. It occurs over the total available time until the thermal equilibrium is reached, at the same time the sodium hydroxide has a chance to mix with the condensed molecules of water, steam.

Q But as the condensation is occurring, at the point at which the water has come to the surface of the drop, iodine, which is attempting to come to the same point, if you will, directly behind the water, is there sodium hydroxide immediately there available for it, and if not, does it take a longer time for the sodium hydroxide to get up through that atom of condensed water than it would for it to get up to the surface of a drop that has merely had a reaction with iodine occur on the surfact at a point?

A It is conceivable of course that one has the juxtaposition of a molecule of sodium hydroxide hidden behind a molecule of water separating it from the elemental iodine on the surface.

Q Does your model assume that never happens?

A We do not specifically put that in our model. We have an overall factor of conservatism which considers that point.

Q How much credit do you give for thatpoint in your conservative model? How many drops out of the total number of reactions that are going to occur in the reactor containment, times in which iodine will come in contact with the drop and be available to have a reaction, what percentage of those are assumed to not occur because of the presence of a molecule of water from condensation blocking the occurrence of the reaction?

A I have not assigned a numerical value to that.

Q I am going to ask you for a great deal of help on something. I have read the term "liquid film resistance." Can you tell me what it means in the context of the effectiveness of the spray?

I am not even sure I read it correctly.

A Right.

This is a hypothetical term which refers to a barrier preventing the even transport of the iodine into the main body of the drop and also refers to the finit time limit to transport the iodine from the surface into the interior of the drop.

Q Is that a factor which needs to be taken into account in evaluating the effectiveness of the drop's ability to absorb iodine?

A Very definitely, yes.

Q In what manner is it taken into account in the staff's evaluation of the effectiveness of this spray system?

A In the evaluation of the partition coefficient between air and the sodium hydroxide solution, which is in effect in the main volume of the drop.

Q Has it been done by tests or is this a calculational --

A Both.

Q Can you describe for me what sort of tests were run?

A The tests consist of either inducing iodine into the gas volume or into the liquid volume and measuring the distribution of iodine between the gas phase and the liquid phase at various time intervals. And the instantaneous partition factor which pertains to the instantaneous uptake of iodine into the drops then is the extrapolation of these data points to time zero.

Q Time zero, did you say?

A Time zero, zero time, instantaneous.

Q I wish I understood that, Dr. Burley. That sounds very interesting.

Did the data, the tests, were those conducted, all of the tests which the staff is familiar with, conducted at the time when this model was originally formulated, two or three years ago?

A There have been additional tests since that time.

Q Have those tests confirmed by being identical to the earlier tests, or did they vary from it, and if so, in what manner and how did the variance get taken into account?

A There is an easy consistency between all of the tests.

Q By whom have these tests been conducted?

A The recent tests have been conducted by the Battelle Northwest Laboratory staff.

Q And the earlier ones?

A They are scattered all over the literature. There are several hundred references.

Q We have been talking about the drops and its physical characteristics. Let me direct your attention for a second to the other half of the chemical reaction, the iodine itself.

What forms is that iodine in? In other words, is it a gas, it is also a liquid drop; is it free floating or attached to something else?

A The initial form of the iodine is the gas phase. It can take the form of several different chemical species in that gas phase.

Q I understand. But it is a gas essentially?

A It is a gas. And the least soluble form of iodine is the elemental form of iodine. Therefore this is what is used for all of the calculations. If one assumed that one had iodine for instance, in the form of hydrogen iodide, this has a much greater solubility in water and one wouldn't even need the sodium hydroxide to have a comparable uptake velocity.

Q Now the nature of the reaction that occurs between the iodine and the drop, is that a molecule of iodine that comes in contact with the drop, or an atom of iodine? What do you call the piece of the iodine that comes in contact with the drop?

A This is conceivably either, but one assumes a molecular reaction.

Q Okay.

How are these molecules, in what form are they in the gas? Are they tied together in bunches of molecules, or are they individual molecules floating around?

A At the concentration we have, individual molecules.

Q When the gas molecules escape from the reactor vessel, and enter the containment, are there any possibilities that the molecules may, isolated or not isolated, in any instance, be in the form of gas bubbles, where several molecules will be together, some on the inside and some on the outside of a larger one?

A Yes, you can also have groups of molecules aggregating together.

Q If, just assuming for a moment, that one of these larger aggregates of molecules came in contact with a drop of this sodium hydroxide mixture, would all of the molecules react with the drop, or would there be sort of a contact between the drop and this aggregate of molecules in which one molecule from the aggregate would react with the sodium hydroxide.

A I would expect all of the iodine molecules to react with the sodium hydroxide, simply because a group of molecules occupies a larger volume and has access to a greater number of sodium hydroxide molecules.

Q In terms of the contact, are the molecules, when there is a group of molecules, do they form -- it is like a ball, or is it like a flat, like a piece of paper, two-dimensional?

A It depends on the mode of formation. The rapid formation gives you a non-crystaline type aggregate. If you have a longer time period for molecules to get together, they would come together in a more goemetrical arrangement.

Q Which kind would we have in the reactor?

A It is hard to tell.

Q You are not sure?

A There is no certainty as to what happens in the formation of larger aggregates, no.

Q Would the crystaline type, if it came in contact with the -- I assume by "crystaline"you mean what I non-scientifically call rounder?

A No. Crystaline means a regular array of attoms in three dimensions.

Q Well, in terms of coming in contact with the drop, if this is the molecule over here, and this is the drop over here, are there some parts of that molecule crystal which aren't in contact with the drop -- or should I say, could there be?

A Let's get back to the scale of things. Clusters of molecules are still of the same order of size as individual molecules, and one molecule in contact with the surface, essentially assures that all the molecules of a small cluster are in contact with the surface.

Q There is no such thing as having one molecule behind the other molecule?

A Yes, and in turn it would be absorbed into the liquid.

Q But --

A But the whole aggregate would be in contact with the surface for all practical purposes, and probably be enveloped by the surface.

Q Is there a size of gas bubble, to go back to the layman's term here, is there a size of gas bubble -- for a moment let's not assume it is in the containment, but could there be a size of gas buble, iodine gas bubble, where there would be an inner part of the bubble, much like there is an inner part of the drop, which would have to diffuse out to the surface before it could have a chemical reaction with something that the bubble was coming in contact with?

A If you had aggregates of molecules which approached the size of the droplets of the spray, yes. This would not occur under the circumstances of this type of situation.

Q Is that because of the way in which the iodine is released from the reactor vessel?

A It is primarily a function of the concentration of the iodine in the atmosphere of the containment.

Q You mean --

A We are talking about very, very minute concentrations.

Q In other words, if the concentration were much higher, this type of bubble might or would be more likely to be formed.

A Yes, if one had a million or so times higher concentrations, one could expect higher or larger aggregates of iodine. They would not in any case approach the condensation limit which we envisage in that question.

Q What about the space inside the reactor vessel that the spray would have greater difficulty reaching under some of these cranes? Are there possibilities for greater concentrations of a magnitude up to the point where we can start talking about bubbles of iodine gas, for those to form?

A No.

Q Is that something that has been tested by releasing iodine gas into a comparable structure and seeing whether in fact it got trapped under cranes and similar things? Or is that based upon a calculation or what?

A Knowing the concentration of the iodine released into the containment, and knowing the general behavior of iodine, that is essentially a conclusion which any experimentalist would reach.

Q It is your judgment, in other words?

A It is a judgment, yes.

Q At the time of the Indian Point #3 construction permit hearing, some doubt was expressed by the Board, members of the Board, regarding the reliability of the containment spray, and particularly concerned with tests that had been run or had not been run with regard to certain items.

Were you involved in the evaluation of the Indian Point #3 containment spray system?

A The evaluation of the effectiveness of the Indian Point #3 system, not the development of criteria for the testing of the spray system.

Q Yes. Are you saying you were?

A I was not involved in the criteria for testing of the containment spray system, no.

Q But you were in the evaluation of the effectivness, right?

A That is right.

Q In your opinion has the staff, does the staff now have available to it, has this data been applied to this particular case, data sufficient to answer the concerns of doubts that existed in the minds of the Board as you understood them in the Indian Point #3 construction permit?

A I never understood any specific doubts in the minds of the Board at the Indian Point #3 hearing other than a reservation as to whether the plate out mechanism would operate in conjunction with a containment spray system.

Q In Exhibit T of Intervenor's Exhibits on page 16 -- Exhibit T is the opinion of the Board in the Indian Point #3 case -- page 61 is a portion of the separate opinion of Board member Pigford. Footnote 11 indicates -- first let me read you the sentence to which the footnote appears.

CHAIRMAN JENSCH: Excuse me a moment. Do you have a copy or does counsel have a copy so the witness may review it as this is being read?

MR. TROSTEN: Yes.

END#5

(Handing document to witness.)

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CHAIRMAN JENSCH: Will you proceed, please?

BY MR. ROISMAN:

Q Dr. Burley, directing your attention to the second sentence on page 61, "The Staff has identified certain technical uncertainties in the Applicant's simplified design model," and the footnote indicatory is there, "and has made its own evaluation of the iodine removal rate for the spray system on the basis of its conservative assumptions regarding liquid film mass transfer resistance and drop size distribution and coalescence."

Have the uncertainties that the Staff identified at that time, are those uncertainties resolved or are some of them resolved, all of them resolved, in your opinion by the Staff at this point?

I think the uncertainties are spelled out in that Footnote 11.

A I think the fairest way of putting that is the uncertainties have been greatly reduced.

Q Has it changed the Staff's credit for the spray effectiveness for this plant, for Indian Point No. 2 --

A It has not changed the evaluation of the spray effectiveness for this plant over that of Indian Point 3 at the construction permit stage.

Q That is the Staff's --

A The Staff's analysis, yes, sir.

Q So at this point, as far as the Staff position is concerned, the doubts that existed at the time of the Indian Point No. 3 construction permit that warranted the Staff in making, I think, conservative assumptions regarding liquid film mass transfer and drop size distributions and coalescense, while those doubts have been alleviated to some extent, the Staff is not prepared to depart from its same credit to be given on the amount of effectiveness of the spray system that existed in the construction permit stage?

A I would answer that differently than the yes or no context. The question is primarily that of Staff time. We have, I think, greater certainty on the improved operation of the spray system now than we did two years ago.

The complete development of a Staff model necessarily involves both time on the developmental stage as well as many, many review procedures.

These have not been accomplished at this point and in view of the lack of Staff time for the complete evaluation, we are still using the same model we used at the Indian Point 3 stage. This does not necessarily mean that we do not have a greater confidence in spray removal rates at this point. We do.

We think most of the uncertainties which plagued us two and three years ago have been alleviated or removed. We understand much more about the effectiveness of spray removal. We have much more experimental data available than we did two or three years ago. And the development of a different model, one which incorporates those changes in our thinking is only a matter of time away.

It was not ready at the time of the evaluation of the iodine spray removal system for this plant.

Q On that same page, Dr. Burley, at the end of Footnote 11, Dr. Pigford, after listing the uncertainties which the Staff had taken into account, then makes the following statement, "Other uncertainties characteristic of spray absorption systems are the extent of missing of the unabsorbed gas within the containment space and the possibility of nongaseous forms of the absorbing component, (iodine).

"These are not taken into account in the analyses by either the Applicant or by the Regulatory Staff."

Have any changes been made in terms of evaluating the effectiveness of the spray to take account of those uncertainties between the situation at the time of the Indian Point 3 construction permit hearings and now?

A I personally find the possibility of nongaseous forms of iodine impossible. So I have not further developed that point.

Q What about the mixing of the unabsorbed gas?

A That is inherent in the model which has been used in terms of transport into the spray pattern and removal from the spray volume essentially uniformly.

So we have not further developed that point either in the context of spray effectiveness.

Q In other words, the model upon which the effectiveness of the spray for this plant has been evaluated assumes relatively uniform mixing of the gas within the containment atmosphere?

A That is correct. And that is the experimental experience.

Q Can you describe for me the experiments that have been run to verify that condition?

A There have been approximately 40-some odd experiments in the small scale facility, the nuclear safety power plant facility at Oak Ridge National Laboratory, most of which used a single nozzle of the type which is going to be used in this containment, the spray solution which is going to be used in this containment, the atmospheric conditions which one would expect at the peak post-accident period, and iodine concentrations comparable to those one would expect.

These are almost uniformly indicated much more rapid iodine removal than one would even calculate with theoretical model. Then there is a second series of experiments at the containment systems experiment facility at Battelle Northwest Laboratory which has a total volume of about 30,000 cubic feet or about one hundredth or less that of the total Indian Point containment. These have included a number of nozzles, steam injection during the removal process, and sampling at a number of different locations within the containment, so that the effect of mixing within the containment could be ascertained.

And the Staff of the Battelle Northwest Laboratory has made detailed analyses of the iodine concentrations which appear both in the sprayed and in the unsprayed portions of that facility.

And the maximum difference at any one time observed is of the order of about 20 percent difference in iodine concentration. And this is a very small factor.

Q Those tests, are these the ones we discussed earlier -- I won't go through them again with you -- but earlier we talked about whether or not turbulence factors and the presence of fans and the flow of liquid through the bottom of the containment and the presence of the same baffles and so forth existed.

Are these the same tests we were talking about earlier when we discussed that and I asked you questions about those various items?

A I am afraid I lost the train of that question.

Q Earlier this morning I was questioning you regarding some experiments that had been run. As I remember it, you described them as one would be at the NSPP facility and the other at the CSE facility. And I was asking you about turbulence and the extent to which turbulence inside of the reactor containment following its loss of coolant accident was taken into account in the experiments which attempted to simulate the conditions of a loss of coolant accident.

And we talked about the effect of the fans and the flow they had, both intake and outflow, and the presence of various structures at the bottom of the containment which would affect the movement of the air around, and the drains in the floor which were at certain points and the water was moving that way and we talked a bit about heat convection and so forth.

Rather than go through all of those questions again, I am trying to find out, were those the same tests you are talking about now?

A At that point we were talking about the effect on drop coalescence. As I recall, we did not specifically go into details on measurements of drop coalescence in the CSE tests. The other reference to the CSE facility holds, yes.

CHAIRMAN JENSCH: Excuse me, may I interrupt?

I was going to inquire if this would be possible -- I know it will interrupt your being able to apply yourself for some time to the work of your regular assignment, but I wondered if that interruption is likely to be of such duration that after which you might have an opportunity to review your testimony as reflected in the transcript and to the extent that you can, in a sense, annotate your testimony. Now, I have a note here that in many places you have said you had calculations for many phases of this containment spray consideration.

Then I think you used a phrase something like this, As these data came in, they tended to confirm our calculations.

I wonder wherever you find a sentence like that, if you could give us a reference to what those data are, so we would have an opportunity to have the benefit of the reports, which I assume they are, reflecting these data.

Could that be done with convenience when you are done with this interruption you are contemplating?

THE WITNESS: We will endeavor to do so.

MR. KARMAN: Mr. Chairman, we will do that.

CHAIRMAN JENSCH: I think this is the kind of thing that I think Intervenors' counsel is having trouble trying to recall to the witness when he said what. I think more precisely if he had an opportunity to review the testimony --

MR. KARMAN: We will go over the transcript and wherever Dr. Burley has indicated there was a reference to some report or evaluation, if it is available, we will certainly furnish it to the Board.

CHAIRMAN JENSCH: This would be very helpful.

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Thank you.

Will you proceed?

BY MR. ROISMAN:

Q Dr. Burley, in terms of uncertainties associated with the effectiveness of the spray system, to what extent in your opinion does the fact that tests have been run on, experiments, actual experiments, have been run using reactor containments or simulated reactor containments that are not as high as the one that is used for Indian Point No. 2?

To what extent would that create an uncertainty in terms of the data produced by those tests?

A None in terms of height. I might as well explain it at this point rather than develop it through a series of questions.

Q All right.

A As I stated previously, the effectiveness of the spray system is derived from theoretical calculations. The preciseness and the accuracy and the applicability of these theoretical calculations is determined by comparison with a number of experiments.

And once one has established a correlation of experimental parameters in this equation with the observed removal effectiveness, then that parameter is essentially considered as proven and subject to no uncertainty.

Q To what extent does the history, that is the events

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which occur in the life of the drop as it comes from the top of the containment to the bottom affect the ability of the drop to absorb iodine, either the rate or the quantity or both?

Are there events which occur as it comes down through the containment, which will have an impact on that?

A The only one that I can think of would be drop saturation, which we discussed earlier.

Q What about not condensation? That is, you don't believe that that will affect, will have an impact on the effectiveness of the drop in absorbing iodine?

A No.

Q How about if the drop is pushed through turbulence or something else, away from where the iodine is? For instance, if it is up against the wall of the containment?

A It is an improbable question, with an improbable answer. If one had a situation like that where one physically separated drops from the iodine, you don't absorb iodine.

Q Right. But what I am asking is, I didn't mean it merely to be that -- I think we already discussed that question some time ago. We talked about uniform mixing and you indicated tests showed there was something on the order of, perhaps, a 20 percent difference if the worst situation in a test between the concentration of iodine --

A The concentration of iodine in various parts of the containment, sprayed and unsprayed, yes.

Q What I am asking is if most of the drops were where the lowest concentration of iodine was, would that have an impact on the effectiveness of, or your evaluation of the effectiveness of the spray?

A Yes, in the absence of mixing, definitely. One has to also have movement of the iodine towards the point of lowest iodine concentration.

Q Reference has been made in some of the written documents that have been supplied by, I believe, the Applicant here to two different spray nozzles which I believe are identified as Spraygo 1713, and Spraygo 1713-A is the nozzle that is installed or will be installed on this plant.

But a number of tests that have been run using the nozzles which I think have been described as the same as the ones on this plant have been run with nozzles where the designation "A" was not included.

Do you know what difference that designation makes in terms of what the nozzle is like?

A Only by means of conversation with Spraygo representatives. As I understand it, the subscript "A" refers to a more precise manufacturing technique and keeping the tolerances down to lower limits. It is the same nozzle otherwise.

Q In other words, it is not a design change or a change in materials or specifications or anything like that, it is just a better way of manufacturing the same nozzle.

A That is my understanding.

Q Dr. Burley, you spoke yesterday in response to some questions from the Board with regard to the question of plate out.

Can you tell me if you used the calculational model that you have indicated has been used by the Staff in analyzing the effectiveness of this spray system and all other aspects of it remain constant, except that no credit was taken for plate out, would you then come up with analyses which would demonstrate that the plant under certain loss of coolant accident conditions would have doses in excess of 10 CFR Part 100 standards?

A The Staff has used the calculational techniques of TID-14844. If you go into a hypothetical situation where one would assume absolutely no plate out, and if at the same time one used the conservative model for iodine removal which the Staff has used for this particular reactor, then one would calculate thyroid doses in excess of those specified in the 10 CFR Part 100, yes.

Q Now, is there an interaction -- let's start with TID-14844.

Is there an interaction in TID-14844 between the effectiveness of sprays and the existence of plate out? In other words, if you assume plate out, then you should assume a lower effectiveness of sprays, if you don't assume plate out, then you should assume a higher effectiveness of sprays?

A If one goes to a realistic model, the answer is yes, the two are interdependent. The assumptions in TID-14844 are based on a nonmechanistic analysis, just assuming that simply because iodine is such a very highly reactive substance, that very rapid plate out on all the many internal surfaces of the reactor would occur very quickly.

We have attempted to analyze the situation on a more realistic basis and made some very, very conservative assumptions, namely, that we have neglected the transport of iodine to all the internal surfaces by the transport of steam to those surfaces, and only assumed an internal convection model, and at the same time used the Staff model for the depletion of iodine in the containment atmosphere by the chemical additive spray systems.

The two processes would realistically be expected to go on simultaneously and compete with each other for the removal of iodine.

Q You mean, in other words, the plate out on the one hand and --

A The plate out would compete with spray removal, and both would tend to remove some of the iodine over the entire period when iodine is available for removal. If one uses this type of approach, and if one also assumes that the only surfaces in the reactor on which plate out could occur are the stainless steel surfaces which really have the lowest affinity for iodine, then one comes up to the conclusion that the present model which the Staff has used based on TID-14844, which specifies 50 percent plate out of iodine, gives a slightly less conservative result than the more realistic model would be expected to show you that the iodine removal is actually better than that calculated by the Staff.

Q In this context, can you tell me what is the -- is plate out just another name for a chemical reaction between iodine and some other substance?

A Plate out is a complex phenomenon and it is not really well defined. It is the removal of a substance on a surface and as Chairman Jensch and I discussed yesterday, one can also apply it to the removal across a film of liquid on a surface. It does not necessarily have to be just the sticking of the iodine on a dry surface.

Q In other words, this is iodine that remains iodine, it is just that it does not float around any more. Is that correct?

A It remains iodine unless it comes in contact with the sodium hydroxide spray solution, in which case it is converted to the iodide form, taken out of commission, so to speak, and washed into the sump.

Q I understand. In other words, this is not a chemical change in iodine merely because of the sticking to the surfaces, it would have to be something else that would be doing it?

A It could conceivably in certain cases lead to a chemical reaction if the particular surface under consideration is chemically reactive with iodine.

This would be a consecutive process again. Elemental iodine would stick and then the chemical reaction would occur at its own rate.

Q In the Staff plate out assumptions, the assumption is it is not the reaction assumption, it is the sticking assumption.

A It is not specified.

Q If the surfaces to which the plate out would normally occur have a film of water on them and the water contains sodium hydroxide when the iodine comes in contact -- first of all, will it come in contact with the surface or will it come in contact with the film of water?

A If the surface has a film of water on it, it would come in contact with the film of water.

Q And what would happen is that there would be an absorption, just as though the film of water were like a drop. Is that right?

A Yes. The reaction of iodine with the sodium hydroxide solution occurs regardless of the physical shape of the water medium.

Q In other words, whether it is flat or round or what-have-you?

A Right.

Q When you do the analyses that have been done on the effectiveness of the spray systems for the removal of iodine, does the quantity of spray that is available for contact with the iodine take account of that portion of the spray which is forming a film on the surfaces inside the containment?

A Yes. This is a relatively small fraction of the total volume which is sprayed into the containment at any one time. A few percent.

Q In other words, a couple of percent of the spray itself is film that is on the walls and the various baffles and things we talked about before?

A That is right.

Q In the course of the loss of coolant accident, once the sprays have begun working, would you say that all of the surfaces have a film like that or all of the -- the top side surfaces, not so much bottom surfaces now?

A I think it is highly probable that in the water reactor accident that all or most of the surfaces inside of the reactor containment would be coated with a liquid, yes.

Q Then am I correct in assuming that in the situation that will occur in a loss of coolant accident, in point of fact there won't be any of this so-called plate out, what will really happen is that iodine that comes in contact with the wall surface or the baffles and so forth will have a reaction with the film that is on those walls that has sodium hydroxide in it?

A Presumably the majority would, yes.

Q Now, you told me a moment ago that in figuring the effectiveness of the spray for the containment, you assumed the availability in effect of all of the spray including this very small percentage which has already adhered to the walls, that comes the wall film? Am I correct in restating that?

A Yes.

Q Why is that not double counting of the effectiveness of the spray then in terms of its ability to absorb iodine being taken into account on the assumption that it is a drop floating through the containment, and at the same time its ability to absorb iodine being taken into account to the extent that it forms a film on the walls of the containment?

A It is not a double counting, because the two phenomena are distinct. One is a driving of the iodine toward the surfaces and if you want to make it stick there, that is one thing, if the water film is there it is put in solution.

But essentially the iodine is scavenged at the surface. The effectiveness of the spray in the free volume is dependent only on the action of the spray droplets.

So the two are distinct.

The fact that sodium hydroxide is in two places at the same time is essentially immaterial.

Q No, I am afraid I am not following you. The iodine removal will occur by coming in contact with sodium hydroxide. You have just testified that as a practical matter the only way that iodine is going to be, or the only way the Staff evaluation considers iodine removal inside the reactor has got to be by reaction with sodium hydroxide, because all of the surfaces formerly available for plate out are filmed over with sodium hydroxide mixture and all of the air is filled with sodium hydroxide falling down.

Now, to have this sodium hydroxide and the iodine combine, they have to come in contact. In one case the contact -- I won't try to use qualitative words on this -- in one case it occurs because a moving drop comes in contact with moving iodine, in another case it happens because moving iodine comes into contact with moving liquid film to the extent that it is continuing, there is a continuous amounts of film being added to the wall.

Now, the drop is not moving around freely in the air and the drop is not really a drop now, but is joined with some brother and sister drops and formed a film. I don't accept the practical difference between those except that a drop smashed against the wall has half as much surface area available to have contact, because half of its surface is next to the wall and there is no iodine there, it only has its outer surface available, if we think of it as somewhat two dimensional.

I don't understand that.

Q Let me try to explain it then. As I said, only a very small fraction of the total volume of the liquid is in the form of a wall film and covering the internal surfaces of the containment. Actually the situation is even worse than you say, it is not just half of the surface of the drop, when many drops come together the surface of all these drops is considerably smaller than the surface of the individual drops. That is immaterial.

The fact is that the surface is covered, that iodine deposition and removal occurs at the wall through the wall film and the iodine is removed at one place. In the other case we have iodine removal taking place simultaneously in the gas volume of the containment where the spray is operating and the two processes are quite distinct. Have I made the point? I am afraid I may not.

Q Well, you didn't make my point, I don't know whether you made the point or not. If I understand correctly, the effectiveness of the sodium hydroxide mixture to absorb iodine is computed using the calculational model we talked about before, it includes all of the sodium hydroxide mixture and takes account of things like film resistance, size of drops, which relates to surface, condensation and these other kinds of things, to whatever extent those have been taken account of.

Now, some of those drops that are included in the total effectiveness of the spray actually aren't out there moving around inside the spray, they are up against the wall.

You said it is a small fraction, a couple of percent. What I don't understand is how is it that the effectiveness of those drops, when they are stuck up against the wall, goes up to, 50 percent of all of the iodine is assumed to be removed instantaneously by coming in contact with 2 percent of the spray stuck to the walls, and yet nothing like that in terms of the effectiveness of the spray is assumed for those drops that are continuously falling through the air, going back through the sprays and being sprayed out again.

That is what I don't understand?

A I am afraid you are confusing two models, sir. One is the present TID-14844 assumption of 50 percent instantaneous plate out. This I said is a nonmechanism assumption. One can also look at the situation in terms of a realistic model, which has a finite plate out half-life for iodine at surfaces.

And this is the comparison I was trying to draw, not that there is instantaneous removal of 50 percent of the iodine at surfaces, but that if one had no sprays available, that a finite plate out or removal of iodine at the surfaces would occur with a specific half-life, namely, something of the order of 10 or 15 minutes, to remove half the iodine in the containment.

But since this would continue over more than just the 15 minutes, during the first 15 minutes you remove one-half, during the next 15 minutes you remove half of what is remaining, or you reduce the initial concentration down to one-fourth, during the next 15 minutes you would again reduce what is remaining by another half, or a factor of eight, and so forth.

And this is a slower process than the spray removal by the much larger surface area which is available to iodine molecules in the form of these smaller spray drops than the area which is available on the walls of the containment.

Q You say that it is slower than what is actually calculated? In other words, if I understand correctly, you tell me that it is appropriate to assume that 50 percent of the iodine is removed instantly, by plate out, because, in fact, averaging it, you will find that it is an average of 50 percent.

A Not precisely. May I make the previous statement a little bit more clear perhaps?

Q All right.

A What one has done and what the Staff has done is to compare the averaged iodine concentration available for leakage, using two different calculational models, one assumes the traditional TID approach of 50 percent instantaneous plate out, and then removal of the rest of the iodine by the sprays alone, with no additional plate out during the entire period.

The other one is what I would call a realistic model, that plate out does occur with a finite time and this is the model which we have compared then, using both the simultaneous plate out and spray removal, comparing this to the initial model, and concluding that the iodine which would be available for leakage with the TID plate out assumption would actually be higher than that which would be derived from the more realistic model.

Q Let me see if I can express the problem I am having in a somewhat different way.

You told me now that TID-14844 compared to the realistic model is more conservative. So --

A For this particular plant.

Q Yes, I understand. The more conservative model would assume 50 percent plate out instantaneously, that is what TID-14844 does.

You tell me that that rate, the rate of removal that occurs as a result of plate out is actually slower by some margin than the rate of removal of iodine by the use of sprays.

Is that correct?

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A That is a realistic approximation of the situation, yes.

Q Now, if there is 50 percent of the iodine removed instantaneously by the plate out, the slower process, would it be conservative to assume that 50 percent of the iodine is removed instantly by the faster spray process?

A If you want to use a nonmechanistic assumption, why not?

Q Now, if those two 50 percent removals occur instantly, why does the Staff come up with an assumption there is any iodine after the first incident, since we just removed both halves of the iodine, half of it by the spray, half by the plate out.

A If you want to use that assumption, I am perfectly willing, no iodine were available from time zero.

Q But my problem is the Staff's computation of the effectiveness of the spray shows that iodine is available for leakage and we even have statistics that show the thyroid dose at the site boundary at the end of 720 hours and it is not de minimus, I mean it depends I guess on the definition of the term, it does not exceed Part 100 standards, but it is in the order of several rems or a couple of hundred rems in a couple of cases.

THat is the problem that I am having. I have just made some assumptions that seem to suggest that what you have said is that there wouldn't be any iodine available at time zero and you have told me that plate out moves more slowly than sprays, but your model on sprays still allows for the leakage of a fair amount of iodine.

I am trying to find out why doesn't the slower plate out process allow for its 50 percent to be leaked out for awhile before it has been completely removed.

A I am afraid there are so many misconceptions at this point I don't know where to start.

Q Start at the beginning, please.

A The TID assumption of plate out does specify 50 percnet iodine removal instantaneously. This is an assumption which has been used traditionally, and certainly was applicable of reactors using no spray system. We have attempted to see if this is a realistic and conservative model in the presence of sprays, and, therefore, we have looked at both the spray and the plate out mechanisms operating simultaneously on the iodine in the containment.

And one can derive numerical values for the iodine removal by both processes operating simultaneously. The depletion of iodine in the containment atmosphere then is an exponential which includes the removal constants for both processes as an additive function.

And one cannot say that either process is not operative and the other one then takes all of the iodine out of the

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containment. So what we are trying to say is that we have a realistic evaluation of iodine removal and one can't say that plate out itself is the predominant mechanism, one can't really say that sprays are the predominant mechanism, without going through some comparative calculations.

The reason why the 50 percent plate out assumption has been used is because this is part of the tradition of the Regulatory process, it is written in TID-14844. There was no similar precedents for giving so-called instantaneous credit for iodine removal by sprays.

Therefore, we went through a calculational process for that. And had to then reconcile it with the calculational process for plate out. This we have done and I have tried to give you the results of such a model.

CHAIRMAN JENSCH: Would this be a convenient time to interrupt your examination?

MR. ROISMAN: I don't know, Mr. Chairman.

CHAIRMAN JENSCH: Let's try it.

The Reporter has been at it an hour and a half.

Mr. Briggs has a question or two before we recess for probably 15 minutes.

MR. BRIGGS: I think it might be worth interrupting to ask a question or two because I share Mr. Roisman's problem here and maybe if we think about it during the recess we can straighten it out.

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I would like to ask two or three questions.

Dr. Burley, can you tell me the half-life that you used for transfer from the bulk of the containment to the walls? A characteristic number for that? I mean if it is not necessarily available --

THE WITNESS: The number which we have used in the analysis is a plate out half-life, which includes the transfer to the surfaces of ten minutes.

MR. BRIGGS: The plate out half-life is ten minutes?

THE WITNESS: Yes, sir.

MR. BRIGGS: What is the half-life for transfer to the drops?

THE WITNESS: The removal half-life again is of the order of about five to six minutes, using the Staff model, it is of the order of one minute or thereabouts using the model proposed by the Applicant.

MR. BRIGGS: So one says that -- no, I won't make any comment here, I will think about it for awhile.

Does methyliodide enter into this at all, or is this entire calculation based on the iodine only?

THE WITNESS: This is only based on the iodine, elemental iodine fraction.

MR. BRIGGS: It seems that a part of the problem is this, that as I understand it you say that if the droplets area as droplets more effective than they are if they are on

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the wall?

THE WITNESS: That is correct, there is a larger surface area available.

MR. BRIGGS: Yes, they have more surface. Yet it appears from the remarks that you have been making that when you put the droplets on the wall they take out more than this 50 percent and I think that is some of the problem one might think about during the recess.

THE WITNESS: Cumulatively, sir.

MR. BRIGGS: Yes, I understand that.

CHAIRMAN JENSCH: All right.

Did you have something further?

MR. TROSTEN: Mr. Chairman, I asked Mr. McAdoo to stand by this morning in case the questioning of Dr. Burley resulted in an inquiry the Board might want to direct to him. I understand he does have an urgent matter to attent to back in his office.

I wonder if it would be acceptable if Mr. McAdoo were excused for today?

CHAIRMAN JENSCH: Yes. Another item in that regard, because I don't know how much more Mr. Roisman has, but when we conclude with his examination of the Staff witnesses, I understand the public hearing aspects will be ready for recess.

So we are making plans in that regard and contemplating

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the public hearing for next week.

The Board will indicate that it is not likely that the Board will have a necessity for interrogation next week of Witness Grill, so he may proceed to his assignment for next week.

MR. KARMAN: Thank you, Mr. Chairman.

CHAIRMAN JENSCH: We will consider other matters for next week's session later on, and Mr. McAdoo may be excused for this afternoon.

(Witness McAdoo excused.)

MR. TROSTEN: The Board did ask one question about spray nozzles earlier this morning. We do have something --

CHAIRMAN JENSCH: We will take it from Mr. Grob or Mr. Wiesemann, either one.

At this time we will recess to reconvene in this room at one o'clock.

(Fifteen-minute recess.)

CHAIRMAN JENSCH: Please come to order.

end 6

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Dr. Burley has resumed the stand. Mr. Briggs has a further discussion with Dr. Burley.

MR. BRIGGS: Dr. Burley, during the recess have you thought any more about the statements you made? Are there any changes you wanted to make or any amplification of them?

THE WITNESS: In answer to your questions.

MR. BRIGGS: Yes.

THE WITNESS: I would like to answer or maybe clarify the last question you brought up on whether, with a ten-minute half-life for the plate out, one does not remove more than 50 percent by that process.

And I think the way I would like to approach it is that the iodine removal is a competitive process, and the faster process is always going to remove the greater quantity of iodine.

So in the operation of plate out and spray simultaneously, if the spray has a shorter half-life for iodine removal, it would tend to remove the greater fraction of the iodine. In the absence of the spray, one would come up with the plate out eventually removing all the iodine in the containment.

MR. BRIGGS: Yes, I understand that. I think some of my confusion and maybe some of the other confusion is related to the following. With just the sprays operating, and with nothing on the wall, you assume that, or you calculate that you have a removal half-life, if you wish, of, say, five minutes, and then as I understood your testimony, you indicate that now if you take 2 percent of this spray, or some small amount of the spray, and you put it one the wall, you haven't changed that removal half-life by very much, I mean the removal half-life for the sprays, because you only took 2 percent.

But now the material as it is on the wall gives you a removal half-life at the wall of ten minutes which certainly implies that you have a much greater surface on the wall than the 2 percent of surface that you removed from the droplets. That seems to imply that you have, let's say, half as much surface on the wall as you have in droplets?

THE WITNESS: We have not in our calculations used any of the containment surface as being coated with a liquid. The ten-minute half-life calculation assumed a steam-wetted surface and not a solution-wetted surface.

It is not necessarily true that the total surface area of the containment, in other words, is wetted by the spray solution. And in addition to that, the spray film may be as thin as a monomolecular layer.

MR. BRIGGS: I don't think we should go into quite that amount of detail. I think I understand what you are doing and I think my problem is solved, I am not sure about Mr. Roisman's.

MR. ROISMAN: No, it is not, Dr. Briggs.

BY MR. ROISMAN:

Q Dr. Burley, let me see if I can approach it from a different light. You have indicated the Staff assumed relative uniform mixing of the iodine within the containment atmosphere.

Is that correct?

A That is correct.

Q That the surface area of the drops available for contact with the iodine gets an equal chance, all surfaces have an equal crack, if you will, at the iodine. Is that the practical effect of uniform mixing?

A That is correct.

Q Two percent of those surfaces -- I think I am taking not as conservative as one might -- but, let's say, 2 percent of those surfaces, those drop surfaces, are over on the walls, 98 percent of the drop surfaces are floating through the air. Is that correct?

A That is correct.

Q Roughly correct?

A Yes.

Q Does more than 2 percent of the iodine in the reactor containment have a chance to come in contact with the 2 percent of the spray that is stuck to the walls?

A Yes. Also remember the wall film is not necessarily a stagnant film, it is being replenished.

Q Much the same as the spray drops themselves are being --

A Correct.

Q You tell me that the spray, that when you took account of the effectiveness of the spray, you assumed that 100 percent, you thought this little 2 percent figure was not worth being concerned with, and so your evaluation of the effectiveness of the spray assumed 100 percent of the spray in the containment atmosphere?

A It is included in our overall factor of conservatism.

Q Okay. Now --

A May I give you an idea of the numerical magnitudes and the differences?

Q You mean between what is on all --

A No, the difference between the calculated spray effectiveness one would calculate on the basis of the theoretical model and that of the Staff model assumes a factor of conservatism of about 380 percent. Two percent is a very small fraction.

Q I understand. Okay. Now, if there were no plate out at all, the situation that we would have is that the only thing that would be available for removing the iodine that we are talking about would be the surface area of the drops of the spray to remove the spray. That is correct?

A If there were not plate out, yes.

Q All of the surfaces of the reactor vessel, you testified earlier, are probably going to be covered with this film, that is made up of sodium hydroxide?

A I would think so.

Q And actually you tell me there will be probably a little bit less sodium hydroxide surface available than if those drops stayed away from the walls, because when they are at the walls, the film thickness may be thicker than the drops would have been, the backside of the drop is not available.

A Equivalent surface areas of the drops is decreased, or is expected to be decreased by impinging on the walls, yes.

Q My problem is I don't understand why, if the sodium hydroxide drops are on the wall instead of floating in the containment, why it is that we don't come up with the same iodine removal factor, assuming uniform mixing?

Irrespective of the existence of the sodium hydroxide on the wall or sodium hydroxide someplace else, we don't have any plate out as such, we don't have dry surfaces, what we have is sodium hydroxide on the walls, sodium hydroxide floating through the air.

It is all sodium hydroxide reaction removing the iodine. Why is the amount of iodine removed any higher than whatever the effective rate is for the spray itself?

A The actual amount of iodine expected to be removed by the plate out surfaces, per unit time, is less for the plate out process than by the sprays.

Q I am talking now about total iodine removed from the containment. If 100 percent of the spray is drifting through the air, it is going to remove iodine -- I think the figure you use is a removal rate constant of 4.9 per hour, that is what was used at least in Indian Point 3.

A That is correct.

Q You testified earlier if a removal rate constant of 4.9 per hour were applied to this plant and no credit were given for plate out, then the plant's performance in the loss of coolant accident in some instances would result in doses in excess of 10 CFR Part 100 limits.

Is that correct?

A For the hypothetical case where no plateout is assumed and the Staff's very conservative model for iodine removal by sprays is applied. That would be the case.

Q What I don't understand is how do you get anywhere above the 4.9 per hour spray removal rate constant merely because some of the spray which removes iodine is on the walls rather than floating through the containment?

A As I said, the plateout mechanism on surfaces is an impingement process. The iodine may not be immediately removed from the place of impingement and this may then be washed away subsequently or immediately by the spray solution. Or by the spray solution washing down the walls.

The net effect -- any iodine which impinges on the wall is effectively removed from the quantity of iodine which would be available for leakage from the containment building and this is the quantity that is of interest.

Q But that is the same -- that is no different than iodine that gets absorbed by a drop floating through the air in terms of its availability for leakage.

Is that right?

A That is correct.

Q Does more iodine go over the walls than floats through the air?

A If one uses a conservative plateout model such as I have described, the amount of iodine which would be removed by the spray with the Staff asumption would be greater than that removed by the impingment upon walls and internal surfaces.

Q Then why isn't the total effectiveness of the removal of iodine from the reactor containment simply the spray removal rate constant of 4.9 per hour, or a lower figure?

A Because plateout is operative simultaneously with the spray and in addition to that in this containment filters are operating on the iodine concentration. I think we should keep all removal processes in mind.

Q But my problem is that plateout as described in TID-14844 was the process of iodine coming in contact with dry surfaces. We now know from your testimony here that there aren't any dry surfaces, there are liquid surfaces.

Those liquid surfaces are covered with a sodium hydroxide liquid. Its ability to absorb iodine and the rate of that absorption is taken into account in determining the spray removal rate constant of 4.9 per hour, which you tell me is actually higher than the removal rate that would be applicable to the removal by the same drops when they are hanging on the wall.

But it is the same spray, it is 2 percent of that spray, some figure close to that. Now, it is on the wall, now it removes it at a slower rate, and yet you tell me the total effectiveness, the total amount of iodine removed is increased by taking a portion of the spray, putting it on the walls and making it less effective. I am having difficulty understanding how that happens.

A I think I am beginning to see your source of confusion. The removal mechanism for iodine is a surface phenomena. And the removal is proportional to the amount which actually impinges on surfaces. The area which the spray exposes to the iodine is greater than the area of the walls and the internal surfaces.

Therefore, the sprays having the larger surface area remove the greater proportion of the iodine. Does that clarify it?

Q No, that confuses it.

MR. BRIGGS: May I try to help?

MR. ROISMAN: Please, Doctor.

MR. BRIGGS: Mr. Burley, don't you mean -- I will try, I don't know whether I will be successful -- this, that if you didn't have the containment surface there, just had the spray and it occupied a certain volume, the spray droplets have a certain area and they take up iodine at a certain rate.

Now, when you consider plateout, in liquid, you put the containment in place, and you add that surface area to your calculation. Is that right?

Just let me say one thing more, I am sorry. This 2 percent of the drops have hit on the surface, you don't know whether it is 2 percent of the drops of 5 percent, but when the drops are falling through the containment, they fall like this (indicating). When they get on the surface, they go down like this (indicating). So they wet the surface, but their effective area now, the time of contact or the effective area is much greater.

THE WITNESS: Thank you, sir. I fundamentally was trying to get that idea across, with the addition that instead of using the more reactive surfaces which would obtain if all of the surfaces in the containment were wetted by sodium hydroxide, if only wetted, steam-wetted stainless steel surfaces were in the containment, this gives you a slower removal.

MR. BRIGGS: But the effect is, when you put the plateout surface into your calculation, you have greatly increased the amount of surface that you have for absorbing the iodine.

THE WITNESS: Correct, sir.

MR. BRIGGS: How you tie it into the 2 percent of droplets is the problem.

MR. ROISMAN: Thank you, Mr. Briggs.

I think I understand at least now better where I want to go in terms of this question.

BY MR. ROISMAN:

Q Dr. Burley, let me see if I can state it and see if I understand what I am saying. Plateout adds to the total amount of iodine that is removed because in addition to all of the surface areas that are added to the inside of the containment by the presence of drops, there are a whole number of surface areas that are already there?

A Correct.

Q Now, those surfaces areas, if we didn't have any spray at all, they would according to these assumptions from TID-14844, they would take out 50 percent of the iodine, or we could assume that 50 percent of it was absorbed instantly or stuck to it, whatever it is.

A That is a nonmechanistic assumption, but that is the one made in TID-14844.

Q Now, I think the place that I am having the difficulty is when the drops that have surface area, that are falling through the air, when those drops leave the air and go over to the wall, 2 percent of them or so, if the situation was static, that is if the drops weren't moving, would there be now the same amount of surface or a little less available for iodine to be absorbed on?

A May I perhaps clarify that? Ignore the 2 percent hitting the walls, just assume that 2 percent less spray gets into the containment.

Q All right.

A And that is the situation I was trying to explain. Iodine has two sinks. It can go either to surfaces or it can go to the surfaces of drops. And both of these depletion mechanisms are operating simultaneously.

Q The total surface area of the drops, is it substantially greater than the total surface area of the other surfaces in the containment?

A It is greater -- I am not quite ready to give you numbers. This is, of course, dependent on the drop size spectrum assumed.

Q I understand that.

A The two numbers are somewhat comparable. The surface area of the drops are somewhat larger.

Q Doctor, in figuring out the realistic plateout now, how did you compute the amount of iodine that would be removed through iodine coming in contact with the surfaces in the containment?

A The fraction of iodine which is removed by the plateout is calculated by letting both mechanisms operate simultaneously.

Q Both of which?

A Both the spray removal and the plateout. And then one can determine which removes what fraction of the iodine over a particular time period. One can calculate, in other words, the removal which would obtain if one let the sprays operate alone. And one can calculate the removal which would obtain if one let plateout and spray both remove iodine simultaneously.

And the difference then is the effective plateout.

Q My difficulty stems from this factor -- maybe if I explain the difficulty you may be able to give me some explanation that will clarify it. The spray has a certain amount of surface area which in a sense is constantly changing because new drops are constantly coming into the --

A May I stop you there?

Q Yes. I don't mean it is increasing.

A No, the spray area at any one time in the containment is relatively constant.

Q Right. But the presence of new drops coming in --

A You have the same number being deposited at the bottom.

Q All I meant was the new drops coming in, as they first come out of the nozzle, they are a little better able to do their job of absorbing iodine, they don't have the condensation problem, they don't have the problem of diffusion or the other things we talked about.

A Well, I thought I had already said that those add very small increments to the inability of the drops to --

Q I didn't want to get into the record a statement that indicated there weren't any changes.

A Your initial assumption was on areas and I wanted to keep the areas constant.

Q Now, the amount of wall space is also constant, I assume?

A Right.

Q Now, just roughly, do you know what is the relative amount of wall space to the total amount of space on the drops? Is it half, a quarter, a tenth, something like that?

A I can't give you an exact number, no. I would offhand say that the area on the spray drops is roughly two to three times higher than the available area on surfaces. That order of magnitude.

Q Assuming that the rate of removal of iodine from contact with the surfaces, the containment surfaces, is the same as the rate of removal of iodine from contact with the droplet surfaces -- and I understand you made assumptions that would be more conservative than that, but assuming they are the same -- if the difference in area of the drops, the constant, and the difference in area of the containment surfaces is in the magnitude of two or three that you suggested, drops having two to three times more surface than the containment, then would the effectiveness of actual iodine removal -- I don't mean anything other than the total amount of iodine removed by the surfaces -- would be one-half to one-third of the total amount of iodine removed by the sprays?

A Making that assumption, not necessarily a valid assumption, but an assumption.

Q Which assumption, you mean the two to three?

A The removal after all surfaces at the same rate as drop surfaces.

Q Okay. Actually the assumption should be the removal of wall surfaces is slower.

Is that correct?

A Somewhat slower.

Q Okay, I understand that. Would that be another way of coming up with a figure for the effectiveness of the iodine removal systems for the reactor containment than the one which you have indicated to me and which I confess I don't fully understand has been done?

In other words, could you take this 4.9 figure spray removal constant of 4.9 per hour, for the sprays, and divide it by the surface area difference with regard to the walls and come up with -- let's just assume the figure is a half,

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okay. And take and add the 4.9 figure, 2.45, and give them the total spray removal constant for the surfaces and the spray droplets?

A Effectively, yes.

Q How difficult would it be for the Staff to provide that figure, or looked at a different way -- well, strike that, how difficult would it be.

What is the total spray removal constant rate for the realistic model in which you take account of for the TID-14844 plateout, but the plateout that you have done calculations on that we have been discussing, and included in the total, of course, the spray removal constant of 4.9 per hour.

A I have not calculated that, no.

Q Can you give me an idea?

A I was only interested in the overall removal, and not fractional parts.

Q What was the overall removal rate?

A The rate constant in the exponential expression is the sum of the rate constants for the individual processes.

Q The figure?

A The Lambda for the removal by drops is given as 4.9.

Q I am looking at the construction permit for Indian Point 3 and it was 4.9 there.

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A Right. And the Lambda for spray removal, or removal constant for plateout is proportional to the deposition velocity on those surfaces. And this is available from the literature. I don't have those numbers here.

Q Is it less than 4.9 or greater than 4.9?

A It is less than 4.9, using the most conservative assumptions.

Q So that the total spray removal constant for the -- I am sorry. The total iodine removal rate, the Lambda figure, in computing under TID-14844 formula, but replacing the 50 percent plateout assumption with this new thing, would be a figure where the Lambda would be under 10?

A Yes.

Q Have you run those figures through and can you indicate to me what the 10 CFR Part 100 limits would be -- I am sorry -- what the dosage rates would be in terms of the kinds of figures that 10 CFR Part 100 looks for and what assumptions have you made with regard to effectiveness of filters in making those assumptions.

I am sorry, making those calculations?

A Right. I will need a few moments to get my notes together on that.

Q Okay.

A Okay.

Q Is that the FSAR you have?

ln4

A No, I am speaking from some calculations I had made independent of the FSAR. Using the Staff figures now, using the spray removal constant of 4.9 and the TID assumption, one obtains a two-hour iodine reduction factor of 5.2.

This is the averaged value for the iodine in the containment over that which would obtain if no iodine reduction occurred.

For what one might call the realistic model, I would prefer to call it finite plateout model, I have used an iodine half-life of 10 minutes, and it appears that the surface areas in the containment and the surface area on the drops are of comparable magnitude rather than two or three times higher.

The removal constant is 4.3 hours, and I only allowed this to operate for 30 minutes, because one assumes that eventually one is going to saturate surfaces with an iodine film unless there is a continuing supply of sodium hydroxide spray impinging on the walls and carrying this away.

The overall two-hour iodine reduction factor obtained from this model is 5.5.

Therefore, the difference is about 5 percent or 6 percent lower than what the Staff has given in the Safety Evaluation.

end 7

Q In terms of the doses that would actually be obtained at the site boundary as a result of the use of the more realistic model, do I assume correctly that in this more realistic model you assumed that there is 100 percent of the noble gases, 50 percent of the halogens, and one percent of the particulate, and that the spray reduction of 4.9 plus 4.3, or 9.2 operates on that?

A Operates only on the iodine fraction, sir.

Q All right.

And that iodine fraction is 50 percent of the reservoir?

A That is correct.

Q In other words, the reduction of the further 25 percent is taken care of in a different way by including in this 4.3 figure?

A That is correct.

Q Have you done the dose calculations for what would occur under those circumstances?

A Well, one would just have to compare the reduction factors of 5.2 and 5.5 with the thyroid doses given in the Staff Safety Evaluation. Other members of the staff have done that calculation; I am sure they can give you the comparison right now.

Q Well, what I was going to ask is, if they are here, I would appreciate it if they let me know what effectiveness they assumed for the filters in making the calculation of doses unde those assumptions?

A These assumptions were done for the Indian Point #3 containment and did not involve the filter effectiveness.

Q No filter effectiveness?

A The filter effecdivensss would further tend to increase the iodine removal capability of the overall containment .

Q Doctor, can you tell me -- Mr. Kruger is helping me, since I don't know the difference between gamma and beta, he does the calculations. He is telling me what figures we need to know to do the rest of them. Can you tell me what percentage of the iodine is assumed to be organic in these?

A The Staff Safety Evaluation has assumed that 10 percent of the iodine available for leakage, which is the 25 percent fraction, is in the form of organic iodine, and other difficult to remove species.

Q Doctor, I want to direct your attention to one of the answers that was given by the Applicant to questions asked by the Board and see if we can take this and analyze that we have just been discussing, and apply it to that answer.

If Mr. Karman would give you a copy of the document dated July 6, 1971, by Applicant, entitled "Answers of Applicant to Questions Raised by Atomic Safety and Licensing Board on May 13, 1971," and may I direct your attention to

Question 6

A I have the document in front of me.

Q Let me direct your attention to the page of this question or the answer to this question, page number two, the bottom of the page. Would you -- you needn't read it out loud, but just read the paragraph at the bottom of the page that begins "As noted further..." -- and read over to the bottom of page 3, where they begin to do the computations of what the amount of doses would be.

A "As noted further, the Indian Point #3 Board findings (page 70, et seq), the Staff model did not account for the possibility that the dominance of the containment spray as a heat absorbing medium would prevent condensation on surfaces from acting as a plateout sink of the required effectiveness. This reasoning does not consider the fact that the containment fan coolers act to condense steam concurrently with the sprays, and their iodine removal effect (analogous to that of the heat absorbing surfaces) is considerable.

"The Safety Analysis (FSAR, Section 14.3.4), shows that the condensation occurs via the fan coolers and the heat absorbing surfaces at an average rate of about 0.072 pounds per hour per cubic foot of containment volume over the first two hours. By comparison, removal of elemental iodine was demonstrated to occur in the CSE Test A-11 (without spray) at a rate corresponding to a half-life of 16 minutes when the surface condensation rate was .014 pounds per hour per cubic foot (BNWL-1457, page 5.22, 5.34). Applying the CSE observed plateout rate to the Indian Point Containment volume and correcting for condensation rate, one would calculate a plateout removal coefficient of 13 reciprocal hours. This rate applies to a process independent of spray absorption and spray condensation. Thus, a combined removal coefficient, using the Staff's Indian Point Unit #3 spray removal coefficient of 13 plus 4.5 or 17.5 reciprocal hours, could be justified. This dose reduction factor integrated over two hours would be 35 for inorganic iodine."

Shall I stop there?

Q That is fine.

Can you tell me, except for the fact that the figure they use for the effectiveness of the plateout removal is 13 instead of 2.3, are the other assumptions that they use in computing the two hour dose consistent with the assumptions that you have indicated the staff has used in making its computation of the two-hour doses taking "realistic account" of the plateout?

A Similar, not identical.

I think the staff assumptions may have been somewhat more conservative.

Q You mean in terms of --

A Transport to the surface.

Q Those rate figures that are there?

A Yes.

Q In other words, there isn't any difference here insofar as you know in terms of the assumptions about how much radioactivity is available to be released, or anything of that nature?

A correct.

Q Looking, if you would, but not reading it, if you would just look at the figures at the bottom of page 3 and the top of page 4 of that answer, you will notice that the organic iodine does with filter -- there is a figure that is put in there -- are you able to tell from this whether or not the filter effectiveness that is used is the same as the staff uses?

A I am not sure I understand the calculations as they stand here. I would like to confer with members of the staff who have done dose calculations.

Q Okay. I consider the question relatively straightforward and I would be satisfied if the Board would be satisfied with a written answer, if your chance for consultation doesn't come before the end of the day today. I wouldn't mind if you would just give me a written answer to the question.

A Fine.

Q If it is the same, then the answer is yes; if it isn't, perhaps you could tell me if it is possible to tell from this what the difference is.

A I believe, as I read it, that the effectiveness for filters in this calculation is based on somewhere around 70 percent effectiveness per pass or pass, ours, as we said yesterday is based on 10 percent. I don't have the exact numbers.

Q All right, Doctor. Turning a moment at least, away from this, and perhaps Mr. Kniel will have to assist on this question; we talked earlier about the assumptions which the staff uses in making its evaluation of the effectiveness of the sprays and one of them was the concept of uniform formation of the iodine released. I had asked about the effect or the extent to which effect is taken of convection from the source of heat in the reactor vessel.

I think Mr. Kniel was going to check on that, and I wonder if he can tell me now to what extent that source of convection is specifically taken into account.

A (Mr. Kniel) Well, there is no particular -- the reactor vessel itself is not a particularly important source of heat or driving mechanism as far as the containment is concerned. You had a question regarding whether there was a hot spot on top of the vessel, something like that.

Q Yes. I understand, or at least I think I understand that the major source of heat following the double ended pipe break is the fuel rods that are in the reactor vessel. That is it is their residual heat that has yet to be cooled that is causing a lot of the heat that is inside the containment. Maybe I am mistaken on that. Maybe the bulk of the heat is coming from the heat that was already there from the heated system and so forth -- the heated water that becomes steam when the pressure is released.

In either case, perhaps you can tell me what portion of the heat that would cause convection is centered at the reactor vessel itself?

A Well, I think you are correct when you say that the additional heat that is being added to the containment vessel originates essentially in the fuel, and the pressure vessel. The mechanism for transport of this heat into the containment atmosphere is essentially --- well, during the transient recovery from the loss of coolant accident, there is some additional steam being formed. Subsequent to the cooling of the rods, the cooling to the point where it is not forming steam any more, after you have recovered water over the core, you remove the heat.

Now just through heating up of the water, you are not forming any more steam. And the water spills out the break and runs into the sump and gets pumped through the heat exchangers, and gets pumped back in again. So that there is no additional steam being added to the containment vessel.

Now, that is the principal mechanism for transport of the heat from the fuel rods.

Q In other words, the top of the reactor vessel itself is not hot to the touch as a result of the internal heating? I don't mean hot to the touch -- if some hot water spilled on it from outside?

A The inside of the reactor vessel is still hot, as you put it. It reflects the temperature of the water inside the vessel. But the vessel itself is covered with insulation, so that there is no direct access to the vessel. Of course, on top of the vessel, you have the control rod drives and on top of that you have the control rod drive shield, and at that point you have reached the deck, where the drops fall.

Q What I am interested in finding out is if this place where the vessel sits, creates sort of a hot air shaft that runs up through the middle of the containment, a portion where there would be a tendency that drops wouldn't be as likely to fall into thatarea?

A The area around the vessel is cooled during operation and subsequent to the accident by the air directed there from the containment fan cooling system. That area is always cooled during operation by air that is pushed into there by the containment fan coolers, and that continues to operate subsequent to any accident.

So that air is all convected and mixed below the deck before it arises above the deck.

Q Do I understand then that your testimony is that it is not any warmer than the air into which it is going above the deck, once it wends its way out?

A I think the air that escapes from below the deck into the area above the deck is somewhat warmer than the area above the deck. But it leaks out in all sorts of places. And it is not localized.

Q I think, to go back to the original question, where this arose, and I don't know whether it will be you or Dr. Burley that would answer:

The effect of that convection on the distribution of iodine throughout the containment and the distribution of thespace, is it taken into account or not?

A (Dr. Burley) We are assuming mixing in the containment. Whether it is due to heat convection or to the turbulence introduced by the spray itself is quite immaterial.

But the interior of the containment is not stagnant, not a stagnant atmosphere.

Q You are assuming it is uniform from the very beginning, is that correct? From the beginning of the time the sprays begin to operate?

A Essentially, yes.

Q In terms of the mixing of iodine and the effectiveness of the spray, what does the staff do about that portion of the containment atmosphere which is down among, between and under the various structures that are inside the containment? Beneath the floor, under overhanging walls, under the crane, and so forth?

A The portion below the deck is assumed to mix into the volume, main volume of the containment. The advantage of having the spray run down the side walls and collect in the sumps is essentially to prevent any portion of the iodine from escaping the containment in that area. The volume above the crane is mixed into the main volume both by the action of the sprays and also by the action of the filters.

Q What account is taken of the fact that the spray doesn't fall on all of the areas? Once we get down to where structures exists, that there are areas that the structures prevent the spray from falling into, and the spray comes almost exclusively from above in the sense it doesn't mix up and down and all around; or at least in earlier assumptions you said indicated they don't. What account do you take of the fact that there are some spaces that the spray doesn't get to?

Is that taken into account specifically in the evaluation of the effectiveness?

A That is not taken into account specifically.

Remember that without any exception that I can think of, these are interior spaces, not close to the wall, and not near any leakage path.

Q In other words, the fact that the iodine is there is not of any concern, because -- or your assumption is it is not of concern because it doesn't get over to the wall where it would have to be leaked out?

A Correct. So if there is a temporary, very small buildup in one of these non-sprayed areas, it is of no concern.

Q But in terms of figuring out how much iodine has been removed at the end of a specific period of time from the containment atmosphere, you have to take into account the fact that some iodine didn't have a chance, didn't have, if you will, a random chance or fair chance to get in contact with the spray?

A Yes, you do, because the removal rate is proportional to the residual airborne iodine convections.

So, if the residual iodine concentration is higher locally, the removal rate is also higher at that point.

Q Let me see if I can give an example, and see what the difference would be.

If, instead of having all 50 percent of the iodine released from the containment, from the reactor, instantly, as I gather it is assume to do under TID 14884, and under staff analysis, 45 percent of it came out instantly, and then five minutes later, after the sprays and everything had been working, the other five percent came out. Would the calculation of doses for the two hour and for the 720 hour be different; would you come up with different figures?

A Yes, you would. The most conservative assumption is an instantaneous release assumption.

Q But that is also instantaneous availability for contact with the spray , too, isn't it?

A That is correct.

Q But some of this is not --

A The only thing I can do is repeat my answer and I can show mathematically, that any conceivable case -- other than the instantaneous release model, will give you a more conservative result.

Q In terms of doses?

A In terms of iodine available for leakage.

MR. ROISMAN: I think that about covers the spray questions, questions of spray by itself.

We are now at the point where the applicant wants to make an objection. I am going to ask you to talk to me about the comparative effectiveness of sodium thiosulfate and sodium hydroxide spray, and to give the applicants a question to tussle with.

As I understand in the original Preliminary Safety Analysis Report for this plant, it was designed to use a spray that had sodium thisulfate in it, and that a change was subsequently made to have a spray with only sodium hydroxide.

I would like to know what factors went into the staff evaluation of approving that change.

MR. TROSTEN: Mr. Chairman, I think it would be appropriate at this time for me to make the objection, rather than waiting for the next question. I assume from Mr. Roisman's remarks and our discussions at previous sessions that the purpose for Mr. Roisman asking this question is the purpose indicated in his supplemental statement of proposed factual findings, proposed finding number ten, and also the purpose indicated in the previous session of the hearing on page 934. That purpose being in order to demonstrate that the sodium thiosulfate is a better system for controlling the release fo radioactivity in the event of a major loss of coolant accident inside the containment.

This is essentially the same purpose indicated in the proposed finding of fact. As I indicated at the previous session, Mr. Chairman, the applicant objects to this question.

I also indicated, incidentally, Mr. Chairman, that I intended to submit a memorandum to the Board on this point at this current session of the hearing. But it appeared to me after analysis of the transcript and the colloquy between myself and the Chairman that perhaps it was premature at this time to submit such a memorandum, and I am asking to address this orally at the present time.

The Applicant objects to the introduction into evidence of the answer because of the expressed purpose of the offer. The Applicant's position is that the regulatory framework which has been established for licensing nuclear power reactors and the notice of hearing in this proceeding are such that it is not the function of this Board to determine whether the applicant has provided a better system in this plant, namely, sodium hydroxide, than a possible alternative, namely, sodium thyosulfate, or in the alternative, that the applicant has provided the best spray additive system that could be provided under the circumstances.

It is the applicant's position that under the Atomic Energy Act and the Commission's regulations, the question before the Board is whether the sodium hydroxide spray additive, using this particular example, is adequate and meets the standards and requirements established by the Atomic Energy Commission. Now, I might add for the information of the Board, as Mr. Roisman mentioned the other day, this is a matter which has been raised in another proceeding, actually in two other proceedings, the Midlands and Points Beach proceeding, and it is a matter which I am advised is going to be certified in a form similar to the question I have just posed, to the Atomic Safety and Licensing Appeal Board.

I am also advised by, again for the information of the Board and the parties, I am advised by counsel for

Westinghouse Electric Corporation that this matter is considered to be of sufficient seriousness to that company that Westinghouse intends to petition to be heard with respect to this matter by the Atomic Safety and Licensing Appeal Board and to participat in some fashion in proceedings before that body.

Returning to the substance of the question, Mr. Chairman, it would appear that the Citizens' Committee is arguing that somehow there is something inconsistent in the regulatory staff's approval of the sodium hydroxide system for this plant, and the sodium thyosulfate system for another plant. And of course, there is nothing unusual or peculair about the fact that different vendors will properly propose different designs for different plants. This is an entirely accepted and understandable and perhaps the only possible engineering approach since there are many ways of achieving design objectives which appear suitable to different firms, to different engineers.

As a result of that fact, there may be different types of components and systems which are proposed for the AEC in different types of facilities. And the concept that the Atomic Energy Commission reviews and approves differing types of systems is in no way inconsistent with the concept that the Commission is in different cases acting in a manner which is entirely consistent with the public health and safety.

A further point of considerable importance here is that to follow the approach suggested by the Citizens' Committee would be to impose a tremendous burden upon the Atomic Energy Commission, whereby the Commission would be required somehow to determine not simply whether a system is adequate, which in itself is a very complex question, but whether two different -- but of which two different systems or which of a multitude of systems is the best. And all of this incidentally without any significant bearing -- applicant would submit -- on the public health and safety.

In any event, it is the Commission's duty to determine whether the system proposed is adequate. The Citizens' Committee, counsel for the Citizens' Committee referred in the previous session to the fact that the staff makes comparisons. And this is entirely true. The staff does and the applicant does make comparisons between the system proposed in this facility and the systems proposed in other facilities. But these are similar systems, and the purpose of this is to show the normal evolution of design, and to show that there is experience with a particular system upon which the applicant rests in part for his safety analysis.

Now, I think at this point, Mr. Chairman, I have stated the principal elements of the applicant's objection to the receipt -- to this question, and to the receipt in evidence of the answer. I suppose I should wait for further comments from the Board and Mr. Roisman.

CHAIRMAN JENSCH: I might just say that as long as we are comparing cases, this subject has come up in many cases prior to this one. I don't quite understand the sudden alarm about this. I know in two Florida cases we got into consideration of sodium hydroxide and sodium thyosulfate, and so we have two cases going with this, and I think we have to figure out what we are going to do here. Are we going to stand up and be counted?

I think we have to figure out, are we trying to see whether we use the gimlet eye on this precise little item, or whether we are trying to achieve comparative considerations that might be important in the whole thing. We have run through a lot of calculations here this morning on what different approaches might be for calculations about dose releases and that sort of thing.

I think necessarily when we recognize that the Staff Safety Evaluation, the FSAR, have considered a lot of components, and said don't you worry about this, because this was handled in some other PWR case, that suddenly when we come to the solution inside of the pipes, we just talk about one. I don't quite understand that.

MR. TROSTEN: The Chairman goes to the heart of this problem. Applicant does not object to the receipt of of evidence pertaining to the sodium thiosulfate system per se. In response to questions from the Board, one of the applicant's witnesses testified with respect to the reasons that led Westinghouse to determine that sodium hydroxide system should be used in this plant -- and there was no objection in principle, I would say, to the receipt into evidence of matters pertaining to the sodium thiosulfate system, or to the comparison of a system with another system. It is the expressed purpose of the offer into evidence of this that is extremely troublesome to the applicant.

CHAIRMAN JENSCH: Aren't you having trouble with the purpsoe of the offer rather than what interpretations can be placed upon the evidence. He may contend one thing, you may contend another. The important thing is what does the evidence reflect or the correct inference therefrom.

Now, in this case, if I understand it, the construction permit talks about sodium thiosulfate.

MR. TROSTEN: That is correct.

CHAIRMAN JENSCH: So we have a question here of whether the plant has been constructed for the kind of facility and operation that was contemplated in the construction permit stage. I understand the answer, insofar as this particular aspect is concerned is no. So therefore I assume the burden of proof is on the applicant to show us why we are going to something different. And therefore, it seems to me the applicant must come forward with some evidence as to where they are making a change in what the construction permit authorized them to do.

MR. TROSTEN: Yes, Mr. Chairman. This was a design change which was made and it is up to the applicant to show that the sodium hydroxide system, which is currently proposed, is a fully adequate system.

The problem that we have with this, Mr. Chairman, is as follows:

Mr. Roisman has submitted a proposed finding. He has raised an extremely significant, I believe, legal issue, in his proposed finding. If Mr. Roisman has advised the applicant and indicated in his proposed finding that he was seeking to introduce evidence with respect to the sodium thiosulphate spray system for the purpose of attacking the validity of Mr. Fletcher's testimony or other evidence that applicant introduced concerning the adequacy of our system, I feel quite confident there would really not be a problem here.

The problem is -- as I made clear at the last session of the hearing, we have no objection to Mr. Roisman cross-examining Mr. Fletcher or other witnesses of the applicant concerning what Mr. Fletcher said at the recent session of the hearing, or why it was that the applicant now feels confident that the sodium hydroxide system is adequate and what were the factors that led into that determination.

CHAIRMAN JENSCH: But you don't want him to argue on the basis of the facts that may be developed. Is that your position?

MR. TROSTEN: No. He may certainly argue, Mr. Chairman, after he has completed his cross-examination, that the sodium hydroxide system is inadequate -- if that is what Mr. Roisman intends to do he is at liberty to do it.

CHAIRMAN JENSCH: What does the applicant have to do? The applicant has made a design change here and I think the applicant has to start out to show just why hydroxide is better than thiosulphate. He heard an echo from the other side. I don't know if that assists you or not.

MR. TROSTEN: I think, Mr. Chairman, that it is up to the applicant to demonstrate to this Board that sodium hydroxide system, the system that applicant now proposes, is adequate.

If the board has a question about whether the applicant has made a correct choice in the sense that the board is concerned that sodium hydroxide may not do the job, Mr. Chairman, and that it may not perform as applicant says it will, and as the Commission's Regulations require it to perform, then applicant is prepared to bring forth the evidence necessary to convince the Board of this matter.

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CHAIRMAN JENSCH: Mr. Briggs has a question.

MR. BRIGGS: Mr. Trosten, the point you make is that as long as the addative meets the requirements of the regulations, then it is sufficient? Is that the idea?

MR. TROSTEN: It is a little different than that, Mr. Briggs. The point I am making is as long as the applicant can show that the sodium hydroxide addative, when considered in conjunction with the entire iodine spray removal system, meets the requirements of the AEC as expressed in 10 CFR Part 100, the TID14844, and such matters as the reactor safety guide sets forth, in other words, in toto meets the regulations and requirements of the AEC, then we have done what we are supposed to do.

MR. BRIGGS: If you got a construction permit and what was described therein was a system and if that system were demonstrably better than the system using sodium hydroxide, you wouldn't feel obligated to keep the system that was proposed in the construction permit stage? You would feel it would be all right to install an inferior system?

MR. TROSTEN: Mr. Chairman, let me say this: If the applicant had determined -- I will answer you as a lawyer if I may, Mr. Birggs.

MR. BRIGGS: That is all right.

MR. TROSTEN: If the applicant, for reasons satisfactory to it, decides that it wanted to install a system

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which one could argue was inferior and nevertheless the system installed did meet the standards and requirments of the AEC, did have the necessary margins, did satisfy, as I say, the standards and requirements of the AEC, the fact that there might be or arguably was a system that was better, would not mean that the applicant could not stay with the system that it had ultimately proposed, in my opinion.

MR. BRIGGS: If you went to an automobile dealer and you said you needed a car to drive to work, and he showed you a Cadillac and you said that is fine, and you were going to get that Cadilliac and presumably pay for it, and then he delivered the car and it was a Datsun, and he said "This meets the specifications, it is transportation to work," then that is acceptable?

MR. TROSTEN: That is not quite the same thing, Mr. Briggs, I don't think.

MR. BRIGGS: I agree with you, but as far as the people here are concerned, and as far as the Board is concerned,what they saw originally was a solidum thiosulphate system. At the construction permit stage that is what was to be installed. Then the plant was built, and it had a sodium hydroxide system in it.

Then I guess the argument becomes one have you decreased the safety of the plant by making this change, even though the change may be within the requirements.

MR. TROSTEN: That is an entirely legitimate questio and it should be addressed, Mr. Briggs.

First of all, let me say that the applicant is convinced, the applicant and its contractor are convinced, and we have adduced testimony to the effect that we believe that we have installed the better system in this plant. It is our opinion that we have done so.

MR. BRIGGS: I think that is probably the point to be argued.

MR. TROSTEN: Yes, and we have attempted to do that. But that doesn't really change the legal question though, Mr. Briggs.

CHAIRMAN JENSCH: I think what you are bothered by, as I understand your position, you are worried that somebody is going to make an argument you are not going to like.

I think the imporatnt thing is to get the facts on the record, and then we will see what the arguments are. Then if it looks like he shouldn't make that argument, you can argue the other way.

But it seems to me the factual situation here obligates the applicant to do more than object to a comparison. I infer from the diqscussion that the applicant has not received any order of the Commission authorizing a change from sodium thiosulphate. Is that correct?

MR. TROSTEN: Mr. Chairman, no. We have described in a supplement to our Safety Analysis Report, an amendment to our application, the sodium hydroxide system, and this of course is consistent with the provisions in the Act and the regulations which allow for taking into account changes in technology and the two-step licensing procedure.

CHAIRMAN JENSCH: I understand you filed the papers. But there wasn't a hearing or anything authorizing the change.

MR. TROSTEN: No, sir. This is the hearing at which the adequacy of the sodium hydroxide system is to be tested.

CHAIRMAN JENSCH: Yes, and perhaps this is the time to test the sodium thiosulphate one, because that is what was authorized.

MR. TROSTEN: The problem we have with what Mr. roisman is proposing to do is this: In an administrative hearing there is great latitude, as the Chairman well knows, for receipt of evidence. And the applicant is certainly not attempting here to take a legalistic position with regard to receipt of evidence. Far from it. We believe that great latitude should be afforded in what should come in. And we have followed that principle I think quite scrupulously in our discussions with Mr. Roisman and we have refrained from making legalistic objections to offers into evidence.

But the fact is that he has proposed very carefully a finding of fact which poses a very significant legal question.

And if this legal question is allowed to drag on in this proceeding, Mr. Chairman, toward the end of it I don't think that that would be a very satisfactory state of affairs.

CHAIRMAN JENSCH: Maybe this would be a good time to take a recess and maybe the Board can come back and keep this legal issue from draging, try to resolve it and go ahead.

At this time let us recesss and reconvene in this room at 2:35.

(Thereupon, a short recess was taken.)

CHAIRMAN JENSCH: Please come to order.

MR. KARMAN: Mr. Chairman, I just wanted to reiterate at this time the position I took several days ago at the hearing wherein the staff, consistent with the position it has taken in other licensing hearings, does not believe that a comparison of the spray systems is essential for a Board's determination during the hearing of an application for an operating license.

The staff has evaluated the applicant's system, has found it adequate, and within the confines of the regulations, rules and regulations of the Commission and we feel that it serves no useful purpose to have a comparison of the various sprays because sodium thiosulphate is not at issue in this hearing at this time.

CHAIRMAN JENSCH: The objection is overruled. The witness may answer.

MR. TROSTEN: Mr. Chairman, may I just make an observation? We will want to look at the transcript of the hearing to make a determination whether we should request a certification in this proceeding.

CHAIRMAN JENSCH: All right. This may be an occasion then to pause a bit to permit you to do that and maybe we can proceed to other matters of contemplating the session next week and you will have a chance to review the matter over the weekend. Would that be satisfactory? Except of course Dr. Burley won't be here then.

MR. TROSTEN: The applicant does not object, Mr. Chairman, to the cross-examination -- I am sorry.

The applicant does not take the position that the cross-examination of Dr. BUrley should be deferred.

CHAIRMAN JENSCH: Let's proceed then. Does the witness have the question before him, or can you re-state the question, please?

BY MR. ROISMAN:

Q Doctor Burley, I had asked you to indicate to me what analysis the staff undertook in concluding that the replacement of sodium thiosuophate by sodium hydroxide in the spray system would continue to provide adequate protection and make the spray sufficiently effective?

In fact, and correct me if I am wrong on this, there has been no change made in the staff's spray effectiveness analysis since the change in the spray addative. Is that correct?

A (Dr. Burley) We have never evaluated, or at least I have never evaluated thiosulphate as an addative for the Indian Point 2 reactor. This preceded my time at the Commission. I have only evaluated the effectiveness of the sodium hydroxide spray for iodine removal in the Indian Point 2 containment.

Q Have you ever evaluated the effectiveness of sodium thiosulphate as a spray in any other reactor review?

A On an individual case by case basis, yes, for those applications where the applicant has proposed sodium thiosulphate.

Q Right. I understand. Can you tell me just in general terms, in terms of the effectiveness of the spray for the removal of iodine from the reactor containment, which of those two sprays, sodium thiosulphate or sodium hydroxide, is more effective in your opinion?

A For the removal of elemental iodine, I can see very little difference between the two spray solutions.

For the case of the organic iodide, sodium thiosulphate does afford a very limited, very slow removal of that particular compound.

Q Which is methyl iodine?

A That is a member of the organic iodides.

Q What about hypoiodous acid?

A That is an inorganic form of iodine.

Q Does sodium thiosulphate, is it more effective in the removal of that than is sodium hydroxide?

A There is no data that I am aware of that point in that direction.

MR. TROSTEN: Mr. Roisman, excuse me, may I interrupt for a moment?

MR. ROISMAN: Yes.

MR TROSTEN: Mr. CHairman, before the cross-examination proceeds further, I wanted to make certain that I made the point that by allowing the cross-examination to proceed, applicant does not waive the right to proceed with certification of this matter.

CHAIRMAN JENSCH: Well, if the applicant is not allowing the cross-examination, the Board is. You may reserve all of the rights you desire.

MR. TROSTEN: Excuse me, Mr. Chairman, I used the wrong word.

CHAIRMAN JENSCH: Your rights are reserved in every respect.

Proceed.

BY MR. ROISMAN:

Q Do you have BAW 11024 with you?

A I do.

Q Are you familiar with this report, topical report dated 18 January 1971, "Effectiveness of Sodium Thiosulphate Sprays for Iodine Removal," nonpropriatary report BAW 10024, prepared by Babcock and Wilcox?

A Yes, I have it.

Q Have you in the course of your review of the effectiveness of sodium thiosulphate for any other reactor had occasion to reivew this particular report?

A Indirectly, yes, but only insofar as this report pulls together a lot of information which is scattered in the literature.

There is no original work except in the appendices of the report.

Q But you have looked at it, is that your statement?

A We have looked at it, yes.

Q And it has been a part of the analysis that you have done of the effectiveness of sodium thiosulphate as a spary addative?

A It was not specifically the basis of the analysis.

Q Was it a part of the analysis?

A Well, it depends if you -- my previous information on the effectiveness of thiosulphate came from the reports and from conversations with the indiv iduals involved at the Oak Ridge National Laborator and Batelle Northwest. So this really did not do anything other than just make the information available more conveniently. As such the report was really ot the basis of an evaluation, it did not really inform me of any additional information available. It is a compilation of already available literature data.

Q Are you saying that the compilation that is in here is an accurate compilation of that data?

A Insofar as I can ascertain, yes.

Q In other words, when you were interested in knowing about that data, this was sort of like a reference book rather tan an original source material?

A Actually I have never used it in that context. Most of the information of course is part of my general background knowledge on this point and one doesn't really have to go to reports. When there is a question, I go back to the original report, rather than to the BAW compilation.

Q Give me an example of a time when you did look at it?

A When it was submitted, I read it carefully and attempted to evaluate whether there was new and significantly different information in this report.

I concluded that there was not. Other than that, in the appendices.

Q Roughly when was it submitted? That is, when did you first have access to it?

A The submittal letter is dated January 15, 1971. I think I had access to it shortly thereafter.

Q And this document was submitted to you by Babcock and Wilcox?

A It was submitted to the Division of Reactor Licensing and distributed to certain personnel within the Division.

MR. ROISMAN: Mr. Chairman, I would like to renew my request that the document originally marked Exhibit AA by the Intervenors be accepted into evidence as being a document which has been the subject of review by the staff in the context of its review of sodium thiosulphate spray, and that it be permitted to be included as one of the documents upon which we will rely in demonstrating what the factors are that are involved in considering the effectiveness of sodium thiosulphate spary.

CHAIRMAN JENSCH: Any objection?

MR. KARMAN: Objection, Mr. Chairman.

MR. TROSTEN: Mr. Chairman, objection. The reasons for our objection are essentially the same, the same in principle as the objections I already stated to Mr. Roisman's questions.

CHAIRMAN JENSCH: And the same for you?

MR. KARMAN: Yes.

CHAIRMAN JENSCH: The problem I have with this report as foundation evidence, I do realize that in many cases of this kind investigatory reports are received almost without objection. I think we get in the habit of accepting investigative report as if they were acceptable at all time. I think there may be some problem arising because of that. I have not seen this report, but my impression would be that this document reflects certain experimental work, I presume undertaken by the company designated, and the person who either undertook the experiments or reported them or summarized them should be present to give us some background, it seems to me, on how the tests were conducted and the accuracy of the report and that sort of thing.

Sometimes a document can be admitted through a witness who can vouch for its accuracy or indicate his reliance thereon in the preparation of his opinion. But I don't think this witness has gone that far. He has seen it. As I understand it, he goes back to the original reports for his data, and what those original reports are, I don't know. But from that statement I have the impression that this is a summary of some other reports which raises some additional problems.

But as to this document itself, I have great concern as to foundation. If you can speak to that, please.

MR. ROISMAN: Mr. Chairman, first of all perhaps I can somewhat simplify the situation here. The witness testified that with the exception of Appendix A, the document was in his opinion a compilation of already existing knowledge. And this document has at the end of it three appendices and just before the appendices begin it has a list of references numbered 1 through 94, and if I understood the witness' testimony, he said that he read the document in detail, and that as far as he was concerned, this was an accurate compilation of the data which is included.

As to Appendix A, B, and C, we will remove our request that they be included, since they seem to fall within the ambit of the Chairman's concern with foundation, namely, that the person who actually prepared the independent studies that are contained in Appendices A, B and C is not here and apparently the witness is not prepared to testify with regard to the accuracy of those analyses.

As to the general question of the introduction of the document into evidence, what a document like this represents at least to the intervening parties is an opportunity to get before the Board a variety of sources of information without subjecting the intervenors to the substantial cost that would be involved in producing the 94 underlying documents and the people who prepared each one of those.

We would note, at least as I understand it, that none of the parties here are objecting tothe foundation question per se. They are objecting to the relevance of this document and this whole line of questioning. But they are not bothered by the fact that they don't have available the original author of the report for purposes of cross-examination.

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CHAIRMAN JENSCH: May I interrupt?

MR. ROISMAN: Yes.

CHAIRMAN JENSCH: Is that statement correct, you don't object to lack of foundation.

MR. TROSTEN: That is not quite correct, Mr. Chairman.

Let me say this, we feel that the fact that the original author of the report is not here, we are not objecting because the original author of the report is not here. We do feel that there is a lack of foundation for this document.

We feel that what the witness has testified to does not make the document sufficiently reliable and relevant in order to provide an accurate foundation.

So in the Chairman's terms, we do say there is a lack of foundation for introduction into evidence of this document.

MR. KARMAN: Mr. Chairman, I don't know why the counsel for the Intervenor assumed that either the Applicant's counsel or myself had waived any objection to foundation evidence.

The Chairman raised this issue the other day. I certainly didn't feel I had to add at that time any question. But especially today Mr. Burley has indicated in my opinion very little reliance on this report.

CHAIRMAN JENSCH: I think we are back then to the problem of foundation for the evidence. I don't know what

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the appendices reflect, I don't know what Intervenors are able to assemble in this regard. I think the most the Intervenors could expect would be the witness would somehow vouch for it to the extent that it would make it valid in reference to his testimony.

And I am having difficulty as to whether the witness has gone that far. He did say that he felt it was accurate insofar as he could ascertain. That isn't quite enough, because I think he has got to say it is accurate, if it is accurate.

Then he said he had never used it, he had always gone back to the original report. So I feel there is a serious deficiency in foundation. Whether the subject matter is solely contained in that document or not, I don't know.

Would you care to speak further to the matter?

MR. ROISMAN: Not at this time, Mr. Chairman.

I will go ahead and question the witness on the assumption the document is not in evidence and if I feel that at a subsequent time there is a basis to answer the Chairman's concern, I will request again that the document be included.

CHAIRMAN JENSCH: In the meantime the objection is sustained.

BY MR. ROISMAN:

Q Dr. Burley, getting back to this acid, would you pronounce it?

A Hypoiodous.

Q When sodium thiosulphate is used as the spray for a containment, is there any amount of hypoiodous acid that is produced?

A There is very little information on the existence of hypoiodous acid in the containment atmosphere. The compound itself is very unstable and has a very transitory existence. Therefore, the temporary existence of hypoiodous acid is a very difficult problem to prove. The only information and the most complete investigation on this subject has been performed by the Staff of the Idaho Nuclear Corporation and the results have not been completely accepted by the pertinent experts in the field.

Even their results show that the existence of hypoiodous acid in large quantities is questionable.

Q What is your judgment as to that subject? Would you say that there is any hypoiodous acid present in the containment following the loss of coolant accident, assuming no sprays are being used for the moment?

A The Staff has perhaps skirted that question and made the assumption of 10 percent nonremovable or difficult to remove components and that number would include any fraction of hypoiodous acid which might be present.

Q If the spray used were sodium thiosulphate, would there be a basis for reducing that 10 percent figure as a result of the ability of sodium thiosulphate to remove whatever hypoiodous acid might be present in the containment?

A I don't think there is any evidence which has been produced to date which points in that direction.

Q Are you indicating that there is no evidence upon which you rely, there are no calculations that have been done, or that there are no hard tests that take into account all of the variables?

A Well, the calculations would indicate that regardless of whether one has sodium thiosulphate, that one would have minutely small quantities of hypoiodous acid in the containment atmosphere.

There just isn't any experimental data on which one can rely which points to any different conclusion.

Q What I am trying to understand is when you say there is not any information, is it that there are statistics around which you don't consider reliable, or no one has ever said that hypoiodous acid is removable by sodium thiosulphate?

A Nobody has ever said that, except as a matter of opinion rather than fact as it is stated that sodium thiosulphate would remove hypoiodous acid.

Q You mean a matter of judgment?

A It is a matter of conjecture, I would say, it is not even a matter of judgment. A matter of judgment requires available experimental evidence. This has not been adduced at this time.

Q In other words, without experimental evidence, you could not have what you would consider a reliable judgment as to a subject relating to reactor safety.

A If one were to assume the presence of considerable quantities of hypoiodous acid, I would say one would need experimental evidence to show that fact.

Q I am puzzled now on the term "considerable."

A Of the order of even a fraction of a percent.

Q Is that a standard that you generally apply to determining whether or not someone's judgment is reliable, or are you only applying it to the question of hypoiodous acid?

A A lot of decisions in this field obviously are made on the basis of theory supported by experimental data. The production of hypoiodous acid is well understood, and all of the chemical equilibrium numbers have been published.

Therefore, one can calculate what concentration of hypoiodous acid one would expect in the containment atmosphere based on data which has been adduced previously and is available actually in elementary textbooks in chemistry.

So it is not an unknown compound, it is not a new compound. And the existence and equilibrium situation is, I think, reasonably well understood. Anyone who makes judgments on the basis of a little experimental information which are contrary to the conclusions of many experts I think has to produce a little bit more evidence before the Staff will just jump into an unknown situation.

Q Can you tell me, are you familiar with the process by which hypoiodous acid is produced?

A In the case of the presence of hydroxyl ion, the process is a hydrolysis reaction.

Q This ion you just referred to, is that present in the containment building after a loss of coolant accident?

A The hydroxyl ion is the OH part of sodium hydroxide. It is also part of the OH part of water. So in the presence of water, one also has formation of minute quantity of hypoiodous acid.

Q From what other element is the hypoiodous acid --

A Iodine has to be present obviously.

Q This is elemental iodine?

A Elemental iodine and a hydroxyl fragment.

Q If I understood your testimony correctly, you said that both of these are present in the reactor containment following a loss of coolant accident.

Is that correct? Both of the --

A If you have an iodine release following a loss of coolant accident, this is the premise which has not been established, but let's make that assumption, one has the iodine, one has water, and that is all one needs to form some quantity of hypoiodous acid, the fraction would be very small.

Q You said the presence, the assumption that there is iodine in the reactor following the loss of coolant accident has not been established. You mean there is no basis for assuming that elemental iodine is there?

A It is conceivable, of course, that one has all hydrogen iodide. It is also conceivable even following the loss of coolant accident, if all of the engineered safety features work, that there is no release of iodine from the internal parts of the reactor.

Q Perhaps it would be simpler if we answered the question with regard to the assumptions made by the Staff in evaluating the effectiveness of the spray system. In that case --

A In that case, using the TID-14844 assumptions, one has iodine and one has hydroxyl ions.

Q If sodium thiosulphate were also present, would that affect the creation of hypoiodous acid?

A I don't think significantly. The sodium thiosulphate, of course, also removes iodine and the iodine is depleted either by the reaction with the thiosulphate or with the hydroxyl and in either case as soon as the elemental iodine is reduced to negligible quantities, the existence of hypoiodous acid is also eliminated.

Q It is your testimony that there is no possibility when the sodium hydroxide comes in contact with the elemental iodine, of there being the least hypoiodous acid as a result of that reaction.

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A That was not my conclusion. My conclusion was the quantity would be small.

Q And I was asking you, I understand that it is your judgment that the quantity would be small, but it will occur. Will the same thing occur if the elemental iodine comes in contact with the sodium thiosulphate?

A Remember the sodium thiosulphate solution also is made basic by addition of sodium hydroxide, the same situation would obtain.

Q In other words, there would be no differences in the amount of hypoiodous acid that would be produced from the reaction of the sodium thiosulphate spray additive, with the sodium hydroxide in it, or the spray additive with only sodium hydroxide?

A Speaking from my experience as a chemist, I would say the difference would be very small if at all.

Q Small in favor of what?

A If one had to pick favorites, maybe one would pick sodium thiosulphate. But we are talking about negligibly small quantities to start with.

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Q I understand. Is the reaction between sodium hydroxide and iodine a reversible reaction; are there conditions in the containment following a loss of coolant accident in which that reaction might be reversible?

A The reaction of iodine with sodium hydroxide is reversible. Under the conditions in the containment where your ph is quite high, the equilibrium is very far towards the production of the important soluble forms and one would have to considerbly reduce the ph or the alkalinity of the solution before one would expect not to essentially clean up, all of the iodine in the containment.

Q In your analysis of the loss of coolant accident situation, when the spray addative is sodium hydroxide, is it a fair assumption to say that there will be some reversal of the reaction between the sodium hydroxide and the iodine?

A Some yes. It depends on whereyou put your equilibrium and the equilibrium would maybe keep one elemental iodine atom or molecule in that form compared to let's say 10,000 atoms in the soluble form.

Q Now I am talking about, when I mentioned sodium hydroxide addative, I mean in the mixtures proposed for this plant?

A The net effect is essentially that both solutions remove elemental iodine from the containment atmosphere at approximately the same rate, and approximately as effectively.

The only difference is that the residual iodine concentration at time infinity could be a slight bit higher for the sodium hydroxide than for the sodium thiosulphate. But we are talking about hundredths of a percent. We are not talking about large quantities of elemental iodine remaining air born.

Q Just tomake sure I have it absolutely clear, in other..words, this reversible reacdtion doesn't occur when the spray is the one percent weight sodium thiosulphate addative, but it does occur with the sodium hydroxide, but in an amount that you consider to be negligible?

A It does occur all of the time. The effect at the beginning when the concentrations are high is trivial. The only place where it shows up is for very very long time periods, where one would expect to have a really very minute quantity of elemental iodine above the sodium hydroxide solution.

But of course at that point the iodine has also decayed and one doesn't have the same radiological hazards.

Q I understand. But that is a phenomena which doesn't occur if the spray is sodium thiosulphate, is that correct?

A The observed residual iodine concentration above the sodium thiosulphate solution is smaller, but again it is not zero.

Q I understand, Dr. Burley. I wonder if you would look at this document, BAW 10024, which you have there, and

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if you would, direct your attention to page 4-31.

A 31?

Q Yes. Do you have it?

A I have it.

Q That particular chart purports to show a comparison between the effectiveness of the removal of gas, gaseous iodine in a loss of coolant accident as a result of tests run at CSE -- that is at Battelle,I think you said?

A That is correct

Q And the comparison is between runs A-10 and A-12. First of all, are you familiar with the runs that are purported to be shown on this chart?

A Yes.

Q To the best of your knowledge, this thing underneath the run A-10 and run A-12, the various components that were used, is that an accurate statement of the parameters of those two experiments?

A I think so.

Q To the best of your knowledge, is the chart that is shown here an accurate description of the results of those runs?

A Yes, it is.

Q COuld you state in your own words what this comparison indicates regarding the effectiveness of sodium thiosulphate and sodium hydroxide in terms of removing gaseous iodine in the reactor building following a loss of

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coolant accident?

A This is a composite of the removal of both inorganic and organic iodides by both of these solutions. Therefore, a long-term difference exists between these two in terms of the thiosulphate having a higher overall iodine removal capability than the sodium hydroxide solution by itself. This relfects almost entirely the differenc e between the methyl iodine removal capability of that one solution,

to some extent the difference in equilibrium above the solutions, which as has been indicated is quite small

Q This is the reversible reaction?

A That is correct. There has been a fair bit of controversy about the immediate effects of these two sprays during the early period which does appear to indicate that sodium thiosulphate is slightly more effective in reducing the elemental iodine concentration.

The staff of Battelle Northwest Laboratory thinks that some of this may be due to mixing considerations, entrance of the iodine into the containment vessels. In other words, the two experiments were similar, but in a large facility like this, it is almost impossible to run two identical experiments. And they considered that within the range of reproducibility of their results, that there is really no difference during the early period for elemental iodine removal by either sodium thiosulphate or sodium hydroxide.

Q Do I understand that you say that even with the very same facility, in an attempt to run two identical tests, it is virtually impossible to do so, and when you get the results, in this case the spray addative was changed and an attempt was made to keep everything else virtually identical, that you can't always rely upon the difference shown as being an accurate difference b ecause of inherent difficulties?

A That is correct. The apparent difference between these two woiuld really amount to less than a five percent difference in effectiveness of the two solutions, if one took these as they are reproduced here.

Q To just deviate for a second, as I understand it from your testimony earlier, results of studies that have been run at Battelle Northwest facility, which is substantially in size, shape, different from the Indian Point 2 facility, do form the basis for the staff analysis that certain theoretical data is justified by experiments that have been run. Is that correct?

A It forms the first basis, yes, on which we superimpose factors of conservatism.

Q To make up for, among other things, this kind of--

A Exactly, yes.

Q Do you know, when you do that, how much the error might be?

A If you have a sufficiently large number of runs, you can get a better estimate of errors. For one or two runs, it is very difficult. And one has to ascribe a larger error limit. The number of runs in the CSE experimental facility in the aggregate is now greater than 10. And the statistics are fairly good.

Q Are the results that are shown on this chart of the differences in effectiveness between sodium thiosulphate and sodium hydroxide, have they, as far as you know, been duplicated in general -- I understand not specifically -- in other tests run either by Battelle Northwest or Oak Ridge or somewhere else, to your knowledge?

A The general results have been duplicated in a number of different cases, both as to residual iodine concentration and also as to methyl iodine removal rate.

Q Can you summarize for me -- we are so far on the positive side of this equasion. But can you summarize for me the merits of sodium thiosulphate versus sodium hydroxide in terms of their reliability and their effectiveness in the removal of iodine in the post-loss of coolant accident containment environment?

A Well, you put your finger on the problem. There are two sides to this coin. On the question of over-all iodine removal effectiveness, and certainly on the side of methyl iodine removal effectiveness, there is an advantage on the side of the alkaline sodium thiosulphate solution.

On the other side of the coin is the fact that the sodium thiosulphate system requires addition of two solutions, so one gets into the question of reliability, and also the fact that sodium thiosulphate itself is an unstable compound.

Q You neatly led into that for me. I was going to ask you about what is evil about sodium thiosulphate. Can you take the two features you just spoke about and expand on them a little bit for me?

A Shall I start on stability?

Q Why don't we start with the problem of having two solutions?

A I would rather have the other people from the staff talk about that, because I am not an engineer.

Q All right, go then to the stability if you wish.

A Thank you.

Q And if you don't mind, correct me if I am wrong, but I understand that sodium thiosulphate has two types that can be used, that is, that the mixture that you get -- I am sorry, when I read it I had a difficult time compre-he-ding it, but it is like a plus or minus solution.

A I will preface my remarks by explaining your difficulty.

If one takes the boric acid which is injected for reactivity control, and adds only the sodium thiosulphate, one gets a solution whch is acetic. And if one then either heats that solution to the temperature which one would expecdt in the post-accident environment, the solution very rapidly decomposes and in the process deposits substantial quantities of free sulphur and the free sulphur of course would be in solution.

If one, however, adds to the solution of the boric acid and sodium thiosulphate an amount of sodium hydroxide which is essentially the same as the amount of sodium hydroxide which is added to the Indian Point 2 system, then one also gets up to the same alkaline solution in the range of pH 9.5.

The stability of sodium thiosulphate is somewhat enhanced by this addition of sodium hydroxide, but one still gets a decomposiion both by thermal and by radiation energy. The only and a major and fundamental difference is that from an alkaline solution one gets either no or negligible precipitation of sulphur or of sulphides, which could play havoc with the heat exchange characteristics in the core and other places in the containment.

So as it goes, by adding the sodium hydroxide to the sodium thiosulphate solution, you haven't really changed the stability aspects of the sodium thiosulphate, you have just changed the nature of the products which you obtain from that decomposition.

On the other hand, sodium hydroxide is a stable compound, it is not affected to any extent by either the temperatures or the radiation conditions which would obtain in this reactor under the post accident conditions.

So this is the con of the sotry. We may have a slight advantage in terms of iodine removal effectiveness with the sodium thiosulphate, and against that we balance the problem of less stability and competent people in a number of organizations have argued the point, there is no consensus as to which is preferable. Obviously two different vendors have chosen to go the two different routes.

Q As I understand it, this particular vendor has also gone both routes, one for the construction permit, one for the operating license.

A I think I should maybe put that in perspective if I may.

I don't recall the date of the construction permit application for this particular plant, but that was in the very early stages when even the use of chemical addatives to containment spray solutions was still under discussion. There was little, if any, experimental data to indicate either the effectiveness or the stability of these solutions. And as any chemist knows, if you go into a laboratory and so mebody tells you to determine iodine, you titrate the sodium thiosulphate. This is the reagent of choice. And I think this is undoubtedly the reason why Westinghouse proposed sodium thiosulphate. It is a natural reagent that ones looks at.

Some of these other problems appeared later as experimental evidence accummulated and at that point the Westinghouse organization had to make a choice, do they want to go with a less stable addative or do they want to take the penalty of a slightly smaller iodine removal effectiveness and obviously they made the choice, they have amended their application and decided that this was the preferable way to go.

Q Doctor, let's go back to the question of the production of solids.

Do I understand your testimony to be that in the conditions which exist in the loss of coolant accident, the mixture of sodium hydroxide and borated water and sodium thiosulphate will produce solids that can cause difficulty for other purposes of the reactor?

A The alkaline solution of sodium thiosulphate will produce no or negligible quantities of solids. The acetic solution without the sodium hydroxide added will produce sulphur and sulphides.

Q What are the risks then in the context of the alkaline solution ?

A One has to keep the pH up to a region where this will not occur.

Q Is the sodium hydroxide spray that is designed for this plant, it has a designed pH of 9.5, is that correct?

A That is correct.

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Q Why is it that there is a pH of 9.5?

A The removal effectiveness of the sodium hydroxide solution increases as the number of hydroxyl ions increase. Therefore the higher the pH of the solution, the more effective it is in removing iodine.

However, one has to again, in sodium hydroxide's case, balance this against the cons. If you get your solution too alkaline, you start h aving possible problems with alkaline corrosion materials. And 9.5 just happens to be an optimum value, where you don't have any problems in that direction , and still have adequate removal of iodine.

Q WHat techniques exist to hold that 9.5 pH level in the sodium hydroxide borated water solution?

A Up or down?

Q To hold it from going down.

A Well, if all the sodium hydroxide is added to the solution, this is the design level, and there is no way that I can conceive which would appreciably reduce that hydroxide concentration over the operating lifetime of that system.

Q Then explain to me why, if sodium thiosulphate is part of the spray, there is really any problem with the possibility of the pH going below 9.5? I don't mean problem, if it does, but why you feel there is some concern that it might?

A Because the reaction which describes the decomposition of the sodium thiosulphate also uses up sodium hydroxide in the same process. So as you decompose the sodium thiosulphate, which we said occurs, you also use sodium hydroxide, and as you use up sodium hydroxide, the pH of the solution decreases.

Q Have any statistics been prepared to indicate what the period of time is over which that would occur?

A It depends on the temperature and the radiation exposure of the solution.

Q Well, we are concerned here about temperature and radiation exposure in a loss of coolant accident situation. So to the extent those same parameters have been used to study sprays generally, have they ever been done to figure out --

A The staff calculates that one may reudce the pH of the sodium thiosulphate solution in a reactor using the alkaline thiosulphate solution in probably less than 10 days under the worst possible conditions.

Therefore we also intend to require all the plants which use sodium thiosulphate to both be able to measure the pH of the solution and to have the ability to replenish the pH.

Q Is it your feeling, when you say it reduces it, did you mean reduce it to zero, or reduce it to the point where the formation of solids starts to become a problem?

A We consider a pH of the order of about 7.5 which is comparable to a pH of 7 which is neutral, so it is just a little more alkaline than neutral, to be essentially the cross-over point where you have to be careful. We intend to require all of the plants which use sodium thiosulphate to keep the pH considerably above that point.

Q Do you foresee any difficulty involved in having that requirement, that is the addition of sodium hydroxide on a regular basis, in order to keep ph up? For instance, are there means readily available to measure the ph of the spray solution during the time that it is being used following the loss of coolant accident, and means readily available --

A It can be done, and of course it is not an emergency situation. One has a number of days to do this sort of thing.

CHAIRMAN JENSCH: While there is a pause, I wonder if intervenor's counsel can indicate how much more examination he has of this witness?

MR. ROISMAN: Just a few more minutes, Mr. Chairman. I received a document from the staff and I don't think that -- it didn't have a cover page on it, and it relates to this question, but I don't know what it is and I can't ask the witness about it, because I can't tell him what I am talking about.

CHAIRMAN JENSCH: Maybe he will recognize it.

BY MR. ROISMAN:

Q Dr. Burley, can I direct your attention to page 21 of ORNL-TM-2412-1?

A I have it.

Q In the middle of the page, subsection (g), it says, "Radiolytic solids generation." It is discussing this problem that we just discussed a moment ago about the formation of these radiolytic solids through the use of sodium thiosulfate spray additive. At the bottom of the page there is a statement, "However, it should be pointed out that it is stated that thiosulfate can readily be obtained by boiling sulfer with solutions of sulfate, so under the conditions of the postulated accident, the reaction to decomposed $S203^{2}-2$ is $D3^{2-0}$ should be minimal."

Can you tell me what that means in the context of our discussion about the formation of solids? And let me state that to my very untrained mind, I thought it meant that the conditions that exist in a postulated loss of coolant accident are such that the formation of solids is not a problem with sodium thiosulfate.

A This in effect says that if one has a boiling solution and if one has a sulfer, and if one has a sulfate atoms all together, then there is a chemical reaction which reduces the sulfer which may already have been formed.

I don't think this really changes our conclusions. We had previously said that in the case of an alkalyne sodium thiosulfate solution, that the production of free sulfer is negligible. So to go back around the circle and say the sulfer which might be formed in small quantities can again be regenerated and form a more soluble form. only essentially reinforces the idea that sulfer productions for an alkalyne solution is a negligible problem.

Q Would you agree with me -- would you direct your attention to page 25 of that document, I guess subsection (i). Would you read it.

A Radiolytic ph change.

Q Yes.

Without reading it into the record, but just read through the paragraph and then read the last sentence under that subsection (i).

A The last sentence in that paragraph reads as follows: "The data previously reported, reference 26, have indicated some small lowering of the ph upon radiolysis. There is of course always the possibility in the real case of some reaction between metal ions present and OH to form insoluble hydroxide. If this does occur, obviously the solution will become more acetic. In any case, it should be stated that the ph of the spray solution must be monitored during the accident. If loss of basicity does occur, addition of more sodium hydroxide should present no major problem."

Q Are you basically in agreement with that conclusion?

A I am not in agreement with the last statement, because I think it is a hypothetical situation which has been analyzed by the staff and felt to be impossible in the Indian Point #2 case.

In other words, what we are talking about here, just to put things in perspective, is if one has, let's say, aluminum, metal, in the reactor, and also sodium hydroxide, that it is possible and probable both, that one would form aluminum hydroxide at a finite rate and therefore use up some of the sodium hydroxide by this process, rather than having it available for iodine removal.

The quantity of such materials in the Indian Point #2 reactor has been analyzed by the staff and we have concluded that even if all the aluminum in the reactor reacted with the sodium hydroxide, that the ph would not be decreased below 9.1 or .2.

Q So there is no provision for the addition of additional sodium hydroxide after a loss of coolant accident occurs?

A I don't think so. I would like to refer that to Mr. Kniel.

A (Mr. Kniel) The question was is there any provision for the addition of sodium hydroxide?

Q Yes.

A After the loss of coolant accident?

Q Right.

A The sodium hydroxide is contained in a separate tank which is added to the spray solution as it is injected. When that tank is empty, there is no present provision for adding further sodium hydroxide to that tank. However, that tank is accessible, and further hydroxide could be added to it.

Q You mean it is on the outside of the containment building?

A That is correct, it is in the primary auxiliary building.

Q Thank you.

Dr. Burley, the other half of the problem that you mentioned with sodium thiosulfate, you said was somewhat outside of your area. Would you just describe it to me again?

What was the other problem, other than the formation of solids? I think you said it was the mixing of two --

A (Dr. Burley) It is the reliability of injecting two solutions versus the reliability of injecting one solution.

Q Can you just explain to me what you mean by that? Tell me when it gets beyond your expertise. But you mean two solutions being one that has got only sodium thiosulfate in it and one --

A I am referring essentially to your roll of the dice the other day, that as one wants more events to happen simultaneously, the probability, even though it may still be tremendously high, is reduced for both events occurring simultaneously.

Q I wasn't talking about your analysis of the probabilities of there being difficulties, but I didn't understand what you meant by the use of two sprays. I thought --

A Two solutions. Sodium hydroxide and sodium thiosulfate are stored separately. They are not stored --

Q Not premixed in other words.

A They are not premixed. They have to be mixed during injection phase.

Q What you are saying is trying to make sure the mixed quantity that comes out the nozzle is exactly right is increased to some extent by having to mix three things, borated water, sodium thiosulfate and sodium hydroxide, over what it would be if you just mixed borated water and sodium hydroxide?

A Did you say increased or decreased? It should have been decreased.

Q Okay. That is correct.

A That is correct.

MR. ROISMAN: I have no further questions of Dr. Burley.

CHAIRMAN JENSCH: Very well.

We have had a long day today without having lunch because we hoped to try to permit full cross-examination of Dr. Burley, who will not be able to be with us next week. We therefore feel we cannot go forward with the further procedure considerations that we had in mind.

The Board does desire to express some matters for the concern of the parties and it is our present contemplation to recess after the statement of the Board's expressions until Wednesday, in view of the fact that we are getting such a late departure out of here today that we want to give time for consideration to the motion that has been made for low power and subcritical testing.

And I should announce on the record a conference as to a procedural matter with a member of the New York State delegation -- ordinarily I do not like to have any conversations with any party or representative except on the record, unless I make disclosure of it.

I fortunately caught the gentleman who is Director of the Technological Branch I guess for the Department of Commerce for the State of New York, and he said Mr. Davies could be here on Wednesday.

Mr. Briggs has some expressions to present at this time.

MR. BRIGGS: The applicant asked whether we had any questions that his staff and others might work on while we were in recess, and so the Board did indicate here some possibility of questions. I would like to ask Mr. Roisman a question first.

Mr. Roisman, the intervenors have asked a number of questions related to the strength of the reactor vessel and rupture of the reactor vessel. Could you briefly summarize the position of the intervenor on this subject at the present time.

MR. ROISMAN: I will try as best I can.

Basically the position is this: We believe that from what we can tell from the data that we have obtained, the applicant has not established its burden, that the method of construction that was used for this reactor vessel, testing, inspections that were carried out, were adequate to reach the assurances that there are no flaws or other -- as you may remember, we got into some argument about what the words meant -- but imperfections of some kind or other, without indicating a qualitative judgment, in the materials out of which the reactor was constructed.

Because we don't know, we are not sure how many there are, what size they are, there are problems, we believe, with the long term, particularly long term operation of the vessel during which time we think that those flaws or imperfections could expand become weak spots in the reactor vessel, particularly, say, in the worst possible condition of design basis loss of coolant accident occurring many years in the future, 25 to 30, or even occurring in the next four or five years, where the projected strength of the materials and the transients that would take place inside of the reactor vessel might not prove out and that there might be a rupture of the reactor vessel as a result of that.

Part and parcel of that is our concern with the methods planned for inspection of the reactor vessel, to wit: The hoped for but not guaranteed 10-year inspection of the internal portion of the reactor vessel by some remote method. And out concern that they are not an absolute requirement of the technical specifications. That is not to say that our ultimate position would be that those inspections alone would remove our concern, but they certainly would help us in terms of placing reliance on the strength of the reactor vessel.

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MR. BRIGGS: Thank you, Mr. Roisman.

A question for the Staff: Is there a regulation that says what the design basis accident must be?

MR. KNIEL: Well, the general design criteria address themselves to what criteria certain systems have to meet, and they do, I believe, discuss that the primarily system, the reactor has to be capable of enduring a double-ended break in the primary system.

MR. BRIGGS: You say that there is a regulation that says that it must be capable of doing this. Does it say that this is the largest design basis accident that one can have?

MR. KNIEL: Well the only reference I was making was to the general design criteria, Appendix A of Part 50 of the Commission's regulations. I would have to look up the exact wording.

MR. BRIGGS: In the absence of a regulation that says what the design basis accident is, it seems that we are confronted here with a situation where the Intervenors consider the rupture of the reactor vessel to be credible, and the Applicant considering that the rupture of the reactor vessel is incredible.

MR. KNIEL: Under the new Appendix A, general design criteria, Appendix A of Part 50, under definitions and explanations, it says, "Loss of coolant accidents. Loss of coolant accidents mean those postulated accidents that result from the loss of reactor coolant at a rate in excess of the capability of the reactor coolant makeup system from breaks in the reactor coolant pressure boundary up to and including a break equivalent in size to the double-ended rupture of the largest pipe of the reactor coolant system."

MR. BRIGGS: So it does not say where rupture has to occur, it just says it must be equivalent to that.

MR. KNIEL: That is correct.

MR. BRIGGS: Thank you.

As I indicated, the Intervenors have suggested that a rupture of the reactor vessel, a break in the reactor vessel is credible, and the Applicant's witness, I believe, the other day suggested that in his opinion this was incredible.

So we are sort of confronted with a situation where one says maybe it is, and the other says no, we think it is not. But I think there is considerable lack of evidence to support either position at the present time. So the Board feels that it should have more information on this subject in support of the Applicant's position and we would hope to have evidence introduced by the Intervenor in support of its position.

I am sure whether this information can be provided when we meet next week, but if not next week, some time in the future.

I must say that this concern is not only the concern of Intervenors, at least it has not always been, because the ACRS published a letter, sent a letter to Chairman Seaborg in November of 1965 in which they talked about reactor pressure vessels and although they indicated their belief that the possibility of a failure, the probability of a failure of the reactor vessel was low, they also indicated that, and I quote, "Nevertheless, it seems desirable and possible to make some provisions in future designs against this very unlikely accident."

They indicated further, number one, "to reduce further the already small probability of pressure vessel failure, the Committee suggests that industry and the AEC give still further attention to methods of details of stress analysis, development and implementation of improved methods of inspection" and so forth.

And this is pretty much reflected in the research and development programs that the AEC has sponsored. As Point 2, they say, "The ACRS also recommends that means be developed to ameliorate the consequences of a major pressure vessel rupture, some possible approaches include, A, design to cope with the pressure buildup in the containment and to assure that no internal generated missile can breach the containment."

As I read the FSAR, provisions have been made in this plant to prevent missiles generated by rupture of the pressure vessel from breaching the containment.

"B, provide adequate core cooling or flooding which will function reliably in spite of vessel movement and rupture."

I believe in response to a question some months ago it was indicated that the core cooling system that is provided here will not necessarily function reliably in spite of vessel movement and rupture.

I should not say it won't function, but it may not accomplish its purposes of flooding and cooling the core. There is a third point here which says, "If breaching the containment cannot be precluded, provide other means for preventing uncontrolled release of large quantities of radioactivity to the atmosphere."

Apparently, the Applicant has taken on the job of preventing the containment from being breached. The ACRS further goes on to confirm its belief that no undue hazard to the health and safety of the public exists, but suggests that"the orderly growth of the industry with concommitant increase in number, size, power level and proximity of nuclear power reactors to large population centers will in the future make it desirable, even prudent to incorporate in many reactors the design approaches whose development is recommended above."

In this plant the position then in the design has been taken that the rupture of the reactor vessel is not credible and that these things need not be done. And we would like to have information supporting this position.

I think under the circumstances, the Board needs to know in considerable detail why the design basis accident proceeded from rupture of pipes attached to the primary system to rupture of the largest pipe in the primary system, but stopped short of rupture of the reactor vessel.

The information required is principally in justification that failureof the vessel is incredible. But what studies did the Staff make to justify this in their own minds? Are these studies described in reports and are these reports available? If not, would the Applicant and the Staff please provide such a justification in considerable detail.

What are the features of the ASME Code and the design basis that provide the necessary high degree of assurance. What are the features of the fabrication processes that continue this assurance? What are the features of operation maintenance and so forth that maintain the assurance?

Are there data for high pressure vessels and piping systems that have been manufactured and installed according to codes that provide the confidence in the conclusion that failure of the vessel is incredible?

For instance, I don't know of failures of large pressure vessels in the past year or two, but I understand that there have been at least two failures of high pressure piping in large fossil fuel plants in this time and one piping failure during the testing of the H. B. Robinson II plant.

Presumably, this piping was installed according to particular codes. If the Applicant and Staff are familiar with such failures in the systems andtheir design and installation were governed by codes, how should these failures be viewed in the context of deciding whether failure of the reactor vessel, for instance, is or is not credible?

In providing the above information, we encourage the Applicant and the Staff to be certain that it is prepared by persons who are very highly qualified in the fields involved.

It may be that the people who are presently Staff witnesses and Applicant's witnesses have those qualifications or they may not have the qualifications.

I believe in Mr. Wiesemann's cross-examination he suggested that the Indian Point 2 vessel would rupture at a pressure of about 8,000 psi. I think it would be worthwhile to include information in the evaluation mentioned above to indicate the basis for such calculations and the basis for suggesting that the pressure could be this high.

Are there any questions concerning this information?

MR. TROSTEN: I have no questions at this time, Mr. Briggs. I would like to make this observation, that the Applicant has provided Mr. Briggs considerable information in

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the Final Safety Analysis Report and also in response to informal questions which were raised by the Citizens Committee for the Protection of the Environment concerning the possibility or likelihood or whatever word you wish to use of a pressure vessel rupture.

Copies of these answers to informal questions have been furnished to the Board. Of course, the Applicant will be prepared fully to respond to you concern, Mr. Briggs, with the necessary witnesses who have the necessary qualifications.

In the event that we have any questions, any questions occur to us after a review of the transcript, we may possibly be in communication by telegram with the Board, should this prove necessary. I am not aware of any questions that we have at this point.

MR. BRIGGS: Let me say I have look at, I believe the answer to most of those questions; I may have not seen them all, and in general I get the following information I believe: That the vessel was constructed according to Section 8 at the time I suppose of the ASME Code; it meets the requirements of Section 3 of the ASME Code, and it was inspected and there were good quality assurance practices used.

But what is it about Section 3 of the ASME Code that provides such confidence that the vessel cannot fracture?

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And then how much margin, if you wish, is there in the design of the vessel, if it is fabricated according to this?

I realize this involves going into considerable detail and technical matters, but I think if reports have been written that have gone into this detail, those reports could just be presented to the Board.

MR. KARMAN: Mr. Briggs, you mentioned some pipe failure in fossil fuel plants. Is there any particular reference you have in mind?

MR. BRIGGS: I am sorry, I don't have them with me. But I believe the Westinghouse people know of a rupture in the crossover piping on a turbine up in the Northeast. This is my understanding of where it occurred.

I believe there has also been a failure in high pressure piping in a steam plant in the Midwest.

Now, there may be many other failures that you people have knowledge of. I wouldn't restrict it to this. And I think you are acquainted with the failure in the H. B. Robinson II plant.

I just offer these as examples, that here are pieces of pipe that presumably were installed according to Codes and they did fail rapidly, maybe brittly, I am not sure. One asks now why is that Code so deficient that it permits such failures, whereas Section 3 of the ASME pressure vessel code is not deficient and it precludes such failures.

MR. TROSTEN: Mr. Briggs, just one reservation --

MR. KNIEL: Could I make one addition to your comments, in that the Robinson failure was during a pre-operational test.

MR. BRIGGS: Yes, I think the plant operator was lucky.

MR. KNIEL: I mean part of the program we have in assuring quality is pre-operational testing. I just wanted to state that that failure was during the pre-operational test.

MR. BRIGGS: Yes.

CHAIRMAN JENSCH: That gives you some assurance, does it? I would think you would be more concerned than ever.

MR. BRIGGS: No, I think that is reasonable. It shows in certain cases at least the testing procedures we had found some of these failures. One sort of asks, though, suppose there had been just a little more margin in the design -- presumably it was designed according to a code -- that would make such a failure not impossible, but not very probable either.

MR. KARMAN: We certainly will endeavor to supply the Board and the parties with the information requested.

CHAIRMAN JENSCH: Mr. Briggs has some further statements.

MR. BRIGGS: On the inspection business, the applicant has replied to several questions that I have had on that. The last answer provided considerable information about research and development programs. I had hoped that after reviewing the information the applicant would be so confident of the status of inspection that he wouldn't like to retain the reservation in the technical specifications but apparently he is not so convinced, so it is a little difficult for me to be convinced.

Certainly the question concerning the cladding on the inside of the vessel that had been prepared for inspection and had been inspected after the pressure test I believe from the inside to give baseline data fulfilled the requirements of one of my questions. Apparently as I understand it the problem now is that of providing equipment that will make it possible to do the same measurements when the vessel is filled with water and the persons making the measurements are working from a platform above the vessel, if devices are then required for making similar measurements in piping or through holes in the shielding.

Could an applicant witness tell me whether that is the status of the inspection.

Whereupon,

WILLIAM CAHILL

resumed the stand and, having been previously duly sworn, was examined and testified as follows:

WITNESS CAHILL: Dr. Briggs, the reservation we have is not that such equipment is not now available; such inspections have been made, but our interest is in the development of efficient equipment for the remote inspection, so that the outage associated with this inspection is not of excessive duration and our simple hesitancy to volunteer to give up that flexibility that the tech spec offers, which in my mind just covers a hypothetical situation where equipment is on the verge of development, maybe even in manufacture, and there is some period of time wherein some leeway might be allowed.

I am personally confident from having followed parts of this program that we will within the 10 years of the -- that practical equipment will be available. But it is against our principles to offer something that is not already in our pocket.

MR. BRIGGS: Yes. I understand.

I wonder if the applicant could provide some of the following information for us, -- or the staff. I did read the answers to the questions and I looked up the references that were provided in the answers to the questions, and I found things in there that were very interesting and that would improve the efficiency with which one could make the inspections, but I didn't find anything in there that indicated what work was being done to develop the actual device that would be required for going down and making the measurements.

So could the applicant or the staff please provide the Board with references, if they exist, and if the inspections have been done, to reports that describe the in-service inspections and the results that have been obtained from the reactor vessels for the Shippingport Reactor, Dresden #1, Yankee-Rowe, San Onofre, Connecticut Yankee -- if any inspections have been made there, and any other plants. In other words, references to reports that describe the inspections, the equipment that has been used, and the results of the inspections.

I have two other questions here. One that may draw some objection. I am not sure. It has to do with the removal of the crucible and I suspect it is in the same category as the sodium thiosulfate spray. But could information be provided on the amendment and date of the amendment that requested or proposed the indicated removal or indicated the removal of the crucible, the date that any letters were returned to the applicant from the AEC indicating approval for such removal, and then any reports of documents that were prepared other than those that indicated considerations that were involved in deciding to remove the crucible and in accepting the removal.

MR. TROSTEN: Mr. Briggs, at least some of that information, perhaps not all of it, was provided in the response to the questions raised by the Board on March 24.

MR. BRIGGS: Yes. If there is any additional information, I would like to have that.

MR. TROSTEN: All right. Any additional information that you just identified, not already furnished.

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Mr. BRIGGS: That is right. There was one other short one and that is the following. There was some talk about single failure criteria or design according to single failure criteria.

Could the Applicants provide for us a reference in the FSAR, if it is there, to the back-up that is provided or the effect of failure of a single component such as the refueling water storage tank, and whether that is considered to be a component in this sense?

MR. TROSTEN: Yes, Mr. Briggs.

CHAIRMAN JENSCH: Very well, if there is nothing further, at this time we will recess to reconvene in this room on Wednesday, July -- did you have something further?

MR. TROSTEN: I was waiting until you were finished.

CHAIRMAN JENSCH: I am about to adjourn, so if you have something, proceed.

MR. TROSTEN: I simply wanted to inquire of Mr. Roisman, I guess, whether he intends to start of f the next session of the hearing with cross-examination of Mr. Davies and whether he has any other intention with respect to the next session?

MR. ROISMAN: Mr. Chairman, we still have the parallel session, which I assume will be held on that Wednesday, and I am amenable to any arrangement the Board wants to make as to when to hold that. If we can start with that, I think I have maybe an hour and a half.

CHAIRMAN JENSCH: Very well. We might consider the parallel procedure first thing in the morning and consider that the public hearing would reconvene at 11 o'clock.

Would that be agreeable to the parties?

MR. TORSTEN: Yes.

MR.ROISMAN: That is fine.

MR. KARMAN: Yes.

CHAIRMAN JENSCH: Does that conclude your statement?

MR. ROISMAN: The only other thing, to answer Mr. Trosten's question, would be the questioning of Mr. Davies and ultimately questioning on the emergency core cooling system.

CHAIRMAN JENSCH: Very well. We will have some recess I take it before the latter item is reached.

MR. ROISMAN: THat is my understanding.

CHAIRMAN JENSCH: Very well. At this time this public hearing will recess to reconvene in this room at 11 o'clock on July 21, 1971, for further consideration of the matters relatd to the application of cConsolidated Edison Company.

(Thereupon, at 4:15 p.m. the hearing was recessed, to reconvene at 11:00 a.m., Wednesday, July 21, 1971.)

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