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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

THERMAL HYDRAULICS SUBCOMMITTEE

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MEETING

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ROCKVILLE, MARYLAND

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THURSDAY

FEBRUARY 16, 2006

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The Subcommittee met in Room 2TB3 at Two White Flint North, 14555 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Graham B. Wallis, Subcommittee Chair, presiding.

PRESENT

GRAHAM B. WALLIS	Subcommittee Chair
RICHARD S. DENNING	Subcommittee Member
THOMAS S. KRESS	Subcommittee Member
WILLIAM J. SHACK	Subcommittee Member
SANJOY BANERJEE	ACRS Consultant

NRC STAFF

RALPH CARUSO	Designated Federal Official
MICHELLE EVANS	RES
TOM HAFERA (by teleconference)	NRR
ERVIN GEIGER	RES
SHANLAI LU	NRR
MIKE SCOTT	NRR
ROBERT TREGONING	RES
MATT YODER	NRR

OTHER PRESENT

TOM ANDREYCHEK	Westinghouse
MAURICE DINGLER	WOG
ANNE P. FULLERTON	Naval Surface Warfare Center
BRUCE LETELLIER	LANL

A-G-E-N-D-A

P-R-O-C-E-E-D-I-N-G-S

8:31 a.m.

CHAIRMAN WALLIS: Please come into session. Good morning. This is the third day of our meeting on sump screens. We have an extra presentation early this morning about downstream -- possible downstream effects on fuel, or the way that fuel bundles are organized, the shapes, and the passageways, and all that sort of thing.

Mike Scott is going to make the introduction.

MR. SCOTT: Good morning. What we'd like to do is shed a little additional light, both from the NRC staff and from the industry, regarding the question of what happens with debris going downstream from strainers, especially in consideration of the fact that the staff is pushing the industry to enlarge its strainers.

So, we'll start off with Tom Hafera, of the NRR staff. Tom, unfortunately, got snowed in up in Pennsylvania by this blizzard we got over the weekend, so he couldn't be here personally, but he is on the phone.

And so, Tom, if you hear me okay, if you'd please share perspective with us on that issue I'd appreciate it.

MR. HAFERA: Okay, Mike, thank you.

Yes, the staff has been working with the owners group contacts, reviewing the WCAP, regarding downstream facts, and the WCAP does address, it has a chapter and appendix specifically related to downstream effects in the reactor vessel.

We also had a contractor help us develop staff guidance, and we issue a draft paper for how the staff is going to review the issues associated with downstream effects in -- and we have a tendency to say the fuel, but it's actually the vessel. You take vessel as a whole.

So, we've done a lot of work ahead, but at the same time we still have some issues associated with the WCAP. We've identified those, and we have some ongoing discussion with those.

Critical things to keep in mind with downstream effects in the reactor vessel, not only do you have to identify your source term, in terms of how much debris is going to penetrate through the screen, whatever the new screen design is, but each -- most reactors have, and particularly all PWRs are open course, and most reactor designs have openings outside the fuel assembly region that provide significant openings that will provide flow, or at least maybe not necessarily flow, but at least a path for the water to go and get to the fuel and cool the fuel through emersion.

Distinct issues -- but also, you know, other issues are, if you -- hot leg breaks and cold leg breaks are going to behave significantly differently, because your low pressure point in the system, in a cold leg break, is at the inlet side of the core, essentially, and on the hot leg break your low pressure point is at the outlet side of the core. So, for evaluating effects for a cold leg break, the flows at the bottom of the vessel are very low, you are going to get probably a high degree of settlement, if not complete settlement, but at the same time you won't compact any debris, so you probably won't create any significant head loss there.

For hot legs, on the other hand, you are going to have high flow through the core, the debris will all pass through -- either pass through the core or the bypass paths, and then go back to the containment floor where it has an opportunity again to either settle or be captured.

If you think dynamically in terms of hot leg breaks, the best example I can think of is I hand drew an electrical circuit modeling the flow paths, so you end up with very multiple flow paths and multiple resistances through the system. You use your fundamental centrifugal pump curve with a system head loss curve, you recognize that if you increase the resistance at the bottom of the core to infinity, all that essentially does will create flow, increased flow through the other core paths, and probably the most significant one is you can -- if you are injecting cold leg into the cold leg, and you increase the resistance on the core inlet to infinity, it will just back up flow backwards through the loop, and it will end up dumping into the hot leg onto the top of the core, and, therefore, you will still keep the core under water and it will probably cool.

Now, there are some questions in regard to, you know, how does debris interact in high boric acid concentration, and boron precipitation, and operators -- there's operator actions associated with flushing the core out periodically, so that could also be effective in flushing debris out, obviously. So, there are a number of other issues, and we've discussed these with the WOG, well, we've identified these with the WOG, and there's ongoing discussion.

So, right now our position is, there are some issues with downstream effects, and how does the particulates and fibers interact in the vessel, but at the same time we don't see it -- we haven't identified anything that would want us to stop putting in larger strainers. We don't necessarily feel that this would -- because you are on the discharge side of the centrifugal pump, and taking centrifugal pump theory and plastic fluid mechanics, it really doesn't appear to be something that would cause a catastrophic failure.

And, I think one of the best examples I heard of was, one of the people I'm working for, or working with, with Reactor Systems, explained to me, you know, the TMI core melted and relocated and then resolidified, so it had areas of complete blockage. But, at the same time when they put water back in there it cooled, and it found a way to cool itself. So, it's a pretty gross -- pretty -- you don't necessarily need a real highly technical or exact method to get this water in there, the water will find a way.

And so, we are working on that, and we think we've got -- we think we've got a path of success. Even as late as last week, I discussed this with Mo Dingler and Tim Andreychek, we felt that the fuels people -- and I also discussed this a little bit with the Framatome guys last week, the fuels people believe we've identified those areas that need to be investigated a little farther, and we think we've got a path going forward to give us the answers that we need.

MR. SCOTT: A point to emphasize here is that the staff's expectation is that the industry in evaluating its modifications to increase the size of the strainers will consider downstream effects, such as, and including, those related to inside the vessel and the core. So, nobody puts in a MOD without having done that analysis.

CHAIRMAN WALLIS: Can I ask a question?

You mentioned the head from the centrifugal pumps, now are there some locations of the break where the actual driving head for flow through the core is not from the pumps, it's from natural conflection of the head in the downcomer or something, because of where the break is? The pump doesn't necessarily always pump through the core.

MR. HAFERA: That's correct, Dr. Wallis. Again, that was as I explained, the difference between a cold leg break and a hot leg break, and you have to recognize that the fluid dynamic differences in a cold leg break, the majority of your flow will, essentially, bypass the core and go back out around the downcomer or annulus region and out the break.

However, you know, the flow is still -- still goes into the -- there's an amount that will go down the downcomer into the -- and then back up through the core based on gravity and the differential water levels. And again, if you think of it in terms of an electrical circuit, and you say to yourself, okay, that's my flow characteristic with no debris whatsoever, and I say to myself, okay, now I have a resistance at my bottom core plate, or my bottom fuel screen, if I take that resistance and change it to infinity what does that do to the entire system. Well, it creates a higher resistance at the bottom of the core, it causes back pressure into your cold leg injection that will, essentially, force flow backwards through the reactor coolant pump through the steam generators, into the hot leg, and then dump onto the top of the core.

And, your pumps, your pump discharged head can easily overcome that, because that's only about a 60 foot head. So, there's really not -- and again, if you block the bottom of the core you still have alternate flow paths, in particular, the B&W designs have flow holes and slots, the Westinghouse designs have flow holes through the baffle, and the shield -- the thermal shield area, that still provide large, very large, in relation to the screen hole size, flow paths to get water into the core region.

CHAIRMAN WALLIS: It looks to me as if we really need a subcommittee meeting on this subject, to clarify all these issues. You talked about complete settlement at the bottom of the vessel, well, there may be enough debris to fill that area, that volume, and if you are going to then rely on dumping on top of the core, I'm not quite sure how it works out when you have debris-laden material dumped on top of the core.

You may well be right that everything is fine, but I think we need to have a proper technical discussion of it.

MR. HAFERA: Bill, don't characterize as everything may be fine, I would much more -- I would rather characterize it as, it has to be analyzed based on the screen design chosen by the utility and the vendor.

You are correct, depending upon the screen design, depending upon the amount of debris, that has to be evaluated, in terms of how much debris is actually going to build up in the bottom of the vessel, or how much debris will actually build up on top of the core. And then, once that is evaluated, and the WCAP has that methodology in it how to do that evaluation, and then once that evaluation is done most likely, the most likely success path then is to look at when does it occur, in terms of probably, you know, it's going to be hours most likely, it's going to be on the order of hours after the onset of recirculation, and if you say to yourself, well, just as an example, I say three hours in, three hours after the onset of recirculation, I reach a point where my debris load is approaching something that I consider, you know, not good, well then I have to initiate hot leg recirculation faster, which all plants have as part of their emergency operating procedures for boron precipitation control.

So again, to characterize is to say it's okay, well, I wouldn't go that far. I would say it needs to be evaluated. It needs to be evaluated using good engineering judgment, and it needs to be robust, and most likely it will -- it may, it may result in changes to your emergency procedures in terms of, you may change from, say, a plant may right now under existing circumstances go to hot leg recirculation eight hours after recirculation for boron precipitation control. Well, they may need to change that to five hours. They may need to change that to three hours. But again, for them to find that out they are going to have to do a robust evaluation, and that has to be looked at, and it's part of the factors that need to be considered are, what's the screen design chosen, what's the amount of debris that's going to bypass, what are the characteristics of the debris that are going to bypass, and what are the other system interactions associated with downstream effects, and how does that all fit in.

CHAIRMAN WALLIS: I think we probably have to move on, but I do have a comment. I find it difficult for this subcommittee to reach any conclusion about whether or not you are on a success path when we have to wait for evaluations of every possible incident in every possible reactor with every possible change to the screen. This is really rather difficult for us to get hold of.

MR. HAFERA: That is correct also, and that's why -- and our approach has been, because a lot of that is -- and it goes even -- the next step even goes farther, and that is, a lot of that is plant specific. So, you really cannot, from a generic standpoint, evaluate it, and say, yes, this is going to be okay, or this is not going to be okay. The only thing you can do from a generic standpoint is outline the methodology that you are going to use, what the critical parameters are that you need to identify and include in your evaluation, to make sure that that methodology is done the right way and is robust.

And, essentially, that's what the WCAP at least aims to do. At this point, we are not sure it does or doesn't do that, but that's where our discussions with the owners group and the fuel vendors and people are headed, because we recognize from our perspective it's more advantageous for us to get a single approved methodology that then all the plants could use, rather than trying to evaluate 60 million individual plant-specific configurations.

DR. DENNING: You know I hear that, but I think that there's no reason why you couldn't take an example. I mean, we heard yesterday or the day before about active screen designs in which it sounds like there's a very large fraction of fiber that's going to pass through the screens. I'd like to see an example of the cold leg break and have somebody run through and tell us how much material is really passing through that system, and see an analysis.

I don't think that we're going to feel

comfortable until we see that analysis and get a chance to get into the details of the assumptions, like the entrainment of fibers or particulate in the upper plenum if you stagnate up there, and some indication of how much mass we are really talking about winding up in the system.

So, I hear you saying you can't do a generic analysis, but we certainly could see some example analyses in the area -- in the regimes in which we think there could be the most problems.

MR. HAFERA: Yes, I agree. I agree that would be a good thing, and I think we are headed in that direction, we are just not there yet, because most plants have not completed their downstream effects analysis as of yet.

MR. SCOTT: If I could, I'd like to ask Mo Dingler to show you a couple of slides that he's brought in to illustrate --

CHAIRMAN WALLIS: Can we thank Tom first?

MR. SCOTT: Certainly.

CHAIRMAN WALLIS: All right, thank you, Tom.

MR. SCOTT: Tom, if you'd stand by, we may have additional questions for you.

MR. HAFERA: Sure.

MR. DINGLER: I'm Mo Dingler from Willow Creek and represent the owners group also.

There is some discussion last couple days of what a fuel assembly was, and the bottom of the grid, so we wanted to show, and we worked these up late last night, so it will be the first, and Tom will do this, we are still working through some of the issues, so I'll give you a high level here.

We wanted to show you what the core looks like, the bottom. This is where most of the flow will come up through.

CHAIRMAN WALLIS: So, it's coming in from the bottom, and it's going through something called a protective grid.

MR. DINGLER: That's what I call the P grids.

CHAIRMAN WALLIS: Which looks something like a screen.

MR. DINGLER: Right, and these openings are about the size -- the same size we talked Wednesday about.

CHAIRMAN WALLIS: And, if that screen gets blocked we are told the flow will go somewhere else?

MR. DINGLER: This is a diagram of some of the alternate flow paths.

CHAIRMAN WALLIS: But, in normal operation, not much flow goes that way. It's designed to go through the protective grid, presumably.

MR. DINGLER: There, but there is still even during ops some of this.

CHAIRMAN WALLIS: There is some of it, but it's not the main flow path.

MR. DINGLER: The main flow path comes down the cold leg, up through this way, and here is the hot leg.

So, when these -- a lot of plants have these openings, the B&W, the upflow plants have the openings right here.

CHAIRMAN WALLIS: Now, we have experiments on how rod bundles behave in the normal operation. I don't know if we have experiments on how they behave when the main flow path is blocked and the flow is coming in through these other passageways.

MR. DINGLER: I can't speak for that, Dr. Wallis.

MR. CARUSO: Mo, you are not showing how the flow that goes up the side of the baffles gets into the core. It comes up through the --

MR. DINGLER: Right here.

MR. CARUSO: -- through the flow nozzles

themselves, into the core which is open to the side.

MR. DINGLER: The core is right here, so in other words you've got some of it going through the baffle barrels and that is right through in here.

So, you have openings all the way up through here.

CHAIRMAN WALLIS: So, it comes in from the outside and has to somehow get to the middle of the core?

MR. DINGLER: Correct. It comes up, here's the core plate, it comes up and there's the protective grids are right here for normal ops to keep threading down --

CHAIRMAN WALLIS: Right.

MR. DINGLER: -- and then we've got the flows that will come parallel and up through this.

MR. CARUSO: So, you are saying -- you are talking about bypass flow between the bottom bracket on each fuel assembly. The assemblies sit in a grid, and you are saying that between two assemblies on those flat faces on each side, okay, the bottom nozzle, excuse me --

MR. DINGLER: On this is the bottom nozzle.

MR. CARUSO: -- on that bottom nozzle

there's another one right next to it, and there's this tiny little gap between the two of those, and you are saying that there's bypass flow up through that gap.

MR. DINGLER: There is some, but what I'm showing --

CHAIRMAN WALLIS: What you are showing is a bigger flow.

MR. DINGLER: -- is much more flow, I'm saying the core is right here, and assume it's all blocked, the flow will come up and it will go out here in the baffle barrels and through openings.

CHAIRMAN WALLIS: It will come up, essentially, on the outside of the core, around the core.

MR. DINGLER: That's the outside of the core.

CHAIRMAN WALLIS: And, somehow it has to percolate into the middle.

MR. DINGLER: That's correct.

CHAIRMAN WALLIS: Someone has to analyze that kind of a situation, presumably.

MR. DINGLER: That's correct.

MR. ANDREYCHEK: If I may, this is Tim Andreychek from Westinghouse. There's typically an angular gap on the order of an order of an inch, an inch and a half, for the upflow plants that provides a flow path.

Under normal operating conditions, that provides cooling flow to keep the baffle barrel region at acceptable temperatures. It provides a fairly large flow area in that circumferential gap to provide flow.

There are, I want to call them pressure relief holes up the height of the baffle barrel at certain locations, that provides for flow to go back into the core, should the bottom nozzles become blocked.

DR. BANERJEE: How big are these holes?

MR. ANDREYCHEK: They are on the order of approximately two inches.

DR. BANERJEE: And, the barrel?

MR. ANDREYCHEK: The baffle barrel gap is about an inch and a half angular gap that runs around the periphery.

MR. DINGLER: What we are saying is, we are still working with the staff now.

CHAIRMAN WALLIS: You are showing me a figure, and you are saying that's probably what happens, but I don't see any kind of code calculation of what happens.

MR. DINGLER: And, I wasn't -- between 6:30 last night and 10:00 --

CHAIRMAN WALLIS: Yes, but it seems to me something like that might be needed. I mean, if you are going to really technically convince us, you might have to have a --

MR. DINGLER: We are not arguing that.

CHAIRMAN WALLIS: -- whatever your appropriate code is that you are going to use for this.

MR. DINGLER: Yes, we are not arguing, I'm not here to present all the detail technically, late last night --

CHAIRMAN WALLIS: See, it's not good enough to just talk about the problem.

I agree that there could be some flow that way, but we need to know how much it is, and whether it's enough, and so on.

MR. CARUSO: Can I ask a question?

CHAIRMAN WALLIS: Yes, Ralph.

MR. CARUSO: If you have holes that communicate from the bottom of the lower plenum up through the baffles, the baffle region, and then into the core, that are on the order of an inch, an inch and a half in size, why do you bother installing debris screens on the bottom of the fuel, okay, to try to catch debris during normal operation to avoid having it come up into the core region? If you already have these other inch and a half holes all over the place that provide lots of bypass flow into the core region, why do you need debris screens on the bottom of the fuel bundles?

MR. DINGLER: I guess to simply it a,s the head loss up this way is much lesser than through here, so the flow goes up through -- most of the flow goes up through here at this point, during normal ops.

DR. BANERJEE: I guess Ralph's question is, if you get this cross flow, and it brings debris in, it will just deposit it in an outer ring, and the inner fuel --

MR. CARUSO: The reason why those debris screens are there is because people don't like to have debris get on their fuel during normal operation, and cause damage to the fuel during normal operation.

So, the vendors have been very good at designing these debris screens to trap all that debris, to make sure that it doesn't get up into the fuel during normal operation. It would defeat the purpose of installing those debris screens to have a parallel flow path, with holes an inch, an inch and a half in diameter, that allow debris to flow up around into the baffle region, and then get into the core from the side. That defeats the entire purpose of installing debris screens.

MR. ANDREYCHEK: The debris path is a torturous path, and you are talking about 98 percent or so of the flow going up through the core. That's, basically, a leak flow for normal operating conditions to keep the baffle barrel region cool.

And, the reason the holes are there is to provide, during an upset condition where you depressurize the reactor quickly, pressure release yield implode or explode the baffle barrel region. You know, it's not a main flow path under normal operating conditions.

CHAIRMAN WALLIS: So, I think what you are saying is that you need much less flow at this time after a LOCA when you want to cool the core and maintain a normal operation. So, having what used to be a 2 percent flow path now becomes an adequate flow path.

But again, we'd have to see that. We'd have to see --

DR. BANERJEE: Except now the fuel acts as the debris screen in cross flow, and the fuel will take out the debris in the outer region.

CHAIRMAN WALLIS: So, what happens to the middle of the core?

I don't know, if you are going to cool it from the top, I don't know what CCFL looks like when you have debris in there. So, there's a lot of interesting technical questions here.

MR. DINGLER: We understand that.

CHAIRMAN WALLIS: You've indicated that there may be ways to cool the core, which may be perfectly satisfactory, but these are all maybes at this point.

MR. DINGLER: Based on the investigation we have done so far, some plants will have more challenges. As you heard, with the active sump screen, some have very -- have very little.

CHAIRMAN WALLIS: So, is there going to be a WCAP or something that addresses these in technical terms, rather than just sort of saying you've got to calculate all these things, but actually gives a lot of example calculations?

MR. DINGLER: As Tom says, we are working with the staff, and one section gives a generic evaluation for it, this is -- you had a lot of discussion on proprietary information, just on the normal between the two fuel vendors that we have, proprietary becomes very commercialized, and we are working through some of those issues.

CHAIRMAN WALLIS: I think that's for existing plants, how about something like the AP 1000, are we going to have to revisit that and have calculations of this kind of way of cooling the core?

MR. SCOTT: Yes, this is unresolved for AP 1000.

MR. DINGLER: I think that should be addressed with the AP 1000 folks, I'm not prepared to answer that.

CHAIRMAN WALLIS: You have a new reactor, so you can make the calculation for that, it's not going to be plant specific, or is it going to be, again, plant specific, because the debris is plant specific, even in an AP 1000.

MR. DINGLER: I can't answer that, because I don't even worry about an AP 1000, I'm worried about existing plants.

CHAIRMAN WALLIS: Some people must be worrying -- must be thinking, nobody ever worries in this business, thinking about it.

MR. DINGLER: Thinking about it, I'm not even thinking about AP 1000 right now.

The other one is, I guess the next slide is --

CHAIRMAN WALLIS: Are we going to have these slides available to us, or are they proprietary?

MR. DINGLER: You've got them right in front of you.

CHAIRMAN WALLIS: We have them somewhere, oh.

MR. DINGLER: Yes.

The next one was just to show that there's plants that have more bypass, others are going to have more challenges.

And, as Tom said, in other words, dependent on that I wanted to give you a perception, the last slide is the free volume of lower plenum. It's based on reactor vessel design and reactor vessel size, really what we should say is, we've got between 300 to 375 cubic feet of free space down there to settle. In other words, I'm not here to say that all -- I'm just giving you a perspective that the lower plenum can act as a source of debris collection, and give you a sense of the volume. That's all I'm here to say.

CHAIRMAN WALLIS: That's useful, thank you. That's good.

MR. SCOTT: Is that it?

MR. DINGLER: That's all I have, yes.

CHAIRMAN WALLIS: We have to compare it with the numbers we've heard already, about the amount of debris, don't we?

MR. DINGLER: That's correct.

CHAIRMAN WALLIS: And, we are going to hear in the next presentation about bypass -- screen bypass?

MR. SCOTT: The next one is throttle valve clogging.

CHAIRMAN WALLIS: The throttle valve clogging study is really about screen bypass so far, or is it going to talk about -- we'll find out.

MR. SCOTT: Before we go on, Tom Hafera is still on the phone, do you all have any other questions for Tom?

MR. HAFERA: Hey, Mike?

MR. SCOTT: Yes.

MR. HAFERA: I guess I'd like to just add one other thing before I answer questions, and that is, listening to some of that discussion, I think it sounds like some critical ideas were identified, but things that have to be recognized, you know, you can do a quick back-of-the-envelope calculation, Q is equal to M dot delta H, and you can determine what type of flow rates are needed, you know, recirculation doesn't happen until at least 30 minutes after reactor trip. So, if you take a 3,000 megawatt core, and you use 30 minute decy heat, and your delta H is determined by your sump pool temperature, and saturation conditions for containment pressure, you can do a quick calculation and you find out that you can handle decy heat at that time by about 200 gallons per minute. So, it is, it's a very small amount of water that needs to be added to the core to maintain, you know, to remove decy heat.

The other thing to recognize, and I heard some discussion, you know, you talk about differences between normal operation and post LOCA, you know, normal operation your reactor coolant pumps are running, they are 90,000 gallon per minute pumps, so you are pumping a lot of water through the core, very high velocity, that's what your fuel screens are for.

Post LOCA, your fuel, you know, your flows are extremely low, and again, if you talk about a cold leg break, where, essentially, you are cooling the core via natural circulation, you actually have -- you'll have a circular flow within the core, in terms of, you'll have upflow in the center, and you've actually have downflow on the outside, and what that -- you have to keep that in mind, too, because that prevents you from building up the debris bed, because you have eddies and flows that really mean you don't get a consistent -- you know, you don't have like laminar flow where your flow vectors are all in one direction. It just doesn't happen that way.

MR. SCOTT: Yes, I think the committee, although they are certainly interested in the conceptual -- the concepts like you are talking about, they want to see the results of the analyses.

DR. KRESS: We want to see some trace calculations.

DR. BANERJEE: Not trace, how does trace get those eddies that you are talking about?

MR. HAFERA: I'm sorry, Dr. Wallis, I couldn't hear you.

DR. KRESS: Dr. Wallis is no longer with us, this is -- he couldn't take anymore -- this is the Co-Chairman, Dr. Kress, talking.

Never mind what I said.

MR. SCOTT: Do you all have any questions for Tom?

DR. KRESS: No.

MR. SCOTT: Okay.

Tom, we are going to go ahead and go into the research presentations. If you want to continue to listen in, you are welcome to.

MR. HAFERA: Okay.

MR. SCOTT: And, thank you very much for phoning in.

MR. HAFERA: Thank you.

DR. DENNING: WE ought to really thank Mo, too, for being brave enough to --

DR. KRESS: Mo, thank you, we appreciate it.

So, Rob, you are up next.

MR. TREGONING: Yes, this is back on the original schedule.

The next presentation is going to be on a test program to look at throttle valve clogging, but as you are going to see it's more of a surrogate for clogging in general under certain flow conditions, restricted flow conditions within -- that may occur in some places within the ECCS system.

As co-presenters, myself and Bruce Letellier from Los Alamos. The objective of this work on slide two was to evaluate the effect of insulation debris on blockage. We wanted to look at where, to some extent, but it was really just a more qualitative assessment of where, there was no quantitative or no rigorous study of where that was applied during this project. And, we wanted to evaluate these effects on surrogate HPSI throttle valves.

Motivation, we talked a little bit about this yesterday, the throttle valves are one possible source of performance degradation, due to flow restrictions. I think as Bruce mentioned, it's one of the smaller clearance areas, typically, within the ECCS system. So, it's a potential trap for debris.

Also, prior to this program there was really little information on the severity of nuclear valve degradation due to these blockage phenomena.

This is a two-phase effort, as Dr. -- as Professor Wallis mentioned. Phase I, which we weren't prepared to discuss in depth today, although we can certainly field questions and bring that into the discussion, was a scoping study to examine the variables that affect the amount of debris which can pass through the sump screen or sump strainer screens. That's a bit of an alliteration I'm not ready to handle yet this morning. That work is being completed, and it's documented in NUREG/CR-6885, so that was Phase I of this study.

And then, what was done is based on some of the ingested debris characteristics in Phase I that were measured and observed. Those same characteristics were then ingested into a closed loop to measure blockage within the throttle valves, and that's Phase II of the program.

The presentation today is going to focus primarily on Phase II. We did provide a brief overview on this topic in the July meeting, but today is obviously going to be much more extensive, in that most of the testing, in fact, all of the testing, has been completed.

The regulatory use I can go through quickly. Again, this is to aid in the staff's evaluation of the generic letter responses, and, specifically, we are hoping to use the data to at least determine variables that are important to consider when determining if ingested debris is a problem, and it may lead to blockage in some of these regions downstream of the ECCS strainers.

DR. KRESS: Now, that particular throttle valve, it's totally blocked, there's two of those, right? It depends on which sed loop you are in?

MR. TREGONING: At least.

DR. KRESS: At least.

If they all get blocked, then you

completely block the ECCS flow to the core in all the units?

MR. LETELLIER: Under high pressure injection, you may, but there are multiple injection paths, and we talked a little bit about that.

DR. KRESS: But, each ingestion path has a valve like this of some kind in it.

MR. LETELLIER: Yes.

DR. KRESS: So, each of those valves might possibly get blocked.

MR. TREGONING: It is conceivable, certainly, although blockage is one of the things we didn't investigate in this project. We just were looking for the onset of blockage. We didn't evaluate any, you know, post blockage operator action that could be taken, potentially, to alleviate the blockage concerns, you know, cycling the valve or things like that.

I mean, there may be some things that the operator could do that could clear it out, yes. We didn't look at that in this program at all.

Quickly, status, before I turn it over to Bruce. The testing is complete. We have an initial NUREG/CR that we are preparing for publication. We are still reviewing it. We are planning to have a final version ready for publication in March. We'll be sending it to Ralph some time in the near future for your review as well.

And, with that, I'd like to turn it over to Bruce, and he's going to go into more of the technical details associated with the testing and the results.

MR. LETELLIER: Very briefly, I'd like to acknowledge the team members at Los Alamos, Crystal Dale and Pratap Sanisdavan, and also our graduate student at the University of New Mexico, Felix Carles, who did most of the real work.

You might say I'm just the pretty face, but then I know things will vary.

The questions that you asked, Dr. Kress, about the operation of the HPSI system are very relevant. Rob explained a couple of the reasons that we chose the HPSI throttle valve gap in order to examine the phenomenology of potential blockage.

One is simply that it's one of the smallest identified internal gaps that you can find, but on the other hand it also has some of the higher velocities. And so, if we are able to observe onset of blockage under those conditions, then, perhaps, there's motivation to look further into other areas of the core, like the fuel channels and the spray nozzles, et cetera.

But, as you said, there are multiple injection paths, and I'm not -- I'm not the systems expert, but there are at least four to sometimes 12 of these paths, depending on how many levels of pressure systems that the plant has in their ECCS cooling.

So, I would say four is the minimum. There are other plants that have fewer than that, perhaps. There are plants that have as few as two injection paths.

The purpose of these valves is twofold. They are, basically, there to balance the flow into the core, but also to throttle the high pressure pumps, so that they don't experience a run-out condition during the accident.

Just a few other background numbers, there are no set specifications for what constitutes a throttle valve. The designs and applications vary. They are typically globe valve type of designs, between one and four inches in diameter, with two inches being the most common. So, you see a diversity of industrial products that are applied for this function.

And, that made it challenging for us, how to initiate this project. We had a couple of options. We refer to the figures now.

Option number one was simply to procure and test multiple industrial valves, but that's problematic because it would require repeated calibration of the system, and these valve bodies are impossible to inspect. It would be very difficult. It does have the advantage of exactly matching inservice equipment.

We chose the second option, actually, which was to fabricate a surrogate valve chamber with a flexible geometry, where we could swap out the internal details of the flow path and also give us a chance to inspect and to clean the valve between tests.

Our objective there was to match the nominal dimensions of the valve, and the tolerance, and also the flow path complexities, but not necessarily to endorse any single industrial product.

The pump selection that we used for our study was intended to have a HPSI-type design, and be capable of matching the initial delta P and flow conditions of the LOCA transient.

Obviously, at an academic setting, we are not able to match the total delta P capacity of the

HPSI, but we want to be on the initiation condition of the transient. And, one of the objectives was to give us some -- at least some margin for increasing pressure and decreasing flow in the event of blockage.

If you think about what would happen in the plant as the throttle valve would block, if it was a very severe blockage the HPSI pumps would continue to push up to the full system pressure.

DR. KRESS: Now, you are going to design to match the flow in delta P.

MR. LETELLIER: Initial delta P.

DR. KRESS: Yes, but you are not going to match the actual pressure.

MR. LETELLIER: That's correct, not the absolute pressure.

DR. KRESS: Is that a problem or not?

MR. LETELLIER: I don't think so. We made that judgment very early, that the driving force for lodging, and clearing, and extruding debris would be based on the differential pressure across the valve.

DR. KRESS: I was thinking that the characteristics of the blockage may depend on the pressure, if it blocks.

MR. LETELLIER: They may. We could discuss that in detail.

As we'll see, we were able to identify some evidence of the onset of blockage and the velocities with which it approaches the valve, and the randomness of the orientation, its relative sizes are all very important.

MR. TREGONING: Can you give an example of where you think the absolute pressure might be important with respect to the characteristics?

DR. KRESS: It might have something to do with the nature of the debris. It may have different characteristics.

MR. TREGONING: So, it might change the morphology of the debris.

DR. KRESS: The morphology.

MR. TREGONING: Okay, understand.

MR. LETELLIER: The local forces on the debris are only a function of its surrounding differential, so that's the reason that we pursued the path.

For debris types, we chose the three usual suspects, our reflective metallic fragments, fiberglass shreds, and calcium silicate.

The sizes that we tested in the surrogate valve were those sizes proven to pass through prototypical sump screen configurations, and that was

Phase I of the project.

DR. BANERJEE: No NUKON?

MR. LETELLIER: That is the fiberglass.

DR. BANERJEE: Oh.

MR. LETELLIER: NUKON is a trade name, common trade name, for fiber insulation.

The sizes, we did have some quantitative information for, however, the quantities and the rates could only be studied parametrically. That's some of the issues regarding the current or future sump screen designs, and what the fraction of debris that might pass through.

And, it also -- it requires some estimate of debris transport and generation in the vicinity of the break.

So, we studied it parametrically. It was very much an exploratory effort to look at what phenomena might exist that leads to the onset of blockage and, perhaps, accumulation.

DR. KRESS: So, when you say parametrically, I would have envisioned that if you keep increasing quantity and rate being produced in blockage, and you say that's the -- you don't have that -- is that the kind of thing you were thinking of?

MR. LETELLIER: Well, you'll see, we were never actually successful at completely blocking the valve. In fact, the limiting factor was the size of our two-inch lines, and how much debris we could physically pack into the introduction chamber. You'll see a schematic in just a moment.

MR. TREGONING: But, you are correct, we were looking for conditions that would lead to at least partial blockage. So, the initial test plan, the very few tests we did initially that looked at very small quantities of RMI debris, we saw quickly that we needed to up those quantities in order to get blockage in many conditions, and we modified the test matrix accordingly at that point.

MR. LETELLIER: Next slide, please.

I think you'll see that some of the debris charges that we passed through the valve would not be considered prototypical. They were very highly concentrated slugs of material.

DR. KRESS: Did you introduce this mixture of debris all at the same time?

MR. LETELLIER: We will talk about the matrix, but the answer is yes, we studied various combinations and various quantities of the three debris types.

So, this next slide shows a schematic of the apparatus. The business end is the high pressure pump on the floor, down to the lower right. The surrogate valve body is elevated about five feet off the floor, and this debris insertion manifold is the combination of valves that let's us introduce debris under pressurized flow. So, there's a combination of valves we'll look at in detail, so that we can switch them over and add debris to the flowing system.

DR. KRESS: Is it a positive displacement pump?

MR. LETELLIER: It is actually an impeller type pump, it's an eight-stage impeller. We could look at the performance curves, but it basically has a peak pressure of about 485 psi, and a --

DR. KRESS: I was wondering what that did to the debris? You were introducing it --

MR. LETELLIER: Downstream of the pump.

DR. KRESS: Downstream.

MR. LETELLIER: At this point here.

DR. KRESS: Okay.

DR. BANERJEE: I guess there's a settling tank, right?

MR. LETELLIER: The large flow is a reservoir of water, and you are correct, it provides an opportunity for settling. We have debris capture at several points, at a trap in the flowing pipe, there would typically be buckets with sieves.

We are trying to achieve a mass balance. We know how much we put in, and we'd like to know where the remainder resides.

There are also screens, mid-flow screens, within the flume, and also a fine-mesh screen just upstream of the pump.

DR. KRESS: So, you are not trying to recirculate stuff that might get --

MR. LETELLIER: That's correct. We have talked about looking at the effects of debris ingestion on the test pump, but our immediate objective was to allow it to survive the test campaign.

DR. KRESS: Yes, that's a good idea.

MR. LETELLIER: That is, obviously, the most expensive piece of the system.

Diagnostics, we included flow meters just upstream of the pump at this point, thermocouples upstream of the pump, and also downstream of the valve, just to monitor -- well, the reason we have two is to monitor the differential temperature across the test apparatus, and, in fact, the pump does consistently heat the flow about ten degrees between the inlet and the outlet, at flow rates of 75 gpm.

The primary measurement of interest are the differential pressures across the valve body at these locations.

So, let's look at a photograph.

MR. TREGONING: Do you want to mention the downstream flow meter at all, just as a potential -- another potential trap?

MR. LETELLIER: The one we took out?

MR. TREGONING: Yes.

MR. LETELLIER: Initially, we actually had our flow meter downstream of the valve in this long section here. We were mostly concerned about the hydraulic flow regime, so we could get a good consistent measurement.

However, it would consistently foul with the debris. These were impeller -- or turbine type flow meters, with a triangular strut arrangement.

And again, the debris loadings are large slugs of concentrated debris. Had we fed it in a more dilute quantity, perhaps, it wouldn't have been an issue, but we did have evidence of trapping on sharp-edged obstructions.

So, the photograph makes this a little

more realistic. The pump is again on the floor, with the debris introduction manifold at this level, and the valve body is the aluminum milled -- the insets provide more detail of the valve body.

You can see the direction of the flow path is an under/over type arrangement, it comes into the lower cavity, passes through the throat, and out through the outlet.

At this point, you can see the manifold with the four separate valves that let us control -- isolate the flow, so that we could introduce debris to the top, so all the pressurized flow continues in the bottom. And, after we've introduced the desired quantity and types, then we can valve it over and flush the lines.

All of these tests were conducted when we introduced the debris, both valves, 1, 3 and 4, were completely open, so we had a parallel flow path during the test, so there was no possibility to trap debris in cavities.

The next slide shows the internal details of the surrogate valve. Again, you can clearly see the under/over flow path, with the lower chamber. The valve seat, this valve is shown in the fully closed position, so the points of contact are here at the ring and the stem.

We designed this valve body to be able to put in different combinations of the ring and the stem, and the three configurations we tested are shown to the left. We have a shorthand nomenclature. At the top is the five degree small, which describes the contact angle of five degrees from the vertical and the diameter, which is smaller than the large one.

MR. TREGONING: It's a complex nomenclature system that was developed for this testing.

MR. LETELLIER: I'm sorry, I'd have to look in the report to say exactly what the diameter is. We chose these configurations based on a survey of commonly available industrial globe valves, and again, because there's no set specification the plants might employ any variety of these.

MR. CARUSO: Does the NUREG document explain the basis for picking these particular ones? I know it's based on the survey, but, I mean, somewhere you looked at how many are of this type and how many are this type, and said, well, because most of them are this type this is what we are going to test?

MR. LETELLIER: The explanation is very

brief. At the onset of this study, we had informal discussions with the industry at various workshop settings. We asked for the staff's advice on specific information. And, in the end we simply looked in the catalogs. We looked at what was available for these flow rates and delta P applications.

MR. CARUSO: And, some one person made a decision that this is the typical -- typical configuration?

MR. TREGONING: Well again, typical is probably too strong a word. What we tried to do is get something that was within the ballpark, essentially, of the types of contact areas and stem dimensions that would be --

MR. CARUSO: Who made that decision, was that research or was that you guys?

MR. LETELLIER: It was primarily our recommendation on the contractor, on LANL's side, and it obviously has been reviewed by the staff.

We don't have any contradictory evidence to cause us to change our choices at the moment.

As we get into the test metrics, you'll see that our characterization of the valve will be very familiar to the plan engineer, and they will be able to look and see, what are my valve loss coefficients? Does this make sense to me?

DR. KRESS: When you change configurations, you change both the stem and the seat, is that correct?

MR. LETELLIER: That's right. They are made in pairs.

DR. KRESS: Those are the only two things you change.

MR. LETELLIER: That's right.

This configuration gives us a couple of choices for inspection. You can take off the lower plate, without removing the throat, to actually change out the internals we take off the top head and remove the flow path.

There is --

DR. SHACK: There was some discussion yesterday that these valves are set to balance flow. How did you choose your valve openings?

MR. LETELLIER: That is true, to my present level of understanding, that some plants balance and lock these valves in position. Other plans can actually actuate them remotely, or manually, during the accident sequence.

We chose our flow gaps, we chose a range of flow gaps, in order to achieve the delta P and flow that we wanted to have.

DR. BANERJEE: Is this flow gap the critical thing, or is it the whole combination of up stream chamber, where you might get vertical motions and things?

MR. LETELLIER: There is clear evidence in the data that we look at that it is a combination. It's an integrated system.

Obviously, the gap, as measured perpendicularly to the ring in this manner, that controls the flow path area, but the details of the internal flow dictate how much the debris is entrained and how much will reside in the cavity.

DR. BANERJEE: Good.

Now, those cavities are typical?

MR. LETELLIER: Yes, to the best of our ability. We tried to match the adjacent volumes and the flow path complexity.

There is a typographical error, you want to change this annotation to 45 large at the bottom, and it simply means that the diameter of the two large stems are identical, but the angles are not.

DR. KRESS: Now the active -- through the gap you go to was a -- and an annulus for going to the outlet, and is the designs in the mentioned event critical?

MR. LETELLIER: On the outlet side?

DR. KRESS: Yes, on the outlet side, on Figure 2, the annulus.

MR. LETELLIER: Through the upper chamber?

DR. KRESS: Yes.

MR. LETELLIER: One of the problems with testing, studying this type of phenomena is, it's very difficult to determine exactly where the debris may have been lodged. We can test delta P flow across the system, but once we take it apart it may have moved, whether it was initially trapped in one location or another.

To my knowledge, any of the debris that was recovered was from the lower chamber, and once it had passed through the gap it was easily flushed out.

There may have been small quantities lodged in the upper cavity as well.

MR. CARUSO: Did you get any debris in that upper balance chamber at all?

MR. LETELLIER: That was the question that Dr. Kress was just asking.

MR. CARUSO: Oh, I'm sorry.

MR. LETELLIER: That's what he just asked.

MR. CARUSO: I thought he was talking

about the outlet.

DR. BANERJEE: Do you do some calculations of the streamlines and the fluid dynamics of this valve before you happily put stuff into it?

MR. LETELLIER: No, we have not.

DR. BANERJEE: It seems an obvious thing to start with.

MR. LETELLIER: We have offered to do that, but instead chose to go to the data.

Now that we have evidence of loading in the chamber and trapping in the throat, we could use a CFD analysis to reduce that data and understand why it happened.

DR. BANERJEE: Because otherwise you don't know that you have anything similar between valves, or what are the controlling phenomena, what's going on.

MR. LETELLIER: Part of the difficulty with that is the diversity of designs in the industry.

DR. BANERJEE: Yeah, but it's cheap to run a computer calculation, rather than doing an experiment, if you can actually show that the two correspond.

MR. LETELLIER: Yes, I agree, that that cross comparison is useful.

DR. BANERJEE: Because this is a bit ad

hoc.

MR. LETELLIER: I absolutely agree that that calculation would be of benefit. But, on the other hand we would have dozens of designs --

DR. BANERJEE: Sure, run them all, it takes you what, an hour or two?

MR. LETELLIER: -- no.

DR. BANERJEE: I'll do it for you.

MR. LETELLIER: The run time is incidental compared to the set up, having the geometries.

DR. BANERJEE: There are, of course, now you can scan these in from SDL files and CAD files. I don't think you should make a big issue of that, it is really fairly easy to do nowadays.

MR. LETELLIER: I would love to do it if we had a cooperative vendor to provide the geometry files it would not be a huge effort.

DR. BANERJEE: Yes, if you have the CAD files it's fairly trivial to set up the mesh.

MR. LETELLIER: That's true.

MR. TREGONING: But, I'd agree with Bruce, I mean, we could have done it first, but it seemed pointless if we were going to go through this experimentation and even in the surrogate condition when we are trying to create blockage, even if we couldn't create blockage in those conditions, you know, it would have served very little purpose at that point.

DR. BANERJEE: Yes, maybe, but this really points to the point that Graham was making, that, you know, it would be nice to do some pre-experiment predictions and see how well you did, because eventually the science, I mean, this is not all ad hoc stuff that you are doing continuously.

MR. TREGONING: It is science. I would say that blockages is prone to some of the same issues that we've been dealing with over the last day, day and a half, you are going to see that in the data. It's a very stochastic, non-linear process to try to predict.

DR. BANERJEE: But, even that was science.

MR. TREGONING: Yes, I would agree.

MR. CARUSO: What is the black pad on the 12 o'clock position on the picture of the ring?

MR. LETELLIER: I believe that this is actually one of our shims, and I'll explain the test matrix. We needed to calibrate the percentage of blockage, and so we actually used epoxy to introduce a rubber gasket, in essence, blocking part of the flow.

We did experience some technical difficulties during the course of testing. We occasionally would score the face and have to remachine them. That was part of the benefit of this design, is we could remanufacture the replacement parts at minimal cost.

DR. SHACK: Did you actually try a hard face, I mean, or is this just stainless steel?

MR. LETELLIER: These are -- these are hardened stainless steel, but I'm not familiar with the industrial quality that's used in the plant. It was not our intention to study erosion, and, in fact, our apparatus has only a few tens of hours of service life, compared to a plant condition.

DR. SHACK: That's why I was surprised at the wear in the title.

MR. TREGONING: Again, we were looking for anecdotal evidence, again, just within these valves, but you are right, they weren't hard faced in any way, so we wouldn't claim that that had any particular applicability.

MR. LETELLIER: This next slide itemizes the measurements that were taken and what parameters we were interested in. We were interested in a spectrum of debris characteristics, the types, the three insulation products, their size, their shape relative to the throat opening, how much mass we introduced at one time, and at what rate we introduced the debris.

DR. KRESS: When you say size, is that a distribution?

MR. LETELLIER: Clearly, the one exception is the RMI, as surrogate debris we actually had regularly shaped square and rectangular strips of 1/8 and 1/4, and 1/4 by 1/2 inch aspect ratios.

The shredded insulation was blender processed, as we've already discussed, and it was of sizes representative of those that can pass, have been shown to pass, through a sump screen.

And incidentally, when we talk about penetration through a sump screen, it's very important to distinguish between the clean configuration and the partially-blocked configuration. All of our tests were done with nominally clean sump screens.

We also looked at the shape of the debris, the location, and the mass that was recovered at various points. The RMI oils, for example, showed clear signs of creasing and bending, as it was extruded through the valve.

We changed out the stem geometries and a range of gap settings.

DR. SHACK: Bruce, you mentioned, you have no sump screen in here, did you pre-filter the debris through a sump screen, so that the stuff you had you knew would pass through a sump screen, or is this just blender processed?

MR. LETELLIER: It's simply blender processed, and the -- the size distribution of what's able to pass through the screen looks very much like the debris that we introduce. However, the total quantity that passes through is less.

DR. SHACK: Okay, and you know that from the previous work.

MR. LETELLIER: That's right.

So, we were able to avoid that complication of pre-sieving.

We conceived this design in exactly the way you suggest, with debris introduced in the flume,it would pass through a screen, through the pump, and through the valve. And, our primary concern was the pump.

MR. TREGONING: Well, and the other down side of that, you can't -- it's harder to quantify what gets through, so it's harder to make it a systematic --

MR. LETELLIER: Yes, you have to catch it in order to quantify the penetration.

DR. SHACK: Right.

DR. BANERJEE: Your pump was not typical of the ECCS pumps, right?

MR. LETELLIER: It was typical in its design, but not in its capacity. It was able to -- it was able to achieve the flow rates of 75 to 100 gallons per minute that you might expect in a HPSI line, but the pressure was limited to about 485 psi.

Just for point of reference, in the clean configuration the delta P across the HPSI valve in service is between 20 and 200 psi, so we are easily able to initiate the sequence, but not to achieve the maximum delta P from the event of blockage.

To continue, we monitored the water temperature upstream and downstream, the gauge pressures across the valve, and the flow rates. Most of the data were collected at 75 gpm.

DR. BANERJEE: Is there a way to characterize what you are seeing, or are you going to tell us about that.

MR. LETELLIER: That's the next bullet, the key parameter was the valve loss coefficient. We struggled initially to decide on a metric, minimal level of detectable pressure loss, minimum amount of flow path area. In fact, there is no criteria in the industry for degraded performance. They install, test and operate these valves as if they are completely clean, and in pristine condition.

So, the best we could do was characterize the performance of our particular apparatus, and we chose the valve-loss coefficient as a good indicator that's proportional to blockage. It's a very simple relationship from first law energy balance in the lower left, that if the elevations are the same, and the velocities are the same before and after the valve, you can attribute any delta P to the body itself, and that's the coefficient K.

In the appropriate units of gpm and psi, it has a simplified expression that we use commonly.

Alternatively, a plant engineer may be more familiar with the CV or the flow cap coefficient, which is a reciprocal square root.

DR. KRESS: It's the same thing, just a different --

MR. LETELLIER: Exactly, and we prefer to have something that was directly proportional to the delta P.

We've discussed most of the debris

characterization already. The NUKON was pre-shredded as supplied by the vendor, and then blender processed on site. The calcium silicate that we used was coarsely crushed, I'm told, by a forklift at the vendor.

DR. BANERJEE: It's not a lead shredder.

MR. LETELLIER: No, and we also sieved it on site.

DR. SHACK: How big was the truck?

MR. LETELLIER: In order to improve the consistency of our size distribution, we pre-sieved it to a couple of size gradations.

Did we intentionally remove the fiber?

MR. TREGONING: Usually.

MR. LETELLIER: So, we took out the fiber binder.

MR. TREGONING: Well, intentionally, is too strong a word, quite often the fiber binder would be in clumps and it would be removed by the sieves.

MR. LETELLIER: But, if there were any remnants that got through, we didn't go through and pick them out, certainly.

DR. SHACK: Right.

MR. LETELLIER: And, the debris loadings were parametric, we'll look at those amounts as we go.

The broad overview of our experimental approach was first and foremost to establish the baseline loss coefficient for our clean configuration over the range of gap openings and flows that we're interested in.

Our loss coefficients were then calibrated to a known -- known obstruction, in terms of blockage area, using simulated shims, and I'll show you the calibration curve next.

So, eventually, we can relate a change in K to some physical proportionality of blockage.

DR. KRESS: Just percent of the gap area blockage, is that --

MR. LETELLIER: Essentially, yes, and we recognize that there is internal structure to the flow. We were not able to look at various locations, it was a very difficult test to do, to epoxy the shims in place and actually set the valve tightly against it.

In most cases, our data are reported in terms of the percent increase in the loss coefficient, compared to the clean configuration. And, occasionally, we do report them in terms of a conversion to the equivalent blockage area.

So, in order to implement the experimental approach, the test metric included three components, the baseline tests, the shim tests, and then a series of debris introduction tests.

The debris effects were examined in three series or three phases. Initially, we introduced them one at a time, the so-called single debris test of various quantities, and then next in combinations of two component mixtures and then three component mixtures, also in various orders of introduction. And finally, we looked at the potential for gradual or continuous accumulation in the third series.

The figure that illustrates the quantitative data are for the shim test calibrations. We tested -- one, two, three, four, five -- we tested six discreet fractions of valve blockage, all at 75 gpm.

DR. BANERJEE: You mean valve openings.

MR. LETELLIER: No, actually, these were all for constant volumetric flow of 75 gpm, and so that predetermined the proportion of the valve that we could block.

DR. BANERJEE: Well, I'm a little confused by what you mean by block, that's all, is it the valve opening which is changed?

MR. LETELLIER: If you imagine a fixed

valve opening that we simply -- we glued or epoxy adhered a rubber shim, so that it physically prevents flow from passing through a portion of the annular gap.

DR. BANERJEE: What was the reason for doing that?

MR. LETELLIER: So that eventually we could interpret our data, which are measured conveniently in terms of a valve loss coefficient, we could relate that to a physical configuration of debris.

DR. BANERJEE: So, you are interpreting that as some percent blockage looking at that line there.

MR. LETELLIER: Yes, at least we've made the connection. It's possible to interpret it that way.

DR. BANERJEE: So, these shims were put sort of uniformly, or were they just blocking one area of the valve, or how did you place them?

MR. LETELLIER: I was about to mention that we understand that there are complex internal structures in the flow, but we didn't have the luxury of randomizing the percent of blockage for the calibration tests. They were all place contiguously on one side of the valve.

If we would go back to the --

DR. BANERJEE: Go back to that diagram.

MR. LETELLIER: The schematic?

DR. BANERJEE: Right.

MR. LETELLIER: If you'd jump back to that.

So, we do have, obviously, eddies, internal flow during this bend in the flow. And, at the moment I'm not prepared to describe whether the shims were placed on this side of the gap or this side.

DR. BANERJEE: But, they were placed on one side of the gap.

MR. LETELLIER: Yes, at about 90 degrees.

CHAIRMAN WALLIS: I'm sorry, I've been out, are you just going to talk about valve blockage today, that's all?

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Because the most interesting part of your report, I found, was the bypass of the screen. Aren't you going to talk about that at all? Is anyone going to talk about that today?

MR. TREGONING: We hadn't planned on it,

just because that --

CHAIRMAN WALLIS: That's the more interesting part.

MR. TREGONING: Apparently, we thought this was the most interesting.

CHAIRMAN WALLIS: Can you bring that to the full committee. I mean, you sent us the report, we are very interested in it.

MR. TREGONING: Okay. If you have questions, we'd be happy to answer them.

CHAIRMAN WALLIS: We're excited to hear about it.

MR. TREGONING: No, and the reason we didn't focus on it is because that work has been completed for well over a year. I guess I assumed that --

CHAIRMAN WALLIS: Well, we didn't know about it.

MR. TREGONING: -- okay, I guess I had assumed that there may have been a prior presentation. We can certainly put together some of the salient points for the --

CHAIRMAN WALLIS: I'm sorry to interrupt, but I needed to know that.

MR. LETELLIER: Just a thumb nail sketch

of the penetration study, I mentioned before you returned that all of our penetration tests were conducted in a clean configuration. So, if we introduced a charge of debris, it did not have the benefit of a previously established bend.

CHAIRMAN WALLIS: Yes.

MR. LETELLIER: The calcium silicate was very difficult to retain any of that mass, 95 to 99 percent passes through easily in the sizes we tested.

The RMI is problematic. It's difficult to introduce it against the debris without confounding that with a transport -- transportability, and we tried different mechanisms, resuspension from the floor, dropping it directly in front of the screen, but in general penetration fractions are very sensitive to the relative size, and on the order of 30 to 40 percent --

MR. TREGONING: Tops.

MR. LETELLIER: -- maximum penetration.

CHAIRMAN WALLIS: It's not close to zero.

MR. LETELLIER: That's true.

MR. TREGONING: But again, these were relatively elevated flow rates to remove some of the difficulties with having debris either settling out or accumulating at the bottom of the screen.

We didn't want the debris to interact during those tests, so the flow rates were higher than might be expected in the modified designs.

CHAIRMAN WALLIS: And, what about fiberglass?

MR. LETELLIER: I am thinking on the order of 15 percent blockage, somewhat less.

MR. TREGONING: NUKON was very sensitive to processing, that's where you saw quite a bit of description between leaf shredding and blender process. We had a much higher percentage of blender process NUKON that would get through, it's finer, obviously.

CHAIRMAN WALLIS: Right, do we know what the sizes are in the LOCA?

MR. TREGONING: I'm sorry?

CHAIRMAN WALLIS: Do we know what the sizes are from a LOCA?

MR. TREGONING: In terms of whether --

CHAIRMAN WALLIS: Whether it's a leaf shredder or blender process.

MR. TREGONING: I'll let you take that one.

CHAIRMAN WALLIS: There's probably some --

MR. TREGONING: Distribution, certainly.

CHAIRMAN WALLIS: -- there's got to be some standard size distribution from a large LOCA or something. You didn't make any calculation.

MR. LETELLIER: Of course we do not have calculations for that, we do have evidence from the air jet tests that were done for the boiling water reactor studies that show us a range of sizes between individual fibers, to small clumps, up to shreds of two inches and larger, to partial blankets.

For practical reasons, we've chosen the leaf shredder approach as giving a visual representation of the small shreds.

CHAIRMAN WALLIS: Yes, a guy off the street could have told you you get something between small fibers and large chunks, you have to get some kind of a quantification, and the problem is, if you are going to just have numbers like 30 percent, something, transmission through the screen, someone can just take that and use it, and it may be completely inappropriate. You've got to have something that's scaled or related to --

MR. LETELLIER: The distributions in size from experiment, they are available, and we tend to focus on the transportable sizes, at least under flow velocities typical will fill up. The partial blankets are, perhaps, not as much a concern, unless they degrade in turbulent flow.

Now, the other piece to your question was, how representative is blender processed material, and, in fact, during the integrated tank testing done at UNM, where we had a 1/10th scale plant containment sump, we have evidence that the debris -- piles of debris in the leaf shredder configuration actually degrade slowly over time. It's possible to build very uniform mats out of individual fibers, and that's our motivation for blender processing. The details will vary, as we've learned.

DR. BANERJEE: You have no way, if I remember the report, to characterize your vendor process stuff, because if you drive the glomerate or something, right? You didn't give any distribution of sizes or anything.

MR. LETELLIER: That's correct, we have not. Due to the difficulties of forming uniform beds for the purpose of head loss testing, UNM and LANL started implementing the blender.

When PNNL adopted that approach, they recognized that they needed something that was much better controlled, and they developed the R4 metric for screen penetration, as something proportional to the degree of separation between fibers.

DR. BANERJEE: So then, it gives you a measure, but it doesn't tell you what the physical size of the things are.

MR. LETELLIER: That's correct.

DR. BANERJEE:

CHAIRMAN WALLIS: If you follow the guidance that came out, I think that it's very crude. I mean, it said that you assume half of it is shredded and half of it is 4x4 clumps or something, it's something very, very crude, if I remember from the guidance that we looked at. That would, perhaps, lead to very conservative predictions.

MR. LETELLIER: It does. the proportion of fine material is intentionally over estimated, and it's assumed to be 100 percent transportable.

CHAIRMAN WALLIS: Right.

MR. LETELLIER: And, in fact, the degradation component of even the large has been --

CHAIRMAN WALLIS: This doesn't help you very much, because it means that whatever they assume is 40 to 50 percent if it gets to the sump anyway, and gets to the screen anyway.

MR. LETELLIER: Right.

CHAIRMAN WALLIS: Which may turn out to be very conservative. I just don't know.

Okay, thank you. I've made you digress.

DR. BANERJEE: I just wanted to go back to what you are doing with the shims. If I understand the reason for doing that, it's to get a way to interpret the data you get with blockage.

Now, let me ask you, in reducing the flow area why did you choose shims rather than just opening and closing the valve, change the flow that way?

MR. TREGONING: We did both, actually. The baseline test did exactly that. We didn't show that, because they are rather rote, but -- and if you look at -- one of the concerns and one of the genesis behind the shim test is, we had a question whether if we had nine uniform flow through the value if the correlation between valve area and the K increase would change.

Actually, we don't show the spot here, but when we did the shim test and compared it with the more uniform valve opening test, with respect to this plot, you know, delta K increase versus low blockage, they actually end up lying pretty close to one another.

So, the shim test, at least as we've been able to demonstrate, provided confirmation that the more simplistic test was not an unrealistic measure of what the blockage was.

DR. BANERJEE: You looked at the effect of non-uniform flow through the data.

MR. LETELLIER: Essentially, that's right. Thank you, Ralph.

DR. KRESS: Those look like the same curve to me.

MR. TREGONING: These are different valves.

DR. KRESS: I know, but they look like I would have drawn the same line to the data.

CHAIRMAN WALLIS: It's just area, isn't it? I mean, this is --

MR. LETELLIER: Yes, they are pretty consistent.

CHAIRMAN WALLIS: -- this one over here --

MR. LETELLIER: Yes, this was a calibration where we intentionally introduced the non-uniform blockage.

CHAIRMAN WALLIS: Reduced the area, all right.

MR. TREGONING: Although, I will -- before we get off of this, some of the apparent consistency is due to scale as well. I think the biggest differences are down at the lower percentage increases of K, the more lower valve blockages around 10 to 15 percent, where your K increases are on the order of 20 to 30 percent.

DR. KRESS: Yes, and that's the level that we don't care much about, right?

MR. TREGONING: Well, but you are going to see in the testing that that's the level that we tend to be in for most of the test results.

CHAIRMAN WALLIS: Is this curve a theory?

MR. TREGONING: No.

CHAIRMAN WALLIS: It could be a theory which simply says that the area is --

DR. KRESS: What's the form of the equation?

DR. BANERJEE: I guess they were looking at whether just changing the shape of the area --

CHAIRMAN WALLIS: That's the easiest problem to solve, tell us what to do with the difficult ones.

MR. LETELLIER: One other purpose for conducting this test is to quantify our minimum level of detection for this system. How sensitive are we in terms of blockage effects, and through this and other studies we'd like to say we have a detection threshold of about 5 percent blockage and no less.

As I said earlier, there is no performance standard for degraded performance. These valves are assumed to be clean when they are operable.

DR. KRESS: Do you have K values for real valves out there, just to compare and see if you are close?

MR. LETELLIER: I don't have them in front of me. We did compare them in terms of the CV metric, the reciprocal square root.

MR. TREGONING: Yes, that is information we've tried to get, even getting the K information for real valves is not -- you know, it's not an easy exercise. You have to do a good bit of digging.

CHAIRMAN WALLIS: What about number two, based on the propensity for NUKON to transport, I thought you told me earlier when I asked you this, the fiberglass -- lesser -- 95 percent of the CalSil one, but sometimes most of the fiberglass was cored, and I think you said --

MR. LETELLIER: The statement also --

CHAIRMAN WALLIS: -- 15 percent went through or something, but you said it was sensitive to processing, so if it was chopped up this propensity for NUKON to transport is when it's really chopped up fine, is that it?

MR. TREGONING: Read the whole line.

CHAIRMAN WALLIS: Oh.

MR. TREGONING: This is combining the screen penetration results with the propensity to cause blockage.

CHAIRMAN WALLIS: No, it says based on the propensity for NUKON to transport and penetrate the screen.

MR. TREGONING: Right, keep going.

CHAIRMAN WALLIS: That's a statement in its own right.

MR. TREGONING: Yes, but it's a conditional clause of which --

CHAIRMAN WALLIS: Well, I don't know.

MR. TREGONING: -- you know, read what follows.

CHAIRMAN WALLIS: No, but it says -- you are saying already that it's highly likely the NUKON will go through the screen. Therefore -- no, it stands on its own, doesn't it?

MR. LETELLIER: This is -- it is intended to be a combined judgment, based on both its penetration potential and its retention potential in the valve.

CHAIRMAN WALLIS: Yes.

MR. LETELLIER: And, you'll see that as we look through the data, that NUKON does tend to accumulate in the valves.

MR. TREGONING: And, just to explain it, CalSil was certainly much more likely to penetrate. However, CalSil by itself --

CHAIRMAN WALLIS: That is true, I don't think the NUKON is flexible enough and there's enough pressure.

DR. BANERJEE: And, the CalSil would happily carry on into the reactor, right, block the little holes there?

MR. LETELLIER: But, in combination with other debris types the CalSil does become an important contributor, and we'll see that.

MR. TREGONING: Right, you'll see that later.

DR. BANERJEE: Well, eventually it will catch on the NUKON, right?

MR. TREGONING: It could catch on other seeds that might be there before it.

MR. LETELLIER: In general, and not surprising, the higher the loading and the larger the debris sizes resulted in a proportional increase in the valve loss coefficient. There were no surprises there.

However, there is variability in the data.

For an equivalent mass loading, the valve loss coefficients were typically higher for reflective metallic chards than they were for an equivalent mass of NUKON, I think simply because if you are able to lodge a fragment of RMI that obstructs a very large area immediately, and then the proportional mass of NUKON is simply smaller.

CHAIRMAN WALLIS: I wasn't here earlier, but it seems to me clear, you can put the valve in a position where it is almost closed, where it is bound to catch everything.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Then you build up a huge plug in the pipe. Is that not something that can ever happen?

MR. TREGONING: Well, but again, normally the way -- and again, my understanding of how these valves are set, is they are normally set with a higher gap than your screen penetration or mesh sizes, with the idea that --

CHAIRMAN WALLIS: When they are open?

MR. TREGONING: -- yes, when they are open and operating.

CHAIRMAN WALLIS: My question is, if you open and close them, and you've got some debris there which doesn't open and close fully, then you have flow through a very small aperture.

DR. BANERJEE: But, yesterday they told us that there's some minimum.

CHAIRMAN WALLIS: Does that mean it can't be closed completely?

DR. BANERJEE: No, apparently, that's not the way they are operating.

CHAIRMAN WALLIS: It will be closed at some point. Who told us that yesterday?

MR. LETELLIER: Typically, these are -- they are not intended to be actuated during the accident sequence. Some plants are physically locked in place and cannot be actuated, some plants can be actuated if blockage is perceived, or if they need to rebalance the flow injection paths.

CHAIRMAN WALLIS: If they are blocked, you just open them wide and clear it out.

MR. LETELLIER: Perhaps.

MR. CARUSO: Are these manual valves usually, or do they have motor operators on them?

MR. LETELLIER: My impression is that

typically they are manual, and I have heard of some pneumatic actuation.

MR. CARUSO: They are set and left. As far as I know, they are usually set and left.

MR. TREGONING: Yes.

MR. LETELLIER: Yes, they serve two primary functions, to balance the flow and also to throttle the pump, to prevent run out.

If it were completely wide open, then the pump would have no back pressure to work against.

The plants have -- anecdotally, they have recognized the potential for -- first of all, I think their primary concern is erosion and loss of function for that valve, so they have started to -- I know of plants that have introduced orifice plates downstream of the valve to take -- to burn off some of the head, so that they can relax the gap opening inside the throttle valve.

CHAIRMAN WALLIS: What is the typical velocity, just a number, through the gap opening?

MR. LETELLIER: I knew you would ask that.

CHAIRMAN WALLIS: Ten meters per second, one meter per second.

MR. LETELLIER: We tend to think in terms of the volumetric flow.

CHAIRMAN WALLIS: All right.

MR. LETELLIER: The dimensions are typical, the flow rates are set to be nominally 75 gpm or less. The velocities are tens of feet per second.

CHAIRMAN WALLIS: What's the Reynolds number in pipes?

MR. LETELLIER: I don't have that information.

DR. KRESS: I was wondering if there was a FROUDE number for debris that you change the nature of the debris just to do the turbulence, before it ever gets that.

CHAIRMAN WALLIS: FROUDE number is for a different field altogether. You are talking about a FROUDE number?

DR. BANERJEE: A fraud number.

CHAIRMAN WALLIS: That's for lawyers.

DR. KRESS: Any way, I was wondering if the debris has the ability to change its nature before it ever gets to that.

MR. LETELLIER: It is possible, and I think in the power plant situation that's most likely to occur within the pump itself, and those details we are not able to reproduce, internal flow.

However, the FROUDE number, fraud number, is actually important for the blender processing, where we are intentionally trying to separate the debris from itself.

DR. BANERJEE: But, the velocity is pretty high.

MR. LETELLIER: Yes.

DR. BANERJEE: That's the main thing.

MR. LETELLIER: As we said, for the size of opening within the internal RCS this probably experiences the highest velocities, which introduces the possibility for self-scouring. If a blockage starts to accumulate, the velocity will increase, and we show evidence of that potential.

MR. TREGONING: In some of these tests you see that happen.

MR. LETELLIER: Another aspect of the HPSI system --

CHAIRMAN WALLIS: How abrasive is CalSil? How abrasive is CalSil? It's pretty soft, isn't it?

MR. LETELLIER: Well, yes and no, it's a calcium silicate. It could be considered --

CHAIRMAN WALLIS: So, it has some hard edges too?

MR. LETELLIER: Yes.

DR. BANERJEE: But, it's not like sand.

MR. LETELLIER: It is not a hard silica type of sand.

Steve Unikewicz could probably help us describe the metrics for how we would assess the abrasive qualities of CalSil. It has not been tested from that perspective.

Another aspect of the HPSI system performance is the extremely high capacity for delta P. If these valves were almost blocked, you could experience 2,400 psi, which might have the potential to extrude the debris simply, or, alternatively, permanently compact it and press it in place.

We don't have that amount of margin. We can test the initial conditions at about 350 psi, up to a maximum of 485 to 500.

CHAIRMAN WALLIS: So, that was my concern, if you had a valve that were closed, not quite closed, if it ever go that, and they you said it would build up this plug of debris, and then you tried to get flow and you got 2,200 psi compressing the debris.

MR. LETELLIER: Right.

That's one of the phenomena we were aware of, and we chose our pump to have some margin to investigate that, within our safety limits.

The very last bullet describes some of the random variability that you are going to see in the data points. First of all, we worked very hard to minimize the electronic noise to less than 3 percent of the mean signal, but the data jump around within factors of two to five, and the very random nature of the flow is my explanation for this. The self-interaction of the debris, with the internal structure of the flow, the debris size relative to those internal eddies, the random orientation as it arrives at the gap, for example, and simply the number of debris elements in a unit mass. We are typically testing ten grams, 25 grams, maybe 50 grams of material, but that is a discreet number of RMI fragments.

A couple of photographs to show you what debris looks like when we open the chamber, and, Dr. Wallis, you'll find the schematic on one of the earlier pages, number seven, that shows you the lower chamber. That's what we are looking at here, as we pull out the internals. You can see 1/4 x 1/2 inch strips of RMI. This test would have experienced an increase in K value, but I cannot tell you whether the fragments were lodged in the throat at under flow.

DR. BANERJEE: They probably have dropped down when you took it out.

MR. LETELLIER: Yes.

MR. TREGONING: That's one possibility.

MR. LETELLIER: And, similarly, you can see the yellow deposits of fiberglass that are in the bottom cavity.

DR. BANERJEE: They are deposits?

MR. LETELLIER: The quality of this photograph is not extremely good, but you can see the granularity that represent the sort of largest flocs or agglomerations here. These are composites of multiple strands.

MR. CARUSO: Did you see any -- on the seat, and on the disk, on the RMI testing, did you see any impacts or evidence that the RMI, you know, had gotten larger and deformed?

MR. LETELLIER: Scoring?

MR. CARUSO: Scoring at all?

MR. LETELLIER: Scratching?

I don't think that I could say we have direct evidence of that. As I said, just the manipulation of the valve in some cases led to scoring. When we would tighten it down to the seat, if we would go past that point, if there were any kind of abrasive material like calcium residue we would score the machined face.

CHAIRMAN WALLIS: It's grinding in the valve.

MR. LETELLIER: Yes, right.

We did experience that and we intentionally took the opportunity to test or see if we could determine the effect of K, because we had good baseline information in the pristine condition, and we were not able to discriminate those kind of minor damage conditions.

The next slide begins our presentation of data for the single debris series.

CHAIRMAN WALLIS: All right, so how did these pieces get into the valve, you dropped them in somewhere and flow them up to it, and some of them go through?

MR. LETELLIER: On slide number six, it will show you the manifold.

CHAIRMAN WALLIS: You drop them in, and then -- I was just wondering, how many pieces could -- you go up to 60 here, under what conditions could you get, say, 300 pieces in there, keep on dropping pieces in, some of them get stuck.

MR. LETELLIER: Well, first of all, keep in mind that the axis on the figure, that is the number of pieces that were recovered from the chamber.

We would typically introduce them in batches of 50 to 100 grams, which is a nice handful of RMI.

That was our metric for the debris loading.

CHAIRMAN WALLIS: What happens in the path, this thing is running for some time, maybe thousands of pieces go through, I don't know.

MR. LETELLIER: Perhaps, that's true, but distributed over time.

CHAIRMAN WALLIS: Right.

If there is some way for them to get trapped in there, they can build up, I just don't know.

MR. LETELLIER: We will look at that question at the very last series, under the accumulation potential.

CHAIRMAN WALLIS: Let me ask, the pieces of RMI that are trapped, were they larger than the gap, all of them, or were there some smaller ones?

MR. LETELLIER: That's a loaded question. On edge, they are all smaller, but in aspect ratio they are all bigger.

CHAIRMAN WALLIS: It depends which way they try to get through the gap.

MR. LETELLIER: It just depends on which

way they are oriented.

MR. TREGONING: Well, and again, most of these things post test you saw a lot of crumpling of the RMI debris, so there was some variability in terms of how it would crumple that would affect the dimensions as well.

CHAIRMAN WALLIS: Usually, once they get held up against the gap, they stay there, because of hydrostatic dynamic forces, then they maybe fall down when you turn off the flow.

MR. TREGONING: Yes, right.

CHAIRMAN WALLIS: And, you can imagine them sort of flying off, and some of them get stuck in the position where they are held against the wall, others go through.

DR. BANERJEE: Are these small pieces, or they were all large pieces?

MR. LETELLIER: They were all uniform size of either 1/4 inch square or 1/4 x 1/2 inch strips.

CHAIRMAN WALLIS: This is what you fed in.

MR. LETELLIER: This was our prototypical chard or fragment of RMI.

MR. TREGONING: And again, those sizes were picked based on the earlier screen penetration work, where those were some of the biggest pieces of

RMI that we could get through.

MR. CARUSO: How thick were they?

MR. LETELLIER: 15 mls, we can look in the report for the exact number.

MR. CARUSO: Very flexible.

MR. LETELLIER: They are very think, very flexible. Our graduate student desperately hoped we could reuse this material, just to avoid the tedium of cutting more source material, but, in fact, most of them were folded.

DR. BANERJEE: By the time they got through they were crumpled.

MR. LETELLIER: That's correct.

CHAIRMAN WALLIS: But, you could do a calculation of the pressure it would take to crumple it enough to drive it through the gap.

MR. LETELLIER: We could have done that, yes.

CHAIRMAN WALLIS: Take it as a beam that's loaded in.

MR. LETELLIER: Yes.

So, the first data figure shows the introduction of batches of RMI as a single debris type. The plots are designated by symbol, designating the different stem types, the 45 degree angle with the large diameter --

CHAIRMAN WALLIS: Go back to my previous question. When you've got this 65 or whatever it is, the point over there, what fraction is that of the amount of stuff you put in? That's a handful, 65 must be a pretty good handful. Do you put in a few handfuls, it's a fairly large fraction of what you put in?

MR. LETELLIER: No, it's less -- easily less than half, I'm going to guess 20 percent.

CHAIRMAN WALLIS: Half, that's a big fraction, I would say 5 percent is a big fraction if you are dealing with a lot of debris.

MR. LETELLIER: Yes, that's true.

It is a surprisingly large number of fragments.

CHAIRMAN WALLIS: But, if you are comparing it with half, that tells me it's a lot.

MR. TREGONING: Again, of this size RMI debris, which again, was the maximum size that we put in.

CHAIRMAN WALLIS: If you put in three handfuls, and one handful got stuck, that's a pretty good measure, something like that, is that it? If you put in five handfuls, and one handful got stuck.

MR. LETELLIER: That's probably reasonable.

DR. BANERJEE: SO, when you got a percentage increase in K, did your flow stay the same, too?

MR. LETELLIER: We did not control the flow from that point of view. We preset the gap size to achieve the initial condition, and we measured the delta P, and we also measured the flow rate, which would tend to decline.

DR. BANERJEE: Would decline.

MR. LETELLIER: Yes, with increased blockage. So, we had both terms of the equation, we had the flow rate and the delta P as the data point.

DR. BANERJEE: So, you only show the delta P here, but the flow rate goes as the square root of -- no, I guess it was proportional to the K.

MR. LETELLIER: We are presenting the K factor, the loss coefficient, which has both. It's the ratio.

MR. TREGONING: But, most of the time, not all of the time, the delta P drop dominates, I think, in many of these tests, because the blockage is in many cases, were still partial, so you are still able to get plenty of flow through the valve.

MR. LETELLIER: As we speculated, the velocities will increase to maintain a special plant condition where the pumps have so much excess capacity.

CHAIRMAN WALLIS: I would like to see where we are with this presentation. We had some extra presentations earlier, so that's the reason we are behind.

MR. TREGONING: We have been going about an hour, though, I think.

MR. LETELLIER: We are taking our share of the time.

CHAIRMAN WALLIS: You are taking your fair share, so if you finish by 10:30 or something, at that time we could take a break, and we just set back the other presentations?

MR. LETELLIER: Sure.

CHAIRMAN WALLIS: After the break.

MR. CARUSO: We have one more presentation.

CHAIRMAN WALLIS: We have one more presentation, then there's the sum up and so on.

It looks as if we are supposed to finish by 11:00, maybe we'll finish by about 12:00.

Okay, sorry to interrupt you. Go ahead.

MR. LETELLIER: That's not a problem to finish within that time.

One trend of interest to note in this figure, if you focus on the solid square symbols, which represent the smaller diameter five degree stem, they are all in the lower percentages of pipe loss coefficients.

This goes back to the questions of internal flow. For some reason, this configuration performs much better in all cases as we go through, you will see that the smaller diameter channels the flow in some way that tends to clear the debris better than the others.

That's not what I would have expected, because there's more shoulder room in the cavity in the dead eddies, but, nonetheless, the data are very clear in that regard.

DR. BANERJEE: Just say it again, so that I follow what you said.

MR. LETELLIER: The square solid symbols represent the smaller diameter five degree, the shallow angle stem, which has a smaller diameter so the flow path is more tightly centered in the cavity, in the chamber.

DR. BANERJEE: And, is the chamber smaller too?

MR. LETELLIER: No.

DR. BANERJEE: No, the chamber is the same size.

MR. LETELLIER: Same size, fixed dimension, and yet we consistently show less debris retention and a lower impact in terms of --

DR. BANERJEE: If there is a bigger chamber and a smaller stem area, you get less debris retention.

MR. LETELLIER: In our case, for --

MR. TREGONING: The interesting point is that the blue square right around there where we found 40 some odd pieces of debris in the chamber after the test, yet there was no increase in -- there was no measurable degradation in the valve loss coefficient.

What could be happening with that big shoulder, there could be eddies that are created, or maybe the shoulder is even serving as a debris blockage point.

CHAIRMAN WALLIS: It may be that it aligns differently.

MR. TREGONING: It's causing it to trap in the --

CHAIRMAN WALLIS: And, this is a little

bit heavier, the heavier material gets thrown out of the vortices, so if that's thrown out of the vortices it could be that if you have large vorticle structures it could be that they are oriented as they flow towards the gap, if they are oriented more in the direction of going through it than across it.

DR. DENNING: You didn't tell us how you set the gap, though. There's a standard gap size that you have for each of the three different configurations? How did you do that?

MR. LETELLIER: During our initial characterization of each pristine valve, we calibrated the physical gap dimension to the flow rates and the pressure drops. And so, when we are ready to initialize a test we can return to that physical setting, in terms of a thread count. There's actually a fiducial measurement block where you can use a micrometer to reproduce the valve setting.

DR. DENNING: But, the flow areas, what are the relative flow areas for those different --

MR. CARUSO: What we are wondering is, for each of these points, what's the geometry of the flow path?

DR. DENNING: Yes.

CHAIRMAN WALLIS: Is it the same area?

MR. LETELLIER: First of all, we have to have a common understanding of flow area, and it's always measured perpendicular to the flow across --

CHAIRMAN WALLIS: The narrowest part.

MR. LETELLIER: -- across the narrowest aspect of the flow channel.

The gap settings, obviously, we --

CHAIRMAN WALLIS: Is that how you define your K, or is the K based on something else? What area is K based on?

DR. BANERJEE: Let's go to the next slide defining the K.

CHAIRMAN WALLIS: It's based on Q?

MR. LETELLIER: K is simply delta P over Q squared.

CHAIRMAN WALLIS: So, if you change the area, the K changes.

MR. TREGONING: Yes, the baseline K can certainly change.

CHAIRMAN WALLIS: My word, it's the sixth significant figure.

MR. LETELLIER: Yes, very accurate.

CHAIRMAN WALLIS: That's amazing.

DR. BANERJEE: The smaller valve, actually, the seat is shorter, that hole is smaller.

MR. LETELLIER: Yes, both the ring, which I'm showing here, and the stem, are both changed as a mated pair.

DR. BANERJEE: You show the diagram there.

MR. CARUSO: Are you showing there -- that diagram is a 45S -- 45L configuration.

MR. LETELLIER: Yes.

MR. CARUSO: And, the 5S and the 5L configuration, is that the whole side of that stem there, is that the entire seating surface, so it's got a much larger seating surface?

MR. LETELLIER: It does, yes, it has a much longer flow path.

MR. CARUSO: A longer flow path.

MR. LETELLIER: So, the geometry is very different for these things. I don't know how the changes with K -- the same K with a 45L has a different flow length than a 5S and a 5L.

CHAIRMAN WALLIS: Sure.

MR. CARUSO: And, that would be interesting to know, because that might tell you how it is that some debris gets through and some doesn't.

CHAIRMAN WALLIS: Since nobody knows what's happening, we can talk about this forever.

MR. LETELLIER: All of those aspects are

quantified in the data report, as part of valve characterization.

DR. BANERJEE: I think if we go back to the data, it was quite interesting.

CHAIRMAN WALLIS: The data is interesting, but the explanation is speculative.

MR. LETELLIER: Yes.

DR. BANERJEE: You have to start somewhere.

CHAIRMAN WALLIS: It's very interesting data to report on.

MR. LETELLIER: The next slide, Figure No. 16 --

DR. BANERJEE: One thing that you don't show is the -- you had another S, there were two S's in that diagram you showed us, there was a 45S.

MR. TREGONING:That was a typographical error, as Bruce mentioned earlier, it should have been a 45L. The seat area is the same size.

DR. BANERJEE: So, you had only one S.

MR. LETELLIER: Large and small, two diameters, two different contact angles.

This next figure, perhaps, answers the question -- your original, most recent question. It presents the RMI single debris data in terms of the gap size, and here we've done the geometry to convert the stem setting to the cross sectional flow area, and they are in the range of 1/16 to 1/8 inch in gap cross section, and you can convert that to an annular area to compute volumetric flows.

DR. BANERJEE: So, you show similar trends.

MR. LETELLIER: It does. This is simply an alternative presentation of the same data. We are now --

DR. BANERJEE: Oh, okay.

MR. LETELLIER: -- we are converting this to be explicit in terms of the gap size.

CHAIRMAN WALLIS: Once you are above one, it's not clear it's statistically significant.

MR. LETELLIER: Above one?

CHAIRMAN WALLIS: Once you are above one, it looks as if below one it's less likely to block, but, you know, four, and eight, and ten, are not statistically different.

MR. LETELLIER: Keep in mind that the symbols represent different quantities of debris in combination with each stem.

CHAIRMAN WALLIS: So, some of the points shouldn't be compared with some of the other points.

MR. LETELLIER: That's right.

You need to focus your eyes on a single group of symbols.

DR. BANERJEE: So, all the S's still lie below, right?

MR. LETELLIER: Yes.

DR. BANERJEE: The 5S group.

MR. LETELLIER: Yes, all the 5S's are below the threshold of detection.

This presentation also gives you an impression of the relative size of the debris fragment. Remember, the RMI are discreet dimensions, and the gap can be a range, a continuous range. And so, we are increasing the -- actually, as we proceed across the figure to larger ratios, we are physically reducing the gap. We are closing the valve.

CHAIRMAN WALLIS: 10g means gallons per minute or something?

MR. LETELLIER: No, the units are gap size.

CHAIRMAN WALLIS: In grams, or what's 10g?

MR. TREGONING: 10g is the mass of loading of the RMI, so that was 10 grams.

CHAIRMAN WALLIS: But, the flow rates are different for all of these experiments? I don't know what to say then.

MR. TREGONING: No, they are all nominally.

CHAIRMAN WALLIS: They are all the same flow rate.

MR. TREGONING: 75 gpm.

CHAIRMAN WALLIS: Nominally, but they change slightly as you go up in K, right, but that's not the first order of it.

MR. TREGONING: Pressure is certainly much more sensitive.

CHAIRMAN WALLIS: So, this could be presenting increase in pressure drop.

MR. TREGONING: Yes. In many cases there's a direct correlation.

CHAIRMAN WALLIS: Well, it's the same flow rate, that's what it is. We are talking about a difference between five and 50. Okay.

MR. LETELLIER: You are exactly right, the previous figure is more --

MR. TREGONING: Let's not go backwards.

MR. LETELLIER: Let's move ahead to the single debris NUKON tests, which we have more limited data. It shows a positive correlation, as we increased the mass of the loading in a charge, a slug of debris, we tend to have higher amount of retention, a higher percent increase in K.

DR. DENNING: But now, the 5S valve still is by far the best.

MR. LETELLIER: Now shown as diamonds, it's by far the best.

There's actually --

DR. DENNING: There's something weird going on.

MR. LETELLIER: I agree, I'm not prepared to say that it's anomalous, but I think it is interesting, and it's worthy of -- it's worthy of comparison now to the actual plant configurations, what do the valves look like, why could this be atypical compared to plant performance.

Although the data are sparse, these are reasonably well behaved trends. The X is for the 45 large stem show a nice proportionality, as do the 45 large and the 5 large.

CHAIRMAN WALLIS: This mass of NUKON, is that the amount that gets stuck in the valve now?

MR. TREGONING: No, that was the amount that was introduced at the beginning of the test, that was the charge.

MR. LETELLIER: Yes, these are the amounts introduced.

DR. BANERJEE: But, you do have a measure of how much was stuck, from a post test exam, right?

MR. LETELLIER: Yes.

The RMI pieces are easy to count.

CHAIRMAN WALLIS: The other graphs were for -- based on the amount that got stuck.

MR. LETELLIER: That was for RMI, which is easy to count.

CHAIRMAN WALLIS: Right.

MR. LETELLIER: For the NUKON, it's much harder to recover all of the residue, but we do save and dry that. It's reported.

MR. TREGONING: The mass loadings, and there's legends where the amounts was the mass introduced.

CHAIRMAN WALLIS: Yes, those are the ones, I can see that.

MR. TREGONING: Yes.

MR. LETELLIER: Moving on to the two component mixtures, just some overview observations.

CHAIRMAN WALLIS: I don't know how to translate RMI pieces to grams.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Okay, sorry.

MR. LETELLIER: 50 grams is a handful.

CHAIRMAN WALLIS: 50 grams is a handful? Okay, 50 grams is a handful.

MR. TREGONING: I don't think that conversion factor is going to be sufficient for Dr. Wallis. That is in the report. We do -- I just can't pull the number off the top of my head.

CHAIRMAN WALLIS: No, you don't have this.

MR. TREGONING: No, like I said, we'll be submitting the report shortly.

MR. LETELLIER: Some overview observations of two component mixtures. Compared to single test -- single debris tests, the CalSil-RMI mixtures were the only combination that exhibited a clear increase in K, compared to introducing them by themselves.

The other pair-wise combinations were not as evident.

DR. BANERJEE: CalSil-RMI, that's a weird one.

MR. LETELLIER: Given that you have RMI lodged in the valve, it was effective that accumulating CalSil --

DR. BANERJEE: But, the CalSil is so small.

MR. LETELLIER: But again, this RMI is

bent, so there's plenty of trapping pockets, you know,

DR. KRESS: Yes, but I would have thought NUKON combination with CalSil would have been a bad combination.

CHAIRMAN WALLIS: Yes, you would have thought.

DR. BANERJEE: That's the danger of engineering judgment.

MR. LETELLIER: We would have thought as well. The data are admittedly sparse in that regard. It deserves further comparison, but it's clearly evident in the information that we do have.

The mixtures of fiberglass and RMI, and in the CAlSil-NUKON, they did not differ significantly from the analogous separate tests, except for one exploratory case where we've literally packed the chamber with CalSil-NUKON to what you might consider to be an unrealistically high -- we physically could not fit anymore into the chamber, and in that case --

MR. TREGONING: It was unsieved, though, I think that was the other key component. So, it still had binder in the CalSil, as well as there was some initial clumping.

MR. LETELLIER: So, page 19 shows mixed RMI-NUKON.

CHAIRMAN WALLIS: Where's the one that showed the clear increase in K? That's a single test.

MR. TREGONING: Yes, there was only a test or two of that RMI-CalSil mixture, so that --

CHAIRMAN WALLIS: With this kind of stuff it's difficult to conclude anything from one test, right?

MR. LETELLIER: We would agree. That's why we noted that as an exception and have not presented it.

CHAIRMAN WALLIS: You presented your test on this figure here, conclude what you want to conclude, if you are basing it all on one test.

DR. BANERJEE: But, you don't show the small stem.

MR. LETELLIER: On the previous slide for the small stem?

DR. BANERJEE: No, I mean --

DR. SHACK: On the mixed debris test, you don't seem to have tested the small stem, or at least it's not on this graph.

CHAIRMAN WALLIS: It's not on this graph. This is a funny trend, because if the valve opening is zero you'd expect a percent increase in K to be infinite, or huge.

MR. LETELLIER: No, it's a percent increase.

CHAIRMAN WALLIS: A very tiny opening would get blocked up.

MR. LETELLIER: No, this is a differential, it's a comparison of the clean configuration, you essentially have a ratio of zero over zero.

CHAIRMAN WALLIS: That's what I mean, that's what I mean, though, a very small gap, which has trapped all the debris, would surely give you a figured percent increase.

MR. LETELLIER: No, because that graph --

MR. TREGONING: But, that gap in the clean condition would have had high K initially.

CHAIRMAN WALLIS: You are talking about percent increase.

MR. TREGONING: Right, compared to the baseline of the clean condition.

CHAIRMAN WALLIS: If it traps all the debris, a percent increase you'd think would be --

DR. BANERJEE: Yes, that's why engineering judgment is bad, that's what you would have been thinking, and I would have thought.

CHAIRMAN WALLIS: I'm still thinking. It

depends, I think, on the experiment, and how much stuff you put it, for how long, it's a very small gap. If you put in enough stuff for long enough it's going to fill the pipe and everything else, and it won't let anything through, and that's sort of the limiting case.

MR. TREGONING: Potentially.

CHAIRMAN WALLIS: And, then, it's going -- then you are going to apply your 2,200 pair size, squash all that stuff.

Well, that's my engineering judgment.

DR. BANERJEE: How long were these tests run for?

MR. LETELLIER: These tests, from the time the debris are introduced to the time it passes through, is only a few seconds. It literally is flushed through the valve.

DR. BANERJEE: It's introduced as a pulse.

MR. LETELLIER: It's introduced as a pulse.

DR. BANERJEE: And, when you have the very small gap openings, do you, basically, capture all the RMI there that goes through?

MR. LETELLIER: No.

MR. TREGONING: We had question earlier

for Dr. Wallis, 10 to 30 percent, less than 50 percent certainly, even in the most cases with the highest amount of capture.

CHAIRMAN WALLIS: I think actually that what I said was probably reasonable, because I can take your black diamonds and extrapolate, and it's increasing, it's increasing K going up, as you go down in the valve opening. The points that are .05 are all for different kinds of valves from the other ones, so you can't extrapolate anything based on just looking at the data here, because different valves are the different flow rates.

MR. LETELLIER: It's critical to keep in mind what configuration you are looking at.

CHAIRMAN WALLIS: Why did you do that? Why didn't you test the same valve at three flow rates?

MR. TREGONING: There's a range of settings -- there's a range of settings and gap sizes out in the plants, we wanted to at least look at that range.

CHAIRMAN WALLIS: But, you are doing science here as well, you are trying to understand.

DR. BANERJEE: They are not.

MR. TREGONING: The initial objective was just to determine if we could get blockage in these configurations.

MR. LETELLIER: Under representative plant configurations.

Now that we --

CHAIRMAN WALLIS: Well, I guess if someone else brings in a different valve then you have to test it. There's no theory of anything, no predictive capability. If someone changes the opening in the valve then they have to do an experiment.

MR. LETELLIER: No, let me say first that now that we have evidence of potential blockage, this is the time to understand it to drive the system.

CHAIRMAN WALLIS: This is like the ICET test, you are just trying to see if something happens, and then you are going to go and make it quantitative later.

MR. LETELLIER: That was our initial objective, to see if it was possible --

CHAIRMAN WALLIS: Right.

MR. LETELLIER: -- to block.

DR. BANERJEE: Now, coming back to this question of the valve opening, when you have very small valve openings, you have a much higher velocity through there, right?

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: You have the same pressure, you have the same velocity.

MR. TREGONING: For a given flow rate.

CHAIRMAN WALLIS: Ah.

DR. BANERJEE: Given the flow rate is constant.

MR. LETELLIER: That's what's critical to the plant, is to maintain flow.

DR. BANERJEE: So then, if you have a piece of RMI or something, potentially, you could deform it because of the higher forces due to the velocity head, and, perhaps, that's the effect, speculating. You see what I mean, let's say you get a piece that comes up against that gap, and you have over five velocities, so, potentially, it could deform, whereas at a low velocity just sit there and sort of hang out.

CHAIRMAN WALLIS: So, what controls the flow rate in this then, you say it's all the same flow, in the plant what controls the flow rate? You run the pump, and doesn't this control the flow rate, this valve?

MR. LETELLIER: It does, the power plant actually has an enormous capacity for delta P, for driving the system --

CHAIRMAN WALLIS: But, that would mean you would have to run the pump at a different speed or something? You've got a pump curve, the flow rate you get depends on the setting of the valve, doesn't it? These are independent variables, you can't say we are going to keep the valve opening -- the flow rate constant to vary the valve opening.

MR. LETELLIER: These are typically constant speed pumps that rely on the valve throttle.

CHAIRMAN WALLIS: So, you have a pump curve, so as you change the valve opening you change the flow, which is the problem with the low flow rate, the small clearance, you also get a low flow rate.

DR. BANERJEE: Well, your pump cover is like this, right, so here --

CHAIRMAN WALLIS: I don't know what my pump cover is like.

DR. BANERJEE: -- well, let's assume it looks like a normal pump, okay, it's kind of flattish and it falls off. If you are in the flat portion --

CHAIRMAN WALLIS: Then you've got a constant flow rate?

DR. BANERJEE: -- more or less.

CHAIRMAN WALLIS: But, when you throttle

down you are in the other part.

DR. BANERJEE: I don't know where they are.

CHAIRMAN WALLIS: Anyway, that's another -- that's the engineering study of this.

We don't want to interrupt you anymore. We've made the point, I think.

MR. LETELLIER: Let's move on to the overview for three component mixtures, slide 20. In this case we are introducing all three of our debris types in different sequences, in order of introduction, and find that in this case there are apparent increases in valve blockage compared to the analogous single debris tests by themselves. But, no particular order of introduction seems to give us marked differences.

CHAIRMAN WALLIS: I'm sorry, I want to go back to this. You did all these tests at constant flow rate?

MR. LETELLIER: They were initialized at the same flow rate, and then they were measured.

CHAIRMAN WALLIS: I would want to go back to NRR and say, in the plant how does the flow change as you change the valve opening. Then you could figure out, perhaps, something a bit more realistic in relation to the real problem?

MR. LETELLIER: We have been asking and searching for those reality checks, if you will.

CHAIRMAN WALLIS: You were aware of this then.

MR. LETELLIER: I wouldn't say that, actually. We have not gotten all of the information that we needed to make the best possible choice of flow conditions and valve geometries.

CHAIRMAN WALLIS: This seems to be a problem then, getting them -- you guys are doing research, you need to have access right away to the necessary information.

MR. LETELLIER: It's not that we haven't tried, the staff has pursued all avenues to obtain more quantitative information. Largely, it comes from anecdotal discussions with the industry reps.

CHAIRMAN WALLIS: But, the rule of thumb is that half of what you hear anecdotally is wrong.

MR. LETELLIER: And, half right.

DR. BANERJEE: But, don't they have specs, tech specs, and things which give this stuff?

MR. LETELLIER: Flow rate is a technical specification, but the particular valve design is not specified uniquely.

DR. BANERJEE: Right, but they provided whatever it is, it's documented, enormous piles of stuff of there's.

MR. TREGONING: I think we've reached a clear limitation if we -- you know, and especially given the time constraints that we've got, you know, if we want to have a more in-depth presentation on throttle valve performance how the specs, what information we have, versus we don't have, I suggest we either defer that either to the next May committee meeting or set up a separate subcommittee meeting.

CHAIRMAN WALLIS: Well, we are going to probably have a meeting on downstream effects anyway, correct?

MR. TREGONING: It sounds like that was potential.

MR. CARUSO: We are scheduled to have another meeting like this in June, so that might be a good time to --

MR. TREGONING: Potentially, I mean, so if that's a topic that we want to pursue, we could -- I would offer --

CHAIRMAN WALLIS: I'd like to see the NRR timeline that says by a certain time we will have resolved this issue, by a certain time we will have resolved that issue, and this issue, and depending on what happens in these tests we are going to have various piles, we are going to go on.

At the moment, it seems higgledy, biggledy, I mean, they are going to present something to us in June, maybe something in September, maybe we are going to say it's not good enough.

DR. BANERJEE: I think maybe the downstream effects meeting should be earlier than June.

CHAIRMAN WALLIS: As soon as possible, as soon as possible.

MR. LETELLIER: Moving quickly through the accumulation test, which was one of the more interesting studies, it addressed a lot of the questions you've raised about long-term accumulation in a plant environment. This plant -- this test was conducted over a period of three hours, with sequential introductions of debris, at 15 minute intervals, and the trace of data on slide 23 you can see how the system responds to that in terms of delta K.

We did see a steady increase, or rather, a loss of performance over time, but each sequence or each addition of debris did not necessarily give you a measurable effect. So, in the broad picture, there's evidence of accumulation, but it's not deterministic in terms of the next incremental quantity.

CHAIRMAN WALLIS: It seems to clear itself to some extent.

MR. TREGONING: In some cases you saw that.

MR. LETELLIER: It does.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: From like a screen, where it typically goes up and keeps on going up.

MR. LETELLIER: In this case, if we had time to examine the pump curve you would see that the differential pressures are increasing, and our pump does have some capacity for that.

Also, the velocities are increasing, so it self-scours, it tends to weaken any kind of mechanical debris lodging and --

CHAIRMAN WALLIS: Do you see evidence of self-scarring at the end of the test?

MR. LETELLIER: What I'm saying is, the velocities increase, so that there's the potential for that explanation for this behavior.

CHAIRMAN WALLIS: Oh, that's a hypothesis.

MR. LETELLIER: Yes.

DR. DENNING: Well, is it hypothesis or is it pretty much clear that somehow the blockage must be reduced? I mean, isn't it clear the blockage --

MR. LETELLIER: It is clear that the blockage was reduced, and it is a fact that both the delta P and the velocities are increasing. So, if you can speculate about the physical mechanics of what occurs.

And finally, just the overview summary. Just to recap, the screen -- the penetration rates and the quantities were parameterized in this test for various reasons. We did want to challenge the system to the point of blockage. We were never able to do that because of the limited volume of the chamber, the debris chamber.

We had to do this because presently and possibly there never will be a predictive capability for debris generation transport and arrival times.

Also, there are sump screen and LOCA specific dependencies that led us to choose a parametric approach.

Our choice of a surrogate throttle valve chamber proved to be effective for investigating these phenomena. There's clearly room for improvement. For example, once the debris blockage occurred, I could not tell you if it was physically trapped in the throat or lodged in the chamber. But, we did achieve a minimum blockage detection of about 5 percent of the flow area, and again, there is no performance standard, no standard for degraded performance. The plant engineer will not tell you, is this acceptable. We were simply characterizing the sensitivity of our system.

DR. BANERJEE: But, if you get a 200 percent change in K, it goes as a square root with K, the flow rate, so it's roughly --

CHAIRMAN WALLIS: That's not so bad.

DR. BANERJEE: -- yes, who cares.

MR. LETELLIER: But again, that represents a degraded flow condition, and the plant will not tell you, the vendors will not tell you, am I safe, is this acceptable.

DR. BANERJEE: That's a separate issue, but your flow is going to drop by -- I mean, if you change this by 200 percent, your flow will go with the square root.

CHAIRMAN WALLIS: Depending upon your pump cover.

DR. BANERJEE: Yes, if your pump cover is on the flat part.

MR. LETELLIER: That's correct.

DR. BANERJEE: So, it's not a big deal.

DR. DENNING: No, but what I think that potentially is a big deal, I mean, we size the system to -- you know, obviously, there's redundancy in this kind of stuff, but, you know, I mean, I was asking myself, you know, is it a big deal or isn't it a big deal to see this, and I think that these are big enough that one worries.

DR. BANERJEE: But, they should be evaluated.

CHAIRMAN WALLIS: But you might fail to meet some success criteria.

DR. DENNING: You might fail to meet a success criteria.

DR. BANERJEE: On the surface the flow will drop by --

CHAIRMAN WALLIS: I'm trying to remember what you said about the first bullet, there's no predictive capability. Do you remember what you said?

MR. LETELLIER: No, sir.

CHAIRMAN WALLIS: I thought you said there never will be. Did you say there never will be?

MR. TREGONING: I don't think he said

never will be. We'd have to go back and look at the transcript, but I think he said --

CHAIRMAN WALLIS: I don't need to look at the transcript.

MR. TREGONING: -- not likely.

MR. LETELLIER: I did say perhaps.

CHAIRMAN WALLIS: So, what you meant to say, if you said it never will be, was that it's unlikely there will be?

MR. LETELLIER: I said perhaps there never will be a truly predictive --

CHAIRMAN WALLIS: No, I missed the --

MR. LETELLIER: -- capability.

CHAIRMAN WALLIS: -- perhaps, okay.

MR. TREGONING: It's an important omission, though.

MR. LETELLIER: But, there are obvious reasons why we had to parameterize it in this study.

To conclude, all debris combinations except CalSil alone showed evidence of a blockage potential. We are not judging severity, but we have demonstrated that the phenomena exists, it can't happen.

There is clear evidence of accumulation over longer-term tests, and also corresponding evidence of self-cleaning by whatever physical mechanism.

Our test blockage regimes did not exercise the full range of our test pump, and, obviously, they could not exercise the full range of the HPSI system.

DR. BANERJEE: I think you don't mention one interesting thing you found, which was that the upstream chamber to the stem configuration has an effect on the results, and that seems to also hold for CalSil, if I remember by just looking at the curves.

CHAIRMAN WALLIS: Now, if these things are blocking the flow area, and if you go to higher pressures than you have, and that distorts this material and jams it into the gap, then we might speculate that it would be difficult to close the valve if you wanted to.

DR. DENNING: I don't think you want to close the valve.

MR. LETELLIER: We have never intended --

CHAIRMAN WALLIS: You never want to close the valve?

DR. DENNING: Then you have total flow blockage.

CHAIRMAN WALLIS: I don't know.

DR. DENNING: It would just exacerbate the problem.

CHAIRMAN WALLIS: You never want to close the valve.

DR. DENNING: This particular valve, I don't think you do.

DR. BANERJEE: Aren't there any blocked valves after this is captured? There must be some blocked valves or something, somewhere.

CHAIRMAN WALLIS: Some other valve in series with this one, the shut-off valves.

DR. BANERJEE: Yes, but even if you look at NUKON, the 5F series show a much lower percentage increase in K. So, what you saw with RMI you see with NUKON as well. So, it seems that there's something interesting if you look at those diamonds and those triangles.

DR. DENNING: It is dramatic.

DR. BANERJEE: What?

DR. DENNING: It is dramatic.

DR. BANERJEE: It's pretty dramatic, yes.

DR. DENNING: Particularly, since it's kind of counter intuitive to me.

DR. BANERJEE: Yes, in fact --

DR. SHACK: Big shoulders give you a place to trap stuff.

DR. BANERJEE: Yes, but --

DR. SHACK: Instead of the valve itself.

DR. BANERJEE: Until you saw the results, maybe, as I said, in a pre-experiment CFD calculation you are a part of that, but without that I wouldn't --

CHAIRMAN WALLIS: I'm not sure that CFD can predict the orientation of these particles.

DR. BANERJEE: No, but it can get the vortice.

CHAIRMAN WALLIS: But, it treats them as spheres or something, which is completely wrong.

DR. BANERJEE: Whatever, but it doesn't matter, it gives you the qualitative picture, and what we look at is --

CHAIRMAN WALLIS: Well, if you accelerate an object, and acceleration is the only thing happening, it tends to orient itself so the entire mass is maximum. In other words, they will cross the flow.

So, if you accelerate it very rapidly, it's trying to turn across the flow, that may have something to do with it.

DR. BANERJEE: But, what you see is, generally, even in turbulence, that particles get thrown out of vortices, which is why you get these streaky structures. Turbulence doesn't actually mix, it's acting as a very good separator, and that's probably what you are seeing, is some segregation in the vortices.

DR. DENNING: I think now the $64 question is, I think this was exploratory, now there are results in, and so there's a question to research, and then there may be questions to NRR, you know, what does it mean, where do we go from here, do we now have to have a predictive capability here to analyze this. Does it mean more? So I guess I'd ask Ralph, I mean, what's your interpretation, where do we go from here, is there more research that's required now because things have come up, or this is kind of what you expected anyway.

MR. TREGONING: Yes, I think, you know, the next step is clearly to -- again, this was a surrogate generic study that was meant as a scoping study. The next step clearly is to look at these results and try to see how applicable they are, with consideration of plant specific or actual valve designs.

So, that's clearly the next step, and that's something that we'll be certainly looking at with interfacing or interacting with the industry about.

DR. BANERJEE: And, whether these types of pressure loss -- I mean, flow losses --

CHAIRMAN WALLIS: I'm trying to think of what it is --

DR. BANERJEE: -- can be lived with or not.

CHAIRMAN WALLIS: -- what it is you folks would -- we have three hours of the full committee meeting on these topics.

MR. TREGONING: Is that how long we are scheduled for?

CHAIRMAN WALLIS: Three hours.

MR. TREGONING: Okay.

DR. KRESS: Total.

CHAIRMAN WALLIS: And, that means that there may be up to two hours for presentation of these research results, and I think it's important that you present the committee sort of the results, not a lot of -- not a lot of spending time describing the history or how you shred it and all that, that might come up. The thing is, what's the message, yes, we have found something, we found an effect, we've got some idea of what causes it, but, you know, here are the gaps in our knowledge or something like that, so we can see where you are and, perhaps, give some advice about what needs to be done.

MR. TREGONING: For the main committee, we would certainly focus on significant findings and results, and some of the main themes. We could do this globally for every research program, or we could try to focus on --

CHAIRMAN WALLIS: I would like you to do it for the one that we didn't get to talk about today, the bypass and the screen, give us an overview of that one.

MR. TREGONING: So, for main committee, a little bit more detailed overview with respect to the bypass study, but --

CHAIRMAN WALLIS: And, more concentration on results and implications, and where do we go from here.

MR. TREGONING: Do you want us to cover all of the research areas in main committee or focus on chemical effects?

CHAIRMAN WALLIS: Well, I think everything we heard at this subcommittee from my point of view was interesting, and the one that probably, from my point of view, needs not quite so long attention is the ability to predict the chemistry, but even that's very interesting, the fact that you can predict the chemistry, it's all important.

MR. TREGONING: Okay. We'll certainly take that under consideration -- that's what we'll plan to do then for main committee.

CHAIRMAN WALLIS: The fact that you can predict the chemistry is maybe the most optimistic of all the stories we've heard, so that's a good one to put forward.

DR. SHACK: The other thing that seems strange here is, the 45L, where you have the most tests, seems to give you the most scatter. I mean, I'm not sure that if we just ran more tests everything would have an order of magnitude variation. That was particularly more scattered for some reason.

MR. LETELLIER: The 45L has a much shorter flow path length. As you raise it above the ring, then the flow geometry changes much more rapidly than in the shallow angles. It's a much longer valve contact surface, actually.

MR. TREGONING: But, if you look at the data, a lot of times we saw this with 45L, it's almost like a bifurcation occurs, where --

DR. SHACK: Well, I'm looking at the mixed RMI-NUKON one, where I go from 5 percent to 55 percent, and, you know, it's just stacked up.

MR. TREGONING: Even there, I mean --

DR. SHACK: Well, there's those two, and then there's the two up there.

MR. TREGONING: Right, so in many cases with the 45L we saw, again, almost a bimodal or bifurcation type of behavior.

DR. SHACK: Oh, I see, you are saying it either does or doesn't.

MR. TREGONING: You reach these configurations where you start to get accumulation, and then they become just very efficient catchers at that point. In some ways it's analogous to what you see across -- or it may be analogous to what you see with respect to head loss through a bed, where you --

DR. SHACK: Well, of course now, with the mixture one you've got three points spread.

MR. TREGONING: Yes, the mixture one is spread --

CHAIRMAN WALLIS: Yes, be careful about going bimodal and getting anything into that.

MR. TREGONING: Well again, I don't want to over sell it, but it is interesting that not only for these -- for at least these tests, but you see it in a lot of the -- debris tests --

CHAIRMAN WALLIS: We can take bets on when you do the next black diamond test, where it's going to be.

MR. TREGONING: We've got a pretty good range here.

CHAIRMAN WALLIS: I think we should stop and have a break. I was hoping we could take a break earlier, but we'll take a break until 11:05, and then we will hear the next presentation, which is due to take about an hour. We hope to finish by noon. I think some committee members have to leave. We are taking a break now, 15 minutes, 11:05.

(Whereupon, at 10:50 a.m., a recess until 11:04 a.m.)

CHAIRMAN WALLIS: The last, but not least, is a discussion of the transportability of coatings debris.

MR. GEIGER: Yes, thank you, sir.

CHAIRMAN WALLIS: Please, go ahead.

MR. GEIGER: Good morning. My name is Ervin Geiger. I'm with the Office of Nuclear Regulatory Research, Division of Engineering Technology, until the 19th of this month anyway when the name changes.

I'm new to the NRC, this is the first time

I've had the opportunity of speaking to this group. I'm the Project Manager, I'm the NRC's eye for this transport research.

My previous experience has been 30 years in the nuclear industry working for Bechtel Power, and I'm a Mechanical Engineer by training. I've been involved in a number of initial plant designs back in the '70s, I started in '75, so '75 through '85 I did a number of nuclear plant designs. And after that, when the construction sort of went into maintenance work, primarily in steam generator replacements on PWRs, and as part of that we did a lot of studies on some blockage, where we'd replace the insulation on the primary piping, the mainstream piping, and steam generators, we did a lot of evaluations on transport to the sumps, and that's how we selected the types of insulation. A lot of times we selected the RMI because that wouldn't transport to the -- as much as the blanket, where we felt there was plenty of margin in the sumps we chose blanket, because of its thermal efficency, and also ease if installation and maintenance.

With me is Anne Fullerton, from the Naval Surface Warfare Center in Carderock, Maryland, and they are -- that's the lab that's actually doing the

-- conducting the testing for us. I'll let Anne say a few things about her experience.

MS. FULLERTON: Thanks, Ervin.

Like Ervin said, I'm from the Naval Surface Warfare Center, Carderock Division. I work in the Special Projects Group of the Maneuvering and Control Division, and somebody said yesterday, what do like these projects here have to do with the Naval Surface Warfare Center, how is that like a ship? That's a very good question. But to us --

CHAIRMAN WALLIS: Well, if a ship ever turns into a lot of debris it might --

MS. FULLERTON: That's about it, which is an even bigger problem.

So, to us, it's just another hydrodynamics problems, and in the Special Projects Group we tend to do a lot of different kinds of research, mostly in support of ship design, but some things that are more of just the hydrodynamics problems. So, to us, this was interesting. It was something we could do, and we were happy to help out.

I've been at the Naval Surface Warfare Center for about a year. Previous to that I was at Stevens Institute of Technology. I was a graduate student there, so I have a Ph.D. in Ocean Engineering, where I did other problems related to hydrodynamics. So, that's a little bit about my background.

MR. GEIGER: Okay, thank you.

The purpose of the study was just to study the behavior of debris -- coatings debris in water, whether it was stagnant water or moving water, as it would be in a containment or in a LOCA.

CHAIRMAN WALLIS: Do you worry about how they react when they come near objects? I mean, if they are flowing in the containment building, they go down staircases, and around walls and all kinds of things.

MR. GEIGER: This study involved primarily how it behaves in still water, in other words, when it landed on the surface how quickly it --

CHAIRMAN WALLIS: So, how they behave in a pool maybe.

MR. GEIGER: How they behave in a pool, and then also under flow conditions, actually, with a steady stream flow, we didn't look at turbulences or anything, we just looked at flow rates, so one direction was flow.

CHAIRMAN WALLIS: Some of these flows on floors may be fairly shallow streams.

DR. BANERJEE: How fast were the flows,

were they turbulent? I mean, were they --

MS. FULLERTON: No, they weren't that fast.

MR. GEIGER: They were not that fast, no.

CHAIRMAN WALLIS: What was their Reynolds number?

MS. FULLERTON: We didn't calculate the Reynolds number. The tank was about 30 feet long, with a three foot by three foot cross section, and the fastest we went was about one foot per second.

CHAIRMAN WALLIS: So, it's a high Reynolds then.

DR. BANERJEE: Very high Reynolds.

MS. FULLERTON: Yes.

DR. BANERJEE: Very turbulent.

CHAIRMAN WALLIS: Unless you smooth it very carefully at the inlet, in which case it might not develop.

MS. FULLERTON: Yes.

DR. BANERJEE: Most likely it was very turbulent.

MR. GEIGER: Most likely further down, yes.

DR. BANERJEE: No, one foot high --

MS. FULLERTON: Three feet high.

DR. BANERJEE: -- three feet high.

CHAIRMAN WALLIS: It's a really high Reynolds number.

DR. BANERJEE: It's a very high Reynolds number.

MR. GEIGER: Actually, when we get into it we'll show you some diagrams of what it looks like. It's kind of a unique setup that we had because of the facilities they had. It was rather interesting.

DR. BANERJEE: The reason we ask is settling --

MR. GEIGER: Settling, yes.

DR. BANERJEE: -- is very dependent on the turbulence.

MR. GEIGER: Yes, I understand.

And again, you know, we had discussed a number of times what the reasons for these studies are, and this is, again, a safety evaluation, it considers all of the unqualified coatings in a containment, plus the qualified failed coatings in the containment to transport to the sump during a loss of coolant accident.

So, what we are trying to do is to see how conservative this assumption is, and historically a number of nuclear plants have not experienced failures in the coatings. Initially, the plants had what we considered qualified coatings. They did a DBA test, temperature, radiation, and pressure, but those were all the initial tests, and they verified that over those conditions the coating will stay on the wall. Of course, these days there have been plants where the coating is actually failing prematurely, and there's a lot of studies to look into why this is happening. Of course, this was not part of this study. What we were looking at is what happens to these coatings when they actually end up on the containment floor, and then how are they transported, if they are transported.

The information gathered from these tests will aid in assessment of the response to GL 2004-02, and further, some of this information we gathered can actually be used to look at the sump screens, and come up with parameters for seeing how the flows will affect or maybe affect the design of the sump screen, so it could be used for that.

CHAIRMAN WALLIS: So, are you going to -- I don't think you are going to show us how it can be actually used to make plant-specific predictions.

MR. GEIGER: Well, the data we gathered is very generic in nature, as to how -- so, what we did was, we studied -- if a coating landed on a surface, and the different types of coatings, what would happen, how long would they stay on the surface, would they sink, and if they sink while the stream is moving, how long it took them to actually reach the bottom, and then what would happen when they did that.

So, that was all --

CHAIRMAN WALLIS: So, you think all this could somehow be put together to provide a method for predicting what happens in a containment?

MR. GEIGER: Yes. These could all be, you know, contribute to that sort of an evaluation.

The testing is complete in the lab, and the data is now being analyzed and evaluated. We took an awful lot of data, so it's taking quite a while to put it all in a format that we are sure we can present it.

The NUREG is scheduled to be published some time this fall, fall of 2006, so we are moving pretty well along with it.

DR. BANERJEE: You looked at qualified and unqualified.

MR. GEIGER: Yes, we did.

CHAIRMAN WALLIS: But, your sizes, they are like flakes, they are not the tiny little particles that we heard about yesterday.

MR. GEIGER: Well, they range, yes, and I will pass -- we tested -- what we did, the concept for the testing was, it's a not plant-specific test. We are looking at gathering, you know, just establishing certain parameters on behavior of these coatings, that then could be applied in a number of ways. Okay, it's just basically a study of what happens to coating chips -- different types of coating chips, sizes, and densities. So, we looked at -- we studied --

DR. BANERJEE: Inside the ZOI or outside the ZOI?

MR. GEIGER: Anywhere.

DR. BANERJEE: Anywhere.

MR. TREGONING: Primarily, outside the ZOI, I mean, within the ZOI assumptions are made that it's particulate.

MR. GEIGER: Yes, what we studied is, we did the -- the assumption is that in the ZOI, the paint is basically small, micron size, right, okay, so we didn't study those because when we did this facility we didn't have the capability anyway, it really wasn't the intent to study that, because all those particles are assumed to go to the sump, it would be entrained, so what we are looking at is larger particles that were outside the ZOI but maybe on the floor from previous failures, or could be washed off the walls and structures by spray. So, that was really the intent of this study.

MR. TREGONING: And, the concern now is with ECCS, there's a very conservative assumption that says any unqualified coatings, or previously qualified coatings that have visual evidence of degradation, are assumed to fail. So, that can be a very large coating loading potentially that needs to be designed around. So, transportability was obviously an obvious question for these, and those failures are expected to be larger size than just particulate.

DR. BANERJEE: Yesterday we heard from the industry, right, about the experiments they've been doing.

MR. TREGONING: Well, yes.

DR. BANERJEE: So --

MR. TREGONING: Initiating. These are complimentary, because they are looking at more failure mechanisms and failure amounts. These are looking at transportability.

Did you want to say something, Matt?

MR. YODER: Yes. As I said yesterday, this testing is confirmatory testing.

CHAIRMAN WALLIS: You have to identify yourself for the record.

MR. YODER: I'm sorry, Matt Yoder, from NRR.

This is confirmatory testing, so as I said yesterday, if the licensee were to take exception to the position the staff has and they were to try to assume some debris characteristic size of a chip, and then try to save it, that would not transport, or would only transport X distance in their containment.

This is the testing that the staff would use to inform our evaluation of that.

DR. BANERJEE: And, in fact, in this pre-testing, if I recall, it didn't measure the particle size.

MR. YODER: Again, the testing you are referencing was just of unqualified coatings.

DR. BANERJEE: Yes.

MR. GEIGER: And, that wasn't transport testing, that was just to see if it failed.

DR. BANERJEE: Yes, I realize what it was, so this is complimentary.

MR. GEIGER: Yes, this is complimenting that, yes.

MR. TREGONING: But again, Matt had a good clarification. This is intended, primarily, for informing the staff on the evaluations.

MR. GEIGER: So to continue, we studied five different coating systems, and I will discuss them in the next few slides, and we brought some samples we'll pass around to show you what we did.

We studied the three sizes ranging from two inch down to 1/64 inch in size, and the debris shapes were random. We had randomly generated the shapes in a commercial blender, and we tested curled chips, flat chips, and some we did temperature curing to --

CHAIRMAN WALLIS: So, you chopped up these sheets of paint and then you somehow segregated them into the different groups by -- not by picking out each one.

MR. GEIGER: By chipping and sieving.

MS. FULLERTON: No, we used sieves.

MR. GEIGER: So, the samples were manufactured by applying the coatings to a polyethylene sheet, and letting it cure, and then it was peeled off, and then broken into sections and shipped to us.

DR. BANERJEE: So, this was almost like a parametric study, not necessarily what you expected would happen in the steam environment at LOCA conditions.

MR. TREGONING: Yes, this is totally parametric. All the testing was not -- was done at room temperature water, so it was not done at LOCA, under LOCA pool conditions.

DR. BANERJEE: But we haven't seen a report on this yet.

MR. TREGONING: You are not even going to see data on this.

MS. FULLERTON: No, we just finished this at the end of January.

MR. GEIGER: We just finished the testing.

MR. TREGONING: This is as fresh as fresh can be.

MR. GEIGER: We are anxious to see, you know, we are anxious to get it out to everybody, so we are working diligently on it.

The chips, as they were applied, there's a range of specific gravities, and they range from one for the alkyd to about 2.6 for zinc epoxy system, and we studied different thicknesses, these were applied, we have a one coating system for 1 to 3 mls, and then we also have a six-coat epoxy which we studied, which is a much thicker chip, and then we studied it in water velocities where we had stagnant water in a stagnant pool, and then we had velocities up to one feet per second.

So, Anne is going to pass around the samples.

DR. BANERJEE: There are all sorts of alkyds, right?

MR. GEIGER: Oh, yes, there are.

DR. BANERJEE: Each manufacturer does a different one, correct?

MR. GEIGER: There's many formulations, so we couldn't really -- and that's why this is parametric, because there's so many formulations of epoxy, the alkyds, the zincs, so that it would be pretty much impossible to study them all. So, what we did was, I mean I think the primary mechanism that's involved is the specific gravities, as to how readily, you know, it sinks, and then the shape factor, and just the thickness, which is part of shape factor, so those will contribute quite a bit to the characteristics of how it will flow.

MR. TREGONING: And the size, those were the principal variables.

MR. GEIGER: And the size, thank you.

MR. TREGONING: So, yes, we couldn't test everything, so we tried to span the range of --

DR. BANERJEE: So, you took some representative alkyd and --

MR. TREGONING: -- yes, it was certainly representative, and again, it would have had a density similar to most, or specific gravity similar to most other alkyds.

MR. GEIGER: And, we picked the manufacturer that we found was used quite a bit in the plant, and this was all unqualified. The alkyd was an unqualified coating.

DR. BANERJEE: So, let me try to understand this, because if I was in industry, I would try to take my piece of alkyd or something, do a little test on it, see how much came off, and then have some characterization of particle size.

MR. GEIGER: Correct.

DR. BANERJEE: And then, you would have some sort of relationship that I could use to find out how much would get to my screen. Is that the whole -- because they don't seem to be doing that part of the testing, they are only doing --

MR. GEIGER: No.

DR. BANERJEE: -- how much falls off, right?

MR. GEIGER: Right.

DR. BANERJEE: At the moment, that's what they seem to be doing, unless they are doing also some transport stuff, I don't know.

MR. YODER: Matt Yoder, let me -- I think what you see is for a study like what you've been referencing for the past couple days, the EPRI report, where they have some rough idea of the particle size you expect, to make some kind of analysis, and that may or may not include testing to say that those particles will transport or not. And, the staff would use this testing to say whether we found that credible or not.

DR. BANERJEE: All right.

MR. TREGONING: There has been prior transport testing of coatings that have been done historically, so this isn't the only study of its kind, by any stretch of the imagination. And, I think a lot of the plants, as Matt said, once they get their loading they would use CFD with some assumptions to determine really what transports or not.

MR. CARUSO: Quick question. Which one of these samples is the highest density?

MS. FULLERTON: That's the zinc.

MR. GEIGER: The zinc, it has a silver

packing.

MS. FULLERTON: Yes, that one.

MR. GEIGER: Yes, that's it, yes.

MR. CARUSO: This is the highest density?

MR. GEIGER: Yes.

MR. CARUSO: And the stuff that --

MS. FULLERTON: That's the alkyd.

MR. GEIGER: I was going to label them, I'm sorry, I didn't have a marker.

And, let me just clarify, as to exactly how this information will be used is up to NRR. Okay, we are presenting some data and NRR is going to use that to evaluate the submittals.

CHAIRMAN WALLIS: You could have a contest for designing the chip which will go the furthest, like a paper airplane, because some of these might, if launched the right way, go quite a way.

MR. GEIGER: Potentially, yes.

So, we tested an alkyd, and zinc primer, and then with an epoxy -- two epoxy topcoats, the nomenclature, ALK is the alkyd, the zinc primer has the ZE, then we had an epoxy primer, and the epoxy topcoat, which is the E2, and we had an epoxy that was six coats, now that's not a qualified coating system, but we were trying to replicate the maintenance in the plant where they applied a lot of coats.

DR. BANERJEE: So, do these float like leaves, do they get wet?

MS. FULLERTON: The latest one does, the one that looks like a garbage bag when you see it, the other ones mostly sink.

DR. BANERJEE: They sink.

MS. FULLERTON: Yes.

DR. BANERJEE: So, you gently put them on the surface.

MS. FULLERTON: Yes, we did -- yes, a couple different tests, but yes.

MR. GEIGER: Okay.

Then we tested an epoxy sealer with a surfacer and two epoxy topcoats, which is typically used on a concrete surface, so there's five coating systems we used.

And, the specific gravities ranged from about 1.0 to, like I said, 1.58 for the zinc epoxy system. So, I mean, and those are a combination, you know, we checked the entire volume to see the weight, it wasn't the individual components.

CHAIRMAN WALLIS: So, what you passed around is one inch to two inch, is that right?

MR. GEIGER: Yes -- well, those are

actually --

CHAIRMAN WALLIS: Bigger.

MS. FULLERTON: One to two inch.

DR. BANERJEE: What is this ultra think stuff?

MR. GEIGER: That's the alkyd.

MS. FULLERTON: Alkyd.

MR. GEIGER: That's the alkyd, and that typically comes from, it's equipment furnished by manufacturers.

We tested one inch to two inch size chips, and then we tested one sample of 1/8 and 1/4 size chips.

CHAIRMAN WALLIS: What is typical of the containment?

MR. GEIGER: The containment, well, as you can see, some of these are pretty brittle, and if you look at the pictures of the containment, they tend to come off in sheets, but I think --

CHAIRMAN WALLIS: Those are the thick ones in there, in the containment?

MR. GEIGER: Yes, the two coat -- typically, the zinc and the epoxy, the two-coat epoxy was typical in the picture.

CHAIRMAN WALLIS: So, they might come off in sheets, as you say.

MR. GEIGER: They may come off -- not in large sheets, but from photographs I have seen, you know, they start to curl and peel, but they are rather brittle, so if they would fall I imagine they would break into different types of shapes.

MR. CARUSO: All these tests were done at room temperature, right?

MR. GEIGER: Yes.

MR. CARUSO: You didn't do any tests at higher temperatures?

MR. GEIGER: No, we didn't, no. We did presoak some at 140 degrees, so we did that. We took some samples, before we put them in the water, we presoaked them at 140 to see if they would change their shapes or anything like that.

DR. BANERJEE: I have a question. Obviously, for the lighter things, if you put them on the surface, surface tension will have a very important effect on the wetting characteristics. And, obviously, if you have surfactants and things in the water, this will start to effect things.

So, did you look at, at least in broad terms, whether the wetting characteristics might change with containment-like water, compared to just normal water? Whether it floats or not really will depend on how it is wetted in the early stages.

MR. TREGONING: Yes, there's a lot of conditions that will determine whether --

DR. BANERJEE: Yes.

MR. TREGONING: -- it floats or not, including how agitated the surface of the pool is.

DR. BANERJEE: Sure.

MR. TREGONING: As it fills up, or even as something is traveling over the surface of pool, so we didn't look specifically -- there were no measurements of surface tension, for instance, done on these tests, and there were no --

MS. FULLERTON: Actually, there were.

MR. TREGONING: Oh, I'm sorry, I'll just be quiet then.

MS. FULLERTON: That's okay.

MR. GEIGER: We measured surface tension in the tank, and, of course, we didn't mix any chemicals, there was just tap water, it was tap water.

DR. BANERJEE: So, at least you knew whether there were surfactants, whether there were monolayers, and that sort of stuff.

MS. FULLERTON: That's correct, and there are a few different tests done. We did a time to sink test, and Erv will talk about it a little bit more, but we did some tests where we placed the paint chips on the surface and saw how long it took for them to sink. And, we also did some tests where they were placed just under the surface, and we measured the terminal velocity of the chips. So, there are different, a few different measurements, so that there would be something, you know, including the effects of surface tension.

MR. GEIGER: We did some pre-wetted.

DR. BANERJEE: Well, the Navy should know about surface tension.

MS. FULLERTON: That's right, we do a lot of other things with surface tension.

MR. GEIGER: We did the mixture, and the mixture was -- the definition was, basically, it had to be between 10 percent and 25 percent chips smaller than a 1/4 inch, and that was the ranges, and the rest would be larger chips.

We did two types of testing. We did what I said, I mentioned, about quiescent zinc testing, where we had a vertical acrylic tank, and I said we measured the surface tension, and then what we did was, the test, we did a time to sink test and a terminal velocity test. The time to sink test was, it consisted of dropping the coating chips from a distance of one foot above the surface onto the surface, and then timing how long it would take for it initially to sink, and then for the remainder to sink to the bottom.

CHAIRMAN WALLIS: You must have had a lot of orientation with the chip?

MR. GEIGER: Yes, but it was random.

MS. FULLERTON: We had to make it random, because it definitely did make a difference how it fell.

CHAIRMAN WALLIS: The terminal velocity, these things don't fall just like a sphere, I mean, they --

MS. FULLERTON: They have no sideways component, right.

CHAIRMAN WALLIS: Do they hit the wall of the tank, it looks like a pretty small tank.

MS. FULLERTON: No, no, we would put them in the middle of the tank, and if they did hit --

DR. BANERJEE: It's a flume.

MS. FULLERTON: No, that's in the second one.

MR. GEIGER: We did a quiescent --

MS. FULLERTON: It's lab space, and

there's the --

DR. BANERJEE: This was a stirred tank or a quiescent?

MR. GEIGER: This is quiescent, this is totally still, yes.

And then, we did the transport test, which was in a flume, where we measured the tumbling velocity which consisted of initially placing the chips on the bottom and then increasing the flow rate until the chips initially started moving, we called that the incipient tumbling velocity, and then we cranked it up to see 80 percent of bulk tumbling velocity.

DR. BANERJEE: Did you put any on the surface of the flume, like the water surface?

MR. GEIGER: We initially were going to do that, but from the quiescent testing we saw how they behaved, so we knew that, you know, if you put them on -- like the alkyd, if you put it on the surface, it all went to the end.

MS. FULLERTON: It would just all go to the end.

DR. BANERJEE: It would what?

MS. FULLERTON: It would just stay on the surface.

MR. GEIGER: It would stay on the surface and transport.

MS. FULLERTON: So, we knew what would happen.

MR. GEIGER: The heavier chips, most of them, even on the quiescent test, we put them on the surface, they very quickly broke the surface and sank. There were some, occasionally one would remain.

DR. BANERJEE: But, the reason is in a quiescent tank they would sink a lot faster than in a turbulent flow.

MR. GEIGER: Sure.

MS. FULLERTON: Right.

DR. BANERJEE: So, did you do some experiments in the flume where you put --

MR. GEIGER: Yes, we put them right on the surface, yes.

MS. FULLERTON: They were --

MR. GEIGER: That's the other one, that's the steady state transport test, where we took -- where we came up with the tumbling velocity, and then we used that as a factor for selecting a -- well, we tested it at the tumbling velocity and a lower velocity, right?

MS. FULLERTON: Lower velocity, yes.

MR. GEIGER: So, we tested what the transport was at those, at the tumbling velocity. We put them in just -- introduced them just below the surface, and studied the way it went.

DR. BANERJEE: All right.

MR. GEIGER: Okay.

And then --

MS. FULLERTON: So, they still had about three feet to fall, maybe a little less, 2-1/2 feet.

MR. GEIGER: And, we selected 0.2 feet per second, because actually we got very encouraging results from the tumbling velocity test as to what it really took to move these, so we initially were going to do it at 0.1 feet per second, but then, you know, we felt pretty comfortable using that higher, to try to get an upper range.

So, that's the quiescent test.

So, that, basically, is the facility.

MS. FULLERTON: There's a camera over here that was used to record the images, this is a tensiometer up here to measure the surface tension.

CHAIRMAN WALLIS: Surface tension doesn't change much, does it?

MS. FULLERTON: Yes, this is a pipe that was one foot, we dropped the chip one foot from above the surface, so that we were consistent in putting the paint chips in.

DR. BANERJEE: Okay.

MR. GEIGER: Transport testing, and we can go to the next slide and she can explain it.

DR. BANERJEE: You didn't use your model basin, huh?

MR. GEIGER: Part of it.

MS. FULLERTON: We considered a lot of options, because this is something a little different than we do. You know, we needed a smooth bottom. We needed it to be very clean, and we needed to be able to see in from the sides so that we could take the images to track the particles.

CHAIRMAN WALLIS: So, you dropped a dry chip in from one foot?

MS. FULLERTON: Yes.

CHAIRMAN WALLIS: There's almost no place in a containment where a dry chip is going to fall from one foot.

MS. FULLERTON: Presoaked, right, they were presoaked chips.

CHAIRMAN WALLIS: It was presoaked.

MS. FULLERTON: Right. The time to sink test, I believe, were all presoaked.

CHAIRMAN WALLIS: Presumably, in reality they'd fall from 30 feet or something.

MR. GEIGER: Well, that's true.

MR. TREGONING: But then, they'd end up --

MR. GEIGER: -- terminal velocity.

CHAIRMAN WALLIS: I mean, with water, yes.

DR. BANERJEE: You never know.

MR. GEIGER: Okay. This is the transport tests. This is the diagram. So, we had a 30 foot long flume, and we had a filtering system and segregation system at the end, so that we could see what portion of the fragments actually floated to the end and what entrained in the middle, and then what transported along the bottom.

And, these are two views of the flume itself. Actually, one of the water tanks that they have out there is circulated water, channels with -- and the technician could go out on that platform and actually place the chips in the bottom or drop them at the top. And, this is an end view so you can see.

Do you want to explain a little bit about this?

MS. FULLERTON: Sure, just a little bit more.

What we did have was, we had ordered a 30-

foot long acrylic tank, so that we would have access to the side, and originally we were planning on making more of a stand-alone apparatus that we would just have piping and a pump that we'd be able to set up the currents that we would need inside the tank. And then, we decided we already had a circulating water channel that was capable of this feed range that we were looking for, so we took the acrylic, the smallest tank in this schematic here, and we actually suspended it in the circulating water channel, so we could control the velocity from the channel, but we were able to put our cameras under water to look along the side. And, we had access to the top part, we were able to keep it clean and smooth and recover the paint chips from it.

So, that was how we set it up.

DR. BANERJEE: You just put it into one of your basins.

MS. FULLERTON: Yes.

MR. GEIGER: How accurate was your flow control inside?

MS. FULLERTON: I think it was -- it's within a tenth of a foot per second.

CHAIRMAN WALLIS: You tested all these different things.

MR. GEIGER: Yes, the next three slides I just put in here to give a feeling for how many different tests we actually ran, because you see all the chip sizes we ran, and we ran five different samples, dry and presoaked, we did a terminal velocity test, and also for the large epoxy-based systems we did -- we tested an oven cure, where we oven cured it for 48 hours at 120 degrees and --

CHAIRMAN WALLIS: These velocities are all one particle at a time?

MR. GEIGER: Yes. Well, one -- a batch.

MS. FULLERTON: We would track one particle at a time.

CHAIRMAN WALLIS: Throw in a batch of chips to measure the velocity, or just one?

MS. FULLERTON: Into the transport flume, we would put in --

CHAIRMAN WALLIS: Into the plexiglas tank.

MS. FULLERTON: -- oh, we would put not just one at a time. The time to sink we would drop one at a time.

CHAIRMAN WALLIS: One at a time.

MS. FULLERTON: The other, the terminal velocity, we would put as many as we could without having them touch each other.

CHAIRMAN WALLIS: So, there's a group of them.

MS. FULLERTON: There's a group of them, yes, but we were able to --

CHAIRMAN WALLIS: Were you able to relate that to how one falls?

MS. FULLERTON: We were able to track them separately.

CHAIRMAN WALLIS: Did you relate that to how one falls?

MR. GEIGER: There was only a group --

CHAIRMAN WALLIS: Did they touch each other?

MS. FULLERTON: No, well, and if they did we would ignore it, because the point of the testing was just to see if the behavior of one individual chip, and not how it interacted with other chips. So, we were always careful to try to make sure that they wouldn't interfere with each other.

MR. GEIGER: So, this is like for the quiescent test, the different tests we ran, and then the tumbling velocity test, and again, we ran the incipient and the bulk.

For the steady state again --

CHAIRMAN WALLIS: If you dropped two

chips, one behind the other, one is in the wake of the other one, they do interfere with each other.

DR. DENNING: You are sure that they weren't close enough that -- I mean, you dumped them together, I mean, you would expect bubbles.

MS. FULLERTON: Well, for the time to sink test, they are all on the surface, so I suppose if one moved over and was close to the other one, but they were varied apart. With the time to sink, we did try to wait a long enough time that it wouldn't be in the wake of another chip.

CHAIRMAN WALLIS: Yes, I can see that.

MS. FULLERTON: But, I suppose it's possible, yes.

CHAIRMAN WALLIS: When they are falling.

MS. FULLERTON: Right.

CHAIRMAN WALLIS: Terminal velocity test, were groups of them or one at a time?

MS. FULLERTON: Well, it would be one group at a time. I guess to clarify, so you would put like say five large chips on the surface and let them fall. And, if it moved over, you know, presumably, they are all separated.

CHAIRMAN WALLIS: So, there are very few chips.

MS. FULLERTON: There are very few chips, right.

MR. TREGONING: And, the tumbling velocity test, obviously, all the chips were introduced on the floor before flow was started.

CHAIRMAN WALLIS: Okay.

MR. GEIGER: Okay. Now, Anne will describe how the software they used, and the methodology they used, to actually run the tests.

CHAIRMAN WALLIS: Do you have any results to show us?

MS. FULLERTON: No, not at this time.

MR. GEIGER: We do have some --

MR. TREGONING: Just observations.

MR. GEIGER: -- we have some observations.

MR. TREGONING: Again, these tests were just completed, you know, a week or so ago.

CHAIRMAN WALLIS: So, we don't know if they made any sense in terms of interpretation yet?

MR. TREGONING: We are still making sure that we understand that as well.

MS. FULLERTON: Okay.

So, as Erv said before, we used a blender to get the size chips that we were looking for. Then we used sieves to segregate them into size classes.

Then we would also -- before each test we would take an image of the sample on a contrasting sheet of paper underneath, and we used some chip sizing software to get the average chip area, the major and minor axis, and a few other characteristics about the chips.

Then, when we actually --

DR. BANERJEE: So, how did you characterize it? I mean, you have an area and the perimeter, is that what you did?

MS. FULLERTON: We have an area and a major and minor axis, so the smallest axis and the largest axis.

DR. BANERJEE: So, you didn't measure a perimeter, because in --

MS. FULLERTON: But, we could, we have the images.

DR. BANERJEE: -- in a regularly-shaped device --

MS. FULLERTON: Right.

DR. BANERJEE: -- there's an issue as to how you characterize it.

MS. FULLERTON: Right.

DR. BANERJEE: So, if you got the perimeter and the area you'd get a fractal dimension for it.

MS. FULLERTON: Right.

DR. BANERJEE: Maybe you can use that.

MS. FULLERTON: We could use that.

DR. BANERJEE: Yes.

MS. FULLERTON: And, we have the images, so we could, you know, go back and get the perimeter from those images.

DR. BANERJEE: Yes.

MS. FULLERTON: Basically, the point of the sizing software was to make sure that what we had in the sieves was what we thought we had, you know, that would make sure that we had what we were looking for.

Slide 17 has a histogram, just a characteristic histogram for 100 chips for an E3C time to sink test. That was the one that has the white one with the ridges on it, so it's just an area histogram, and major axis histogram.

We also used some different --

CHAIRMAN WALLIS: So, the one which is 2600 square millimeters is a pretty big chip.

MS. FULLERTON: Right, and there's only a few of those, so there were some larger.

CHAIRMAN WALLIS: So, when you say you got them in the range of one to two inches, this is really a five-inch chip or something, isn't it?

MS. FULLERTON: It could be, I don't think so. I mean, it had to pass --

CHAIRMAN WALLIS: Maybe I'm wrong.

MS. FULLERTON: -- it had to pass through the sieve.

CHAIRMAN WALLIS: Okay, okay.

MS. FULLERTON: One axis of it has to be one or two inches. It's possible the other axis was bigger.

CHAIRMAN WALLIS: All right.

MR. GEIGER: It does represent the random size in the plant.

MS. FULLERTON: Right, and, you know, we could have made them all squares, but we were trying to make them as irregular and random as possible.

We also used some software to track the chips, so during the transport testing we had four cameras, two looking above, two looking from the side, and we also actually had an extra camera at the end, which was looking at the capturing device at the end of the filtration system.

DR. BANERJEE: They have to be in your field of you, were you moving the whole group of cameras with the chips?

MS. FULLERTON: No, the cameras were fixed, but looking at different places within the tank, so that we would be able to capture them.

And, we had two cameras right at the starting line, or whatever you want to call it, and making sure that when we would introduce the chips that they were always in that -- we had marked off a certain area.

CHAIRMAN WALLIS: It's just like a ski race, isn't it, you start them --

DR. BANERJEE: You have your basin structure, you move the cameras, you know --

MS. FULLERTON: With a carriage, right.

DR. BANERJEE: Yes.

MS. FULLERTON: But, in the actual basin that we used, the circulating water channel, the whole point is to keep the model fixed and move the water past it, so there's not that kind of a thing there.

DR. BANERJEE: Okay.

MS. FULLERTON: If we wanted to use one of the bigger basins --

DR. BANERJEE: Yes.

MS. FULLERTON: -- it's not really good for this kind of test.

MR. TREGONING: It would have been over

kill for this test.

MS. FULLERTON: Yes.

So, we would take these images of the chips and use this chip tracking software, which would calculate the area of each chip.

So, what we would do is, find the center of each chip and track every frame. We would use a camera that took 30 frames per second, and from that we are able to get the velocity of each chip.

So, we'll go on to the movie now. This is from the quiescent tank. Now, we can track both components of that velocity, too.

DR. BANERJEE: It's falling leaves.

MS. FULLERTON: Right, and you definitely see a lot more of that in the larger chips than you do with the smaller chips.

CHAIRMAN WALLIS: But, they are in a tank, so it doesn't affect the wall repelling them that would keep them from going off on a long trajectory sideways, which they might do in a big tank. I don't know.

MR. GEIGER: They are sort of coming down in the middle.

CHAIRMAN WALLIS: I know they are, but that's because they are -- it could be because they are in the tank.

MR. TREGONING: If there directional flow and other things associated with your actual conditions, yes, that could dramatically affect how far they transport.

CHAIRMAN WALLIS: No, I mean if there's no flow at all, and if there's something, a wall effect which repels them, I'm just hypothesizing that.

DR. BANERJEE: Well, there will be a wall effect. Were these all in the center, more or less?

MS. FULLERTON: Pretty much, yes, they didn't get very close to the wall.

MR. TREGONING: Can you characterize that, what was --

MS. FULLERTON: Measurement?

MR. TREGONING: What is the frame to view of the -- this is a one to two inch chip, so it looks like --

MS. FULLERTON: Yes.

MR. TREGONING: -- you've got about, what, a six inch --

MS. FULLERTON: It's about -- it's probably not even that, it's probably more like four inches.

MR. TREGONING: And, what was the

dimension of your tank again?

MS. FULLERTON: It's about one foot by one foot.

DR. BANERJEE: Yes, so if it's toward the center it will be minimized.

MS. FULLERTON: Right.

DR. BANERJEE: But, if it gets near the wall, obviously, there will be an effect.

MS. FULLERTON: Right, but then it would be out of the camera view, so we wouldn't be able to track it.

MR. TREGONING: Did you notice that during any of the time to sink test?

MS. FULLERTON: Most of them, because we were dropping them in the center, so --

CHAIRMAN WALLIS: These are ones going down one after the other, so they are falling through the wake of the other one, which is sharing vortices.

MR. TREGONING: No, this is a repetitive loop.

MR. GEIGER: There's two different chips here, I think.

CHAIRMAN WALLIS: You watch the same one time, after time, after time?

MS. FULLERTON: There are two chips, two

different chips, but it's --

CHAIRMAN WALLIS: There are two that look different. But, it just keeps on repeating and repeating?

DR. BANERJEE: It is two chips following each other.

MR. TREGONING: We can't lock up the ACRS computer with ABI files, so that's why it's relatively small.

CHAIRMAN WALLIS: But, you see what we are getting at, is that if there are two, then one is in the wake of the other one.

MS. FULLERTON: Right. Yes, this is played at half speed.

MR. TREGONING: So, they are actually coming faster than this. You can see the smaller ones are --

CHAIRMAN WALLIS: So, it's interesting, they do want the fall flat, they don't want to go, you know, sideways.

MS. FULLERTON: Sideways, right, unless they hit the surface sideways, then they'll go straight down.

DR. BANERJEE: But, that's a very unstable configuration to actually go straight down.

MS. FULLERTON: Right.

DR. BANERJEE: A small perturbation will take --

CHAIRMAN WALLIS: They will move across the flow.

MS. FULLERTON: Well, these are also the E3C, which has the ridges.

CHAIRMAN WALLIS: Those were accelerated through a valve, that's different.

MR. TREGONING: Oh, okay, apparently.

MS. FULLERTON: So, Ervin, I don't know if Ervin mentioned it or not, but for the quiescent testing we also did an experiment with thermal cure on some of the chips, to see if that would change any of the characteristics.

So, for all except the alkyd sample we heated them in an oven, one for two weeks at 150 degrees, and one for two days at 120 degrees, to see if there were any effects. There were no significant effects, all the effects were within the standard deviation of all of the other testing. So, that was the only information we collected with that thermal cure was for the quiescent testing.

DR. BANERJEE: What happens if you wet the alkyd, would it sink?

MS. FULLERTON: Yes.

MR. GEIGER: No, no.

DR. BANERJEE: Like sometimes leaves, they will sink.

MS. FULLERTON: No, when they were dropped onto the surface they were not, yes.

MR. GEIGER: We also did like on the epoxies, some of them we did a curl -- we heat curled them and put them on a drum or something to get them to -- to try to get them so we could study the shape effects.

CHAIRMAN WALLIS: They were dry when they landed, wouldn't they, or were they not dry when they landed?

MS. FULLERTON: They are dry when they -- when we introduce them.

MR. GEIGER: They don't soak up water.

MS. FULLERTON: No.

So, we would take the water out and separate them, and then put them in. So, there would be some water on them, but not -- it wasn't like they were in a water mix.

DR. BANERJEE: Is the alkyd's true density higher than water?

MS. FULLERTON: It's just about the same.

MR. GEIGER: The manufacturer says 1.15 specific gravity, we measured about one.

DR. BANERJEE: Okay, so that explains that.

MS. FULLERTON: So --

CHAIRMAN WALLIS: Heavier samples fall faster is not being contentious, is it?

MS. FULLERTON: No, stating the obvious.

MR. TREGONING: We have general agreement with that.

MS. FULLERTON: And, the alkyd didn't sink when we dropped it on the water. We covered that.

So, we have another movie of the tumbling velocity tests, these were when we would replace the chips along the bottom of the tank and then slowly increase the speed of the flow.

CHAIRMAN WALLIS: Are they being picked up?

MS. FULLERTON: To see what velocity --

MR. TREGONING: You see them lifting.

MR. GEIGER: These are two views, from the top --

MS. FULLERTON: And from the bottom.

CHAIRMAN WALLIS: Now, once they are picked up and get oriented around, then they could shoot up to the surface, couldn't they?

MR. GEIGER: Well, I don't think --

MS. FULLERTON: That didn't --

CHAIRMAN WALLIS: You never saw that?

MS. FULLERTON: Not to the surface, some would get --

CHAIRMAN WALLIS: But, they do shoot up some ways.

MS. FULLERTON: -- some would get suspended, especially the lighter ones, the alkyds.

MR. GEIGER: If you look at it this way to see how some of them lift up, they just kind of go along the top.

DR. BANERJEE: It depends on the specific gravity and the size. What's happening is that the turbulence, which is catching it in an ejection --

CHAIRMAN WALLIS: They seem to get their back end picked up, so the lift that picks them up, and then now you see it on the right there.

MR. CARUSO: What samples are these?

MS. FULLERTON: These are the E6, which is an epoxy.

CHAIRMAN WALLIS: Once it gets lifted up --

MR. CARUSO: What specific gravity?

MS. FULLERTON: I think it was 1.75.

DR. BANERJEE: Pretty dense.

MS. FULLERTON: Yes.

DR. BANERJEE: So, the smaller -- if you made them smaller, did they get entrained up further?

MS. FULLERTON: The smaller ones, yes, would get entrained, more than the larger ones.

CHAIRMAN WALLIS: Did it come back down again, I think that the condition for one lying on the ground to be picked up is a rather different condition for one in the flow to land, because I noticed that once they get off the bottom they get across the flow, and the force on them goes up tremendously.

MS. FULLERTON: Sure, they orient themselves.

CHAIRMAN WALLIS: So, whether or not they will come down and settle is quite different.

MS. FULLERTON: Getting picked up, and also --

DR. BANERJEE: The wall structures, you know, the ejections will take them off, and what brings them down are the sweeps.

MS. FULLERTON: Yes.

DR. BANERJEE: So, what you see, the lighter particles will get -- I mean, not lighter, but the smaller --

MS. FULLERTON: Smaller, right.

DR. BANERJEE: -- they will go higher, and then if they come down they'll come down with a sweep.

MS. FULLERTON: Right.

DR. BANERJEE: Real fast. So, you know, the wall structures --

CHAIRMAN WALLIS: This is a turbulent flow.

DR. BANERJEE: There are periodic ejections scaled with the wall sheer, so you can probably find the frequency with which this would happen.

CHAIRMAN WALLIS: I think they are very unlikely to come down just so that they land and stay in sort of a protected little boundary there.

MS. FULLERTON: Some of them would.

DR. BANERJEE: It will, because --

CHAIRMAN WALLIS: Well, they have to land just right. If they land on the tip, then the force is going to blow them away.

MR. CARUSO: And, the surface has to be relatively flat.

CHAIRMAN WALLIS: Right.

MR. TREGONING: But, this is very smooth.

MS. FULLERTON: Yes, it was very smooth.

DR. BANERJEE: In reply to Graham's question, if you -- depending on the level of turbulence you have, these can actually -- the sweeps can go right to the surface -- I mean, the ejection, so you could take them almost up to the surface, depending on how turbulent the flow was.

MS. FULLERTON: It would be very rare to see any of that, that went up to the surface.

DR. BANERJEE: You had --

MS. FULLERTON: No, that was the highest, some of them are much lower. The alkyds were much lower, more like .3 feet per second, the curl chips were lower, it was easier to pick up a curl chip because you already have some of the area in some cases oriented in the direction of the flow. So, it's easier to pick them up.

MR. CARUSO: What velocity is this?

MS. FULLERTON: This I think is about one foot per second.

MR. CARUSO: One foot.

MS. FULLERTON: Yes.

CHAIRMAN WALLIS: And, is it changing, or there will be just different ones picked up because of the turbulence?

MS. FULLERTON: It is changing very slowly, so we would start out at a lower speed and slowly increase, and we are measuring velocity at the same time, water velocity.

CHAIRMAN WALLIS: Why did you call this pick-up velocity or entrainment velocity?

MR. TREGONING: It's just an historical term.

CHAIRMAN WALLIS: It's misleading.

MR. TREGONING: It is, it's a bit misleading, but it's an historical term.

DR. BANERJEE: There's a huge literature on this area.

CHAIRMAN WALLIS: I would want to know what happens to them after they are picked up, do they ever -- if they are picked up at one foot per second, do they ever land again?

MS. FULLERTON: Yes.

CHAIRMAN WALLIS: They do, do they land and stay?

MS. FULLERTON: They do -- well, very -- not very many of them, most of them would get transported to the end, and end up on the screen once they get picked up.

MR. GEIGER: At this velocity.

MS. FULLERTON: At this velocity, right.

CHAIRMAN WALLIS: Okay.

Well, you are going to sort all that out.

MS. FULLERTON: Right. We have to -- we have a lot of data to go through.

MR. TREGONING: Again, there were other tests where they injected paint into the flow with a certain velocity, and those test are more illustrative in terms of what the transport distances are like before settling occurs, or I'll say should be more illustrative.

CHAIRMAN WALLIS: I guess most of the literature with regard to rods and spheres, and it's flat shapes are not that often studied, it's sort of interesting.

MS. FULLERTON: Yes, it is. I did a little bit with sediment transport, and this is a lot -- definitely much bigger than I had ever dealt with before. But, not quite a ship.

CHAIRMAN WALLIS: So, you did do sediment transport?

MS. FULLERTON: No, it was on -- just on estuarine circulation.

Some preliminary observations, again, very preliminary. The flat ALK debris had the lowest tumbling velocity, so that was about .3 feet per second. The E6 and the zinc had larger tumbling velocities. Again, these are the heavier chips, so that's pretty obvious. In general, the curled chips had lower tumbling velocities than the same debris flat chips, because they are having more of their area into the flow, and very little transport at the .2 feet per second, which was the lower velocity that we tested. Most chips did not make it all the way to the end, except for the ALK, more of that transported in general.

SO, in summary, the testing is complete. We just finished it about two weeks ago. We are in progress for data analysis, and the report will be available from NRC in fall, 2006.

CHAIRMAN WALLIS: So, you are doing the usual thing, doing the tests and then trying to figure out what it means.

MS. FULLERTON: That's right.

MR. GEIGER: Mostly to make sure we have all our data, you know.

CHAIRMAN WALLIS: Well, for the report you want to have it right, and I would hope that you were actually trying to plug some data and think about it while you are doing the testing.

MR. TREGONING: No, we did, of course we did that.

MS. FULLERTON: Right, we had to do that for -- especially for the tumbling velocity, because that's what we tested at the following week.

MR. TREGONING: We modified the matrix fairly significantly, based on some of the initial results from both the time to sink and the settling velocity.

DR. BANERJEE: Typically, what was the -- I mean, just -- is there a correlation between -- let's say in the quiescent tank, between the area of your flake and the settling velocity, in rough terms? Can you give us some preview of this? Say the same material, the same --

MS. FULLERTON: So you are asking, the smaller chips are sinking quicker or slower?

CHAIRMAN WALLIS: I would think it's about the same.

MS. FULLERTON: It's about the same, but there is the sideways component.

CHAIRMAN WALLIS: Mass goes as area, and drag goes as area, so you don't think it's the same.

DR. DENNING: The sideways components are the bigger.

MS. FULLERTON: The bigger chips, the sideways component is bigger, which gives us --

CHAIRMAN WALLIS: It's like going down a half pipe.

MS. FULLERTON: Right.

MR. GEIGER: The smaller chips we are doing that a lot faster.

CHAIRMAN WALLIS: Yes, well you may want to correlate the frequency.

DR. BANERJEE: Well, the drag goes with the area, right?

CHAIRMAN WALLIS: And the weight goes with the area too.

DR. BANERJEE: But then, the sideways motion.

CHAIRMAN WALLIS: It might make a difference.

DR. BANERJEE: It's worth understanding that.

CHAIRMAN WALLIS: So, this is very fundamental stuff, and we have to -- somebody has to figure out how to apply it.

MS. FULLERTON: Right.

DR. BANERJEE: One thing that you can do relatively easy is to put a little turbulence into your quiescent tank with a moving grid.

MS. FULLERTON: Yes.

DR. BANERJEE: There's a huge and beautiful set of experiments done by Faeth and -- General Fluid Mechanics, where they look at the effect of turbulence on settling velocity. It's homogenous turbulence, and I think that would be interesting for you to read.

MS. FULLERTON: Okay, sounds good.

DR. BANERJEE: There's two papers, that's F-A-E-T-H.

MS. FULLERTON: F-A-E-T-H.

CHAIRMAN WALLIS: How much spread is there in the results? Is there a lot of statistical variation if you take, say, 100 flakes that look similar and do a test of terminal velocity, is it a pretty narrow distribution?

MS. FULLERTON: For the tumbling velocity?

CHAIRMAN WALLIS: Right, or for anything.

MS. FULLERTON: Well, and we did about, I think, 50 chips for each for the tumbling velocity.

The quiescent testing, there was more -- I think a higher deviation from the average for the larger chips than for the smaller chips, and we thought that maybe that had something to do with that sideways component that was a little bit different. But, we got pretty consistent results for -- we had good repeatability. We tested many, many chips.

CHAIRMAN WALLIS: Any other questions?

Thank you very much.

MS. FULLERTON: Thank you.

CHAIRMAN WALLIS: Do we have a wrap up now from the RES?

MR. TREGONING: Quick wrap up.

CHAIRMAN WALLIS: Then we go for lunch.

MS. EVANS: Okay. My name is Michelle Evans. I'm from the Office of Research. I'm a Branch Chief in the Division of Engineering Technology.

I just want to summarize what you heard with regard to the research in the last day and a half, talk a little bit about where we are headed, and maybe how the committee could help us.

First of all, I'd like to thank the committee for the opportunity to present our research over the last day and a half. We appreciate the opportunity for your dialogue, and incites, and questions.

Research invested a significant amount of resources in this research over the past year. Several of the things that you heard about yesterday were just implemented, or initiated, in the last year. the ICET testing yesterday, and also the downstream effects throttle valve work that you heard about today, those tests were -- that research has been going on for a few years. However, the rest of this research was initiated about a year ago.

At that time, management had made decisions about the scope of the research that we would move forward with, and the basis for the scope was a balancing of the need to move forward on the designs by the industry, how research could best support NRR in their review of licensee submittals, what the NRC's role is in the research versus what industry's role is in conducting related research, also balancing the resources and the timing of everything.

So, about a year ago there was a decision made to fund this research, and to move forward with the idea that it would be completed in the spring of this year, but that decision was revisited in the fall of 2005, and again additional resources were made available to complete the scope of the testing that you've heard about in the last day and a half here, to complete it by April.

CHAIRMAN WALLIS: You wanted to finish

this work by now.

MS. EVANS: We want to finish what you've heard about.

CHAIRMAN WALLIS: And, it looks as if quite a bit of the work is, in fact, raising more questions, rather --

MS. EVANS: True.

CHAIRMAN WALLIS: -- than resolving issues.

So, is it finished?

MS. EVANS: Is it finished? This scope will end --

CHAIRMAN WALLIS: Are you going to cut it off before you've really solved the problem?

MS. EVANS: No. The scope of work we've laid out, we still intend to finish what we had laid out in April.

CHAIRMAN WALLIS: Yes.

MS. EVANS: And, I understand additional questions may have been raised.

Our intent is to be done with this current research and to allow the industry to move forward with what they are doing, come forward with their submittals, and once we see those and NRR has had the opportunity to review what comes forward, there may be a need to do additional research, but we are not at a point right now where we are committing to do that.

We have looked internally in the Office of Research and NRR staff-wise at, you know, potentially, we've got a wish list on additional research that could be useful, but there are no commitments at this point that we would do any of that until, you know -- the need may arise, but at this point we are not moving forward with any additional research.

I think we've got commitment to meet again with the committee to talk more about the results of the research. I believe we've got a June subcommittee meeting. We'll come forward then with any additional significant findings and results over the next couple months.

There's also a Chemical Effects Peer Review, which we will be wrapping up in the next month or so, so we'll be at a point to present that to you in June.

CHAIRMAN WALLIS: Can I ask you something now?

MS. EVANS: Yes.

CHAIRMAN WALLIS: We heard this morning about the flow paths through the core.

MS. EVANS: Right.

CHAIRMAN WALLIS: Are you doing any research, have you done any research on what would happen if you got debris in the core?

MS. EVANS: No, we haven't.

CHAIRMAN WALLIS: Do you plan to do any?

MS. EVANS: We've looked at the need to, you know, and the thought of doing that has entered into discussion, yes.

We have a budget process where we look at what may be on the horizon in '07-'08, and, yes, that is definitely an area that may need additional research. But, no, we haven't committed to do that at this point.

As you heard, the WOG and industry, they've got their approach that they are working through, and NRR staff is working with them to understand that.

So, at this point is there a clear need? No, but is there potential? Yes.

CHAIRMAN WALLIS: Do you get involved in reviewing the WOG guidance, or work, whatever we call it, the WOG work that we saw on what to do with the debris in the core, did you folks review that at all, or it just went to NRR? Did you review it from the point of view of saying, when they say calculate this, did you review it from the point of view of what's to say you ought to be able to calculate what they suggest you calculate?

MR. TREGONING: Shanlai Lu is going to elaborate a little bit on that.

CHAIRMAN WALLIS: This is an NRR problem, it's not an RES problem, so it's not something that RES has been doing, reviewing the WOG.

MR. LU: The WOG is planning to submit a report to us, that's not here yet.

CHAIRMAN WALLIS: What you've seen is a draft.

MR. LU: Yes, what we saw was just draft, and it's coming in.

MR. SCOTT: It's coming in this month, right, I think by the end of February.

MR. LU: Yes, by the end of February they are going to have us our review from the NRR side. So, I think we probably will talk to Rob and Michelle, once we have more questions about that.

MS. EVANS: At this point, as far as the committee goes, and what we've presented, you know, information on our research, we are looking for the committee to give your view on the credibility of what we did and told you about over the last day and a half.

We do understand that there --

CHAIRMAN WALLIS: Do you want a grade on credibility? That's a dangerous thing to ask for.

MS. EVANS: Well, we are very interested in the incites that the committee has to offer, so, yes, we would accept a review on credibility. We also would --

CHAIRMAN WALLIS: I think more the issue is adequacy.

MS. EVANS: Okay.

We also understand that things may be missing, there may be areas we haven't touched on, and like I said, we also internally have identified potential future research also. But, at this point in time the decisions were made to, you know, allow the industry to proceed down the path that they are heading with the submittals, and additional decision on additional research be made at a later time.

MR. SCOTT: At the appropriate time, I'd like to present a few closing remarks.

CHAIRMAN WALLIS: Sure, maybe now is the time.

MR. SCOTT: Okay.

We also would like to thank you all for

the opportunity to present our views and what's going on with the issue, and to hear your views and perspectives on GSI-191.

We would like to, and are happy to, facilitate your continued review. We are ready and willing to meet with you at an appropriate time to discuss follow-on issues, including the downstream effects. As Michelle mentioned, and as was discussed, we are expecting a WOG report on downstream effects, and we'd be happy to discuss the staff's review of that at an appropriate future time.

You asked, Dr. Wallis, a couple days ago, how you can help in this area, and we've kicked that around a little bit. We have some thoughts.

First of all, as I think Michelle referred to also, we would greatly appreciate the committee's perspectives on the chemical effects issues, as clearly was brought out again and again this week, these are very complex issues, and we would appreciate your input on those.

Would like to address one remark that you made a little earlier today, regarding the NRR dates for issue resolution. Please remember that the process that we've presented to you is a process that involves, rather than research providing sufficient information by itself to resolve the issue by a date certain, research has provided perspective, and is providing perspective for the staff. The staff also expects that the industry will provide the information in addition to what research has provided sufficient to resolve the issue.

So, when the industry submits its responses to the requests for additional information that we are sending out to them regarding the generic letter, we will have that information from research in hand to provide additional perspective and confirmation for the staff's review.

We did not expect, and this process has not been developed to have research take the whole burden on.

CHAIRMAN WALLIS: You folks have sketched out all the strategy. I'm just thinking about history. Does history show that given a complex issue like this, with a whole lot of interacting, not very well understood phenomena, does history show that relying on industry to solve it works?

I remember when we were dealing with LOCAs, the Agency actually made a commitment to do the fundamental work so that we understood what was going on and could make the right decision, and there have been other examples where the Agency has said, we've got to take the lead because it's too disbursed to go out to the industry and so on.

But, in this case you seem to be always saying, well, it's up to industry to solve it, come back, and we're going to somehow judge it. Does history show that that approach works?

MR. SCOTT: I'm going to dodge that issue a little bit, but I would say that if industry --

CHAIRMAN WALLIS: I'm thinking of the record, because someone might read the transcript to get the answer to this.

MR. SCOTT: I don't have the answer to it.

CHAIRMAN WALLIS: No.

MR. SCOTT: But, I would add the following, and I think Michelle referred to it as well. If the industry submittals come in, and we identify in our review of those submittals that there is an issue with the way that we've gone through this, then that may need to necessitate a change in course.

So, we are not going to be, you know, fixed on this to the end, unless the approach works, clearly.

One more point I'd like to make is, we would appreciate the committee's support for what the staff believes to be a prudent course of action regarding enlarging the strainers. We recognize that these are very complex issues that are going to take a long time to resolve, and the uncertainties are large and are likely to remain so for some time. The staff believes that the enlargement of the strainers at this time in parallel with the continued development of information by research and by the industry that this is a prudent activity to undertake.

We expect, and have communicated to the licensees, that as they make modifications to their strainers that they will fully assess downstream effects, such that the installation of larger strainers does not inadvertently cause a problem with downstream effects.

And, that concludes my remarks, subject to your questions.

CHAIRMAN WALLIS: Well, the issue of enlarging strainers, the prudent course of action, presumably, has to be taken in the framework of understanding of the NRC's philosophy and methods for ensuring public safety, not just within the sort of technical judgment of people like the ACRS.

So, it seems to me that we can certainly tell you something about what's involved with enlarging strainers, but in terms of what's the prudent course of action we may be getting outside the terrain in which we are comfortable and confident to maneuver.

MR. SCOTT: I understand that. Some of the remarks that were made earlier today and yesterday led us to believe there were some concerns in that area, and we're trying to address those.

DR. DENNING: Well, I think that the concern is -- relates to downstream effects, and what we've heard today, or the last few days, indicates that there really has not been enough research examination of downstream effects at this time. We haven't seen the evidence, and we hear things like, well, there are going to be plant-specific, and we'll review what the industry provides, and this type of stuff, I don't think that that is a prudent way to go forward.

I think that you ought to be looking at some examples today to see if we really understand what the implications are of potentially large loadings of material passing through the screens with larger screen areas. That seems -- I mean, and I don't speak for the Advisory Committee clearly here, but it certainly seems like enlarging the screens is a very good idea, relative to many of the other concerns that we have, but the one area that hasn't been, I don't think, adequately looked at before one could answer that question is the downstream effects.

MR. SCOTT: One point I didn't make, but should, is that the staff's technical adequacy audits that we will be doing on the industry's work will provide an opportunity to focus on just the same thing you talked about.

CHAIRMAN WALLIS: I think there's going to be a tradeoff, that you have too big a strainer it's probably bad for downstream effects, and if you have too small a strainer it's bad for NPSH. How do you make the decision, it has to be based on some kind of a measure of success, or some measure of risk, or something, where you see one going down, one going up, and somewhere there's a minimum of some kind of value of something.

MR. SCOTT: Yes.

CHAIRMAN WALLIS: Now, has the Agency ever tried to address it that way, I don't think you have, I haven't seen any kind of a prospective on what's the increased risk of this versus the decreased risk of that, and how does it all balance, and this is how we are going to make a decision.

MR. TREGONING: But, wait a minute, I mean, that's one way to solve it, is trying to optimize it, and there may be an optimal screen size.

CHAIRMAN WALLIS: How else would you do it, when you've got to trade off things against one another?

MR. TREGONING: Well, you can have a non-optimized design that still may satisfy the regulatory requirements that are -- again, there's quite a --

CHAIRMAN WALLIS: So, how are you going to decide what those requirements should be then?

MR. TREGONING: Well, we are not changing the requirements to meet 5046 as part of this exercise.

CHAIRMAN WALLIS: The core remains --

MR. SCOTT: That's the whole basis for the success criteria for this entire exercise that we're undertaking.

CHAIRMAN WALLIS: -- well I think --

MR. TREGONING: To ensure that we can meet 5046.

CHAIRMAN WALLIS: -- you may be trading off uncertainties, too, in which case it is a tradeoff.

DR. DENNING: Should we do some summary?

I mean, there are some summary statements I'd certainly like to make.

CHAIRMAN WALLIS: I think we can have some summary statements now. What I would really like from you and Sandra, I've got some from Tom, maybe I'll get some more, is some written stuff, which I can -- which will help me to write a report or a position of at least the subcommittee. I guess it would be very appropriate to give some final remarks now.

DR. DENNING: Okay.

Well, let me say that with regards to things we heard on the first day, I have serious concerns, and I'll talk about those a little bit more, in terms of the regulatory approach, not because -- I think it's being driven by an industrial approach that I don't think is the right way to go forward, and so I'll comment a little bit more on that later.

With regards to the last day and a half, I have been very impressed by the research that is going on. I think that almost uniformly the things that I heard are important things and the quality of the research is excellent, but it is done, and to think that we are bringing it to a conclusion in April to me just seems premature.

One of the reasons that I think that it

isn't done is that, I think that my greatest concern about the approach that I see to closure, as it now exists in the industry's mind, and I think in NRR's mind, is one in which there's a lot of post testing, as we get down to this last stage of the overall analysis, and you look at what happens at the screen, what I heard was an integral post test approach idea as to how to do that, and I think that that integral proof test, without a real understanding of phenomenology and without models is very difficult to support technically. And, I think that the importance of the research that we have going on is that it does give NRR the ability to make -- to ask the right questions of industry.

I think at the moment, because we haven't really brought the research together, we don't really have models, and when I say models I don't want to imply that I think that we can have first principle models for all of these things, but I do think that we have to take the results that we are getting and develop models of some degree, because without those I don't think you have an understanding, or we can't -- we don't really have a technical understanding of what's going on.

So, the value of the research, obviously, is to provide tools to NRR. I don't think that those tools have been really drawn together. I think that we are on our way to doing that, but I don't think we are there, and I don't think we are going to be there for another nine months from what I'm hearing. So, I think there's a lot of analysis of the experimental work that's been done, a lot of putting it together with interpretive tools, so that NRR has the right tools to challenge what is presented to them by industry.

That includes on the chemical effects side the difficult problem of bringing that into the pressure drop. Now again, I don't really believe that we are going to have a correlation where you are going to dump in some chemical effects and come up with a modification to a head loss correlation, but you are going to have to have some quantitative understanding of its impact, so that you can get -- can develop alternatives, technical alternatives, like the removal of TSP, perhaps, control of amount of aluminum, those types of things. But, in order to be able to develop those as technical alternatives you are going to have to have some quantitative drawing together of these results on the chemical side.

And again, I heard things that I thought were definitely headed in the right direction, but the thought that one could -- that we are ready by April to do that, we definitely didn't hear that.

I guess the final thing I would say is, and maybe I am repeating myself, and that is that we need some perspective on these downstream effects, and I think that just taking some cases and having either research or NRR personnel run through some cases, and see how much debris are we really talking about downstream in the screens, and where does it wind up, and what happens in cold leg breaks versus hot leg breaks, that we've got to do some of that thinking in advance and not waiting for an audit of an industry analysis.

CHAIRMAN WALLIS: Thank you.

Sanjoy, would you like to give some closing remarks?

DR. BANERJEE: Sure.

Just a few things. First, I think that we should divide the problem into before the strainers and after the strainers, and my impression is, we know quite a bit about what happens before the strainers and, perhaps, even when they get on the strainers, but not that much after the strainers. And, both are important problems.

I feel a little uncomfortable with letting industry come and do their testing, and then we examine these tests and say are they good enough or not good enough.

I think we should sort of interact with them, even at this stage, to give them some feedback as to whether it's likely to be adequate or not, because if they have done all these tests with all these screens, what they are really doing is they are taking pieces of screen, taking a particular situation, a particular flume, looking at the dropout, perhaps, trying to get some benefit for the near field effect, whatever it is, it's a pretty ad hoc approach, and it could be that when we examine these later down the line we'll find it's not adequate, in which case they've spent a lot of money doing plant-specific work, even making modifications to plants, which may turn out to be inadequate, or to lead to another set of problems.

So, I do think that they have been doing -- they have been going this course for 30 years now, or whatever, some period of time, the issue is, is it such a big problem that they need to immediately increase these areas? Is it such a big safety issue? And, I can't judge that. If that's felt that it is such a big safety issue, then, of course, we should do whatever we can based on our judgment today to take care of this.

But then the understanding should be clear that this may not be the answer, you know, that eventually they make these MODs, they increase the area, but that may lead to a whole new set of problems, which we need to take care of in the future.

So, I would just feel a little more comfortable if there was some more time, I don't think the problem is going to be solved completely, but it could be that there are some innovative ways to take care of this. I imagine you can think of several, which is other than just increasing the screen area. It might be able to take care of both problems, and it might be a design solution, rather than a solution which is based on analysis.

So, my sense of this problem is that we are still some ways from resolving it. I do feel there is need for more research, particularly, after the screens, if you like. I do feel that you made very good progress on some areas of research, which can support NRR.

I'm particularly happy with what I saw on the chemical analysis part, the modeling there, and a little bit more work there I think would be very helpful to just close that out in a nice way.

I think you are making very good progress on the studies you are doing on blockage, probably the answer you will get is one that nobody will like, that there's no way to actually handle the chemical effect without having a very high pressure loss, however big the screen is, if it covers the whole screen, unfortunately, I think it's going to be clogged. So, one will need to take care of the chemical effects in some other way.

But, you know, without saying there's any guidance, one way might be to remove the aluminum ladders, or scaffolding, or whatever. The other way might be to find a new buffer, other than trisodiumphosphate. Who knows, I don't know the answers, I'm just throwing these out.

So, there may be innovative ways to take care of these problems, so that -- and you've identified this very nicely I think. So, I'm just -- I feel a little bit uncertain about going ahead and just examining what industry submits, and saying is it good enough or not to meet the safety goals that we have, because first I don't think we have enough information to make that judgment very well right now, second I think getting that information isn't a long way off, and we could probably get a better, more safe design eventually, and NRR might have an easier job in handling this in the future if we just were a little bit more cautious on this, and took a little more time.

So, that's my view.

CHAIRMAN WALLIS: Thank you.

Well, I don't think I'm going to give you a definitive judgment. I agree in many ways with what my colleagues have said, and I have spoken out during the meeting here. You can read the transcript.

It would appear that we have a long way to go in terms of a technical understanding, an ability to predict things, quite a few things associated with this problem.

We are continually finding new information, which indicates that we didn't have it before, therefore, we would previously be making decisions based on something other than information, and some of this new information includes significant surprises, which indicates that we are not towards the end of a research program.

I feel that the downstream effects, particularly, what happens in the core, are an important issue. I'm surprised it hasn't been addressed with the same kind of vigor and questioning that the sump screen blockage is being addressed with.

I suspect that when we do begin to investigate downstream effects, we may find the same kind of lack of information, and the same kind of unexpected results in some areas, that we've been finding with the screens.

And so, if I were making a decision on this matter, I'd be very nervous about making any decision at the moment, and what I would look for, I think I've said this at times in the proceedings here, is what I would try to look for would be a way around the problem which was less subject to uncertainty, where I could be clear that I knew what I was doing.

Now, I'd invite my colleagues to send me written statements, which I can use helping guide the full committee. The full committee is going to, I understand, give you a letter, and then we'll be responding to what we see as the stated things. It won't be a prescription for what you ought to do, or a decision on the size of strainers or something like that. It's going to be a much more preliminary type of judgment, I think, saying this is where you are, clarifying the situation, rather than saying it's clear to us what you ought to do or where you ought to go, because I don't think it is clear just what he path should be, and there seems to be a hope that giving it to industry, and having 69 plants all submit different ad hoc experiments and calculations, is the right way, and that seems to me a very big risky thing to attempt to do.

Now, maybe if industry can get together as a group and solve some of these problems, that would be a step forward. But, I would be very nervous to have an individual plant, with the resources they have, come up with a really convincing answer to the question, what should we do.

MR. SCOTT: Can I make a couple of clarifying remarks, Dr. Wallis, or a couple of points?

One, regarding the point you just made, of course, the individual plants are not 69 of them contracting people to do testing. They settled on five vendors, and the vendors are doing the testing, so it's not like there are 69 different sets of tests involved.

CHAIRMAN WALLIS: But, by the time that they -- they are going to interpret those for their plants.

MR. SCOTT: Correct.

CHAIRMAN WALLIS: So, I guess, there are different tests, but there may be some things which are so plant specific that they will actually require plant-specific tests.

MR. SCOTT: Yes.

CHAIRMAN WALLIS: I mean, the plant that has acres of aluminum is going to be very different from all the other plants, if it has that. I expect by now they don't have it anymore, but there may be some plants which, because of the way the architect engineer has arranged things, that you just cannot do something with what's in the sump, for instance. There may be some solutions which are not available to some plants, and by the time you put together all these things you may end up with a lot of -- quite a few rather unique situations.

MR. SCOTT: Well, maybe tech staff can correct me if I state this wrong, but I would say that we're going to have 69 plant-specific solutions based on five basic approaches provided by the vendors.

CHAIRMAN WALLIS: And, I think that's part of my colleagues nervousness, is whether those basic tests are going to be general enough and have -- so that they can be used for different -- somewhat different situations from the tests.

MR. SCOTT: Which brings me to another clarifying remark Dr. Banerjee made regarding observation or, let's say, waiting on the industry to come forth with test results. The staff is not doing that. The staff is out while the industry is doing the testing observing the tests. We have a number of trips that have happened in the past several weeks, and a number more on the horizon. So, we are not just waiting for them to send something in, just to clarify that.

DR. BANERJEE: Yes, I know that you are watching these tests, I'm more concerned that implicitly almost agrees with this approach, because part of it, you see, at least in my view, is that approach is fraught with a great deal of difficulty in terms of acceptability, because they are taking little pieces of the screen, or whatever, putting it in the flume, and then putting some stuff, which is depositing out, hopefully they don't ask for too much near field effect credit, but that, when you take it to a real situation, imagine the top hat strainers now, there are a whole area of these, so you've taken a little piece of screen and you've tested it, but there are all sorts of potential blocking effects which occur.

To give you an example, when you stack the strainers in the design that went into Vermont Yankee, they did testing of one clean strainer. Okay. When you stack them the stuff gets in between, and it builds up a bed. So, they used the approach velocity for a clean strainer to get the pressure velocity.

As you build up the bed, your approach velocity completely changes. So, they submitted the calculations based on the approach velocity for a clean strainer, but in reality it was the circumference and the approach velocity was up by a factor of ten. So, when you actually take the real configuration it's very different from doing a little test on this sort of thing, and they have no way to go from that test to the full scale system. So, where is the bridge, you know, and the way they do it, if you look at their -- you have to actually read their reports to understand what they have done.

So, for Vermont Yankee we did that, we went back to every report and we read it. Otherwise, it's very difficult to understand what they are doing, and it's a very dangerous thing to say even implicitly that these tests actually are going to be even relevant.

You almost are saying, okay, if you do

these tests, we are observing them, it's going to be okay.

You know, maybe I'm interpreting it wrong, but there is a whole lot more than testing a little piece of thing and saying this is the pressure velocity.

MR. SCOTT: You are probably not going to find this a highly satisfying answer, but we don't -- I mean, I agree with what you just said, however, we expect the industry to provide that bridge, and we will look at it and see if we agree that it is adequate.

DR. BANERJEE: And, there will be 69 different bridges for 69 different plants.

MR. SCOTT: Five different bridges with variations.

DR. BANERJEE: No, there will be pretty big variations, because the way they put those top hats, and if you take a top hat it will make a big difference, and each plant will have a different way of doing it, of course.

So, I think this is a very, very dangerous approach.

MR. LU: Well, in terms of scaling, I think you have a very valid point, I agree 100 percent with your point there. That's the reason we raised the issue last March when we first saw the test, and at my recommendation the staff started to have tours to look at the vendors testing as early as we can, and actually that's what we have been doing during the past year, to make sure that all five vendors are doing the right tests and following the right procedures.

In terms of scaling, you have a valid point, that's exactly what we have been after the industry to provide that bridge, and what we are looking for is that five vendors, now it all depends, if you want to take the credit from the near field sediment, most of them -- actually, some of them are dumping out all the debris on the strainer surface. So, if those are scaling issues, some plants have to address that. And, they are looking for answers from the five vendors to address those issues

CHAIRMAN WALLIS: I think we are going to have to stop. We are not as a subcommittee now trying to solve the problem for you by discussing it with you, but --

MR. SCOTT: May I make one more clarifying remark, please?

CHAIRMAN WALLIS: Yes. You can make

another clarifying remark.

MR. SCOTT: Okay. Final one, I promise.

What I wanted to emphasize here, the remark was made, or there was a statement regarding whether the staff believes that the enlargement of the strainers is the final answer to the problem.

We certainly do not at this point take that position. We fully recognize that as we get smarter and as the industry gets smarter on this issue, that there may need to be additional changes. So, we are not by any means at this point saying that the installation of larger strainers is the end of the problem.

CHAIRMAN WALLIS: I am trying to think of what letter the committee will write. What I'm anticipating is that we will write a letter which is not wordy, and it will have some very crisp statements about how we think you are going in various areas, and there may be just three or four sentences, and those that will be memorable, and recognizable by the reader, rather than having our usual letter where we have a couple of crisp sentences and a lot of explanation.

That's my anticipation at the moment, and i may be completely wrong.

Thank you all very much, it's been a very interesting and useful meeting on a very important problem. Thank you. We'll now close.

(Whereupon, the above-entitled matter was concluded at 12:35 p.m.)