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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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WEDNESDAY
FEBRUARY 15, 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 PRESENT:

RALPH CARUSO	Designated Federal Official
RALPH ARCHITZEL	NRR
MARK CUNNINGHAM	RES
MICHELLE EVANS	RES
B.P. JAIN	RES
PAUL KLEIN	NRR
WILLIAM KROTIUK	RES
SHANHAI LU	NRR/DSS/SSIB
PAULETTE TORRES	NRR
ROBERT TREGONING	RES

ALSO PRESENT:

JACK DALLMAN	LANL
CARL ENDERLIN	PNNL
VIJAY JAIN	CWRA
BRUCE LETELLIER	LANL

C O N T E N T S

PROCEEDINGS

(8:30 a.m.)

CHAIRMAN WALLIS: Good morning. Anything you say now will be on the record.

This is the second day of the meeting of the Thermal Hydraulic Subcommittee of the ACRS. We are looking forward to presentations on research and actually just seeing some figures and data points and that kind of thing.

We're also happy to see Mark Cunningham once again before us, but in a new role, and I believe you have a few words to say.

MR. CARUSO: I have one thing to say. We've got a speaker phone set up today because we received a request from Research to set up a line in case they have some contractors at one of the great national laboratories who wants to chime in with a pearl of wisdom.

CHAIRMAN WALLIS: So this is connected to a national lab right now. Is that --

MR. CARUSO: It's connected to a bridge line, and whenever the national labs wake up.

CHAIRMAN WALLIS: Well, if it's on the West Coast, they must be up pretty early.

Okay, Mark. Go ahead.

MR. CUNNINGHAM: Good morning. My name is Mark Cunningham, the Director of the Division of Engineering Technology in the NRC's Office of Nuclear Regulatory Research.

It's a pleasure to be here before the committee in a capacity other than risk analysis or security analysis, believe me.

(Laughter.)

MR. CUNNINGHAM: For the past several years, as you know very well, the ACRS has been involved with discussions with the staff on a number of issues related to formation of chemical byproducts, and the adequacy of data for predicting head loss due to debris accumulation in some screens.

I'm coming into this late in the game, hopefully near the end of the staff's discussions on this issue, but we'll see.

The role of research in these is to provide and support NRR with technical information that allows them to make regulatory decisions. For the vast variety of decisions they have to make, we're focusing on providing technical information on five particular areas. One is chemical byproduct formation. One is the transport of insulation debris and paint chips to the sumps. The third is the head loss associated with sump screen clogging. The fourth is the correlations, numerical correlations for understanding head loss, and the fifth is related to downstream effects, potential effects of debris passing through the sumps into some throttle valves and other equipment downstream.

Over the next two days you will have the benefit of expertise from a number of different organizations that we have working in concert with the staff. It includes the Center for Nuclear Waste Regulatory Analysis at Southwest Research Institute, Argonne National Laboratory, Pacific Northwest National Laboratory, Los Alamos National Laboratory, and the Naval Surface Weapons Warfare Center, the Carderock Division, local here.

CHAIRMAN WALLIS: What does that have to do with the problem?

(Laughter.)

MR. CUNNINGHAM: They're dealing with the issue of paint chip transport and things. They're good at understanding how things go through water.

I should note, and I think we'll touch on this later, that with respect to the chemical effects part of this, recognizing the complexity of it, we're having a peer review performed which Rob will touch on.

Our research is again, as I noted, intended to support NRR's making regulatory decisions. In that context, we provide more generic information so that it should be clear that the information you'll hear over the next few days does not provide sufficient information for any plant specific decision that has to be made either by the staff or by licensees.

And we look forward to a continuing dialogue with the committee. These are very, very complicated issues, and we appreciate the insight the committee provides to us.

With that, I'll introduce Rob Tregoning. Rob is kind of the technical ringmaster in all of this. So he'll have an introduction, and he and Michelle Evans over on the staff will be the kind of co-leaders throughout the discussion of the next couple of days.

thank you.

CHAIRMAN WALLIS: As a ringmaster, what kind of a show have you got today? Have you got lion taming?

MR. TREGONING: I don't know if that characterization is accurate. I would say maybe wild bronco riding would be more appropriate here.

Thanks, Mark.

I'm going to be following up Mark with an overview of the research activities that we have ongoing supporting the Generic Letter 200402 resolution.

This presentation is merely just to set the stage for the good stuff that we have coming over the next day, day and a half. So my goal and my objective is to be as brief as possible and turn it over to what I think are the real stars of the next day, day and a half that are going to be providing a lot of very good, detailed, technical information to address some really thorny issues that we've been dealing with.

So the objective of the research presentations that you're going to hear over the next day and a half. Each of the programs will be discussing the motivation, objective and goals for the research initiatives. They'll be providing overviews and discuss interrelationships among programs where it's appropriate. You'll see that we have several overlapping initiatives ongoing at different labs. So coordination and cooperation has been a fundamental consideration as we go through these research activities.

I'm going to talk a little bit about regulatory coordination and peer review.

DR. BANERJEE: How many people are working on this?

MR. TREGONING: How many people or how many labs?

DR. BANERJEE: People, bodies.

MR. TREGONING: Oh, geez, that's almost a semi-rhetorical question. I would guess each of the labs probably has a staff of ten or so people that are supporting this. We're doing work at five labs.

DR. BANERJEE: Fifty?

MR. TREGONING: That's a rough guess. I mean, we've got NRC-wide just in research, we have a team of about six people that work pretty close to full time on this if not. So, yeah, we've got a fairly large staff of expertise that we've assembled in a relatively quick manner, and we're trying to do a lot of things in parallel to support the resolution schedule.

Okay. Again, each of the research programs here in number four will be, as I had mentioned, outlining for each specific program the objective motivation and intended regulatory use of the information being developed as part of that program. They are also going to go, each of the labs and project managers, will go into their technical approach, but a primary focus is going to be to summarize important results, observations, and analysis conducted to date.

These are status report presentations. Nothing that you're going to hear over the next day and a half is completely finished. The ICET program is probably the one that's the closest to being finished at this point.

All of the other programs are in progress. So, again, these will clearly be status reports and not final findings and conclusions and analyses will be discussed. Some programs are more mature than others. So some you might not hear any results. Some you'll hear quite significant volume of results.

And the other thing that each of the project managers and presenters will do will be to provide the plan and schedules for the remaining work that needs to be completed before we can wrap up each of these projects.

I wanted to provide a little bit of a research philosophy for how we identified and selected not only research topical areas but programs to pursue. I think this is well recognized and certainly ACRS has done a very good job at pointing us towards some issues that we need to provide more technical insight and understanding. So it's certainly widely recognize that some research is necessary to address some important technical areas that we have within the generic letter resolution.

What we've tried to do is we've tried to pick research topics to focus on technical areas having the highest uncertainty, and how have we sought out what that uncertainty is?

CHAIRMAN WALLIS: Those are better words than reduce uncertainty because you might in your experiments find that there's more uncertainty than you thought.

MR. TREGONING: That's certainly possible. That's certainly a possible outcome.

CHAIRMAN WALLIS: Your focusing on the highest uncertainty makes sense.

MR. TREGONING: We're focusing --

CHAIRMAN WALLIS: But promising to reduce uncertainty is something difficult to deliver.

MR. TREGONING: Did I say that on my slide?

CHAIRMAN WALLIS: Yeah, it says to reduce uncertainty, yeah. You're going to make the data come closer together, are you?

I understand what you're doing.

MR. TREGONING: Well, we have the potential to reduce uncertainty. You're right. We could do --

CHAIRMAN WALLIS: You need to understand the uncertainty.

MR. TREGONING: We need to understand it. That's true.

CHAIRMAN WALLIS: Get a handle, measure it, and so on.

MR. TREGONING: That's entirely true.

So how have we tried to determine which technical areas have the greatest uncertainty? Well, there's been a lot of interaction over the prior two years between not only ACRS comments and recommendations; also quite a bit of interaction with staff, both Nuclear Regulatory, NRR staff and staff within the Office of Research.

And we certainly discussed with industry quite a bit a lot of these thornier issues. So we've searched out areas that have high uncertainty, but the other constraint that we have is we're trying to focus on areas where we think generic evaluation will provide the most impact.

I think you heard a lot of the phrase "plant specific" bandied about quite liberally yesterday, and it's true. I think if you look at this problem in total, there are very plant specific attributes, and some technical areas can really only be tackled from a plant specific perspective. We've tried to focus on areas where we think generic evaluation can provide some insight into some of the issues that we're dealing with.

For the most part the studies --

DR. BANERJEE: Like what? What generic issues? Can you name a couple?

MR. TREGONING: Yeah. Chemical effects, head loss, coatings transport, some downstream clogging issues, and we used a surrogate throttle valve study. All of the programs that you hear will be areas that we've identified that we think meets this broad objective.

DR. BANERJEE: Yesterday though they seemed to feel that chemical effects were fairly plant specific, right?

MR. TREGONING: Specific loads and products I would agree are plant specific in nature. However, again, we still try to mine as much as we can from general study. I think there's a value from trying to understand in a global sense what's happening, and then as best as you can apply it to your plant specific.

CHAIRMAN WALLIS: You might even have a model for some of these effects, which could be applied everywhere, and if you had a really good understanding of what's happening to the chemistry and you have some predictive tools, they could be used everywhere.

MR. TREGONING: At least one of our programs we've had an objective where we did some initial exploratory work to see how feasible that is, and you're going to hear about that today. I don't want to -- it's a difficult thing to model. I will say that, and understanding your plant specific environment is crucial to the accuracy of any model that you could possibly develop.

So we have done some exploratory work in that area that you're going to hear about.

CHAIRMAN WALLIS: Well, everything is difficult until it's easy.

(Laughter.)

MR. TREGONING: Yes. No argument there.

DR. BANERJEE: Are you going to talk about modeling efforts somewhere?

MR. TREGONING: Yes, we're going to talk about two sets of modeling efforts. One is, again, today we'll talk about chemical speciation prediction, which is an analytical modeling study.

And then you're also going to hear today work that we've done to do additional head loss correlation development. That so far has focused primarily on particulate and fibrous debris. So at least initial work with the correlation model development has not focused on debris sources such as coatings and chemical effects.

But we feel like we need to walk before we can run in some of this model development work. So we want to see if we can handle the standard source term loadings first and then move to some of the newer considerations.

DR. BANERJEE: Yesterday they said that it was very difficult if you didn't know the structure of the screens. These results have to, therefore, be tested for each different screen, each different manufacturer.

So what are you doing that's taking care of that problem?.

MR. TREGONING: Obviously we can't test

every potential hypothetical screen design that's out there. A lot of that information is evolving. What you'll see is in many of our programs we've done at least parametrically tried to evaluate if screen type has effects on some of these phenomena.

So you'll see testing today that was conducted on -- with a wider mesh screen which, you know, no one to my knowledge is planning to use that sort of a screen in modified sump designs. However, there's some historical basis for the type of wire mesh screen. There was a lot of historical head loss data developed for the wire mesh type of screen, and historically, again, it's finding some use within plants.

So there is some work looking at that, but then we're also doing some additional work using the more modern perforated plate types of screens, and in some cases we are definitively having tests which we think or designing tests where the screen may have the biggest impact to see what impact that could possibly have.

DR. BANERJEE: Yesterday we also hear that perhaps the time of arrival of various components and so on had an effect on the head loss. How does the correlation take that into account?

MR. TREGONING: Yeah, I don't want to steal thunder from this afternoon.

DR. BANERJEE: Right, right, but just give us a preview.

MR. TREGONING: If I can give you a flavor at the risk of being, you know, usurped by somebody in the audience, what you try to do is or there's a couple of different strategies that you can do.

One of the strategies that we're pursuing that Bill Krotiuk is going to be discussing later is looking at essentially sandwich models where you can consider one layer within the debris bed to have a certain concentration of particulate and another area within the debris bed to have possibly a separate different concentration of particulate. To try to address some of these non-uniform, I don't want to use the word "think bed." We're trying to get away from thin bed because we think it's a confusing term. We want to say bed saturation effect, and by that we mean bed saturated with particulate either uniformly or over a very thin layer.

Either of those situations can be onerous in terms of clogging. So those are both situations that you want to understand from a modeling perspective, and then from a plant perspective you likely want to avoid those types of situations if you can or at least design against it.

DR. BANERJEE: So you can predict when this sandwich will form?

MR. TREGONING: No, no. Again, one of the things you'll see, there's a lot of information being presented later. I think you can identify certain variables that might promote cake filtration or sandwich formation or particulate saturation. I think there are certain variables that we can identify that would get at this.

Saying that we can predict it though is probably too strong a word. You're going to see that the bed formation of the sump screen is a very stochastic process. I don't know how else to describe it.

DR. BANERJEE: What do you mean by "stochastic"? It's like turbulence?

MR. TREGONING: No, there's a lot of variables that go into determining what's actually going to arrive at the screen, how it's going to arrive there, how the particulates are going to form within the fibrous bed to lead the head loss. There's a number of very important variables that go into that.

So trying to predict when relatively small changes in some variables could dramatically affect your head loss, that's what I mean by --

DR. BANERJEE: You have a fluid dynamic calculation going on, right, which delivers these particles or fibers or whatever?

MR. TREGONING: You have transport going on, yes. I don't want to say --

DR. BANERJEE: But they are carried by fluid, liquid, right?

MR. TREGONING: Sure.

DR. BANERJEE: They don't arrive by themselves. So --

MR. TREGONING: That's right, unless they were deposited directly on the sump screen by the LOCA itself. I mean, that's another possible transport mechanism.

DR. BANERJEE: But now assuming you're doing these fluid calculations, you should be able to calculate at least the first approximation, what deposit is out, what arrives, and you have to have sort of a dynamic model of what's going on on the screens, right? Whereas I see you just have static correlations.

MR. TREGONING: Well, again, I don't want to get into too much of today. I mean, I think there correlations are, you know, beyond just static correlations and flow --

DR. BANERJEE: Not in the material you've sent us.

MR. TREGONING: Flow transport is just one variable here.

DR. BANERJEE: But I'm saying the correlation itself. Now, you are giving us an overview of what's going on. I don't see anything tackling the dynamic nature of this, at least in the material you've sent us.

MR. TREGONING: And just so I understand, when you say "dynamic nature," what do you --

DR. BANERJEE: Build up of the bed. I mean there are whole lots of technology out there today which handled this type of modeling. I have given a copy of the paper to --

CHAIRMAN WALLIS: Predicting how things vary with time during an experiment.

MR. CUNNINGHAM: Would it be okay if we held this until the particular experts --

CHAIRMAN WALLIS: We're going to hear about that. I do have another question. Are you studying back-flush? You said the difference between wire screens and perforated screen. I think wire screens are particularly susceptible to having the fiber sort of go through and get tangled up on the screen, and they're harder to get away when you back-flush.

So back-flushing is something that might be interesting to plant, obviously the question of do chemical effects glue the stuff to the screen more effectively. I'd suggest that you do some simple back-flushing experiments if you haven't done so already.

MR. TREGONING: Okay. Certainly, certainly.

CHAIRMAN WALLIS: I know there's a whole bureaucracy that says what you can and cannot do.

MR. TREGONING: No, no, it's not bureaucracy.

CHAIRMAN WALLIS: You actually can go out and say, "Go and do it"? You don't have --

MR. TREGONING: We do that all the time, but again, we try to do things here that make the most sense.

CHAIRMAN WALLIS: I'm glad that you have freedom to do what's sensible instead of having to go through all the paper work.

MR. TREGONING: I didn't say that.

(Laughter.)

MR. TREGONING: We still have to go through quite a bit of paper work.

MEMBER KRESS: Another question, Rob. Since it's very difficult to predict the dynamics of how the things would build up on a screen and in what order and how much, if you're given a given source of materials, different types of debris and different amounts of it, potential chemical forms and so forth. Have you thought about looking at what combination of those and in what order on the screen would give you the worst and maybe you could bound the problem with that sort of thing?

MR. TREGONING: When we set up our testing matrix, quite often we're trying to, again, parametrically search out what are benign versus malignant sort of conditions. So one of the things we've clearly tried to do, and you're going to see information later, we have tried to search out conditions that might be particularly onerous.

Getting back to -- I'm happy that you let me get to Slide 3 so quickly.

DR. BANERJEE: We still have not understood how pieces fit together. You're going to tell us that, right?

MR. TREGONING: Yeah, a little bit, but I think, you know, again, there's five broad technical areas. You're going to see later there's some areas that we have quite a lot of overlap. There's other areas where we have less overlap. Certainly in the area of chemical effects, there is a tremendous amount of overlap. Some of the coatings transport work, there's almost no overlap because that's really a separate, stand alone project. So you're going to see that in a second.

I think we've covered this. Mark covered this. The goals, again, provide basic technical knowledge. There's only one program that you're going to hear that's non-confirmatory in nature, and that's the integrated chemical effects testing program.

This is a joint program that we conducted with industry to even determine if we had to worry about chemical effects at all. So that was really the basic objective of that program.

Beyond that test program, all the other programs that you're going to hear about over the next day and a half are confirmatory in nature, and the intent is to provide information for the staff's use in conducting their review and assessment of these generic letter evaluations.

Certainly while that's the goal, as we get a report and report results, we have in many cases already and will continue to strive to make results publicly available so that they can inform the industry and ongoing activities with respect to this resolution.

So while these are confirmatory programs by their nature we are trying to be open and make sure that industry has the benefit of this knowledge as well as quickly as we can.

CHAIRMAN WALLIS: I think it's wise that you have several different labs at times doing what appears to be the same experiment, especially when those experiments have given anomalous results in one lab. You want to see can you get anomalous results in another lab or is it something to do with the way they did the experiment, but they weren't aware of --

MR. TREGONING: I wouldn't categorize any of these results as anomalous that you're going to hear in the next day and --

CHAIRMAN WALLIS: Well, I've seen the word "anomalous" in some of the results.

MR. TREGONING: Okay. Well, maybe I need to go back and edit those reports a little bit more carefully.

(Laughter.)

DR. BANERJEE: Really? Meaning politically correct or --

MR. TREGONING: No, no. Anomalous may not be the correct word to use. I mean, I think we've seen --

CHAIRMAN WALLIS: Well, there are other words, but we wouldn't use them.

MR. TREGONING: I think you're going to see that a lot of the effects we are able --

CHAIRMAN WALLIS: Interesting effects.

MR. TREGONING: Interesting effects. We're able to replicate them. We may not fully understand them, you know. So it depends on how we're using the word "anomalous" there.

I don't want to use it in the sense of meaning sporadic.

CHAIRMAN WALLIS: Well, anomalous is something that you expect it to behave this way and it behaves some other way, and you can't understand why.

MR. TREGONING: Yeah.

CHAIRMAN WALLIS: Presumably that's anomalous.

MR. TREGONING: There's one result that I would.

CHAIRMAN WALLIS: Or you can get an anomalous presentation sometimes, and you probably get anomalous questions.

(Laughter.)

CHAIRMAN WALLIS: Let's move on.

MR. TREGONING: Okay. These are the technical areas of study. I think a little bit is trying to get at your question of how do the pieces fit together.

We've got four areas that we're looking at that Mark mentioned, one in the area of chemical effects. The basic objective of that is to determine the potential for chemical byproduct formation within containment pool environments and characterize and predict as best as we can the byproducts that form.

These are the first two talks you're going to hear today. The first one will be the ICET test that was conducted at Los Alamos National Lab.

The second is the speciation prediction work that was conducted by CNWRA.

One point I will make here with respect to synergy. Even thought CNWRA is the lead in chemical speciation prediction, LANL in support of their own experiments has done a lot of their own predictions on the side to try to develop baseline variable conditions for the ICET test, determine how much product that they need to mix in to get a certain pH. So, again, there's some synergy and overlap there in terms of some of the expertise that's being applied.

In the area of head loss, we have two head loss programs that are primarily, again, a confirmatory research program to looking at head loss associated with PWR containment materials both with and without chemical effects.

You're going to hear about the chemical effects head loss testing program at Argonne National Lab, and then later today the particulate head loss testing program. And by "particulate" I really mean standard insulation debris. At least so far the results you're going to hear will be mainly fibrous and CalSil type of particular testing, although there is plans to move on and look at some coating head loss testing at PNNL.

And then the more stand alone programs. We have one program that touches on the area of downstream effects, one aspect of the downstream analysis, and particularly it's looking at blockage. I'll say flow blockage due to restricted pathways.

Now, we've studied it here by using

surrogate HPSI throttle valves, but I think you'll see a lot of the findings have some more generic application as well.

MEMBER DENNING: Rob, that is a very limited objective as well.

MR. TREGONING: Yes.

MEMBER DENNING: And from the things we heard yesterday there are broader areas of uncertainty. Do you have plans to look at in-core types of blockage?

MR. CUNNINGHAM: We have no plans for that right now.

DR. BANERJEE: Or even transport to the core. I mean, that was the issue. Where does it deposit out or like that?

MR. CUNNINGHAM: Our focus has been on what would be happening in the -- we would expect to be happening in the containment. See, this is a little step into that next regime, but it's the only steps we're doing right now.

CHAIRMAN WALLIS: But if it doesn't block the throttle valve, then it goes further. Where does it go?

DR. BANERJEE: How big is the throat of the throttle valve?

MR. TREGONING: That varies. That is truly a plant specific consideration. I think what you'll see in the testing is we did some surveys to try to understand the range of throttle valve settings, and we studied in this LANL work blockage conditions over those ranges of applicable settings.

There was some --

DR. BANERJEE: Are they typically one inch, five inch?

MR. TREGONING: No, no, no. They're typically less than the screen openings -- I'm sorry -- slightly greater than the screen opening size. So if the screen opening size is a quarter inch, they might be around a quarter inch. If it's an eighth inch, they might --

CHAIRMAN WALLIS: But when it's closed it's got no area. the problem is when it doesn't quite close because maybe it closes on a piece of metal or something. There's flow through it, but it's a very small hole. Then you can bung (phonetic) it up, and then when you try and start it up again, that's the sort of situation where you really gather debris. It's where it's not quite closed for some reason. It could be it just didn't close all the way.

MR. TREGONING: Right. If you've got a

valve --

CHAIRMAN WALLIS: If it's open, there's probably no problem at all, but it's just when you close it and then want to open it again. They may find it's clogged up.

DR. BANERJEE: Come again?

CHAIRMAN WALLIS: You close the valve; you almost close the valve. Then you've got a flow through it, but you've got a restriction. You've got a small area. So that's when you could lock it up with debris.

So now you might have a pile of debris going back from the valve in the pipe that has built up this stuff, which is now ready to lean on the wall and so on, and actually create a --

DR. BANERJEE: The valves are used to control the flow.

CHAIRMAN WALLIS: Yeah, they can open and close them, and in fact, in events they sometimes open. They do and close them, don't they?

MR. TREGONING: Yes. At the risk of overstating my knowledge in the area, I mean, my understanding is they have set points and --

CHAIRMAN WALLIS: Especially when they think they're losing their net positive suction head.

They may vary the throttle valves in it.

MR. TREGONING: Yes, that's true. Ralph, did you want to enlighten us a bit?

MR. ARCHITZEL: Architzel from NRR staff.

Particularly like in the Westinghouse house, they flow balance ahead of time, the flow through the HPSI injection lines, and so they have set points. Like Rob is saying, those are fixed normally and they stay and they're throttled down to achieve balanced flow in case a line breaks. So those aren't variable types of situations. They're normally set, that condition, during the accident.

DR. BANERJEE: The opening is pretty small in each of these?

MR. ARCHITZEL: Yes, as Rob said, the opening is small, and that's why we had things reasonably asked to be looked into, but the valves are typically set in those conditions to achieve balanced flow.

CHAIRMAN WALLIS: But they're instructed to throttle under certain conditions. Don't they have some flexibility in where they actually end up in the position in the valve?

MR. ARCHITZEL: But ahead of time these are set before the accident.

CHAIRMAN WALLIS: It's either all or nothing then? It's either throttled or nothing or --

MR. ARCHITZEL: No, they're set to specific conditions to balance the flow through various lines.

CHAIRMAN WALLIS: So how do they control the flow?

MEMBER SHACK: I guess they don't.

CHAIRMAN WALLIS: They don't control the flow?

MR. ARCHITZEL: Once the accident starts, it's balanced. You expect --

CHAIRMAN WALLIS: No, but they're total throttle, and when they think they're losing their pumps from the sump, they're instructed to throttle.

MR. ARCHITZEL: Those are different situations to throttle down. Those weren't the valves we were talking about.

CHAIRMAN WALLIS: Oh, it's different valves which they use to control. Maybe you test those, too. Okay. Well, we'll get to that.

Thank you.

DR. BANERJEE: So these are the most restrictive, these throttle valve.

MR. TREGONING: I wouldn't use that word.

Again, you'd need to look at a specific ECCS layout in a given plant. There's certainly one potential or likely minimum gap area, you know, restricted area flow.

DR. BANERJEE: The velocity is pretty high through them.

MR. TREGONING: Yes, the velocities are pretty high.

DR. BANERJEE: So if you had fine, they would go through.

MR. TREGONING: Potential, although I think, again, at the risk of stealing too much thunder from tomorrow, I think you'll see when we did this program we had basic questions if we could under these conditions with relatively high velocities, if we could even get blockage at all, and we were able to under certain conditions generate significant measurable blockage in these valves.

So, you know, I think maybe at that point let's defer to tomorrow because I think you're going to have a lot of additional interesting questions that we can tackle then.

DR. BANERJEE: But I guess it's the broad issue of the arrangement of this program. In arriving at these things, did you do something like a PIRT and look at all of the downstream things and decide this was the most important component or that was? How did you do this systematic? That's what I'm trying to understand.

MR. TREGONING: Yeah, we didn't do an official PIRT, but I mean, certainly some of those, we had those discussions among staff as well as with contractors. We had been trying to identify areas like I mentioned that we thought particular vulnerabilities existed and, again, where we thought we could do generic research to deal with topics that maybe hadn't previously been addressed.

Certainly pump wear and degradation if you were doing a PIRT would be something that would be relatively high. We --

DR. BANERJEE: Or core blockage.

MR. TREGONING: Or potentially core blockage. With pump degradation, I mean, one of the issues we've had with that, as Steve Unikewicz mentioned yesterday, there's been a lot of work in the area of wear and tribology that can be applied to pump design studies, and the other aspect that we've often stumbled under is there's such a wide variety of pumps that it has been difficult to try to craft any sort of program in a generic sense that really captures the variability that's out there in the area of pumps.

So when we've picked programs, we've had discussions to try to identify specific vulnerabilities, as well as, again, try to have an eye toward what we think we can tackle in some sort of generic sense.

CHAIRMAN WALLIS: It would seem you cannot ignore core blockage. We had discussions yesterday --

MR. TREGONING: No.

CHAIRMAN WALLIS: -- and since we're talking about much larger screens, it's quite possible there will be screen bypass debris. We're talking about self-cleaning screens which chop up debris and debris goes through the screen. Where does it go? It's hard to imagine why someone is not doing a program on core blockage.

DR. BANERJEE: Well, there was a PIRT done in the early days. I've been looking through the old documents. I thought there was something done, oh, way back. Somebody, Ralph may know or somebody might know.

MR. TREGONING: Well, Bruce?

DR. BANERJEE: Wasn't there something like this done back in the early '90s or something or '80s or something?

MR. LETELLIER: Yes, you're right. GSI-191 was initiated with a PIRT on both the large dry and the ice condenser containments, but at that time in the regulatory process, the GSI specifically excluded downstream effects. There was an interface defined where that safety concern would be treated separately.

So although it may have been itemized, it was not thoroughly investigated.

DR. BANERJEE: So this program that's coming out, does it come out of that PIRT, at least leaving out the downstream effects, or is it just sort of reacting to concerns which are arising in a sort of semi-random way? Maybe NRR concerns. I don't know from where they're coming.

MR. TREGONING: Let me follow up with what Bruce said. There was an early PIRT that was done. There has been a lot of research in this area over the last ten, 11 years. So this is really just touching on the research associated with the last year to six months of the program. So there's been a lot of baseline information.

I wasn't involved in that. So let me speculate. My speculation is, and Bruce --

DR. BANERJEE: But somebody was, right?

So we can hear from the horse's mouth here.

MR. TREGONING: My speculation would be that a lot of that baseline research was a direct result from the PIRT that was conducted. That's why you do PIRTS obviously.

I mean, Bruce, do you want to elaborate on that? No?

DR. BANERJEE: Because there is usually --

MR. TREGONING: Thanks.

DR. BANERJEE: -- systematic --

(Laughter.)

DR. BANERJEE: There is a systematic procedure after a PIRT I evaluates whether the experiments are applicable, they do a scaling study. There's a whole process there, which is laid out, and there doesn't seem to be an equivalent process being taken care of.

MR. TREGONING: Well, Bruce talked about the PIRT, but there has been other generic studies that have been done as well. I mean, back in, you know, the latter part of the '90s or early 2000, there was a very extensive knowledge based study that was done at Los Alamos National Laboratory, and part of that knowledge based study was to really lay out what we knew and what we didn't know.

Now, that wasn't a formal PIRT, but in some ways it had many of the same results as a PIRT would in that it identified areas where we had particular uncertainties, particular concerns.

DR. BANERJEE: But it dealt with the whole thing, including downstream effects, everything.

MR. TREGONING: Well, refresh me. Was downstream dealt with by the knowledge based study?

MR. LETELLIER: The purpose of the knowledge based report was to capture all of the work that had been done to date, and to my knowledge, this work that you're going to hear about tomorrow is the first of its kind regarding downstream blockage, and my understanding of the history behind this is that the throttle valve was specifically examined because it represents one of the smallest gap openings in the internal flow. It's not the only area of concern, but it is one of the smallest, and in proportion to the debris sizes and the screen penetration, it was simply chosen for examination, as Mark said, for the first step in looking at downstream concerns.

CHAIRMAN WALLIS: Well, we probably have to go on, but I think we learn with this program that PIRT or not, you can discover things while you do research which you didn't expect. Then you have to respond somehow.

MR. TREGONING: Okay. Here's the group of research team that we formulated to deal with these issues. We have a very informal working group. The working group -- and I've identified Los Alamos, Southwest, Argonne, and Pacific Northwest National Laboratory -- they're primarily dealing with issues and chemical effects, and then debris head loss.

The Carderock Division work is separate from the working group. They're looking at the coating transport issue. So the working group of the four labs and the NRC, we were charged with test plan development, test coordination and review of results.

So this is something that all the members do. We usually ask, you know, if we have results that were developed at Argonne, we ask Los Alamos or the Center to at least be aware of and in some cases review that work to make sure that we're happy with not only the quality of the work, but more importantly, to make sure that we understand it as much as we can.

CHAIRMAN WALLIS: That's a very interesting plot because it shows that everybody communicates with Los Alamos, but PNNL has nothing to do with Southwest Research.

MR. TREGONING: That's true. That's true.

CHAIRMAN WALLIS: That's true?

MR. TREGONING: Because PNNL -- Southwest is the group doing chemical speciation work. PNNL is not doing any chemical effects work.

CHAIRMAN WALLIS: I see. Okay.

MR. TREGONING: So that is a true plot.

CHAIRMAN WALLIS: It's true. It's true. Okay.

MR. TREGONING: It's a true plot. Now, we're certainly all together on a lot of research costs. So they are at least, you know, indirectly communicating with these guys, but there's no direct collaboration.

CHAIRMAN WALLIS: There's no direct -- a completely independent phenomenon we're looking at.

MR. TREGONING: Yes.

DR. BANERJEE: Do you have access to the industry data or is it all proprietary and you can't use it? For example, in model development and things like that.

MR. TREGONING: We have some access. We certainly have access when industry submits that information either formally or informally. Now, you know, some of the Alion work, I mean, I think you heard yesterday, I think, John Butler, maybe John Butler; somebody characterized that they had done, you know, 2,000 some odd tests in the Alion test loop. I personally have not seen that work. That would be very valuable data to add to the experiential database, to say the least.

So, you know, I'd say it's a bit sporadic. We certainly see the things that they give us, but we don't see everything.

DR. BANERJEE: Yeah, you'd also probably need quite a lot of details of the tests to see how this stuff went there and how it deposited.

MR. TREGONING: Yeah, raw data may or may not be very useful certainly.

DR. BANERJEE: Now, the Carderock Division, why did coatings become such an important issue? What happened? I missed that.

MR. TREGONING: Coatings have always been an important issue.

DR. BANERJEE: Is it because they form very fine particles or --

MR. TREGONING: Coatings have always been important, and I think you heard a presentation from NRR yesterday to talk about some of the concerns. One of the issues with coatings, because there was a lack of knowledge in terms of failure mechanisms and amount of coating loading that you could get, the safety evaluation, and the staff took a very conservative position and essentially assumed that you had a large amount of debris which formed.

So then the next question is, well, if you really have that much debris formed, how much of it do you think will transport.

Now, the particulate is one thing, you know. Anything within the ZOI is assumed to be particulate. One of the reasons it's assumed to be particulate is that it's more readily transportable.

But the bigger question and the bigger loading potentially is for coatings outside of the ZOI, which the particulate assumption is much less defendable in that case.

DR. BANERJEE: But yesterday we heard -- and maybe these tests will clarify this -- we heard they did some autoclave tests for the region outside the ZOI, and these were on samples that were sent. It wasn't a very comprehensive program. They only did about 15 -- I don't remember. It's a small number anyway, but they didn't measure the particle sizes.

If particle sizes are concerned, they only sort of measure how much of the coating spalled off or whatever it did, you know, in the steam spray system. So is your program trying to fill those gaps or what is the motivation?

MR. TREGONING: As much as we can. What you'll see, again, it's a parametric study. We've looked at transport of different sizes of chips and --

DR. BANERJEE: Oh, this is transport rather than actual formation.

MR. TREGONING: This is transport. This is not formation. The industry is dealing with the formation and the damage issues. We in this program are looking at the transportability issues.

DR. BANERJEE: Because the program that was discussed yesterday doesn't give us any sense of what the size of the flakes or particles are. There seems some disconnect between knowing 50 percent is gone, but we don't know in what form it is, and doing all of this transport work because --

MR. TREGONING: We do have some independent information beyond just what has been measured or not measured in some of these experiments. There's certainly visual evidence within some of the plans of coatings under normal operating conditions which are not adhered anymore, and they've come off in chips and so we at least have a crude sense in terms of what sizes we're seeing, you know, and we've used that to try to guide the range of study within the Carderock Division.

I mean, there's some other evidence as well that's been applied in setting up these experiments.

Move on?

CHAIRMAN WALLIS: Yes. You're way behind.

(Laughter.)

DR. BANERJEE: It's all his fault, right?

MR. TREGONING: I'm more than happy to go right through these and get us back on schedule.

CHAIRMAN WALLIS: Well, we can move through the next slide pretty quickly.

MR. TREGONING: Okay. We talked about regulatory coordination a little bit. We have staff in both NRR and Research that we're coordinating with. We've got three levels of peer review, NRR and Research review. We also, as I discussed a little bit, we have peer review among the research team members.

And in the area of chemical effects, we're also conducting an external peer review. We have five members on the peer review group, and we've received some preliminary feedback. We're not going to touch on the peer review effort much today, but I know when we come back in June, if not before, we'll have a more extensive session concerned with some of the peer review comments.

I did want to indicate who the peer reviewers were. We've tried to get a pretty diverse group in terms of not only their affiliation, but their areas of technical expertise.

DR. BANERJEE: So you have a chemical industry person. That's wonderful.

MR. TREGONING: Yes, we do. So we have five people from both national labs, academia, and industry. We have filtration guys. We have speciation modeling guys. We have people that have experience with nuclear waste, analytical chemistry, experimental chemistry, electrochemistry, and experimental testing. So I think we've got a pretty good group of external peer reviewers here.

DR. BANERJEE: And Digby is a very thermodynamicist as well.

MR. TREGONING: Yes, yes. Understanding Digby has been my personal challenge.

DR. BANERJEE: He's from New Zealand.

MR. TREGONING: No, I don't mean that. I just mean technically.

(Laughter.)

MR. TREGONING: It's not communication. It's level of understanding. He's up here; I'm down here.

DR. BANERJEE: Well, that's a good team.

MR. TREGONING: So just briefly, we've touched on most of these, and Mark touched on these more thoroughly. I just wanted to outlines some important messages that I think all of the talks are going to touch on within the next couple of days.

We've designed these research programs to provide some basic conceptual understanding. We've tried to identify several important technical issues which impact functionality of the ECCS system. As we mentioned several times, our primary role --

CHAIRMAN WALLIS: I think, Rob, one of your jobs here is to avoid future surprises. You'd better know now if something is going to happen.

MR. TREGONING: Sure. We want to know.

CHAIRMAN WALLIS: You recognize that when you do an experiment and say, "Gee, whiz. That's something new. We'd better pay attention to that," rather than saying, "Let's correlate it," or something.

Thank you.

MR. TREGONING: No, understanding if there are features of this phenomena which might surprise us in either a bad or a good way is really --

CHAIRMAN WALLIS: And then it might be a good way.

MR. TREGONING: It might be a good way, and that has been really a fundamental consideration in the research as well. Again, primarily we're providing confirmatory information. You're going to hear about a lot of interesting findings over the next day or so.

One point is, again, that these findings are going to be generic in nature and are really understanding, and considering plant specific related issues, that's what's really needed in order to assess the importance of some of these research findings for a particular situation.

So that's where the plant specific part of this really comes back into play. We might show some very onerous head loss results, but if they're not representative of any particular plant conditions, they're not applicable to that particular plant.

CHAIRMAN WALLIS: It would be interesting if at the end of the day you could give us a homework assignment which says explain particular obligations in Experiment X-23 and you know.

MR. TREGONING: You're offering that?

CHAIRMAN WALLIS: Well, I don't know. It may well be that we'll go away with some assignments to think about.

MR. TREGONING: You know, we're always -- Research, given our limited constraints, we're always looking for additional help and support in understanding phenomena. So if ACRS is offering that, I'll certainly be more than happy to accept.

CHAIRMAN WALLIS: Sometimes we can't restrain ourselves.

DR. BANERJEE: One member of ACRS.

(Laughter.)

MR. TREGONING: I noticed you didn't make the same offer.

That's it.

DR. BANERJEE: I think before you take that slide off, basically I've understood the various components of the program, but taking this from this generic point to the point where it's useful on a plant specific basis usually needs some sort of modeling glue, right, to do that transformation?

I mean, there is understanding which is the first thing, but then that has to be translated into something relatively quantitative so that people can use it. How is that going to happen?

MR. TREGONING: You know, modeling is one approach. There's quite a lot of things that we're an engineering organization, and these are engineering problems that at the end of the day we're concerned with making sure that we have solutions to, and we have a lot of things that we have engineering solutions for that we're confident work without necessarily having rigorous, robust models to understand all of the details associated with whatever phenomena are studied. So --

DR. BANERJEE: So then you have to put live safety models or something.

MR. TREGONING: Potentially, potentially not. It depends on how well you understand the phenomena. So that's the challenge here. I mean, modeling is one avenue. I don't think it's particularly --

DR. BANERJEE: It's complementary, right?

MR. TREGONING: It's certainly complementary. Certainly modeling in the sense -- and that's what we're trying to do with the correlation work. Modeling, I think, is really good to make sure that you've covered your bases, that you make sure that there's no phenomena on those particular set of conditions that can get you into trouble.

But the models themselves, quite frankly, traditionally have limitations as well, and I think we need to keep in mind some of the uncertainties in our knowledge base, and I think that needs to factor into the engineering solutions that we develop for this problem.

So modeling is important. Don't get me wrong, but I don't want to paint modeling up as a savior that if we understand everything well enough we'll be able to get the best engineering design.

I think some of these situations can be dealt with without modeling at all, just by making some prudent choices within, you know, potentially how the plants are configured, how they're designing against these functionality issues.

DR. BANERJEE: So how will you, let's say, predict or study, let's say, the problem of downstream effects without a model to look at where deposition might occur? Are you going to take a full-scale reactor and pump this stuff in if this would happen?

MR. TREGONING: Well, you know, again, you need to walk before you can run. We need to see in these new screen designs. We need to understand what types of things and the volume or the amount of things that are going to be ingested beyond the sump strainer screens.

That fundamental understanding is still unknown to some extent because, again, we're still in the design modification phase.

DR. BANERJEE: We know right now that there are stream designs where a significant amount of stuff will pass downstream. Yesterday we heard about the active stream designs where 30 percent of stuff goes through, and they go through as fine.

So this is a given. So what happens after that?

MR. TREGONING: Again, 30 percent of stuff, but you still don't know. It's a plant specific determination of how much that stuff is.

DR. BANERJEE: Well, I know, but what I'm saying is without a model what are you going to do? There's going to be a very wide range of stuff, whatever it is, going through. So if you can characterize it by size distribution, by composition or whatever, to go from there to the amount of blockage that you might experience or where it might be, I just don't see you doing it without a model.

I don't think any engineering judgment, at least not mine, is going to give you that.

MR. TREGONING: Well, it depends on what you mean by model, and you know, the Westinghouse Owners Group at least has a methodology for what needs to be considered in a downstream effect analysis, and again, that's where the details of the particular ECCS system design are very important. Just because you've ingested debris, how much of that makes it to the core is still potentially highly variable, and we --

CHAIRMAN WALLIS: Well, Rob, I think we're going to have to stop your presentation because we could talk to you all day.

MR. TREGONING: All right.

CHAIRMAN WALLIS: Now you've started with the first slide again here.

(Laughter.)

CHAIRMAN WALLIS: I'm getting nervous.

DR. BANERJEE: We'll come back to this.

CHAIRMAN WALLIS: You're going to be around, aren't you? You're going to be around.

MR. TREGONING: I'm more than happy to defer at this point.

CHAIRMAN WALLIS: Yes, but you're going to be around the rest of the day if we need to talk to you again.

MR. TREGONING: I think I'm going to be up here poking slides the rest of the day. So I'll be readily visible. I'll be readily visible.

CHAIRMAN WALLIS: So you can even get involved in the conversation from time to time.

MR. TREGONING: I'm going to try to avoid it, but, yes, I will be here.

CHAIRMAN WALLIS: Thank you. That was very good, I think, but we do have to move along.

B.P., are you going to start this one?

MR. B.P. JAIN: Yes.

CHAIRMAN WALLIS: Okay. So when you're ready, please go ahead.

MR. B.P. JAIN: Good morning. I am B.P. Jain, the Office of Research, and with me is Bruce Letellier from Los Alamos National Laboratory, and together we will provide you a brief status of the ICET program.

The ICET program was concluded last August, and we have presented to the committee the results of the first three tests from last July meeting.

The research program was a cooperative program with industry, and the industry had provided input to the test plan, the test matrix, and test operation, as well as the characterization requirement. So it's a truly joint program, and the results of this program have been shared with industries and they have been made available to the public as well.

Next one please.

CHAIRMAN WALLIS: Can I ask you? The overview here and to go back to the concern that we've had all the time, we just hear it from Dr. Banerjee.

MR. B.P. JAIN: Right.

CHAIRMAN WALLIS: These tests, do they result in some understanding which enables you to predict things better or are you just finding out things which might be important? Is there some predictive capability that results from all of this?

That's, I think, one of the key questions that we're going to have for you.

MR. B.P. JAIN: Well, the objective of this program was -- I'll get to the next slide -- was primarily to find out if --

CHAIRMAN WALLIS: Find out if things happened because if you read your preamble to the report, it sort of says, yeah, we found out some things, but it's going to be plant specific and, therefore, industry has to do all of that work to study each plant.

MR. B.P. JAIN: Right.

CHAIRMAN WALLIS: There's no model that has come out of your work which will help shorten their work in any way?

MR. B.P. JAIN: Not as a part of the ICET program.

MR. TREGONING: The one thing I will say is the results from that program have been used both by us to try to calibrate some of our models and then also industry as well. I think you heard about the Westinghouse program where they tried to calibrate their model with respect to the ICET program.

CHAIRMAN WALLIS: I'm sure they've been useful. It's a question of how far you have to go before you can actually use it for plant predictions.

Okay. I'm sorry to --

MR. B.P. JAIN: So basically would provide this brief recap in this presentation, rehash our objectives, the test plan and significant research findings we provided last July. What we did not provide last time was Test 4 and 5 because they were being planned or partially complete. So we'll have more details of Test 4 and 5. And then where we go from here and where we get more information.

This is core data. Last time we presented the test plan, the matrix, and the test loop and operation itself, much more detail, and the results of Test 1 through 3.

Now, Bruce is going to go over briefly on this test plan, test operations, so will give you the completeness of the whole presentation. But primarily focus would be Test 4 and 5 results in this one.

Next one, please.

Just to emphasize the objective of this program, we're to determine if the cantil (phonetic) byproducts can be formed in the LOCA sump pool environment, and if there could be some gelatinous-like material that could be formed. Those were the primary two objectives, emphasizing objective was not to conduct a head loss study under this program. It was just being done separately at ANL, mechanical head loss effects.

Some of the main, obviously the tests have been completed, and we have made an objection where the test results did show that chemical products and precipitates can form, and some of these products are amorphous in nature.

As we had shown last time, and we will

show some of the slides today, when there's a change in pool variables, for example, the pH, the temperature, or the insulation can affect the type and the qualities of the kind of products you get.

And obviously some of the tests like, for example, Test 3 showed that depending on the nature of the environment, you can get products quickly or some time later as in Test 1. So those are the objectives.

Next one, please.

The staff is using the results of the ICET program in their evaluation for GRE clarit (phonetic) 2004-02, and factoring that in our head loss testing at ANL and the industry as well using those results.

Next one.

This will give you a brief overview. This is a test matrix which we used to run this program. The five tests were planned. Each test ran for 30 days, and the primary difference among the differing tests was the buffering agent and the insulation type.

CHAIRMAN WALLIS: You didn't run any tests with no buffering agent? I mean, if a plant takes out its TSP and has no buffering agent, what happens?

MR. B.P. JAIN: Well, that was not part of the --

CHAIRMAN WALLIS: Yeah, but you did not do any tests with no buffering.

MR. B.P. JAIN: We did not.

CHAIRMAN WALLIS: What is the pH of the liquid which comes out of the reactor, the primary coolant loop? What is the pH and the LOCA for the normal boration?

MR. B.P. JAIN: Paul.

MR. KLEIN: Paul Klein from NRR.

It's one of the questions we asked the CalSil TSP plants, would be the minimum expected pH before any TSP would dissolve, and the answers were typically around four and a half or so.

CHAIRMAN WALLIS: Thank you.

So it might be interesting to do an experiment with four and a half pH without any buffering. I don't know. The program may be over, but, you know, when we get a question of a plant which is going to take out its buffering agent, it would be interesting to have some idea of what might happen.

MR. B.P. JAIN: In tests, there are three types of buffering agents. I'm sure by now we all know that: sodium hydroxide, TSP, and sodium tetraborate. Tests 1, 2, and 5 consider 100 percent fiberglass insulation, and Tests 3 and 4 have 80 percent CalSil mixture with 20 percent Nukon.

Next one, please.

This gives you what we had shown last time just to give you a flavor of significant findings from Test 1 through 3. Test 1 is a sodium hydroxide and Nukon combination environment where we saw a white precipitant at room temperature, and on the right side of the figure shows insulation where you see the web structures of chemical products in the individual fibers, a web-like material.

You see a similar trend in Test 2 to a lesser degree. The difference in Test 2 obviously is the buffering agent is different. Insulation remains the same 100 percent insulation.

CHAIRMAN WALLIS: And so it's your determination of what this web-like stuff is?

MR. B.P. JAIN: Well, on the precipitate it's more like aluminum hydroxide. That analysis indicated that.

CHAIRMAN WALLIS: Okay.

MR. TREGONING: Let me clarify here a little bit. We had a lot of discussion about this last July when we showed the first pictures of this. Now, since that time LANL has gone back and done quite a bit of characterization work.

Are you going to cover this or not?

That's the only reason I was dealing --

MR. B.P. JAIN: Not today, I guess.

MR. LETELLIER: We're probably not going to revisit the composition of the Test 1 precipitates and the coatings, film coatings. I did want to mention, however, though that some of the photographs that we showed in July of the film type of webbing between fibers may have exaggerated the concern.

After that briefing we actually went back to the lab and reproduced that in an artificial manner by dipping fiberglass into solutions of various types.

So some of this is a surface tension effect that occurred when we drained the tank. I'm convinced that some of the more granular deposits were formed in situ, but there's a visual misperception there that doesn't need to be exaggerated.

MR. TREGONING: Yeah, we shared pictures where it looked like film in July, and we truly believe as Bruce said, after some additional work that those are film, films that were brought about during the drying process.

DR. BANERJEE: Do you think those little powders on those stick-like structures are just due to drying?

MR. LETELLIER: Some of the more granular appearing deposits I believe to have been formed in situ because this type of structure appears on a lot of the solid surfaces.

DR. BANERJEE: Can you just point so I will know which structures you're meaning?

MR. LETELLIER: These more granular structures on individual fibers, they look like barnacles perhaps, and this is not a particularly good example, but in the lower frame the webbing in between fibers is much thinner and that can be formed from a surface tension effect like a silt bubble. We've demonstrated that in an artificial environment that did not experience 30 days of exposure. We simply dipped the fiber and produced the same structures.

DR. BANERJEE: Okay.

MR. TREGONING: We don't have a picture of the webbing in this presentation. We did in July, but the appearances are quite different.

MR. B.P. JAIN: This is for the Test 3, the gel-like material.

CHAIRMAN WALLIS: Excuse me. Is the flow through this? There's no flow through this matrix. So whatever gets in there is diffused in there somehow?

MR. LETELLIER: That's correct, except for at the end of the test the water was drained away from the sample, and that is a directed flow. During exposure we did expose fibers to various localized flow conditions, but in my opinion, none of these bulk samples had a substantial flow through the medium.

MR. TREGONING: But we did have additional samples that we put by the drain column and then a few at later tests where we specifically put near the inlet water jets to try to simulate some more higher flow conditions.

MR. LETELLIER: But these samples were never exposed, for example, across a sump screen environment so that the bulk flow is drawn through them.

CHAIRMAN WALLIS: But there was some agitation in the tank. So it probably isn't just diffusion. Diffusion is very slow.

DR. BANERJEE: So you did agitate the tank.

MR. LETELLIER: We'll get into the physical attributes, but you'll see that where the water was injected there's clearly structure in this tank.

MR. B.P. JAIN: The next one, please.

This is for Test 3, which is a TSP in CalSil, which we've talked about quite a bit. It just shows the picture of that gel-like --

CHAIRMAN WALLIS: Is this something like pizza dough or something. What kind of --

MR. B.P. JAIN: Looks like, yes.

CHAIRMAN WALLIS: Is that the way it feels? Is it gooey and tough?

MR. B.P. JAIN: It's a gooey stuff, yes, and I think we have a little more information later on in this presentation on this Test 3 stuff.

CHAIRMAN WALLIS: It's stuff which would be hard to wash off if you just put it under a faucet?

MR. LETELLIER: It would be disbursed under a faucet. It has a texture of face cream.

CHAIRMAN WALLIS: Oh, so it's not quite as gooey as something like pizza dough.

MR. LETELLIER: It's not as sticky as it might appear.

CHAIRMAN WALLIS: I don't even know what face cream is.

(Laughter.)

DR. BANERJEE: It's an emulsion.

MR. B.P. JAIN: Bruce will go over the details, give you a brief overview of the test plan and the test operation and the results.

Bruce.

MR. LETELLIER: From B.P.'s talk you're going to want to hold onto page number 6, the test matrix, just to remind yourselves of which pH conditions and which combinations of debris that there were.

I'd like to begin by acknowledging the other team members at Los Alamos. I'm representing the work of a group of very dedicated individuals. In particular, our principal investigator, Jack Dallman, who is responsible for daily operations for developing a quality insurance plan and largely for documenting our results, and I think Jack is on the line this morning to help me answer questions.

I do want to offer the committee an overview of the project because several of you were not present in July, but this is review material. So if we need to skip ahead, we can do so.

Page 10 shows the develop time line. Conceptual design started almost two years ago now when we first became serious about committing resources to chemical test investigation. Structural design occurred during the summer of '04, and our first test began in late November of 2004.

As BP said, we had a series of five 30-day tests that were run in sequence with approximately a three-week delay time in between each one. The fifth and final test was just completed in August the past summer.

Photographs on page 11 give you some impression of size and scale. The central piece of apparatus is a tank of approximately 250 gallons of reverse osmosis water. The principles of scaling we're trying to preserve here are the proportionality between the surface area of metals and the dilution volume of the liquid.

So in essence, this is a miniature containment building. Although it does not preserve the spatial scale of flow, it does preserve the chemical proportionality of corrosion and dilution.

The samples were introduced in racks as shown in the lower frame. There are actually six of these racks are suspended above the water in the head space and one of these racks is submerged throughout the duration of the 30 days.

So this volume does accommodate the proportionality of the entire containment building. We were concerned about exposure to sprays and wash-down of corrosion products to the pool. Opinions vary about whether that was important in retrospect, but all of the tests were executed in the same way.

The water level in this photograph extends up to that, about one third of the tank to that lower flange. So there are observation ports that you can see, one below the surface, one immediately above the surface, and then one on the covering of the lid.

This tank was wrapped in thermal insulation throughout the test, which is not shown in this picture.

The next frame --

DR. BANERJEE: Was the water agitated at all?

MR. LETELLIER: You can see -- let me point -- where the water was introduced at this point near the top of the water. There is a parallel injection manifold on the opposite side, and this is literally a distribution header that was meant to inject the water uniformly across the cross-section of the tank. That's the point of highest velocity and highest turbulence.

DR. BANERJEE: And is this recirculated, the water?

MR. LETELLIER: It is continuously recirculated. The drain is at the bottom of the tank

DR. BANERJEE: And so this was like a spaja (phonetic), was it?

MR. LETELLIER: Exactly. We did design that header to try and minimize, I guess, reduce the maximum flow rate and try to avoid direct impingement on the samples. We wanted to have velocities that were representative of conditions in the containment pool throughout the long period of exposure.

DR. BANERJEE: And the drain was a multi-drain or just a single point?

MR. LETELLIER: No, the drain is a single point at the bottom of the tank, which is not shown here.

DR. BANERJEE: To get flow to the drain then.

MR. LETELLIER: Of course. That's right.

DR. BANERJEE: So does that give a region of high velocity at the bottom?

MR. LETELLIER: It does, and as Rob mentioned, we later introduced fiberglass samples in a collar around that drain area to try and increase the velocities that we were exposing our material to.

CHAIRMAN WALLIS: You have a screen down there presumably, too, don't you?

MR. LETELLIER: There was a screen just

to protect the pump from the large quantities of debris that were introduced. It was not intended as a surrogate sump screen design, but in fact, we took good advantage of that localized flow area to collect our samples.

DR. BANERJEE: But the samples you show, they're all consolidated. There's no debris there, right? If there's debris formed, it would be just washed off there just in time or what?

MR. LETELLIER: Oh, you're talking about deposits on the plates that are shown here?

DR. BANERJEE: Right. Those plates are your various materials, aren't they?

MR. LETELLIER: They do represent the stainless steel, aluminum, copper, zinc, paint and concrete that are present in containment. In addition to this we introduced stainless steel bags, if you will, of fiberglass and calcium silicate.

DR. BANERJEE: Where are those?

MR. LETELLIER: There's actually a photograph two slides down.

DR. BANERJEE: Okay. I went back.

MR. LETELLIER: And the one that B.P. showed give you an impression of.

CHAIRMAN WALLIS: But you didn't throw in zinc paint chips or anything. All of the materials were in the form of these plates.

MR. LETELLIER: Let's itemize the debris types. There are actually stainless steel mesh bags containing the Nukon fiberglass that were also immersed. There was a certain quantity of crushed concrete that was mixed into the solution, as well as dust and dirt, literally containment sweepings that would represent latent debris.

We did not specifically include degraded coatings in any way.

DR. BANERJEE: And the fiberglass or the insulation, did you chop these up or what did you do with them?

MR. LETELLIER: These were pre-shredded and also heat treated to simulate the service life of the insulation. So we did a crude form of accelerated aging on the insulation process.

CHAIRMAN WALLIS: You took off the organic coating from this fiberglass?

MR. LETELLIER: That's part of the reason for the heat treatment. This was done dry on a hot plate simple to induce a thermal gradient across the blanket. This was done before it was shredded to simulate the accident environment.

As it is immersed in the hot solution, the organic biter (phonetic) does degrade as well.

DR. BANERJEE: So when you say accelerated, you heated it up above what it would be expected to be heated at and looked at some kinetic law like Arhennius' Law or something, right?

MR. LETELLIER: It was rather crude by comparison to your description. We did do roughly a time-temperature integral to simulate the service life of 20, 25 years of plant service.

DR. BANERJEE: So the reaction is roughly double every ten degrees. Okay.

CHAIRMAN WALLIS: Now, what is in plant service? Does the organic coating evaporate off because the stuff gets hot?

MR. LETELLIER: Yes. Yes, it does.

CHAIRMAN WALLIS: So after a while it's all gone.

MR. TREGONING: Well, again, a lot of it -- we went a little bit above the reactor operating temperature, but not a substantial amount above, and what you see, you know, they do these bake-offs in the plant. Most of it has evolved very quickly, and when they did the bake-off in the Nukon here, you could smell when the bake-off was occurring certainly. So a similar type of phenomenon.

CHAIRMAN WALLIS: Sniffing Nukon is hazardous to your health.

MR. TREGONING: Among other things, yes. Yes, it can be.

DR. BANERJEE: Now, you shredded it with what, a leaf shredder or a blender or what was it that you used?

MR. LETELLIER: The product that we emersed in the tank was run through a leaf shredder so that the flocks are roughly one inch and smaller. Now, that's the case when it comes out of the leaf shredder as it sits in the boxes and as we pack the blankets, it does re-agglomerate.

So these bags were necessary to control the migration of fiber in the test, but people will argue about how realistic this represents the exposure environment in the accident. We have conducted prior testing in past years where we introduced debris into a much larger containment pool, a simulated one-tenth scale environment, and the debris has the luxury of oscillating and fluctuating with local flows. In this case it did not. We were simply trying to reproduce the exposure conditions so that the chemical leaching could occur in a proportional way.

DR. BANERJEE: Do you think the size of the shredded samples -- I mean the particle size, would that have any effect on what you see or it's independent of that?

MR. LETELLIER: The degree of shredding was minor compared to the size of the fiberglass strands. We did not chop or degrade this into an appreciably larger surface area. We physically separated it, and that's all.

CHAIRMAN WALLIS: The difference would be if fiberglass when it's intact is less reactive than when it's broken. The ends presumably are fresh, and they're sort of broken up more. They might be more reactive than the --

DR. BANERJEE: It may affect the kinetics.

MR. TREGONING: The separation of the fiberglass I would argue is probably more important then.

DR. BANERJEE: Now, in the ZOI do you form fibers or do you form particles/ Experiments, what do they show?

MR. LETELLIER: Well, in past testing that has been done with air jet surrogates looking at debris generation, you form flocs. You form shreds of the original insulation blanket.

DR. BANERJEE: But in a steam water jet, which is coming out of these enormous velocities what happens?

MR. LETELLIER: There is no direct evidence of two phased jet impingement on fiberglass. However, I still think that the primary effect would be to physically separate rather than to break the fibers.

DR. BANERJEE: There would be no erosion to particles?

MR. LETELLIER: These are very low density materials that will accelerate quickly. They're not held in place. In comparison to the calcium silicate product, for example, which is bound to the piping structures and physically experiences an erosion phenomenon, there is test data to support the behavior of that product.

DR. BANERJEE: Okay.

CHAIRMAN WALLIS: So that's an interesting statement. There's no direct evidence of two phased jet impingement effects on fiberglass.

MR. LETELLIER: Not to my knowledge. Now, there may be evolving evidence on the industry side where they're beginning to commit resources to that question.

DR. BANERJEE: So what happens? Steam generators have fiberglass insulation, don't they?

MR. LETELLIER: There is a diversity of insulation products throughout containment.

DR. BANERJEE: But many of them do have fiberglass.

MR. LETELLIER: Many of them do, yes.

DR. BANERJEE: So if you get a break near that and you hit the steam generator with a jet, it will be a two phased jet hitting this fiberglass.

MR. LETELLIER: That's correct.

CHAIRMAN WALLIS: It peels it off, I think.

DR. BANERJEE: God knows what it does.

MR. LETELLIER: Our estimates of debris generation are very large. You can easily estimate up to 2,000 cubic feet of debris that might be stripped from a steam generator compartment.

DR. BANERJEE: If you hit most of the surface, right? Ten diameters.

MR. LETELLIER: Yes, ten diameters is probably an exaggeration for the equivalent volume.

DR. BANERJEE: Why is that? A liquid two-phased jet will penetrate pretty --

MR. LETELLIER: It does extend --

DR. BANERJEE: That seems actually small to me.

MR. LETELLIER: It does extend to that distance. No question about that, and beyond.

DR. BANERJEE: Have you ever seen a steam water jet coming out? That water will make a hole through it.

MR. LETELLIER: That's true. It extends to 25 and 30 diameters. The methodology at present is to take that physical geometry and convert it to a spherically equivalent volume wherein the pressure contours are capable of damaging the insulation.

MR. CARUSO: We were told yesterday that that was not what they did, that they went out to the distance at which it was affected and then just took a sphere of that diameter.

MR. LETELLIER: In the case of coatings, you're exactly right. That assumption was made because no prior evidence existed for the damage pressure associated with a paint coating, and so the staff took a conservative position consistent with prior regulatory applications to simply, as you suggest, take a reasonable penetration distance and revolved that or rotate that into a comparable sphere.

MR. CARUSO: But they did something

differently for insulation.

MR. LETELLIER: That's correct. It was a conservative position that was adopted before recent evidence of damage pressures for coatings.

MR. ARCHITZEL: Ralph Architzel from NRR.

I just wanted to make a comment, and Bruce knows this. I just remind ACRS for the pressurized water reactors, we did take a 40 percent reduction in insulation damage compared to the air jet test which effectively tripled the volume that was used for the air jet testing for insulation materials absent two phased testing.

So it wasn't the same test volume that was used for BWRs or that were demonstrated by the air jet testing. It was a triple volume basically. I didn't want to really get into this too much, but there was some recent testing. We have had some fiberglass and that coatings test. My understanding, it would sort of confirm that the destruction was similar to what was observed in air jet test, and it was floc. It blasted all over the place with fines as well. That wasn't intended to be the target, but was over the material.

MR. LETELLIER: You're speaking of recent industry testing?

MR. ARCHITZEL: Yes.

MR. LETELLIER: Very recent testing.

MR. ARCHITZEL: It wasn't the intent to test the fiberglass, but it did basically.

DR. BANERJEE: Inadvertently.

PARTICIPANT: Collateral damage.

MR. LETELLIER: That is a topic in and of itself that we could devote a significant amount of time to. I'll just mention that more of the physical attributes are itemized on page 12. We don't need to go through them, except to mention that the flow rate, pH, and temperature were controlled through an automatic data acquisition system.

So this needed to sit unattended for 30 days with daily samples extracted for the purpose of monitoring concentrations.

MR. CARUSO: You used RO water. You didn't use tap water. Why didn't you use tap water? Everyone else seems to be using tap water. Is there a difference?

MR. LETELLIER: Well, the tap water is adopted for convenience in cases where you're looking at the fluid dynamics of head loss. We're trying to have a much more controlled environment where we're looking at chemical effects. We did not take the extra step of going to reagent grade purity water, but we felt that RO water was easy enough to do, and it was appropriate for this test.

DR. BANERJEE: But it was deionized, right? There were no ions.

MR. LETELLIER: There are subtle differences between reverse osmosis process and deionization, and we had an RO unit physically on site to generate our make-up water. I apologize I can't explain the differences.

MEMBER SHACK: Do you know what your resistivity was coming out?

MR. LETELLIER: We do. I'm not sure that I can quote that.

Jack, are you on line?

MR. DALLMAN: Yes, I am.

MR. LETELLIER: Can you help? Bill Shack is asking about the resistivity from our RO unit.

MR. DALLMAN: I don't recall right now, Bruce.

MR. LETELLIER: That was monitored just to look at the cleanliest level just to make sure that the unit was operating within its specifications.

So let's move on to page 13 to talk a little bit more about the daily tank operations. The upper frame shows loading the racks into place to initialize the test. The upper right-hand panel shows you how congested the space is to physically fit all of the coupons.

There are 374, 375 plates in each of the tests, which were all identical. The lower left panel shows you what the water condition looks like just before initiation of the test. It is very murky as we throw in the dust and dirt, and the lower right-hand panel illustrates our limited visual perspective during the test. We could basically see the edges of the plates, and at some perspective you could look across the depth of the tank to judge the clarity.

Daily water samples were extracted from the associated plumbing. The temperatures were monitored and held constant throughout the duration of the test at 60 degrees Celsius.

Page number 14 lists, in the first bullet, lists some of the common test parameters with one exception which I'll note at the bottom. You see that various types of metals were used, including concrete and zinc coated paint. The temperature was held constant. These are all ambient pressure tests.

Actually the staff used some of the CNWRA modeling results of a year and a half ago to make judgments about the pins (phonetic) of corrosion on pressures and temperatures. We were basically making a tradeoff between the very early phase of high temperature LOCA sequence compared to the long duration of moderate temperature exposure, and we opted on the size of a conservatively high, long-term exposure and simply ignoring the very short duration of high temperature.

The recirculation flow throughout the tank is 25 gallons per minute out of --

DR. BANERJEE: Did you do any sort of scoping test to look at this brief duration, high temperature on a smaller scale or anything?

MR. LETELLIER: We actually did the modeling simulations to help make that judgment, and using published literature values for corrosion rates as a function of temperature. There were no bench scale experiments done to make that determination.

That was one of the first commitments we made before designing and building the facility, whether it needed to be pressurized to sustain a higher temperature or not.

DR. BANERJEE: Well, this facility clearly is too large, but I wondered if you had done some smaller tests in that little autoclave or something, but anyway, you haven't.

MR. TREGONING: No, but a lot of the basic corrosion information for most of these metallic and nonmetallic species is pretty well known. So we've relied on literature for those parameters.

DR. BANERJEE: But this has got boric acid and stuff in it, right?

MR. LETELLIER: That's what we were less certain about, was the dependence of chemical reaction rates on pressure and temperature. That's what we took advantage of the modeling efforts to help make judgments.

The last three bullets which cite the --

MR. VIJAY JAIN: Vijay Jain, Center for Nuclear Waste.

We did do some studies with respect to the individual materials. We looked at the corrosion rates from 60 degrees to 110 degrees Centigrade, and you are right. As the temperature increases. Your corrosion rate increases by a factor of two or three. You go from 60 degrees to 110 degrees.

But if you will do the same study with a function of time as Bruce said, you can basically replicate that increased corrosion rate can be accommodated as a function of time running for 15 days instead of half an hour.

Thank you.

MR. TREGONING: Thanks for clarifying.

MR. LETELLIER: You did it with boric acid and things.

MR. VIJAY JAIN: Yes. We used the same conditions that were used in ICET test for Test No. 1 and Test No. 3.

MR. TREGONING: Thank you for clarifying that.

MR. LETELLIER: The last three bullets in the first topic of common parameters, those were held constant for Tests 1 through 4, but in Test No. 5 when we introduced the sodium tetraborate pH control system, the boron concentrations in hydrochloric acid were modified as appropriate for that test.

Tests No. 1 and 4 used the sodium hydroxide as a pH control system with a target of ten. Test No. 2 and 3 used the trisodium phosphate, the TSP, with a target pH of seven.

And test --

CHAIRMAN WALLIS: Doesn't the sodium hydroxide eat up the HCls pretty well? You put in this HCl and then you eat it up with sodium hydroxide?

MR. LETELLIER: That's true. The purpose

of the HCl was to represent chlorides that might be present from polyvinyl cable degradation due to the thermal environment. So it does not really participate in the pH system. It's simply there for the chlorides.

Next slide.

We took full advantage of our simulated containment environment to collect samples on almost everything that we could see. Because chemical speciation is very difficult in a complex system like this, we made the judgment to treat physical samples as being unique through our analysis and investigation process. We tried very hard not to presume that two things are equal, and until we had some chemical assay to help us confirm that.

So we have a proliferation of sample types. The primary fiberglass blankets you've been able to see, but in addition to that there were smaller sacrificial samples of about three to four inch square which we could extract at various time points during the test. Those were exposed in high flow and low flow regions of the tank.

We collected daily water samples obviously which were sent for off-site chemical analysis. We conducted during our daily samples, we conducted --

well, we collected both a raw, unfiltered sample, and also a filtered sample, filtered out the tap, so that we could differentiate between suspended solids.

We noted any visible precipitates and collected samples. At the end of the test we collected floor sediment which was composed of the initial particulate debris, which was introduced. In addition, any sort of chemical products which happened to form and settle.

The metal coupons were examined for corrosion, corrosive attack. In one case, in test number one, we collected significant quantities of precipitate, this sludge material which was produced after cooling the test solution.

We also examined the tank and pipe residue during our cleaning process and in Test No. 3 noted significant build-up of material internal to the circulation system.

DR. BANERJEE: Build up in what sense? Was it --

MR. LETELLIER: It was a surface coating. We do have a back-up slide to that demonstrating that.

DR. BANERJEE: It was adherent?

MR. LETELLIER: It was adherent, loosely adherent. It was easy to remove by mild agitation, but, in fact, it built a substantially uniform coating on the walls of the pipe, on the struts of the flow meter, and including fouling the turbine at one point.

DR. BANERJEE: What was it?

MR. LETELLIER: Test No. 3, if you'll remember from the matrix, was our first experience with calcium silicate in combination with trisodium phosphate. That test was unique as we explained in July in that we observed a chemical flocculent within 30 minutes of the test. It was visibly suspended in the tank. We're speculating that that was a calcium phosphate product, and in fact, the compositions of the deposits on the walls are consistent with that assumption.

I'm hesitating to say that's exactly what it was because it is a very complex chemical reactor, if you will.

The EDS analysis that we did showed calcium phosphorus and oxygen in the proper proportions for calcium phosphate.

DR. BANERJEE: Was it in suspension then, this floc?

MR. LETELLIER: Visibly so.

DR. BANERJEE: Do you think that's what happened?

MR. LETELLIER: Very early in Test No. 3 it was visibly apparent swirling in the circulation flow, and we can examine the turbidity to show how quickly it settled, over what rates the water clarified, and at the end of Test No. 3, as B.P. showed in his figure, substantial quantities of this material on every surface.

DR. BANERJEE: Would it also be on the reactor core surfaces?

MR. LETELLIER: It very likely would be. That's one of our principal findings from Test No. 3.

DR. BANERJEE: How thick was it there?

MR. LETELLIER: On the walls of the pipe or in the tank itself?

DR. BANERJEE: Wherever.

MR. B.J. JAIN: The tank was about an inch and a half or so.

DR. BANERJEE: and the walls of the pipe?

MR. LETELLIER: One-eighth inch nominal.

DR. BANERJEE: Of that order.

MR. LETELLIER: Of that order.

CHAIRMAN WALLIS: I thought it was thinner than that. It was actually an eighth of an inch on the pipe?

MR. LETELLIER: There's actually a backup slide that you can flip to.

DR. BANERJEE: And it's friable, very friable.

MR. LETELLIER: No, it's -- I'd have to describe it as a finely divided particulate. It is not brittle in any way. It could be scraped or removed with mild agitation, but in fact, it was surprisingly adherent under flow.

DR. BANERJEE: So it's a bit like fouling of some sort.

MR. LETELLIER: Yes.

PARTICIPANT: Yeah, you have to page through or try and blow it up.

MR. LETELLIER: Yeah, I think it is the very last slide in your package.

DR. BANERJEE: That certainly is an interesting finding.

MR. LETELLIER: It is. The upper right-hand panel shows the whitish-yellow deposit with a scraped section that shows a fingernail or some sort of cleaning that was attempted. This material built up on the streets of the flow meter on the left-hand panel.

DR. BANERJEE: And it was only observed in Test No. 3 or in all the tests?

MR. LETELLIER: Primarily Test No. 3.

MEMBER SHACK: Well, you've labeled them here as CalSil rather than calcium phosphate.

MR. LETELLIER: I apologize for that. As I said, we were trying not to presume what the actual chemical composition was. Because this test was our first experience with calcium silicate as a debris type, in very high concentrations, that's certainly one of the constituents.

It is noteworthy, however, that the other labs who have -- and actually Los Alamos has conducted head loss testing with calcium silicate. We've never experienced the same type of accumulation, and I don't think that Argonne or PNNL has experienced that either, at least not with tap water and nominal temperature conditions.

So it's not unreasonable to attribute this to a chemical product formation. It's something unique.

DR. BANERJEE: You didn't see any deposit in any of the other tests?

MR. LETELLIER: Not on the internal -- well, that's not true. Jack, if you'd like to make a comment on our cleaning process for the other tests.

MR. DALLMAN: Well, we cleaned the piping and the tank thoroughly between tests, and I recall that after Test 3 that was certainly the hardest test to get it cleaned up.

We did see some of this what's called deposits on the other tests, but not nearly to the extent of this one.

DR. BANERJEE: What was not nearly? It was much thinner than one-eighth inch or did you find any deposit at all?

MR. DALLMAN: On the other tests?

DR. BANERJEE: Yeah.

MR. DALLMAN: Yes, we did. I mean, it was very fine, and I would not say that this was one-eighth of an inch thick by any means.

MR. LETELLIER: Not as thick as that.

MR. KLEIN: This is Paul Klein from NRR.

If I could jump in for a second, Bruce, my recollection at the end of ICET 3 was that the deposition on the side of the tank was probably less than a sixteenth. It certainly wasn't an eighth inch thick deposit that I recall.

MR. LETELLIER: Not in the tank. I was basing that on my just visual assessment of these turbine struts in the panel, which are very, very thin to begin with and they've formed an accumulation.

Maybe Jack would describe the deposits in other tests as a residue that was actually flushed during our cleanliness procedure.

Test No. 3 took a fair amount of effort to clean the pipes to our tolerance.

DR. BANERJEE: So where did this fine stuff come from? I mean, what do you speculate the origin was?

MR. LETELLIER: The calcium silicate debris was precrushed and contains a large, large quantity of fine particulate by itself. What was most interesting about Test No. 3 is the formation of a chemical product that's also extremely fine. I would like to say that it's a crystalline product in the case of calcium phosphate, very small particulate.

MEMBER SHACK: I mean, you had vast amounts of CalSil in Test 4, again, and you're not seeing a deposit.

MR. LETELLIER: That's right. That's why I'm saying that the chemical reaction behaved in a unique manner. It's not simply the calcium silicate by itself. We simply don't see accumulation on internal piping from CalSil in exclusion.

DR. BANERJEE: It's very interesting.

MR. LETELLIER: Jump back to page --

CHAIRMAN WALLIS: B.P. said something about an inch thick tank wall. Did I mishear that?

MR. LETELLIER: In the bottom.

PARTICIPANT: Sediment.

CHAIRMAN WALLIS: In the bottom.

MR. LETELLIER: Settling on the bottom. That was in the introductory slide. It showed a very good photograph of that material.

DR. BANERJEE: Slide No. 8.

To return now --

CHAIRMAN WALLIS: We're going to have to speed up, I think. Maybe when we get to the figures we can pick out the ones which are most interesting.

MR. LETELLIER: Sure.

CHAIRMAN WALLIS: Or are they all backups?

MR. LETELLIER: I'd like to present some of the trends actually.

Page number 16 is just a laundry list of the diagnostics and supporting analyses that we brought to bear. I will, however, note that chemical speciation is a very difficult task. We were moderately successful in the case of precipitates from Test No. 1 at conducting nuclear magnetic resonance to actually look at the chemical bonds in the precipitate and confirm that at some phase of aluminum hydroxide.

Beyond that, elemental compositions in proportionality is the most accessible diagnostic that we have.

So moving into a survey of results, we'll focus on Test 4 and 5 because the committee has not seen this information before.

CHAIRMAN WALLIS: But Test 3 is also the interesting one, isn't it?

MR. LETELLIER: Test 3 is --

CHAIRMAN WALLIS: You might want to say a bit about that.

MR. LETELLIER: It is also included as backup slides, and we're free to look at that as well.

In Test No. 4, just to remind everyone, this was a sodium hydroxide pH system with 80 percent calcium silicate, 20 percent fiber. My sort of bird's eye view of this is it was one of the more benign tests as far as chemical product formation goes. So there's not a great deal to say in the observations.MEMBER SHACK: You've actually found a beneficial effect of calcium.

MR. LETELLIER: That's perhaps true. In Test No. 4, although we had a high pH, we had very little aluminum corrosion, and it may well be due to a surface passivation by the presence of the calcium.

One of the few good things that I could say about it.

MR. TREGONING: Let me just elaborate a little bit. Test 4 was nominally similar -- I don't want to say identical -- to Test No. 1 with the sole exception being the incorporation of CalSil in Test 4, the same buffering agent, and in less amounts of Nukon in Test 4, and in Test 1 we saw relatively copious amounts of precipitate at least at room temperature. We didn't see that at all in Test 4.

So there's an indication of at least one single variable change having a relatively large effect.

MR. CARUSO: All of the metallic samples were electrically isolated from one another, correct? So there was no galvanic -- opportunity for any galvanic results.

MR. LETELLIER: The coupon racks that were shown in the previous figures were made of chlorinated polyvinyl chloride. They're plastic pipes, and they are simply physically restrained in the slots. They are not physically touching in any way.

We did perhaps observe some evidence of electroplating transfer between copper and aluminum plates in one very localized case. There was no evidence of pitting or aggressive electrochemical behavior.

So I think that our coupon racks were reasonably successful in that regard. They served a convenient way to load and to manage a large volume of samples.

CHAIRMAN WALLIS: Well, presumably someone has done this. I Mean, the difference between one and four is some sort of effective calcium preventing aluminum coming into solution. You could do that in a benchtop test, couldn't you?

MR. LETELLIER: Indeed, we've tried to reproduce some of that. It's a plausible explanation for the suppressed aluminum concentration.

CHAIRMAN WALLIS: Yes, but it would be useful to have some check that it wasn't some peculiarity of a particular experiment if you could duplicate it in some other way.

MR. LETELLIER: As we get into some of the trends and concentrations, you'll see that there's a very large reservoir of calcium. I've tried to remember. Do we have 50 pounds?

PARTICIPANT: Fifty-five pounds.

MR. LETELLIER: Fifty-five of crushed

calcium silicate, which basically almost fills the lower one-third, I guess one-fourth of the tank. Up to the level of the bottom of the coupon rock is completely full of this sludge material. Not all that is immediately available to dilution, but nonetheless, there's a very large inventory.

CHAIRMAN WALLIS: It's a huge amount.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: That's not typical of a plant though.

MR. LETELLIER: No, it is actually.

CHAIRMAN WALLIS: It is?

MR. LETELLIER: The volume, the amounts were scaled in proportion to our estimate of debris generation. What is obviously not preserved here is the spatial scaling of the containment versus the tank. This is a much more congested environment, and, in fact, were it not for the fact that our calcium concentrations are very high and remained noticeably high, you might suspect that we were isolating this material in some artificial way.

We talked about mixing this tank or repeating this in some other manner, but the fact is you have more than enough CalSil participating here. This dominates the chemical system.

DR. BANERJEE: So the volume or the mass of various components to the water is preserved in comparison to the real system, right?

MR. LETELLIER: To our best available estimates, yes.

DR. BANERJEE: Yes, it is estimated.

MR. TREGONING: Let me clarify Bruce's CalSil statement. This CalSil load was I don't want to use the word "representative." It was certainly chosen to be more of a conservative type of estimate. It was based on the assumption that you had a break near a steam generator that had CalSil insulation.

PARTICIPANT: And it took 5,000 cubic feet.

MR. TREGONING: Yeah, it took 5,000 cubic feet of CalSil off. Most plants do not have -- in fact, I'm not aware of any plant that has that concentration of 80 percent CalSil compared to 25 Nukon. Most of the plants are down around 20 percent CalSil, and many have as they're replacing steam generators and doing other plant modifications, removing all of the CalSil that they can.

So even at this point in time, that CalSil loading represents a bounding condition that I wouldn't expect in any of the plants as they're currently configured.

DR. BANERJEE: So what would happen at a lower CalSil rate?

MR. TREGONING: Well, that's a very good question, and that's one of the questions that this testing has raised, especially with this passivation phenomena. How much do you need to enjoy the benefits of passivation in this one instance?

MR. LETELLIER: But nonetheless, Argonne has showed that the dilution rates of calcium are very, very rapid. So it doesn't take a large physical quantity to dominate and saturate the chemical inventory of calcium in the system. We could argue about how much physical product, but nonetheless, it's available, readily available.

So let's move on to the photograph on page 19.

CHAIRMAN WALLIS: I'm puzzled by that because I thought you crushed the CalSil, but it looks like rocks down there.

MR. LETELLIER: We tried our best to reproduce a size distribution at --

CHAIRMAN WALLIS: It looks like big rocks.

MR. LETELLIER: Indeed, a large portion of the volume is in physical sizes three inches nominal diameter.

CHAIRMAN WALLIS: Didn't crush it very well then. I mean it's pretty friable stuff.

MR. LETELLIER: That represents approximately 40 percent of the inventory. Sixty percent was crushed to fines.

CHAIRMAN WALLIS: Which is probably on the bottom?

MR. TREGONING: If you see the sediment to the left, a lot of that is mixed particulate CalSil with -- in fact, in that test it's largely particulate CalSil.

CHAIRMAN WALLIS: -- enough anyway whether it's crushed or not.

MR. LETELLIER: Yes, and it's a very porous medium nonetheless.

This photograph is markable by its comparison to Slide No. 6 in the introduction. This is a calcium silicate test, but you see only minor residue on the piping. You don't see the large volumes of secondary product.

MR. TREGONING: And the residue is the white scale. It's nominally gray CPVC. So some of the white scale you see there.

DR. BANERJEE: So this is after the test?

MR. LETELLIER: Yes. This is after 30 days when the water has been drained.

DR. BANERJEE: So how did you get sediment to go on one side?

MR. LETELLIER: It's a little bit deceiving. This bag of CalSil is actually placed in last.

DR. BANERJEE: Oh, I see.

MR. LETELLIER: It's on the top.

Test No. 5, general observations. This test was unique in that we were investigating a sodium tetraborate system, which is utilized for ice condenser plants. The sodium tetraborate is frozen into the ice column, and it is released gradually during the ice melt.

We made every effort within the limits of our experimental apparatus to reproduce the time history of the chemical introduction. For example, sodium hydroxide sprays, we tried to match the maximum initial pH and to introduce it over a time period so as not to exceed our expectations for the actual plant conditions.

And similarly, for the sodium tetraborate we introduce a portion of that inventory before the sprays and a portion during the sprays in order to match most closely our understanding of the accident rates.

The turbidity of the tank was very small, .7 NTU before latent debris and the concrete dust, about 14 NTU after the latent debris. Just a little bit of intuition about these unfamiliar units. Nephelometric turbidity units are a little bit arcane to my experience. As you would visually look through the windows of the tank, a level of one NTU represents visual clarity through the depth of this tank. Above that there's a noticeable level of suspension.

MR. TREGONING: And tap water is about .5. So just for frame of reference there.

MR. LETELLIER: This test seemed to remain turbid. It was cloudy much longer -- well, relatively longer than the other tests, and we'll look at the trend in a later slide.

For example, we could not see through the depth of the tank until after day six.

CHAIRMAN WALLIS: Even though this stuff is -- well, it is being stirred. So I guess this is all in suspension around the whole loop with this stuff.

MR. LETELLIER: It is never mechanically stirred. It's only agitated by the --

CHAIRMAN WALLIS: -- stay in suspension.

MR. LETELLIER: That's correct. The other tests would clarify and settle relatively quickly, within two to three days' time frame.

The hydrogen generation, we're going to look at a slide. It remained very low through the first half of the test, and it was basically nondetectable thereafter, which hydrogen generation is an indicator of corrosive behavior which was clearly evident in Test No. 1. It was almost nonexistent here.

I will qualify and say that our measurements of hydrogen were executed to serve a safety function. We were concerned about flammability limits. It was a rather crude field survey instrument and was not collected to be quantitated. However, I believe that it's very useful as an indicator of reactivity in the tank.

CHAIRMAN WALLIS: Well, those hydrogen bubbles might affect what happens in the sump if they were to be formed there.

MR. LETELLIER: It's interesting to note -- we'll look at the slides later -- that even in Test 1 where the hydrogen generation was clearly evident, the turbidity remains very low and so --

CHAIRMAN WALLIS: Yeah, but hydrogen in combination with particulate matter.

MR. LETELLIER: There's a large quantity of particulate present, but the turbidity remains low, which indicates to me it's not agitated to the point of mechanical resuspension, and there's no significant buoyancy change as well.

MR. TREGONING: We did see in Test 1 some bubble formation on the surface of especially the aluminum specimens, right?

MR. LETELLIER: Yes, yes.

MR. TREGONING: But at the risk of being overly simplistic, it was -- you know, there was a relatively periodic sort of -- it wasn't like it was deluged with hydrogen initially. It was relatively consistent at least over the first ten days or so of the test. Is that an accurate characterization?

MR. LETELLIER: I'd have to defer to someone who had daily visual experience, but the traces of hydrogen generation for Test 1 show a fairly constant level of activity, and yes, there are micro bubbles present on the surfaces. It --

CHAIRMAN WALLIS: Of the aluminum?

MR. LETELLIER: Of the aluminum primarily.

We've conducted various exposure tests looking at hydrogen generation all the way from reagent grade aluminum powder that's held in inert environments. That literally fizzes like Alka Selzer as it corrodes very rapidly compared to an industrial grade sample which has a well established oxide layer, which is passivated to these exposure conditions.

PARTICIPANT: Thanks for clarify that.

MR. LETELLIER: Continuing on page 21 with general observations, remember now this is similar in pH to the sodium hydroxide conditions. We did observe visible precipitates in the daily water samples after they had cooled to room temperature. Remember we're extracting 100 milliliter samples and archiving them, sending them for analysis.

And also Test No. 5, we had learned enough, anticipated enough about our concerns that we were able to do a rudimentary heat exchanger test where we took some of the raw solution from 60 degrees and rapidly cooled it over a period of about ten minutes, and we were able to visibly see formation of precipitates.

It was a rather qualitative test at that time, but we were able to collect some direct evidence of head loss behavior, which will be presented in the future.

No apparent trend in the kinematic viscosities. Relatively little discoloration and mass loss in the metal plates by comparison to other tests.

CHAIRMAN WALLIS: I think you're measuring dynamic viscosity. We had this little -- kinematic is Nu over Rho, isn't it?

MR. LETELLIER: Yes, and these are all presented in meters squared per second. These are kinematic.

CHAIRMAN WALLIS: So they are Nu over Rho.

MR. LETELLIER: Yes. Some clarification to the very last bullet. The fiberglass condition, I should have just rewritten this. It was relatively clean compared to other test, but there were minor floc deposits on the interior and the exterior, but only minor particulate deposits on the exterior of the fiberglass samples.

The remaining slides are probably best presented on the floor of your living room with everything out in front on the table. This is the first opportunity we've had to look at the trends over the full 30 days for all five tests in combination.

Pages 22 and 23, you can compare those two side by side. It represents the kinematic viscosity at room temperature compared to the same viscosity at the test temperature of 60 degrees.

Test No. 1 shows a clear departure as visible precipitates were being formed more and more rapidly during the course of the test at room temperature, but at 60 degrees there was never any visible precipitate formed.

There are some reference points noted there in the text box, what pure water, the behavior of pure water would be at the same temperatures, and in all cases, you can convince yourself that this was at the measurement accuracy or slightly above the behavior of water within the limits of the measurement.

There is a slight bump in the test temperature for viscosity for Test No. 1 which represents a procedural change. These tests were confounded --

PARTICIPANT: You mean at 60 C.

MR. LETELLIER: Yes, at 60 C. I can point to it on the viewgraph at approximately day number 13 or 14. We had to modify our procedures to account for the unexpected presence of chemical precipitate.

DR. BANERJEE: So the increase at 23 degrees Celsius for the Test 1 you attribute to the formation of the suspension due to precipitates.

MR. LETELLIER: Clearly so. This was a visible quantity of material. In fact, we had difficulty executing the procedure as we extracted it from the tank and moved it to a water bath. The solution would floc in real time, visibly so.

DR. BANERJEE: And it would stay suspended. It wouldn't settle out.

MR. LETELLIER: It did settle over the course of -- I don't know -- a few hours, tens of minutes to an hour. In fact, at the end of the test it's useful to look at our archive bottles and just simply look at the depth, increasing depth of the precipitate over time.

DR. BANERJEE: So these measurements were made almost immediately after extraction, and so they were still suspended.

MR. LETELLIER: That's correct.

DR. BANERJEE: So there's sort of a stock (unintelligible) effect.

MR. LETELLIER: That's right. So there's a clear perturbation of the particulate to the viscosity measurement.

MR. TREGONING: I think these were made within ten minutes of extraction. Even the room temperature results, the higher temperature of viscosity measurements were made immediately obviously.

MR. LETELLIER: We corrected our procedure in mid-course to introduce a water bath so that they could be immersed immediately, you know, within seconds. So until the apparatus was properly prepared to avoid temperature fluctuation.

MR. CARUSO: Why didn't you see this effect in ICET 3?

MR. LETELLIER: That's a very good question.

MR. TREGONING: Do you mean ICET 3 or 4? ICET 3 if you've got your matrix is a TSP buffered environment. ICET 1 is a sodium hydroxide buffered environment.

MR. CARUSO: Which was the one that produced the large amounts of precipitant?

MR. TREGONING: ICET 3.

MR. LETELLIER: I understand your question. I think if we look at the turbidity, you'll see that although this precipitate was formed very quickly, it also settled. So it was not present in solution as a solid.

MR. CARUSO: So these were measurements of the solution without the precipitant in ICET 3, but in ICET 1 the precipitant was still there, and it had an effect.

MR. LETELLIER: At the test temperature in Test No. 1, there was never any visible precipitate, but when we extracted a raw sample, it would immediately flocculate. So there's a very high concentration near a saturation or agglomeration point.

DR. BANERJEE: So due to the cooling, right?

MR. LETELLIER: Yes. It was a temperature effect.

MR. TREGONING: It becomes visible upon cooling. I think that's an important observation.

MR. CARUSO: Does that imply that if you had changed those ICET 1 conditions slightly, you may have seen a lot of precipitant form?

MR. LETELLIER: There has been that speculation, that if the test had been executed at a lower temperature, in effect, we would have reached a saturation point.

MR. CARUSO: I mean not just temperature, but I mean if you had changed the -- let's see. That was sodium hydroxide?

MR. LETELLIER: Yes.

MR. CARUSO: If you had changed the sodium hydroxide slightly, if you had changed the boron concentration slightly, if you had changed something else slightly, you may have seen a lot more precipitant?

MR. LETELLIER: It is possible. I would be most suspicious of the temperature. That's why I'm emphasizing the fact that it precipitated upon cooling because our tests were isothermal in nature, but the power plant is not. There are clearly heat exchangers and other temperature variations.

MEMBER SHACK: But since we have to try to replicate this stuff without going through this thing, what we do find is, of course, it is a tricky balance between temperature, pH and concentration. It's not surprising because if you were at a different pH, you would get to a different concentration, and obviously. So the concentration and the pH are not independent variables, you know. You have to kind of couple them together. So to say what would happen at a different pH is a little tricky to answer because you don't know what the corresponding concentration. If you kept this concentration and lowered the pH, yeah, you're going to drop stuff like a rock.

MR. CARUSO: I'm just saying there are lots of degrees of freedom here, and you know, you found that when you lowered the temperature, but maybe something else might have precipitated it.

MR. LETELLIER: There are. As we struggle with the issue of chemical speciation, trying to determine exactly what chemical form these materials have, you begin to understand that the phase of the association between these materials affects its physical solubility. It affects the saturation levels. It affects the physical formation of colloids and larger agglomerates.

DR. BANERJEE: So why did you measure kinematic viscosity? Because it seems to me that it's a function of time after you do this cooling or whatever, you know. So it's not a robust measure. It's an indicator of maybe a suspension, if nothing else.

MR. LETELLIER: Indeed, that's very true, and we modified our procedure to accommodate the time dependence. So we've proceduralized the cooling times and the measurement times in order to have a common basis for comparison.

But, in effect, this was exploratory in nature. We knew that turbidity could be a sensitive indicator of the presence of products, and indeed, we found some.

MEMBER SHACK: It's still interesting though. I mean, you know, we've had discussions before whether there's a trend. I think you really have to agree there's still an upward increase in viscosity there from Day 15 on where the concentration is constant.

DR. BANERJEE: So you've got more stuff suspended.

MEMBER SHACK: No, no. If he shows his aluminum concentration.

MR. LETELLIER: That's right. If we could flip back, we'll see that. The aluminum concentration has apparently flat lined. It has reached some equilibrium level. I won't say that it is saturated because I don't understand the physical chemistry, but nonetheless it has stabilized, and Dr. Shack is right. The room temperature viscosity continues to increase.

As I describe it, these samples would precipitate faster and in larger quantities as the test progressed.

CHAIRMAN WALLIS: There's an anomalous dip at three weeks. I don't think we need to spend too long on this slide, do we?

MR. LETELLIER: No. Let's move on.

CHAIRMAN WALLIS: I'm not sure this is going to affect the sumps very much.

MR. LETELLIER: These slides will evoke many questions --

CHAIRMAN WALLIS: I bet they will.

MR. LETELLIER: -- that I'm not prepared to answer. There are many open questions here, although there are coherent pictures. Explanations of the trends and the behavior of our chemical systems are starting to make sense. They're starting to emerge.

I will note a couple of --

DR. BANERJEE: So this is the supernatant solution you're taking off.

MR. LETELLIER: That's correct.

CHAIRMAN WALLIS: Yes.

MR. LETELLIER: Page number 24 simply illustrates that the pH is relatively stable throughout the test. They were initialized as close as possible to a target level, and they did not drift.

You should understand that we made no attempt to control the pH. The test was initialized and observed.

Slide number 25 is one of the obviously more interesting and somewhat erratic plots. However, the quantity of suspended solids is also an important indicator of --

CHAIRMAN WALLIS: To see the yellow one you have to sort wiggle this thing around.

DR. BANERJEE: You can't see it here.

MR. LETELLIER: It is very difficult on the photograph. If you look at the figure on the wall --

CHAIRMAN WALLIS: You don't see it at all? It doesn't appear at all on the black and white ones.

MR. LETELLIER: If you look at the screen, you can probably trace it. You can make out between the dots what the trends are. That is for Test No. 3 in yellow.

CHAIRMAN WALLIS: Well, the other one is something remarkable on Day 24.

MR. TREGONING: That measurement.

MR. LETELLIER: It may very well be an anomalous measurement on day 24.

(Laughter.)

MR. LETELLIER: For Test 3.

MR. TREGONING: Not using that word, remember?

DR. BANERJEE: How do you do these suspended solids?

MR. LETELLIER: This is actually measurements that's made by our water quality laboratory off site, and my understanding is they do an ultra filtration basically to look at the comparison of suspended --

CHAIRMAN WALLIS: These are milligrams per liter. All you need is one little grain, which is big, and it warps everything.

MR. LETELLIER: That's true, but if we look at the next few slides on turbidity, you will see that the supernatant is very clear. It's very clean, except for the finely divided material.

MR. CARUSO: What sort of error bars should appear on these data points?

MR. LETELLIER: That's a fair question that's immediately begged by a plot of this level of variability. The measurements at the lab were taken within quality assurance standard to the limits of accuracy, and their methods were repeatable. Those errors vary according to the concentrations that were measured and we can look that up for you.

What I would like to say is that the measurement errors were small compared to the variability between the samples, and that's what we're seeing evidence of here.

CHAIRMAN WALLIS: Now, the lines you show here through the dots, those are the theoretical predictions?

MR. LETELLIER: No. No, sir. They're simply spline threads to guide the eye.

MR. CARUSO: I mean, what would you estimate that they are? On the order of a few units or tens of units or a fraction of a unit?

MR. DALLMAN: Bruce, are we talking about TSS?

MR. LETELLIER: Yes, we are.

MR. DALLMAN: Those were not sent to the lab. That was just the difference in weight of our filter paper.

MR. LETELLIER: That explains a lot of the variability. These are very small quantities. Jack, this is the comparison between the raw water sample and the filtered sample; is that correct?

MR. DALLMAN: I'm sorry. I'm having a hard time hearing you, but I just realized what you're talking about. The total suspended solids we determined just by weighing the clean filter paper and then the filter paper after we had run solution through it. And so there is a fair amount of -- as you said, there's not much difference in weight. So there could be a lot of uncertainty here.

MR. LETELLIER: But the accuracy of the mass measurement is within one-hundredth of a milligram.

MR. DALLMAN: That's correct.

MR. LETELLIER: Typically. The volumetric measurements are more uncertain.

DR. BANERJEE: So let's understand the protocol here. You take the sample out, and then you let it settle for a period of time. So you take out the larger things?

MR. LETELLIER: No, as Jack --

DR. BANERJEE: How do you do these experiments?

MR. LETELLIER: As Jack reminded me, there is a sample tap on the primary plumbing. So when we extract the water, it comes directly out of the flow, and we are able to measure the mass of a filter paper and attach it to a cassette and draw off a measured volume of water. And by drying the filter and comparing the weights, we have evidence of total suspended solid.

DR. BANERJEE: So this is recirculating

water. You're drawing it off as you go along.

MR. LETELLIER: Yes, that's right, and these do have a limit of filtration efficiency. These are, I believe, Jack, .45 micron filters.

MR. DALLMAN: Yes, yes.

CHAIRMAN WALLIS: How big is the sample in liters that you draw off?

MR. TREGONING: About 100 milliliters?

MR. LETELLIER: These were also the 100 milliliter samples, Jack?

MR. DALLMAN: I believe so, yes.

CHAIRMAN WALLIS: A hundred milliliters. So you're talking about milligrams per liter. You're talking about measuring pretty accurately.

MR. LETELLIER: Averaged over a volume of 100 milliliters. Now, we also extracted higher volume samples periodically, but not daily.

CHAIRMAN WALLIS: The one speck of dust would make a difference here.

MR. LETELLIER: It would. That substantiates my comment that the variability in the samples is driving the behavior here, not the accuracy of the measurement.

Now, the filtration efficiency is important because we have evidence that there are very large inventories of aluminum perhaps bound in colloidal formations that would happily go through this filter. So although they're present in the liquid, there's a fine line between suspended solid and colloidal suspension.

DR. BANERJEE: Did you do any light scattering?

MR. LETELLIER: We did, not as a daily exercise and to some limited success for particle sizing.

DR. BANERJEE: These are colloidal. Do you think these are colloids?

MR. LETELLIER: We do. In fact, there will be later presentations in the summer that show some of our particle sizing looking at the aluminum hydroxide suspensions.

CHAIRMAN WALLIS: These are all the tests.

MR. LETELLIER: We can quickly flash through the turbidity, Slides 26 and 27. It's important to note that most of the tests quickly clarify at the test temperature they're substantially clear within three to four days, and test number two seemed to take slightly longer by comparison.

At room temperature, the turbidity follows the viscosity trend for Test No. 1, and there is a typographical error on Slide 27. This is turbidity measured at 23 degrees C., the same as the viscosity.

DR. BANERJEE: Is it possible to get a volume fraction of suspended solids from the turbidity?

MR. LETELLIER: We have attempted in the past to do some controlled calibration where we introduce a known mass of material into a suspension and try to calibrate the units. The turbidity meter, turbidimeter is an optical measurement, and I apologize. I don't know enough about the theory to speculate on the success of that conversion factor. It's primarily a physical dimension and has nothing to do with the mass of the material.

DR. BANERJEE: If you filtered this in the way you did before, there was no trend, if I remember, to the mass for ICET 1. There was just --

CHAIRMAN WALLIS: Well, he's speculating that the colloids go right through the filter.

DR. BANERJEE: Oh, I see.

CHAIRMAN WALLIS: But the affect the turbidity now.

DR. BANERJEE: Yes.

MR. TREGONING: At the risk of over generalizing, I think most of the measurements that we conducted in all of these tests, there was not a significant difference between filtered and unfiltered results in general.

MR. LETELLIER: That's correct.

I just noticed something. Ralph had a question about the presence of the precipitate and test number. No, I'm sorry. Test No. 2 -- I'm going to retract that statement.

MR. TREGONING: You didn't make it.

MR. LETELLIER: Just in time. Let's move on to Slide 28, the hydrogen generation. Test No. 1 obviously shows the most activity as far as our semi--

CHAIRMAN WALLIS: What is the unit on this?

MR. LETELLIER: These are percent composition.

CHAIRMAN WALLIS: Of the gas.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: So it doesn't tell you about the rate of evolution.

MR. LETELLIER: That's right. It does not. There is some qualitative information. For example, in the curve for Test No. 2, which is the solid diamond, there is a modest increase at Day 11. Just as an exercise, we closed the passive ventilation lines to let this accumulate and so the slope of that curve does tell you something about the rate of evolution, but keep in mind these gas samples were taken from the head space, and it is not designed as an airtight system. The measurements were done from the point of view of safety consideration, but nonetheless the trends here are evident.

In Test No. 1, there were several days, several times where we would remove the cover for various reasons, the equipment hatch, and completely evacuate the head space through passive ventilation. that's evidenced here through these dips in the trend.

DR. BANERJEE: So the highest one is Test 1.

MR. LETELLIER: Test 1, yes.

CHAIRMAN WALLIS: That's the aluminum, yeah.

MR. LETELLIER: And now we're getting into pages 29 and 30. We're beginning to look at the trends in elemental concentrations. Test No. 1 showed the highest levels of aluminum in the solution, and here's where you can see that there may have been evidence of some plateau in effect.

It's very tempting to say that this is dissolved material, but there are very fine definitions between suspended and dissolved ions in solution. Because we speculate that there's a large inventory of colloids, I would not describe this as the dissolved aluminum inventory.

There's clearly a bad data point on Day 16 that's suspicious, but the trend is there nonetheless.

Test No. 5 also showed a measurable aluminum concentration. None of the other tests really appear on this scale, and in many cases they were not reported, period.

What is notable for Test No. 1, which we'll see later, is the silica concentrations were surprisingly low. We had some evidence from beaker tests and simulation studies that the fiberglass should be leaching substantial quantities, and in fact, --

CHAIRMAN WALLIS: This is what you found in the tests you did before this.

MR. LETELLIER: Yes, in the bench scale.

CHAIRMAN WALLIS: It was a big discovery, as I remember. There was a lot of silica.

MR. LETELLIER: The silica concentration is shown on page 32, and for Test No. 1 it's extremely low. That surprising effect led us to investigate more, looking at the interactions between the silica and the aluminum phases and how they affect the joint solubility.

So in Test No. 1 we speculate that the evolution of silica was suppressed by the high concentration of aluminum, and conversely, in Test No. 4, we had a suppressed aluminum concentration with a high inventory of silica, which came from the calcium, calcium silicate.

So there's sort of a converse relationship here where they're interacting in a dominant fashion, but on opposite ends of the concentration spectrum.

Calcium concentrations on page 30 --

CHAIRMAN WALLIS: How do I convert this to what's in the sump? I've got milligrams per liter. So how many liters are there in the typical sump? I'm trying to get some idea of how much stuff there is in a typical sump.

MR. LETELLIER: Five hundred thousand gallons or less.

CHAIRMAN WALLIS: Could you do the arithmetic?

PARTICIPANT: Two million liters.

CHAIRMAN WALLIS: Roughly two million liters?

MR. LETELLIER: Or slightly less.

CHAIRMAN WALLIS: If I multiply milligrams by two million or at least a million, so I get kilograms.

DR. BANERJEE: And this is all sort of --

CHAIRMAN WALLIS: So I get something like hundreds of kilograms.

DR. BANERJEE: Clear liquid.

CHAIRMAN WALLIS: That makes it sound more dramatic than milligrams per liter.

MR. LETELLIER: Indeed. And some of the small scale chemistry work that was done indicated that, for example, solubility saturation limits would be reached within a few kilograms of corroded material, 23 kilograms of aluminum in a million gallons of water, for example, represented the saturation point for -- Mark will correct me -- for a crystalline form, a solid phase of aluminum.

Now, we're seeing much higher inventories of aluminum than the solid phase would suggest, but much lower levels of aluminum than the amorphous phase would permit. And so we're stuck somewhere in the middle, and it's a very complicated system between the aluminum and the silica that are in the solution.

CHAIRMAN WALLIS: When all of this is done, is someone going to tell us what this means for the sump program?

MR. LETELLIER: We are --

CHAIRMAN WALLIS: It's fascinating.

PARTICIPANT: Dr. Shack.

CHAIRMAN WALLIS: Dr. Shack will.

MR. LETELLIER: We are presently working to analyze this information and put it in context. I suspect we will fall short of implications. It's probably LANL's job and responsibility to explain what we've seen. Extrapolating that to the plant condition is another level of effort we'll talk about.

DR. BANERJEE: But this is all relatively clear liquid.

MR. LETELLIER: Yes, visibly clear.

DR. BANERJEE: Visibly clear liquid, and these are, let's say, the unfiltered calcium. Is this also in colloidal form you think or let's say you're trying to explain this stuff, right? Or is it in solution?

MR. LETELLIER: I have less information available about the physical form of the calcium. For example, Test No. 3, which obviously was introduced through the calcium silicate as a dominant source term, some of this material is suspended as a physical particulate.

Bill would you like to comment on the true dissolution of calcium?

That's also very high as a constituent.

MEMBER SHACK: The CalSil fines will remain suspended for a long time. So that you will get some contribution there, but you also get substantial dissolution of the CalSil. So you probably have a combination of both.

DR. BANERJEE: So this curve there is a combination of suspended --

MR. LETELLIER: And it's really a question of which dominates. I would suspect that this is more soluble than suspended based on --

DR. BANERJEE: How did you determine this curve?

MR. LETELLIER: This is done by ICP analysis. This is part of our daily water sample that's sent off site. So there's actually a spectral signature of concentration.

DR. BANERJEE: but will that give you the suspended concentration as well?

MR. LETELLIER: No, it gives you the total

MR. TREGONING: It can't differentiate what the form is. The only way to do that, if you filter something out, if it's physically large enough to be filtered out, you can get some inference that way, but if it's not physically large enough, distinguishing whether it's dissolved or colloidal, this diagnostic is not applicable for that.

DR. BANERJEE: Yeah, I'm trying to understand this deposit that you form there. So you're circulating the stuff around, and when you draw it out, it looks clear. But there is some amount of this which is obviously coming out over a period of time on the five quals, right?

MR. LETELLIER: Yes. Jack, could you remind me at what point the flow meter was fouled by deposits in Test 3?

MR. DALLMAN: Day eight.

MR. LETELLIER: Day No. 8?

MR. DALLMAN: Correct.

MR. LETELLIER: So the large inventory of precipitates was formed within 30 minutes, very short term, and the tank clarified within a few days. So it's very reasonable to assume that those deposits occurred relatively early in the test.

MR. TREGONING: And just go be clear, "clear" is a relative term. It didn't look -- most of the time the water at a minimum was discolored, quite often either yellowish in appearance or rosy in appearance, depending on the exact composition of the initial make-up of the tank.

So I mean, there was discoloration observed in all of these tests, at least in the water sample. So, yes, it's clear in a turbidity sense, but you know, there's certainly evidence that there are additional constituents within this water than would be apparent in either --

DR. BANERJEE: But it could be in solution.

MR. LETELLIER: It could be in solution, certainly.

MEMBER SHACK: Right, and one of the other things he's not showing is phosphate levels here. But, in fact, his phosphate levels have dropped substantially through the tests. So basically he's got enough calcium here to take all of the phosphate out, and then the CalSil still keeps dissolving, and there's no more phosphate to take it out. So it builds up.

So it's all, you know, a consistent picture.

MR. LETELLIER: Day three in Test No. 3 is when the phosphate became depleted at least by Test

3. It's interesting that you see a little dip there in the calcium concentration about day one, and then it elevates pretty quickly by Test 3 and then the trend is more gradual, but still accumulating.

That initial trending may be related to this phosphorus depletion.

CHAIRMAN WALLIS: Tell us what CalSil is and what is its chemical constituents? We always call it CalSil, but in what form is the calcium? Is it a silicate or a carbon? It's not a carbon?

MR. VIJAY JAIN: It comes from silicate.

CHAIRMAN WALLIS: Is the calcium aluminum silicates, no aluminum silicates? Isn't it made from diatomaceous earth or something?

MR. LETELLIER: Indeed.

CHAIRMAN WALLIS: So that has all sorts of stuff in it.

MR. LETELLIER: Yes.

MR. CARUSO: But depending on the source it could potentially, yes.

CHAIRMAN WALLIS: But it's mostly calcium silicate. Is that what it is?

MR. VIJAY JAIN: This is Vijay Jain from the Center for Nuclear Waste.

The majority of it is calcium silicate,

but it has about ten percent sodium silicate which is used as a binder to put it together, and it has aluminum and some other impurities that go along with it, including potassium, magnesium, and so on.

CHAIRMAN WALLIS: It probably has aluminum silicate, doesn't it? These things have that.

MR. VIJAY JAIN: Compared to calcium, the aluminum is very, very small fraction.

CHAIRMAN WALLIS: Very small, okay.

MR. LETELLIER: The diatomaceous earth is a biological product, and you wouldn't necessarily expect the aluminum silicates to be concentrated in that manner.

CHAIRMAN WALLIS: All right.

MR. LETELLIER: But we talked yesterday about the diversity of the insulation product. In some respects CalSil is much like concrete. It depends on where you take the raw materials and how you thermally combine it, the history.

CHAIRMAN WALLIS: So that's something that has to be taken into account then. That's why everything is plant specific, is it?

MR. TREGONING: That's another factor for consideration. How important it is, I think, remains specifically on what portion of the problem you're really looking at.

MR. CARUSO: Do any of the chemical species you saw have reverse solubilities?

MR. LETELLIER: Indeed, they do. The calcium is one.

MR. CARUSO: So what happens to all of that calcium when it gets in the core?

MR. LETELLIER: It may precipitate depending on where its point of origin, if you will, in terms of dissolution occurs. If it's principally dissolved in the containment pool, eventually it will experience both higher and lower temperature regimes in the RCS.

CHAIRMAN WALLIS: Well, it goes through the RHR heat exchanger.

MR. TREGONING: Right.

CHAIRMAN WALLIS: Isn't that the place where it gets cold?

MR. LETELLIER: Yes. And it also goes through the core.

CHAIRMAN WALLIS: Well, it doesn't get so cold in the core, does it?

MR. LETELLIER: I imagine it would experience both extremes compared to the pool. That's right.

DR. BANERJEE: but all of your deposition you'd think occurred in the early part of this.

MR. LETELLIER: For Test No. 3 specifically that --

DR. BANERJEE: Yes, specifically.

MR. LETELLIER: -- that's right.

DR. BANERJEE: And the other stuff was slowly growing over a period of time or what? You never measured. You didn't have a coupon or something that you were measuring deposition with over time.

MR. LETELLIER: Except for our sacrificial fiberglass samples which were pulled out periodically, that's basically our only evidence of surface phenomena. Post test we were able to break open the lines and inspect the surfaces.

CHAIRMAN WALLIS: You are about ready to wind up, I think.

MR. LETELLIER: I think we are. Some of the most coherent explanations are associated with the silica and the aluminum trends in Test No. 1 and also the calcium and phosphate interactions for Test No. 3.

If we jump to the very end of the package, page number 35, I'll just remind you that the ICET program has been very prolific as far as generating information and documenting sharing this evidence. So there's a library of test reports for your reading pleasure.

DR. BANERJEE: Is it on the Web site?

MR. LETELLIER: Yes.

MR. CARUSO: You have all of this stuff that's on the CD.

DR. BANERJEE: It's on the Web site, too.

MR. TREGONING: Theoretically, yes.

CHAIRMAN WALLIS: But the striking thing, as I say, back the beginning when you read your summary in the report, you sort of indicate we found all of sorts of interesting stuff, but each plant is different. Therefore, everything has to be done on a plant specific basis, and we don't have any sort of general conclusions or models or something which you can take and apply right away.

MR. LETELLIER: But by its nature the ICET test was a very sparse sampling of the parameter space.

CHAIRMAN WALLIS: Sure.

MR. LETELLIER: And yesterday I forget who presented the matching of how many plants most closely associate with each test. I think that the ICET series represents a very good starting point. We now have benchmarks on which to stand and look for the more subtle variations between plants, if you will.

CHAIRMAN WALLIS: We have a starting point, and the question is when do we get to the finish line or how do we get to the finish line.d

DR. BANERJEE: Marathon.

MR. LETELLIER: Many of the questions associated with the resolution involve rates, rates of production, rates of accumulation. ICET test was not specifically instrumented to study that level of detail.

There are proposals and a lot of discussion about what we would do next if given the opportunity, but right now we're very busy digesting the information we have.

DR. BANERJEE: Did you measure the total amount of precipitates and reaction? You talked a lot about this clear liquid, but there was the reactions produced, precipitate that stayed in the tank, and so on. So what happened there?

MR. LETELLIER: You're basically asking whether we were able to do a mass balance on the whole system, and --

DR. BANERJEE: And compositional balance.

MR. LETELLIER: I cannot claim that we were successful in compositional balance, and even the mass balance was rather crude because of the difficulties of reclaiming the material from the tank and the fact that we have evaporation and water replacement during the course of the test.

It's physically dispersed. While we might start with a handful of crushed concrete at the beginning, it now appears as residue on every single surface. So it's quite problematic to look at that.

In the global perspective, tests like number three, as Dr. Shack mentioned, we can look at the stoichiometry of available calcium versus available phosphate, and we can argue that that reaction went to completion essentially, and because we know the concentrations of phosphate in the chemical buffering system, we can speculate on the quantities, actually reasonably good estimates of total quantity that would be formed over time.

Again, it's the rate. If it's all produced in 30 minutes or if it takes substantially longer in the plant environment.

DR. BANERJEE: So let me ask this. Suppose you took what was there after the event and you mixed it all up, and now you did a chemical analysis to find out what the species were and how much there was in a sample of this awful mixed goo.

Did you do something like that or this is obviously a very unrealistic experiment, but nonetheless is there a way to say what happened looking at the residues?

MR. LETELLIER: I'm afraid that the physical heterogeneity of this goo, this residue would preclude that effort. We did recover sediment samples, and under SEM analysis you see granules of sand. You see broken fibers. You see chemical products. It's very diverse.

In addition to that, on a larger physical scale, you see shreds of original insulation material an even larger than that, you see the metal plates.

The question of what sample size would you choose to homogenize is very difficult.

DR. BANERJEE: So the clear samples provide two types of -- maybe more -- types of information. One is it indicates what reactions have occurred potentially and what have gone to completion or not. It gives you some indicator of that.

Second, it tells you, I suppose what materials might be transported past the filters and so on because it's basically clear liquid or maybe turbid, but very fine. So it has implications on downstream deposition.

MR. LETELLIER: From a chemical

perspective, yes, that's right.

DR. BANERJEE: From a chemical perspective, right?

MR. LETELLIER: Un-huh.

DR. BANERJEE: But the third thing, I suppose, is that it might clarify the deposition that you're getting on the walls. I don't see that connection having been made.

CHAIRMAN WALLIS: Did you scrape this stuff off the walls and then take it away and analyze it?

MR. LETELLIER: We did, but you need to remember that we recovered those samples after the tank was drained and substantially cooler. So whether or not that's present in situ --

CHAIRMAN WALLIS: Well, I think in the pipes it probably was present because it's unlikely to have that much liquid in the pipe to throw out the stuff on the wall after it has cooled.

MR. LETELLIER: For Test No. 3, that's obviously true. For the other tests, we're talking about white residue.

CHAIRMAN WALLIS: Very small.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: You said eighth of an

inch. I think it isn't an eighth of an inch, is it? It's more like --

MR. TREGONING: Maybe thumbnail, smaller than that.

DR. BANERJEE: Thirty-second of an inch, but still --

MR. LETELLIER: Again, that may be too quick of a judgment based on the thickness of those veins and the flow meter, which they're actually a flow perturbation which tends to have an impaction perhaps larger than the walls and accumulate much thicker than the walls.

DR. BANERJEE: There's a lot of information left over in the non-supernatant liquid, right? So what did you learn from that? What stuff is left in the tanks?

MR. LETELLIER: Well, again, recovery of those samples was complicated by the location and the mixture. In cases like Test No. 3 where there was simply such a large inventory of chemical product, we were reasonably successful at recovering a sample which we thought represented the more pure form, and so we were able to put more credibility in the elemental proportions of that sample.

In Test No. 1, which showed indications of precipitate upon cooling, it was more difficult to isolate that material in the sediment of the tank. Where we discovered large volumes is in the inventory of the bulk water in the storage containers. After it was cooled, it suddenly precipitated and formed 15 or 20 gallons of semi-solid sludge.

That material was also examined for its chemical composition.

CHAIRMAN WALLIS: Well, really what the big messages are, that with NaOH and no CalSil, you get a fair amount of aluminum dissolved, and if you put in CalSil and this TSP, then you get calcium phosphate in fairly large quantities. Those are the two big messages, aren't they?

MR. LETELLIER: Yes. And the other tests actually substantiate those observations in a more subtle way.

CHAIRMAN WALLIS: Are we ready to take a break do you think?

You've done very well. Thank you. A very nice, clear presentation, very responsive to questions, and we're going to see you again today or, no, we don't?

MR. LETELLIER: Tomorrow.

CHAIRMAN WALLIS: Tomorrow, tomorrow.

Gee, whiz. Twenty-five after can we get back? Do you remember that we're going to be back here at 25 past 11? We'll take a break.

(Whereupon, the foregoing matter went off the record at 11:11 a.m. and went back on the record at 11:25 a.m.)

CHAIRMAN WALLIS: Please come back into session where we will hear the next presentation about chemical speciation prediction. And maybe we'll make it to lunch at a reasonable time.

Please go ahead.

DR. BANERJEE: You're in an advantageous position right now.

MR. B.P. JAIN: We're here to talk about the chemical speciation predictions. To my right is Vijay Jain. He's from the Center for Nuclear Waste Regulatory Analysis, part of Southwest Research Institute, and they have done a lot of work in this area.

Next one, please.

And this presentation will cover the objective of this program, the motivations, a technical approach, some of the preliminary findings, and any other useful information which can be used for ongoing work.

Next one.

This program was initiated, the ICET program, with the objectives of clearly to have a sort of analytical tool whereby we can predict chemical byproducts in any given sump environment.

In doing that, obviously it also included a survey of all readily available commercial programs, computer codes, and then also ask the center to recommend a suitable code where we could use prediction of these chemical byproducts.

CHAIRMAN WALLIS: These are equilibrium predictions or are they kinetic?

MR. B.P. JAIN: They are mostly equilibrium predictions.

Like any other code, we were also interested in knowing what the limitations of these codes were and what the code can and cannot do. So those were the primary objectives of this study.

Next one, please.

And the program right away, obviously we tested only five environments in the ICET program, and the results as presented today, they showed that a change in insulation or buffering agent, temperature can greatly affect the type of chemical product or the concentration you could get.

So, again, the motivation was to be able to predict, if we could, what these potential products were in a given environment. The bottom line, as we go over the details, but the bottom line is that computer codes can predict better if those models are properly benchmarked and calculated with the test results from the data observed in the tests. That's basically the bottom line.

Vijay will go over some of the program directions and technical approaches.

MR. VIJAY JAIN: Good morning. Thanks, B.P.

We adopted a phased approach in looking into the speciation modeling. We started our work by looking at the thermodynamic models using the inputs that were based on the values that we obtained from the literature, especially for corrosion, and we used the exposed surface area in containment water composition from the ICET test plan.

Again, the literature was very sparse in the actual corrosion data for the specific containment environment, specifically looking at 2,800 ppm boron. So we started with a review of what's in the literature, and then we followed the pre-test, pre-ICET modeling that was based upon the input values that were tabulated based upon the experimental results. We conducted independent standardized tests to get the corrosion rates for metals, for insulation, and concrete.

We also did some experiments, a standardized test using aluminum and Nukon because some of the literature indicates that aluminum has a strong suppressive effect on Nukon release rates.

So we combined those experimental data, developed a new set of input values, and did our simulations, and finally, we benchmarked the simulation based upon the observations we saw from that ICET test.

So in this particular representation I'll focus on the pre-ICET and the post ICET thermodynamic simulations. There are lots of commercial codes in the market. Some of them are listed here, such as EQ3/6, geochemistry workbench, PHREEQC. These three codes are typically used in geochemical, geochemistry industry where they look at the weathering phenomena, rock interactions with streams and so on.

The codes like Stream Analyzer and environmental simulation programs, they are developed by OLI systems and they are more geared toward the chemical process industry.

In this particular analysis, we adopted to use the Stream Analyzer for our analysis because the reality is it's more of a chemical process that the water, steam comes in, interacts with different components, and produces the byproducts.

Furthermore, Stream Analyzer is a very powerful simulation tool. It covers a wide range of pressure and pressure conditions. It can handle concentrated solutions, and its thermodynamic database is 250 solid species, along with many of the organic species we need to model the organic phases.

Well, there's some assumptions that went into doing this type of modeling. As you know, the thermodynamic simulations are basically equilibrium simulations. We assume all reactions achieve equilibrium extently (phonetic). We exclude any consideration of reaction kinetics, but in our case, some of the reaction kinetics was partly included by the user of experimental corrosion rates. So at different times we used the corrosion rates to calculate the amount of byproducts that were generated, and we used those as input values for simulations.

Again, the reactive materials as far as those limited to ICET simulations or ICET tests and excluded paints and organics, and we also excluded uptake of (unintelligible) CO_2 in this particular set of slides that I'm going to show, but we do have a future plan to look at CO_2 uptake into the solution.

Well, the way we do this simulation is we select the containment water composition which has 2,800 ppm boric acid concentration or boron concentration. We select a buffering agent, and then we calculate the corrosion amount as a function of time based upon corrosion rate of different components.

I have about ten backup slides that provide the pictures of the samples we use for testing. It also provides the corrosion rate measurements of different debris, metals, insulation. Basically we used corrosion rates for zinc, copper, aluminum, and carbon steel.

Depending upon what type of simulation we were doing, we either chose Nukon or we used a mixture of Nukon and calcium silicate, and we included concrete.

DR. BANERJEE: These corrosion rates that you estimated, were they corrosion rates change with velocities and all of these types of things? How did you choose these rates to use?

MR. VIJAY JAIN: We didn't really include any effect of velocity. These corrosion rates were using electrochemical techniques, linear polarization, and potential dynamic polarization methods, and therefore fairly conservative rates.

DR. BANERJEE: They'll be high enough or would they be too low if you expose. For example, you've got this jet sitting places, right, and dissolving stuff, that sort of thing happening. Now, how do you take that into account here? You've got water, steam water jet sitting in various components, right?

MR. VIJAY JAIN: One of the ways that is accounted for is exposed surface area. Corrosion rates are milligrams per meter squared per power R (phonetic). So if the impingement of the jet produces debris of a certain specific area, you include that specific area and on your specific area you will have -- the corroded amount will be higher.

DR. BANERJEE: So there is no flow induced effect, corrosion or aluminum?

MR. TREGONING: There was no account of any erosion of metallic or nonmetallic components due to the impingement of the LOCA jet.

DR. BANERJEE: No, that came off as

erosion. They produced fines, right?

MR. TREGONING: Yeah, that wasn't considered. I mean, that certain is source term, although you have to remember that's a relatively localized event compared to the rest of the containment materials which are either exposed to either submerged within the containment pool or located in regions which are affected by the containment spray.

So we didn't specifically look at the erosion of, again, anything which might be in the way of the jet.

MEMBER KRESS: Well, I suspect what you have is liquid flowing by the surfaces , and the corrosion rate is the function of mass transfer in the liquid phase plus some sort of reaction going on near the surface with the competing movement of materials, corrosion products.

So what I think I hear you saying it's the chemical corrosion rate itself that controls this and not the mass transfer in the water so that you have some sort of a way of having measured these corrosion rates where the flow of the water just didn't matter.

DR. BANERJEE: If the assumption is that it is reaction controlled --

MEMBER KRESS: Reaction controlled. That's what I'm interpreting.

DR. BANERJEE: Whether it's true or not is another issue.

MEMBER KRESS: That's true, yeah.

MEMBER DENNING: Well, there are two different things. I mean, right now we're just looking at ICET and the ability to predict ICET. The second question is given that you have validated this kind of methodology, how do you apply it to the real system?

MR. VIJAY JAIN: The corrosion for Nukon and calcium silicate, we opted to select the forward reaction rate, and these forward reaction rates are way conservative, as you know. As a function of time, the insulation material becomes passive and formation of second (unintelligible) phases, the rate tends to decrease with time.

For modeling purposes we assume the forward reaction rate which is the initial portion of the corrosion rate measurements for insulation materials and also for concrete.

So the next few slides provide the assimilation results using measured corrosion rate.

This slide shows the amount of predicted solid phases at 60 degrees, at 90 degrees, and at 110 degrees. Sixty degrees and 90 degrees, the simulations were conducted at one atmospheric pressure and 110 degree Centigrade, it was conducted at three atmospheric pressure.

What you see here is the solid phases are dominated by the solid circles which represents sodium aluminum silicate and solid triangles which represents calcium silicate, calcium and magnesium silicate. These constituents, almost 99 percent of the solid phases that are formed in the system containing and a simulation that contains insulation, all the metals, and concrete.

DR. BANERJEE: Is there a time associated with this?

MR. VIJAY JAIN: These were all done for half an hour.

I show here the conclusion from this were greater amounts of various silicates were predicted to form with increasing temperature action here, but not significantly, not even a factor of two. Calpression (phonetic) indicate that 99 percent of the solid phases predicted in pressurized system would be similar to the phases predicted in nonpressurized system at lower temperature.

Finally, the corrosion products from insulation and aluminum are the major contributor to the secondary solid phases. The contribution from zinc and iron were very limited, and it doesn't even account for one percent of the solid phases that were formed in the system.

DR. BANERJEE: So what is corroding here?

MR. VIJAY JAIN: In this case you are corroding Nukon fiber, and you are corroding significant amount of aluminum, which is forming sodium aluminum silicate. Calcium magnesium comes from both fiber and some from concrete.

DR. BANERJEE: So copper is coming from where?

MR. VIJAY JAIN: Copper is one of the inputs coming from the heat exchanger pipes and some wiring, I presume.

MR. TREGONING: I think cooler fan blades is the predominant contributor. Maybe John Gisglom (phonetic) would want to commenter on that.

MR. GISGLOM: John Gisglom, EPRI.

It's the basic material --

PARTICIPANT: John, I think you have to get up to the microphone.

MR. GISGLOM: The copper that we accounted for in ICET was the copper mainly in the heat exchanger tubes and fins for the containment fan cooler units.

DR. BANERJEE: Thank you.

And the calcium magnesium silicate?

MR. VIJAY JAIN: Calcium could come either from concrete or come from Nukon fiber.

CHAIRMAN WALLIS: Excuse me. This cooper, the fan cooler units are not submerged in the sump. So why do they come into this? I mean, maybe there's an atmosphere of steam up there, but that's not the same as having them in the sump itself, is it?

MR. TREGONING: John can elaborate, I think.

MR. GISGLOM: They would be exposed to containment spray for the initial period, and there are also a very minor amount of copper that would be in the sump itself, and that's reflected in the ICET series where there was significantly more copper in the nonsubmerged region than in submerged region. There's a small heat exchanger near the bottom of the sump, and that's what's reflected.

PARTICIPANT: Okay. Thank you.

DR. BANERJEE: I have a test plan here that talks about 25 percent submerged copper and 75 percent nonsubmerged and instrument air lines were also part of that copper.

So to go back, the calcium magnesium silicate is the concrete and what else?

MR. VIJAY JAIN: And insulation, Nukon insulation.

DR. BANERJEE: Nukon, and just the main other constituent that dissolved was the sodium aluminum that dissolved was the sodium aluminum silicate, and where did that come from?

MR. VIJAY JAIN: Aluminum comes from aluminum, metal scaffolding present in the containment building, and silica comes basically from Nukon fiber dissolution, and they combined to form sodium aluminum silicates.

The next slide shows the predicted amount of solid phases as a function of time at 60 degrees Centigrade, and what you see here is, again, you see the same results. You see the solid phases are dominated by sodium aluminum silicate and calcium aluminum silicate, but at longer time to start seeing participation of silica because silica tends to exceed its solubility limit, and also some formation of calcium silicate.

Again, these are originating from the

byproducts of Nukon insulation, concrete, and aluminum.

DR. BANERJEE: So what temperature this is?

MR. VIJAY JAIN: These are at 60 degrees Centigrade in a pH 10 environment, which will flex the ICET Test No. 1.

DR. BANERJEE: So the first set of results are essentially the 60 degrees in your previous slide.

MR. VIJAY JAIN: Our previous slide was --

DR. BANERJEE: Half an hour you said.

MR. VIJAY JAIN: -- half an hour for 60 degrees, 90 degrees, and 110 degrees.

DR. BANERJEE: All right, but if you take that 60 degree vertical set of data there, that is your first set on the left.

MR. VIJAY JAIN: Yeah, sure.

DR. BANERJEE: And this is what is coming together.

MR. VIJAY JAIN: Coming together, yes.

MR. TREGONING: I'm going to suggest just for point of clarification all of these first sets of results are pre-ICET simulations. So they don't include benchmarking with respect to the observations that we're seeing in the ICET test. So I'd suggest that we move relatively quickly through these and get to the ones that we feel are more pertinent with respect to the ICET test.

Some of these silicates, while they were predicted initially in the speciation modeling, were not observes in the ICET tank. So no point in necessarily fixating on them.

CHAIRMAN WALLIS: Most of these lines have a slope of one?

MR. VIJAY JAIN: I'm not too sure if --

CHAIRMAN WALLIS: I think it looks like it anyway.

MR. VIJAY JAIN: These are on the log scales of --

CHAIRMAN WALLIS: Yeah, but if you do, it looks like a slope of one if you take the log scale. So it's a uniform rate of dissolution.

MR. VIJAY JAIN: For metals we assume, yes, it is because the user forward the action rate for insulation.

MR. TREGONING: Yeah, that's an assumption, a modeling assumption.

MR. VIJAY JAIN: So those are the results we obtained before we had ICET test results, and we went and examined ICET test results. We basically found that silicate phases were not observed in ICET environments.

Many precipitation and dissolution reactions are kineticly controlled at pressure temperature and time conditions of the ICET experiments, and some of them are very sluggish.

Silicates are the most thermodynamically, most stable phases, but kineticly they are very sluggish. Some of the silicate phases only formed the type of pressure and may take several years to form. Some of the glassly pressure (phonetic) indicates that some of the silicate phases depending upon the glass composition can form at 95 degrees Centigrade or a period of one to two months.

So in the future simulations, what we did is we suppressed the formation of silicates from the remodeling based on the observations we had from ICET test.

MEMBER KRESS: How did you do that physically with the model? Do you give it a different GIBs (phonetic) per energy or something?

MR. VIJAY JAIN: No. What we do is basically the input asks you what phases you wanted to put your input values to.

MEMBER KRESS: I see. You just said it

wasn't there.

MR. VIJAY JAIN: Yeah, basically it says if it doesn't exist. The first test we did, we didn't take off anything because we just liked to get observations that we would observe from the most stable phases.

The second thing that we observed or last observed was aluminum hydroxide phase, which is $AlOH_3$, was not observed to form in ICET environments. Rather, aluminum oxyhydroxide, which is AlOOH, was observed.

So we suppressed the formation of aluminum hydroxide while allowing the formation of aluminum oxyhydroxide phase.

MEMBER KRESS: Now, you had to do that in a different way, that suppression.

MR. VIJAY JAIN: No, the same thing. There are ten aluminum phases. I say don't equilibrate my inputs with aluminum hydroxides, but do it for the rest of the things.

MEMBER KRESS: You can just tell the code.

MR. VIJAY JAIN: Good.

DR. BANERJEE: So there is no way to dial in a kinetic --

MR. VIJAY JAIN: The only way the kinetic information is built in is through the reaction rates of the different components.

MR. TREGONING: In these codes.

DR. BANERJEE: In these codes there's no way to dial it in though.

MR. VIJAY JAIN: There is one code called EQ3 equals six where you can do some kinetic studies, but we didn't use that code to start with.

MEMBER KRESS: The Canadians have a code called FAST, F-A-S-T, the committee, but it's not in the liquid phase. It's in the gas phase.

MR. VIJAY JAIN: Well, again, when you start going into kinetic phases, you have to look at formation of flocculent, gel formation. Everything needs to be incorporated, and I presume to do all of those things will require so many exemptions that you won't have credibility of that data coming out from kinetic analysis. Extremely difficult to do kinetic modeling for these types of environments.

MR. TREGONING: Unless potentially you build your own model from --

MR. VIJAY JAIN: Even then you have to make assumptions at what rate the colloids are formed and gel formation takes place and how they grow, how they precipitate out.

As Bruce indicated, there were five ICET tests, and in this particular presentation, I'm going to focus on the results from the ICET test number one, and have provided the results to ICET test number three as the back-up slides. If we have time, we can go back and look at the back-up slides.

Basically we did simulations for all five tests in this particular study.

This slide shows the first ICET simulation result based upon ICET test conditions, which also includes the phases that we observed not to form in ICET environments. The slide shows the release of silica into the solution, silica, aluminum and calcium. These are three key elements that are formed that were observed in the ICET conditions.

The slide also shows the pH dependence as a function of time. It shows the amount of silica released as a function of time, up to 30 days, calcium and aluminum.

You'll see here for up to 15 days the prediction for calcium and aluminum are right on mark. Given the complexity of the systems and the simulation assumptions, it shows extremely good correlation for calcium and aluminum for 360 days or 15 days.

We see a high release of silica in our

simulation, and that's because we assume all of the latent debris, which is concrete order to be dissolved at time zero, which is a fairly conservative assumption, which provides additional source of silica, and you see a high amount of silica in our predicted values.

We believe that the higher predicted values for aluminum and calcium are related to the fact that as a function of time the surfaces may become passive, may become less reactive, which is, again, a kinetic form, a kinetic issue which is not incorporated in our models.

It shows a fairly good prediction for pH, and this slide basically summarizes what I just said. The model predicts high silica concentration because concrete particles were assumed to dissolve instantly.

Silica concentration was well below the saturation concentration in pH 10 of the pH 10 containment water, and that because aluminum clearly inhibits the release of silicon into the solution.

CHAIRMAN WALLIS: These are dissolved into the water? I thought the aluminum was in particulate form, some of it, colloidal.

MR. B.P. JAIN: It could be. Some of it could be.

MR. VIJAY JAIN: Could be colloidal, but it still --

CHAIRMAN WALLIS: Could be colloidal.

DR. BANERJEE: It could be still present in the solution. Again, that test assumption we are assuming that it could be colloidal. It has not been proven it's colloidal. It could be just an ionic form.

MR. CARUSO: Were all of the metal samples pure metal or were they alloys?

MR. VIJAY JAIN: Well, I think I received these samples from the ICET test. So whatever they used, I think copper was cooper metal. Aluminum had -- I think John can answer that question better than me.

MR. TREGONING: It depended. I mean, there were steel samples. There were copper samples. I think Bruce is right, industrial metals. So even the copper and aluminum samples had some impurities in them. At least the copper and aluminum were nominally pure.

John, do you want to elaborate?

MR. GISGLOM: They were industrial metals. The copper was copper. It wasn't 90-10 or something like that. It was --

MR. CARUSO: But I mean, aluminum, you have silica aluminum. I mean you go buy aluminum alloys to use, and it's usually some silica aluminum alloys. Does that have any effect?

MR. TREGONING: These weren't aluminum alloys, right? I mean, this was, again, industrial grade, pure --

MR. GISGLOM: It was basically industrial grade aluminum. It wasn't --

MR. TREGONING: There's impurities certainly.

MR. GISGLOM: There certainly are some minor amounts of impurities in the metal, but it wasn't pure aluminum. It was aluminum, basically industrial grade alloy.

MR. VIJAY JAIN: Continuing, the model predicts high concentration for aluminum and calcium at 20 days or 720 hours, and we believe it is attributed to the reduced reactivity of the surfaces with time. It could be for formation of passive foam or secondary phases that form on the surface that are released into the solution.

The model also predicts formation of solid phases. We see formation of ferrous hydroxide at 148 hours, and we see the formation of zinc hydroxide at

32 hours. Again, these are fairly small quantities that were observed in the simulations.

MEMBER DENNING: And is there any way to compare those against solid phases suspended in the tests?

MR. VIJAY JAIN: I presume these phases may be mostly adhering to the metal surfaces, and I haven't seen any information from the ICET test that indicates some. There were some coating on the metal surfaces in ICET tests. I'm not too sure it has been characterized yet or not.

MR. TREGONING: If there's coatings, and again, at the risk of overstepping, Bruce might correct me here, but I believe in some cases in the sediment you could identify or isolate particulate that may have been either zinc or iron in nature, but again, it was within the sediment which we've already discussed was a very heterogeneous mix.

Certainly if we observed it, it was a relatively or very small percentage of the sediment. So, Bruce, if you want to elaborate.

MR. LETELLIER: We did attempt to examine the surface products that were formed, but we didn't have easy access to a shallow angle EDS equipment that would be needed to separate the substrate from the surface deposits.

So while we could visually examine a surface for what appeared to be clean metal versus corroded metal, it was difficult to separate them for quantitative assessment.

I do agree with Vijay that a lot of those forms are deposited on the surface rather than suspended or precipitated out of solution.

MEMBER KRESS: On your previous slide, you speculate that the silicon prediction was higher because you let the concrete particles dissolve instantly. Why wouldn't that give you the same effect on the calcium?

MR. VIJAY JAIN: Well, it should, but again, the amount of silica that you're seeing is ten ppm compared to calcium that increases on five ppm for silica and about ten to 20 ppm for calcium.

MEMBER KRESS: Not a lot.

MR. VIJAY JAIN: Maybe some of that effect might have been lost in the increased concentration of calcium because calcium comes from other sources also.

MEMBER KRESS: Yes. That may be it.

MR. VIJAY JAIN: This provides a summary of our simulation results. For ICET No. 1, we saw a good correlation with major elements in solution up to ten in 60 hours. Simulation predicts high concentration in solution at 722 hours, as I indicated, could be attributed to the passivation of the surfaces.

ICET No. 2, we saw a good correlation with major element except calcium, up to 360 hours. Simulation predicts calcium to be precipitated as phosphate similar to the one that Bruce talked about for ICET Test 3, but in ICET Test 2 where there was a limited observation of calcium phosphate on the fibers.

In Test No. 3, we saw a good correlation with major elements. Again, we didn't accept calcium, up to 360 hours. Assimilation predicts high concentration of calcium in solution after 96 hours, and it was significantly higher compared to what was observed in ICET Test No. 3.

For Test No. 4, prediction did not correlate with ICET results because the simulation inputs were based on separate corrosion experiments for CalSil, insulation, and aluminum. As Bruce indicated, there was a strong synergetic effect between CalSil and aluminum. In our standardized test that we did in the lab, we did a combination of Nukon and aluminum together, but we did not do it for CalSil and aluminum.

So partly our results don't predict ICET No. 4 environment is because we have assumed very high rates for CalSil and aluminum without any synergetic effects.

A prediction for ICET Test No. 4 did not correlate for ICET results because simulation inputs were based upon either corrosion measurement at pH seven or ten, and we had no corrosion measurements done independently for pH of 8.2.

And we know that at pH of ten, least from Nukon and suppressed aluminum, but pH seven there is no aluminum released. It's almost passive, but Nukon releases at a very fast rate.

DR. BANERJEE: Let me understand these a little bit. These are the results of your simulations done with the code using some corrosion rate.

MR. VIJAY JAIN: Measured corrosion.

DR. BANERJEE: Measured corrosion rates, and those were measured in your lab.

MR. VIJAY JAIN: Yes.

DR. BANERJEE: Okay, and how were those corrosion rates? They were just coupons exposed?

MR. VIJAY JAIN: Yeah, they basically used -- these are slides. The first backup slide shows the size of equipment we used.

DR. BANERJEE: Okay

MR. VIJAY JAIN: And for Nukon we used the same ratio of volume to mass that was used in ICET. We preserved the surface area to what's in issue.

DR. BANERJEE: You're trying to bridge your coupon experiments to the ICET experiments using your code as a bridging tool somehow. I mean, one can look at the small scale experiments that were done, and you're trying to say something about the large scale.

MR. VIJAY JAIN: Yeah, those were used to get the corrosion rates that were input to the simulations, and the output for the simulations were then compared with the ICET results.

MEMBER KRESS: How do you incorporate those corrosion rates? Do you take the corrosion rate and predict at a given time how much would have been --

MR. VIJAY JAIN: Exactly.

MEMBER KRESS: And then you input that as an equilibrium amount at that time.

MR. VIJAY JAIN: Exactly.

MEMBER KRESS: Okay.

MR. VIJAY JAIN: There is a slide which I would like to show you, a backup slide, Slide No. -- here. Let me see which slide it is -- 29. This slide basically provides --

MR. B.P. JAIN: Twenty-nine?

MR. VIJAY JAIN: Yeah, 29 shows how well our lab results compare with the ICET results, which provides confidence. No, this is not the one.

MR. B.P. JAIN: That's 29.

MR. VIJAY JAIN: That's the one, Slide No. 26. Sorry about that.

This slide shows the key element, silica aluminum and calcium, what we saw in our lab tests and the amounts of these elements observed in ICET tests. These lab experiments correlate, given the complexity of the systems, given that ICET had so many other things. These experiments show how well the little lab experiments correlate with the observed behavior in ICET for silica, aluminum, and calcium.

So this provided me confidence that the numbers that I'm using which drive corrosion rates are very well represented of what was observed in ICET tests.

DR. BANERJEE: Now, the difference between Slide 26 and 14 is due to what? These on 26 are just your coupons. It doesn't have any --

MR. VIJAY JAIN: Simulation results. It doesn't have any simulation results.

DR. BANERJEE: No simulation results.

MR. VIJAY JAIN: Slide 14 has ICET results and simulation results.

DR. BANERJEE: But those simulation results in 14 were not informed with your corrosion rate data from your lab? I'm trying to understand the difference between --

MR. VIJAY JAIN: There are three sets of data. It has the observed corrosion behavior in the lab test, predicted simulation results, and ICET results, and all three indicate a very good correlation with each other.

CHAIRMAN WALLIS: Well, 14 is comparison with theory, I understand. Twenty-six is comparison between two experiments, isn't it?

MR. VIJAY JAIN: Well, one experiment is the lab.

CHAIRMAN WALLIS: Yeah, one is the Uricks (phonetic) test and the other is the ICET test.

MR. VIJAY JAIN: Yeah, and my lab experiments only had Nukon and aluminum. It didn't have anything else.

CHAIRMAN WALLIS: So 26 is experiment

versus experiment, and 14 is theory versus experiment.

DR. BANERJEE: But was 14 informed with corrosion rate data or not?

MR. VIJAY JAIN: Yeah, the corrosion rate data was obtained, what you see in slide 26, based on that, and translated as an input value for simulations to predict what are the solid phases that will form.

So the lab experiments cannot give you a prediction of the solid phases of byproducts that are going to form. You have to simulate and indicate what are the solid phases you're going to see as a function of time.

MEMBER KRESS: You could interpret the results on 26 as being the change in the mass of your coupons.

MR. VIJAY JAIN: Yeah.

MEMBER KRESS: So, you know, you say that's all in there somewhere. You don't know what form it's in.

MR. VIJAY JAIN: But, for example, aluminum. The amount of aluminum we measured in the solution correlates very well with the amount of weight loss, indicating there is no aluminum precipitation at 60 degrees Centigrade, and that's why we say that there's no --

MEMBER KRESS: But that may be a difference between the two slides.

DR. BANERJEE: So if you take, say, the aluminum in Slide 26, which is a measurement --

MR. VIJAY JAIN: Yeah.

DR. BANERJEE: -- and you take the aluminum in Slide 14, which is a simulation, but to drive that simulation you took the corrosion rate data from Slide 26, right? That's what I'm confused about.

When you say that you obtain --

MR. VIJAY JAIN: But there are two things. We did two measurements. We measured all the metals using electrochemical metals. We measured the solution chemistry with Nukon insulation and aluminum together. The corrosion rates that were used for simulation were based upon electrochemical metals. So it was not based upon --

DR. BANERJEE: On these coupon tests.

MR. VIJAY JAIN: -- the combination of insulation and aluminum together, but the two correlate very well. If I plot the aluminum released into the solution and based upon the corrosion rate they predict almost similar --

DR. BANERJEE: So if I understand it, you have two sets of corrosion rate data. One is the data you're showing on Slide 26.

MR. VIJAY JAIN: Yes.

DR. BANERJEE: Another set goes into the calculations you show on Slide 14.

MR. VIJAY JAIN: That's true.

DR. BANERJEE: Where is that corrosion rate data that goes into Slide 14?

MR. VIJAY JAIN: That's shown on Slide -- it's in the backup slide -- Slide No. 24, which should be easy to get from here. So you see here these are electrochemical tests. It gives you the corrosion rate of aluminum at 60 degrees, at 90 degrees, and at 110 degrees.

So basically you take this corrosion rate, which is given in grams per meter square per hour and just put the exposed surface area and time you're going to simulate and give you total amount that you're playing with in that simulation.

MEMBER KRESS: Now, this I understand is the initial slope in --

MR. VIJAY JAIN: For electrochemistry there's only one. What you do is basically you do a linear localization where you put a small voltage across the sample and see the current density, and from current density you use the failures law to get the corrosion rate.

MR. TREGONING: But, again, you apply the constant corrosion rate, and there's a difference as well, within the simulations, and if you look at the experimental data, corrosion is certainly evolving as a function of time.

MR. VIJAY JAIN: Yeah, and again, these simulations don't take into account any passivity that happens as a function of time, which is, I presume, a kinetic issue which needs to be incorporated.

MEMBER KRESS: I guess what Sanjoy, I think, was getting at -- at least I would have got it -- was you're not using sort of data to predict the theory which is then used to predict the data. It looks like that.

MR. TREGONING: It's not circular like that.

CHAIRMAN WALLIS: No, it's not quite as circular. It's an independent measurement of corrosion rates.

MR. VIJAY JAIN: Right. Independent and initial measurement.

CHAIRMAN WALLIS: Right, but it's a different kind of measurement, which is useful.

MR. VIJAY JAIN: These are all

standardized tests for metals and for Nukon fiber we have to tweak it a little bit, but it's a standardized test used for glasses which I adopted for fibers.

CHAIRMAN WALLIS: So now you can predict what happens in a sump.

(Laughter.)

DR. BANERJEE: With a little coupon, right?

MEMBER DENNING: Now, one thing I don't understand though is, of course, what you've done is you've focused on what's in solution, and I realize one has to start there, but as far as us understanding what's happening in the sump, we have to know what comes out of solution.

MR. VIJAY JAIN: And that's what was shown on Slide 15, where I showed that the model solution predicts hydroxides of iron and zinc, and if you look at the call for aluminum, you see a linear change indicating that 100 percent is in solution till 15 days. So there was no solid precipitation occurring for aluminum in the solution.

So I have to get back to Slide 15, I guess, where I was.

MEMBER DENNING: But we don't have much in the way of validation of that. I mean, that's really the hard part of the problem.

MR. VIJAY JAIN: You mean the validation of the solid phases?

MEMBER DENNING: The validation of solid phases, right, and part of it is it's not part of this equilibrium process.

MR. VIJAY JAIN: Well, I guess I think --

MEMBER DENNING: I mean, it is someplace. You know, some place it's a solid, but we don't know whether it's suspended or whether it's on the surface.

MR. VIJAY JAIN: That's true.

DR. BANERJEE: But in your coupon experiments, you could look at -- I mean, you could do those little coupon experiments and see what the solid phases were, too, right?

MR. VIJAY JAIN: Yeah, I could have done it, hindsight, yes, but of course, I did it when we did it.

MEMBER DENNING: All right, but you could have done it at some point.

MR. VIJAY JAIN: Yes, it could have given at least some insights to it,b ut that was done much before we had ICET results.

MR. TREGONING: I think the important point to make is that, you know, the best simulations were ones that were informed in an educated way with respect to experimental observations that were made in the ICET tank, and that's a very important point.

MEMBER DENNING: In fact, it was necessary if you were going to use equilibrium thermodynamics.

MR. TREGONING: That was a necessary step. That's certainly true.

DR. BANERJEE: So you couldn't have taken your coupon tests and found out the same thing.

MR. TREGONING: Well, the pre-ICET simulations was the closest to doing exactly that, and you saw what the results were.

DR. BANERJEE: Yeah, but they were using electrochemical data.

MR. TREGONING: It wouldn't have mattered. I mean, what you saw there was the species that were predicted. You might have gotten different amounts with different corrosion rates, but those species would have -- and, Vijay, correct me -- at least still dominated.

MR. VIJAY JAIN: Because silicates are the most stable phases. Any time you run a thermodynamic code, if you have silica aluminum and sodium and calcium, it will predict silicate --

DR. BANERJEE: All I'm saying is that if

you wanted to study kinetics, you could study the kinetics on a small scale and find out the same thing, that silicates perform slowly.

MR. VIJAY JAIN: Yeah, and in fact, my background is in glass science, and I've studied --

DR. BANERJEE: You don't have to do these huge tests.

MR. VIJAY JAIN: My background is in glass science, and I've studied to corrosion of glasses for the last 20 years, and if you look at it, you do form silicates, but not in ICET conditions. It takes high temperatures and longer times to form those silicate phases of the surface of glasses.

MR. TREGONING: The value of the ICET, there have been a number and even predecessors to ICET, a number of separate effects types of tests looking at single or maybe even dual effects, but this is really unique in the sense that it was the first time that we tried to put all of these things that may have interaction and synergistic effects together, and one of the things we've noticed in ICET certainly is that in many cases the synergistic effects can be important.

DR. BANERJEE: Like which ones?

MR. TREGONING: You have passivation

issues, effect of aluminum. Even the effect of aluminum on silicate dissolution from Nukon, I mean, that was something that initially we had predictions that we expected about an order of magnitude more silicon than we actually observed in the ICET tank, and that was something we really had to go back, scratch our head a bit to try to figure it out.

Some good characterization work at LANL had indicated some of the forms are potential reasons for that, and the Vijay went off in his lab and just did some small scale experiments when he tried to look at dissolution of Nukon in the presence of aluminum.

That simple synergistic effect has a huge effect on the types of products, the amount of silicon that we saw in the test, and that's just one example.

MR. VIJAY JAIN: It shows right here in this slide two types of tests, Nukon fiberglass in the containment solution and Nukon fiberglass in aluminum, but pH seven as a function of time, you don't see any difference, any at all of aluminum in the corrosion of Nukon. In fact, the mass released from Nukon mass gets dissolved.

But at pH ten, what you see here is the Nukon glass shows a fairly high release rate, but after you all aluminum to it the dissolution of fiber goes down by an order of magnitude.

CHAIRMAN WALLIS: I'm trying to translate this into amount of silicon in the sump. You've got these strange units. I understand what they are, but how do they relate to what's happening?

MR. VIJAY JAIN: This is milligrams of Nukon fiber released by meter surface.

CHAIRMAN WALLIS: So meter squared of surface.

MR. VIJAY JAIN: Surface area of the fiber.

CHAIRMAN WALLIS: Right. How do I translate that into --

MR. VIJAY JAIN: The ICET test gives how much surface area of Nukon is there. You can multiply it and it will give you --

CHAIRMAN WALLIS: I know I can, but you can't tell me right away.

MR. TREGONING: I think Bruce is going to look at the silicon floc, but I think in ICET 1 we were on the order of ten to 15 milligrams per liter. So that's roughly --

CHAIRMAN WALLIS: Silicon in solution?

MR. TREGONING: Yeah, ten kilograms or so. And we were predicting from this on the order of 80 to

100 milligrams per liter. So again, about an order of magnitude higher.

CHAIRMAN WALLIS: So how much on the sump?

MR. TREGONING: Yeah.

CHAIRMAN WALLIS: How many kilograms in the sump?

MR. TREGONING: Somewhere between maybe ten in actuality kilograms versus 100 predicted. I think I'm -- am I close there?

MR. LETELLIER: Those levels are right.

MR. TREGONING: Okay. Thank you.

MR. VIJAY JAIN: So these are the things that we have to be very careful in assessing the specific effect in one. Some of the synergetic effects that happen especially with aluminum on insulations.

MEMBER KRESS: So now we have to throw in paint chips and coatings.

MR. TREGONING: This is a relatively simple one. It's just two input materials.

CHAIRMAN WALLIS: There's nothing that's likely to act as a catalyst for any of these reactions that's hanging around in the containment?

MR. VIJAY JAIN: Well, at least if you look at ICET 1 environment, the large studies that were done with aluminum and Nukon basically represents what you observed in ICET, large scale test which had everything else in it, indicating that other components are not really playing any catalytic role in enhancing or --

CHAIRMAN WALLIS: If anything, they inhibit. They don't catalyze. They inhibit the reaction, but there's nothing to promote the reaction.

MR. TREGONING: Yeah, at the risk of spinning off here, we've asked all of these very same questions to our peer reviewers, and we've had a lot of initial discussions. We've done a lot of brainstorming just within the group, but then also with the peer reviewers to try to determine.

You know, ICET was unique. We tried to account for a lot of things within the ICET test. We obviously didn't account for everything, nor could we ever account for everything. So we're trying to identify possible contributions to either things that we didn't study or the effect of relatively small amounts of materials like organics and things like that, how they might have resulted in different observations.

So these are all incredibly valid questions that we're certainly pursuing at least in a brainstorming sense, but it does take a bit of discussion and a bit of analysis just to try to get your arms around.

CHAIRMAN WALLIS: But there doesn't seem to be any basis for assuming that there's some sort of a catalyst which promotes reactions.

MEMBER KRESS: Now, how about educating me? I was under the impression that catalysis was a kinetic condition and you would not see catalysis with an equilibrium code.

MR. VIJAY JAIN: That's right. The codes wouldn't predicted it.

MEMBER KRESS: Wouldn't predict it. I mean, wouldn't see anything.

CHAIRMAN WALLIS: The codes wouldn't predict it.

MEMBER KRESS: No.

MR. TREGONING: The thermodynamic.

CHAIRMAN WALLIS: So you have to do the test, right?.

MEMBER KRESS: Or do a kinetics code.

CHAIRMAN WALLIS: So what's the bottom line of all of this?

MR. VIJAY JAIN: Well, there was a question asked on the Nukon fiber. Just make sure that the CM photograph of the Nukon fiber.

These are about seven to ten microns in diameter, and each of these fibers is about a centimeter to two centimeters in length. So these are fairly small pieces that are bonded together with the organic polymer, and during impingement, these are very high strength fibers, I mean, small. You can't really break that easily. So you might not see too much of fragmentation of the fiber size itself.

Well, in summary, the chemical evolution of the sump environment was the aggregate assumption of temperature, pressure, and time. The calculation indicated that the phases predicted in a pressurized system would be similar to the failures predicted in the nonpressurized system at a lower temperature.

It also indicate that in solution and aluminum are the major contributors to the corrosion products.

We benchmarked the thermodynamic simulation to ICET. The ICET data indicated lack of formation of silicates and aluminum hydroxide in the containment water in a 20-day test at 60 degrees centigrade. We revised our thermodynamic simulations and indicated that provided a good correlation with Test No. 1, 2, and 3 up to 360 hours.

Modeling results. The calculations tend to diverge after 360 hours, and I attribute it to selection of initial dissolution rate, which might decrease as a function of time and reduction in the surface reactivity with time due to the formation of passive layers or second phases on the surfaces.

Experimental data indicates a strong synergetic effects between in solution and aluminum. A combination of -- this is the bottom line -- of ICET, lab tests, and simulation provided sites into reactor specific chemical effects. By itself one cannot really do the job. If you look in what we are doing together, it could provide some specific insights.

Well, the plan for the upcoming program. We will continue the modeling based upon ICET results. We would like our future program to include the effect of CO_2 which might additionally form some calcium carbonate.

We will again examine the ICET results and try to incorporate them. Similarly, the gradual evolution of ICET containment chemistry instead of having discrete times, we'll try to have a continuous time dependence and see what type of information we get, and who would like to use PHREEQC for future investigation.

PHREEQC is a jucamental (phonetic) code that can incorporate the effect of CO_2 and other gases. So it might benchmark that particular effect, and the argument is to develop a generalized modeling approach for other reactor specific conditions. So hopefully a combination of the work that's going on.

CHAIRMAN WALLIS: Can you give us some blind predictions of what's being done somewhere at Argonne or somewhere? You make a prediction ahead of time and see if it works out?

MR. VIJAY JAIN: The pre-ICET was a blind prediction.

MR. TREGONING: And we did exactly that.

DR. BANERJEE: And you also can get, of course, what's happening in the solid phase, the precipitates.

MR. VIJAY JAIN: That's right, from simulations, again, that need to be somehow ratified.

DR. BANERJEE: Yes, but it's not easy to verify those in the ICET test because there was such a multiplicity of stuff, but maybe in smaller or different tests you could validate that at some point.

CHAIRMAN WALLIS: Well, industry is doing all sorts of small scale tests, aren't they?

DR. BANERJEE: Right.

CHAIRMAN WALLIS: You have great opportunity to.

DR. BANERJEE: Do you have access to that that they talked about yesterday?

MR. VIJAY JAIN: I got the presentation. I didn't get a chance really.

MR. TREGONING: We haven't seen the test report. We had access to the test plan prior to testing, and we observed that. This was discussed yesterday. We observed some of the testing. We expect, I think, you know, maybe any week now that at least for informational purposes we'll get an advanced look at the --

CHAIRMAN WALLIS: Yeah, I would think they would consult with you because these tools might be useful to them.

DR. BANERJEE: So your initial approach, anyway, is to look at the equilibrium thermodynamics using measured corrosion rates to the use of kinetic effect.

MR. VIJAY JAIN: That's right.

DR. BANERJEE: Now, when you do look at some of these, is it possible then to use kinetics to a limited extent to do more ab initio things than this stuff? I mean use some combination of kinetics codes in a careful manner so that you don't need so many constants. Combine it with some dynamics codes.

MR. VIJAY JAIN: And some of these kinetics codes will also require input of some rate of corrosion so that the rates of corrosion have to be developed.

DR. BANERJEE: But to sort of try to get this on a sound, scientific basis to minimize the amount of stuff you have to do.

MEMBER KRESS: Well, I think you might use the kinetics code to check your assumption of how fast you reach equilibrium and then that would validate your use of the equilibrium code. You're still going to have the dissolution rates in, either one.

MEMBER DENNING: Dr. Shack, could you comment? I saw you dying to do that, and I know you --

MEMBER SHACK: No, I'm not going to say anything.

MR. VIJAY JAIN: You can tell us privately.

MEMBER SHACK: Well, I mean, you just need the kinetics data. It's not as though the kinetics code is sitting out there waiting for you to use it.

MEMBER KRESS: You don't have reaction rate coefficients.

MR. VIJAY JAIN: Yeah, we don't have that information.

MEMBER KRESS: And you may have to put in about 100 reactions or more.

DR. BANERJEE: Oh, that many?

MEMBER KRESS: Yeah, with this many.

DR. BANERJEE: It looks like combustion then

MR. TREGONING: So,m again, it's a plausible path forward, but again, it's certainly non-trivial, to say the least.

DR. BANERJEE: But even combustion, I mean, you can usually take these 80 or 90 reactions and boil them down to eight or nine.

MR. VIJAY JAIN: Yeah. If you really look, I mean, what's boiling down here is the effect of aluminum and insulation. To me if I have to really go and do a plant specific, I would look at those two parameters very closely. I know that based upon corrosion rates of cooper, zinc, and carbon steel, they are very low in amount and doesn't seem to have any significant effect on what's observed in ICET test and lab studies and simulations.

DR. BANERJEE: So you're saying there are only a few reactions.

MR. VIJAY JAIN: Yeah, it could limit down to a few reactions that contribute. You see, we're making a risk informed judgment. You will say that aluminum and solution are the key players in the evolution of the containment water and the byproducts in the system.

And the final slide is some information we published NUREG 6873 that has basically nine predictions which are probably not valid after ICET information came in.

MR. TREGONING: That's the pre-ICET work.

MR. VIJAY JAIN: Pre-ICET work.

CHAIRMAN WALLIS: So this actually is fairly encouraging, that you can actually make these predictions and with some further work you might be able to make some more, and they might actually be useful to industry and the NRC.

MR. TREGONING: Potentially, although, again, I'm a bit of a cynic here. I don't want to be overly optimistic that we're going to be able to develop some model that's going to be able to predict all of the various effects that we're really concerned about.

Will we possibly have a tool that will provide some additional insights, as Dr. Banerjee has been --

CHAIRMAN WALLIS: It's encouraging. I mean, he has had some successes. It hasn't been invalidated and all of that, and --

DR. BANERJEE: And what makes it credible is had some failures. Failures are very good. When things work, you always worry about it, that somebody is fitting the data with something.

CHAIRMAN WALLIS: That's why you ask some of your questions.

Are we ready to take a break now for lunch?

MR. VIJAY JAIN: I'm done.

CHAIRMAN WALLIS: Thank you very much.

MR. VIJAY JAIN: Thank you.

CHAIRMAN WALLIS: We'll take a break. The easiest thing to remember would be 1:30. Shall we go to 1:30 then?

We'll go to 1:30.

(Whereupon, at 12:25 a.m., the meeting was recessed for lunch, to reconvene at 1:30 p.m., the same day.)

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

(1:30 p.m.)

13. CHEMICAL EFFECTS HEAD LOSS TESTING:

OVERVIEW, TECHNICAL PROGRAM, AND RESULTS

CHAIRMAN WALLIS: This is another one of those presentations we have been looking forward to: chemical effects head loss testing at Argonne National Lab. Who is going to start? Bill, are you going to open up for us?

MS. TORRES: I'm going to start.

CHAIRMAN WALLIS: Please go ahead.

MS. TORRES: Good afternoon. My name is Paulette Torres. And I represent the Office of Nuclear Regulatory Research Division of Engineering. I am the project manager of the chemical effect head loss testing. And this is my first time addressing the CRS. Right next to me is Dr. Shack, who represents Argonne National Lab. And he will be presenting right after me.

The chemical effects head loss testing is a complementary research activity designed to evaluate head loss associated with chemical byproducts which form integrated chemical effect test environments, also referred as ICET.

The reason for this project is that we

have little information on head loss associated with chemical byproducts and that we need to understand how chemical byproducts formed in plant-specific environments can affect head loss formation.

The NRC and the nuclear industry developed the ICET program. The ICET program simulated the chemical environment present inside a containment water cooler after a loss of coolant accident.

Chemical byproducts were formed in the environment tested. However, the head loss associated with chemical byproducts was not evaluated in the ICET program. This testing program at Argonne National Laboratory is investigating the head loss associated with chemical effects products.

From a regulatory perspective, the research underway at Argonne National Lab is assigned to help resolve general safety issue 191 resolution. To support this gaol, the work conducted at Argonne provides information to help the staff review their chemical effects part, licensee submittals, in response to general letter 2004-02 and to inform the auditing process.

CHAIRMAN WALLIS: The treatise?

MS. TORRES: The what?

CHAIRMAN WALLIS: I don't understand this.

Something is wrong with this sentence. Did Bill Shack write that sentence?

(Laughter.)

MS. TORRES: No.

MR. TREGONING: I don't think we can finger Bill for that one. Treatment, don't you think?

CHAIRMAN WALLIS: Well, I'm trying to figure out what it is trying to say.

MR. TREGONING: Treatment.

CHAIRMAN WALLIS: This means treatment is where it should be? Okay.

MR. TREGONING: See, Bill, I told you.

MS. TORRES: Our chemical effect head loss testing program investigated the potential head loss associated with chemical byproducts of trisodiumphosphate in environments containing the soft calculation.

We also did dissolution and saline tests. The dissolution tests were intended to identify the dissolved calcium concentrations produced in simulated containment pool conditions. And the settling tests were performed to assess the settling tests of calcium phosphate precipitate.

CHAIRMAN WALLIS: The last sentence is wonderful, too, "Measure the expected settling rate."

I mean, Bill, you're doing very well here.

MS. TORRES: Benchmark testing without chemical products is currently ongoing at Argonne. And it's scheduled to be completed in February. The result of this benchmark testing will be used to ensure consistency in testing methods among the research labs.

Tests to examine the head loss from chemical byproducts in sodium hydroxide buffered environments and sodium tetraborate environments are scheduled to commence immediately after the benchmark tests. All testing in Argonne is scheduled to be completed by April 2006.

MEMBER DENNING: Can I ask the function of -- you said used to -- I can't remember the exact words, but there's something like consistency among the different laboratories. What were your words there? I didn't quite understand.

MEMBER KRESS: They're just seeing if Bill Shack knows how to make measurements.

MS. TORRES: The best way to say it is Argonne along with Pacific National Laboratory. They are doing also benchmark testing.

MEMBER DENNING: Yes, but there is a methodology that is supposed to give consistency treatment of the calculation of --

MS. TORRES: There is a procedure established between the two labs and the NRC.

MR. TREGONING: In anomaly, the test programs are different in that the Argonne work is evaluating primarily chemical effects contributions to head loss wall. The PNNL work is looking at standard insulation materials.

However, we have a certain number of replicate tests that are anomaly-identical that will be performed at both laboratories. These will be cases with standard insulation to bring no chemical effect.

The purpose of those tests is to do dual benchmarking to make sure that the measurements that we get are not loop-specific for the most part or if they are, we want to understand possibly some of the either operator variabilities or things that may occur that could lead to differences. So it's just a way for us to try to assess some of the independent ways of running these tests, some of the variability that might result from that.

MEMBER DENNING: Would you be developing correlations that would be used to calculate head loss?

MR. TREGONING: One of the objectives of the PNNL work is to supply data to develop correlations. You're going to hear about that this afternoon. Initially the Argonne objective is not to develop data to be used for correlations. However, it's certainly available if and when we reach that state where we're ready to try to develop some correlations based on certain chemical products.

CHAIRMAN WALLIS: This is one of my gripes always, that when you do research, you should really make a prediction of what you expect to find before you do the test. Then you learn more.

If you just stack up all kinds of data and then a year later someone tries to explain it, that's a very ineffective way to proceed. You should actually develop your understanding in terms of theory while you examine the data as soon as you can. Then you make much more progress.

MR. TREGONING: Right.

CHAIRMAN WALLIS: Then you can sort of see anomalous things, you know, unexpected things and all of that.

MR. TREGONING: At the risk of stealing too much thunder from this afternoon, I mean, the 6224 correlations and some of the insights that we gave for those were applied and developed as --

CHAIRMAN WALLIS: Yes, they were. That's right. That makes sense.

MR. TREGONING: Certainly agree with your point there.

MS. TORRES: I am turning over the discussion to Dr. Shack, who will be discussing test results.

CHAIRMAN WALLIS: Thank you.

MEMBER SHACK: And, again, I will be presenting work that's really done by my colleagues at Argonne. The principal investigator who designed the loop facility and was leading our effort was John Oras until he broke his leg in two places or broke two bones in his leg.

Ken Kasza is now following up on the thermal hydraulics work. And John Hee Park and Ken Natesan are really handling the chemistry studies that support the work that we're doing here.

The measurements we're making are done in a fairly conventional test loop. We, you know, build up a bed on a horizontal screen. And, again, this is not really meant to replicate a situation in a plant. It's meant to measure the head loss associated with a bed of certain characteristics so that one should recognize that this is really the development of a head loss characterization, rather than trying to represent what really happens in a sump screen design.

CHAIRMAN WALLIS: How do you put the stuff in?

MEMBER SHACK: We put the stuff in basically here. We just open this up and pour it in.

CHAIRMAN WALLIS: So you open the top?

MEMBER SHACK: We open at the top and pour it in.

CHAIRMAN WALLIS: While the stuff is flowing around or what?

MEMBER SHACK: While the stuff is flowing around. When I discuss the actual tests, we'll talk a little bit more about how we treat the material before we put it in.

CHAIRMAN WALLIS: It seems to make a difference how you put the stuff in, the sump tests.

MEMBER SHACK: Yes. It can. One of the things I should point out, again, the tests that we have done to date have been with the perforated plate, rather than a screen.

The perforated plate that we have been using has staggered 3/16th-inch holes and a 51 percent flow area. The sump screens or the perforated plates that people seem to be using for fabricating the new strainers, have somewhat smaller holes, one-eighth inch, and somewhat smaller flow area. And we're switching to that perforated plate for our further testing. And we're using that for our benchmark testing.

As mentioned, the tests to date have been done with a horizontal screen, but we can also run with a vertical screen if that were desirable.

The loop can operate up to 180 degrees F. when we're running with a LEXAN test section, which we can do with some chemistries. For other chemistries, we have to use a clear PVC test section. In that case, we're limited to 140 degrees F.

CHAIRMAN WALLIS: What is the size of the piping in the rest of the loop? It's six inches in the test section. What is it when you get down to the --

MEMBER SHACK: It goes down to two inches so that we can essentially reduce the possibility that we're going to have debris --

CHAIRMAN WALLIS: So the velocity there is about one foot a second, right?

MEMBER SHACK: It goes up.

CHAIRMAN WALLIS: So why does it take so

long to go around the loop?

MEMBER SHACK: It's a big loop.

CHAIRMAN WALLIS: It must be.

MEMBER SHACK: Well, wait until you see the PNNL loop.

(Laughter.)

MEMBER SHACK: They've got a taller building than we do. You know, when we are doing these things, there is always this question of how much do we put in.

CHAIRMAN WALLIS: But if it's a big loop, what sort of typical dimensions? Is it for five feet tall or something?

MEMBER SHACK: Oh, no, no, no. Twenty feet tall.

CHAIRMAN WALLIS: This is 20 feet tall, this thing?

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: Oh, great gods.

MEMBER KRESS: Are we looking at --

CHAIRMAN WALLIS: Why is it so huge? Your test section is just a small part of it, then.

MEMBER SHACK: Well, there was a great desire since some concerns have been raised about earlier loop testing that there wasn't enough space after the elbows to develop a fully developed flow. And so the tendency was to essentially make the loop bigger, have more L/Ds before you got to the test section.

CHAIRMAN WALLIS: Which means that anything that gets through the screen has to take all of these four minutes before it comes around again.

MEMBER SHACK: Comes around again.

CHAIRMAN WALLIS: Okay.

MEMBER KRESS: Are we looking at top down or the side in this?

MEMBER SHACK: This is the side. You know, it's a vertical test loop at the moment.

MEMBER KRESS: So when you want to do horizontal tests, you put an elbow in there and --

MEMBER SHACK: We put a hair pin. You take this section out. And you put a hair pin in that goes over here, comes down and around. And then you put your test section in up here.

CHAIRMAN WALLIS: Because you want to keep things long.

MEMBER SHACK: We want to keep things long. We keep things long.

DR. BANERJEE: What is the total length again?

MEMBER SHACK: I actually don't know. I'm guessing 20 feet by 20 feet, but I might -- you know, it's something on that order/

DR. BANERJEE: So it's trying to switch right there.

MEMBER SHACK: Right.

CHAIRMAN WALLIS: At one foot a second.

MEMBER SHACK: Those are rough figures. Don't hold me to it.

CHAIRMAN WALLIS: To get a four-minute time, you're going to have an awful long pipe.

MEMBER SHACK: It's three and a half minutes, to be precise, but I rounded it off to four for the presentation.

CHAIRMAN WALLIS: Okay. Well --

DR. BANERJEE: One foot per second roughly, right?

MEMBER SHACK: Yes.

DR. BANERJEE: Except in narrow bits?

MEMBER SHACK: Right.

CHAIRMAN WALLIS: Except in the fat bits.

MEMBER SHACK: Now, again, coming back again, one of the decisions we make is what we put in the loop and how much that we've put into the loop. We have tried to essentially say that the head loss, whatever is going on is going to be characterized by the massive chemical products and debris per unit area of the screen.

So if you're making comparisons between our loop and the Los Alamos loop and the PNNL loop, in our loop, one gram of debris is really 47.6 grams per meter of debris.

DR. BANERJEE: What is the difference compared to the Los Alamos, original Los Alamos?

MEMBER SHACK: It is taller. We can also control temperature. You know, our tests are basically plus or minus a tenth of a degree typically when we're running the tests. I believe they have no real temperature control there. So that they had pump heat and some variability.

We also, again, have more L/D. You know, theirs was a much smaller loop, a larger diameter. So their L/Ds were quite different.

CHAIRMAN WALLIS: Well, just to go back to this, if you have a loop which is so long and has a velocity of one foot a second, then you're making these precipitates in a long reactor before they come around to the --

MEMBER SHACK: Our precipitates form very quickly. Yes. Let me discuss exactly how the precipitates come in and arrive at the screen in a little bit because that is a variable that we are interested in. That becomes an important variable that is really very different from the three minutes in our loop. It's not a characteristic time that we want to look at.

Much of our testing today just focused on the ICET-3 test conditions. And the bullet says it's plants which use sodium triphosphate for pH control after an accident. Well, the other important element is that they have cal-sil insulation.

And the ICET-3 test we found -- and, again, the ICET-3 was actually one you could probably predict without the integral test. You know, cal-sil, calcium silicate, will dissolve when you put it in hot water. And you combine calcium with phosphate, and you will get a calcium phosphate precipitate.

CHAIRMAN WALLIS: So it is really STP, not TSP that you keep talking about in all these slides. It's STP.

MEMBER SHACK: Yes, sodium triphosphate.

CHAIRMAN WALLIS: It's not trisodium phosphate.

MEMBER SHACK: Trisodium phosphate. I'm sorry.

CHAIRMAN WALLIS: Well, which one is it?

MEMBER SHACK: Trisodium.

CHAIRMAN WALLIS: Oh, okay. Oh, okay. The dissolved calcium, again, we focus on cal-sil, but you could get calcium from other sources, even from the NUKON, from concrete. Again, when you have the cal-sil, you have an overwhelming source of calcium. It's a plentiful supply. The critical parameter for the production of the precipitate is the massive cal-sil per unit volume of sump fluid.

Plants are now estimated to be somewhere around 1.5 grams per liter if you look at the amount of cal-sil that ends up in the sump and the volume of the sump.

The ICET-3 loading was 19 grams per liter, which looks like a tremendous difference, but you have to recognize that for cal-sil loadings greater than 2 grams per liter, you basically run out of phosphate. So that the amount of phosphate, the calcium phosphate, they generated in ICET-3 was not as un-prototypical as it seems when you look at the sheer mass of cal-sil that was present.

The one thing that will go on is we will get precipitate formation here that will proceed until either we use all the phosphate or we use up all the calcium. The kinetics of the process will depend on the chemistry, particularly on the rate of the TSP addition.

Most of the time in the plants will be essentially calcium-limited. That is, there is an ample supply of phosphates around, but the amount of dissolved calcium that you have is limited by the amount of calcium silicate that you have.

Our initial head loss test was just essentially to replicate the conditions in ICET-3. Then we did a second test that was more parametric to look at a range of chemical product loadings.

Again, the baseline environment here always is 2,800 ppm of boron as boric acid, lithium hydroxide. Typically there are about four grams per liter of the phosphate material. And the temperature was 54 degrees C.

The screen loading, again, if I put in the 55 pounds of cal-sil scaled to my test loop size that they use in ICET-3, I wouldn't have to look for chemical effects because nothing would move.

So we have picked loadings per unit area that seem relatively representative of what we might expect to find in plants. That is, these loadings range up and down from plant to plant and from scenario to scenario in the plant, but we're somewhere in a reasonable range.

This particular one uses 15 grams of cal-sil and 15 grams of NUKON. We build the bed at .1 feet per second. And the intent in the test is typically to build a bed at .1 feet per second. And then we'll get a stable bed. And at that point, we'll do some velocity cycling, typically down.

Our understanding is that the .1 feet per second is sort of an upward bound for the actual velocities that we might be interested in. So we typically are interested in looking at lower velocities

CHAIRMAN WALLIS: Can you build a bed which is made of NUKON, then after --

MEMBER SHACK: That varies from test to test. In most of the tests, we mixed the NUKON and the cal-sil together. And I should mention that our NUKON is preprocessed.

You know, we start out with the leaf shredder kind of NUKON. Then that's processed in a blender. And our blender comes from Wal-Mart. And we use the ice crush setting for 30 seconds. The PNNL people have --

MEMBER DENNING: Prebaked?

MEMBER SHACK: Prebaked. The PNNL people have worked out a test to sort of characterize to some extent the processing that goes on at the NUKON. They look at sort of the retained water that comes out of the NUKON. And they have sort of systematically looked at that.

We get a bed that is reasonably uniform. And so we have stuck with this one preparation procedure. Again, it comes out with a fairly finely disbursed amount of NUKON. The cal-sil we again -- we crush the cal-sil, the mortar and pestle, to a fairly fine grade.

What we do in most of our tests is then presoak the cal-sil/NUKON mixture for 30 minutes. This sort of represents what happens in your waiting in a reactor for a certain amount of time before you start recirculation. So before this stuff arrives at the screen, it has had a certain amount of time to dissolve. And I'll talk about that --

CHAIRMAN WALLIS: So you have got this 15 grams of one and 15 grams of the other. You just pour it on top of the pipe. And then it somehow finds its way to the screen?

MEMBER SHACK: Yes. We mix them together. We make a slurry of 15 grams of cal-sil and 15 grams of --

CHAIRMAN WALLIS: Then you establish the bed for a long time, don't you?

MEMBER SHACK: It depends. The fiber beds -- well, it also depends on what you mean by "establish."

CHAIRMAN WALLIS: You say "establish."

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: We want to know what you mean.

MEMBER SHACK: My standards aren't as high as PNNL's. So for a NUKON bed, I can establish the bed with a fairly few recircs. And our benchmark tests we're running for 20 --

CHAIRMAN WALLIS: PNNL, one of you guys takes hours to recirc.

MEMBER SHACK: Yes. In fact, if you look at the cal-sil beds, it takes a long time to get an established cal-sil bed.

CHAIRMAN WALLIS: It's a mystery, isn't it?

MEMBER SHACK: No. It's just the filtration keeps increasing. And you keep taking out finer and finer amounts of cal-sil.

CHAIRMAN WALLIS: There's always the

debate about whether that is what is happening or something else because from the turbidity measurements they did at Los Alamos, you conclude that all the cal-sil is taken out pretty early on.

MEMBER SHACK: No. I mean, you can see the cal-sil in the loop for a long time. It remains milky, yes. The NUKON disappears to the eye very quickly. You can argue over how long it takes to get the last few percent of it out, but the cal-sil was there for a long time.

CHAIRMAN WALLIS: So how can you correlate anything if you don't know how much cal-sil there is in the bed?

MEMBER SHACK: We know how much we're adding and how much the potential is. In this particular first test, the way we did the test, we wanted to essentially look at the ICET-3 condition, where when they did ICET-3, they added the cal-sil. And they waited for four hours before they added the TSP.

CHAIRMAN WALLIS: Now, you have ICET-3, 4. Is it ICET-3 point what when you say "first test"?

MEMBER SHACK: ICET-3-1.

CHAIRMAN WALLIS: 3-1?

MEMBER SHACK: Because I don't think --

CHAIRMAN WALLIS: It doesn't appear in the quick look report.

MEMBER SHACK: No. That's in the first quick look report.

CHAIRMAN WALLIS: Okay. This other one, the one from last year.

MEMBER SHACK: The one from last year. So in this test, we established the bed. Then we added calcium chloride to give us the amount of dissolved calcium that we estimated was in ICET-3.

CHAIRMAN WALLIS: Along with establishing the bed, you didn't take very long to make this bed compend with PNNL?

MEMBER SHACK: Mostly because we sort of thought when we added the calcium phosphate, that it was going to overwhelm everything and we wouldn't be worried about small changes.

DR. BANERJEE: So you establish the bed first and then --

MEMBER SHACK: First. Then we added the calcium as calcium chloride. We had TSP in the loop. And so we immediately performed a precipitate, which built up on the bed. And we had a very high head loss essentially. The first recirculation, we essentially took the loop to its capability, about five or six psi.

MR. TREGONING: Just a point of clarification. The establishment of the debris bed initially and then adding calcium chloride was really limited to these first two tests here and then a few tests later on. But there are really a multitude of ways in which the bed was established with different amounts of particulate cal-sil, NUKON, and calcium phosphate.

CHAIRMAN WALLIS: So you did various things, it seems to me.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: You run it for about three-quarters of an hour. And then you increased the velocity if I'm looking at the right -- are you going to show us these traces?

MEMBER SHACK: Well, we will when we get to the test. You know, replicating ICET-3 was one, the first test. You know, when we get to the tests that we think are more representative, we will look at those traces in a little more detail.

The next test that we ran again had the same kind of prebuilt bed, a mixture of NUKON and cal-sil. But, instead of adding the 200 ppm of calcium, dissolved calcium, that we expected from the

ICET-3 test, we started out adding 10, 25, and 50 ppm of dissolved cal-sil or dissolved calcium, which, again, formed a calcium phosphate precipitate.

And, again, we had measurable increases in head loss with as little as ten ppm calcium. Twenty-five ppm gave us much larger ones. And, again, 50 and above gave us very large head losses with these debris beds.

Again, the results from these initial tests, we had increased head loss for all the dissolved calcium concentrations down to ten ppm. We did see in one test an interesting phenomenon where the calcium phosphate agglomerated. And it comes down as a white milky substance.

At a very, very low velocity, the particle sort of agglomerated into rather large snowflake kinds of things. So, instead of a fine milky precipitate, we had sort of isolated large snowflakes in a relatively clear solution.

DR. BANERJEE: Why did that happen? Any clues?

MEMBER SHACK: No because we don't seem to be able to reproduce that. We don't call it an anomalous result, but we expected that to happen every time. Because it happened when we had a very low velocity, we expected to find that happen every time we dealt with a very low velocity thing. And it doesn't seem to. I don't know. It seems to happen sometimes and it doesn't happen other times.

CHAIRMAN WALLIS: It may depend on what is happening in the rest of the loop.

MEMBER SHACK: It is not clear I guess is the only thing I can say.

DR. BANERJEE: Has it happened more than once?

MEMBER SHACK: It was most dramatic in this one test. And we again see a tendency towards agglomeration in the slower velocities but nothing as dramatic as it was in the first test, where we again -- I think there's a photo in the quick look report. I mean, it really is very large snowflakes in what looks like to be a fairly clear solution; whereas, in almost all the other cases, there's a real milkiness to the solution.

MEMBER DENNING: Are they truly agglomerated or are they growing crystalline?

MEMBER SHACK: It is hard to say. They look to me like agglomerates, but it's hard to say.

MR. TREGONING: There were some unique things about that test, though, that we haven't tried to demonstrate. I mean, we had deposited and filtered out a significant percentage of product on the bed.

And, really, we didn't drop back to lower velocities in that test until we had reached a point where we were starting to ingest air into the pump. So then the flow rate was cut back. So it's hard to even ascertain how much product was really in the loop. And the product could have had very different particulate sizes than standard calcium phosphate that hadn't been prefiltered in any way.

So there were some unique things. I'm not using the word "anomalous." There were some unique things about that test that may partially explain some of the --

CHAIRMAN WALLIS: There seems to be a snow of precipitate, both in test 1 and test 2.

MEMBER SHACK: Well, yes. The snow in test 1 is completely, you know --

CHAIRMAN WALLIS: It looks more like a pile of down or something.

MEMBER SHACK: Right, yes. You know, the 200 ppm just gives you an enormous amount of calcium phosphate, but that --

CHAIRMAN WALLIS: So the snow here you are talking about is in test 2?

MEMBER SHACK: Is test 2. Now, the interesting --

CHAIRMAN WALLIS: Figure 9 is the snow, isn't it, flocculent precipitates?

MEMBER SHACK: I don't know the figure numbers, I'm afraid.

DR. BANERJEE: Figure 9 has the flocculent precipitates.

MEMBER SHACK: Okay. The important thing is that when we go off and we look at dissolution tests with -- and, again, it's at that time we were looking at fairly higher loads of calcium silicate, 6 to 25 grams per liter. We can form the 220 ppm fairly quickly, 30 minutes in an initial acidic environment. And, again, we expect the calcium to keep dissolving until we -- to continue.

Now, we wanted to go on to look at additional head loss tests for the ICET-3 environment.

CHAIRMAN WALLIS: Are you going to show us any data?

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: All these words --

MEMBER SHACK: It's coming. It's coming.

DR. BANERJEE: Most of the precipitate, the floc seems to be forming in the region away from the bed itself.

MEMBER SHACK: It is forming, you know, essentially as rapidly as we are adding them through.

DR. BANERJEE: Right.

MEMBER SHACK: I mean, the reaction time for calcium and phosphate is instantaneous. So it's forming essentially as soon as we add it to the loop. And then it's just carried on down to the bed.

CHAIRMAN WALLIS: Well, I guess you are going to get to the data. We're going to talk about it --

MEMBER SHACK: Yes, we are going to get to the data.

CHAIRMAN WALLIS: So we can anticipate it. You put it in in pieces in test two. You put in a little bit and then some more and then some more.

MEMBER SHACK: Yes. That was just so we could get one test to cover a range of calcium additions. You know, you can argue whether that is a realistic way to do it, but we were just trying to get some sort of feel for what kind of levels of calcium it was needed to get, you know, a measurable chemical effect.

When we started to do a little bit more systematic look at this, you know, there are a number of important variables here. And one of them is the degree of calycle dissolution that occurs prior to the debris bed formation.

If we get dissolution before the debris bed forms, then we have a debris bed that forms from NUKON, from calcium silicate, and calcium phosphate all mixed together.

If, in fact, we form the bed before we have much dissolution. Then the transformation to the calcium phosphate occurs within the bed. And you could argue --

CHAIRMAN WALLIS: This is where it makes a difference what is in the rest of the loop because if you put the stuff in just above the bed, then it will go through and doesn't come up short until it goes all the way around the loop and comes back. By that time, it has probably made some calcium phosphate.

DR. BANERJEE: No. He is saying the reaction is instantaneous.

MEMBER SHACK: Yes, once you get to the solution. You're limited by the dissolution rate of the calcium silicate, not the formation rate of the calcium phosphate.

CHAIRMAN WALLIS: You think if you put it in your plans, by the time it gets to the bed, it's already precipitated?

MEMBER SHACK: Oh, yes. It is calcium phosphate long before it ever gets to the bed if there's this flow of calcium. The calcium silicate arrives at the bed and then proceeds to dissolve.

CHAIRMAN WALLIS: So why does it keep on building up for minutes after that, then?

MEMBER SHACK: Because we don't have all the calcium silicate dissolved when we add it to the bed. We are adding a mixture of calcium silicate and partially dissolved calcium silicate.

CHAIRMAN WALLIS: So when it says "10 ppm calcium," that means calcium in what form?

MEMBER SHACK: Yes. In that particular test, we were adding dissolved calcium. So we were controlling that in the test.

CHAIRMAN WALLIS: It still keeps building up over time after that. It doesn't instantly --

MEMBER SHACK: Because we are going to dissolve calcium silicate.

DR. BANERJEE: So if I understand the model, it is like, or at least your thinking, it is that as you precipitate calcium phosphate, normal calcium silicate dissolves and if you have an excess of --

MEMBER SHACK: Phosphate.

DR. BANERJEE: -- phosphate, it will continue to --

CHAIRMAN WALLIS: Okay. Well, I guess we have to look at your data --

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: -- because it looks as if nothing much happens for 20 minutes, although you have added cal-sil, until you suddenly put in this dissolved stuff. And then it really goes off.

MEMBER SHACK: Hopefully we'll get to that.

MR. TREGONING: It depends dramatically on the test --

MEMBER SHACK: Test, yes.

MR. TREGONING: -- and the way in which the debris was prepared.

MEMBER SHACK: But the important thing here is we want to look at the degree of dissolution that occurs to the --

MR. TREGONING: Which we think we understand pretty well. Sorry. I didn't mean to leave you hanging.

MEMBER SHACK: And this degree will depend on how long it basically takes to get to the bed. So you've got the time to start recirculation, which is typically 30 minutes.

You've got a transport time, which will depend -- you know, it's variable depending on where the accident occurs and how things are moving within the bed, and the rate of TSP dissolution. That is, we can alter that or at least it certainly seems conceivable that that would have an effect.

DR. BANERJEE: But you are adding dissolved TSP or TSP --

MEMBER SHACK: We are adding dissolved TSP, but we are simulating essentially how long the TSP takes to --

DR. BANERJEE: Comes from that basket.

MEMBER SHACK: Comes from the basket. So that's a variable for us, is that rate that we're adding the TSP.

CHAIRMAN WALLIS: So you are adding TSP at some steady rate as well. And it doesn't say on the graph when the TSP gets in there.

MEMBER SHACK: Because on some tests, it's instantaneous. Let me go through it test by test, and I'll try to --

CHAIRMAN WALLIS: Okay. You explain it.

Okay. It's just I've got more data than you are going to show.

MEMBER SHACK: Well, I am going to show enough data that we ought to keep us busy. Of course, the NUKON and the cal-sil screen loading is another critical factor.

CHAIRMAN WALLIS: So you're not to explain test 2?

MEMBER SHACK: I want to go on to real tests. Tests 1 and 2 were --

CHAIRMAN WALLIS: Just too --

MEMBER SHACK: Those were scoping tests to give us an idea.

CHAIRMAN WALLIS: It gives very interesting results.

DR. BANERJEE: What was wrong with them?

CHAIRMAN WALLIS: What was wrong with them, right? It couldn't be explained.

MR. TREGONING: Nothing was wrong with them.

MEMBER SHACK: Nothing was wrong with them.

DR. BANERJEE: They look valuable.

CHAIRMAN WALLIS: They look very valuable, yes.

MEMBER SHACK: They were valuable in indicating that you could get large head loss with small amounts of dissolved calcium.

CHAIRMAN WALLIS: But it also showed the way things build up with time after you do something, which is very interesting.

MEMBER SHACK: We can come back to those tests. I think the other tests are more interesting if I could get to them.

DR. BANERJEE: Why are they interesting?

MEMBER SHACK: I will explain that when I get to them if I ever get to them.

Now, just to look over our test procedure, what we have done is to conduct baseline tests with no TSP to assess the effect. Again, we get head loss. with cal-sil and NUKON. That happens.

So we get that amount of head loss. We want to see the change in head loss that occurs when we have the chemical effect, which in ICET-3 requires the TSP. So we do the baseline test without the TSP and then the test with the TSP added in some way.

As I have mentioned, we typically presoak the cal-sil in NUKON flurries at temperature for 30 minutes to simulate the time prior to recirculation. I would argue that this is really a minimum residence time for dissolution. That is, in the real situation, the cal-sil will be in the loop for something greater than 20 minutes before it actually arrives to the screen. So it's sort of a minimum time.

We have also essentially used calcium chloride additions to represent the sort of limiting case of complete. You know, rather than wait for the calcium silicate to dissolve, we have just taken the equivalent amount of dissolved calcium and added it by getting calcium chloride.

So when we do the test with the NUKON bed and the calcium chloride, we're looking at a test where you have a very long residence time in the sump before you build the bed. And that's a limiting case for us.

DR. BANERJEE: So you have a nice table in your report --

MEMBER SHACK: Right.

DR. BANERJEE: -- which summarizes all of these things?

MEMBER SHACK: Right, which tries to put those together. Again, we have looked at various additions of the TSP to represent. Because that is another uncertainty, we don't have a good grasp of exactly how rapidly the TSP gets added. We wanted to see how large that effect was.

Okay. Now we're off to data. The rest set of data we wanted to look at, the very faint on is my baseline test here, which is really ICET-3-11. It's this number 11 test.

So this is 15 grams of cal-sil, 15 grams of NUKON, no TSP. And I'll get this again. It takes me roughly at least an hour to get to some sort of steady state condition or pseudo steady state condition for the bed as it builds up. I get a pressure loss of about a psi.

CHAIRMAN WALLIS: Now, in the tests at LANL, sometimes it took a long time to equilibrate and the pressure drop was building up with time. Sometimes it didn't.

MEMBER SHACK: Well, the NUKON tests build up very rapidly. With the cal-sil, it's much slower.

CHAIRMAN WALLIS: The NUKON, I think we understand that.

MEMBER SHACK: Right.

CHAIRMAN WALLIS: But with cal-sil, it seems to take different amounts of time to come to equilibrium if it ever comes to equilibrium.

MEMBER SHACK: Well, in our experience with our cal-sil tests, we have never found one that came rapidly up.

CHAIRMAN WALLIS: No. It's a question of is it 10 minutes to a 50 or a 100?

MEMBER SHACK: No. It's more like is it 60 minutes --

CHAIRMAN WALLIS: Right.

MEMBER SHACK: -- or is it 200 minutes?

MR. TREGONING: That is not quite true. The initial mixtures of debris, we initially looked at different concentrations of debris. The first test in there in the report had seven grams of NUKON and 25 grams of cal-sil. If you look at those, there is some very rapid head loss that occurs in those tests.

Now, did it equilibrate? I would argue probably not. But I think those results are somewhat analogous to what we have seen in prior LANL studies as well as subsequent PNNL studies as well.

DR. BANERJEE: This is ICET-3-25 you're talking about, baseline?

MR. TREGONING: This would have been --

DR. BANERJEE: Seven, 25 cal-sil, 25?

MEMBER SHACK: We will come back to the 7 and 25. I wanted to go --

MR. TREGONING: Sorry.

MEMBER SHACK: There the head loss with

the 25 grams of cal-sil was so large we couldn't really discern a chemical effect. We were already -- we died from the physical debris. I wanted to go to a test condition, at least initially, where we could see a dramatic effect of the chemical effect.

DR. BANERJEE: So you're showing a presoak effect versus a no presoak?

MEMBER SHACK: No, no. Again, test 11 is a no chemical effect. This is purely physical debris. It's just cal-sil and a NUKON, no TSP. I don't form any calcium phosphate. I get one psi pressure drop.

If I take essentially the same conditions, the physical debris loading, same pretreatment of the cal-sil and NUKON except that now I have added TSP and I add the TSP, I put half of the TSP in during the presoak of 30 minutes and I add the remainder of the TSP over the 30 minutes after the presoak is added to the loop, I guess, of course, now a much, much larger head loss.

CHAIRMAN WALLIS: So it goes up in steps if it has something to do with the residence time in the loop? Is that what is going on there?

MEMBER SHACK: Well, I suspect that it is building up as I take recirculations around the bed and I'm filtering and I'm filtering and I'm filtering.

DR. BANERJEE: Typical recirculation time is what?

MEMBER SHACK: Three and a half to four minutes.

MEMBER DENNING: So it is consistent with the steps, --

CHAIRMAN WALLIS: So it is reasonably consistent with the steps, right.

MEMBER DENNING: -- although it could be --

MEMBER SHACK: This is a test where I have added the TSP during the presoak.

CHAIRMAN WALLIS: Which is that test?

MEMBER SHACK: That's test 10.

DR. BANERJEE: Half during presoak, half --

MEMBER SHACK: Half.

DR. BANERJEE: -- metered in after?

MEMBER SHACK: Now, you know, the half presoak, half afterwards, I could have forgotten the half afterwards because I had all the phosphate I needed to do the job already, but, you know, we're just sort of -- it sort of looks nice to be semi-prototypical-looking.

DR. BANERJEE: But more complex to

interpret, right?

MEMBER SHACK: The next interesting test to look at now is test number 6, where we have essentially added -- again, here is my baseline test with no TSP. And, again, it's the same baseline test that I had up here. But in this test, I didn't add the TSP to the presoak.

CHAIRMAN WALLIS: When did you add it?

DR. BANERJEE: Which number is that?

MEMBER SHACK: Test 6.

DR. BANERJEE: I thought you added 18 initially.

MEMBER SHACK: That's to the loop, but not to the presoak. Now --

CHAIRMAN WALLIS: So there is TSP in this?

MEMBER SHACK: There is TSP.

CHAIRMAN WALLIS: Why is there no effect until 35 minutes?

MEMBER SHACK: I'll explain that. One of the things that's, again, unprototypical about our situation is that -- and we'll discuss this when we come to the dissolution test -- typically adding TSP slows the dissolution rate if you have got low concentrations of cal-sil because, again, the cal-sil dissolves more rapidly in an acidic environment.

If you have the cal-sil and you add the TSP, you get a more neutral environment. You slow the dissolution of the cal-sil. That's not true in concentrated cal-sil solutions because what happens in those cases, you add the cal-sil. It begins to dissolve. And you essentially now saturate the solution with dissolved calcium.

You come to an equilibration of the calcium-calcium silicate dissolution at that condition. You can increase the dissolved calcium in that test by adding TSP. So you take the calcium out of solution, and you allow more calcium to dissolve.

DR. BANERJEE: In this test 6, you had a presoak, right?

MEMBER SHACK: We had a presoak, but we didn't add any TSP, which means that --

DR. BANERJEE: During the presoak?

MEMBER SHACK: During the presoak.

DR. BANERJEE: Right.

MEMBER SHACK: So in the presoak, we got up to 200 ppm dissolved cal-sil because the concentration in our presoak happens to be just about what it is in ICET-3. That's an accident. And so you get the 200 ppm. So when we dilute that into the loop, we're only adding four ppm of dissolved calcium at that point. And you see very little effect.

Now, what happens is that as you're in this loop, calcium continues to dissolve. Most of the cal-sil or much of the cal-sil has been trapped in the bed. Other of it is circulating around. But it is dissolving. It's combining with the calcium phosphate.

CHAIRMAN WALLIS: TSP came in at the beginning?

MEMBER SHACK: TSP came in at the beginning, but if there's no dissolved calcium, it doesn't --

CHAIRMAN WALLIS: It doesn't say so. It would be useful if it said "TSP" at that arrow or something so that we can --

MEMBER SHACK: Okay. I can --

DR. BANERJEE: TSP is there. One-sixteenth of the --

MEMBER SHACK: One-eighth.

DR. BANERJEE: One-eighth, yes, is already there.

MEMBER SHACK: But, again, since I have very little dissolved calcium, it doesn't make any difference. But, again, as I begin to dissolve the calcium, I build up my head loss. And essentially I get up eventually to a very high head loss, again given enough time.

MR. CARUSO: Let me just say this again. We add all the TSP and one shot at times zero.

MEMBER SHACK: No, no, no. I had one-eighth --

MR. CARUSO: You had one-eighth there.

MEMBER SHACK: -- in there. And then I added the rest of it over time.

DR. BANERJEE: Over how long? Over how long?

MEMBER SHACK: Essentially a half an hour.

CHAIRMAN WALLIS: When did you stop?

MEMBER SHACK: When I ran out of TSP.

CHAIRMAN WALLIS: So that should be shown on graph 2 or something --

MEMBER SHACK: Yes, yes.

CHAIRMAN WALLIS: -- so we can tell what's going on.

MS. TORRES: I think it was in test 6. Wasn't it metered into seven-eighths over an hour?

MEMBER SHACK: It may have been over an hour. Again, since you're calcium-limited here, it almost doesn't matter which rate you add the -- I could have dumped all the phosphate in at T equals zero and it wouldn't have made a dime's worth of difference to what is going on here. I have to wait until the calcium dissolves.

DR. BANERJEE: I am trying to understand the mechanism. I guess Graham is, too. So let's go over it step by step. If you presoak the stuff without TSP, some of the calcium and the cal-sil goes into solution at that point.

MEMBER SHACK: At 200 ppm roughly.

DR. BANERJEE: Two hundred ppm. But since there is no TSP there, it has nothing to react against.

MEMBER SHACK: Right.

DR. BANERJEE: So no precipitators form, nothing.

MEMBER SHACK: Nothing. The dissolution stops.

DR. BANERJEE: At that point, it stops.

MEMBER SHACK: It stops.

DR. BANERJEE: Now you start adding TSP.

MEMBER SHACK: I know. Now I pour this into the loop.

DR. BANERJEE: The loop. And you add TSP.

MEMBER SHACK: Adding TSP.

DR. BANERJEE: So forget the one-eighth

that you added.

MEMBER SHACK: Right.

DR. BANERJEE: So as you do that, you start to precipitate our --

MEMBER SHACK: I start to dissolve calcium.

DR. BANERJEE: But you must make room for it so it's reacting, right?

MEMBER SHACK: Right. Well, my dissolved calcium level once I've poured the presoak into the loop is four ppm. It's going to start dissolving and immediately react with the phosphate.

DR. BANERJEE: Okay. Because you're diluting it?

MEMBER SHACK: Because I'm diluting it.

DR. BANERJEE: So it starts to dissolve, --

MEMBER SHACK: Right.

DR. BANERJEE: -- starts to react with the phosphate.

MEMBER SHACK: And eventually I see a buildup in the pressure.

DR. BANERJEE: So does it get to sort of the equilibrium level of around 200, the calcium, or what happens to the calcium levels?

MEMBER SHACK: No. The calcium all disappears because there's more than enough phosphate to take out all the calcium.

DR. BANERJEE: It eats it?

MEMBER SHACK: The phosphate just eats it all up.

DR. BANERJEE: So the rate-determining step becomes the dissolution of the cal-sil.

MEMBER SHACK: Dissolution of the cal-sil.

MR. CARUSO: Until the phosphate is exhausted?

MEMBER SHACK: Well, in the real world, it appears that you're almost always calcium-limited, rather than phosphate-limited. In ICET-3, you were phosphate-limited.

DR. BANERJEE: So it is not a very complex mechanism.

MEMBER SHACK: The important thing here is how fast it can all happen. You know, people talk about having margins for head loss. It's one thing if you're building up chemical effects over 30 days. You know, we're talking about chemical effects that occur over an hour, you know, 30 minutes.

DR. BANERJEE: So it's basically dictated by dissolution kinetics.

MEMBER SHACK: Dissolution kinetics.

DR. BANERJEE: Which depends on the fluid mechanics probably of the dissolution, right?

MEMBER SHACK: Well, I would argue, you know, you're in a fairly quiescent pool.

DR. BANERJEE: This thing is in this basket with flows going through it.

MEMBER SHACK: Right. Now, the TSP --

DR. BANERJEE: It has a plume behind it or something?

MR. CARUSO: Are you really in a quiescent pool? I mean, it's raining throughout.

MEMBER SHACK: Well, it is raining throughout, yes.

MR. CARUSO: Raining pretty healthfully, too.

MEMBER SHACK: Whether it is raining or it's quiescent, it doesn't make much -- you know, whether it gets you in 20 minutes or 45 minutes, you know, I'm not going to argue over that. The answer is it happens fairly quickly. I think that's the important point here. You know, I'm not going to argue over prototypically exactly what time it is, but it's not 30 days.

DR. BANERJEE: So you're saying it's

phosphate-limited, this whole thing?

MEMBER SHACK: It is calcium-limited.

DR. BANERJEE: Isn't there a lot of calcium around?

MEMBER SHACK: No, no, no. The calcium primarily comes from the cal-sil insulation. Now, we could argue how much calcium is in the concrete, how much you could get out of the NUKON itself, but the cal-sil is the primary source of the calcium. So it is really cal-sil-limited.

DR. BANERJEE: So there is excess triphosphate.

MEMBER SHACK: Excess triphosphate.

CHAIRMAN WALLIS: So this cal-sil is mostly trapped in the bed, isn't it? So the reaction is taking place in the bed?

MEMBER SHACK: Well, there are cal-sil fines that are recirculating. There is cal-sil trapped in the bed. Exactly what fraction is in the bed and what fraction is recirculating, which portion is dissolving is not clear.

DR. BANERJEE: You didn't take any samples and do this sort of thing that these guys did?

MEMBER SHACK: No. We probably should have been doing that, but we didn't.

Just to come on again, we did some replicate tests. And, again, this is a replicate test now of the one with the TSP addition, where we're adding the half TSP to the presoak. And so we get, you know, a little difference, but we get high head losses of about the same magnitude in each case. And, you know, it happens fairly quickly, a little more quickly.

CHAIRMAN WALLIS: Now, this argument that the buildup with time is all due to continually filtering out more cal-sil I think needs to be sorted out, whether that's true or whether it's due to the changes in the structure of the bed, which is another explanation, which seems to be consistent with some of the observations from previous tests.

MEMBER SHACK: I mean, if you look visibly at the turbidity, it's decreasing as the test --

CHAIRMAN WALLIS: Well, one way you could do it easily -- well, I don't know if you could do it easily -- is to simply take out the fluid and put in clean fluid.

MR. TREGONING: The PNNL people are going to be doing things like that.

CHAIRMAN WALLIS: Doing that kind of thing. Okay.

MR. TREGONING: Well, I think the PNNL results will show you some differences between continual filtering due to accumulation of debris --

CHAIRMAN WALLIS: See, this continual filtering, why is it in this bottom test, jump up in steps like that? If you look at what happened after you increase the flow, it goes up in another step like that.

MEMBER SHACK: Okay. You know, just to replicate this test, we're coming down here to the no TSP test. So this is a pure physical debris test. Again, I would argue that as I decrease velocity, these beds remain fairly stable. That is, you know, there are undoubtedly changes. And we measure changes in the bed depth as we decrease the velocity, the pressure drop decreases, and there is a kind of an elastic expansion of the bed.

When you increase the velocity, you get a sort of a non-linear effect. And so you compress the bed. It now becomes a more effective filtration mechanism. And so you trap more. And you're trapping more within the bed.

So you not only get the immediate jump-up of the elastic or of the compression, you get a continual buildup due to the more effective filtering you get of the cal-sil that's still in the --

DR. BANERJEE: But the bed permeability doesn't go linearally within Darcy's law.

MEMBER SHACK: No.

DR. BANERJEE: So, in fact, that's what you're seeing that effect, that even though the porosity mentioned and the permeability won't. That's why I think you get the higher pressure drop.

MEMBER SHACK: So you get this buildup here that's continuing on. Now, again, so we have shown, at least under some conditions, you get a very dramatic contribution of the calcium phosphate to the head loss. That is, you're getting a much higher head loss --

CHAIRMAN WALLIS: Bill, now, I have found this now. To go back to the figure 13 you showed us, which is --

MEMBER SHACK: Which slide?

CHAIRMAN WALLIS: The one on the bottom there. In your report, you have a corresponding figure, which shows the trajectory of pressure drop and velocity go up.

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: And then when you decrease the velocity at around 210, right, you decrease the velocity, what you show in your figure is that you go to decrease the velocity from .1 to .09, .09 something, you hardly decrease it at all and the pressure drop goes down by about 60 percent, which seems very strange. It doesn't show on this figure because you haven't got the --

DR. BANERJEE: What is the number?

CHAIRMAN WALLIS: This is figure 14. I mean, usually you expect it to go back the way it came up more or less.

DR. BANERJEE: It's a hysteresis effect.

CHAIRMAN WALLIS: Well, yes, but the hysteresis effect is usually the other way around.

DR. BANERJEE: Right.

CHAIRMAN WALLIS: Usually it stays up, rather than leaping down? There's something strange about that. If it's --

MEMBER SHACK: Partly that could be just the way that we're sampling the data when we make a rapid change here.

CHAIRMAN WALLIS: If it's a correct plot. Maybe the plot is misleading in some way.

DR. BANERJEE: Well, the top branch has the hysteresis effect that you expect.

CHAIRMAN WALLIS: That you expect, right.

And the bottom one is odd, unusual. Maybe it's an artefact of the way you're sampling or something. You're going to sort that one out.

MEMBER SHACK: We will sort that one out.

CHAIRMAN WALLIS: And then there is something odd, too, in that if you look at the trajectory, after about, you know, we get to 0.14 feet a second or something, it seems to be going up very steeply. Presumably if you kept on that trajectory, it would go up to an even much higher value.

So there are a whole lot of questions you get from -- I'm sure you asked yourself these things, too. The more you look at the details, the more you want to do another test.

I'm sorry. The audience doesn't understand it because I'm looking at another figure, but if you look at the other figure of how velocity is changed and pressure drops, there are some anomalous things or some odd things about that, the way it works.

DR. BANERJEE: But you also show us tests 1 and 2 because they have a very interesting --

CHAIRMAN WALLIS: He doesn't want to do that, though.

MEMBER SHACK: We don't want to do that.

Let's go through all the tests I want to show you first. And then we can come back to those if we have time.

DR. BANERJEE: You should not send us the quick look reports.

MEMBER SHACK: We did.

DR. BANERJEE: You should not.

MEMBER SHACK: And, again, what we're looking at here are rests where we have had now the same 15 grams of NUKON, but we changed the cal-sil from 5 grams to 10 grams, 15 grams.

CHAIRMAN WALLIS: Of course, your numbers of grams don't correlate with the Los Alamos tests precisely, do they?

MEMBER SHACK: No. We have different --

CHAIRMAN WALLIS: Right.

MEMBER SHACK: What you need to compare are screen loadings because we have different diameters.

CHAIRMAN WALLIS: You have the same diameter, don't you, almost essentially the same diameter?

MEMBER SHACK: No. Theirs is almost a foot.

CHAIRMAN WALLIS: Is there a big

difference there?

MEMBER SHACK: Oh, yes. This is a very significant --

CHAIRMAN WALLIS: I thought we had a six-inch one, too.

MEMBER SHACK: No, no.

CHAIRMAN WALLIS: It was PNNL that has the six?

MEMBER SHACK: PNNL. We're close. There's a big difference between ours and the --

CHAIRMAN WALLIS: I was confused, then. Okay.

MEMBER SHACK: Again, with these loadings, we see a relatively -- you know, almost no measurable effect of --

CHAIRMAN WALLIS: But you don't have a plot showing how your data compare with somebody else's?

MEMBER SHACK: We're running the benchmark tests now. We were supposed to run the benchmark tests a long time ago. We ran the first ICET-3 test. It turns out the results were so interesting everybody wanted to keep testing chemical effects. And we never got around to the benchmark testing for a while.

DR. BANERJEE: So 3-11 does not qualify as a benchmark, then?

MEMBER SHACK: Well, I should say that if we go back and we look at our pure no TSP test and to the velocities -- we have built better, comparable ones with PNNL. We actually compare pretty well.

You know, we didn't run those as benchmark tests. You know, they build their bed. And then they go off and do their velocity cycling. And we go off and add our chemicals. But if you just looked at the initial portions where we could compare them, I would say that we are comparing fairly well.

So I think that the benchmark tests will demonstrate that we get comparable results, but that at the moment we have no systematic comparison of those.

DR. BANERJEE: Did you do a premixed --

MEMBER SHACK: Yes. Again, you know, if we're comparing premixed with premixed, obviously if we compare different addition histories, you'll get different results. But, again, when we're actually trying to match tests where the conditions are expected to give us the same results, we get it.

Again, my point here only is that our relative contribution of the calcium phosphate depends strongly on the debris loading. These are the tests that --

CHAIRMAN WALLIS: Although on the bottom figure there, you haven't really reached an equilibrium because your velocity is continuing to decrease, I understand.

MEMBER SHACK: Yes. Well, we can't --

CHAIRMAN WALLIS: Velocity number 10 is drifting down. If you kept it constant, one might hypothesize that the red curve would keep on going up.

MEMBER SHACK: Right. But when we --

CHAIRMAN WALLIS: Pump more than five psi? Is that it?

MEMBER SHACK: Right. You know, we're pushing the loop to its --

DR. BANERJEE: It's acanthotic.

CHAIRMAN WALLIS: But it hasn't reached an equilibrium, essentially. It's still drifting down.

MEMBER SHACK: Yes. If we push the velocity back up and if we pressurize the loop and push the velocity back up, we would get more head loss. But that wasn't what we were really trying to demonstrate there. All we wanted to do was demonstrate that the calcium phosphate was giving you a greater increase to the velocity.

CHAIRMAN WALLIS: But if someone were

looking at this and saying they have reached equilibrium, that would not be a right conclusion.

MEMBER SHACK: No, that's not. That's not the correct conclusion because we can't maintain velocity in the loop.

DR. BANERJEE: Are there still fines being generated at that point, do you think, or are you just taking out entrained fines or is it bed relocation occurring?

MEMBER SHACK: Well, all sorts of things are happening at that point. I mean, the pressure is going up. The bed is compressing. The cal-sil is continuing to dissolve.

You know, in these chemical tests, almost there's nothing that really is a steady state because the cal-sil is continuing to dissolve in all of this. You know, if we kept running this test for days on end, we would probably still get different results.

DR. BANERJEE: But your phosphate is all used up.

MEMBER SHACK: No, no, no, no. I have plenty of phosphate. I always have plenty of phosphate.

CHAIRMAN WALLIS: Eventually if you dissolved all the cal-sil, you might have some --

MEMBER SHACK: Yes. Eventually it dissolves all the cal-sil.

CHAIRMAN WALLIS: It's quite different then.

MEMBER SHACK: We can't run the test that long or we haven't run the -- or let's say if you're dead, it doesn't matter what happens after the cal-sil dissolves. If the head loss is already so high --

CHAIRMAN WALLIS: If it dissolves, then your bed no longer has cal-sil in it. So something else is going on. What happens to --

MEMBER SHACK: Well, you've got calcium phosphate.

CHAIRMAN WALLIS: What happens to the sil pod of the cal-sil? The cal goes into the phosphate. What happens to the sil pod?

MEMBER SHACK: You're getting the silica levels.

CHAIRMAN WALLIS: Isn't silica built up in the bed, then?

MEMBER SHACK: No. Most of that stays as a soluble.

CHAIRMAN WALLIS: Stays as a soluble.

MEMBER SHACK: Yes. You know, we have measurements essentially of the silica in the loop.

And it is going up as the test continues. If we now come to the, again, 7-gram NUKON, 25-gram cal-sil, again, here we added these in steps, which is why you see the debris load.

Again, the debris load went up so fast that it's difficult to tell whether there is a chemical effect or not. The debris itself was enough to essentially give you a head loss that essentially exhausted the capability of our loop.

CHAIRMAN WALLIS: When does it come down again?

MEMBER SHACK: Because we lowered the velocity.

CHAIRMAN WALLIS: Yes, but you didn't lower it that much.

MR. TREGONING: We lowered it from .1 to about .5.

CHAIRMAN WALLIS: Yes, but -- well, maybe that's enough to do it. Maybe that's enough to do it, --

MEMBER SHACK: Now, what we did, again --

CHAIRMAN WALLIS: -- although before you had it down to even lower velocity. And the head was --

MEMBER SHACK: It was sort of going up.

CHAIRMAN WALLIS: So there's something odd there.

MEMBER SHACK: Well, yes. I can't remember. We may have perforated the bed under the pressure load. All sorts of things begin to happen when you get up to very high pressure loads. And so you blow through, and the pressure drops. I don't remember exactly what happened in that test.

If we run the test without any NUKON at all, just the 25 grams of cal-sil on the perforated plate, again, we don't get complete coverage. It doesn't really bridge it effectively. You know, it seems that it takes a certain amount of fiber to get -- so we get some elevation in head loss, but we have enough essentially flow through to keep the head losses fairly low.

CHAIRMAN WALLIS: With a little more cal-sil, you might --

MEMBER SHACK: With a little more cal-sil or just a little bit of NUKON, yes, things would probably go up much more dramatically.

CHAIRMAN WALLIS: Now, this is still going up after four hours? Slightly.

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: Not much.

MEMBER SHACK: Not much. Okay. This is a test now where we kept the cal-sil --

CHAIRMAN WALLIS: Let's take the perforated bed, where it would be rather difficult to reproduce. You might do another experiment and be somewhat different.

MEMBER SHACK: It may well come out, too. If we take the -- now, a test where we have 10 grams of cal-sil and 5 grams of NUKON, you remember the 15 grams of NUKON and 10 grams of cal-sil gave us virtually no chemical effect. And we've got a relatively low head loss.

Again, here it is with the 15/10. The 5/10 now gives us the very high head loss. Again, the saturated head, this bed is only three millimeters thick. This is an 11-millimeter bed. But the head loss is far greater in the thin bed.

So, again, for a given cal-sil loading, we get a highly non-linear, non-monotonic function of the fiber loading. That is, if we have no fibers at all, we get a relatively low head loss.

With a little bit of fiber, we jump the head loss up. With additional fiber, it can come back down again. And if we keep adding fiber, of course, it will continue to go on back up. So it's, again --

DR. BANERJEE: Why did it stop sort of I or is that an optical illusion? Look at the other line.

MEMBER SHACK: This one? Do you mean this one?

DR. BANERJEE: I may be mixing up the velocity --

MEMBER SHACK: I think you're mixing the velocity and the --

DR. BANERJEE: Yes. So the velocity is going down.

MEMBER SHACK: Yes. This one we just couldn't keep the velocity up. It was jumping up so fast the velocity -- we lost control of the loop. The head loss had just gone up.

DR. BANERJEE: That was 3-18.

MEMBER SHACK: Right.

DR. BANERJEE: But that's not in our table here. What happened to that or is it?

MEMBER SHACK: Well, that was in the quick look report. We're on from the quick look report.

DR. BANERJEE: Oh, we are beyond the quick --

MEMBER SHACK: We are beyond the quick look report.

DR. BANERJEE: That's why we don't have this.

MEMBER SHACK: We keep testing, right.

DR. BANERJEE: So that --

MEMBER SHACK: The quick look report was late December.

DR. BANERJEE: Okay. So 3-18 now. You have five grams NUKON, ten grams --

MEMBER SHACK: Cal-sil.

DR. BANERJEE: -- cal-sil.

MEMBER SHACK: We have a three-millimeter bed.

DR. BANERJEE: And it a presoaked for 30 minutes.

MEMBER SHACK: Presoaked, same treatment.

DR. BANERJEE: And is there trisodium phosphate?

MEMBER SHACK: There is trisodium phosphate.

DR. BANERJEE: Added during the presoak?

MEMBER SHACK: Presoak.

DR. BANERJEE: Okay.

MEMBER SHACK: So it's a complement, essentially, of the previous test, where you had the 15 grams of NUKON and 10 grams of cal-sil. Again, by reducing the NUKON, we have jumped the pressure loss from here to here.

CHAIRMAN WALLIS: And, again, if we jump the pressure drop, then the velocities come way down.

MEMBER SHACK: I would suggest if I could maintain the velocity, it would be even more dramatic.

CHAIRMAN WALLIS: But the pressure drop per unit velocity is enormously --

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: What is that curve to the right? That is an interesting one. Is that for the same cal-sil proportions?

MEMBER KRESS: He described it.

MEMBER SHACK: That is just a schematic.

CHAIRMAN WALLIS: A schematic.

MEMBER SHACK: No data, just a picture of what happens here. The fact that if you have a given amount of cal-sil --

CHAIRMAN WALLIS: A given amount of cal-sil?

MEMBER SHACK: A given amount of cal-sil.

CHAIRMAN WALLIS: Because it says you were given -- okay. Okay. Now I understand.

MEMBER SHACK: Cal-sil loading. It can be a highly non-linear, non-monotonic function. So with no fibers, it's equivalent to what we saw before. We couldn't form a continuous bed. We would get a very low head loss.

We had a small amount of fiber. We're in this sort of a test, where we get a very high head loss. We add more fiber. We're back to this situation. The head loss drops. Of course, if we keep building up the fiber, we're going to keep building up head loss.

DR. BANERJEE: Is that partially a function of the rapidity with which cal-sil dissolves so you're not making that gooey stuff to --

MEMBER SHACK: No.

MR. TREGONING: You see a similar phenomenon even without chemical products.

MEMBER SHACK: You know, the chemical products will eventually shift the curve around and change things around.

CHAIRMAN WALLIS: It must depend on how you put it in because you could have gotten to the peak and then put some more fibers in. It wouldn't make any difference.

MEMBER SHACK: You would get a different curve depending on how you put them in. You know, our tests have all been done so far for homogeneous additions.

CHAIRMAN WALLIS: Well, they put them in at the same time to get that.

MEMBER SHACK: Right. And we're still getting that now.

CHAIRMAN WALLIS: But if you could make a sandwich, you could get all kinds of --

MEMBER SHACK: If you make a sandwich, you can get different ones.

DR. BANERJEE: So the hypothesis would be that with this thin bed you are likely to jam up all the pores.

CHAIRMAN WALLIS: Exactly. If you have the right proportions of stuff, you can jam up the pores.

DR. BANERJEE: Pores, right.

MR. TREGONING: And, again, essentially the same mechanisms we saw in LANL experiments two, three years ago at this point.

CHAIRMAN WALLIS: Except for theirs sometimes would suddenly happen, rather than --

MEMBER SHACK: I mean, the difficult thing is it's hard to know when you're being conservative here. If you overestimate your debris loading, you're not necessarily being conservative. And that makes life difficult when we don't know that we can --

CHAIRMAN WALLIS: You don't know where the real pickiest peak is.

MEMBER SHACK: Right. Now, this was again an attempt to look at sort of the -- you know, we looked at one bounding thing where we had very little dissolution of the cal-sil before we formed the bed. And we found that we eventually built up to the pressure loss. It took us time to do that.

What I've got here now is a bounding case where, instead of adding cal-sil, I'm adding calcium chloride. So I get a dissolved calcium level that is equivalent to full dissolution of the cal-sil.

So this is the other bounding case where I go a very long time in the pool, high dissolution before I form the bed, in which case, again, I get an even more rapid buildup than I do in the case where --

DR. BANERJEE: That means you have got your 200 ppm or whatever is that solubility.

CHAIRMAN WALLIS: This is all the constant velocity?

MEMBER SHACK: This is all the constant, yes. We would like to do the velocity cycling tests except when the pressure drop gets so large we can't control the loop anymore and do those tests.

But, again, what I would argue is that the real behavior is somewhere between these two. This is sort of, at least from my engineering judgment, in our dissolution data sort of the minimum amount of dissolution, I would think, would take place before you formed the bed. This is clearly a bounding amount of dissolution before you form the bed.

And so, at least for this particular loading, you have this range of possible behaviors, somewhere in between, depending on, again, the exact details of the --

CHAIRMAN WALLIS: Maybe give the same --

MEMBER SHACK: Eventually you'll get there. And, again --

CHAIRMAN WALLIS: What is the blue dot thing?

MEMBER SHACK: The blue dot thing is just -- again, this is --

CHAIRMAN WALLIS: You waited all the time before you --

MEMBER SHACK: I'm just adding nine ppm of calcium. I'm adding a small amount of calcium. But, again, this is adding the -- I build the five of those first.

CHAIRMAN WALLIS: When you eventually put it in, it leaps up to join the other curves.

MEMBER SHACK: Yes. And I get there with only essentially 27 ppm of calcium. I'm all the way up here. But, again, I've now added this calcium in a somewhat perhaps non-prototypical manner because I've built the bed. And then I've precipitated the calcium phosphate on top of it.

So this you might argue is kind of an arrival time kind of phenomenon.

CHAIRMAN WALLIS: There are some tests where you essentially plug the thing up completely, the velocity goes to zero. Aren't there some tests like that?

MEMBER SHACK: Oh, yes. Yes. I mean, most of our tests or more of our tests than we planned on seem to end up that way.

CHAIRMAN WALLIS: Actually, I don't know if we show them here, but in your reports, there are some places where the velocity seems to go to zero almost.

MR. TREGONING: Just a point of reference, I mean, we are talking about 220 ppm of dissolved calcium, but the 15 grams of cal-sil given the volume at a loop equates stoichiometrically to about 45 ppm of equivalent dissolved calcium, so not at the 200 ppm level, where you can get, again, fairly significant head losses.

DR. BANERJEE: One of the things I notice is that in your previous slide, you had 15 grams of NUKON and 10 of calcium silicate for 3-16; 3-18, you had 5 and 10; but now if you go to 15 and 15, as you have in ICET-3-10, your pressure drop goes way back up. So if you keep the NUKON constantly by 15 and you just increase your calcium --

MEMBER SHACK: It depends on how you want to -- you know, if you want to keep the NUKON constant and vary the cal-sil or you want to keep the cal-sil constant and vary the NUKON, --

DR. BANERJEE: Right.

MEMBER SHACK: -- you get to the same place.

DR. BANERJEE: Yes. But, you know, if you keep the NUKON constant --

MEMBER SHACK: It's the NUKON:cal-sil ratio.

DR. BANERJEE: You can get a very high pressure drop, 15.

MEMBER SHACK: Yes.

DR. BANERJEE: So that loading, fiber loading, which is going down with cal-sil constant, all you have to do is increase the cal-sil constant and you just shift the --

MEMBER SHACK: Yes, yes.

DR. BANERJEE: Right? Completely.

CHAIRMAN WALLIS: I am looking at all of these very interesting results. And I'm wondering how many more tests you would need in order to really understand what is going on to the point where you could predict something.

I'm wondering. I don't know if you want to even hazard an answer to that one.

MEMBER SHACK: I think we understand a great deal from these tests already.

CHAIRMAN WALLIS: You have learned a lot.

MEMBER SHACK: Yes.

CHAIRMAN WALLIS: You have not shown us any predictions of anything.

MEMBER SHACK: No, no.

MR. TREGONING: I think we have a pretty good conceptual model of what is happening in these environments. Actually, making predictions, again -- and I touched on this earlier. Given some of the non-linearities of the behavior, that's a much more difficult proposition. There's no doubt about it.

CHAIRMAN WALLIS: And some of the lack of repeatabilities, if it turns out that it makes a big difference how you put something in or something, then you have to --

MR. TREGONING: Yes. I wouldn't call that lack of repeatability. That's just a lack in different --

CHAIRMAN WALLIS: Well, lengthy. If you haven't controlled something --

MR. TREGONING: It's an initial value problem. So you've changed the initial value.

CHAIRMAN WALLIS: Well, maybe the way someone put stuff in is different. I mean, did you shake the ingredients in rapidly or it may be just an uncontrolled --

MR. TREGONING: No. Certainly, again, I think I might have said it earlier there are some --

CHAIRMAN WALLIS: So you have learned a great deal. You have learned a great deal. But you are still some way from having a complete set, which enables you to with confidence predict things.

MR. TREGONING: Well, I don't want to minimize the problem. This is one particular chemical interaction we saw in the TSP environment. There are other chemical interactions that are potentially important as well that we don't have as complete --

CHAIRMAN WALLIS: But if you were given a mission to by X date have a prediction tool for this problem, how many more experiments would you need? And what would you do with them?

MR. TREGONING: Are you allocating unlimited resources as well or --

CHAIRMAN WALLIS: No.

MR. TREGONING: -- as well as stipulating the --

CHAIRMAN WALLIS: Make an assessment of what it would take to get to the point where you had a predictive tool. And then we'll figure out what it costs.

MEMBER DENNING: Relative to what you have spent at Argonne already.

CHAIRMAN WALLIS: Would you need ten times as many tests or just twice as many or 100 times as many?

MR. TREGONING: The number of tests is just one consideration. I think the initial consideration is trying to evaluate, you know, what sort of capabilities are out there analytically that might be even applicable to try to apply these to this problem.

We have done some initial looking, but

it's in a very rudimentary stage at this point. So, you know, step one is understand what is occurring, develop conceptual models, provide information that can be used in an engineering sense, which I think we have been able to demonstrate that we can do that with the type of testing that has been done, not only in Argonne but I think in PNNL and LANL as well.

So actually building a model is certainly a separate step. How much that is needed for the engineering resolution of this problem is still unknown to me at this point.

MEMBER DENNING: Well, you know, I would like to say that I think it's critical. What we heard yesterday from NRR, which is really a reflection of what the industry plans to do, is an approach that, at least in this head loss area, is very empirical and done in a plant-by-plant basis but very empirical.

Without Research's ability to interpret what these various processes are, I just don't see how you're going to be able to come to regulatory decisions with a technical basis.

Questions are going to be raised that are not going to be answerable by the very empirical way that the industry plans to go about this particular piece of the process. And if you guys can't provide some model that enables one to interpret and say what the effects are, I don't think you're going to be -- I don't think that NRR's decisions will be defendable.

MR. TREGONING: Right. But, again, there is conceptual understanding. And there is actually predictive modeling. And those two can be quite a bit different in terms of the level of effort that is necessary to achieve one versus the other.

I would argue that if you did rely on empirical evidence to demonstrate the sufficiency of your modification, if it could be argued on a technical basis that you have done so in a semi-empirical deterministic, yet conservative way, that potentially I could see as a way forward.

Now, I am not trying to minimize that. That is not an easy thing to do. And the justification of conservativisms, again, it's an ongoing discussion that the staff has been having with the industry to make sure that, at least in our opinion, we feel like they are appropriately addressing some of these issues.

CHAIRMAN WALLIS: Your conceptual understanding isn't really complete until you have started to model it in some way because it may be that what you think is happening in words, when you try to describe it in terms of numbers, it turns out not to work very well.

MR. TREGONING: But, again, models by their very nature have limitations as well. So what we understand in a model may or may not be physical reality versus the limitation model.

CHAIRMAN WALLIS: Various scientists through the ages have said until you can start predicting things with numbers, you don't really understand things.

MR. TREGONING: We have done a lot of things in an engineering sense without having predictive models.

DR. BANERJEE: Yes, but I keep hearing this engineering sense all the time. What does that really mean?

MR. TREGONING: Catch word.

DR. BANERJEE: Through these ad hoc experiments, you get some numbers, you stick it in, and you hope for the best. But I think that some time later, somebody actually understands it and says, "Look, these numbers are completely wrong because the small, little factor here completely changes that answer."

So the model gives you a framework to

phrase your understanding of the physical world. Otherwise you're just talking.

MR. TREGONING: I agree, but the model is only as good as it's able to actively represent the --

DR. BANERJEE: Yes, can be improved.

CHAIRMAN WALLIS: What you should say is yes.

MR. TREGONING: Yes.

MEMBER DENNING: Well, he says yes. And then he has got to follow up.

DR. BANERJEE: I don't understand a credible design not to put it in some sort of a framework of a model. Everybody says you should do it.

CHAIRMAN WALLIS: But then you might have to confront a real reality.

DR. BANERJEE: A real reality.

CHAIRMAN WALLIS: Otherwise you might talk about it forever.

MR. LU: This is Shanlai Lu.

I think right at the beginning when we talked to the Research to establish the testing program, it wasn't intended to conduct confirmatory analysis.

CHAIRMAN WALLIS: To confirm what? You

don't have a model. How can they confirm?

MR. LU: Right. To identify major issues; for example, the chemical. This was actually, the TSP issues of it were among the issues surprising us. And so the overall testing program from our perspective, we want to have our own as a confirmatory.

And the real burden should be the burden of proof should be on the industry to prove they can handle all kinds of environments. So that is the thing we are looking for.

So we want to use these test results as our leverage or position to launch our questions so that we can ask a fair question to industry other than relying on NRC's resources to resolve and develop an entire theory and knowledge from our own limited budget.

CHAIRMAN WALLIS: Well, I think that this is the case where the word "confirmatory" is really inappropriate. These guys are doing investigative research. They're finding out new things. They're not confirming anything yet. And that is what you are up against. And it may be it can't be used for confirming anything because there is nothing to confirm.

If you have a theory or correlation or

something you're going to use, you're going to say, "Well, we'll confirm it against this," but you can't just confirm based on a lot of qualitative understanding of what has happened.

MEMBER SHACK: Can I show you some more interesting empirical --

CHAIRMAN WALLIS: Yes, please do. That would be good.

MEMBER SHACK: -- off-the-wall experimental results.

CHAIRMAN WALLIS: You're going to confirm something for us here?

MEMBER SHACK: Okay. The next environment we want to look at is the ICET-1 environment, which seems quite interesting. In the ICET environment, we were able to do the test directly. That is, we didn't have to simulate the product, although we did with the calcium chloride. In fact, it's a relatively easy chemical product to simulate.

CHAIRMAN WALLIS: This is one we don't have any quick look reports for or something?

MEMBER SHACK: You don't have any quick look reports for this.

CHAIRMAN WALLIS: Thank you.

MEMBER SHACK: The ICET-1 environment is

really much more complex. I mean, Bruce didn't go into the full description of just how complex it is to sort of understand something that isn't quite crystal and it isn't quite amorphous and we don't really understand the structure very well because, you know, it's the Heisenberg thing. Every time we look at it, we're disturbing it. So we really don't know what it is that was there before we went off and we poked it to look at it.

And so we're trying to essentially simulate that. Let me just sort of go on to our first attempts to do that and some of the practical problems that we had in doing it.

So, again, these aluminum hydroxide emulsions, colloids seem to be the principal chemical product that cause head loss from the ICET-1 environment. We didn't want to take 30 days to dissolve aluminum plates in the solutions. So we got to those concentrations by essentially trying to do it with aluminum nitrate additions but maintaining the pH and temperature conditions of ICET-1.

So what we were matching was the pH, the concentration, the temperature, and most of the chemical environment; that is, the borate and complexing. We did essentially add things that weren't in the ICET-1, the nitrate. But, you know, there's a feeling that that is not likely to lead to complexing that will muck things up, but, again, that is something that we were looking at.

We thought we were fairly successful when we did this on a lab scale, when we mixed things up in a beaker, and we sort of let them precipitate out. We get products that look like the ICET-1. We sort of get qualitative behavior that seems like ICET-1. When we measure kinematic viscosity, we get kinematic viscosity that resembles ICET-1. So we were ready to do a loop test.

Now, our loop test turned out to be compromised by non-prototypical behavior. When we tried to make our aluminum additions on this loop size scale, we didn't get the nice, clear solution that we were supposed to get until we cooled down in temperature. We sort of produced the snowfall.

And the snowfall sort of occurred. You know, we were hitting the average concentrations right. Obviously we had high local concentrations. Perhaps we lowered the local pH with the nitrate addition so that we were getting too high a concentration, too low a pH locally. And we ended up with our snowfall. And, again, you can see the initial snowfall.

Now, that's the bad news.

CHAIRMAN WALLIS: It looks like the same one you got in --

MEMBER SHACK: Yes. Well, all snow looks alike in your loop test.

If we stay at 160 degrees and we just wait a few minutes, the snow redissolves. And we're back to the clear solution. So, although we had the sort of non-prototypical behavior to begin with, we did seem to end up with a solution at 160 F. that was clear. It did have 375 ppm of aluminum. So to that extent, it was like the ICET-1 solution.

As we began to cool down, we would get increasing turbidity, again, sort of looking like the ICET-1. And I don't have a picture as we went all the way down to room temperature, but we ended up with a -- you know, it looked like a very large layer of vanilla ice cream on top of the bed when we were all done. So, you know, we got a large amount of precipitate as we cooled down to room temperature.

The interesting thing was that if you're looking at the way we got head loss, we started going up in head loss even before we began cooling down the temperature. And you'll notice that we didn't wait until steady state. And let me just sort of tell you the reason we did that was the dilute was cracking up. So we were running the test here to completion before the loop suddenly split and fell apart.

CHAIRMAN WALLIS: You were cracking the plastic?

MEMBER SHACK: We were cracking the plastic. So we --

CHAIRMAN WALLIS: Which is something that you understand.

MEMBER SHACK: Well, it was --

CHAIRMAN WALLIS: You understand crack propagation very well, I understand.

MEMBER SHACK: It was sort of a controlled gamble. We knew that the LEXAN did not take well to the sodium hydroxide environments, but we thought we were going to do this for a relatively short term.

CHAIRMAN WALLIS: Did you get any crazing of it --

MEMBER SHACK: No, we didn't get crazing.

CHAIRMAN WALLIS: You get crazing of plexiglass.

MEMBER SHACK: We got cracks inches long is what we got when we finally ended up --

MEMBER KRESS: Did it leak before it

broke?

(Laughter.)

MEMBER SHACK: We closed the system down successfully, a little water on the floor. So the test was sort of complete. I mean, in ICET-1, they have a LEXAN or a polycarbonate window that held up fine.

We think essentially the forming of the tube has residual stresses that they don't have in the window-type shape. So with the residual stresses and the environment, we had real problems.

But, at any rate, you can see that even before we begin to cool down, before there is anything visible going on here, we're getting very substantial increases in head loss.

So, again, this correlate -- and, again, it doesn't seem to correspond with what we see in the changes in the kinematic viscosity. You know, Bruce showed some data where he doubled the kinematic viscosity. When we do the measurements, we double the kinematic viscosity. But we're seeing, really, much larger --

CHAIRMAN WALLIS: Is this the NUKON on the screen here?

MEMBER SHACK: Yes, the NUKON is on the

screen.

CHAIRMAN WALLIS: It's already been laid down?

MEMBER SHACK: Yes. We can see if we look over here to the temperature thing, what we did was, you know, at room temperature, we introduced the NUKON. That essentially jumps up the head loss.

CHAIRMAN WALLIS: Okay.

MEMBER SHACK: Now we begin to heap the loop up. And, again, this is all pure water at this point. The viscosity is dropping. And so we get to 160 degrees in pure water.

You know, we have decreased the head loss because we have decreased the viscosity of the pure water. We now add our aluminum solution. And we're staying at 160 F. And there's no visible precipitate. But the head loss is going up quite dramatically.

And as we begin to cool down, again, the head loss just keeps going up as we cool down, as we cool down.

CHAIRMAN WALLIS: Why is it not in phase? It looks as if the jumps in pressure are not quite in phase with the --

MEMBER SHACK: There is a kind of a lag in the system.

CHAIRMAN WALLIS: There's a lag of something.

MEMBER SHACK: There is a lag in the system that it somehow --

CHAIRMAN WALLIS: Goes all the way around before it comes back?

MEMBER SHACK: Well, no. I don't think it's that. I think it's a chemical equilibrium, you know, that the material is changing, you know, it's agglomerating. It's doing something that we don't quite understand.

DR. BANERJEE: Perhaps even just --

MEMBER SHACK: Well, I have to go back to the lab notes as to exactly when we could begin to see visible signs. Again, at 160, we're seeing these jumps with nothing visible in the way of precipitate --

CHAIRMAN WALLIS: We're talking about in the -- pressure goes in steps, although the temperature --

DR. BANERJEE: If they are in the nanometer range, you won't be able to see them.

MEMBER SHACK: No. We do see this murkiness as we come down. By the time we get to 110, you can definitely see a difference. And, as I say, when you get even cooler, you know, you begin to pick up a large visible amount of precipitate, somewhat akin to what Bruce showed from the ICET 1.

DR. BANERJEE: You should be able to see light scattering or something.

MEMBER SHACK: Yes. Let me show you some things that will be coming up in just a little bit on that, at least some of the particles. Again, the LEXAN components were severely damaged. We had numerous axial and circumferential cracks. In fact, we had a 360-degree crack underneath the screen by the time the weekend was over.

The future testing -- and this is going to require a PVC test section, which, unfortunately, will only get to 140 F. But we also have to rethink our way of doing the aluminum additions. We're looking at using sodium aluminate, which will let us not change the pH as we add it. And we will have a better distribution header. But, again, those tests are coming up. We're still --

CHAIRMAN WALLIS: You're sort of using polycarbonates.

MEMBER SHACK: Well, LEXAN.

CHAIRMAN WALLIS: LEXAN.

MEMBER SHACK: Yes. Yes. We wanted

something clear. And the two clear plastics that are out there really are clear PVC, which is temperature-limited; and the LEXAN, which gets up to nice high temperatures but just doesn't like the sodium hydroxide in --

DR. BANERJEE: You have to do what we do in thermal hydraulics. Use a sapphire window.

MEMBER SHACK: We looked at using Pyrex. And, again, it's much heavier, much more awkward. It makes life a lot more difficult. We're going to try to live with the PVC section.

But, again, although the test was compromised, as I said, I think the results really indicate that if you have those kind of dissolved aluminum levels that they saw in ICET-1, you can get things that can strongly affect head loss.

Well, hopefully it comes out better in your handouts than it does on the screen.

CHAIRMAN WALLIS: It comes out the same.

MEMBER SHACK: Oh, it does? Okay.

MR. CARUSO: It looks like you have a movie attached to that.

MEMBER SHACK: No. What I have is a .tif that shows you a time temperature history for a four-loop plant. So you have non0isothermal histories

--

CHAIRMAN WALLIS: Oh, okay.

MEMBER SHACK: -- for four and three and ice condenser plants because what I wanted to do was to go off and compute dissolution now for a more typical history than an isothermal one. And what this shows is that if you look at the amount of aluminum that's dissolved per unit area over the 30 days, you can see that if you consider the actual histories and the ICET-1 because, as Bruce said, they did these calculations when they picked the ICET-1 environment, the ICET-1 isothermal sort of gives you the right average value, typically speaking, for the 30 days.

We also wanted to look at the amount of aluminum that dissolved during the spray time, that first period of time when you're doing the spray because you're at high temperatures.

And in a real plant, the amount of aluminum that's exposed to the spray can be very different from the amount of aluminum that's exposed or that's actually submerged for the 30 days. And so you wanted to look at both of those.

If you go through the ICET test plan, you get a survey of plants where you get some information on the amount of aluminum in various plants. and we have plant R here, which I call the six sigma plant. It's got an enormous amount of aluminum. And, you know, we'll just forget about it. It's got its own set of problems.

But if you look at the other plants -- and, again, I don't know how representative this survey is, but it would indicate that typically these plants, you know, are relatively low in amount of dissolved aluminum compared to ICET-1.

This is one test where the attempt to sort of make the average gave you a rather distorted picture of the population. And so we'll be looking at replicating the ICET-1 levels but will also be looking at considerably lower amounts of dissolved aluminum and see what that effect is.

Again, in some cases, it may be almost -- you know, the one-day total is almost like the 30-day total because the difference you have in the amount of aluminum that's exposed during the spray phase is so much larger than it is during the submerged phase that you pick up almost as much aluminum in the one day as you do in the 30 days. And, again, you have much less margin typically at one day than you do 30 days. But, again, the overall amounts of aluminum are large.

And, again, when I do these calculations, there's a number of assumptions that go in. I probably ought to just review those. I assume that there's no passidation of the aluminum surfaces.

CHAIRMAN WALLIS: This aluminum in R is scaffolding or something. It's not part of the essential --

MEMBER SHACK: Yes. It is scaffolding or something.

CHAIRMAN WALLIS: So it could be removed?

MEMBER SHACK: Yes. And I'm assuming it actually has been removed, but I have no idea.

CHAIRMAN WALLIS: Right.

MR. CARUSO: Don't the ice condenser plants have aluminum? I thought all the baskets were aluminum.

CHAIRMAN WALLIS: No.

MR. CARUSO: They're stainless? Okay.

MEMBER SHACK: Again, to go back, in ICET-1, what I've done here is taken the initial corrosion rate data; that is, the high rate data initially, and just assumed that that is always applicable. You know in ICET-1 that you passidated when you got about this much aluminum loss per unit meter.

Now, again, because we don't exactly

understand the conditions under which that passidation would occur, I didn't want to credit any passidation in the calculation here. But, even without that, I still end up with lower aluminum levels.

DR. BANERJEE: Is there any aluminum other than the scaffolding and ladders and things? Is there any other --

MR. CARUSO: Gradings.

DR. BANERJEE: Gradings?

MR. CARUSO: Gradings maybe.

MEMBER SHACK: Yes. All I know is what I read in the ICET-1 test plan, which came from the plant survey. Again, because the spray corrosions typically take place at a higher pH than normal sump, the actual corrosion rate is higher. So that there is a bump up for the sprays that I've put in here.

For some reason, that doesn't seem to happen in ICET-1 where I actually have to -- if I'm going to make the model predict the results, I have to sort of scale down the ICET-1 results. But I've just sort of kept this here for conservatism and not done this.

So the model is benchmarked only against the ICET-3 results and the labs tests for corrosion. It's conservative for ICET-1.

MR. TREGONING: You assume that all the spray stuff ends up in solution, where in ICET-1 it doesn't necessarily.

MEMBER SHACK: It's a solution.

MR. TREGONING: You could dissolve it on the plate, but then it could reform as an oxide.

MEMBER SHACK: Oxide, right.

MR. TREGONING: That might be one --

MEMBER SHACK: That might be. So there is a conservatism there.

What I wanted to talk a little bit about, again, were cal-sil dissolution tests. One of the things that we are concerned about is the amount of calcium dissolution that can actually occur. What I've mentioned is that we have tried to match the screen loading of cal-sil for the debris loading and the amount of chemical products. That changes the amount of cal-sil per unit volume, which I would argue could affect the dissolution rates; that is, because we don't scale exactly like a sump in a containment.

If we're going to maintain roughly the amount of chemical loading, we typically have somewhat more dilute solutions. And so we could get more dissolution perhaps, more rapid dissolution.

We also wanted to look at the effect of pH on the dissolution rate. And, again, one of the things that sort of simplifies this problem is that if you have concentrated solutions, as soon as you add the cal-sil to this thing, you bump up the pH because the sodium silicate which is in there just raises the pH.

So, even without a TSP addition, unless you use hydrochloric -- you know, unless you have some other way of controlling the pH, as soon as you add the cal-sil, you're going to raise the pH to some level, and the TSP only essentially does that.

Our initial dissolution tests were at relatively high concentrations. We went back and did some that are more representative concentrations, 0.5 and 1.5.

We looked at three different histories for the TSP, one where we add the TSP before we add the cal-sil. So that's essentially an instantaneous TSP dissolution. And that's certainly the most conservative.

The TSP over four hours essentially is the tech spec requirement. And reviewing the plant submittals, the nominal case seems to be that you typically have the TSP kind of dissolve over roughly a one-hour period, something like that.

Now, what I'm showing here is the normalized calcium level. That is, I sort of have done my -- so I could plot all of the data from the .5 and the 1.5 on the same graph, what I have done is normalized the calcium concentration to the amount of calcium I would get if that were fully dissolved, again, according to the thing.

So, you know, these should end up as one. What you see, the -- let's look perhaps at the square figures. This filled-in square is the 1.5 gram per liter. And the hollow square is the .5 gram per liter. And in most cases, the --

CHAIRMAN WALLIS: That plots start off high at one hour and then come down?

MEMBER SHACK: Well, there are some unique test results, shall we say. We never have anomalies. We just have unique results.

CHAIRMAN WALLIS: Unique is even more of a -- that claims there's nothing like it at all.

MR. TREGONING: I think that is an artefact of how they're determining the dissolved calcium levels in those tests. It's much less accurate early on in the test the way they're inferring what the dissolved calcium levels are.

MEMBER SHACK: We can't directly measure

the calcium because we get confounded by the cal-sil that's present. When we do the ICP, it measures everything, whether it's dissolved calcium or cal-sil particulate. So we measure the phosphate. And the missing phosphate is assumed to be taken out as calcium phosphate. And it's really the calcium level.

What we see here, at least for the one hour and the four-hour additions, there are sort of relatively small differences between the .5 and the 1.5. So the fact that we don't replicate the concentration in the volume doesn't seem to affect the relative concentration rate very much. And, again, those two are the same.

Now, if we go with the instantaneous TSP solution, which is the blue stuff, you see it does make a difference. But, again, that's a fairly non-realistic TSP addition rate. And, even in that case, if you go out an hour or so, you'll find that if you have .5 grams per liter cal-sil, you're going to be off to the 75 ppm in a few hours.

So the conclusion that we're making here is that, whatever the TSP addition rate, which we don't understand all that well, and whatever the concentration, the variability is the cal-sil concentration, which could be something or other, you can certainly use this data to estimate when you might get the cal-sil concentrations that are interesting.

And you'll remember back we had some cases. If you're adding the cal-sil properly, even for 27 ppm cal-sil, you were getting large pressure drops if you had built the bed first and added it on.

So, again, this all comes back to the notion that this is a short-term problem. This is not a chemical effect like the aluminum case, which, you know, could take days to happen. It can happen quickly.

Okay.

DR. BANERJEE: Are you going to tell us how to measure particles?

MEMBER SHACK: Yes. I guess I keep hitting the END button, instead of the NEXT button.

In our tests, of course, all the chemical product ends up on the screen. You know, the loop is designed that way. Well, the amounts of cal-sil and NUKON that we're putting on come from essentially the transport calculations that the licensees have done, but we assume all the chemical product is transported there.

Some of that will settle out. We did some initial preliminary settling tests just to get some idea of how fast this could come out of solution.

And, again, this is a fairly simple-minded test. We have a settling tower. We mix up a solution. We get a uniform mixture at T equals zero. And then we just let it all settle out.

CHAIRMAN WALLIS: If you look at the front at the top, there is also a front moving up from the bottom.

MEMBER SHACK: Yes, but we are mostly interested in the one up at the top.

CHAIRMAN WALLIS: If you can do that, then you get a better idea of what is happening. And you can predict it maybe better.

MEMBER SHACK: Well, I just want to --

CHAIRMAN WALLIS: Think about it. It's just like a glass of beer upside down, right?

MEMBER SHACK: We tried first with 300 ppm dissolved calcium so we could get lots of stuff and see it. We were trying to make it visible for ourselves.

CHAIRMAN WALLIS: There's sort of a settling front there, isn't there?

MEMBER SHACK: Yes. Well, in the real world, it's easier to see.

CHAIRMAN WALLIS: Clearer. Okay.

MEMBER SHACK: You know, there's clearly a settling front there.

CHAIRMAN WALLIS: There are probably some fines that hang around above it.

MEMBER SHACK: Well, about half of it is taken out by the front. Half of it remains behind. Yes. We decided that in a -- okay. We did the settling front measurements. And we got essentially a velocity for the settling front that's like four centimeters a minute.

We decided at more realistic concentrations, we went back to 75 ppm of calcium. There was really no settling front. This one looked much more like a uniform mixture that just slowly cleared, which, again, in a simple-minded case gives you a kind of an exponential settling model.

CHAIRMAN WALLIS: There's no way.

MEMBER SHACK: And if I took only two data points, it would fit the exponential perfectly.

CHAIRMAN WALLIS: Fit a line, too.

MEMBER SHACK: Unfortunately, I took three data points. And so it isn't perfect, but, you know, I estimate a settling rate out of that.

DR. BANERJEE: What happens if you take four?

MEMBER SHACK: Well, we did test four where we took five data points, again another big mistake. And we got four of them to go exponential. And we sort of threw out the one.

CHAIRMAN WALLIS: Usually these things settle faster when there is less concentration. You seem to get it settling faster when --

MEMBER SHACK: I think we get an agglomeration in the high concentration. You know, you couldn't see it there, but those are snowflakes that are kind of smashing together in the high concentrations.

CHAIRMAN WALLIS: They don't smash.

MEMBER SHACK: So, again, the two tests, I get something like .8 centimeters for minute for a settling velocity out of that, again, as a crude estimate of a first estimate of how this might do.

One of the other things if one is actually going to do a model of this that you would like to know is the --

CHAIRMAN WALLIS: There is a very interesting one here where it actually goes the other way and it actually leaps up and then comes down again.

MEMBER SHACK: Yes. Well, yes, right.

DR. BANERJEE: Well, is it completely quiescent or there are more --

MEMBER SHACK: Yes. The settling tower is pretty quiet, yes.

DR. BANERJEE: No thermal convention?

MEMBER SHACK: There may be some thermal. You know, we see particles going up and down. And that's presumably some sort of thermal thing that helps keep it mixed. But, again, that was only meant to get a rough idea of what the settling might look like for this.

The other thing that is interesting is to look back at some of the particle size characterization. And, again, Bruce didn't go in this. At LANL, they did some things. And I think they got like, as I recall, a three-step thing.

We see something like the three-step when we do this without deflocculating with the ultrasound. We get an order of magnitude change as we deflocculate. And what we're sort of concerned about here is that, although we haven't really done the TEM, we really think that we have sort of like the nano-sized particles they see at Los Alamos. And they're flocculated together.

I don't know how much deflocculation is

appropriate to use. You know, so exactly saying what the particle size is I think a little bit difficult to say.

DR. BANERJEE: Are these size particles or --

MEMBER SHACK: No. This is in solution, yes.

DR. BANERJEE: Okay.

CHAIRMAN WALLIS: Flocculation is very dependent upon other chemicals, isn't it? Flocculation is very dependent upon other chemicals in solutions, right?

MR. LETELLIER: I am not sure if I can answer your question, Dr. Wallis, but I did want to interject that we took solutions directly from ICET tank 1. And our particle sizes were trimodal in the range of a few nanometers to tens of nanometers. And that's what gave us evidence of colloid formation that --

DR. BANERJEE: How could you find few nanometers? What is the technique you use?

MR. LETELLIER: I am not familiar with it personally. I think it's a laser interferometry test to actually look for the scattering interference from very small objects.

CHAIRMAN WALLIS: Can you see that with a laser?

DR. BANERJEE: The problem is the wavelength is so much longer than --

MR. LETELLIER: No, not for the particle sizing. Mark showed the trimodal distributions. Let me look in our July presentation, where we actually presented that information.

DR. BANERJEE: Well, you can use interference method, but I would be surprised if you can get down to small colloid size.

MEMBER SHACK: Well, we get much larger agglomerations in our tests. And, again --

MR. LETELLIER: It is a surrogate. That's --

MEMBER SHACK: Yes, it is a surrogate. But it may also depend on how you essentially treat the stuff when you do it. I mean, with our ultrasound, we changed it by an order of magnitude. My guess is if we did some more to it, we could change it around some more.

The nice thing about the calcium phosphate was that we changed it by less than a factor of two. And so it's really a particle that kind of sits there. And you can have a little better feel for what its size was.

I think in the colloid to treat it, you're much less certain as to what the actual effective size is, probably because it's just difficult to characterize.

Okay. In summary, then, head losses with the chemical products can be greater than with an equivalent amount of cal-sil. And, again, that can be dependent on the new kind of cal-sil ratio that you're dealing with.

We get large head losses. I wouldn't say there's no difference, but since we couldn't really measure the maximum head loss, we ran out of capacity of the loop. It would be a little unfair to say.

We get large head losses, whether we get significant dissolution prior to the formation of the bed or we get dissolution after the formation of the bed. We can still end up with large head losses. You know, the time scales change a bit to get them large. And, again, the relative contribution does depend strongly on the degree loading and, again, can be highly non-linear, non-monotonic.

The cal-sil dissolution rate is not strongly dependent on the TSP dissolution rate or cal-sil concentrations. And so the important thing here is that if you have the cal-sil, you can probably get enough dissolution relatively quickly, but this is an early time kind of a problem. And, again, you can be 75 ppm for down to 0.5 GPL.

And that's it.

MEMBER DENNING: Has anybody looked at a pure flocculent bed? And is that a degree bed that one can analyze? The size, from what I'm hearing, is -- well, I'm not sure what the characteristic size is, whether it's in the few micron size versus in the nanometer size, where one is looking at the flow through that pure flocculent bed. Has anybody looked at that to see what correlations would tell you?

I mean, it's a regime that's getting into one that's dominated, I guess, by capillary actions. Have you looked at anything like that?

CHAIRMAN WALLIS: I think you get all kinds of strange things. You get under nanometer sizes.

MEMBER DENNING: Well, if you get it under nanometer --

MEMBER SHACK: For the calcium, we're at the micron size.

MEMBER DENNING: Yes.

MEMBER SHACK: It would seem like if you

truly had a cake of this stuff, that, you know, the kind of models that people use for cake filtration for kaolin would look a lot like this.

CHAIRMAN WALLIS: I would worry about its compressibility, too. I mean, if it's very fluffy and calculated and then you start to put something on there but it compresses it, then it's going to get --

MEMBER DENNING: Well, my question is, you know, we're looking at, how do you solve this very difficult problem? Well, perhaps one has to look at one limit, which is this problem of just the flocculent itself, and worry about compressibility and all of that good stuff.

CHAIRMAN WALLIS: Well, if you flocculate suspensions, you just settle them out. You get a very high void fraction. You get a very low concentration of the stuff in the sediment until it compresses or you squash it or something.

DR. BANERJEE: I think the real problem is not fluid dynamics but the squashing because we have done experiments on nanoscales, and Newtonian fluid mechanics holds.

So I don't think that there's anything really strange because at this level, unless you're looking at charges and stuff like this, which might have -- it's all wet. So we're presuming this is saturated forced media. So tension in that sense doesn't come in.

There's a compressibility which is going to be really hard for this program, I think.

MR. LETELLIER: Some of these products are actually hydrated gels, which may not compress but actually deform so that the shear stresses can actually cause them to extrude into the interstitial gaps. At least that is some preliminary thinking that we have for tackling that problem.

CHAIRMAN WALLIS: That's not nice, is it? Now, we don't --

MR. LETELLIER: That would complicate life, yes.

DR. BANERJEE: And they probably have non-Newtonian behaviors, right?

MR. LETELLIER: Perhaps or we have thought about treating this as an effective viscosity effect term, where we modified the flows, some of it.

CHAIRMAN WALLIS: None of it is actually filling the pores by extruding. Well, this is very interesting. We have to --

MEMBER SHACK: I think that is very -- you know, what you kind of have to decide is how realistic these estimates, I think, of the aluminum concentration are. You know, if you can demonstrate you're down here, you're in a different regime than you are in ICET-1 at 375

CHAIRMAN WALLIS: What are you going to take for a plant, though? I mean, if there might or might not be scaffolding in there, you have to probably legislate that no scaffolding will be left in the containment or --

MEMBER DENNING: That could very well be, yes.

DR. BANERJEE: Or not aluminum.

MR. TREGONING: Some of that is taken into account with these, the plant survey information.

MEMBER SHACK: The plant is certainly going to have to demonstrate, I think, that they are going to end up less than -- even based on our one flawed test, it doesn't look promising if you are at these kinds of levels of aluminum. How low you can go is something we have to examine yet, but --

DR. BANERJEE: Is the 30-day number a realistic number or -- I mean, there is only one thing that is a big, 5,000. So those are much lower than --

MEMBER SHACK: That is not a realistic number. That is the only number I'm sure that is not realistic.

CHAIRMAN WALLIS: I am sure it is no longer true, yes.

DR. BANERJEE: But the 375 is quite a bit --

CHAIRMAN WALLIS: That is not a plant, though. That is not a reactor.

MEMBER SHACK: That is not an ICET-1.

DR. BANERJEE: That is why I am asking. Is that a realistic number?

MEMBER DENNING: Well, I think that what Bill is saying is if you can demonstrate that is not a realistic number, that the realistic numbers are substantially smaller, then you could very well be in a different regime.

MEMBER SHACK: Right.

MEMBER DENNING: And then the question is, how clearly can you, then, take it into account?

DR. BANERJEE: You still have this colloidal --

CHAIRMAN WALLIS: I think we should stop by 3:30 so we can have a few more questions, discussion. This is very interesting, but the agency has to decide how much -- we asked you before -- of this kind of stuff do you need to do before you have enough evidence to make decisions? This research is broadening all the questions, it seems to me. They're resolving some and maybe raising some other ones.

MEMBER DENNING: Let me modify that to say technically defensible decisions because I think that this agency can make decisions. The problem is we want to make sure that they are technically defensible positions, right.

CHAIRMAN WALLIS: Well, not just to defend against critics but to defend against nature because if you put in too big a screen and then something happens, you know, there is a consequence.

DR. BANERJEE: You might get the problem solved at one end and clog the other end in the reactor.

CHAIRMAN WALLIS: Are there any other comments besides praise for the quality of the presentation we need to have at this time? Does NRR want to say anything or keep quiet for the moment?

(No response.)

CHAIRMAN WALLIS: In that case, we will take a break until a quarter to whatever the next hour is, a quarter to 4:00.

(Whereupon, the foregoing matter went off the record at 3:28 p.m. and went back on the record at

3:46 p.m.)

CHAIRMAN WALLIS: Please come back to the session. We're looking forward to hearing about tests that are being done at another latitude and longitude, PNNL and I think we're going to be quite interested in these results. I'm waiting to hear, please go ahead.

MR. KROTIUK: Just to introduce myself, I am Bill Krotiuk. And I'm with the Office of Regulatory Research. I'm breaking this presentation down into two parts. The first part I will talk about the testing that is being done and then we will have PNNL come in and discuss their testing facilities and data a little bit more. And then after that I will come back and I will talk about modeling efforts.

Okay, the initial effort of this -- of the -- let's talk about the testing first. The initial purpose of this was really to do the confirmatory testing with respect to previous testing that was done. The objectives it that -- we have some expanded objectives in the sense that we want to characterize the head loss across a model sump screen to standard insulation debris. So I'm really, at this point, talking specifically about Nukon and CalSil. But we also want to look at sensitivity to debris rated composition, to distribute of the debris in the bed, temperature and flow conditions. So get a little bit more information. The facility --

CHAIRMAN WALLIS: How about time? Some of these things seem to be time dependent?

MR. KROTIUK: Yeah, we will address time also, yes.

CHAIRMAN WALLIS: Some of these experiments have been run for quite a long time.

MR. KROTIUK: Yes, they have, yes. The facility itself with PNNL can expand on a little bit, has temperature measurement and control abilities and the ability to measure the thickness of the bed during the testing itself and post testing, the mast of the actual constituents in the bed, for instance Nukon and CalSil itself.

CHAIRMAN WALLIS: We've had a lot of discussion in the past about sort of the structure of the bed and whether there were sandwiches and whether as the Nukon and CalSil went round and round, did it end up on top or on the bottom or something. Is there some way that you can take slices of these sandwiches or something and find out just where the stuff is?

MR. KROTIUK: We have demonstrated that capability and I do not have those results yet. We're in the process of looking at those beds.

CHAIRMAN WALLIS: Well, you'd think that you could take these little whatever you want to call them, pancakes.

MR. KROTIUK: Right, right.

CHAIRMAN WALLIS: You make pancakes, right? Cut them and then you could look at them and see where the Calsil is in there and so on.

MR. KROTIUK: Yeah, we intend to do that, yes.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: But we have not completed that yet, so you're only going to see some -- we'll talk about just some quick samples of that. We don't have real data on that yet.

MR. TREGONING: I think we have some pictures of what those cross-sections look like in the PNNL presentation. So at least conceptually, you'll get an idea.

MR. KROTIUK: We want to extend this also in the long-term to coatings, head loss across coatings. So the intent is, is that we will have some coatings testing planned in the future, and ultimately, want to use the data to come up with a calculation and model. And as I indicated before, Pacific Northwest National Laboratories is primarily doing the testing.

The primary motivation to do this is the fact that the past testing has had questions regarding the head loss effects of particulates which is the Calsil, calcium silicate itself, in the presence of the Nukon fibers and we wanted to address the concerns expressed by the ACRS members regarding the forms of equation -- I'm sorry, regarding the testing, the way the testing was previously performed and we'll have some information on that.

The activity itself supports the resolution of GL 2004-02 and we ultimately want to supply the NRC with additional head loss data and some insights about variation, exactly what you're talking about, the variations within the bed, how that effects head loss. So we have divided up testing, at least in terms of the insulation testing into two series. There's a Series 1 and a Series 2. The Series 1 testing has completed already and the intent of that testing was merely -- was primarily to duplicate previous testing and do some confirmation of measurements.

This testing used a metal screen and in this testing the Nukon and CalSil debris was added simultaneously to the loop and we'll go over that a little bit more but that mimics the testing that was done in the past.

I also did some -- had some testing done on the Series 1 where I had an unblocked screen just to get a baseline for a non -- for a screen without any debris on it. On the Series 2 test, we will do testing with a perforated plate to reflect, you know, proposed new designs.

CHAIRMAN WALLIS: Have you tried to do tests with Nukon and CalSil are added at different times? It says simultaneously here.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: But have you done tests where they're added -- I think you've done tests, haven't you --

MR. KROTIUK: Yes, we have and we'll --

CHAIRMAN WALLIS: You will tell us about those, today?

MR. KROTIUK: -- we will tell you about those, yes.

The matrix that I've developed really, and I'll just briefly describe it. In the past, it appeared that most of the tests with the CalSil and the Nukon beds were done at a mass ratio of CalSil to Nukon of about a half with some done at .1 -- I'm sorry, 1 and 2, and what I plan, my matrix will try to get more points between there to get the effect of differing combinations of Calsil and Nukon.

And one of the key things that I'm trying to identify in the test itself is what I'm defining now as a particulate saturation condition and I think Rob mentioned this earlier. It's basically the condition whereby the Nukon become saturated with particulates such as CalSil and basically becomes clogged and you get an increase in pressure drop. And we believe that this is what was termed the thin bed effect but could actually happen in beds of any thickness.

As you said, a sandwich, you could have a sandwich focally or it could be homogenous.

MR. TREGONING: This could be uniform saturation within a Nukon bed or very local saturation over a thin layer potentially.

MR. KROTIUK: Yeah, that's what I mean by homogenous, right. And as indicated before, we will address -- we did some sensitivity testing that addressed variations of the CalSil itself or within the Nukon bed.

CHAIRMAN WALLIS: I presume that this saturation depends on how much the Nukon is squashed.

Maybe it's in these tests. In some of the tests, you've got a fairly high compression of the Nukon.

MR. KROTIUK: Uh-huh.

CHAIRMAN WALLIS: And so presumably the pore size available for the CalSil is now smaller.

MR. KROTIUK: Yeah, yeah, my matrix will hopefully help me identify that but I don't have the information to address that, really. And as you mentioned earlier, one of the important things, we have seen that the concentration variation can vary by the timing of the addition of Nukon and CalSil.

Currently, the status is that the testing is ongoing right now. We hope to have the insulation testing completed by April and we'll do the coating testing after that. I mean, that's sort of depending upon the time availability and funding availability. The NUREG will be -- we'll have a draft NUREG in June and a final NUREG completed in September. That's what our plan is at this point.

And at that, I will turn over the presentation to Carl Enderlin from PNNL.

MR. TREGONING: So this is a different packet now.

CHAIRMAN WALLIS: Yes.

MR. KROTIUK: And he will address the

specifics regarding the testing.

MR. ENDERLIN: I'm a Mac user, so it may take me a second here to -- okay, just to briefly go over the outline, we're going to talk about test loop capabilities and measurements. If you have questions on this, as I go through it, I have a schematic of the loop at the end, so if we may possibly try to catch your questions while we're talking about that loop. I think pretty much of that will be pretty straightforward. When we get to debris bed parameters, I'm sure that's a place where we'll start to have a lot more questions and begins to maybe introduce more material.

Pretest evaluation testing, what that's referring to is what we thought are things we needed to learn before we could run the test. I'll talk briefly about the bench mark tests that are going to be run between ANL and PNNL's loops and the benchmark cases. Then on the examples of test procedures/results, what I'm going to show is an example of the Series 1 test results and talk through how we've done the testing for there. And again, keep in mind when we're going through this that our Series 1 tests were to mimic the target values. You'll hear me do comparisons of the test. That's what went into the loop and that's how we compare those.

On the Series 1 --

CHAIRMAN WALLIS: Do you have some backup slides for the other tests which you don't have here?

MR. ENDERLIN: Excuse me?

CHAIRMAN WALLIS: Do you have some backup slides? I notice you --

MR. ENDERLIN: I have a significant number of backup slides.

CHAIRMAN WALLIS: You have 6G here but you don't have 6I and 6H.

MR. ENDERLIN: No, my understanding was that you had the Quick Look Reports.

CHAIRMAN WALLIS: We did, but they --

MR. ENDERLIN: And I have some additional backup slides, but --

CHAIRMAN WALLIS: So we would be able, if we wished to, to discuss those other tests.

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: Okay.

MR. ENDERLIN: Again, the Series 1 test, I'll show examples of data and then from those field your questions, and we can talk about the other ones. When we talk about the Series 1 test, this presentation is not completely put in a chronological order. After doing the Series 1 test, we're smarter and we've gone back to do some more pretest evaluation testing. What's important to understand is, we didn't use all of our loop capabilities to do the Series 1 test, as we attempted to follow the LANL test procedure and for the Series 2, well, we'll use perforated screen and some things will be different. We'll also change the test procedure some and I will try to explain that as we go through before that.

On post-test measurement evaluation, those are things after we retrieve the bed, are things that we do to analyze and those will include the sectioning. I can show you an example of that, some trying to assess what the composition was in the bed, taking dimensions of the bed. And I'll talk a little bit more as we go through there, but that post-measurement, some of the things you may be asking to analyze the bed, I've kind of saved for the end just to try to show what we do after we retrieve the bed. And then last, we have a few slides just to talk about issues or things that need to be cited, finalized before we finalize the Series 2 test matrix.

Okay, test loop capabilities, we have -- we consider ourselves to have two loops. We have a bench top loop which actually consists of a number of loops. One main loop that we take data in that we'll talk about. The other loops were there to help us design the large scale loop, including one of the important things that we had was to do a mass balance. What went into the loop, what's on the screen and what we retrieve. We were trying to also take care of issues to make sure that one test does not effect downstream tests by having material deposited within the loop.

This bench top look provides rapid means of investigation, so we can get answers quicker than we can look at trends, but especially some of the first data there, I'm not going to consider it as pedigreed. As we've gone on, all that instrumentation has been calibrated, so you're going to see some plots up here which I'm going to show you relative trends but I've taken the values off, just so we don't confuse actual magnitude of value versus that that's been determined in the large scale.

I will say that as the test program has gone on, we've gotten pretty good comparison but we have not yet done a true bench marking of bench scale to large scale. Again, it was used to assess how we are going to prepare the material and introduce it, repeatability issues, design questions to build a large scale loop, in handling the material and retrieving it. And again, we look at some trends as they come up, just do to the fact we can do it quicker.

Limitations, the pressure ports in the bench top are not in ideal locations. Limited ability to degas the water. So as we get up to higher pressure drops, no matter what we've done I can't put an over-pressurization on the loop to drive the gas into solution and I don't have any temperature control other than running a tube through some cool water and putting a fan on it, so the temperature rise in the loop is greater.

The large scale loop is a 6-inch diameter test section. It has a uniform cross section and the screen extends into the pipe wall just slightly. I've gotten an example of a screen assembly and I'll pass this around. What we've done here is to make sure we have no bowing or stretching of the screen. We've put it in a welded collar and that welded collar matches up to the wall of the pipe so if you look in there, there's a little bit of debris that can get into the seam. The gaskets are custom cut and I'll talk about that later but throughout the loop to try to keep any hang-up.

What we've also done is we pre-test the screens by putting a uniform load across them because in our first tests we've run up to 700 inches and we want to make sure we pre-stretch the screen and we then characterize after we pre-stretch it again to determine what kind of deflection we may be getting just due to the loading on the screen, so we're really making bed height measurements and not bowing the screen.

This it the perforated plate they're going to and it will be put in a screen assembly. I don't have one just because they were just fabricating the parts, but I brought this to show you the difference in the flow areas and the shape of it. I'll pass those around.

CHAIRMAN WALLIS: So you can get something like 30 psi or more across this screen?

MR. ENDERLIN: I could actually get greater.

CHAIRMAN WALLIS: Your capabilities are considerably higher than Argon?

DR. CHAMBERS: Yeah, I have a range of pressure transmitters and I have a high one that I can rerange.

CHAIRMAN WALLIS: So this is one reason

that you're measuring higher values than LANL is that you could.

MR. ENDERLIN: Yeah, there's a lot of differences between those tests, but, yes.

CHAIRMAN WALLIS: They had to stop when they got up to a certain value.

MR. ENDERLIN: I'm not sure if LANL ever reached their highest pressure capability. The screen is fixed in transparent polycarbonate section which I'll show a photo of here. The screen assembly is, again, as I've explained, there to reduce deflection and bowing. And our straight section of piping upstream is 20 L/D and we're in excess of 10 L/D downstream and that's to allow us to take pressure measurements, the same as we would for any component testing and have fully developed flow as we approach the screen.

Again, before I go on with this, we've been tasked and our understanding is the task is to obtain data for doing the correlation. So while we're trying to bound our test within what might exist in utilities, we're trying to make sure that we're in a very well controlled environment and not necessary mimic a LOCA.

This is pictures of the transparent test section. The collar is exactly where the screen rests, is in the same ID as the schedule 40 pipe, so there is no transition other than what little seam exists between the gasket and the pipe wall, both at the screen assembly and where this transparent section interfaces with the -- with the actual flange pipe.

So the screen sits in the middle of this and what it allows us to do is to retrieve our test section and take it for post analysis without disturbing the bed. We have shown when we pull it out of here in some of the manual height measurements that no matter what, you'll create just a little it of unroundness or effect the sides of this just because the stuff has been compressed against the walls of the pipe. So as we pull this out, we can take from the top detailed bed measurements of basically the bed in a dry state and I'll show you some additional methods we take in situ.

MR. CARUSO: Do both of these units have the same nominal opening size?

MR. ENDERLIN: They're exactly -- they've been machined --

MR. CARUSO: These two units.

MR. ENDERLIN: Oh, they have the same nominal OD but that's because the collar -- we're going to put the other --

MR. CARUSO: No, I'm talking about the whole size.

MR. ENDERLIN: No, I can tell you exactly. The one you're holding in your -- the perforated plate, is 40 percent flurry and I believe the other one is a little bit over 50. I have that data and can answer it if you --

MR. CARUSO: Is there a hole size associated with these?

MR. ENDERLIN: With which?

MR. CARUSO: Both of them. Is it the same nominal hole size?

MR. ENDERLIN: No, oh, no.

MR. CARUSO: No.

MR. KROTIUK: The perf plate is a eighth of an inch.

MR. CARUSO: Eighth of an inch, and this is --

MR. ENDERLIN: It's five mesh screens, so every five mesh wires is one inch. Go from center to center five, so it's about a fifth of an inch from center to center on the wire. I'd have to look to tell you exactly what the gauge of the wire is. It's been awhile since I --

MR. CARUSO: Okay.

MR. ENDERLIN: That's been detailed.

MR. KROTIUK: Yeah, we have that information, but not offhand.

MR. ENDERLIN: Okay, this is just showing you an example which I've passed around just because everybody else in the audience wasn't going to have ahold of those. Large scale loop capabilities, again, I'm not going to explain them all and the way we take all the measurements. Briefly, we're recording everything onto a personal computer using Daisy Lab. And we're -- the one thing I don't think I mentioned in the overheads now is we take our velocity using mass flow rates, using Coriolis meters. So we're taking into account the density and mass flow and that allows us in our bench top to keep track of when we start to see any change due to gas coming out of solution.

For our pressure drop measurements, we have an array. The idea is when we're below 150, we've always got at least two pressure transmitters on and it helps us watch if we're getting any difficulty. The zero to 2770, I mean, that's 100 psi. We've scaled that right now to 0750, so when you were asking what our total capability is, we can rearrange those.

The zero to five is mainly used for the screen and may be used if we go to even less material.

Multiple pressure ports, the specifications or function requirements when we built the loop were quite different than what we're seeing in testing now. So we have these two-inch increments that --

CHAIRMAN WALLIS: Let me ask you, either you or Argonne or both of you had consideration fluctuations on flow rate or in pressure drop or something at this -- although things were supposed to be steady, there were fluctuations. Why is that?

MR. ENDERLIN: Okay, one reason is -- several reasons you may see that. If someone does not have where you get fully developed flow -- these are things I've learned from reading your books as we say -- so but as you go downstream 10 L/Ds that's -- ASME standard is 2 L/Ds upstream, 10 L/Ds for doing valve and component. That's to allow the flow to fully develop. Okay, there will be some transition zone as the flow begins to develop leaving the bed. So as you change velocities, you can get that transition zone to change, that's one thing.

The second thing it will create is if you calculate the Reynold's number which some of these tests are being done, if you're in the range of transition flow, then you know, just any slight vibration in the building, the heating system going on, someone leaning on the pipe, you can begin to see high variations. We have not seen that high a variation in most of our tests in a large scale.

Downstairs we'll start to see it as we start to get some gas accumulation at the higher velocities and higher pressure drops and we begin to see some shedding of air bubbles or build-up on the debris bed. So there are a number of things that can explain based on fluid dynamics for that variability other than just the instrumentation.

CHAIRMAN WALLIS: Well, I don't know, I don't have it right here, but there seems to be in some tests, quite a considerable fluctuation on flow rate. I think it was your tests.

MR. ENDERLIN: Yeah, I don't know if we reported any of those.

CHAIRMAN WALLIS: Just go on. We'll take that up later.

MR. ENDERLIN: Okay.

MR. TREGONING: Were these non-planned fluctuations?

CHAIRMAN WALLIS: No, they were supposed

to be steady flow and the flow was fluctuating. It was either Argonne or yours but I don't have the right piece of paper here, so I can't tell you.

MR. ENDERLIN: If you find that and just reference the Quick Look Report.

CHAIRMAN WALLIS: Right, that's right.

MR. ENDERLIN: Okay, in situ debris height measurements, we have a method now, optical triangulation that was not in place for most of the Series 1 test. We have visual observations of test section walls, so those are in situ and we refer to the visual as manual measurements just for referencing them. So you've got optical triangulation, which I'll explain later but basically we take a photo and do post-processing. Visual observations are looking basically at the effects of the wall. We really can't see the center of the bed with any accuracy.

And then we take manual measurements; while it's still in the test section, we take detailed post-bed measurements. And I'll say right now as I'm going to compare that you are going to see some significant differences between optical triangulation which you can do the center of the bed, and the manual measurements. Okay, this is just to emphasize our operating conditions. We can pressurize the loop to

150 psi. We don't need that. That's just standard code for building, for most of the piping systems. It's 150, 250 and you move up, so we're at 150 and we're able -- we use a Argonne cover gas with an expansion tank that allows us to pressurize the system after we form the bed to put the stuff -- the gas into solution, anything that may come out as we go to higher pressure drops. The cover gas system using Argonne was just to help after we degas water to help reduce additional gas absorption.

Temperature control is 60 to 185 degrees F. The 185-degree limit is basically our polycarbonate test section. We've put it under testing but just -- it really begins to creep as you begin to go up to 200. And the main reason for the temperature control is we can alter the fluid properties, density and viscosity. Velocity range is .02 to two feet per second with the current pump. We can go lower but we have to put in a different mass flow meter and a different pump to get control on the flow. The velocity is controlled mainly using a VFDon the pump but we do have a pinch valve that we have to throttle and we use just a neoprene pinch valve so there's nothing to hold up and catch material.

Filtration system was added. It will be

more apparent when I show some of the flow history of why we have it, but a big question we had when we started this test is when I take a data point being able to report to the NRC what the mass was on the screen at that time, which is one way to do it, versus wanting to look at accumulation of the masses. We increase the velocity.

So what the filtration system allows us to do when its full capabilities are there, is that we can run -- say the bed is completely -- this is all the material we want retained on it, and at that point we can then filter out anything that's in solution. Then we can start running back to an open look system with no filtration or we have the ability to now go to a second set of filters that says, "Okay, let's begin to capture material that may be leaving the bed", and then through that we can begin to characterize for example, if we got to CalSil saturation or something, that as you go up in velocity, is the bed losing mass.

So as we look at these ramp ups and ramp downs other than bed heights which we're now showing are changing, how do we know exactly what the mass is there? And again, that's mainly for the purpose of trying to create ideal conditions for doing the correlation. And again, I believe you mentioned about reducing uncertainty. This is a way that we'd talk about reducing uncertainty, is that there's more parameters you can control or if you know, uncertainty in the mass initially is what I dumped in and what I retrieved, I have an uncertainty that it's anywhere in between some minimum bed height to higher. If I can begin to know exactly what's on the bed, it's a way of reducing uncertainty.

CHAIRMAN WALLIS: Do you, in your backup slides, have a plot of the output of the pressure transducer?

MR. ENDERLIN: No, not with me, I don't. You're talking about pressure transducer as a function of time?

CHAIRMAN WALLIS: To see how fluctuating it was, yes.

MR. ENDERLIN: Our fluctuations, again for what we'll call the pressure transducer that we're using, on a -- it's sampling at, I think, 10 hertz and recording at 1 hertz. Our fluctuation is at steady state or less than two percent change.

CHAIRMAN WALLIS: Okay, so it's not you. Somebody in what I was reviewing had a fluctuation of 50 percent or so. I was really surprised. I can't tell you who --

MALE PARTICIPANT: Your pictures don't show any transducer results.

MR. ENDERLIN: No, not at that moment. That wasn't a request to put in them at the moment. If that's a suggestion from the ACR, I'd be more than happy to find out what other things you think should be in the Quick Looks. And what we've actually done to those Quick Looks is you have the latest copies, as we've -- like bed height measurements or determine another parameter, we go back and try to add it and update the whole suite of Quick Look Reports.

DR. BANERJEE: Are these flush mounted transducers or how are they mounted?

MR. ENDERLIN: No, they're delta P transmitters and so what we're using is as we go up to higher temperature, they're below the entire test loop and so what happens is they're flushed. We have a thermal couple down at the manifold. The assumption is that all loops will heat up the same, they're not next to anything. And then they're basically a tubing running up to the pressure port and there's two ports on either side of the loop, to help evaluate against clogging and stuff.

DR. BANERJEE: So the little vortex that forms doesn't clog with --

MR. ENDERLIN: No, we -- when we first started testing, it's not debris, it was some constructions things they'd left in we had to go clean out but we actually take those apart and we check them to make sure that we're not getting plugged. Two, we have a cross valving at the transducer, so periodically during testing we make sure we check our zero. And that's our other way to make sure that our --

DR. BANERJEE: You don't have any purge or anything?

MR. ENDERLIN: No, we do. We purge into the loop to keep from drawing material in. So we periodically purge and the only thing we do there is we try to prevent that once test is going where you have to take a little bit because that purging will change the temperature from collate correction when we go to hotter temperatures. So now, I do want to stress that the temperature difference between that thermal couple and the Series 1 test was minimal. It wasn't a lot. But the Quick Look Reports, as they stand now, have not had temperature correction done, okay, between the temperature in the loop and what we have.

So as you go on in ramp ups and you look at those, there is a slight temperature gain and we are correcting for it. We just -- in those Quick Look Reports, it hasn't been done yet.

DR. BANERJEE: Where?

MR. ENDERLIN: If your loop is, let's say, warmer than the temperature of the water from the transmitter up to the port, the pressure port, I have to do a temperature correction. That has not been done in the Quick Looks yet.

DR. BANERJEE: But it's a small amount, right?

MR. ENDERLIN: It's a small amount in these tests. I just want you to know that you start to see slight changes. As we go back and review, there may be one or two real high pressure drop we ran for awhile where things began to heat up a little more.

Okay, debris hold-up, again, these are things we learned from down in our test look and I'll show you again one of the problems we had, but initially we were having a real problem with what went in, getting it all the way out and not finding it on the walls, nor visually seeing it. You know, what we've seen is that the CalSil -- you can filter it out, it's still the size I can filter, but visually, I can't determine, I have no instruments from a visual standpoint other than maybe a laser attenuation or you'll see some -- dilution is -- we have high dilution in ours and that was one thing that's different between the ANL loop, when we start talking recirculations, time at flow versus recirculation, our loop takes about twice as long to recirculate, the same velocity.

Okay, so things that we wanted to make sure that we didn't lose our debris and cut down on, certainly we want to retrieve the mass, is welded fittings versus just flange, custom cut flanges, pinch valve for throttling the flow and where we have these other legs of pipes that we go off the filtering. We've try to minimize the dead legs on those so that we don't have a long length of pipe that may be accumulating debris. We do have it so we can take the loop apart and inspect it, but it would get very costly if we were trying to do our cleaning procedure between every loop. So we've taken a look a feel we have something that's controllable now.

I would say that in a couple, the first series tests there wasn't a little construction debris or something in there. We were still working out our system at that time. And I think that's been stated in the reports where it's an issue.

Okay, so this just gives a quick schematic of the loop. The thing that we're going to talk about next that I haven't talked about is our debris injection system which I think is different than the other labs are using at the moment or was used previously. It's a vertical test section. This is just showing we've got multiple ports. The data in there, I think it states in the Quick Look Reports, while the standard is 2L/Ds upstream, we've been taking -- most of the data has been reported at 10 L/Ds. Just we get a stabler reading and if you take the average, we can't detect the difference that the velocities were at in -- you know, what is that four feet of pipe or so.

We've got a chiller unit pump and then down here we're just representing -- this filter system is actually three parallel pass; a bypass loop, a single filtration for filtering out material at the end of what you call bed formation, and then another loop that shows filtration for the purpose of while you're testing. It's just represented on here by a Y strainer. It was just our concept that we needed some form of filtration. So this is where this deviates from that.

Okay, the debris injection system, it's a closed system so what we're going to do is we're going to load material in and then we can control the flow rate at which this goes into the loop. So we're monitoring at what rate we feed into the system. What we also did in the bench scales, we were trying to determine the critical velocities for settling and resuspension to help design our loop and know what flow rates we being to get in problems when we recirculate.

CHAIRMAN WALLIS: So you introduce a slurry, debris is already mixed up with some water.

MR. ENDERLIN: Yeah, I'll talk about -- the preparation method no the bench top is very similar to what ANL and Argonne did -- LANL did and the initial -- which I'll talk about next, the debris preparation --

CHAIRMAN WALLIS: Did you have it in a beaker or something, you just pour it in?

MR. ENDERLIN: Well, I'll explain that here. So the preparation method is going to be the same using the pass. Now we create this concentrated slurry, if you will, in the blender. Okay, then we have these 160-inch lines which we pour the water into the line and then there's actually a small residual air bubble that won't leave the system that we use and we agitate the line. So we have two lines that will become more apparent in the next drawing, but two lines that we can put the Nukon in one CalSil in the other. That was the reason it was created, so we could introduce constituents separately without having them be effected in the concentrated slurry.

We put it in there and then we agitate that through the whole hose. Visual inspection is what we're relying on, that it's been mixed and we're relying on results in the bench top look that this has been able to create repeatable beds.

CHAIRMAN WALLIS: So it's pretty well mixed --

MR. ENDERLIN: In it's highly dilute --

CHAIRMAN WALLIS: -- when it goes in. It hasn't had a chance to settle out and --

MR. ENDERLIN: No, and the concentrated slurry. One reason is the concentrated slurry, from the time we mix it, it's agitated -- it doesn't allow to set. You put it in the blender, you mix it and it immediately it's being agitated continuously, goes into the hose and then the hose is agitated with a significant amount of manual agitation and we just have -- that's what interns are great for, we have them sitting there and we keep it moving until it's time to go. So, you know, at the most I would say we've never had stuff in the injection line for more than 10 to 15 minutes before we start to introduce it into the loop.

The dilution rates I put in there just because, you know, those are an issue to us if you were trying to inject the concentrated slurry and had things beginning to clump. Those come from what Bruce at LANL said, they started with as kind of a guideline which is the -- I think we took off the other numbers. It was what, 140 grams in five gallons? Is Bruce here?

CHAIRMAN WALLIS: It looks like a lot of water compared with the --

MR. ENDERLIN: Yeah, so that was his guideline, that you're always more dilute than that and that was the guideline for the Series 1 test and our initial -- again, our initial development was picking up on what LANL had learned to date. There's nothing else more critical than that, than someone showing that worked, and so we set it as a guideline. We were able to develop a system that worked to it.

Again, throttling valves are upstream of the mass flow. We originally wanted to record the mass flow rate into the system but using the Coriolis meters, even with very dilute fiber in the smaller Coriolises this material will plug. That's just an observation, but you know, a three-inch Coriolis, for example, does not have three-inch ports. Okay, so when you talk about a one-inch Coriolis, they're like three-eighths -- two parallel three-eighths inch parts through there and those will see the effects of the fiber.

DR. BANERJEE: But these are not these through-flow ends that --

MR. ENDERLIN: No, they're d-tube style micro-motion Coriolises. They're not straight tube like N --

DR. BANERJEE: Yeah, they're d-tubes, right?

MR. ENDERLIN: Yeah.

DR. BANERJEE: But they aren't full flow.

MR. ENDERLIN: No, they're parallel d-tube models. No, they're not full port, when they say a two-inch, it's two -- you know, when you say a two-inch one, I think it's two seven-eighths ones or, you know, it matters what year they created. The models are slowly getting better now. They've gone to the straight tube. So those throttling valves, if we were to report or you were to see values of water flow or velocity, that is prior -- the density there is still measuring just the straight water upstream of the material being injected. And a little more clarified show the picture of it.

So the big throttling valve is not used to control the velocity in the loop as we do additional velocity. It's there for bed formation to creates a pressure drop so that we can drive fluid into the injection loop lines. Currently, the injection loop lines we put it in at, I think, .8 meters per second. And again, if we wanted to change dilution or reduce dilution, we can change the length of those lines.

We've got three micro-motions. This is positioned -- technically, this is downstream, our micro-motion. This is just a schematic from when originally we were putting the loop together. This micro-motion now exists downstream of the screen. Okay, and the reason for this is it sees the mass flow rate immediately after the injection. The other micro-motions are there basically so we can control and repeat the water velocity going through the injection lines.

Okay, now we'll probably get into more questions here. What we're talking about here are debris bed parameters. And these are things when we started to do our pretesting of what our goals were. Again, one of the questions when we see -- when we started to see the initial data that had been done and read some of the past work, the question was, is this really randomness in the debris beds or is it just different initial conditions? You know, I bring back to some of the things, is flipping a coin really random? If you could model it if you know exactly the initial conditions, you can begin to guess a lot more of -- or predict, I don't want to say guess here. We want to predict a lot more of -- with better certainty whether it's going to show up heads or tails.

So in designing the experiments, we wanted to minimize experimental uncertainty and again, identify the parameters. We did certainty analysis ahead of time to determine which instruments we might want to improve, instrument uncertainty and then determined what the important parameters are and initial conditions so we could control them.

Again, a goal of ours is to get statistically significant results. And again, we wanted to assess the true variability of the process. When you begin to narrow down or get more control over those initial conditions, you begin to see the variability come down and say, okay, if I have these initial conditions, the debris beds are much more repeatable. Okay, I can take something that may show you 100 percent variability and bring it down to 10 percent. So identifying what is the true variability versus uncertainties in my measurements.

So that was done to design the experiments. And then once we said, okay, we've identified those parameters, we need to develop procedures so that no matter who's running the test or different conditions we can create comparable tests and we can create repeatable tests. So we're trying to drive to maximize repeatability of these.

The next bullet there is basically just -- what we have here because the other slides kind of got moved out --

CHAIRMAN WALLIS: This business of providing statistically significant results might lead to a lot of experiments if there's a great deal of variability depending upon how the stuff mixes and --

MR. ENDERLIN: Well, what we do is -- and I'm not the expert in the statistics. We have at PNL a whole group of people that do nothing but experimental statistics. And the reason I bring that up is sometimes you'll watch people do parametric tests where they're taking 20 tests to look at a curve that, look, if you guys give me four I can define the trend.

I need you to go down here and repeat a test 10 times, or I need you to repeat these three tests each five. So when we state significantly significant results, you know, when we first started out in my career, I remember a number of times you go to the statistician. They say, "We ran 100 tests. If you'd have run 75 it would give you the answer, but I need you to repeat three cases 25 times." So we're trying to get some statistical design up front in doing our experiments.

CHAIRMAN WALLIS: The problem I see is that if you look at some of your tests and compare them with LANL's tests, you have a significant difference.

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: Now, suppose you did the same tests. That tells me that there's some statistical variation if it's really the same tests.

MR. ENDERLIN: I think --

CHAIRMAN WALLIS: This is the same test and you know, if it's lab specific or loop specific or something. That's a different matter.

MR. ENDERLIN: I think what we'll show you later is that there are some different initial conditions and we really didn't test the same beds.

CHAIRMAN WALLIS: You're going to show those to us.

MR. ENDERLIN: Yes.

DR. BANERJEE: Are you also taking -- when you take these samples out and you look at them and you measure things, do you cross-section them and take some --

MR. ENDERLIN: Well, I'll get to that. We have -- the very first -- there are plans to do that and the NRC has finally directed us to do that. The very first thing was to determine what it takes to do that. And then there will be some questions of -- you know, because our plan was -- an option was to do TEM, SEM to evaluate these both for -- and I'll talk about that later. I don't want to get off on a tangent here.

DR. BANERJEE: But there is some way for you to determine that what you're doing is actually repeatable by looking directly at the samples.

MR. ENDERLIN: There's a number of things that we'll talk about here that we're doing to assess repeatability and that is one of them and we'll all have to consider together, you know, here's what we know, here's what we're proposing. I can't say we've taken it to the end that I have five samples, I can sit here and show they're all repeatable yet, but that is a plan to assess it that way.

MR. TREGONING: And just so it's clear, there's no real -- you know, within certain constrained variables, there's no necessarily right way or wrong way to conduct these tests. Certainly the tests that LANL did was within the realm of possible conditions, you know, the way these tests could be run to be somewhat representative. So what we're really trying to do in the PNNL loop are understand what conditions can lead to those differences and try to characterize those. But because there's no ASTM standardized procedures on doing this, that kind of precursor work is really necessary.

MR. ENDERLIN: Yeah, and again, you know, we didn't have to start out -- Bruce was very instrumental in helping us in the very beginning to say, here's what he's learned, so some of the initial conditions we're identifying are based on experiences learned at LANL.

I've put the last bullet in there as a

point just we hadn't mentioned and we had gotten rid of the other slides on that. But the Nukon and the CalSil we're using are the exact -- from the same vendor, they're from the same lot, shipped on the same day to Argonne and us. So that we had some control and ability to say, you know, "Here's what we're using", and if you go back to the vendors and talk about CalSil, if you have a sample of CalSil in 1970, sample of CalSil in 1980, to 1990 to 2000, or even from batch to batch, based on the fiber content and now they're using cellulose, I believe they actually used -- and Bruce can help me out here -- they actually used asbestos at one time. So they're using the -- was it calcium -- calcium silicate and what's the other constituent in there?

MEMBER SHACK: Sodium silicate.

MR. ENDERLIN: Okay, and that's the bulk of the insulation material but to give it its structural strength, that has changed over time and from a thermal standpoint, it doesn't really matter to them. But what's showing up on your bed that material can change depending on what sample --

CHAIRMAN WALLIS: They have some fibers in it, don't they?

MR. ENDERLIN: Yes, they have a fiber and that fiber content has actually changed data and per run, the volume or the mass fraction of that fiber in there can change as they do quality control to meet the structural strength requirements.

So we've talked to the vendors at length about that and determined that we should at least do these tests. How much that may vary our debris, we don't know, but we've tried to eliminate variations between Argonne and PNNL by using certified materials from the same lot same production. Okay, and the CalSil that we use is not what was used at LANL. We initially did our first assessments with material that Bruce sent us but that was in limited supply.

Okay, these are debris bed parameters. I've also defined them at times as initial conditions. These are things that we feel make a difference when you go to -- that someone would have to know to be able to say three beds are the same. Okay, mass material introduce versus mass of debris material retained on the screen. The reason that's important is for several reasons as far as what you put into the loop. The concentration at which material reaches the screen can have an effect. You know, I like to think we can all walk through the door, but if we all try to walk through the door at the same time, we can get a different build-up or blockage at that doorway.

Second of all is, this fiber as we prepare -- do any breakdown has some kind of distribution. You know, I'll say size distribution but how to characterize the fiber is somewhat difficult. If I only wait till 10 percent of it is on the screen and then I move that out, I refilter the rest. I've taken most likely the largest distribution, okay, so I can obviously, change the permeability or porosity of that bed. So knowing what went into your loop to try to determine what the distribution is in that bed is important.

Then the next thing is what total mass is on the bed, but just given the same total mass that parameter alone isn't enough to make sure you have identical beds. The sequence the debris constituents are loaded onto the screen, I'd like to hold questions on that because I'm going to talk at length about that and show you some data. But I will definitely say that has a major impact.

Debris material consistency, size and distribution, that's similar to what I talked in the first thing, but again, how much has this stuff been disrupted in LOCA, how have we blended or prepared it, what is the size, is it poly-dispersed, monodispersed, and how is it distributed, multiple constituents distributed throughout the bed? The mass ratio of the constituents in the debris bed, and then that's pretty much we're saying bed formation and then next one we have is the velocity at which you introduce it to the screen, so that's velocity history, the bed formation. We've done that two ways in the Series 1 test, so they're not all exactly repeated the same and it hasn't been quantified exactly what difference it makes.

Just so that you know, and I believe it's reported in all of them is the very first test our loop was designed to introduce the material at .2 feet per second to the bed. But Argonne was doing it at .1. On our .2 feet per second, we set a flow and maintained a pump speed. So as you built up material on the bed, you would watch your flow go down as your pressure drop across the bed increased. Almost all of those tests were beginning to end with a pressure drop on the order of .1, .9 feet per second. So we had a velocity decrease during bed n we went to the .1 feet per second to try to eliminate or alleviate settling issues, we then changed pump speed to maintain a constant velocity to the bed. So two different scenarios that can be done, and there's multiple others you can to but just, you know, the Series 1 test, there are some slight differences there.

Okay, then once your bed is formed, why it is still considered an initial condition is because every data point there, if we think of it as standing alone, if Bill goes in and says, "I'm going to compare" -- Bill Shack goes in and says, "I'm going to compare a data point done at .3 feet per second", and he's run it for an hour, that's one way of one flow history and I've only run it for 20 minutes but the other one is, he may have run it for an hour and never changed the velocity where I've cycled up and down 12 times. We've shown both the cycle. The cycle seems to have a greater impact, it's still under investigation, than the time at flow. So flow history at which you're going to compare data points is also important.

Okay, this is -- we're now talking about actually physically preparing the debris material. So I'm going to pass this around. This is what we refer to as "as received material", just as I talk and show a few pictures and you see the consistency of the bed, you'll get an idea of it. This is -- when we've had some tests in the past, I don't believe you have any data at the moment. We've actually put this into the loop to say, "Okay, this is one bounding condition relative to debris preparation". This is the material that's been heat treated by the vendor and then it's put through -- it's now put through a chipper. It used to be a leaf shredder.

Okay, so we're getting material that actually from the vendor the bag sent to Bruce was different than how they would send it to us now. So the first thing we said is, "How are we going to characterize that or if Argonne is going to run a test, how do we know we have the same material, we have the same lot number"? So what we were working was to come up with guidelines on how we could make this, make it repeatable, and what are the requirements. I mean, we can sit here and talk about the formation. Okay. Then --

CHAIRMAN WALLIS: Excuse me, you're only using 15 grounds? I mean, isn't a leaf shredder a rather big thing for 15 grounds?

MR. ENDERLIN: This is to prepare a bag of it. This is how I receive it from the vendor in huge boxes. Yeah, I'm trying to show you what the initial condition I get it in is.

DR. BANERJEE: It's a sample.

CHAIRMAN WALLIS: You make kilograms of it in this leaf shredder?

MR. ENDERLIN: No, I don't operate the leaf shredder. What happens is -- what happens is the manufacturer is going to make this in three inch or three and a half inch blankets. No one is really going to buy that in the industry. So we've come to them and said, well -- and the utilities have come to them. That was the reason it's done, "We need some material that if you went through a LOCA, we need some material". So what they're now doing is they take a set of their blankets, record what the lot number is and they go out there and say this much to get this many cubic feet for Bill and this many cubic feet for PNNL", and they make that lot.

Then they take it out to the leaf shredder and they do it and it comes in boxes. First it goes into big --

CHAIRMAN WALLIS: This goes through a leaf shredder.

MR. ENDERLIN: That one's actually been through a wood chipper. The leaf shredder -- when Bruce ordered his, they thought a leaf shredder was the best idea.

CHAIRMAN WALLIS: So it hasn't been through a blender.

MR. ENDERLIN: No, no, I'm showing it to you know as you see the pictures to say why are we doing this preparation, how are we characterizing it. So they put it through the wood chipper these days, put it in a big plastic bag, stick it in a box and ship multiple boxes to Bill and to PNNL.

DR. BANERJEE: So when you say the leaf shredder LS I think, or something, that is coming from them.

MR. ENDERLIN: That's called as received material. As we go on and you see additional information from us.

DR. BANERJEE: And VP is the vendor or something.

MR. TREGONING: That's LANL notation, LANL notation in the screen penetration report that was put out last year. They looked at two different types of process, Nukon on its ability to penetrate screens. Very coarsely processed Nukon which was processed through a leaf shredder and then more finely processed which is the BP or the blender process. Bruce, do you want to elaborate?

MR. ENDERLIN: Yeah, and we're trying to be as consistent as possible between LANL terminology, so all of our work at the moment is VP, except we have done some of the as received material. Go ahead, Bruce.

MR. LETELLIER: Just a historical note, back in the early days of the sump blockage issue, clear back to USIA 43, the very first debris surrogates were actually cubes of Nukon blankets that were cut into blocks and thrown against the screen. And they were found to have very little head loss effect but as operational events occurred and as we had surrogate data from air jet testing, we needed a different mechanism for generating large quantities of surrogate, hence, the leaf shredder that gives you flocs, anywhere from a few fibers up to several inches square of fiberglass.

If you tried to do some head loss testing using the as received material what you're holding in your hand, you will have a very difficult time forming uniform beds. And so we went to the next step of blender processing. PNNL has refined that approach to a very high degree so that they have very good repeatability. They have very well separated fibers. The next thing you could do is to actually manufacture a filter out of fiberglass and use that for head loss testing. So there's quite a range of interpretation for what is prototypical.

DR. BANERJEE: Have you thought of selling this to the chemical industry as technology for filters?

MR. LETELLIER: Well, there are obvious reasons that fiberglass is used for air and fluid filtration.

MR. ENDERLIN: What I'm going to pass around now, just so it gets around when I get to those pictures is we have done some tests with those and what I'm going to go and explain is how do we determine how to prepare the material? Okay, if someone were to say, "What's going to get to the screen", there's people working on transport, there's what really exists in a LOCA. So we needed to come up with something for Bill to work on as correlation but we want to try to bound it in what would be real reasons to test. For example, if you look at the Quick Look Report, you're going to see testing up to 750 inches. That was to match the same matrix velocity-wise that LANL did.

Our future testing will pretty much be truncated at 405 inches, because we don't need on these suction pumps to do over one atmosphere of pressure drop measurements. I mean, game's over for the most part. So what we've done is we've come up with a preparation and in open literature, what we had found is in most of tests in which they had done simulated -- and I'm not familiar with all the terminology that they use, but in using water jets and stuff to disassociate blankets of Nukon and then passing them through different transport scenarios, their claim was that most of the stuff disassociates into pretty small fiber or small clumps of fiber but the pictures that we saw after they had taken a pool, let it settled, decanted and then dried the material, this stuff was just a very uniform dispersed film on the bottom of the tank in the pictures we saw.

So based on that criteria, we thought there was a basis for disassociating the stuff more that the as received material. This would be in our test matrix, again, I want to keep remembering that the test matrix has been built to date on target, in other words, the introduction of material into the loop. Okay, so this is the thinnest bed we make based on the test matrix. So based on that, to start saying how are we going to prepare this material, we needed to bound it and come up with some requirements.

So these are the five requirements of which our debris preparation process has been based on. The fiber debris material must form a debris bed on the specified metal screen or mesh. If it's going right through all the time and a bed never forms, then, you know, that's not really what we're trying to do for the NRC. The debris bed is uniform in thickness and internally as consistent as possible in a radial direction. Again, whether someone is trying to say that's exactly what happens in the LOCA, we're trying to make sure that that pressure drop is over a uniform bed that we can well characterize versus looking at something that in the radial direction looks different or circumferentially looks different. So that became a requirement that the bed should be uniform.

MR. TREGONING: Hey, Carl, sorry.

MR. ENDERLIN: Go ahead.

MR. TREGONING: Frame of reference question; the debris bed that's being passed around do you recall what sort of pressure drop, maximum pressure drop was measured over that bed? That might be illuminating as it's being passed around.

MR. ENDERLIN: I could pass a -- if you give me a second, I can tell them a Quick Look Report that that would compare to.

MR. TREGONING: Okay.

MR. ENDERLIN: Do you want me to take a

second to do that now or just wait till the end? Okay, I have a picture. It's on the order of, I believe, 178 grams per meter squared. It's also in that test matrix that Bill has. It would be the lowest loading. And the pressure drop measurements are pretty small.

MR. TREGONING: We'll come back to it.

MR. ENDERLIN: Yeah, I think I have a slide, I can show you a similar bed when we get to the backup slides would probably be the easiest thing to do.

Okay, Requirement Number 3, the uniform debris beds formed over the range of debris loading specified by the proposed test matrix. So Bill is saying, here's a loading we want to look at. We wanted to make sure, will I be able to make the thick bed? Can I still make the thin bed? If I make the thin bed, does that mean it becomes very non-uniform at the higher loading? So we're looking for a debris preparation process that will give us very consistent beds regardless of the amount that we've loaded in the bounds of the test matrix.

CHAIRMAN WALLIS: I'm looking at say, Test 6E. You have 18.51 grams on the screen. This one says initial Nukon mass point, page 7.

MR. ENDERLIN: Okay, the first thing I want to point out is, can you hold up the six-inch screen. That's done in the bench top loop. So I don't pay any -- I don't to memory or pay any attention to the amount of grams. It's all mass loading for me. It should report what the mass loading was.

CHAIRMAN WALLIS: Well, this says initial Nukon mass .87 and the experiment was 18.

MR. TREGONING: It also gives you a mass per unit area.

MR. ENDERLIN: Yeah, the 18 grams is purity for that. The amount introduced and --

CHAIRMAN WALLIS: Where does it say --

MR. ENDERLIN: I'll get to that when we get to the backup slides but again, keep in mind mass loading is how we compare these.

CHAIRMAN WALLIS: I don't see any loading here.

MR. ENDERLIN: Are you looking at the Quick Look Reports?

CHAIRMAN WALLIS: Oh, gram per meter square?

MR. TREGONING: Right.

MR. ENDERLIN: Yeah.

CHAIRMAN WALLIS: And this is 100, so this is 15 times this. So this is a pretty thin --

MR. ENDERLIN: Yeah, that is the lowest.

MR. KROTIUK: Yeah, I've been trying to make sure that we report this stuff in mass per, say, screen surface area because otherwise, you know, you have a six-inch screen or a four-inch screen or a 10-inch screen, so this one sort of you would be able to compare them one to the other.

CHAIRMAN WALLIS: Well, this stuff is felting. It's actually pushing its way through the holes.

MR. ENDERLIN: Yes, that bed did go -- at the time we were doing that, it was fairly high pressures to retrieve the beds, you know, and that went up to fairly high velocities. We were interested at the time of would we see that rupture after forming the bed so we could report to Bill for the pressure drop measurements, not to be picking velocities in which we can't retrieve the bed, maximum velocities.

Beds that thin, I know, can go to over 150 inches and the channeling, just going off the top of my head, I think is on the order of 250 or so inches. It's not real consistent but channeling will usually happen right at the pipe wall.

CHAIRMAN WALLIS: But you've got a lot of data which is high pressure than that.

MR. ENDERLIN: Again, those are thicker beds most of those. Okay, Number 4, the debris beds generated for a given composition and target debris loading give us repeatable physical performance characteristics. So we wanted to make sure that whatever our debris preparation is set, we can control and we can produce a repeatable bed. Part of our repeatable bed was that we could go in and get repeatable head loss measurements in our bench top loop when we started the process.

Okay, and Number 5, now this says, once I have these requirements and create the bed and meet the matrix, again, as I'm going to talk later about the loading procedure, or loading sequence, we need to meet the NRC specifications for the debris bed composition to be evaluated. In other words, are we looking for the one that makes the highest pressure drop or what loading of constituents because again, our debris preparation is based on Nukon only and the debris preparation for CalSil only.

So these five requirements are if we start looking at backup slides, you'll see at the top in the title, you'll see requirement. It means those tests were done to show we met, you know, either Requirements 1 or 3 through 4 or something like that. So that's when you see the word requirements throughout, it's referring to these five and it was this guideline that we came up with our method of developing debris.

Okay, the results of the initial matrix I have some backup slides but the matrix were originally based on using dry material. Okay, and this takes a very long time to evaluate this because you have to go to the oven. So what we were looking at is what can we do if we try to do something that's more of a quick qualitative test. And again, you know, I don't know of any standard way to characterize the fiber. We tried to talk to people in industry and they couldn't give us much other than going all the way to SEM or something.

So to decrease the evaluation time, we came up with a metric in which we prepare the material, and then we pour it through a screen. It has a set size and the idea is to continue to pour so this is something you have to take a little bit of time to train your operators, but in developing the matrix, we prepare it, come up with a blender time is what we're after and it blender sensitive. That's what led to this, but we were able to show three operators can go in and repeat this test. So three different people can go in. They can run the R4 test after people have had a little bit of training. This is the way we're doing it. And they can independently get roughly the same R4 value.

CHAIRMAN WALLIS: This must depend on how much stuff you try to pull through.

MR. ENDERLIN: Well, you saw those concentration limits I had initially for putting in the loop. There is -- that concentration limit also exists to what goes into the blender. Okay, so there's a limit. When we -- if I prepare a five -- I have a one-liter blender let's just say. There's a limit to how much material you can put in there. You can only get it so concentrated before you're outside of the bounds of doing the preparation procedure.

Okay, and then the R4 values, so that you guys understand, is plus or minus one. Okay, if we're looking for an R4-11, we accept 10 to 12. Okay, and R4 value of 10 is giving us very close to within 10 percent of head loss --

CHAIRMAN WALLIS: What's an R4-11? It's the Nukon plus the water divided by the Nukon.

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: How could it be 11?

MR. ENDERLIN: There's 94 percent of that mass that's on there is water. So as I said, it's not something that we're going to go patent and sell an instrument on because a lot of what I'm measuring --

CHAIRMAN WALLIS: Then you'd pour more stuff on and more Nukon would get held up. It doesn't matter. This is not the really important stuff.

MR. ENDERLIN: Right.

DR. BANERJEE: They're just trying to characterize it.

CHAIRMAN WALLIS: Right, they're trying to characterize it.

MR. ENDERLIN: Yeah, the method has worked in the bounding range that I gave you with the five requirements to show repeatable beds that for a Nukon bed, I can make five beds and they will all be within 10 percent head loss for any given velocity.

MR. TREGONING: Yeah, the evolution LANL historically had developed methodologies and metrics so that they can insure that they got relatively uniform and repeatable beds. All we're trying to do is take that same philosophy and make it a little bit more global and portable so that any lab or any operator with any particular blender could recreate the same type of result. So that was really the genesis and the philosophy behind this precursor work.

DR. BANERJEE: If you dry these out or whatever, do they show like little fibers when they're dried out?

MR. ENDERLIN: Yes.

DR. BANERJEE: And can you measure their length?

MR. ENDERLIN: That's the question. Yeah, if we go to SEM, that would be one way to do it. The other way is to try to take a digital picture and do some -- using a software analysis. We have not done that at this time. That's something, when we're all done with the process and it's been determined acceptable, we can do a final characterization.

DR. BANERJEE: So these are -- each of these fibers is now free of its coating or whatever?

MR. ENDERLIN: No, well, the organic coating, and I'm not a chemist, will depend on the boiling, I believe.

CHAIRMAN WALLIS: I think that's already been taken off, hasn't it, before they send it to you?

MR. ENDERLIN: Well, that's heat treated.

MR. TREGONING: Again, it's heat treated

in the same way that the ICET product was where is was single sized --

CHAIRMAN WALLIS: You're doing this very meticulous repeatable preparation whereas what you get in the sump of PWR is very irrepeatable and --

MR. ENDERLIN: Yeah, again, I said our goal was to make sure we got controlled environment and --

CHAIRMAN WALLIS: If I did a test where I didn't use the same blender, I didn't use the same process, I might get quite a different answer.

MR. ENDERLIN: And I'm going to show you that and the method that you loaded it, yeah.

DR. BANERJEE: But maybe, that's what I was asking, if you could take a dried sample and measure some parameter related to that, which gives you some idea of --

CHAIRMAN WALLIS: Fiber length or meters squared per cubic meter --

DR. BANERJEE: Something.

CHAIRMAN WALLIS: -- surface or something, yeah.

DR. BANERJEE: You would know at least whether the blenders that you're using are the same or not or giving you different results. Another way, often these suspensions of fibers on the shear, do show fairly non-Newtonian behavior and you know, in a quantitative viscometer.

MR. ENDERLIN: Yeah.

DR. BANERJEE: You might be able to get something to characterize them like a shear viscosity or something like --

MR. ENDERLIN: We've done a first attempt at that. The question was, what concentration do we do it at and at the moment, since this was working, we didn't pursue that any more. We were looking at both. Well, we don't expect the particle sizer -- I mean, lots of times people say, "Well, the vendor says it won't work". It won't work for what the vendor intended it for but it will give us a metric by which to measure it. So we looked at particle sizing and we did go to a rheometer, but at the moment, messing with the concentration, I think we need to go to much higher concentrations and I didn't want to take a tangent once we found something that worked, but that is something we looked at.

DR. BANERJEE: The thing is, eventually, you're going to have to characterize those particles somehow or the fibers somehow by length or something and there are theories which for more concentration suspensions probably can relate the length of the slender body. It's not quite a slender body but on the sheared line to the shear viscosity. There's various viscoelastic parameters.

MR. ENDERLIN: Yeah, we're interested in any information and trying to come to a consensus of how we should characterize the fiber.

DR. BANERJEE: Somehow you're going to have to do it.

CHAIRMAN WALLIS: Well, you have to because, I mean, once you get down to fibers which are average length less than an eighth of an inch, you keep blending and blending and blending them, they'll probably go through the screen.

MR. ENDERLIN: Well, two things on that.

CHAIRMAN WALLIS: -- from what you started with.

MR. ENDERLIN: Yeah, I can talk to that some. So on our R4 metric we say exactly what you say is, if you see a pressure drop begin to increase with R4, continually, eventually you get to a point where you don't form a uniform bed and if you go back to the requirements, that's how our R4 was selected as we began to evaluate and say, "Okay, as I do R4 and I get finer and finer", okay, and that's one thing I bring up that the R4 that created the value that we used on the five-mesh screen, it may be that I can still get uniform beds by going to a lower R4, lower R4 is more finely blended material, because the holes are smaller.

The other thing we've seen in our very first SEM pictures we took is that -- and you can see it when you just do an R4 test, that you don't need -- particles that are less than an eighth of an inch or that screen mesh can make it. What happens is they hang up at the corners of the square and you get bridging, classic bridging. If you look at the bottom of them, it almost looks like a honeycomb beehive where you've began to get the bridging around the corners and then your high flow down the center of the port gives you a little bitty hole when you look at the bottom. It looks like you've had a bunch of cutter bees that have come to the bottom of your debris bed. So smaller fiber than just that size will start to form a bed.

MR. TREGONING: Let me try to clarify and Bruce can speak more eloquently than I on this but the blender does two things. There is some chopping that occurs, certainly, but the primary effect of the blender, at least in my opinion, is to untangle or help disassociation of the individual fibers so that when you get it in a dilute solution it's more readily separable and I think that that's probably the larger effect of the blender. Sure there's some chopping.

CHAIRMAN WALLIS: Well, this is going on too long. I mean, what I'm going to take away from this is the results and rather than all the details of how you chopped and --

MR. ENDERLIN: Okay, no, that's fine. That's --

CHAIRMAN WALLIS: The problem here is you're doing very, very sort of experiment specific experiments here because your preparation is unique. They have nothing to do with what happens in the sump. I'm not sure how you make the bridge. For the interest of doing repeatable experiments, this is fine, but what does it have to do with a sump?

DR. BANERJEE: Well, because as you develop a theory, you have to start somewhere where you --

CHAIRMAN WALLIS: I agree, but it's an awful long way from this to --

MR. TREGONING: Well, we heard earlier the importance of model development. This is a case where we're --

DR. BANERJEE: I think you have to start here.

MR. LETELLIER: The extreme uniformity gives you a chance to develop a correlation that has all of the physics that you think are important and it's repeatable enough to build your confidence.

CHAIRMAN WALLIS: Let's see how repeatable it is.

MR. LETELLIER: Assuming you can satisfy that objective, and you have a correlation --

CHAIRMAN WALLIS: You see, what's nagging me here is you're going through all this stuff and then I see data where you've gone to a factor of 10 different from LANL doing presumably the same experiment. That tells me that something is really important here. You know, I'm not sure that it's in the leaf shredder and the --

MR. ENDERLIN: Yeah, I'm just going to skip through these. This just gives you an example of received material and another R4-B and just to give you an example, the one on your right is the beds that we're testing that you've seen in Series 1 will have a consistent --

CHAIRMAN WALLIS: Somebody else may have a different standard than R4-B2.

DR. BANERJEE: R4-2D.

MR. ENDERLIN: Okay, this is data from the bench top. The difference there is our points are all within 10 percent given the same bed preparation. We're doing CalSil. The characterization for that is just the particle size distribution and it's not as sensitive to blending time like Nukon is.

Okay, just before we get to results of actual debris beds, I wanted to take a second on this. This is just the flow history. Things that we're looking at is cycling and time at flow. This is just showing examples. The number of circulations when we start to do the mass balance that there is a specified amount of time or circulations that for different conditions, different initial bed formations, they're all following this relationship, that we've got to get up here before we have 95, 90 percent of our material on the bed.

So if someone starts taking data down here, part of their history, this is going to be just additional mass showing up on the bed. Okay, you said results. Do you want to hear about the loading sequence then?

CHAIRMAN WALLIS: You tell us whatever you think is most interesting.

MR. ENDERLIN: Okay, when we're looking at the loading sequence, we began to see some difference in our Series 1 test and different from LANL. Our initial injection system looked at trying to simultaneously introduce the material. So it was thought that that may create different ways of forming the bed.

Again, LANL used a premixed material and the question is, premixing causing conglomerations of CalSil to attach to the fiber such that the CalSil is not just introduced to the bed based on velocity and passing it through the porous flow path. So in Case 1 that you're going to see here is introduction to CalSil after a Nukon debris bed has formed. So this would be the classic sandwich case with any CalSil that's being penetrated into the bed.

Introduction to Nukon and CalSil is a premixed slurry. This was to get a comparison to what LANL and Argonne were doing. Case 3, you're not going to see results off. It was an introduction to Calsil and I know there had been a question earlier. We did this two ways and never formed a uniform bed. The idea was we'd look at CalSil first with the Nukon on top but we introduced the CalSil by putting it in incrementally and putting the same loading in all at once. While we do see a difference, we're not able to get that stuff to --

CHAIRMAN WALLIS: You need a little bit of Nukon first.

MR. ENDERLIN: Yes. And the last one is what we call a time delay. And what we're doing there is after you've put the Calsil into the loop and we're talking just short delay so that some fibers can get there, they've never been in contact with the CalSil, and as this thing is recirculating, your Nukon is basically being introduced to your porous media only into the flow paths. Okay.

So if we come out here and look this is where you're going to see some significant differences.

CHAIRMAN WALLIS: Why does the premixed pre-data not go down to the same velocities as the other data, like .05? I can extrapolate it by --

MR. ENDERLIN: Okay, the reason being is again, I said flow history. There is data that does that. What I'm trying to give you is the same flow history of behind these. So these are basically ramp up three. So if I go through a cycle to form the bed and I ramp down, okay, remember that in the bench top the initial bed formation was made at .2. So I -- my system can still get the velocities higher than .2. What's happening is in the bench top loop looking at this, I can't get to that velocity because the pressure drop has gotten so high and what we're trying to see is at least the same number of cycles. Okay, I'm trying to, as much as possible avoid apples and oranges here.

CHAIRMAN WALLIS: I'm taking say the data of a premix debris of .2 which gives me 300 and I'm saying I extrapolate lineally back to .01 and I get something like 15 and I got another experiment where I've got 1,015.

MR. ENDERLIN: Correct.

CHAIRMAN WALLIS: So you're saying there's a factor of, I don't know, 100 or something difference between these different experiments?

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: Well, that's very reassuring. Leaf shredders and all that apart, this is very dramatic, isn't it, if --

MR. ENDERLIN: Yeah, well, now the other thing that's going to hop onto this is the bed height of these beds do not change much. The bulk porosity is calculated based on --

CHAIRMAN WALLIS: It doesn't change much?

MR. ENDERLIN: No, and if you think of it as --

CHAIRMAN WALLIS: CalSil must be somewhere else in the bed then.

MR. ENDERLIN: Well, if you think of the premixed, if you premix it, you're allowed to have -- if you think of a Nukon bed first, just imagine it as being a porous media. I now distribute CalSil material in voids that wouldn't have contributed to flow paths anyway. As I go to the Nukon bed first, I've created a Nukon bed, added CalSil on top. Now, if I take the time delay which are the -- and there's a slight difference between those, the red and the aqua are the same test and I'll tell you the difference of the blue in a minute but in those, essentially each layer of Nukons being added and the Calsil is only going in to plug flow paths. It's being introduced by flow.

Therefore, in essence over here I have a higher concern -- I have more CalSil to plug the flow areas than I do over here, because I've taken CalSil that would have never blocked the flow path and uniformly distributed it in there. And the percent mass retained is, I believe, within -- all these are within approximately three percent. So we've got --

CHAIRMAN WALLIS: Well, one reason the bed doesn't compress much more is you've got about the same pressure drop. It's the pressure drop that compresses the bed. So we don't --

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: -- expect it to compress all that much if it's governed by the fiberglass; is that right?

MR. ENDERLIN: Correct. The bullet case here, just to explain the difference of that, is the question was, well, what's the fiber structure of the Calsil, how much is that effecting it? So, in the blue condition here, we showed we can even get higher by this initial process of CalSil which I'm not going into great detail, but it's basically disassociating the material from the fiber in a dry matter, like a mortar and pestle. And then the fiber was removed. So the CalSil was screened. So in the blue, we removed the CalSil fiber.

CHAIRMAN WALLIS: So let's say this is LANL Test 6E, 6E2?

MR. ENDERLIN: I'm going to show you a comparison of that if you --

CHAIRMAN WALLIS: If I look at your Slide 28, the LANL at .1 has a pressure drop of something like 15 or 20 or something.

MR. ENDERLIN: Okay, is there a pointer here I can go --

CHAIRMAN WALLIS: And so the LANL's that are even lower. I mean, we're talking about a factor of 100. It's look almost like a factor of 1,000, if you take the time delay versus the LANL data.

MR. ENDERLIN: Correct, and I was going to show you that our --

CHAIRMAN WALLIS: This is kind of mind-boggling, isn't it, a factor of 1,000 difference between two experiments supposedly the same? It's almost like something out of surreal science or something.

MR. ENDERLIN: The question is, the comparison here is Condition 6E. So is the mass on LANL's bed the same as ours? Is the debris preparation the same in LANL as in --

CHAIRMAN WALLIS: But you see what I'm getting at?

MR. ENDERLIN: Oh, yeah.

CHAIRMAN WALLIS: I don't care about the -- the sump gives you whatever it gives you. And if you don't know what it's giving you, you don't know where you are between these extreme --

MR. ENDERLIN: Well, we're trying to provide data for --

CHAIRMAN WALLIS: I think it's very, very useful what you're doing but it's extraordinary.

MR. ENDERLIN: Yeah, the question is for the correlation is we can test different constituents. My point is, is --

CHAIRMAN WALLIS: How can you test the correlation when it's a factor of 1,000 difference between --

MR. ENDERLIN: Because you need to govern those parameters which are the initial conditions.

CHAIRMAN WALLIS: You need to say something more about the details of the bed, right?

MR. ENDERLIN: Yes.

MR. KROTIUK: Right, what's --

CHAIRMAN WALLIS: How can they possibly know that for the sump?

MR. KROTIUK: In order to calculate the pressure drop in a correlation, you have to know the composition of the bed and the distributions in the bed. That's what I'm --

CHAIRMAN WALLIS: I think there's a guidance out there which I read which said that it's conservative to assume it's a homogeneous -- I think

I remember reading that in NEI guidance. It doesn't sound like as if that's a very good piece of guidance, is it?

MR. ENDERLIN: I wouldn't use it.

DR. BANERJEE: Now, do you know the reason for the difference between the LANL and your data?

MR. ENDERLIN: I'm do know that we're comparing the same mass on the screen. Bill will address that.

CHAIRMAN WALLIS: Well, it says 6E and you said these are test conditions 6E. They would be loading same as LANL.

MEMBER SHACK: Well, if you extrapolate the premix debris to the right velocity, you're in the neighborhood --

CHAIRMAN WALLIS: It's the same debris.

MEMBER SHACK: -- since LANL was done with a premix test.

DR. BANERJEE: And this is not premixed?

MR. ENDERLIN: Well, this is -- I'll show you -- that's why I've taken these out of order and it's going to create more questions.

CHAIRMAN WALLIS: Well, the LANL premixed even that is quite different from your premixed.

MR. ENDERLIN: Right, but I'm not sure

that this isn't close to premixed.

CHAIRMAN WALLIS: And your 6E-2 -- well, I mean, we can go on forever. It's a value which is very much different from LANL's 6E.

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: We could go on like this forever.

MR. ENDERLIN: Because the series -- well, you can go on this forever, based on our Series 1 tests that we introduced the material by a method that we don't think was as controllable. By understanding that, I can now go in and repeat this test by knowing how to introduce material.

CHAIRMAN WALLIS: Why shouldn't I reach the conclusion that what happens in a sump with these kind of materials is completely unpredictable?

MR. ENDERLIN: Because I don't know that you've seen all the results of the transport test to tell us that we know what can get to the screen.

CHAIRMAN WALLIS: But you see what I'm getting at.

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: If you've got a factor of 1,000 difference for what looks like the same experiment, there are very few areas of science where that happens.

MR. TREGONING: Yeah, it's not the same experiment, no.

MR. ENDERLIN: Yeah, it's not the same experiment.

CHAIRMAN WALLIS: But it is. Officially, it's the same.

MR. TREGONING: No, not at all.

CHAIRMAN WALLIS: It's the same debris loading.

MR. ENDERLIN: If I asked you --

MR. TREGONING: It's the same loading, it's not the same --

CHAIRMAN WALLIS: So if you've got a correlation which only depends on debris loading, you'll be able to predict something --

MR. TREGONING: Can I --

CHAIRMAN WALLIS: -- you repeat one value not a range of values.

MR. TREGONING: Yeah, I think what we're saying and we said this from the very beginning, it's not just the function of the loading.

MR. KROTIUK: But let me say one other thing; I -- the loading, and Carl mentioned this earlier, the loading was -- of the mass going into the loop was attempted to be the same in terms of kilograms per square surface area. However, what caused -- and I have a graph to this, is that they're able to measure the amount of Nukon and CalSil on the bed, and when you look at that, the amount of CalSil that's actually in the bed is only a fraction of what is added. The Nukon is much closer.

CHAIRMAN WALLIS: That's very different from LANL. LANL measured turbidity or something and they concluded from their tables that you were up in the 90 percent of the Calsil being trapped in -- up to 99 percent, I think in some test. So they were concluding all the CalSil was trapped in the bed.

MR. KROTIUK: And that's not what we're finding from --

CHAIRMAN WALLIS: So that's another big difference between you.

MR. KROTIUK: Yes.

MR. TREGONING: Well, again, here's where the --

CHAIRMAN WALLIS: Maybe that's because you've blended it so well. You're really letting the CalSil through, aren't you? You're letting the CalSil through.

MR. TREGONING: Some CalSil does go

through, yes.

MR. KROTIUK: We're recirculating it.

CHAIRMAN WALLIS: You're recirculating it, aren't you?

MR. TREGONING: We are recirculating it.

MR. ENDERLIN: For these tests, that's what I was trying to stress --

CHAIRMAN WALLIS: Not recycling.

MR. ENDERLIN: -- for these tests we're trying to match --

CHAIRMAN WALLIS: You are recirculating it.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: So you'd expect it to get -- eventually to get trapped.

MR. TREGONING: But I don't know -- did you match the exact number of circulations that LANL did? I mean, that's --

MR. ENDERLIN: No, LANL did one -- that's why I'm thinking we're premature to be here if we don't discuss the other results.

DR. BANERJEE: Let's start from where you were before.

CHAIRMAN WALLIS: Yeah, let's go back to where you were before. That was Slide 24.

MR. ENDERLIN: I'm going ahead.

DR. BANERJEE: Give us a reason for these differences.

MR. ENDERLIN: Well, I don't know that we know them all but there are some significant ones. Okay, if we go here again, what we're looking at is all of these -- there are slight differences and additional testing has shown that we can start to get much better repeatability if we use the same initial conditions and so from these results, these are done in the bench top loop. And we're going to come back to that same slide with some additional Series 1 tests on it.

Just to speed things up, there are some bench mark tests being done at LANL, PNNL. We have three cases identified that you can read what they are there. And the main thing is that we're trying to make all the debris preparation, all the bed formation to get rid of those variables so that we compare measurements systems and the actual introduction method. So the loading sequence will be the same, but we're going to be able to determine is our two different ways of injecting the stuff into the loop, is that creating a difference in what's winding up in the bed?

Okay, these are Series 1 test results. This is an example of them. The reason 6E has become the example is because I have two cases and that was a case where we didn't get repeatability and --

CHAIRMAN WALLIS: This case 4C, this black square, very dark blue or black or something, that --

MR. ENDERLIN: Where are we?

CHAIRMAN WALLIS: On the back of 24, is that point up to 1,000. That's a point up to the 1,000, that dark blue color square.

DR. BANERJEE: That's time delay, right?

CHAIRMAN WALLIS: But that's a point which essentially said the thing became completely impermeable.

MR. ENDERLIN: Yes, that's my understanding, is we --

CHAIRMAN WALLIS: Why?

MR. ENDERLIN: -- we have a number of beds that are becoming impermeable.

CHAIRMAN WALLIS: Completely jammed up.

MR. ENDERLIN: Yes, if I pull the thing off, I get just a slow, slow drip out the bottom, when I unhook the bottom piping.

CHAIRMAN WALLIS: It's a bit self-reinforcing, because once it gets impermeable, the pressure drop goes up with squeezes it more.

MR. TREGONING: Right, so you're pressure -- again, once you've got -- once you've reached --

CHAIRMAN WALLIS: Which is well-known in certain problems of chemical engineering. You get this tick valve effect. The thing blocks --

MR. ENDERLIN: Yeah, the final 10 percent fines is what's going to take you out of business.

CHAIRMAN WALLIS: Sugar beet production amongst other things. I mean, trying to get the sugar out of the beet. You know, you can actually plug up the filter completely. But that's a very dramatic -- you stand by that data, do you? I mean, that's something that --

MR. ENDERLIN: Again, I want to stress that I stand by the relationship between the different methods and it was measured in the bench top. So again, I don't want to take the magnitudes and say they've all been verified in the large scale test loop but I stand by the premixed is going to give you pressure drops. We've repeated enough of the tests now that are going to be significantly different compared --

CHAIRMAN WALLIS: If I were in RL looking at that, I'd say what am I going to do?

MR. ENDERLIN: But you'd need to begin -- it would appear to me you'd need to again, the transport of how the material gets to the screen becomes a more important factor.

CHAIRMAN WALLIS: Can you ever predict that really?

MR. ENDERLIN: The question is can you bound it?

CHAIRMAN WALLIS: Okay, I guess we have to go on. I was trying to get to these kind of results because I think these are the most important things we're going to hear today. Okay, which one is this one?

MR. ENDERLIN: Okay, this is Slide 26. This is the 6E test and just to give you an example, of things that may be different between the LANL as we get there is our bed formation process, he formed all his beds at .1 and did you maintain velocity or did you maintain a pump speed? See, so he started at a lower velocity and maintained a pump speed, okay. When we went to .1 we maintained a constant pump speed. We allowed at least 20 recirculations, I believe, and then we had a criteria for what we have as far as what we're calling steady state. Our bed formation is tighter than what we do per data points.

CHAIRMAN WALLIS: You have shown on this figure also 6E, where you --

MR. TREGONING: That's the next one.

CHAIRMAN WALLIS: 6E, no, no, it's 6E something else.

MR. ENDERLIN: 6-2E.

CHAIRMAN WALLIS: 6-2E, yeah, 6-2E comes in at about .2 screen velocity and it zig-zags up to about 700.

MR. ENDERLIN: Yeah, I'll show you a comparison between the two.

CHAIRMAN WALLIS: It's almost constant velocity. It zig-zags up to about three times what this one is. So --

DR. BANERJEE: It's 28.

CHAIRMAN WALLIS: You have that, do you have that here? I'm sorry, I'm going ahead.

MR. TREGONING: One more.

MR. ENDERLIN: That's still 6E and LANL.

CHAIRMAN WALLIS: No, that's all right. It's just that that's supposed to be you repeating your experiment.

MR. ENDERLIN: No, no, no, no, no.

CHAIRMAN WALLIS: No, 6-2E.

MR. ENDERLIN: Correct. That's what I'm

trying to explain. Okay, on this test, if we look at -- I believe Dr. Wallis is talking about right here. This is 6E and this is 6E-2.

CHAIRMAN WALLIS: Yeah, there's quite a bit of difference.

MR. ENDERLIN: There was, based on what LANL did, some difference in the flow history to get to these data points, but what we've tried to do and the reason you're not going to see velocities down here is I was trying to explain on the previous one, is our sequence of how we took tests. And we were looking for input on that is to where you're going to compare this data if we're not getting the steady state. Do remember that if you want to compare LANL, that's the only data you should compare to LANL.

Okay, LANL did it once up and once down. Okay, so this other data, LANL -- you don't know where LANL would have gone if they had done it multiple ramp ups. Two, they have a difference in bed formation, so what would have happened to their bed formation, that's the question. As I do this, do I get more repeatable on the third ramp up?

CHAIRMAN WALLIS: Not only repeatable, they got results where they'd go along and then it would sort of jump up.

MR. ENDERLIN: And that comes down to the importance of --

CHAIRMAN WALLIS: And we don't know how -- yeah, they might have jumped up to agree with you if the cycle was --

MR. ENDERLIN: Correct, and the jump-up may just mean that they added mass to the bed.

CHAIRMAN WALLIS: Well, we don't know what it was. It just happened.

MR. ENDERLIN: Right.

DR. BANERJEE: It didn't seem like they added mass to the bed. At least they didn't say they added mass.

MR. ENDERLIN: Right, but I'm just trying to point out --

DR. BANERJEE: If they done it, it was not to the knowledge in the report.

MR. ENDERLIN: Based on the LANL results these are things we've identified that need to be controlled that when he's trying to match to a correlation, is he using the same bed height, the same mass that's in there? You know, this may become not as random or various as long as you understand how the bed is formed.

CHAIRMAN WALLIS: You've shown us 6E, 6I

the difference between LANL and you is a factor of about 20, I think.

MR. ENDERLIN: Yeah, I think it's 18 or something.

CHAIRMAN WALLIS: 18 or 20, I guessed 20.

MR. ENDERLIN: Yeah, and again, reasons for that is in our Series 1 test, okay, the way we did it, we're not going to do it the same. Okay, we need to come to a decision but the question is, did I have premix because I opened the valves exactly at the same time and it was close to premix and when I went here, I started to form a Nukon bed and then CalSil came behind it. So that's what we're saying is these little perturbations in how you form the bed.

MEMBER SHACK: Now when you say premix then, you mean, that you open the valves at the same time. You didn't sit there and mix --

MR. ENDERLIN: No, I'm saying this gave me results that are close to -- if I go in here and premix and I get repeatable tests, you know, this is a little off but I never get a premix that looks anything like that.

CHAIRMAN WALLIS: See, the problem you have here is that any test that the industry is going to do is going to supposedly predict what happens in the sump. So the industry does a test and they get a form with a green diamond, a green triangle like that. How do they know they're not going to get a black diamond in the sump, because they're not controlling what happens in the sump and they're probably not controlling what happens very well in their fairly large --

MR. ENDERLIN: Right, but do they have --

CHAIRMAN WALLIS: How do you know where you are in this? If I were an engineer, I'd say, I've got to have some completely different solution than to this problem if I'm going to predict anything.

MR. ENDERLIN: Yeah, that question I have is in the transport.

CHAIRMAN WALLIS: That's my first reaction to this. It's intolerable to have this much variation in predictability. So you have to do something else.

MR. TREGONING: The industry does, as part of the SE evaluation, need to look at a thin bed which is essentially again these particulate saturate effects in both either analysis and/or testing, depending on how they want to evaluate.

CHAIRMAN WALLIS: You have a uniform bed. You work very, very hard to have a uniform bed. They don't have anything like that. They have a vertical screen rather than a horizontal screen and all that. How are you going to apply any of this to their stuff as well?

MR. LETELLIER: That was part of the value of recommending that they assess the homogeneous bed formation complete -- with complete coverage of their design screen surface area.

CHAIRMAN WALLIS: We can study this stuff until we're all dead and never get an answer that really applies to a sump.

MR. LETELLIER: The bed formation is intentionally very uniform and very regular. There is no -- there's some evidence that beds can form that way over limited surface areas but I think that's the point of the industry testing is to demonstrate that that condition does not occur over manifold screens.

CHAIRMAN WALLIS: It's very interesting to see where they are because probably where they're going to be is way down below all this stuff, I would hope.

DR. BANERJEE: They obtain no velocity.

CHAIRMAN WALLIS: This sort of velocity? Well, I would hope that --

DR. BANERJEE: .01 or something.

CHAIRMAN WALLIS: -- it would always be

way down below all of yours. The reason you're getting these high head losses is because you're being so meticulous about the way you're doing the experiment. And, you know, that isn't going to happen in the sump except by some fluke.

MR. ENDERLIN: Right, and again, I'm trying to provide data to develop --

CHAIRMAN WALLIS: It's a completely different way of premixing the debris and mixing the debris and all that. And likely, they're going to be lower but that's only a likelihood. I don't know how much confidence I can have in that.

MR. TREGONING: The larger screen area certainly lead to a much lower approach velocities. Now, granted once you start to get clogging, those flow velocities will elevate locally.

CHAIRMAN WALLIS: Look at this though, the blue square, that disconcerting one which I know it's there because you couldn't measure anything higher but it's at the top of the graph.

MR. ENDERLIN: No, because I didn't have any --

CHAIRMAN WALLIS: That's at a very low velocity. That looks to me like .01 or less.

DR. BANERJEE: What happened there? Can

you tell us?

MR. ENDERLIN: The part, I reached -- in the bench top I reached the pump capacity. I didn't want to -- I dead-headed the pump.

CHAIRMAN WALLIS: So the pump -- the pressure drop could have been obviously higher than that.

MR. ENDERLIN: If I could increase the velocity, yeah. I'm up near the top of my range. I instruct them when they get to a certain value, they must turn off.

CHAIRMAN WALLIS: What was the flow rate?

MR. ENDERLIN: It's like .012 or something. It's next to nothing.

CHAIRMAN WALLIS: .01, okay.

MR. ENDERLIN: I mean, this thing is plugged. We don't have data there.

DR. BANERJEE: Back to that condition, what did you do different? You say time delay.

MR. ENDERLIN: No, the only -- the difference between these points and that is that I took the CalSil fiber out. Okay, the time delay, again, says that you put the CalSil into -- get a cloud of CalSil into the loop. Before it's began to recirculate even once, the Nukon gets introduced.

So you have just a time phase length.

CHAIRMAN WALLIS: To have to have something to deposit --

MR. ENDERLIN: What's that?

CHAIRMAN WALLIS: You make something to deposit the CalSil on.

MR. ENDERLIN: Yes, so that the CalSil will be forced into the porous media rather than be evenly distributed as might happen here. So that all CalSil --

DR. BANERJEE: Sorry, the Nukon bed first, you're introducing the Nukon.

MR. ENDERLIN: I'm making a Nukon bed and that Nukon bed meets my steady state requirements before I even think about mixing my Calsil and putting it in.

CHAIRMAN WALLIS: Well, let's look at then the sump. The Calsil being fine arrives first, comes with the water. It goes right through the screen and through the loop which is the reactor, right? By the time it gets back again, there's a little bit of Nukon there.

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: Something like what you did here.

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: Could be, could be.

DR. BANERJEE: Could be or they could arrive at the same time.

MS. BUSHMAN: Yeah, the relative arrival times are going to be a function not only of the density but where they're dispersed within the pool after, you know, compared to the sump screen when recirculation is initiated.

DR. BANERJEE: So when you form this Nukon which comes up the insulation, is the CalSil usually associated with the same areas so that they fall into the same area or widely disparate in terms of where it goes in the pools and things like that?

MR. TREGONING: You're asking with respect to plants, right?

DR. BANERJEE: Yeah, just an idea of what happens in a plant.

MR. TREGONING: I can't answer that. I mean, it's going to certainly be dependent on where break location is and on the specific layout of insulation where --

CHAIRMAN WALLIS: Do you see you're saying it depends on all these things which you're not going to know very well.

DR. BANERJEE: But there's a distance, right?

MR. TREGONING: Well, again, the plants know their insulation layouts very well.

CHAIRMAN WALLIS: They don't know where the break is and they don't know --

MEMBER DENNING: But they're down in that suppression pool for awhile before you go into circulation.

DR. BANERJEE: That's the point, are they sort of mixed up in the suppression -- in the pool area or are they going to be in widely different areas?

MEMBER DENNING: The pool areas are fairly similar areas. They enter the pool area in similar draining areas, you know, I mean, CalSil was wiped out by the same jet that wiped out the --

MR. ENDERLIN: Right.

MEMBER DENNING: -- primarily. There is also spraying, of course, that produces some more --

DR. BANERJEE: Well, I guess Los Alamos had some transport calculations and when you did those transport calculations, did they arrive roughly at the same time to the screens or widely disparate times?

MR. LETELLIER: You have to understand

that the transport calculations were limited to CFD computations of the fluid field and they were coupled with experimental empirical results of the transport initiation velocity. We've never actually modeled the physical tumbling of debris products and it's very dependent on where they're injected into the pool. Whether it's a spray, containment spray cascade that comes down a designed return path or whether it's placed on the floor and pushed around by the fill-up flows which are higher velocity.

CHAIRMAN WALLIS: Well, isn't it like no one's going to ever be able to predict with must confidence whether or not some CalSil gets their first or some fibers get their first?

MR. LETELLIER: That's correct, and that's one of the major reasons -- those uncertainties are the reason that the regulatory guidance demands that they assess a saturated thin bed condition, assuming that the fibers arrive first and they're dominated by a particulate loading thereafter.

CHAIRMAN WALLIS: But we had this discussion a year ago or something. We had this little piece of Calsil on the table here which as enough to block the screen if you did it that way.

MR. LETELLIER: That's correct.

DR. BANERJEE: The problem is that you can always form a thin bed which would block everything.

CHAIRMAN WALLIS: You can always hypothesize about it.

DR. BANERJEE: You know, some scenario like that one.

MR. LETELLIER: That particular piece of guidance has motivated the industry to over-design the screen areas to both reduce their velocities, which removes a head loss momentum effect and also to prevent contiguous bed formation under their design debris loadings. And the guidance --

DR. BANERJEE: Even at .01 you're getting a very high particle loss.

CHAIRMAN WALLIS: A thousand inches of water, whatever that is.

MR. LETELLIER: If the bed is contiguous and if it's saturated.

CHAIRMAN WALLIS: Yeah, 40 psi or something, what is it?

MR. ENDERLIN: 405 is an atmosphere for -- just off the top of my head I'm remembering here.

CHAIRMAN WALLIS: So it's something like 40 psi.

MR. ENDERLIN: Yeah.

CHAIRMAN WALLIS: It's out of sight in terms of MPSH and this is only .01 velocity.

MR. ENDERLIN: Just assume it's a plugged bed. I mean, I'm not going to claim that that measurement versus the velocity, I've had them -- we've had them shut it down for instrumentation reasons and pump reasons.

DR. BANERJEE: Well, if the Nukon arrives first, you've got other points at low velocities which have quite significance, almost an atmosphere at 30 feet of water.

MR. ENDERLIN: Yeah.

CHAIRMAN WALLIS: We're getting all excited over this. I just wanted to be sure that you haven't done something which is so unrealistic, it's never going to happen in the real sump.

MR. WHITNEY: Excuse me, Leon Whitney, NRR. For a moment there we were talking about actual configurations in plants, and I want to go over again what we talked about yesterday with Oconee and their pocket strainer with a design that inherently at very low velocities will tend not to form a thin bed.

CHAIRMAN WALLIS: How much do you mean by very low velocity? What do you mean by --

MR. WHITNEY: .01 or I mean .1 or so,

velocities low enough where they don't lift the fibers and the materials up to the top of the pocket. Remember the pocket strainer design? You have a chance not to form a thin bed on say one-quarter of the surface of each pocket and again, that's depending on -- you have to --

CHAIRMAN WALLIS: You don't lift the fibers up onto the screen at all?

MR. WHITNEY: Excuse me?

CHAIRMAN WALLIS: You don't get the fibers on the screen at all?

MR. WHITNEY: You get, in theory, fibers on three-quarters say of this pocket but the -- if you have a velocity low enough and the velocities are very low and there's some theory it wouldn't lift to the op of each pocket --

CHAIRMAN WALLIS: Okay, so then the rest of the screen has no fibers on it.

MR. WHITNEY: This top portion of each pocket potentially.

CHAIRMAN WALLIS: Has not fibers on it and then they --

MR. WHITNEY: And they'd have to show that through analysis. I'm just pointing out there are screen designs that have the potential to have open spaces and not have -- you know, this vertical test loop is a worse case design, recirculating through a horizontal --

CHAIRMAN WALLIS: Yes, it is, it is. In that case, we get more screen bypass and we have to think about what happens to all the stuff that's going to --

MR. WHITNEY: Then we're back to the downstream issues.

MR. ARCHITZEL: I'd like to contradict my colleague just a second here. It's sort of a similar comment but the testing that we're observing and it's similar but I wouldn't make Leon's comment about there is no thin bed. We insist, you won't get any head loss in that type of configuration where you have a significant amount of open area in that screen. So when these vendors do this testing for the thin bed condition, they test until they get a thin bed, but by the time they've gotten a thin bed over the entire screen where you have these areas like don't have near the coverage of the other areas, there are significant portions of that screen that have thicker coverage.

So that during the actual test, they need to test for the thin bed. When they've got the thin bed, they've got significant areas of the screen that have much different thicknesses and the toleration for head loss is much lower. So even in the case Leon's talking about, CCI will get a thin bed coverage in the upper portion of that screen but by the time they've got that, they've got a lot of fiber at the base of those screens.

CHAIRMAN WALLIS: That's why having a vertical screen is a much better thing than having a flat horizontal one.

MR. ARCHITZEL: Right, but they have to test the thin bed. When they get that thin bed, they've got at thin bed, but in order to get that, they've got significant areas that have much more carrying capability.

MR. TREGONING: Yeah, let's be clear. We're trying to engineer uniform, as much as we can, you know, very consistent well-deposited beds so that we can use that to provide data to look at developing these correlation models. You know, so we're trying to engineer no bypass and things like that where sump designers and licensees are trying to do exactly the opposite. So we're approaching this problem from sort of the opposite --

CHAIRMAN WALLIS: I think the problem is going to be though, since there are a lot of imponderables and uncertainties, what kind of assumptions are you going to allow in terms of conservatisms and so on, and if you're going to say you've got to have a thin bed, in fact, everywhere, it's not going to work. So you can't just way we're going to be conservative. You've got to have something that's much more realistic and then the question is, well, what's a proper judgment or how are you going to evaluate how far you are from realism and so on. It's very tricky.

MR. ENDERLIN: Yeah, I want to stress the objective. I'm in no way saying this represents LOCA. What I'm saying is, I had requirement 5 on my debris bed. The question was, can I make a repeatable bed and can I give you different scenarios. So the NRC can go back and say, "We want this test condition", and I'm saying, how you introduce the material makes a difference and so give me the scenario from a transport test that you may want to look at or the bounding conditions. We feel we can to out there and match those. So this was determined so that they can make an evaluation of what do we do for that requirement 5 and that can you ever put that material on there? Do we need to pay attention to this? Yeah, we can go way up there to high pressure drops.

CHAIRMAN WALLIS: Well, we thought there was a problem with these horizontal screens when we looked at the data about a year ago. You have simply made it more dramatic, extended our concern but it may well be that this is not typical of a sump in any way.

MR. ENDERLIN: And I feel that these tests allow me to explain the differences now created by 6E and 6E-2. If I was asked to repeat those tests, I would now be able to get repeatable tests by making sure I --

CHAIRMAN WALLIS: Why did 6E-2 go zig-zagging up though at almost constant velocity? That's a --

MR. ENDERLIN: Are you looking at the overhead or the Quick Look Reporter?

CHAIRMAN WALLIS: I'm sorry. Well, we probably have to go on. You know, there are questions of history that, you know, time, the time dependence of things that --

MR. ENDERLIN: Yeah, and that's being evaluated.

CHAIRMAN WALLIS: -- is not fully answered by this business of --

MR. ENDERLIN: Right, and our current test what we're finding is cycling is more important than the time. I mean, that's what we've seen to date so far is that it's like shaking the bag of potato chips. Every time that you compress a bed and let it go, did I break the fiber at all and then when I compress it again, did I allow CalSil to migrate down. So if I keep shaking the bag by cycling, am I pushing CalSil farther down and that's the reason, am I getting higher pressure drops due to mass being added or by a rearrangement of the bed?

MEMBER DENNING: Now, the cycling is interesting. Now, do you think that the cycling has an element of reality to the real system though? You're saying if there are shaking going on, you're thinking that's the equivalent of cycling?

MR. ENDERLIN: No, again, I'm trying to look at do we get to steady state? The NRC is going to have to guide me when they're all done saying, "Here's a condition we want to run". I'm saying I can cycle my tests. I see an effect. You can't turn around and say when we go bench mark against ANL that you can't pay attention to the flow history when you compare two points. It may be that the industry is going to turn these on, you're going to see a velocity. There's no cycling; therefore, do you look at ramp up 1, even though you made some addition, or do you do ramp up 2 and that's when you end the test?

We did these to try to say what's going to happen for the purpose of being able to defend the test and the results.

MR. TREGONING: So they're just looking at bed compressibility effects as a function of velocity differences. They're not meant to be representative in any way of anything in the plant.

MEMBER DENNING: There is the question, to be conservative would you go through a number of cycles or wouldn't you, although I'm not saying phenomenologically why you would --

MR. LETELLIER: There is no operational -- excuse me, there is no operational analogy to cycling. In fact, these beds are largely non-adherent. If they ramp the flow-down near zero it would literally fall free.

MR. ENDERLIN: Right. And again, the other task here was to be able to help define Series 2 test conditions, compare to LANL and see if we could define the differences. If we look at my biggest difference, it's in my first cycle, okay. So if I did this test and that's all I showed and did ramp up 2, we'd have a question, but when we begin to look at ramp up 2 to 3, 3 to 4 and we move on, the question is now we see that there's an importance based on building on the LANL test that we're not going to be able to just maybe do this with ramp up 1 and down and you're going to be done.

CHAIRMAN WALLIS: And in 6I, you ramped up once and caught up to very high values on the first ramp-up before it came down --

MR. ENDERLIN: Correct.

CHAIRMAN WALLIS: -- which is different from this. I think we have a real problem here and I'm trying to wrestle with it. The danger is that you'll be told that this is so unrealistic, it should be naught. It has nothing to do with sumps. But then the question is, well, what is realistic for sumps? Now, this is telling you something about how careful you have to be in order to predict anything.

MR. ENDERLIN: And what parameters the correlation must begin to include.

CHAIRMAN WALLIS: So it is valuable and the question is, what are you going to do knowing all this kind of stuff about an engineering situation?

DR. BANERJEE: That's a separate issue.

CHAIRMAN WALLIS: It's a separate issue and it's not a very easy one at all.

DR. BANERJEE: The issue here is whether

you can get reproducible data and try to understand some of the parameters which govern --

MR. ENDERLIN: Right, and the test doesn't start by saying, "Throw stuff on a screen". Someone now has to start looking and investigating, defining how does -- how will it get to the screen and what variability exists in the different ways it can get to the screen. I mean, there is an issue that if I'm making these beds with a larger fiber that never can be transported to the screen, we can eliminate those conditions.

DR. BANERJEE: Yeah, you certainly demonstrated that's important and the compressibility is important because as you cycle more and more, you get --

CHAIRMAN WALLIS: Well, the engineering solution is probably to say, we don't care how it gets to the screen. Once it gets there, we'll scrape it off or something -- something that makes the problem go away, that seems to be the way this is driving, because there's so many uncertainties about trying to predict what happens if we just have a static screen and let things develop in some natural way. You have to have some active thing which controls what happens better.

MEMBER DENNING: It's probably premature to say that, Graham, because there are other solutions, like there are ways that you can do sacrificial screens where the first few screens get clogged up and they go --

CHAIRMAN WALLIS: Okay, it's another engineering solution. It's changing the way things happen not just letting them happen but --

DR. BANERJEE: Well, in fact, to some extent having these top hats in that area, you know, some of those --

CHAIRMAN WALLIS: They'll just get clogged up and then the others ones are all right. Yeah.

DR. BANERJEE: Who knows?

CHAIRMAN WALLIS: But a lot has to be demonstrated somehow, presumably.

DR. BANERJEE: Yeah, maybe.

CHAIRMAN WALLIS: Well, it's certainly not going to be predicted from fundamentals.

DR. BANERJEE: Stranger things have been predicted.

MR. ENDERLIN: So that was an example today and my understanding is you have the Quick Look Reports. I'd be happy afterwards to discuss things with you on the Quick Looks, but --

CHAIRMAN WALLIS: The thing that -- we haven't had any of this stuff sent to us until a week or two ago from anybody, so this is the first time we've looked at any of it. Can we -- is there some way that we can be kept abreast of these things more readily than --

DR. BANERJEE: On a continuing basis maybe.

CHAIRMAN WALLIS: Well, we're not the NRC. We don't want to review it all the time but it shouldn't be such a long gap between seeing something and then seeing something else.

DR. BANERJEE: When you get so much information all together, it's really hard to digest it.

MR. TREGONING: This is relatively fresh information. I mean, this has been, you know, evolved and developed over the last month or two. So it's not like --

CHAIRMAN WALLIS: It is very fresh, so --

MR. ENDERLIN: Oh, the debris preparation sequencing was not sent to Bill Krotiuk till the end of January.

CHAIRMAN WALLIS: I didn't even know you were doing experiments at PNNL.

MR. KROTIUK: Yeah, we haven't even finalized a Quick Look Report on the sequencing study.

MR. TREGONING: I know I should have mentioned it last July at a minimum that we were planning for these tests. I believe that I did, but possibly I neglected to mention it.

CHAIRMAN WALLIS: Quite likely we didn't listen.

MR. ENDERLIN: So you're telling us we're the illegitimate child at the family reunion.

MR. TREGONING: I have a hard time believing that.

CHAIRMAN WALLIS: Well, we have so much stuff that we have to listen to. Okay, so go ahead.

MR. ENDERLIN: Okay, so now let's assume that we've retrieved a bed. These are additional measurements we take. We're taking detailed dimensions of retrieved wet rebed so I cannot stop the flow, as Bruce said, and retrieve my bed. I must continue to allow the flow to go through the bed so that I retrieve the bed when it in error.

We take -- well, as soon as we get it there, so the bed is drained. It has whatever residual water is in it and detail it. The debris bed is dried to obtain the final bed mass on the screen so we know what went in that was dry masses, that was put into the loop and we know what we retrieved on the bed and one of the things we evaluate is the percent dried material. That's for the total mass. In situ bed topography, we've taken a picture of all the test conditions, then we go in there and start to evaluate those and what we try to do is evaluate, say at when you initially make the bed and you've said the bed's formed at the end of the first ramp up and then you'll do it at the end of your last ramp-up at the end of your last ramp-down.

So you can go and analyze the data, but we're first trying to determine is there a change in the function of ramping up in velocity and is there a change in bed dimensions based no cycling. Again, these dimensions are in situ under flow. And I'll explain that in a little more detail for that method. The mass fraction of CalSil assessed by a chemical dissolution, that process is not finalized but what we're using is ion selective electrode probe for calcium and trying to evaluate by dissolving the bed after we're retrieved it.

So one issue that comes up is I can't section a bed that I'm going to assess CalSil in. But when we're all done, if I put in 200 grams, I get out

180 grams, I put 100 Nukon, 100 CalSil, by mass, my uncertainty in the CalSil is 80 to 100 grams.

CHAIRMAN WALLIS: Can't you cut cuts of pie slices and use each one for different evaluations?

MR. ENDERLIN: Well, the other option is we can use SEM to try to -- we need to determine if we can do that because of the many constituents in CalSil when we took our first example over, it just becomes somewhat costly for that analysis. This may be a cheaper analysis.

CHAIRMAN WALLIS: But even optical methods, if you section it, should tell you quite a lot.

MR. ENDERLIN: Yes, using TEM and e have some people who do diagnostic bolsman (phonetic) work on filters and stuff and they have some methods. That is an option. I'll talk about that in a minute.

So again, trying to get the mass fraction of the CalSil is basically a separation process is what we're looking at doing when we're retrieve the bed. Sectioning of the dried retrieved bed allows for transmission of electronic microscopy and scanning electron analysis, we can look at the bed void fraction as a function of height. We can look at the constituents. It can also allow a scan down the road if people are looking to model these, that direct scan using a Gray scale that they use can help generate bed models.

DR. BANERJEE: But you'd need a 3D structure.

MR. ENDERLIN: Yeah, and we can do that. Yeah, in fact, while we're talking, now this is not a bed that's been polished for SEM. This just gives you an example, okay. And these beds were beds that were sent over and there was a process had to be worked on because if you do some of the samples that way they did, you'd actually explode the bed, okay. So they had to work to get that impregnated with the epoxy and the bed has to be dried. So we know that the bed dimensions are going to change some after you pull it out and I'll show you just how radical or how much that's been under-flow and then you have to dry it before you can do this. Okay, as you dry it, there's no way to evenly dry this.

Just as if you are drying felt, you will start to get some lifting at the edges. Once that's done and they impregnate it, I can cut this thing up into very thin slices. I mean the piece that they're actually going to analyze is a very small one. But this is just an example of a bed and that's not necessarily the final process we use today for impregnating it with epoxy.

DR. BANERJEE: So you get the configuration for a dried bed. So somebody wants to do some flow through these dry beds, would have to reconstitute it in some way into a --

MR. ENDERLIN: We'd have to consider the in situ measurements because we know it's not going to have that height any more either but at least the ratio of constituents should still be preserved.

DR. BANERJEE: And probably something about the topography, what is fiber, what is particle.

MR. ENDERLIN: Yes.

DR. BANERJEE: And then it has to be reconstituted.

CHAIRMAN WALLIS: Well, can't you cut it up under water and look at it?

MR. ENDERLIN: We have tried that. Due t the fibrous nature so far, we've even -- I mean, the only way we know --

CHAIRMAN WALLIS: You don't have a sharp enough knife.

MR. ENDERLIN: You still get distortion at the part you're trying to look at. There was talk of trying to do it with a laser but that winds up creating a drying process. We've been to numerous people who make their living doing sectioning and stuff and this, they found to be a very challenging problem that they were not able to provide a successful answer to yet.

CHAIRMAN WALLIS: I guess --

MR. ENDERLIN: So if you have any, we're more than interested.

DR. BANERJEE: you would hope that some form of tomography would to it, but it's not easy to see how it's done. What's the molecular weight of this stuff? It's fairly --

MR. ENDERLIN: I believe the CalSil is on the order of -- we'll it's not a homogeneous material but I think it's on the order of 2.6 and what is the Nukon? It's fiberglass.

DR. BANERJEE: I think they have high atom numbers so they would contrast with water again in a gamma --

MR. ENDERLIN: Yes, the question is if you take the velocity off that bed and you put it in water, if I take a bed and retrieve it and I leave it in water, you wind up with oatmeal. This stuff will not stay together very well.

DR. BANERJEE: So it has to be actually

done in situ.

MR. ENDERLIN: Yes, yeah.

DR. BANERJEE: So you can't actually take it in water and --

MR. ENDERLIN: We've tried for the purpose of doing topography actually rehydrating a bed, even transporting a bed before you dry it out. We're talking extremely challenging to move this thing to the next building. If you take it and leave --

CHAIRMAN WALLIS: Well, I'm still -- as an engineer, why would I want to do all of this stuff? You're examining this as if it was the most important scientific discovery of the ages and do we really need to know all this stuff?

MR. TREGONING: Well, sectioning is -- well, if we want to try to understand what happens with these big pressure differences, understanding what the particulate distribution throughout the bed is, is incredibly critical to understand it.

DR. BANERJEE: Where the holes are and where the particles are, I guess. I mean, maybe it's not a huge effort but I think it's worth doing.

CHAIRMAN WALLIS: But you're trying to understand at the level of detail, you're never going to be able to predict in the reality.

DR. BANERJEE: It's not out of the question. I mean, you won't do it over the whole bed but for small sections of the bed, you might.

MR. TREGONING: So you're recommending --

CHAIRMAN WALLIS: So how many resources are you going to put into this effort to get a perfect solution to this intractable problem?

DR. BANERJEE: Whether you use a clever design or not, you know, for example, these rocket cleaners or whatever they're called, you really do have to calculate what's happening if you're bypassing or whatever. You're going to have to be able to do that calculation, so you're going to have to be able to see where those fibers are going in rough terms at least; otherwise there are going to be arguments like the one that's going on. Is there going to be a thin film, is there not going to be a thin film? Is there going to be particles?

I think you can do some calculations at least to support those arguments. At the moment, they're all hand waving, you know, and the parameter space is so large that you're going to eventually have to do some calculations.

CHAIRMAN WALLIS: I would go back and try to change the problem, but anyway, let's move on here.

DR. BANERJEE: That's a separate issue. Throw the water away, put some new water in.

CHAIRMAN WALLIS: Yeah. Anyway, let's move on.

MR. ENDERLIN: So, to take a topography of the bed in situ, to take dimensions, we basically use a optical triangulation, put a known grid of lines on it that's been calibrated against a standard. Take the digital picture which we can analyze, post-test. These are some measurements that we got. The BF stands for nearing bed formation, so that's not a complete made bed. It was -- this was the first time -- we'd done shake-down testing. This was basically the first official test and so as an example, what we've tried to show is just what the height of the rim is, initially, after material gets -- before we've increased the flow or we've left it there fore very long a period.

So as you can see on the rim at bed formation, we've got something that's .635 inches. Now, Ramp up 1 says --

CHAIRMAN WALLIS: RU 1 is Ramp up 1.

MR. ENDERLIN: This is -- the value here is the velocity in feet per second. So at bed formation, I'm at .18 feet per second at the time the picture was taken, .98, .96 and .05 feet per second. So this tells you your velocity and this tells you the sequence at which we ran the test. So this is Ramp up 1.

You've gone up -- that previous test you've gone through the velocity sequence once. Okay, the body of the center of the bed went from .307 to .055 inches so it compressed quite a bit. Then when we cycle it four time and compare at the same velocity only a flow history where we cycled it four times, we see that the rim is .281 and the body center is .04.

CHAIRMAN WALLIS: Is smaller, yeah. Then when you come back, it springs back?

MR. ENDERLIN: Yes.

DR. BANERJEE: But not all the way, right?

MR. ENDERLIN: Well, I don't have the comparison data here for this test. That hasn't been analyzed at the moment.

CHAIRMAN WALLIS: Oh.

MR. ENDERLIN: I can't tell you at the exact time I made the bed, when I culled the bed.

CHAIRMAN WALLIS: This is the same test and then you've backed off of -- RD4 means Ramp down 4.

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: And you've gone down to a low velocity and it sprung back to .129 from .04 at one time.

MR. ENDERLIN: Right.

DR. BANERJEE: Well, from .307 at a low velocity.

MR. ENDERLIN: But don't consider that we assume the bed was completely made at .307. This occurs right here when the material first hits. At the time of flow it would sit there at compress.

CHAIRMAN WALLIS: Well, it seems to me never completely made. You can keep cycling and it still changes a little bit each time, doesn't it?

MR. ENDERLIN: Correct.

CHAIRMAN WALLIS: It's never completely made.

MR. ENDERLIN: Well, that's the question. Is material passing through, are we changing the structure of the bed? And from this we can take the volume of the bed, which helps us get a porosity for these correlations.

CHAIRMAN WALLIS: Now, lots of your Quick Look Reports don't report thicknesses very much, do they?

MR. ENDERLIN: They report manual

measurements. We've done a test of 1A which the Quick Look has just been sent to Bill Krotiuk. It was a repeat test. That is what these measurements are from and the head loss measurements in the first two ramp downs -- ramp up and ramp downs were on the order of .2 to .3 percent difference in head loss. When we went to the higher -- to follow-on ramp ups and ramp downs, we began to deviate to differences on the order of 12 percent. If we take all our data points, the median and the mean percent difference between 1A and 1A repeat was on the order of five to six percent and that Quick Look the NRC will be passing that to you after Bill Krotiuk has had a chance to review it some time.

Okay, prior to doing Series 2 which are tests that at the moment my understanding is to go to the perforated plate and to be looking at lower velocities. This is more looking towards what the utility solutions are so that we're getting data for Bill's test matrix. Issues that we have to determine are debris loading sequence, procedure to be used, selected to find how we're going to put the material in there, how are we expecting -- you know, what debris bed does he want to evaluate there.

What we've tried to show is, yes, it makes a difference. These are the parameters that make a difference. Yes, we can control it, and we can get repeatable data. Now tell us what do you want to test.

The velocity sequence procedure gearing to be selected, defined. Okay, the test we did was to try -- when we went through the first ramp-up and ramp-down, we obviously saw most of the tests were well over pressures of interest. We don't expect in Series 2 to be going over an atmosphere pressure and the question is, what are we going to use for comparable data. If we compare to ANL, are we going to second ramp up? Do we need to do as many incremental velocity, since the trend seems to be the same? So the sequence of the test needs to be determined.

CHAIRMAN WALLIS: It will be interesting to see if somebody following your procedures with leaf choppers and blenders and all of that kind of thing independently from somewhere else all together --

MR. ENDERLIN: We're going to do that.

CHAIRMAN WALLIS: -- could duplicate your results.

MR. ENDERLIN: ANL and --

CHAIRMAN WALLIS: They're going to do

that?

MR. ENDERLIN: Yeah.

CHAIRMAN WALLIS: They're going to use the same blender with the same sharpened blades and --

MR. ENDERLIN: No, they're going to use the same R4 metric.

CHAIRMAN WALLIS: Oh, the same R4 metric, okay.

MR. TREGONING: That's one of the purposes of the bench mark test, the overlap test.

MR. ENDERLIN: In summary, the Series 1 tests are completed. They used the five-mesh screen. The purposes were to use them for learning the results we might expect and to evaluate these additional parameters, identify since the LANL tests were completed, and to get comparison results to LANL -- results to compare to LANL to determine if we could understand if they were the same or if we could explain the differences.

Results obtained to date, the debris preparation and the sequence that the debris arrives or is loaded on the screen strongly influenced the head loss. Additional investigation is currently in progress. The next large scale testing is the bench mark test to be conducted by NANL and PNNL using the same test plan. Series 2 test, the test matrix is currently preliminary. It includes the bench mark, the three cases in the bench mark test. It will focus on perforated plate and lower approach velocity.

I turn it back over to --

CHAIRMAN WALLIS: Did you guys and Argonne have a plan which said by some date you would have a predictive method or is this completely open-ended research with no time line at all?

MR. KROTIUK: That was what I was going to talk to.

CHAIRMAN WALLIS: Okay, thank you.

MR. KROTIUK: The next topic is head loss modeling.

CHAIRMAN WALLIS: I think we ought to stick with it and try to close up before too late.

MR. KROTIUK: I'll try to accelerate a little bit.

CHAIRMAN WALLIS: Fine, because I looked at all your equations in your report and I thought this is very interesting but given the data --

MR. KROTIUK: Yeah, I was going to try to address that but you know --

MR. TREGONING: So we're back to slide 9 of the previous package, just to keep track.

CHAIRMAN WALLIS: Now you see you've got this to conservatively predict.

MR. KROTIUK: Well, let me explain what my thoughts are and you may have some inputs in that but I'll tell you what I'm thinking as I'm going along. But I will try to accelerate a little bit. I don't think I necessarily have to go through this. I'll just --

CHAIRMAN WALLIS: But you say your objective is to conservatively predict.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: Does that mean only the worst case?

MR. KROTIUK: That's -- when Carl was talking a moment ago about what we would be testing, in other words, how would we build the bed, that was one of the things and we haven't come to a conclusion how we're going to do that. However, I have some thoughts and I'll just go over it quickly on how to be able to put in a model prediction of a conservative limit but that will have to be reviewed.

I'll skip this one, it's just motivation. Okay, yeah, I'll start here. Basically, what I've done is that I want to base the amounts on the classical form of the porous media flow equation. And basically, I'm working on two models. One is a one -- has a one control volume model for the entire bed. So this is essentially equivalent to what was previously done in the 6224 correlation and it's assuming everything is, as I said, homogeneous. The second approach that I'm looking at using a bed that has two control volumes and I could look at the concentration. Say we have a Nukon CalSil bed. I would be able to look and specify what the concentration is of say CalSil and a portion of it and it could be different than another part and then try to use that to come up with a predictive tool.

CHAIRMAN WALLIS: You're going to be able to do an experiment to verify that?

MR. KROTIUK: Well, what I was hoping to do and this is why I asked Carl for these tests that were done with the sequencing, the timing of the CalSil and the Nukon and addition to the bed. I want him to -- I have given him instructions to come up with the sectioning of those so I could see what the distribution is and try to get some insight into that.

CHAIRMAN WALLIS: It looks from his data that if you get the worst case, the layer which is almost impermeable --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- then the rest of the bed is irrelevant.

MR. KROTIUK: That's true but some of the thoughts in the back of my mind is, is that as you were saying, is that how realistic is their case and should we be looking at a case that is conservative but more realistic.

CHAIRMAN WALLIS: Well, let's think about it. This thin bed effect was discovered as a result of analyzing an event in a PWR, wasn't it?

MR. KROTIUK: I'm not -- was it a PWR?

CHAIRMAN WALLIS: Yeah, they were BWR. Nothing significant happened in the PWR in terms of this sort of problem as I understand. These events were in BWR.

MR. KROTIUK: Right, okay.

CHAIRMAN WALLIS: And the pressure drop was higher than expected. This is because of the sludge and the torres (phonetic) or something which actually made a thin -- but this is where the thin bed idea came from, isn't it?

DR. BANERJEE: And the screens were bent or something.

CHAIRMAN WALLIS: We have at least precedent in a real system of finding a thin bed effect.

MR. KROTIUK: Right, and I'm not -- the only thing I'm trying to say is that as I said before, it's a phenomena but it's not --

CHAIRMAN WALLIS: In terms of was it realistic or not, we have already seen --

MR. KROTIUK: Yes, yes, and I'm not denying that.

CHAIRMAN WALLIS: All right, so that's pause for thought, too.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: You can't just dismiss it as being --

MR. KROTIUK: No, I am not dismissing it and I'm just telling you what -- what I was eluding to was my thought process.

CHAIRMAN WALLIS: When I say "you" I mean, a person -- one cannot.

MR. KROTIUK: Okay, one cannot. Okay. My plans at this point is to try to finish the derivation and development of a model by June with a final publication in September. That's what my plans are. Okay, let me just go a little bit about the development of the model. And basically I used -- it's described more --

CHAIRMAN WALLIS: This is -- the LANL work uses a slightly different one which is for fibers rather than particles, I think.

MR. KROTIUK: Well, actually the model that I'm using is applicable to fiber cylinders or particles. And maybe I'll just skip to the -- just to show it in this term.

CHAIRMAN WALLIS: The power is slightly different in the fibers case.

MR. KROTIUK: This, if you look at this term here and this term here and then these terms here without the 6, that's the classical form of an Ergun equation.

CHAIRMAN WALLIS: Now, I think in these low velocities, the kind of data -- stuff you're getting the viscous term is the denomer (phonetic) of one.

MR. KROTIUK: For the very low velocity, that's true but I have the kinetic term in there also. And --

CHAIRMAN WALLIS: It makes some of the analysis easier.

MR. KROTIUK: Yes. The -- what I've done, though, is to try to -- I have this non-dimensional permeability here which really provides a -- in essence, it's a modification to the specific surface area. It's a way of trying to modify it and say as a function of void ratio, which is really related to porosity and all. It's how that -- what -- how you could determine what that change in that Sv really is and that's similar to this term over here for the kinetic term.

CHAIRMAN WALLIS: I think you have a problem in that you don't know Sv and X independently. They both come from pressure drop data. There's no sort of independent way of measuring them.

DR. BANERJEE: Unless you take sections.

CHAIRMAN WALLIS: Well, even then, you have to infer them from the pressure drop data because Sv isn't measured independently. If you just take particle size distribution, you don't get a very good value, you get it from the pressure drop.

MR. KROTIUK: Right, Sv cannot be determined theoretically. It's really a function of your experiments.

DR. BANERJEE: But then it just becomes a fitting parameter if you can't go and look at the data that you might get and find the surface area from that.

CHAIRMAN WALLIS: Ralph, are we -- okay.

MR. KROTIUK: I agree with what you're saying but I didn't include all my --

DR. BANERJEE: That could be a physical thing.

MR. KROTIUK: Sv?

DR. BANERJEE: Yeah, in other words, otherwise it's just a fitting parameter.

MR. KROTIUK: It's -- yeah, but I'm trying not to treat it as a fitting parameter. What I'm trying to say is to keep the Sv constant but there is a multiplier for the Sv which is in this factor here which is the dimensions permeability which is a function of whether the particles are -- whether the porous beds is particles or the porous bed is fibers, there's --

DR. BANERJEE: Sure, I mean, x is a measure of that.

MR. KROTIUK: X is a measure of that but K also has a function to it.

DR. BANERJEE: Right.

MR. KROTIUK: And you have to determine --

DR. BANERJEE: K is a function of X.

MR. KROTIUK: It's a function of the --

DR. BANERJEE: The rod to the particle.

MR. KROTIUK: It's a function of some

relationship of the particles or relationship to the fibers.

DR. BANERJEE: It has to be a universal function to be useful though.

MR. KROTIUK: It's a universal function for all particles or all fibers in a certain orientation.

CHAIRMAN WALLIS: So the way you define it, as the bed is compressed it changes.

MR. KROTIUK: As the bed is compressed that's changing, correct. That is correct, and in my original derivation, I actually have a graph of this KX as a function of porosity.

DR. BANERJEE: It's like a Darcy permeability except your --

MR. KROTIUK: It's related very -- yes, and that's what I was trying to say. I skipped over it very quickly but I was trying to say I used a Kozeny-Carman equation to relate to permeability, velocity and the debris surface area and then you come up with this non-dimensional parameter.

MR. LETELLIER: You still basically have two free parameters, Sv and Epsilon because K(X) is inherently a function of Epsilon given a specific geometry. You still only have two free parameters in the model. Is that correct?

MR. KROTIUK: Yeah, that's correct.

DR. BANERJEE: Yeah, but you can achieve the same Epsilon with different --

MR. LETELLIER: Configurations.

DR. BANERJEE: X, yeah.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Now, this is a pressure gradient, isn't it? Isn't this a pressure gradient you're talking about here?

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Because the compressibility is different in different parts of the bed. The compression is different.

DR. BANERJEE: This is Darcey's equation.

CHAIRMAN WALLIS: The PTL it shouldn't be the P over L, it should be PTL, that's the basic equation.

MR. KROTIUK: You're right.

CHAIRMAN WALLIS: And as you compress it, these parameters on the right side integrate through the bed.

MR. KROTIUK: Right, and that's why I'm trying to look at multiple thicknesses.

CHAIRMAN WALLIS: When you use PTL you get in all kinds of trouble.

MR. KROTIUK: Right, okay. This is the hydraulic portion and I just put it this way but, you're right. Now, what I did, I looked at the CalSil and the Nukon and I just used that equation to come up with an equation that includes both the KX for Nukon which is for fibers and like I say, the KX, the permeability factor for CalSil, which is particles. And this is what I kind of derived.

CHAIRMAN WALLIS: You didn't have to deal with the inertia part. It would be a lot simpler. You have a linear thing in velocity and you wouldn't have these weird 0.071 powers and things.

MR. KROTIUK: Right, that's --

DR. BANERJEE: But even leaving -- yeah, you probably should kill the inertia part but that's a separate issue. The X's though, I mean, the way you defined X was the volume fraction of -- I forget --

MR. KROTIUK: To the void.

CHAIRMAN WALLIS: The void is changing as you --

DR. BANERJEE: Rods, to -- so Nukon is something or the other.

MR. KROTIUK: So it's really the volume of the -- the void to the volume of the solid.

CHAIRMAN WALLIS: Right, which could never be more than a certain amount if it's packed solid.

MR. KROTIUK: Yeah, but as you compress, it could change.

DR. BANERJEE: But the volume of the solid has rods and cylinders, right? Are you going to differentiate between them or not?

MR. KROTIUK: That's why I did this, is I had a different -- this is the equation -- the is the permeability -- the dimension permeability equation for the cylinder portion and this is for the particle portion.

CHAIRMAN WALLIS: What do you do with the calcium phosphate?

MR. KROTIUK: I'm not addressing that right now.

CHAIRMAN WALLIS: Presumably that's another term or another factor or something in here.

MR. KROTIUK: That would have to be.

DR. BANERJEE: So you're getting two different K functions?

MR. KROTIUK: That's correct because that's a function of geometry.

CHAIRMAN WALLIS: So it's like a PhD thesis?

MR. KROTIUK: Excuse me?

CHAIRMAN WALLIS: So it's like a PhD thesis.

MR. KROTIUK: It was work done -- I'll just say it and then you could -- I have references in what I handed out. An individual called Happel actually did -- had his thesis on this subject and he actually came up with these permeability relations and it came out of his thesis.

DR. BANERJEE: He did cylinders and --

MR. KROTIUK: He did cylinders along the direction of flow, cylinders across, you know, and spheres.

DR. BANERJEE: But all mixed together?

MR. KROTIUK: No, he did them separately.

DR. BANERJEE: That's different.

MR. KROTIUK: Yeah, but I'm trying to relate them and put them into one equation.

CHAIRMAN WALLIS: Oh, I like this figure.

MR. KROTIUK: Okay, let's talk about the compression and as we said earlier, hysteresis has been observed. Now, what I've done is I've taken the approach -- you know, we have this velocity going up and then coming down. There is a recognition that, you know, it's a complicated phenomena so I made the simplification that during the first velocity increase, you have a non-recoverable, irreversible process and all subsequent compressions or expansions of the debris bed would be a irrever -- an elastic process after that with a constant compressibility. Now, that's what I've done at this point in time.

That's my assumption. So using that assumption you come up with basically two relationships. One is for the first compression where you calculate your void ratio as a function of your mechanical stress on a section of the debris bed. Which could be the entire bed in my one volume model or could be part of the bed in a multi-volume model and you relate that to the mechanical stress at the start of compression --

CHAIRMAN WALLIS: Is this just what's available in the pore of the fiberglass? Is that all that is?

MR. KROTIUK: I'm sorry say again?

CHAIRMAN WALLIS: This is what's available in the pores of the fiberglass?

MR. KROTIUK: Essentially, yes.

CHAIRMAN WALLIS: Because it says nothing about the CalSil yet.

MR. KROTIUK: Well, this relationship is

really -- I've applied it to both the Calsil and the Nukon both. Basically, I'm saying that during the first compression I can calculate the void ratio based on the void ratio at some starting point and the mechanical stress across the debris bed at that starting point.

CHAIRMAN WALLIS: It's just like the length over the original length in a way, isn't it? Isn't it related to just the strain, the length over the --

MR. KROTIUK: Oh, yes, yeah.

CHAIRMAN WALLIS: So isn't it a simple transformation from the strain to X over X prime?

MR. LETELLIER: He hasn't derived it that way. In essence it would be.

CHAIRMAN WALLIS: I would be.

MR. LETELLIER: He has a decaying spring constant to account for the --

MR. KROTIUK: And it's related with the -- the parameter N is really the material specific parameter, which I would try to get from the test data. And then after the first compression when you have the elastic portion, the relationship comes out in this fashion. You are now relating your void ratio in the section of the debris bed to the maximum mechanic stress at the highest velocity point.

And again, the factor of N is again in there and looking at say the LANL series 6 data, I came up with an invalid of about .3 and a -- somewhere I'm getting around .2 to --

CHAIRMAN WALLIS: These are the typical values from bed compression here, aren't they? It's just compression without any particles at all to get something like this.

MR. KROTIUK: Oh, yeah, right, you're right, yeah, something of that nature but so then this way when I do the calculations, what I'm doing is I'm solving the hydraulic portion for a period of time and then for a given point in time and then looking at where I am, whether it's the first compression or one of the -- after the first compression from the elastic portion, and I do an iterative calculation between the hydraulic conditions and the compression conditions and you could come up with a final bed thickness.

CHAIRMAN WALLIS: And P is the pressure drop or something? What's this PM?

MR. KROTIUK: You can -- the derivation to P which is mechanical stress, is actually equal to the pressure drop across that section of the debris bed or the entire debris bed if you're taking it as a homogeneous bed. So you could actually --

DR. BANERJEE: It's basically the same type of model. It's the total force.

CHAIRMAN WALLIS: Yeah, that's right, that's what it should be.

MR. KROTIUK: It's the force, yeah.

CHAIRMAN WALLIS: The strain is proportional to stress or is not proportional to this N index. Strain is related to stress, not stress gradient.

DR. BANERJEE: Not gradient, yeah.

CHAIRMAN WALLIS: So then you've got a correlation of some sort.

MR. KROTIUK: Yeah, and I just wanted to show you, like this is for the LANL Series6 data and I actually --

CHAIRMAN WALLIS: I'm surprised you got -- I did something similar I think, you find and I actually put in the --

MR. KROTIUK: Yeah, you did something very similar, yes.

CHAIRMAN WALLIS: -- put in different symbols for different tests, which can perhaps tell you something.

MR. KROTIUK: Yeah, I've done this -- I've plotted this a number of ways and I actually have plots with all the different tests separated out.

CHAIRMAN WALLIS: But my curve went through all the data.

MR. KROTIUK: Excuse me?

CHAIRMAN WALLIS: Yours seems to scatter more, that's all -- but that's another matter all together.

MR. KROTIUK: But like, you know, this is the point -- this is with the .23 and then there's these outlying points which are test --

CHAIRMAN WALLIS: A lot of your scatter is artificial. If you look at what they did, they could only measure to a certain -- a quarter of an inch or something accuracy. So the data went in steps and if you actually plot the steps instead of the points, the steps sort of covered the correlation.

MR. KROTIUK: That's a good point.

CHAIRMAN WALLIS: It looks much better than if you just show this like this, it ignores that fact that they couldn't measure accurately, so they go in steps.

MR. KROTIUK: Yeah, that's a good point. I'll replot it as that.

Then I needed that starting point, in

other words, the bed thickness at that starting point.

CHAIRMAN WALLIS: It's just some reference thickness.

MR. KROTIUK: Right, some reference thickness but I tried to relate it to again, this is the Series 6 data.

CHAIRMAN WALLIS: I presume it's proportional to the amount of fiberglass.

MR. KROTIUK: Exactly, that's what happened. There was only one point --

CHAIRMAN WALLIS: So you might as well use kilogram per meter squared.

MR. KROTIUK: That's what I'm doing, kilogram per meter --

CHAIRMAN WALLIS: Because that's a better measurement than undetermined thickness.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: In fact, since you've got P over PM to some power, the prediction is that with no stress on the bed, it's infinitely thick, which doesn't really help you very much. So it's much better to refer to some kilogram per square meter. Do you see what I mean?

MR. KROTIUK: Yeah, I see what you mean.

CHAIRMAN WALLIS: Because it's a

horizontal bed. There's nothing to compress it before you've got any pressure and it's infinitely thick until you get a little bit of something squeezing it up. If it's vertical, it's got its own weight that holds it down --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- which isn't in this theory.

MR. KROTIUK: Because a lot of the --

CHAIRMAN WALLIS: If it were underneath the screen, it wouldn't be there at all. You'd have to have some pressure to bring it up.

MR. KROTIUK: Correct.

CHAIRMAN WALLIS: So I think to tie it to kilogram to square meter is a much better way to do it than to try to measure an uncompressed length.

MR. KROTIUK: Yeah, and I could do that.

CHAIRMAN WALLIS: Right. Isn't that what you've done?

MR. KROTIUK: Yeah, that's exactly what I've done. This is kilogram per square meter versus bed thickness.

CHAIRMAN WALLIS: Right.

MR. KROTIUK: Then this is what I was eluding to earlier. This is just a comparison for the equivalent tests that were run for LANL. This is 6A.

CHAIRMAN WALLIS: So why doesn't it go through zero? I mean --

MR. KROTIUK: Yeah, that -- yes.

CHAIRMAN WALLIS: The bed with no mass would have no thickness.

MR. KROTIUK: Yes, you are absolutely right. And with the -- I didn't include it here but I'm still working on this. I've included some of the measurements from the PNNL data and it actually goes through zero. You know, I need more data points, basically.

CHAIRMAN WALLIS: I think your 6C, 6G and 6H are just the ones which have some rather weird pressure drop data.

MR. KROTIUK: Could be, but I have actually plots of this with the PNNL data and for that one --

CHAIRMAN WALLIS: I mean, they had anomalus data. They had data where the thickness of the compressed bed was greater than the thickness of the uncompressed bed, that kind of thing. 6C was really anomalus that way. Something was very odd about 6C as I remember.

MR. KROTIUK: That's why I threw that one in, but I'm looking at -- I'm trying to, you know, look at this more.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: And this is just what I was eluding to before. Is that this is the comparison of the LANL test and these are somewhat the equivalent of tests that were done at PNNL and the key thing I want to point out is that when we have this added mass, add a Nukon, add a CalSil mass and then I have a column here for bed Nukon and bed Calsil, the key thing that I wanted to point out is that like for instance in this case which is equivalent to 6B, .78 kilograms per meter squared. I tried to do everything in kilograms per unit area, .78 was added but only .67 was measured as deposited into the bed.

And you could see that in t his case, .5, .33, so it seemed to indicate that even though you added a certain amount to the loop, not everything was deposited into the bed. Whereas, if you look at the Nukon, the Nukon in most cases is much closer. There's only one case here that seems to be an outlier, but you know, what was ended up from the actual measurements, you added the Nukon and the Nukon was deposited on the bed.

CHAIRMAN WALLIS: Now, this CalSil, where did that come from? This was the weight at the end of a test?

MR. KROTIUK: That's correct.

CHAIRMAN WALLIS: From whose experiment?

MR. KROTIUK: From PNNL's.

CHAIRMAN WALLIS: Okay, because LANL used this --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: -- and they reached a different conclusion.

MR. KROTIUK: That's correct, and so in their case, you can see that the -- what they estimated was in the bed was very, very close to what was added to the loop.

CHAIRMAN WALLIS: Right, that's right.

MR. KROTIUK: So that -- the question I have is --

CHAIRMAN WALLIS: So that's another cause for uncertainty, isn't it, in the whole thing?

MR. KROTIUK: Yes, so the question is that how do --

CHAIRMAN WALLIS: What wasn't in the bed went through the reactor.

MR. LETELLIER: That's right, several times. Bill, for the PNNL when you're doing the mass balance, you're assuming 100 percent of the fiber is actually on the screen?

MR. KROTIUK: No, I did not.

MR. LETELLIER: How did you separate them post-test?

MR. KROTIUK: Because you measure the weight of the entire bed and then using the technique that they've developed, you could calculate the weight of the CalSil in the bed and then you just subtract it, so you know, total weight minus CalSil weight.

MR. LETELLIER: Through the dissolved concentration, that's how you did it.

MR. KROTIUK: Right, yeah. So it's as good a measurement as you could get.

MR. LETELLIER: How did that compare -- out of curiosity, how much fiberglass continues to circulate or was otherwise lost to the bed?

MR. KROTIUK: In none of these instances, as you could see, here's the -- for the fiberglass, for the Nukon, okay. If you can look at this column here, there's the added Nukon and there's the bed Nukon. You can see that they're very --

MR. LETELLIER: Very close.

MR. KROTIUK: -- very close. So most of the Nukon gets deposited onto the bed.

DR. BANERJEE: I want to ask you one question. I mean, look at the people at Los Alamos and so on, they've done so many pressure loss and other calculations to complex media including porous media with cracks and everything under the sun. If you've got bed sections, they can even scan these into the codes and run them because these are very little random numbers. These are things which is easy to run with the -- takes half an hour. So why don't you do that rather than such an empirical approach?

MR. KROTIUK: We were actually thinking of that and we wanted to take -- this is a simpler approach.

DR. BANERJEE: Right, sure. I mean, this may lead to -- the problem with all of these things is that it becomes very dependent on the geometry of the fibers, you know. I'm sure that you can derive a correlation for Nukon specifically and maybe CalSil of certain size distribution, whatever.

MR. KROTIUK: Right.

DR. BANERJEE: But if you change the thing a little bit and something arrives which is oriented, all aligned one way or something, the permeability correlations will start changing quite a bit depending on the geometries and stuff like that.

MR. KROTIUK: I mean, I hear what you're saying but you know, this is an approach also is that you know, you have to -- when you try to develop a correlation like this, you have to make an assumption and the assumption is is that I have the fibers 90 degrees to the direction of flow. So --

DR. BANERJEE: But they may or may not, you don't know.

MR. KROTIUK: Yes.

DR. BANERJEE: And once you get the core samples you know, when they've cut it.

MR. KROTIUK: Yeah, we will have some of that data.

DR. BANERJEE: The reason I'm saying this that the oil industry even uses these types of methods now for doing their porous media factor calculations and stuff.

MR. KROTIUK: The chemical industry uses it, too.

DR. BANERJEE: Yeah, in fact, I've seen Dow, for example, for their pack beds, they even are looking at the formation fo the pack bed. There's some problems where they're depositing the packing which is very complex, into a bed and then looking at the flow through it. There's a guy named David West at Dow is the head of the Fluid Dynamics. So this is industrial practice today to look at the deposition and looking at flow-through sort of complex media.

MR. KROTIUK: But as Dr. Wallis has said, is that we're looking at a process of deposition into the bed that is not very well defined.

DR. BANERJEE: No, but it's a question of where you put -- you know, if you know -- there are all sorts of things, but if you do know enough to use this correlation which, as you know, when water is being delivered and all this stuff, so --

MR. KROTIUK: If you know what's being delivered, yes.

DR. BANERJEE: Yeah, so that you have to know.

MR. KROTIUK: Yes, that's right.

DR. BANERJEE: What sequence, that has to be connected to something, some estimate, but you are going to need that anyway for this.

MR. KROTIUK: You need that for this correlation, yes, you do need it for this correlation and that's why I was saying from the beginning that I was hoping that with a multi-volume debris bed we could make some sort of conservative yet not overly conservative assumption in terms of --

CHAIRMAN WALLIS: I'm a little puzzled by this table because it looks as if LANL had more CalSil that was retained in the bed than PNNL in the same conditions.

MR. KROTIUK: That's correct.

CHAIRMAN WALLIS: But LANL had a lower pressure drop at the same conditions, typically. So it's not consistent with what you'd expect. You'd expect if there's more CalSil retained in the bed for the same conditions, you would have a higher pressure drop.

MR. LETELLIER: Please remember those were estimated masses based on --

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Based on their stability.

MR. LETELLIER: Right.

CHAIRMAN WALLIS: They're actually in your report, they're on a table so I --

MR. LETELLIER: They are.

CHAIRMAN WALLIS: So I'll use them when I'm considering --

MR. KROTIUK: Right.

CHAIRMAN WALLIS: It's the best we have, really from that experiment.

MR. KROTIUK: So, you know, my feeling is, is that this assumption for the LANL test that all the Nukon is on the bed is probably pretty good but is this really 100 percent correct?

CHAIRMAN WALLIS: That was measured though. Maybe the measurement technique was not a good one for that.

MR. LETELLIER: The primary fault is the calibration. Keep in mind that we're looking at really low concentrations so we had to do a calibration standard to units of NTU.

CHAIRMAN WALLIS: So it's very, very non -- this is very, very clear?

MR. LETELLIER: Visually, it's very clear and --

CHAIRMAN WALLIS: Because I think Bill Shack's was not very clear.

MR. LETELLIER: No, after many circulations with CalSil fiber.

MEMBER SHACK: Right, I mean, it gets clear, yeah.

MR. ENDERLIN: We can physically, in both our large and our small scale and you can see the cloud visibly as the opaqueness in about four passes, you watch the cloud and you can watch after about the fourth pass the fluid is clear and every time you see it come out, you watch a little blip on the pressure measurement as you're doing your bed formation.

CHAIRMAN WALLIS: Now you tap the test section, you knock it, rap it, does stuff come out of the bed?

MR. ENDERLIN: I paid a lot for the polycarbonates so we haven't --

CHAIRMAN WALLIS: No, but you see what I'm getting at.

MR. ENDERLIN: Yeah, we haven't -- we have not see a lot come out when we rap the PVC one down below but I can't -- I don't have as good a visual underneath the bed in that one.

MEMBER SHACK: I mean, if you up the velocity, you get a puff out, don't you?

MR. ENDERLIN: If you don't degas you get puffs, but when we ramped the velocity up, we don't get a lot of visual seeing puffing unless you have air in your bed.

MR. KROTIUK: No shedding.

MEMBER DENNING: Graham, why don't we go ahead.

CHAIRMAN WALLIS: Yeah, we have to finish.

MR. KROTIUK: Yeah, I'm just -- the rest

is my guess at a correlation right now, so --

CHAIRMAN WALLIS: It seems to me that it works on the PNNL but it doesn't work so well on the LANL.

MR. KROTIUK: Right, and this is my first shot through so it's not really meaningful --

CHAIRMAN WALLIS: You have to know which one to check against when they're not the same.

MR. KROTIUK: Yeah, and one of the other things that I do want to look at is just to make an approximation maybe of what would have really been using the data from PNNL, what really the CalSil would have been in the bed and see if that matches closely. This is really my first guess at this and I don't think it's --

CHAIRMAN WALLIS: So who is going to do an experiment with a vertical bed which is something like a real screen to show that you can predict it?

MEMBER DENNING: The green is --

CHAIRMAN WALLIS: This horizontal bed was very, very carefully prepared and stuck. Who is going to do an experiment for the vertical bed with realistic layers of stuff on it, most at the bottom and the top.

MEMBER DENNING: Nobody.

CHAIRMAN WALLIS: So how are you going --

MEMBER DENNING: I'm sorry, the industry is going to do that. The industry is going to do that to some extent. They're going to take sections.

DR. BANERJEE: But they would need to make more detailed measurements.

CHAIRMAN WALLIS: How do you do confirmed -- how do you evaluate the industry stuff if you don't have your controlled experiments for something like what they have because this isn't what they have?

MR. KROTIUK: No, but we said from the very beginning, I remember awhile ago, months ago, that this is for basically a bed that was uniformly --

CHAIRMAN WALLIS: What's LANL going to do? These guys come in. They say, we've done experiments and we've -- we got this, empirically we're going to use this and it's two orders of magnitude away from what you predict using this curve, well, that's because you've got a different distribution. How are you doing to have any kind of a confirmatory measure of what they've done?

MR. KROTIUK: That's not what this correlation was intended to look at.

DR. BANERJEE: Maybe this works locally, you know. Let's look at it this way; you need something locally but I think you really need to eventually bite the bullet and have a particle transport code so you know the distribution is going to be this way, which is thicker at the bottom and thinner at the top. Without that, you don't have anything.

MR. LETELLIER: One approximation that we've used is --

MEMBER KRESS: It would be dominated by the open parts.

CHAIRMAN WALLIS: Yeah, Mark has --

MR. LETELLIER: One approximately that we have used is to assume that these one dimensional models apply at any point locally on the face of the screen. And, in fact, if you assume a uniform suspension, you can actually track the build-up profile that looks very much like some of our vertical screen testing. You can predict head loss over a non-uniform bed by using this as the kernel, if you will, for accumulation.

DR. BANERJEE: But then you have to know something about the fluid mechanics that flow through this bed and the bypassing and so you have to do that -- you have to do a CFD calculation and the NRC is way back in like --

CHAIRMAN WALLIS: But then you've got an array of 20 top hat screens, which are all catching different amounts of debris.

MR. LETELLIER: You tend to incorporate the inertial effects of debris transfer if you have to do that. I was simply assuming a flow following tracer material.

DR. BANERJEE: It's surprising how far we are behind here, behind industry. That really surprises me. People are doing this for chemical reactors and distillation columns and things and we're just sitting here and doing these things.

CHAIRMAN WALLIS: Well, maybe we need to sleep on it. If you can make your bed in under 70 hours, we -- this is all very interesting stuff. I think we're complete saturated with information by now. We'll come back tomorrow with clear minds and no debility of any sort in our heads and see what we can make out of it all.

We have some more interesting stuff tomorrow. We're going to recess. It's now 6:30, we'll recess.

(Whereupon, at 6:30 p.m. the above-entitled matter recessed to reconvene on February 16th, 2006.)