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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS
SUBCOMMITTEE ON THERMAL HYDRAULICS

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MEETING

Wednesday, July 20, 2005

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The meeting came to order at 8:30 a.m. in room O-1G16 of White Flint One, Rockville, Maryland. Graham Wallis, Chairman, presiding.

PRESENT:

GRAHAM B. WALLIS	CHAIRMAN
VICTOR H. RANSOM	VICE CHAIRMAN
RICHARD S. DENNING	ACRS MEMBER
THOMAS S. KRESS	ACRS MEMBER
WILLIAM J. SHACK	ACRS MEMBER
JOHN D. SIEBER	ACRS MEMBER
RALPH CARUSO	DESIGNATED FEDERAL OFFICIAL

ALSO PRESENT:

TIMOTHY ANDREYCHEK	WESTINGHOUSE ELECTRICAL CORP
RICHARD BARRETT	RES
ROBERT TREGONING	RES
JOHN GLISCLON	EPRI
KERRY HOWE	UNIVERSITY OF NEW MEXICO
B.P. JAIN	NRR
PAUL KLEIN	NRR
MARC KLASKY	LANL
BRUCE LETELLIER	LANL

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

8:33 a.m.

CHAIRMAN WALLIS: Well good morning. Welcome to the latest installment of the sump saga. This is a meeting of the ACRS Subcommittee on Thermal Hydraulic Phenomena. Ralph Caruso is the designated federal official. I think you all know the rules of engagement. Please use a mic and speak clearly. We will gather information, and make a report to the full committee on the status of things. I don't think that we're expected to write a letter on this because it's a work in progress. We're all looking forward to hearing what happened in these experiments, and I will turn the meeting over to Rob Tregoning to get us going.`

MEMBER SHACK: Mr. Chairman, before Rob starts I should note that Argonne National Laboratory is a contractor for the Office of Research on some work related to this area.

MR. CARUSO: Did you need to have --

CHAIRMAN WALLIS: We're going to do that at lunchtime. It's all been set up.

MR. CARUSO: Okay, thank you.

CHAIRMAN WALLIS: So we're ready to go. And I don't think we have any members of the public who are going to speak. Is that correct?

MR. CARUSO: That's correct.

CHAIRMAN WALLIS: All right, thank you.

MR. TREGONING: Let me start, because we are going to have a full day, and then when Rich gets here he will -- I know he wants to make some opening remarks.

CHAIRMAN WALLIS: Well, I think with you speaking we're bound to have a full day.

(Laughter)

MR. TREGONING: Nothing like getting off on the right foot. Well, we're going to have a full day because, I think, two reasons. One, it's been some time since the Office of Research has been in front of the ACRS to talk about the work we've been doing in sumps. I think it was last May or so, so it's been over a year, and there's been quite a lot of work that we've done in the interim. So we have a lot of things to present. So yes, it is going to be a full day. I apologize for that. We've probably got a day and a half worth of material that we're going to try to cram into today.

CHAIRMAN WALLIS: Well, give us the essentials.

MR. TREGONING: We've got the essentials

up front, and then as we move on there will be things that we're doing that aren't quite as essential. So what I'm going to do in this overview is just -- I want to go through the objectives the meeting today, which is really essentially just walking us through the agenda.

So the first objective on Slide 2 of your first handout on overview. We're going to be reporting on the development, results, and status of the joint NRC/EPRI Integrated Chemical Effects --

CHAIRMAN WALLIS: ERPI is EPRI? Okay.

MR. TREGONING: Yes. And we affectionately refer to the Integrated Chemical Effects Testing as the ICET Program. You'll hear that acronym program quite a bit today. We have three presentations, actually four presentations related to the ICET program. The first one is the development of the test plan, all the thinking and analysis and judgment that went into laying out the experiments themselves. We think it's very important to present that to you so you can see how we arrived at the conditions and the parameters that we've ended up testing.

After that there will be two talks. One, the first talk will be on how that test plan was implemented by the contractor, Los Alamos National Labs. So they will talk about their approach for implementing the test plan. And then they will present a rather large talk on findings and observations associated with the first four ICET tests. There are five ICET tests planned. Four have been completed to date. One more is scheduled to begin next week, on Tuesday.

Once we get through the results, then we're going to have a presentation to discuss some of the implications from the results.

CHAIRMAN WALLIS: This is a very well organized play in that you don't tell us your results till the afternoon? So we're in suspense for the whole morning and through lunch, is that right?

MR. TREGONING: Yes, that's correct, and that's done by design.

MEMBER SIEBER: They sent us some stuff. You can read it.

CHAIRMAN WALLIS: Some sort of denouement, is that what they call it?

MR. TREGONING: Knowing how the committee likes to, you know, occasionally jump around, you know, we may get to the results prior to this afternoon. But we thought it was important before we, you know, there's a story to tell. And before you actually, you know, before you get to the dessert you have to have your appetizer, and your entrée, and you know, your wine in some cases, and then we'll get to the good stuff, which is the results.

CHAIRMAN WALLIS: The time we're most dozing off. It's just after lunch, so.

MR. TREGONING: Once again --

MEMBER SIEBER: That's also by design.

MR. TREGONING: Also by design.

CHAIRMAN WALLIS: All right.

MR. TREGONING: So we'll be, again, after we look at the results, we'll be discussing implications associated with the results from a regulatory perspective, and Paul Klein from NRR is going to be making a presentation for that. I'm sure there will be lots of questions with respect to regulatory implications. One of the things we're going to try to do is hold the questions as much as we can to that talk. It may be difficult. Then after that we're going to talk about follow-on work that both the NRC Research has and industry has, to do follow-up research to address findings in the ICET.

And 95 percent of today is going to be discussing chemical effects. However, we've been doing research in other areas, and we want to have, if there's time, we have into the agenda a short presentation to talk about other research activities that we have ongoing. So this is really an extra bonus. If we get to it, great. If we don't, if we're running out of time, and we've sufficiently inundated you with information, we can scratch that.

CHAIRMAN WALLIS: Well, this is the second day of our meeting, and I think the majority of the members have plans to leave at the time that the meeting is scheduled to finish. So we can't run over.

MR. TREGONING: Okay. Well, we will be quick, then. And if there are fewer questions, we can be quicker. I don't want to go --

CHAIRMAN WALLIS: No, there are always questions.

MR. TREGONING: I don't want to go over this flow chart too much because we're going to be dealing with pieces of it on the next slide. But this is really how conceptually we've laid out the research that we're doing in the area of GSI-191, and what the information that we're developing is being used for. So each of those boxes essentially represents a testing program. And we've had testing that we've done in the last few years which is represented in yellow. Testing which is represented in green is testing that we have currently ongoing. Testing that is in blue is testing that we have planned but we haven't yet started. And the red boxes show how this testing is going to be used --

CHAIRMAN WALLIS: Is there any testing of paint coating durability?

MR. TREGONING: It's not called out, but it's contained within either Boxes 3 or 12.

CHAIRMAN WALLIS: Because I think we had some questions about how well you knew whether the paint would come off or not. And some of the plants may be forced to take hundreds of thousands of square foot of paint and assume it comes off, even if it's unlikely to do so.

MR. TREGONING: Right. That's, again, there are SE requirements for them to do that. We don't have any failure tests that we're conducting. However, the industry has a couple of programs. I don't want to speak for them, but possibly --

CHAIRMAN WALLIS: They're doing something similar, then?

MR. TREGONING: Well, they're doing specific tests to evaluate failure of coatings. So that's something that they're looking at.

VICE CHAIR RANSOM: Is there any radiological testing of the -- you know, I've heard a lot of talk about gelatinous materials that are formed, and the question would be are these thixotropic, you know, what are their radiological properties, which has to do with how they would behave under flowing conditions.

MR. TREGONING: And we're getting into details right away. Some of the analysis that we do at LANL --

VICE CHAIR RANSOM: I'm wondering where that is on this flow chart.

MR. TREGONING: Again, the flow chart's an overview, essentially. It doesn't look at specific items like that. There's some radiology testing that's done as part of the Integrated Chemical Effects Test, which is Item 3.

CHAIRMAN WALLIS: Where does the Argonne work fit in here?

MR. TREGONING: The Argonne work would be Item 12, Synergistic.

CHAIRMAN WALLIS: Synergistic. That sounds really sophisticated. Okay.

MR. TREGONING: So, I'll be coming back to this slide many times today. So what you'll see at each point, I'll bring this slide back up and tell you what research we're going to be talking about. For the first part of the day and the bulk of the day, we'll be dealing with items on the left-hand side which are associated with chemical effects, either speciation prediction, the ICET testing itself, or the synergistic head loss testing that's being conducted at Argonne. So that's -- let me move on at this point.

Quickly, I just wanted to remind us of the background here, and show that we've done quite a lot of work in a short amount of time, both the NRC and the industry. I think with respect to chemical effects, initial concerns were raised by this body February 2003. After those concerns were raised, we conducted some scoping studies out at Los Alamos. They were completed in November 2003, where we looked at small-scale bench-top single effects sorts of tests with chemical products. What those tests demonstrated is that the issue or the concerns were real, and that we needed to embark on a more ambitious test program to try to understand what would happen in more sump representative like environments.

CHAIRMAN WALLIS: What I remember from those tests was that it was somewhat inconclusive how much zinc was dissolved, but you got silica emerging as a player. Wasn't that what happened, that it was sort of, towards the end it was discovered that silica was an important player in what was going on?

MR. TREGONING: This is Bruce Letellier.

MR. LETELLIER: That was the recommendation of our peer review panel, to get more testing.

MEMBER SIEBER: You have to use the microphone. It's right over here.

MR. LETELLIER: This is Bruce Letellier from Los Alamos National Lab. The question was regarding to the importance of silica in the small-scale chemical effects test. And it was a recommendation of our peer review panel at the time, that we give more attention to that constituent in the chemical system.

CHAIRMAN WALLIS: Because it was found to be appearing in solution, right?

MR. LETELLIER: It was a concern in some of our leaching studies, and it was an industrial concern where silica had dominated the effects in other systems.

CHAIRMAN WALLIS: Thank you.

MR. TREGONING: I think we'll see today

that silicon in some cases can be a fairly prominent player. But it's really, it's environmentally sensitive in the sense that the actual sump pool conditions really dictate how big a player silicon is.

So, after this work, or even before these results were presented to the ACRS, we embarked on this Integrated Chemical ICET program jointly with the industry through EPRI. I think in January of 2004, that's when we really made the decision to do a joint project versus an individual project. Work had been ongoing up to that point to at least start development of the test plan. Between January and October of 2004 there was a lot of work that went into finalizing the test plan and signing that, which actually happened when the MOU was signed in October 2004. After the MOU was signed, we obviously initiated work even prior to that so that we could kick-start the testing. The first test was actually initiated in November of last year, right around Thanksgiving. And we expect to complete the fifth test by August of this year. So you can see, we've -- this has been an aggressive research schedule.

CHAIRMAN WALLIS: Well, the original schedule asked for a July or August start.

MR. TREGONING: Well, original schedules

have a way of --

CHAIRMAN WALLIS: But you said it was aggressive and all that, I just had to ask you about that.

MR. TREGONING: That would probably --

CHAIRMAN WALLIS: Why you were so late getting started if you were aggressive.

MR. TREGONING: That was overly aggressive. We couldn't start until we had an MOU signed, obviously.

CHAIRMAN WALLIS: But you know anything you say may be questioned.

MR. TREGONING: That's your prerogative. And again, we got some of the first ICET results around December timeframe. And we actually initiated some additional work around January of this year. So we didn't wait, or we haven't waited to start addressing some follow-on questions.

CHAIRMAN WALLIS: Once you get to Argonne, things will go really fast.

MR. TREGONING: I won't comment on that. This next slide, I'm going to put off this slide until later in the day. These are messages that we hope to convey to all of you during this meeting. So we'll revisit this. I mean, it's basically that we've seen a lot of interesting results. ICET program has been essential, but it's not --

CHAIRMAN WALLIS: I think what's important is are these important phenomena. I mean --

MR. TREGONING: Yes. We've seen several interesting, comma, important phenomena. And the ICET program is a necessary program to demonstrate that, but it's not sufficient to address all the questions that these important phenomena have raised. And as we move forward, I think it's important to make sure that we keep in mind what historically industry and NRC Research's role in this issue is. We're trying to partner with industry to make sure the right research gets done. I don't think either side has the resources to fully address every issue. So we tried to make sure that we looked at a target approach with each side looking at some issues associated with not only chemical effects, but GSI resolution in general.

CHAIRMAN WALLIS: It's interesting that in 5 you say your role is to provide confirmatory information, when in fact you seem to be generating the necessary information up front, although it's called confirmatory. You are the source of the information.

MR. TREGONING: The ICET program, that's

true. But that's the only program where we have that role. And that is a bit different from Research. That's a role that we do not typically have. We more typically have a confirmatory role. So ICET is the one project, it might be the only project that we have at an office level right now that has that sort of role.

With that, we've juggled the agenda, and Dr. Rich Barrett's here, and I know --

CHAIRMAN WALLIS: Rich, do you want to say something now?

DR. BARRETT: Well, first of all, let me apologize for being late. But as I look at my introductory remarks, I think that there's probably nothing here that won't be brought out, or hasn't already been brought out in the presentation. I do want to emphasize, though, that this is a very high priority effort for us. We recognize, as I'm sure you do, that this is a major challenge for the NRC and for the industry. And the Office of Research is committed to doing whatever we can to provide the confirmatory information necessary for NRR to evaluate licensee submittals.

The other point I think is important to make is that we're doing whatever we can to communicate the information that we're getting in a timely fashion, consistent with providing quality information. So with that, I think I'd like to ….

CHAIRMAN WALLIS: It looks like an ideal research program. You've got a very clear need. There's obviously some gaps in information that need to be filled.

DR. BARRETT: Yes, we think it's a very timely, a great opportunity for the Office of Research to contribute to the regulatory program.

MR. TREGONING: Okay, thanks Rich. Let me turn the microphone over to Dr. B. P. Jain and John Gisclon from EPRI, and Tim Andreychek. We're going to move to our next presentation, which is going to discuss the development of the test plan for the ICET program.

DR. JAIN: Good morning.

CHAIRMAN WALLIS: Good morning.

DR. JAIN: Like Rob had indicated earlier, this test plan is a joint program with EPRI. And this project management for the ICET program is, again, done jointly by NRC and EPRI, but NRC is responsible for directing all the content of this work. And industry brings a lot of knowledge and experience in the plan data that was used in developing the test plan to represent for post-LOCA containment environment. So that's a big contribution industry has made.

CHAIRMAN WALLIS: We're dealing here with essentially PWRs.

DR. JAIN: The PWRs, that's right.

MEMBER KRESS: But when you have a joint program like this, does NRC Research staff develop a test plan? Jointly with EPRI, or Westinghouse, or whoever? I'm just interested in the details of how you put together a test plan in a joint program like this.

DR. JAIN: Well, I mean, initially industry had developed a program, and then the staff had its own program, and somewhere in between the both were merged to come up with the one program.

MEMBER KRESS: I see. And how did you do this merging? The two of you get together in a room and say let's?

DR. JAIN: Well, that's right. We get together and go over the plan, and you know, we discuss and come to some conclusions and consensus that that's the right thing to do.

MR. KLEIN: B.P., if I might add to that. I'm Paul Klein from NRR. Initially both NRC and industry put together independent test plans. And then over a long period of time, and 12 revisions to the test plan, we came to agreement for the test matrix that was used to start the initial ICET test.

MEMBER KRESS: And NRR gets involved in this?

MR. KLEIN: Yes, we do. And there was quite a great deal of discussion with industry over that course of months that the revisions were made to the test plan.

CHAIRMAN WALLIS: This sounds like an ideal model for what should be going on.

MEMBER KRESS: Yes. That's why I was interested.

MR. TREGONING: The test plan, as I indicated in my schedule, we really started working jointly on the test plan in January, and it took really 10 months to get it hammered out. And to be honest, for Test 5, we've had subsequent revisions of this document as we go. It's a living, breathing document. And we hope that we will be signing the final version of the test plan actually today.

MEMBER KRESS: And the memorandum of understanding spells out who does what, and who pays for what?

MR. TREGONING: That's right.

CHAIRMAN WALLIS: Sorry, signing a test plan today having done the work?

MR. TREGONING: We still have one test that's starting next week.

CHAIRMAN WALLIS: When you know what you've done, you can write the plan.

DR. JAIN: It's prior to conducting the test.

CHAIRMAN WALLIS: Oh, for Test 5. I thought you meant for the whole program.

MR. TREGONING: No. We're not that deviant.

DR. JAIN: And we'll discuss the responsibility of EPRI, that they will provide the specification.

CHAIRMAN WALLIS: Okay, excuse me. So you learn as you go along. Test 5 is based on what you've learned before, right?

MR. TREGONING: We had originally -- Test 5 in the original test plan was defined as a TBD test, based on the first four tests. And then once we saw the first four results, I think it became clear to all of us what that test needed.

CHAIRMAN WALLIS: That's also a good

model. Very often test plans seem to be sort of set in stone, and people do them without thinking about what they've learned from the previous tests.

DR. JAIN: Well, you can tell that we are up to Revision 13 of the test plan. The current test plan is obviously, it's 12c, what you have for your information. But we'll be issuing Revision 13 pretty soon.

The objective of the test program really is to determine, characterize, and quantify the chemical reaction products that may develop containment pool under a representative post-LOCA environment. And that's really taken out from what the ACRS had identified their concerns back in February '03.

CHAIRMAN WALLIS: I think I'm concerned with the word "may". I mean, what you want to do is quantify these things, but also the conditions under which they develop. So you need not to just to have that may develop. You need to know why, and if, and how they develop.

DR. JAIN: Well, that's part of analyzing the data.

CHAIRMAN WALLIS: Yes. I mean, you may get some extreme case which is very rare in a real sump. Or you may be very particular of sumps. And you need to understand how this stuff develops, and under what circumstances, and so on. I'm sure you're doing that, it's just that I object to the word "may" develop. You need to understand the developing process.

DR. JAIN: Well, we have covered a variety of conditions in the environment. And hopefully that leads us to, you know, conclude that what are those conditions.

CHAIRMAN WALLIS: Okay.

MR. TREGONING: And again, the test plan was developed before the testing started. So we didn't want to presuppose that we would necessarily have chemical reaction byproducts that would be significant. So that's why it says may develop.

CHAIRMAN WALLIS: Right, that's when you started. But when you found them, you need to understand how, if, and why, and all that.

DR. JAIN: And the second objective is to determine and quantify any gelatinous material that could develop during post-LOCA circulating phase. And by gelatinous, that includes any material that's amorphous or non-crystalline. So it's not really gelatinous, just gelatinous.

CHAIRMAN WALLIS: It includes web-like matrices? Whatever. Whatever's important for affecting the sumps.

DR. JAIN: That's true.

CHAIRMAN WALLIS: It's not just gelatinous. It could be something else. It could be flocculent. Whatever. I don't know.

DR. JAIN: This is, again, an overview of the test parameters, and the industry will go into more detail later. The test parameters, which have been used are not necessarily -- these are not plan bounding conditions, but they are representative. Because you can do only X number of tests.

CHAIRMAN WALLIS: Now, is this going to be useful for industry? I mean, are you going to give them something -- I just wonder if you're there yet. Are they going to be able to say we have certain pH in our pool, we have certain constituents in our environment, therefore we can predict that we have certain products produced? Do you have enough tests to reach that conclusion, or are these exploratory tests where you say these are representative, and we find this kind of stuff, but we don't have enough tests to have a tool for predicting what we get?

DR. JAIN: Well, we have tried to cover

the various, like pH conditions, the insulation material. So that gives you a range of parameters.

CHAIRMAN WALLIS: But are you going to end up with a predictive value, or are you going to end up with a discovery that, yes indeed, there is stuff sometimes.

DR. JAIN: Well, this is not a predictive tool, no.

CHAIRMAN WALLIS: It's not yet a predictive. It will say this might indicate you need further work to get at a predictive tool?

MR. TREGONING: One of the things I think you're going to see today that I think we all realize here is that we have a relatively small number of tests. Those tests were designed to be broadly representative of a different -- each test was designed to represent a certain percentage of the fleet. One of the things we've seen in these tests is that relatively small changes in the environment can potentially have a significant impact in terms of what forms and what the implications are behind the products that form. So I think we all realize that there's going to need to be some additional plant-specific analysis, possibly testing, used to demonstrate how plant-specific environments, how important parameters may differ from the ICET tests, and how those parameters may result in either similar or different species which are formed.

CHAIRMAN WALLIS: So what I've learned is that you're not producing a predictive tool that would go into something like the handbook for sumpologists to use.

MR. TREGONING: That's correct.

CHAIRMAN WALLIS: So we're some way from where we need to be eventually.

MR. GISCLON: John Gisclon from EPRI. Thank you. The data that we have looked at before would, I believe, allow us to categorize some of the anticipated reactions by buffer chemical and insulation types. And when you -- as Tim will talk about a little bit later, we've structured the matrix of the test to follow along that way. What Rob says is true. However, when you start exploring that matrix with those variables, I think there are some insights that can be developed, and in fact were developed through this testing program. And rather than get ahead of myself, we can discuss that later.

CHAIRMAN WALLIS: But if you've found, as I just heard, that the results are sensitive to small changes in things, that indicates that you need a fairly good predictive tool. When you get results that are sensitive to small changes, which could easily occur from plant to plant, you need to be able to model that somehow, or bound it somehow, or whatever. When you give advice to a specific plant about what to do.

MR. GISCLON: That's very true.

CHAIRMAN WALLIS: I'm sure you're aware of that. But I just wanted to see how far along you are on that path.

MR. TREGONING: Let me clarify. Small changes, I would say in significant parameters. So not every parameter. I think one of the things that ICET has been really informative is to focus our thinking and our future work on what are the critical parameters. And it's those critical parameters that we need to focus on. And I think for each test, and I don't want to get ahead of ourselves either, that there may be half a dozen or so critical parameters that really need to be understood. And that's going to be the challenge from a plant-specific perspective, understanding how their plant compares to the half-dozen critical parameters.

CHAIRMAN WALLIS: These plants have deadlines to respond to the NRC. And if you're still just exploring these phenomena, I don't quite know how they take them into account in their response.

MR. TREGONING: Let's table that.

CHAIRMAN WALLIS: We'll get to that. Maybe NRR's going to tell us that. Okay, thank you.

DR. JAIN: In terms of parameters, the industry conducted a survey, and we have a representative type of submerged and unsubmerged materials. That includes aluminum, copper, zinc, carbon steel, concrete, fiberglass, calcium silicate, concrete dust, the whole shebang there. And latent debris as well.

MEMBER KRESS: CS is carbon steel?

DR. JAIN: Carbon steel.

MEMBER KRESS: Okay. I wondered where the cesium was.

(Laughter)

CHAIRMAN WALLIS: Well, do you have products from electrical equipment that contains chlorines for instance? Chlorides?

DR. JAIN: Yes, we do.

CHAIRMAN WALLIS: You do? Okay. And you have --

DR. JAIN: We have that in the next slide, your next slide.

CHAIRMAN WALLIS: It's just that you didn't have it here. I was going to ask about it. Latent debris, of course, contains all kinds of stuff. And plants have learned that they have to be cleaner than they thought. Because some of the original latent debris, that my colleague Jack Sieber knows all about, was quite extensive, and had a lot of stuff in it, blue jean dust, and stuff. I mean, all kinds.

DR. JAIN: Yes, we have included that in this plan. The other parameters include the temperature, test pressure, flow velocity, boron concentration, hydrochloric acid, the one you were just alluding to, as far as degradation of cable insulation material, and lithium hydroxide.

MEMBER KRESS: How did you arrive at this test temperature?

DR. JAIN: You mean the concentrations?

MEMBER KRESS: No, the temperature.

MR. GISCLON: Tim Andreychek is prepared to discuss that when he talks about development of the plan.

DR. JAIN: The next one.

CHAIRMAN WALLIS: And this velocity is determined by CFD or something?

DR. JAIN: Velocity is the target

velocity.

CHAIRMAN WALLIS: I know, but how do you decide that it's limited to 0.1 ft/sec?

DR. JAIN: That's a range of velocities.

CHAIRMAN WALLIS: Predicted in the pool.

DR. JAIN: In the pool.

CHAIRMAN WALLIS: By CFD or something?

DR. JAIN: Well, we did the testing I think with ribbons and the nozzles to get the velocity measurements.

MR. TREGONING: I think what Professor Wallis is asking is what's the basis of that range.

DR. JAIN: Well, the range is based on plant conditions. That's what a typical range --

CHAIRMAN WALLIS: Predicted by?

MEMBER KRESS: Is this following fumes?

MR. ANDREYCHEK: This is Tim Andreychek of Westinghouse. That 0.1 ft/sec is a value that's typically what we would expect to see around the sump region. You're going to possibly get a little higher velocities right at the adjacent area of the break, where the water's dumping out of the break.

CHAIRMAN WALLIS: Right.

MR. ANDREYCHEK: But in bulk, in bulk containment space, approximately one-tenth of a foot per second is representative of what you would expect to see bulk velocity.

CHAIRMAN WALLIS: And this is driven by convection to the sump screen? It's not driven by any sort of natural convection?

MR. ANDREYCHEK: It's driven by movement towards the sump screen during research.

CHAIRMAN WALLIS: Around temperature changes and things? It's a fairly big thing.

MR. ANDREYCHEK: That's correct.

CHAIRMAN WALLIS: You might get circulation due to temperature variations.

MR. ANDREYCHEK: It depends -- the circulation you describe is dependent to some degree not only on the temperature, but also the height of the pool.

CHAIRMAN WALLIS: Yes.

MR. ANDREYCHEK: So there's a number of things that translate into that.

MR. TREGONING: We wanted to do two things with the range. We obviously want it to be representative. But the other thing we wanted to do is we wanted to make sure that we didn't have stagnant flow over the coupons. We wanted to continually wash any products that might be generated out.

CHAIRMAN WALLIS: Now, you've got velocity -- excuse me. If I were scaling mass transfer from a surface, I'd want to use something like a Reynolds number, presumably. And if the surface is smaller and has the same velocity it has a lower Reynolds number. So I don't quite know why you scale velocity instead of something dimensional which is representative of the process.

MR. TREGONING: Again, there were two reasons. It wasn't just to model the exact -- the chamber itself doesn't represent a containment floor in any sense.

CHAIRMAN WALLIS: Yes, that's one of the -- yes.

MR. TREGONING: You have coupons and racks. So the flow in this chamber is not representative of all.

CHAIRMAN WALLIS: Well, I would be concerned if you've got sort of a laminar flow in your chamber, whereas in the pool it was turbulent. Then you have very different conditions for mass transfer.

MEMBER KRESS: Well, this is a leeching process, and I suspect the mass transfer doesn't control. I suspect it's time in contact with the surface area --

CHAIRMAN WALLIS: The reaction of the surface that controls rather than the boundary layer or something.

MEMBER KRESS: I suspect. So in my view, I'd look for the time in contact with the surface area. But I don't know.

CHAIRMAN WALLIS: But in order to scale it to a real system, you'd have to justify that somehow.

MEMBER KRESS: Yes. Yes. I think that sort of thing needs justifying.

MR. GISCLON: If I may add, the scaling that was in fact performed to relate the actual containment conditions to the conditions in the test loop are based on the volume of water in the two different systems, as well as the surface area of the material, including insulation, galvanized metal, other types of metal in there. So the surface-to-volume ratio is maintained constantly between the test system and the actual containment systems.

MEMBER KRESS: That might be the right approach here.

MR. GISCLON: And the velocity isn't enough to assure that we don't have stagnation going on, but it's not designed to replicate the velocity at a sump screen, or something like that.

CHAIRMAN WALLIS: Well, I ask because at 140 degrees F and 0.1 foot a second, your sump transition is really getting there. You're dealing with something on the scale of inches.

DR. HOWE: Hi, I'm Kerry Howe with the University of New Mexico. And if we were talking about mass transfer scaling, we'd want to talk about Sherwood numbers or Pekkle numbers. And the issue here I think is whether or not the mass transfer would be diffusion-limited or not. And so at very low velocities, we would perhaps have a diffusion-limited situation. At very high velocities, we would have something that would be causing erosion as opposed to corrosion. I think there's a large range between being diffusion-limited and actually causing erosion because of high velocity.

CHAIRMAN WALLIS: But if it's diffusion-limited in a boundary layer, then it makes a big difference whether you've got laminar flow or turbulent flow.

DR. HOWE: But those would be very, very low velocities compared to what we've seen here.

CHAIRMAN WALLIS: Well, I don't know what very, very low means. Anyway, we'll get into that. But there's obviously got to be some analysis and scaling of the phenomena that explains why this is useful for modeling pools.

DR. HOWE: I think our pool is turbulent, and I think that in the range of flows that you would see in actual containment, you would see a wide variety of flows. And so I think the wide variety that you would see in containment is represented here as a turbulent condition. I don't think we would get to a laminar condition in containment.

CHAIRMAN WALLIS: When the velocity is zero feet a second, you still have some mixing in the pool, do you? Because of mass transfer, and density effects?

DR. HOWE: I think the zero that's listed here is because there's no selected --

CHAIRMAN WALLIS: You still have some sort of mixing going on in the pool. Okay. Well, we should move on I think.

DR. JAIN: The next slide. So the thinking which went into developing the plan --

CHAIRMAN WALLIS: Well, I guess -- this debris, this latent debris. What do you do about that in terms of what you throw into this pool?

DR. JAIN: Well, we throw in the scaled debris. I think it's about --

CHAIRMAN WALLIS: You vacuum clean something out of a plant and throw it in? Is that what you do?

DR. JAIN: No, it's just sort of surrogate material.

CHAIRMAN WALLIS: Ah.

MR. GISCLON: It was a surrogate material. It was based on what Dr. Letellier had --

CHAIRMAN WALLIS: So you have a real sampling of real plants, and you try to model it. It's got organic stuff in it?

MR. GISCLON: No, it's mainly sand and soil was what the latent debris is.

CHAIRMAN WALLIS: Fibers? There are fibers in plants.

MR. GISCLON: No, we have a lot of fibers, but we don't have any overalls in the tank so to speak.

CHAIRMAN WALLIS: Yes, but there are.

MR. GISCLON: Yes.

CHAIRMAN WALLIS: There is debris from overalls in plants.

MR. TREGONING: Well, and Bruce might want to comment here, but the latent debris meant to be representative of amounts of latent debris. And the sand and soil mixture is something that's relatively common to all plants. If we tried to be specific and have small quantities of overalls and other things, I think it would become intractable at that point, because I don't know that we would say to -- I don't know how we could even justify being representative to any one plant. There's just going to be so much --

CHAIRMAN WALLIS: But it depends on how much there is in a plant.

MR. ANDREYCHEK: To give you an idea of the representative amounts of latent debris that was used, the equivalent amount of latent debris that was put in this particular test facility was approximately three shot glasses worth of debris. Okay?

MEMBER SIEBER: Two hundred pounds of material in a real containment -- in a real plant, which is a very small amount. One thing I noticed, though, is the only source of organics came from the binder and the fiberglass insulation, as opposed to things like paint chips, and reactor coolant pump oil, and other motor oils, and things like that. And I tried to justify why that wasn't included in the conglomeration of prototypical debris. And I guess from the standpoint of oils, it's hard to imagine the transport mechanism where the oil would actually get to the sump. You know, because most of these motors are up pretty high.

CHAIRMAN WALLIS: Well, I think it's pretty simple. I have an oil spill in my basement and I have a flood, the oil floats to the surface, and then it's very -- it's transported by the water.

MEMBER SIEBER: Yes, but you have to make some assumptions. You have to add, for example, the oil spill into the accident to cause the sump to fill up with water. And to me that was probably not a likely thing or a proper thing to do. I think if you had a lot of washdown, you could float the oil out of the oil reservoir in a coolant pump. And there's probably anywhere, depending on the pump, 200 to 600 gallons per pump. But comparing that to 100,000 gallons in the sump, you know, here comes another eyedropper that you would put in there, so.

CHAIRMAN WALLIS: But when you filter it through the screen, you're taking all that stuff and putting it through the screen.

MEMBER SIEBER: Yes, that's true.

MR. LETELLIER: Typically, the reactor coolant pump oil reservoirs are above the flood level.

MEMBER SIEBER: Yes, that's right. It would have to come out of there by washdown. Or, every coolant pump for fire purposes has an oil conduction system which in time there is a tank at the bottom in the sump area that has the residue from that. But plant operators generally clean that once, you know, it doesn't fill up with oil. Again, we're talking drops of oil in lots amounts of water. So I guess in my own mind I sort of justify it with the exception of potential massive failures of unqualified paint that may exist someplace in some plants. And perhaps when you get to that we can -- you can describe that more carefully. It seemed difficult from my viewpoint to form gelatinous material without very many organic substances present from which it would, to my mind, come.

CHAIRMAN WALLIS: Well, when my sump pump works in my basement, and I have an oil spill, I can pump huge quantities of water, and the oil seems to accumulate on the fibrous material which is on the filter of my sump pump. It does.

MEMBER SIEBER: Did you have a LOCA?

(Laughter)

CHAIRMAN WALLIS: It's a very small quantity of oil that's spilled from the oil tank.

MEMBER SIEBER: Yes, it makes everything greasy.

CHAIRMAN WALLIS: It makes everything smelly, and you get this color of oil on the water, and it all goes to the sump pump and filters out, a lot of it's filtered out on the fibrous material. But these are not experiments that are necessarily typical of reactor sumps.

MR. LETELLIER: If I could make a comment. This is Bruce Letellier of Los Alamos National Lab. I don't want the committee to overly focus on the transportability of oil residue, because we're trying to simulate a chemical system. And you'll find innumerable trace constituents that we have not added to this experiment. In our opinion it's already complex enough. It's pointing us in the right direction. Now we have a basis for thinking about the perturbations that would be added by organics, rare earth metals, any number of things that you can put on a laundry list.

CHAIRMAN WALLIS: Well, the reason for us bringing this up is that we've learned that rather small quantities of stuff can have an effect, particularly if you don't have much of a filter cake, and your filter cake is an inch thick on a rather small screen, it doesn't take too much. I think when we're talking about cal-sil it's sort of a little box of cal-sil that can have a big effect on some of the screens. So a gallon of oil isn't a negligible thing. And a gallon of overall dust isn't necessarily a negligible thing either.

MEMBER SIEBER: On the other hand, I think that just looking at inventories of materials in containment, in particular the oil, you're talking 10 ppm, if you really carefully diluted it. To me, that's pretty small.

CHAIRMAN WALLIS: Well maybe someday there will be an experiment in a real containment and we'll know.

MEMBER SIEBER: Well, we already did that.

CHAIRMAN WALLIS: Okay.

MEMBER SIEBER: 1979, that's where the issue came from in the first place.

CHAIRMAN WALLIS: Shall we move on?

DR. JAIN: Okay, the next slide talks about some of the considerations which went into planning the test matrix. Obviously we had to define these parameters to address objectives which we stated before, and come out with the representative parameters. The test matrix considers a combination of major environment differences, basically pH and the insulation types. Those are the two variables.

CHAIRMAN WALLIS: The pH is made up of specific chemicals.

DR. JAIN: That's correct.

CHAIRMAN WALLIS: Tri-sodium phosphate.

DR. JAIN: Sodium hydroxide, sodium tetraborate. And then the relative quantities of different metallic coupons, we have aluminum, zinc, steel, and what have you. And establish what the temperature and the pressure's going to be in the test facility.

The next slide shows the test matrix. There was three to five tests. Each test was planned to run for 30 days. And as you can see, initial pH and the buffering agent, those are two variables. And then the insulations. Tests 1 and 2, we have 100 percent fiberglass, the variable is the pH. Similarly, Tests 3 and 4, we have 80 percent cal-sil, 20 percent fiberglass, and pH of 7 and 10. And the final test, which is planned for sodium tetraborate, the pH varies from 8 to 8.5. And there we have 100 percent fiberglass. So that's the five tests we feel that represent our spectrum in terms of the pH and the insulation types. And all of the parameters have been kept constant basically, the test coupons, and the dust, and latent debris, and HCLs, and the like.

MR. TREGONING: At this point, Tim Andreychek's going to -- or John Gisclon.

MR. GISCLON: The general approach to the development of the test plan, as was indicated before, it's being performed under the NRC/EPRI Memorandum of Understanding for Cooperative Nuclear Safety Research. And we developed an addendum that specifically deals with this chemical effects testing. As Rob also indicated, there were really two test plans at the beginning of last year. And we developed a consensus on the objectives of the testing, and basically merged the two plans. And Tim Andreychek was given the responsibility for actually performing the draft, and the draft went through several iterations and review cycles. It took months to get it to the point of where it was signed off and acceptable.

The test plan itself was intended to address the definition of the test parameters, the bases, which we thought was very important. There's a whole section in the test plan that deals with the bases, and we need to have the bases down there for knowing why we're doing what we're doing. The definition of the test loop itself, what we feel are important instructions to the organization for performing the test, as well as characterization of the chemical byproducts that occur during the testing. With that introduction, I'd like to turn it over to Tim and let him explain how it was, in fact, developed.

MR. ANDREYCHEK: Thank you, John. The test components are based on looking at materials that were inside the containment, specifically thermal insulation, concrete that would be exposed and coated. We looked at the buffering agents, and the pHs that would result from the buffering agents. And I think this was an important -- I tend to look at not so much what the pH is, but what's the buffering agent, and that buffering agent drives the pH that I'm going to look at. So that's the way I would characterize that.

We also attempted to understand and characterize in the test temperatures, flow velocities that were roughly representative of the class of plants, the 69 or so PWRs that we have, and also the timing of spray events, and the initiation and duration of spray. Typically we're using about a 4-hour duration.

CHAIRMAN WALLIS: All these tests were at the same temperature?

MR. ANDREYCHEK: That is correct, sir.

CHAIRMAN WALLIS: So we have no indication of how sensitive anything is to temperature?

MR. ANDREYCHEK: Not quite true.

CHAIRMAN WALLIS: Okay.

MR. ANDREYCHEK: And NRC, when we were looking at using -- for the purposes of trying to maintain some degree of controllability of the test, and understanding what was going on, the NRC did sponsor some work that was done by Southwest Research using a series of codes to look at temperature effects on corrosion products. And that work was published in a NUREG. I don't have the NUREG right in front of me.

DR. JAIN: It's NUREG/CR-6873. Dr. Wallis, we presented that study last June.

CHAIRMAN WALLIS: This was an equilibrium study?

DR. JAIN: Right.

CHAIRMAN WALLIS: Yes.

DR. JAIN: Well -- presented last summer.

MR. ANDREYCHEK: Right. So that was -- and included in the test plan is a description of -- summary statement of this work and how it was used in setting a temperature --

CHAIRMAN WALLIS: So it could conceivably be found that one of these tests revealed something more interesting than all the others, more important.

You might want to go back and vary something around that discovery. You might say, okay, what's the effect of having pools 50 degrees warmer or something.

MR. ANDREYCHEK: That is correct. And I think we see some temperature effects because in the very first test there was a slight temperature dip. So we did get some temperature effect, unintended as it may be, in the actual operation of the test. And I think we're comfortable with the 140 degree.

CHAIRMAN WALLIS: And if something were soluble at one temperature and not at another. Whatever.

MR. GISCLON: That in fact happened, and we'll talk about that.

CHAIRMAN WALLIS: Okay.

MR. ANDREYCHEK: We'll look at the data.

CHAIRMAN WALLIS: Sure.

MR. ANDREYCHEK: So, if I may go to the next slide, please. On Page 10, Slide 10, the materials we were looking at were zinc, aluminum, copper, carbon steel, concrete, and the thermal insulation. And the sources are listed in the column adjacent to the materials. We did use both galvanized steel as well as zinc-coated coupons in the test plan to represent both sources of zinc perhaps having slightly different activation energies when it comes to chemical reactions. Aluminum, of course, is in valve actuator components, instruments, some scaffolding that might be inside the containment. Copper, and this was, I think we were looking at the copper that was in the TMI sump and where did it come from. Our best estimate is that it came from containment fan cooler fins, extended surface heat transfer fins. And so we modeled copper coils using -- copper sheets using surface area. And that's a tremendous amount of surface area when you're looking at the fan coolers.

MEMBER SHACK: When you look at the test plan, the aluminum, there's one plant that stands out, and it's a sore thumb. It has a very high amount. I mean, there's hundreds of thousands of square feet of aluminum. And it's hard to imagine that in valve actuator components.

MR. ANDREYCHEK: Very perceptive. And if I may, that source, that particular plant, we went back in the survey when we collected that information, if there were outliers, things that didn't seem to fit, we went back to try to gather an explanation as to why the numbers were what they were. And that particular plant stores a lot of temporary scaffolding that would be used during an outage underneath the submergence level. And so that's the outlier for that particular plant.

CHAIRMAN WALLIS: Does it store anything else, tarpaulins or anything in there?

MR. ANDREYCHEK: Again, typically that type of material is not stored inside containment. When we went back in and asked for what do you keep inside containment, there were some plants that have gang boxes that are stored in chained off areas, and also scaffolding that's stored in chained off areas that happen to be submerged. So that's what we went back in to try to --

MEMBER SHACK: Have they moved it?

MR. ANDREYCHEK: I think in part that was the recommendation given to them. I'm not associated with that particular plant so I can't answer that question directly. But certainly the amount of aluminum for certain pHs can be a very reactive material.

MEMBER SIEBER: The phenomenon of storing things in containment is much better nowadays than it was in years past, mainly because of seismic issues. If you keep a gang box which is on wheels inside containment, you have to chain that two different ways to keep it from becoming a rolling missile during a seismic event.

MR. ANDREYCHEK: The other thing that I would suggest, Dr. Shack, about storing things like tarpaulins, or even plastic types of sheeting inside containment, that falls under most plants' FME programs, and is taken outside of containment. The reason things like scaffolding and gang boxes are held inside containment is to control spread of contamination. Once the materials that are being used become contaminated, it's convenient to keep them inside containment to control the spread of contamination, and therefore reduce cost associated with that. So there is some rational logic behind doing some of these things.

CHAIRMAN WALLIS: And all these ladders are aluminum. They're not magnesium, or something like that?

MR. ANDREYCHEK: They typically are aluminum, sir.

CHAIRMAN WALLIS: They typically are.

MEMBER SIEBER: They don't have many magnesium ladders.

MR. ANDREYCHEK: Yes, the magnesium ladders get to be pretty pricey pretty quick.

CHAIRMAN WALLIS: But price doesn't seem to be an object for nuclear plants.

MR. ANDREYCHEK: I can't comment without owning one myself.

MEMBER SIEBER: Quite the contrary. Especially now.

MR. ANDREYCHEK: Okay, so if we go to Slide 11, we were looking at establishing representative conditions, material types, and parameters, specifically. We started by looking at safety analyses and evaluations documents, FSARs, UFSARs, to get a baseline on information that we would have. I did put together an industry survey, and we submitted it through the Westinghouse owners group. And at that time, Westinghouse owners group and the CE owners group had already merged, and the BMW owners group also participated in this. We got excellent participation from those folks. So the survey results that we have is truly representative of the broad class of plants that are out there.

And the survey responses were the primary source of data in determining our parameters for that. We used the survey results over and above the FSAR since the FSARs are updated perhaps annually. These survey results came back as here's our current status, this is where we are.

VICE CHAIR RANSOM: These representative plants, do you also sample the latent debris? I'm wondering about materials that are used for personnel protection, like tapes, plastics. I don't know whether cottons and fiber on booties that people wear while they're in containments. How much of that stuff is spread around?

MR. ANDREYCHEK: Well, we didn't specifically try to simulate all of that material. But as a sidebar response to that, having performed walk-downs at several plants, and particularly going in, looking at as-found conditions for latent debris, I've been at site containment at Mode 3 operation looking for latent debris. Typically, we're not finding a whole heck of a lot. In one large dry containment that I ran a latent debris walk-down, we ended up with something on the order of about 80 pounds max.

VICE CHAIR RANSOM: So it's just dust, and latent debris?

MR. ANDREYCHEK: Different latent debris sources. Tape is being taken out, like if you want to call it duct tape. Duct tape is being taken out as a course of FME programs. That's not -- you won't find that in containment like perhaps you would 10 - 15 years ago. And in fact, it's part of the walk-downs that we performed. When we found tape, we advised our utility sponsor, and they pulled it out. So they're very serious about latent debris and trying to minimize it. The other plant was on the order of about 50 - 60 pounds of latent debris max.

MR. KLEIN: If I can add to that for a second. This is Paul Klein from NRR. We've had some discussions with screen vendors as part of the GSI resolution process, and they've presented a number of summaries of latent debris from plants that they've been working with, and those numbers have been consistent with or less than what we tried to model with an ICET test.

MEMBER SIEBER: I think that it's a mistake, though, to think that all the debris is going to be in a place where a person could go and clean it up. You know, you've got a lot of surface in containment, the tops of steam generators, the tops of shield walls, the polar crane, you know. A lot of surface where dust settles. And when you wash it down during containment spray operations, you're going to get as much debris off of those surfaces as you get off the floor.

I agree that plant operators are removing tape. One thing I note, though, is that they don't -- they seem to leave the tape that's used for radiological markings in place. They'll pull step-off pads, and you know, if you use a canvas step-off pad that's taped to the floor, they'll pull those out, but they will not pull down the signs. Some utilities have gone to metal signs, which I think is a better deal from the standpoint of debris generation. But I have seen tape in the form of radiological boundary markings inside containment fairly recently.

MR. ANDREYCHEK: I won't disagree, and I think they're becoming more aware of those kinds of sources. I think you're going to find less of it.

CHAIRMAN WALLIS: Maybe you should try to sample some of these areas which are hard to reach and are not normally cleaned and see if there isn't extra accumulation there.

MR. ANDREYCHEK: I've been on top of the polar crane at two plants, and I have sampled the debris up there. And I feel very comfortable in my statements. We included that in the amount of debris that we're calculating. So been there, and done that, and got the bejesus scared out of me because I'm not real fond of heights.

MEMBER SIEBER: I think another place to look if you want to find debris is underneath the reactor in both boilers and PWRs. Because it's a high radiation area. You don't send people in there for long periods of time. It's a nice collection place for miscellaneous floating things to get in there.

MR. ANDREYCHEK: That's true, but that also is an area that in most plants does not participate in the recirculation flow path. Because it is --

MEMBER SIEBER: Depends on where the break is.

CHAIRMAN WALLIS: Stagnant area?

MR. ANDREYCHEK: It's a stagnant area.

MR. HAFERA: This is Tom Hafera from NRR. We've identified latent debris as a key issue for GSI-191. And there's a lot of work going on in this area. And I think Mr. Sieber probably characterized it the best. What we're finding is all the plants are out now doing latent debris surveys in response to GSI-191, and they are surveying all areas in containment, those inaccessible areas, and the accessible areas, as required. And what we're really finding is there's been drastic improvements in plant cleanliness even in the last few years. A best example I can give you is a recent plant was just here, and they did a survey of their containment three years ago and estimated 160 pounds of debris in their containment, and they went through a major cleanliness program, and they improved their cleanliness program. Then they went back and reevaluated just during a recent outage, and they found 28 pounds. So they reduced it. They reduced it significantly. And when we talk to vendors, as Tim mentioned, he's had to do them, we talk to other vendors that are involved in that, they're all telling us that. They're all telling us the same thing, that you know, as little as three or four years ago there were plants that had problems with dirt, and dust, and tape, and labels, and all these miscellaneous things that were left in containment that are now no longer there because they're no longer permissible, because all the plants have realized that leaving things behind in containment is a bad thing. So they've all pretty much got religion, and the benefits are beginning to show of that.

MEMBER SIEBER: I think if you would go back to the Limerick event, and the amount of debris that was found in the suppression pool, I think the estimate was 1,100 pounds. So that gives you a contrast as to what improvement's been made by the industry in policing latent debris, particularly in suppression pools, and places like that.

I guess when I read through all the documents, I had to reread that, and re-study it, maybe even call a few people I knew and ask questions to justify the constituents and the amounts that you put into your test tank. Because I was going to come here and complain about it, but after I asked a lot of questions, and thought about it, I've decided not to complain. It seems to me to be pretty reasonable.

MR. ANDREYCHEK: Thank you. Thank you. If we could go to Slide 12. The thermodynamic simulations that were mentioned earlier were used to justify the 140 degree Fahrenheit temperature. For the long-term 30-day test. You had asked, I believe Dr. Sieber, about the vendors' use of coatings in the test. We did go to a vendor, and they did do what are called leeching tests for their coatings, epoxy-based coatings. And they got -- the reports that they got back, and this is from a qualified lab, suggest that they got nothing really leeching out.

CHAIRMAN WALLIS: So this was in what kind of environment?

MR. ANDREYCHEK: This was in an environment that was designed to be an attacking environment on the coatings. So they were using several different materials, several different solutions to look for leeching of chlorides and other types of organic --

CHAIRMAN WALLIS: Are they using boric acid?

MR. ANDREYCHEK: I don't know if they used boric acid --

CHAIRMAN WALLIS: And sodium --

MR. ANDREYCHEK: Hydrochloric acid. They used sulfuric acid. They used very corrosive high pHs to try to look for leeching of materials.

CHAIRMAN WALLIS: They used both high and low pHs?

MR. ANDREYCHEK: Yes.

MEMBER SIEBER: This sort of duplicates the original qualification test, you know. There are qualified coatings and unqualified coatings. What makes a coating qualified is not only the constituents, but how it's applied. Certain thicknesses, various layers. And if you look at the chemistry, most of these coatings were epoxy-based coatings. And they're basically as close to insoluble as you can get, other than what you can erode off of a surface. I had a concern about it, and because of that I did look into not only the test reports, but also the basic chemistry of what these so-called high endurance coatings are like. The bigger question is how many pieces of equipment are in the plant that were painted by some manufacturer someplace who did not use a qualified coating. And you know, if it wasn't in the purchase spec you didn't get it, because it's expensive and difficult to do.

CHAIRMAN WALLIS: There's also aging of the coatings.

MEMBER SIEBER: Well, there's aging, and it's also subject to ultraviolet change.

CHAIRMAN WALLIS: And radiation.

MEMBER SIEBER: And radiation. On the other hand, you know, there's sodium vapor lamps, basically, in containment, and most of them burn out between the outages. So the exposure isn't there. I have not seen in 30 years of being occasionally in and out of containment discoloration of the coating. I'm concerned about two things. One of them is unqualified coatings, and the second one, there's wear and tear. You know, you take a gang box on wheels and push it across a grading floor and it goes into a wall, it's going to take the coating with it. And everybody says, oh too bad, go call the painter and he'll go and repair that, but the chips are down in the plant someplace. I think the biggest concern is not so much to worry about chips that get knocked off, or to worry about a qualified coating flaking off, you know, to worry about identifying instances where unqualified coatings exist because there's a vulnerability there. It could be not substantial, but significant, and it could change the chemistry of your sump.

MR. GISCLON: EPRI within the last year undertook a research program to look at unqualified coatings specifically, and in doing so they've surveyed what types of unqualified coatings are out there, and in some cases in what quantities. And the second phase of the research was to actually look at the response of the unqualified coating to the qualified scenarios, including the time temperature and radiation. And there have been some mixed results coming back that have actually come back on that. Some are fairly robust and some aren't.

MEMBER SIEBER: I'm not surprised.

MR. GISCLON: And that's documented in our reports here.

MEMBER SIEBER: The problem is there's such a wide variety that it's hard to characterize some typical kind of non-qualified coating. It could be oil-based, it could be water-based, it could be good stuff, it could, you know, be the cheap stuff. So it's there, nonetheless, and the question is how much, and how much of it makes it to the sump.

MR. ANDREYCHEK: There was another consideration on coatings that we took into account, and that was that a lot of these coatings would not be submerged under water. They would be subjected to containment spray.

MEMBER SIEBER: Right.

MR. ANDREYCHEK: And the approach we took was that we'd know that some amount of major components inside containment, including containment shell, are coated with qualified coatings. And we went with an assumption at the time, when we developed the test plan, that the qualified coatings would stay in place, that the unqualified coatings were going to be removed sufficiently from the pool that they would not affect the chemistry of the pool for the reason that even if they failed, and they were subjected for several hours of containment spray, that would not be sufficient time to either move them down to the sump where they would be submerged, or that the four hours of containment spray would be enough time to leech enough materials from them that it would affect significantly the 30-day chemistry of the containment sump.

CHAIRMAN WALLIS: Was the sample the fluence test of these coatings?

MR. ANDREYCHEK: I'm not sure I understand.

CHAIRMAN WALLIS: Well, I know that --

MEMBER SIEBER: Radiation.

CHAIRMAN WALLIS: -- medical equipment which is sterilized by radiation in its polymeric construction degrades and falls apart eventually if you radiate it too much. Just trying to kill the bacteria. It's very sensitive. And I don't know what needs to -- I don't know if there's enough radiation. I'm just raising the question.

MEMBER SIEBER: Go ahead.

MR. ANDREYCHEK: I was going to say under the standard qualification programs, typically PWR containments that are considered qualified are subjected to anywhere from about 5 x 10^8 to 1 x 10^9 rads.

CHAIRMAN WALLIS: So it's done.

MR. ANDREYCHEK: Yes.

CHAIRMAN WALLIS: That answers my

question. Very good.

MEMBER SIEBER: I noticed in the test report results some statements that justified elimination of coatings from the mixture of debris on the basis of a statement that it's not transportable. Was that in -- did I read that right or not?

MR. ANDREYCHEK: I don't think that statement was in the test, but I'm not sure.

DR. JAIN: It's not in the test plan. It must be some other document.

MEMBER SIEBER: Okay.

DR. JAIN: That's not the basis for excluding it.

MR. ANDREYCHEK: No.

MEMBER SIEBER: Okay. All right.

MR. ANDREYCHEK: The basis was -- that was typically not a significant volume, two, it was not going to be -- the equipment was above the submergence level by and large, and three that because it would be subjected to containment spray primarily, like on the operating deck and so on, that we felt that we did not -- it was not that it was not transportable, but it would take a long time for it to get down to the pool, so it would not affect over a 4-hour period significantly the 30-day chemistry of the pool. And that was the rationale for it. Okay?

MEMBER SIEBER: Thank you.

MR. ANDREYCHEK: Thank you. Appreciate the questions. Let's go to the last bullet because I think we've talked about pretty much everything else. We have things that are reasonable representative in containment. The data sampling capabilities that were identified in the -- requested in the test plan satisfied the principal objective for characterizing the byproducts. That includes characterizing particulates, and doing grab samples, and so on.

If you go to Page 13, the scaling rationale was to maintain a ratio of volume to surface area -- surface area to volume of the various materials that's consistent with what we'd find in the plant. And I give an example in this slide.

If we go to Slide 14, we identify the test values, and the maximum survey values which are included in Section 5 of the test plan. Section 5 of the test plan includes part of the rationale for what we included, and then there's a list of tables in the back of what the materials are. Zinc, and in galvanized steel, that's probably just slightly under what the maximum survey value was. Zinc coatings, we actually upped it a little bit to cover ourselves.

Aluminum, again we went a little bit on the high side, but avoided the plant that you had mentioned, sir, that seemed to have an exorbitantly high amount. Copper, we again went a little high on that. Moving to Slide 15. Carbon steel. We found very little un-top coated carbon steel except on -- with the reactor vessel, which is 509 carbon steel, and the -- some plants have used carbon steel piping that's lined with an ethylnyl liner. So we modeled some un-top coated carbon steel, raw carbon steel, so we could get those products to allow for that. Concrete surface area, again, most concrete surfaces inside containment are indeed top-coated. But we modeled some that would be affected by a jet from the break that would strip off the coatings. So that amount of concrete would be subjected to the pool over that period of time. So we did model and allow for that.

CHAIRMAN WALLIS: This carbon steel is oxidized?

MR. ANDREYCHEK: We did not oxidize it deliberately.

CHAIRMAN WALLIS: Fresh carbon steel?

MR. ANDREYCHEK: Not fresh, but it was allowed to age naturally for a number of weeks.

CHAIRMAN WALLIS: Leave it out in the

weather?

MR. GISCLON: In the atmosphere. In a storage area. It wasn't hermetically sealed. The copper was actually artificially aged to produce an oxide layer on it.

MR. ANDREYCHEK: There was an attempt to try to be somewhat representative where we best could. Concrete particulate was based on the amount of -- sure.

MEMBER SHACK: In your carbon steel for your piping now, is this the total area of the piping, or you made some estimate of how much insulation would be removed in your worst case accident, and used that area?

MR. ANDREYCHEK: We took a look at the amount of carbon steel that we thought would be subjected to spray and to submergence. It was an estimate based on survey results.

MEMBER SHACK: But is this -- I mean, all that steel is covered with insulation in the plant.

MR. ANDREYCHEK: That's correct. Initially.

MEMBER SHACK: Yes. And so is it based on the total area of carbon steel, or the area of carbon steel from which you expect the insulation to be removed?

MR. ANDREYCHEK: The area of carbon steel that we would expect the insulation to be removed, as well as the reactor vessel proper. Because that was going to be most -- in most plants you're going to have the reactor vessel submerged, and you will get water up around the reactor vessel.

CHAIRMAN WALLIS: Does the insulation really protect the steel when it's left on? Doesn't it get soggy, and drips for awhile afterwards? Or the containment sprays come on. Does this insulation really protect the steel?

MR. ANDREYCHEK: Reflective metallic insulation is not going to get soggy. But if you have calcium silicate, there's a potential that it might, but it depends upon the plant, the design of the plant and how the steam generators are protected and secured. And it's whether or not they would be subjected to direct containment spray.

MEMBER SIEBER: The interesting thing about reflective insulation is if you get water behind it, it tends to, since it's hot there, it tends to evaporate and you get a concentration effect. And so the chemistry is altogether different behind mirror insulation than it would be if it was just running down the vessel.

CHAIRMAN WALLIS: So you have borated water getting trapped in there and concentrating.

MEMBER SIEBER: Yes.

CHAIRMAN WALLIS: And there's a certain temperature --

MEMBER SIEBER: Doing whatever borated water wants to do.

CHAIRMAN WALLIS: With a certain temperature and concentration it's pretty corrosive.

MEMBER SIEBER: Generally speaking, though, mirror insulation has seams all over the place. You may well be able to visually determine that something's leaking someplace by looking at the outside of the mirror insulation. Now it usually turns out that where the water, or the fluid, or whatever it is comes out is not where the leak is. It's somewhere below it. But I think all operators know enough when they see discoloration and staining like that to go and make some investigation, and take the insulation off, find out where the leak is. And you know, to do otherwise is against the code.

MR. ANDREYCHEK: Correct. The other thing I would suggest, looking at the phenomena and processes that are going on. When you have the containment spray actuating, and you do have the hot pipes, you will tend to get the behavior that you've identified. But after a short period of time, we've taken the fixed metal heat -- or stored energy out of the piping, and what you have is basically just moisture there.

MEMBER SIEBER: Right.

MR. ANDREYCHEK: From condensation.

CHAIRMAN WALLIS: That depends what's going on inside, too, doesn't it. In the accident there may still be a source of heat for those pipes.

MR. ANDREYCHEK: There may be a source of heat for some period of time, but again, looking at the specifics in areas, once you go on to hot leg recirculation, you stop the steaming, you'll get warming of the fluid, but you're not going to be getting steaming at whatever pressure. As a consequence they think you take away a lot of the heat sources. But around the reactor vessel you're still going to be somewhat warm, but you'll be at no hotter than the roughly saturated conditions at what containment pressure you're at.

MR. GISCLON: Speaking of insulation, and hot pipes, we did condition the fiberglass by heating it on one side for 24 hours to 600 degrees Fahrenheit which simulated service. And there is an organic binder that's in the fiberglass. And after a period of about 24 hours, most of that is driven off. I'm told by our friends in the insulation business that when they put this stuff on and do hot functional testing and heat the plant up for the first time that it drives people out of the containment.

CHAIRMAN WALLIS: Where does it go? Is it played out on other equipment?

MR. GISCLON: It vaporizes, and goes off.

CHAIRMAN WALLIS: If you're suitably ventilating the containment. Otherwise it deposits, presumably, on surface.

MR. GISCLON: Well, after. But this is just an initial reaction to the heating of the substance. We also preconditioned some of the calcium silicate insulation. And as you'll see in some of the slides that'll be presented later on, there's different colors of calcium silicate. There's a small amount of iron oxide in that material, and it changes from a yellow oxide form to a rose-colored form.

CHAIRMAN WALLIS: I understand there are different kinds of cal-sil too. They aren't all the same?

MR. GISCLON: They're not all the same.

The kind that we used is a typical kind that's used in quite a few applications today. It's Johns Manville super-tint gold. It was originally made by an outfit named Papco that -- it's actually fairly interesting stuff. I don't know if you want to get into that detail right now.

CHAIRMAN WALLIS: Yes, just I remember that there are different kinds, and I was wondering how you made it representative.

MR. GISCLON: We actually obtained some from a plant.

CHAIRMAN WALLIS: From a plant.

MR. GISCLON: They had it in the warehouse.

CHAIRMAN WALLIS: But some other plants might have different kinds of cal-sil?

MR. GISCLON: That's true.

MEMBER SIEBER: But they're basically the same stuff. If you look at the basic constituents

MR. GISCLON: The material had been in their warehouse for, you know, 10 or 12 years ago. So it's not something that was produced yesterday.

MEMBER SIEBER: They were probably eager to give it away. I did get the binder and fiberglass is resin-based, right?

MR. GISCLON: Yes, it is.

MEMBER SIEBER: Okay.

CHAIRMAN WALLIS: Tim, are you going to make your?

MR. ANDREYCHEK: Moving quickly.

CHAIRMAN WALLIS: You're getting there, I think.

MR. ANDREYCHEK: Page 15. The last item on there is insulation materials. We looked at the amount of insulation that would come off in a steam generator for the amount that would be submerged particularly. And that was the rationale and the basis for that.

Moving on to Page 16. Some of the materials would be submerged over the 30 days, some of them would not be. This is based on water level flood-up levels in containment. And again, based on the survey, we went through and identified what the percentages of submerged and unsubmerged materials would be, and they're listed here. If there's any questions specifically on that I'll try to address.

CHAIRMAN WALLIS: Well, you produced some hydrogen in these tests.

MR. ANDREYCHEK: That is correct.

CHAIRMAN WALLIS: Did it come from the

zinc, do you think? Or where did it come from?

MR. ANDREYCHEK: It could have come from zinc or the aluminum. My guess is on the high pH, my bet is it came from the aluminum more so than the zinc. And I think Bruce could address some of the hydrogen production issues associated when he talks about his data.

MEMBER SHACK: How much higher would the pool be when the sprays are actually on?

MR. ANDREYCHEK: Well, we looked at terminal pool heights with the sprays. I mean, the idea was to -- what's the maximum height.

MEMBER SHACK: No, no, the temperature.

MR. ANDREYCHEK: Oh, the temperature. Sprays typically initiate for large dry containment somewhere at a temperature of about 250 to 260 degrees Fahrenheit, and cool rather quickly after that. Long-term, you can -- at the end of the year or so you're down to, say, maybe 100 degrees Fahrenheit in the pool. But you're looking at maybe a couple of days, you're down to 140 or so degrees Fahrenheit which is the temperature we chose.

MEMBER SHACK: Right, okay. Now the containment spray is sort of room temperature when it starts? I mean, you're not pulling it out of the sump.

MR. ANDREYCHEK: Yes, you are.

MR. GISCLON: The initial spray in a plant is from the refueling water storage tank. It's ambient conditions.

MEMBER SIEBER: Well, there is a maximum for that, which is in the high 80.

MR. ANDREYCHEK: That's correct.

MEMBER SIEBER: Ultimately there is a heat exchanger that has as its cooling fluid service waters that whatever your heat sink is, that's the assumption for the temperature of the spray after you exhaust --

MR. ANDREYCHEK: Right. You can get about 25 - 30 degree delta T across the heat exchanger of the spray fluid. The same thing is true with the RHR, the ECCS fluid. You can get that kind of a -- early on, when you have a 260 or 250 degree entering fluid, you can come down 30 or so degrees across the heat exchanger. So you're dumping in maybe 230 degree fluid temperatures, both in the containment sprays as well as the ECCS fluid. And that's a rule of thumb number, not necessarily representative of any given plant. So what you're pumping in containment is actually cooler than the sump temperatures proper.

Moving on to the test loop. It was

fabricated, designed and fabricated. We have a sample tank, a pump, piping associated with it, and instrumentation, online instrumentation as well as the capability of drawing grab samples. We do, for operating capabilities, have the ability to spray for whatever period of time we choose to. And using four hours as our process.

CHAIRMAN WALLIS: So your material is going through all these pipes in your loop? Are these pipes that don't interact with the --

MR. ANDREYCHEK: The piping in these are stainless steel.

CHAIRMAN WALLIS: All the piping is stainless steel?

MR. ANDREYCHEK: That is correct, sir.

MR. GISCLON: Except for the spray pipe.

MR. ANDREYCHEK: Except, yes. You're right. Go ahead.

CHAIRMAN WALLIS: And there's some lubrication in the pump and so on? I mean, I'm just wondering if there's any chance of picking up stuff in the loop that might.

MR. LETELLIER: We've tried to examine that from a number of points of view, in particular cleanliness and preparation between tests so that there's no residual accumulation. But all of the circulation piping is stainless steel with the exception of the spray loops, which are chlorinated polyvinyl chloride. And they're a limited service component which could be -- we've actually performed leeching tests, but they could be easily replaced if we find a concern.

MEMBER SIEBER: I carefully read the test results, because I would have thought an interesting phenomenon was that we're trying to figure out whether you can pump this stuff with a sump in a real plant. So now you've set up a test apparatus that has a sloped bottom, and a drain, which you're pumping through a recirculation loop. So I'd look carefully to see if the pump ever failed, or got clogged up, because that would tell me right away that, you know, we're in deep trouble. And that never happened, or at least I didn't find it. Is that correct?

MR. ANDREYCHEK: The pump never failed. The pump has not failed.

MR. GISCLON: That's correct, but --

MEMBER SIEBER: And each test was 30 days?

MR. GISCLON: The loop in the pump suction is not at all meant to replicate a screen.

MEMBER SIEBER: Yes, I understand. Yes,

I understand that.

CHAIRMAN WALLIS: But if we're --

MEMBER SIEBER: That's --

CHAIRMAN WALLIS: -- in solution, then pumping it around the loop is going to create a reactor, which has turbulence and stuff in it, much more than the pool.

MR. ANDREYCHEK: That's correct. That would be correct. The loop capacity is 250 gallons. And the requisite sample coupons were installed in holders that were non-reactive, and put in the areas where they'd either be submerged or subjected to containment spray simulation.

Slide 18 shows the --

CHAIRMAN WALLIS: What do you do all the time you're sitting around waiting for 30 days?

MEMBER SIEBER: Get more data.

MR. ANDREYCHEK: I'm going to let Bruce deal with that, but typically you've got graduate students that are studying while they're waiting for the next sample to happen.

CHAIRMAN WALLIS: Okay. That's what it is.

MR. ANDREYCHEK: So, the Figure 18 shows the loop. Simply, on Slide 19, the operation began by filling the loop to the initial temperature, establishing the pH and boron concentration, and then installing the coupons, and then initiating the test as quickly as possible thereafter. So, simulate spray for the first four hours, and make up inventory as appropriate. Now, the reason you need to make up the inventory, obviously, is we're taking grab samples, and there's some volume that's being taken out. So it needs to be replenished, or we would be ending up with a dry facility if we're not careful.

MEMBER KRESS: This loop doesn't seem to have any heaters or coolers in it.

MR. ANDREYCHEK: Oh, there are heaters in there, sir.

MEMBER KRESS: Oh, I just -- I don't see them.

CHAIRMAN WALLIS: There must be.

MR. GISCLON: Two stainless jacketed Keller heaters. Titanium jacketed, excuse me.

MR. ANDREYCHEK: Basically, immersion heaters heat the temperature at the desired level. And the tank was insulated. And I think you may have seen photographs of it. Does have insulate shine, but there is some natural convective cooling that does occur. Not all the piping was insulated, as I recall, so you do get some cooling from the piping. So there is a need for the heaters.

Go to Slide 20. For the sodium hydroxide runs, we targeted an initial bulk pH of about 10 following containment spray. We simulated containment spray at a maximum of 12, pH of 12, over the first 30 minutes. And the reason the first 30 minutes is that's typically the time you draw down from the refueling water storage tank and borated water storage tank. And that's the pH of that volume. So we did simulate that to try to get that high pH on the materials that were not going to be submerged so that we would not only promote corrosion and chemical reaction, but also the washdown. And that was also scaled based on the volume of water in containment spray to the surface area.

CHAIRMAN WALLIS: Excuse me. When there's a break, the first thing that happens is borated water impinges on things.

MR. ANDREYCHEK: That's correct.

CHAIRMAN WALLIS: Do you do anything about that? Because it's a low pH initially, and then it gets to the sump and it becomes a high pH.

MR. ANDREYCHEK: Well, actually, the source of the sump fluid is borated water from the refueling water storage tank.

CHAIRMAN WALLIS: Which has a low pH.

MR. ANDREYCHEK: Approximately 4.5 or thereabouts. Now, if you have a look at end of life, your boron concentrations in the reactor is next to zilch.

CHAIRMAN WALLIS: That depends on when you have it.

MR. ANDREYCHEK: Exactly. Early in life you might be at around 1,800 or so.

CHAIRMAN WALLIS: But 2,800 is typical of early in life, is it?

MR. ANDREYCHEK: Say again?

CHAIRMAN WALLIS: You chose 2,800. That's the early in life?

MR. ANDREYCHEK: Actually, the 2,800 is typical of the refueling water storage tank.

MEMBER SIEBER: Which is always higher --

MR. GISCLON: The relative volume of the reactor coolant system is rather small in comparison with the tank.

MEMBER SIEBER: Now, when you get spray, there is a chemical addition process that goes on in some plants.

MR. GISCLON: That's true.

MR. ANDREYCHEK: That's the sodium hydroxide.

MEMBER SIEBER: Which alters the pH in the other direction.

MR. ANDREYCHEK: Correct, and that's up to 12.

MEMBER SIEBER: Right.

MR. ANDREYCHEK: And that's why we simulated approximately 12 with the --

CHAIRMAN WALLIS: You're simulating what happens in the sump. You're not simulating what happens when high velocity, high temperature, low pH borated water impinges on things.

MR. ANDREYCHEK: That's correct.

CHAIRMAN WALLIS: Which is presumably pretty reactive for -- it's not for a short time, but if you're firing this stuff at material, I could imagine that you'd get chemical reactions occurring.

MEMBER SHACK: Let's look at the process, and phenomena, and time scales. For a large break loss-of-coolant-accident, you typically have about 30 second blowdown for PWR. And within 30 seconds your containment sprays have actuated, and so you're beginning to throw containment spray into the containment, in the first 30 - 50 seconds. Less than a minute. Even if you argue that you've got a loss of offsite power, so you need to crank the diesels, and do the loading calculations. So within approximately 50 seconds, you've got containment spray which is at about 80 degrees maximum temperature that's being dumped inside containment.

CHAIRMAN WALLIS: Now, it's a question of time, but you have very high temperature for that short time. I don't know what the effect is. It could be --

MR. TREGONING: But if you think of the time scale, it could be 30 seconds.

CHAIRMAN WALLIS: Yes, but 30 seconds at 600 degrees might be as effective as 30 days. I don't know. I don't know.

MR. ANDREYCHEK: From the chemical reactions I've seen that have been shared with me by our chemists, that's not necessarily critical timing.

CHAIRMAN WALLIS: Someone has checked what sort of likely reactions that we'd expect from the LOCA fluid itself?

MR. ANDREYCHEK: For aluminum, and for zinc --

CHAIRMAN WALLIS: It doesn't dissolve the ladder, for instance?

MR. ANDREYCHEK: No.

CHAIRMAN WALLIS: Impinges on it?

MR. ANDREYCHEK: No, it does not.

CHAIRMAN WALLIS: It blows it away, but it doesn't dissolve it.

MR. ANDREYCHEK: It might be slightly displaced. But no, it doesn't -- it's not a critical item in terms of time, the amount of reaction, or how much it would actually react. So I think we're still, you know, from everything I've seen we're still conservative with the 30-day time period.

CHAIRMAN WALLIS: And the soggy stuff on a steam generator sitting there, being heated by the steam generator?

MR. ANDREYCHEK: The soggy stuff? Help me understand what you mean.

CHAIRMAN WALLIS: Well, I'm saying you've got insulation on the steam generator which is filled with borated water, and it's slowly oozing out like a wet towel dripping in a bathtub. That stuff is hot.

MR. ANDREYCHEK: A steam generator is typically sitting at about 500 --

CHAIRMAN WALLIS: So I'm wondering what kind of reactions you get in soggy insulation which is hot, sitting in --

MEMBER SIEBER: That would be an unusual plant that would not have mirror insulation on a steam generator.

MR. ANDREYCHEK: Well, let me look at it a little differently.

CHAIRMAN WALLIS: Are you going to look at that again some more?

MR. ANDREYCHEK: Well, let me look at it a little differently from this point. If you have the affected steam generator, typically, based on the models that we're using, we take basically all the insulation off the affected loop steam generator.

CHAIRMAN WALLIS: You assume it comes off.

MR. ANDREYCHEK: It's blown off. That's part of the 56 truckloads of insulation.

CHAIRMAN WALLIS: I know, but that's -- there are some LOCAs where the break aims somewhere else.

MR. ANDREYCHEK: Correct.

CHAIRMAN WALLIS: And I'm not really convinced that -- it's a nice regulatory trick, but in fact the jet may be aiming at containment, and the steam generator's over here. Even though it's close, it doesn't get affected.

MR. ANDREYCHEK: May be. May be. But

let's look at what happens to a generator that doesn't get impacted like that.

CHAIRMAN WALLIS: It gets soggy.

MR. ANDREYCHEK: Well, let's look at that, because it doesn't -- you don't have exposed calcium silicate without some kind of protective band on it. So you have at least a sheath around it. So will you get some water behind it? I can't say that you won't.

CHAIRMAN WALLIS: Well, yes.

MR. ANDREYCHEK: I won't say that you can't. But it's not being --

CHAIRMAN WALLIS: All you need is to perforate the sheet somewhere and pour water in, it'll stay in there.

MR. ANDREYCHEK: Well, it may.

CHAIRMAN WALLIS: Or try to get out eventually.

MR. ANDREYCHEK: Or it gets way down into the bottom of the steam generator and come out. I won't argue that. But it's not spraying unprotected calcium silicate or fiberglass.

CHAIRMAN WALLIS: But you haven't done tests of this sort of thing where you take insulation and pour in typical borated water, and keep it at typical temperatures --

MR. ANDREYCHEK: No.

CHAIRMAN WALLIS: -- and see what happens. Not in the sump, but up above, and then it drips down, and eventually contributes.

MR. ANDREYCHEK: It's a good question. We have not done that. I'm not saying that someone else might not have done it somewhere.

CHAIRMAN WALLIS: Maybe someone else might --

MR. ANDREYCHEK: I'm not aware of it, and we haven't done it.

MR. TREGONING: Again, one of the things that I think you'll see with the ICET results, it is the submerged material which has largely been the dominant contributor. It's not the unsubmerged stuff that had been at least in the first four tests, and I might speak out of turn here, but have been driving the reactions that we see. So these phenomena that you mention are potentially real, I don't argue that, but I would still argue in terms of the time scales and volumes that we're dealing with here, they're going to be inconsequential compared to what's happening within the submerged containment pool environment in terms of reaction product formation and corrosion.

CHAIRMAN WALLIS: I don't know. I don't know how fast things happen in that sort of environment that I have in mind.

MR. TREGONING: Well, ICET has provided us some good insights for that.

CHAIRMAN WALLIS: So you might want to look at this some more.

MEMBER SHACK: I have done calculations where when you look at the thermal history of the sump, and you take your activation energies for dissolution, and you know, their integrated values after a few days are really conservative for the kind of total. You know, with the transient that you really have versus the isothermal test that they're doing, they get about the same amount of dissolution after -- they're a little bit non-conservative for the first couple of hours, you know, and then it --

CHAIRMAN WALLIS: This is in the sump itself.

MEMBER SHACK: The sump itself.

CHAIRMAN WALLIS: All right.

MR. ANDREYCHEK: And that was one of our design parameters. That's what we had --

MEMBER SHACK: And again, you know, I think there's an enormous difference between the dissolution you get from cal-sil that's been blown apart, and so your surface areas are immense compared to cal-sil that's sitting there as a lump, and solid, behind that bin. I mean, you know, the effective dissolution that you get is really I think probably strongly controlled by the amount that's pulverized.

MR. ANDREYCHEK: But also understand, I take that you also have a question, Dr. Wallis, about perhaps the corrosion that might be going on behind the calcium silicate, that if you spray water for some period of time --

CHAIRMAN WALLIS: Borated water and it stays in there.

MR. ANDREYCHEK: So you've got some amount of stuff that's there, and typically, again, spray is about four hours. You might get some condensation, but the steam generators stay warm.

CHAIRMAN WALLIS: Well, the LOCA water too. It perforates the covering and goes in.

MR. ANDREYCHEK: Yes.

CHAIRMAN WALLIS: Okay.

MR. ANDREYCHEK: That's a valid question. I don't disagree with that. But the sodium TSP tests, the sodium -- tri-sodium phosphate tests mixed up a solution and injected it into the chamber for a target pH of approximately 7. For sodium tetraborate, we're going to start with trying to get a target boron concentration of 2,400 ppm by mixing boric acid and sodium tetraborate. Sodium tetraborate has a concentration of about 2,100 ppm in its solution as it's maintained in an ice bed of an ice condenser plant. Again, the 2,800 ppm comes from the refueling water storage tank. Mix the requisite solutions of both until you come up with 2,400 ppm. Based on calculations that we've done, we would expect the pH to be something on the order of about 8.3. So 8.0 to 8.5 is the range we would expect it to be in, but very closely I would estimate about an 8.3. And the pH target values in initial run test conditions. We made no attempts to try to modify or correct the pH over the course of the 30-day period. We let it go to where it was going to go to.

Slide 21, data collected. So the operating parameters were recorded at predetermined intervals. We looked at and recorded pH, pump speed to make sure that things were still moving along quite well, the liquid temperature to maintain the constant temperature level, and loop flow to make sure that we still had a good flow rate, the desired flow rate running through.

Grab samples were taken, again, at predetermined intervals. We took them a little more frequently during the early part of the test, a little less frequently as the test went on. We figured early in the test things were going to happen more quickly. What we were looking for in the grab samples were precipitants, sedimentation, we looked at some fiberglass samples also to look for the materials that might be forming in there. We looked at viscosity measurements as an indication.

CHAIRMAN WALLIS: I think your report calls it kinematic viscosity, which is actually mu over rho. I think you made it mu, the dynamic viscosity.

MR. ANDREYCHEK: That's correct.

CHAIRMAN WALLIS: So it's just a wrong word you used.

MR. ANDREYCHEK: That's correct. But looking to viscosity measurements to look for non-Newtonian behavior.

Conventional chemical analyses were performed on periodic grab samples. Again, we're looking at pH. Went to a qualified lab for that. There was test materials or test equipment measured the pH in the loop that were recorded pretty much online, sent the grab samples to the labs for conductivity, turbidity, viscosity, and pH, total suspended solids, and particulate size distribution as we could, and precipitants and dissolved species of various elemental products. Pretty much the standard --

CHAIRMAN WALLIS: What's the source of potassium in this thing?

MR. ANDREYCHEK: There wasn't -- we didn't necessarily know there would be one. We wanted it checked anyhow just to be on the safe side.

CHAIRMAN WALLIS: Old bones left around or something?

MR. ANDREYCHEK: You never can tell. Grad students do funny things with chicken bones, you know?

MR. GISCLON: Same thing with lead.

MR. ANDREYCHEK: Yes. I mean, the idea was if we didn't do it, we wouldn't know.

CHAIRMAN WALLIS: That puzzled me in your plan. You were looking for things that you didn't seem to have put in there, and I wondered --

MR. TREGONING: Crushed concrete.

CHAIRMAN WALLIS: Concrete could be, yes.

MR. TREGONING: And soil.

CHAIRMAN WALLIS: Lead is --

MR. ANDREYCHEK: The initial plan was pretty broad in terms of the species evaluated. We obviously narrowed it down once we saw the types of things that were really.

CHAIRMAN WALLIS: It would be interesting if you found something you never put in, though.

MR. TREGONING: Lead can also be a contaminate in the inorganic zinc coatings.

CHAIRMAN WALLIS: Okay. Thank you.

MR. ANDREYCHEK: So --

CHAIRMAN WALLIS: Lead was raised -- the question was raised by one of your consultants on the first report, wasn't it? The effect of lead. I think I remember one of your consultants.

MR. TREGONING: One of the peer reviewers?

CHAIRMAN WALLIS: On a small scale, although it raised a question, lead being important. And mercury I think was also raised. There's no mercury in this at all?

MR. ANDREYCHEK: No. There was none by design, let me put it that way.

CHAIRMAN WALLIS: Not in any of the zinc coatings? There's no mercury there?

MR. ANDREYCHEK: No.

MEMBER SHACK: You really want to create

a problem for them, add some mercury to these solutions.

MR. TREGONING: We did ask the question -- excuse me, during formulation of the test plan, there were a number of questions about trace elements that could serve as catalysts in the chemical system, and it was reviewed to look for.

CHAIRMAN WALLIS: Right. So you're almost there for 10:15 arrival?

MR. ANDREYCHEK: I'm trying hard. Page 22, summary. A test plan has been developed under the NRC/EPRI memorandum of understanding for cooperative research. We did base the survey on industry data. Thermodynamic simulation study funded by and conducted under the guidance of NRC. And facility does achieve the test objectives that we believe. And we've looked at the data so far that we've been able to extract from the test, and we believe there's significant information regarding the formation, or the absence of formation, of different types of materials. We think it's a very successful program.

CHAIRMAN WALLIS: Now, thinking back with the previous test, you have some consultants reviewing this. Do you have the same ones you had before, or do you have new?

MR. TREGONING: We'll get to that. We've started forming, or we're in the process of forming --

CHAIRMAN WALLIS: You're forming it now? I would think you'd form it at the beginning so that you don't miss something.

MR. TREGONING: There was an initial review group that was formed in the beginning. Unfortunately it had to be disbanded for -- there was some contractual conflict of interest considerations. So we have re-formed a peer review group, and I'll talk about it a little bit later. They're not going to be just looking at ICET. They're going to be looking at the whole area of chemical effects in general.

CHAIRMAN WALLIS: They're useful for detecting places where you might have forgotten something, or missed some phenomenon.

MR. TREGONING: Right. And we'll use them to inform our follow-on testing that we're doing now.

CHAIRMAN WALLIS: Right. Does the committee have questions before we break? Well, that was very informative, and very straightforward.

MR. ANDREYCHEK: Thank you.

CHAIRMAN WALLIS: I think you did a good job of presenting. Thank you.

MR. ANDREYCHEK: Thank you.

CHAIRMAN WALLIS: And we're going to meet again at -- well, my watch says 10:15, so we'll meet at 10:30. And it will be 15 minutes from whatever it says on that clock there. If I had a gavel -- well, we're going to break for 15 minutes.

(Whereupon, the foregoing matter went off the record at 10:16 a.m. and went back on the record at 10:36 a.m.).

CHAIRMAN WALLIS: We are coming back into session. We're about 15 minutes late. We'll catch up. We'll try to catch up. Okay, let's get going. We're all ears.

MR. TREGONING: Professor Wallis, what we have next is a presentation by the prime contractor, Los Alamos National Lab, Bruce Letellier. Also at the table are Kerry Howe from the University of New Mexico where the tests are actually being conducted and carried out, and Marc Klasky down at the end from LANL as well. Bruce has really combined two talks and two topics into one talk. So what we will do here, it's a very long presentation, so we'll get started, and we'll go until you guys have had enough and you're ready to break for lunch.

CHAIRMAN WALLIS: We plan to break at

12:00 noon.

MR. TREGONING: What I'm saying, there's no real demarcation between the next two topics, so we will just --

CHAIRMAN WALLIS: I see. That's a very good time for us because we have some other things set up.

MR. TREGONING: Okay, so we'll make sure we adhere to that. Bruce?

MR. LETELLIER: So it's my pleasure to provide the entertainment for the next two hours. This talk is divided into two segments. The first is about 15 slides. It tries to give you an operational context on implementation of the test plan that Tim provided.

CHAIRMAN WALLIS: Did you staple these things backwards?

MR. LETELLIER: Yes, that was Rob's idea.

MEMBER KRESS: Just to give us a test.

CHAIRMAN WALLIS: So if we drop them on the floor you'll know why.

MR. LETELLIER: I apologize for that. Hopefully we'll give you sort of a photographic walking tour of the facility so you can get a hands-on perspective of how this loop works, and how the test plan was implemented at the University of New Mexico.

The first slide lists our staff resources that are working on ICET. It's my privilege and responsibility to represent a team of subject matter experts. In particular I want to recognize Jack Dallman, who's our day to day principal --

CHAIRMAN WALLIS: I notice he investigates "principles"?

MR. LETELLIER: That was an overzealous word check, spell check last night.

MR. TREGONING: Between my stapling and your grammar we're not getting off on the right foot.

MR. LETELLIER: Jack is really responsible for day-to-day management and performance of the test series. We have quality assurance staff on the team for both administration of our existing plan, and also internal auditing. We have three graduate students who have helped us from the inception, design construction and daily operations. And additional engineers as needed at LANL. Our chemical consulting expertise is provided by Steve Chipera, who is assisting us with chemical characterization, the various diagnostics that you see. Mei Ding is a lady and geochemistry expert in colloid formation and transport. She's helping us with particle sizing.

CHAIRMAN WALLIS: These are all University of New Mexico people?

MR. LETELLIER: These are all LANL.

CHAIRMAN WALLIS: Not all people.

MR. LETELLIER: Yes. And to my far left, Marc Klasky is a recent addition to our staff who's trying to diagnose the chemical system, provide some modeling assistance, and interpret the data. Editorial staff is not an insignificant contribution. We will be publishing five test reports in the next six months, including a cover NUREG. At UNM Kerry Howe is also investigating "principles" at the University of New Mexico, with a background in water chemistry.

MEMBER KRESS: You're consistent at any rate.

MR. LETELLIER: That's right. That's what global changes will do for you. Steve Cabañas is assisting us with chemistry, with specialties in inorganic environmental transport. We have a post doc on staff, Dong Chen, who's in charge of daily sample analyses. We have consulting services of Jeff Brinker and post doc who, Jeff is a world recognized expert in solgel formation, and providing laboratory for shear rate viscosymmetry.

CHAIRMAN WALLIS: Planetary physics looks good. I mean, if the phases of the moon and all that matter anyway.

MR. LETELLIER: There's actually an Institute of Meteoritics, and so they have high capability for SEM and mineralogy analysis. So I showed this to you to illustrate the depth and breadth of the capabilities that we brought to the project.

The next slide, Page 3, is just a reminder of how this project started, based in the results of the small-scale chemical circulation loop. And as mentioned before, we artificially induced some chemical products to form, and we observed very high head losses that would occur. However, we never made the operational connection between the accident initiation, the corrosion, and the formation. That was the motivation for an integrated test approach, which we're conducting now.

Page 4, the timeline illustrates the aggressive schedule that we've been under. Conceptual design was reviewed and approved by the initial peer review panel in last summer, June of '04. We commenced design and fabrication through the summer, and began our assembly and shakedown of the apparatus in September/October. Our first test was initiated mid-November, and was completed before Christmas last year. Since that time, we've executed three additional one-month tests. That accounts for the transition time between tests. And we hope to start our fifth and final test next week. That is the final test that's defined under the existing MOU.

Moving quickly to the next page, some of the programmatic attributes that we've tried to control in executing the test plan are -- one key issue is quality assurance. The requirements for QA are specified in the test plan. We developed a project quality assurance program manual, a QAPM, specifically for this activity. That QAPM is implemented through a set of procedures, and specifically project instructions that define the requirements and guidance for implementing key steps in data acquisition and analysis. All of our project personnel are trained to follow these requirements, and they're thoroughly familiar with them. I'm spending some time on this because it's rather unusual for a university-level contractor to attempt to accomplish this. It's more of a production-level research environment than an exploratory research environment, something we're not accustomed to.

All of the QA documents are reviewed and approved by both our NRC and our EPRI project managers. During the conduct of the first test last December, the Office of NRR actually conducted a QA implementation review, and had specifically seven recommendations that were addressed through a set of corrective action reports. I think it's important to note that we do have a process for addressing and disposition of QA findings. I think that while there are no specific standards that we're being held to, we're making every attempt to satisfy the intent of an Appendix B program. Part of our QAPM development was to assess which of the criteria are applicable to our experimental test activities, and to attempt to enforce that level of quality assurance. We strive for continual improvement through the oversight of the industry, and through our own internal QA processes.

One aspect of QA is to maintain the enormous amount of data that we're generating. We currently have an electronic database of over 3,000 images through Test 4, actually. And simply inventorying, tagging, and tracking those, and disseminating them for general use --

MEMBER KRESS: Continual improvement sounds like a managerial-ese. What's the measure of improvement?

MR. LETELLIER: I think continual improvement is implemented or effected by having external oversight and responding to the recommendations that were --

MEMBER KRESS: Responding to the recommendations.

MR. LETELLIER: That's right. That's the measure.

MEMBER KRESS: Well, the other measure is what they say needs to be done, and how well you accomplish that.

MR. TREGONING: There's another aspect to this. I mean, there are lessons that are being learned as we run prior tests, and one of the things we've tried to do is incorporate the lessons learned in earlier tests, in conducting them, and updating the procedures and some of the other quality assurance provisions to reflect those lessons.

CHAIRMAN WALLIS: But the goal isn't improvement by itself, because you could easily achieve that by starting out very badly.

(Laughter)

CHAIRMAN WALLIS: The goal must be to meet some good objectives right away.

MR. LETELLIER: When we did the audit, we audited the program with respect of Appendix B requirements, and the findings --

CHAIRMAN WALLIS: I understand that. That's very good.

MEMBER KRESS: But if you're already perfect, how do you have improvement?

CHAIRMAN WALLIS: That's right.

MR. LETELLIER: And I will say that any QA audit, by definition will find something.

CHAIRMAN WALLIS: Something, okay.

MEMBER KRESS: You're right.

MR. LETELLIER: Somewhat. You can't do a home inspection. Slide Number 6 explains the project instructions which are really the heart of implementing a test. At this level, this is where we modify or tailor fit the activities to match the intended test. For example, for each test we had to modify the initiation procedures to best match the accident environment, or the pH control system that we're trying to imitate. These step by step PIs are approved, and they were approved and in place for the first test as listed. And we have a signature authority chain before test initiation for any modifications. For example, we control the data acquisition system. There is a PI for receipt of coupons, the preparation of inspection and storage, management of those physical sample items. There's PIs for pre-test operations, including cleaning, quality standards, and chemical addition for test initiation. During test execution there are PIs for loading the coupon racks, for spray introduction, and for standards of monitoring during the test. And then we have a number of PIs in place for sample and analysis activities, including the schedule, the type, and the number of samples that will be taken. Keep in mind that there's a large aspect of project risk assessment. We're investing 30 days of project time. We don't want to have a failure in mid-course that would cause us to repeat the test. There are PIs for post-test operations, rack unloading, drainage, sample recovery. It's important to note that our data acquisition system does have an automated alarm. There are alarm settings on various attributes of flow rate, thermal couple response. And those are patched into an automatic paging system so that we can be onsite.

MEMBER KRESS: Just to be sure your data acquisition system is actually acquiring data?

MR. LETELLIER: It's functioning, and it provides a continuous data record.

CHAIRMAN WALLIS: I notice in your logo that your lab had ideas that changed the world, but they stopped in 2003.

MR. LETELLIER: That's true. That was the limit of our creativity as a contractor.

CHAIRMAN WALLIS: So now you've got a picture of the apparatus?

MR. LETELLIER: Yes.

MEMBER KRESS: Are you guys still University of California?

MR. LETELLIER: We are, at least for the next six months until resolution of the contract competition. Let's see, Page Number 7 provides your first --

MEMBER KRESS: How do you measure your viscosity these days? We used to use gel through a thing and time it.

MEMBER SIEBER: There's a picture of the device.

MR. LETELLIER: We use a capillary viscosimeter, and that's a -- we extract a water sample, and that's a bench-top exercise, which Kerry can explain in one of the following slides.

MEMBER KRESS: I just wondered if they had a probe --

MR. LETELLIER: We do not. Frankly, I don't think we trust an online viscosymmetry reading. Some of the basic physical attributes. You can see that this tank is designed to hold 250 gallons, which fills the tank up to the lower seam. It's about one-third full of water. That's where the submerged coupon rack resides. The water is introduced at -- here, I have a pointer for you to follow on the television screens.

CHAIRMAN WALLIS: Now, these coupons don't touch each other, whereas in the reality they do. Doesn't that make a difference?

MR. LETELLIER: It very well may. It's very difficult to estimate the amount of electrical conductivity between structural metals in containment. So our preferred approach was to isolate them. You can see that the metal coupons are standing in a plastic pipe arrangement of CPVC pipe.

CHAIRMAN WALLIS: But again, to look at -- might still, where you have steel pipe connected to copper pipe, after awhile one of them disappears.

MR. LETELLIER: That's true. We've examined these plates for cross-electroplating following the test, but we've not intentionally introduced that effect.

CHAIRMAN WALLIS: It's probably a long-term effect. I don't know. I just wonder about it.

MR. TREGONING: One of the difficulties with galvanic effects, again, it's going to be very dependent on plant-specific layout. There's not one specific material combination that we would couple that would necessarily be representative. One of the things that we're looking at in follow-on research activities is how galvanic effects may influence.

CHAIRMAN WALLIS: Okay. So you're bearing it in mind for future tests.

MR. LETELLIER: We're certainly bearing it in mind that it's a potential phenomena that we at least need to address and see how much of an issue it may be.

MR. ANDREYCHEK: Dr. Wallis, when we developed the plan, we looked at the galvanic corrosion effects, particularly through the corrosion experts at Westinghouse, and we determined that it was a very long-term effect. And over the 30-day period that we'd be looking at, given the limited amount of contact.

CHAIRMAN WALLIS: Right, I think I saw that.

MR. ANDREYCHEK: We would not have a major issue. So that was the reason for not trying to model it in this test.

MR. TREGONING: But the effect over 30 days will certainly be a function of the electric potential of the given materials in contact. So if you had two materials that were very dissimilar, it could potentially have an effect over that amount of time. So I don't want to go out on a limb. I don't think it's a big issue here, but again.

CHAIRMAN WALLIS: Right.

MR. LETELLIER: Some of the other physical attributes of this tank are the polycarbonate view ports. One window is provided below the surface of the water, and one window is provided above the surface. There is an additional view port in the lid of this tank, which is placed after the coupons are loaded. Water is drained from the bottom of this tank. It has a sloped floor, so it's a central drain, and it's recirculated upwards through the diagnostic loop.

CHAIRMAN WALLIS: Presumably, excuse me, you have a lot of gaskets and things, and seals that are materials which are inert?

MR. LETELLIER: They were tested for leechability concerns. One feature of this piping system is the ability to valve off or isolate the diagnostic systems for maintenance. And in fact we have had flow meters fail during the test, but we weren't forced to interrupt the flow at any time. We've also designed to the --

MEMBER SIEBER: The flow meters, as I recall them, were turbine type flow meters?

MR. LETELLIER: That's right, and I have some photographs of the reasons for failure, which were associated with calcium silicate accumulation. I think Tim mentioned that one requirement, one specification of the MOU was a requirement or a request not to measure head loss effects in this particular series of tests. However, we have provided for blind flanges at the far right to add a circulation loop for that purpose, if needed.

The Plexiglas blocks on the left side is for a visual water level indicator, essentially a manometer, that's calibrated so that we can add water as needed to account for sample extraction and evaporation. The sprays are introduced at the top. In each corner there is a spray nozzle. That's what the gray pipe provides. And keep in mind that there's only one coupon rack that's submerged. There are six racks that are suspended in the vapor stage.

So on the next page 8, you can see if I've missed anything. The submersion heaters have already been mentioned. There are two redundant heaters, each 3.5 kW titanium jacketed elements. The heating requirement for convective loss is about 1.2 kW. So we have quite a bit of additional capacity for controlling temperature. However, we do not have any heat rejection system. So we cannot simulate a rapid transient.

External thermal insulation is applied, and the previous picture does not show that. We have three thermal couples at various locations in the pool. On the first test we learned that there's a very small variation between them, and so at this point we consider them a redundancy. There's about a 1 degree C drop in temperature around the circulation piping. So we consider it to be a highly isothermal system which is a well controlled test condition. However, it may not replicate a heat exchanger. There are circumstances in the plant that would not follow this, for example.

MEMBER KRESS: What are you taking out with your reverse osmosis? Is that chlorine?

MR. LETELLIER: We're processing municipal --

MEMBER KRESS: It's municipal water, so you get all that chlorine and fluoride.

MEMBER SIEBER: All the dissolved salts.

MEMBER KRESS: The dissolved salts out of it that way.

DR. HOWE: Yes, the requirement in the test plan was to provide the initial water being below 50 microsiemens. We're using a reverse osmosis to produce that, and it's about 5 microsiemen water that we're starting as a baseline before we do the chemical addition.

MEMBER KRESS: You chose this instead of just vaporizing the water, and collecting the condensate?

MEMBER SIEBER: Right.

DR. HOWE: Yes.

MEMBER KRESS: Because you had some reverse osmosis equipment available?

DR. HOWE: Yes. In terms of producing deionized water, or demineralized water for the test, the choices would be distillation, ion exchange, or reverse osmosis. We had the reverse osmosis readily available of the capacity that was needed for this test, so.

MEMBER KRESS: Is that for some desalting studies you do?

DR. HOWE: That's right.

MEMBER KRESS: Just curious.

MEMBER SIEBER: And also it supplies drinking water.

MEMBER KRESS: Wouldn't want to drink that stuff. It doesn't taste good.

MR. LETELLIER: The very last bullet on Page 8 itemizes some of the additional project risk management attributes, where we have emergency power, backup generators, an additional backup pump onsite at all times. We have duplicate data storage, and again, the valve isolation of the diagnostics.

So I'm very pleased with how robust and functional our equipment has actually been. It's rare that you assemble and turn on a system and actually have it perform successfully the first time.

MEMBER KRESS: Yes, tell me about it.

MR. LETELLIER: There are issues that we continue to discuss about how well or how well we are not able to simulate the initial hours of the accident environment. There are compromises to be made. Again, the scaling parameters that we tried to achieve were the proportionality between surface area, corrosion potential, and the dilution volume of the tank.

VICE CHAIR RANSOM: Coupons are replaced at each of the five tests?

MR. LETELLIER: Oh, yes. Yes. It's a complete exchange of sampled items. Slide Number 9 sort of steps through the initiation of a test. In the upper left is an illustration of the gantry crane that we use to load the racks. They're approximately 120 pounds of metal each. In this photo you can see the external thermal insulation that's been applied to the tank. In the lower left is the submerged coupon rack after the pool has been filled with 250 gallons. At this point, the water's very turbid, so I would expect that the latent debris has been added. All of the baseline chemicals have been introduced. And this figure, you can also see one of the flow distribution headers. There's one on each side of the tank. There are holes aligned to direct the flow across the coupons. And there is one on each side of the tank.

CHAIRMAN WALLIS: Does this latent debris settle out in the tank and stay settled out?

MR. LETELLIER: It does. Some of the larger granules obviously fall to the bottom immediately. But of more interest is the water clarity after as little as 24 - 36 hours. There's a agglomeration that occurs, some charged neutralization effects, that Kerry, you may wish to speculate on the reasons for that. We are obviously immediately adding metallic ions, which is a common coagulating agent added to municipal water quality control.

In this lower photo on the left you can also see the insulation blankets, some of them, that have been wrapped around that lower coupon rack. The fiberglass insulation, as John explained, it's preheated to imitate the service life. It is shredded to imitate debris generation process. And then at this point, we try to encapsulate it in stainless steel mesh in order to keep it out of the pump, the pumping system. So all of our fiber is encapsulated in stainless steel mesh. That doesn't mean that there's not fugitive fiber that we find in the sediment at the bottom, and there is some amount of material that passes through the loop.

CHAIRMAN WALLIS: Presumably the cal-sil works its way out, when it's in the --

MR. LETELLIER: I have a figure later on that shows you both the pulverized cal-sil as a sludge, and also the larger blocks that we place in the stainless steel mesh.

At the upper right is a picture of the

center level coupon rack. And three more of these racks are placed in parallel on these supporting angle irons. So there's a total of six racks that are in the vapor space. One thing to keep in mind when you think about sprays, and condensation, and return paths is that this tank is highly isothermal. And so in my opinion, there's very little condensation on these plates. We do see droplet formation on the polycarbonate window, which is in the lid, and obviously there's some dripping that occurs. Some of these upper racks are also wetted when we return some of the sample volume back into the tank. For example, when we purge the sample line, that volume is not discarded, it's returned to the tank.

But once the lid is in place, the only visual access we have are the windows. And the panel in the lower right shows you the center level window. This is above the water level. And the plates are right up next to the wall. You can see the different, the copper versus the aluminum versus the plates that are coated in inorganic surface coatings. And it's been very interesting to note the differences in corrosion between the four tests.

VICE CHAIR RANSOM: Each of the racks contains the same set of coupons?

MR. LETELLIER: Let me say this. Each of the five tests contains exactly the same loading. The proportionality of the coupons has been distributed more or less randomly.

VICE CHAIR RANSOM: What about the underwater versus above the water?

MR. LETELLIER: Well, those are apportioned in direct proportion to the containment environment, according to the recommendations of the industry. For example, only 5 percent of the aluminum is actually submerged. You've been asking questions about the high inventories of aluminum.

CHAIRMAN WALLIS: It doesn't look round. It looks as if the stuff were the same color. It's actually together.

DR. HOWE: It is. The coupons are pretty evenly distributed between the racks. To the extent possible --

CHAIRMAN WALLIS: Yes, but in a rack, there are sort of four copper, and so on.

DR. HOWE: They are in a particular pattern. That pattern is recorded ahead of time. So that certain coupons are in certain locations, and that location is identical from test to test. That being said, the six upper racks are not entirely identical. For instance, if we had nine copper coupons, six of the racks -- or three of the racks would have one, and three of the racks would have two.

MR. LETELLIER: One final item to note. The submerged rack on the far right is a thick slab of concrete, which represents the exposed structural concrete. That is present in addition to the crushed material that represents an ablation source.

CHAIRMAN WALLIS: So you have totally submerged rack, and totally above. You never have partially submerged.

MEMBER SIEBER: No.

MR. LETELLIER: That's true.

CHAIRMAN WALLIS: Things often happen at the water line that are not typical. At the water line, you have air and water together.

MR. ARCHITZEL: That's true. We have not intentionally introduced that aspect.

CHAIRMAN WALLIS: Things have bridges at the water line that don't happen deep down, and don't happen in the air.

MR. TREGONING: Well, but by the same token, all our unsubmerged racks are relatively close to the water line, considering how you would actually have the situation in containment.

CHAIRMAN WALLIS: Yes. But you don't -- you could've had one partially submerged or something.

MR. ARCHITZEL: So, just to make it clear, to reiterate that after the 4-hour spray phase has ended, then thereafter the water circulates through this lower pool only.

VICE CHAIR RANSOM: Are there some coupons that are only in unsubmerged locations versus the submerged?

MR. LETELLIER: Yes. In fact they are discrete --

VICE CHAIR RANSOM: Not just a matter of how many, but there are some coupons that are not in the submerged that are in the --

MR. LETELLIER: Concrete is the only one that comes to mind.

VICE CHAIR RANSOM: It's in the submerged, but not in the vapors.

CHAIRMAN WALLIS: You have a concrete coupon?

MR. LETELLIER: Yes.

MEMBER SIEBER: Concrete block.

MR. LETELLIER: It's visible here on the submerged rack to the far right, as the very thick gray item.

CHAIRMAN WALLIS: Oh, I see. Okay.

MR. ARCHITZEL: That's the only sample type that's unique to the two locations.

VICE CHAIR RANSOM: And the rest of it's just a matter of how many coupons are distributed.

MR. LETELLIER: Proportion, that's right. And that was a conscious decision because of our estimation that concrete in upper containment is coated. It only experiences the spray phase. It's not immersed for long periods of time.

MEMBER SIEBER: Did you prepare the concrete sample yourselves?

MR. ARCHITZEL: All of the sample material was provided by the industry. Maybe John would like to add some information.

MEMBER SIEBER: You know, safety grade concrete is different than what you pave your driveway with.

MR. GISCLON: The concrete material was procured by EPRI's facilities in Charlotte. And the cement that was used in that concrete was the same type of cement that you'd use in safety grade large pore applications.

MEMBER SIEBER: In the same proportion?

MR. GISCLON: And in the same proportions with --

MEMBER SIEBER: Usually it's pretty heavy on cement and lighter on aggregate.

MR. GISCLON: Right.

MEMBER SIEBER: Okay, thanks.

MR. GISCLON: You're welcome.

MR. LETELLIER: Continuing to Page 10, this is simply a reminder of what the test matrix looks like. You can refer to that as we talk about the differences between the pH control system, and the primary insulation or debris type. We slip into the nomenclature of Test 1, Test 2, Test 4, but this is for your reference so that you can recall what the combinations are.

The sequence has evolved somewhat over time. We started with the high pH sodium hydroxide system, 100 percent fiber. I think originally we had intended to look at a cal-sil test, but for various reasons that was delayed because of the practical problems concerned with handling that material. And again, the schedule is provided. We should be finishing Test 5 by the end of August.

Figure Number 11 itemizes the commonalities that exist between the five tests. And Tim has already provided some of these. Just for your information, there are 373 metal coupons of mixed type, and one concrete slab. Test temperature is constant. Pressures are ambient. Circulation flow rates are constant. And then the background chemistry of boron, hydrochloric acid, and lithium hydroxide are the same for every test. The differences are introduced by which pH control system that we're investigating. Test 1 and 4 had sodium hydroxide for a target pH of 10. Tests 2 and 3 add the TSP, tri-sodium phosphate, for a target pH of 7. And Test 5 will combine 100 gallons of the baseline chemical with an additional 150 gallons of 1.8 percent sodium tetraborate.

CHAIRMAN WALLIS: How does the water get into the tank? It comes in through a pipe.

MR. LETELLIER: If we go back to the figure.

CHAIRMAN WALLIS: Which one shall I look at?

MR. LETELLIER: Page 7, for example.

CHAIRMAN WALLIS: Yes, one of those pipes is just squirting into the tank, is it? Or what is it doing?

MR. LETELLIER: It's introduced through the drain port at the bottom.

CHAIRMAN WALLIS: Introduced through the drain at the bottom as a jet?

MR. LETELLIER: No, it's filled very slowly as it's produced by the reverse osmosis unit.

CHAIRMAN WALLIS: But you say flow velocity of submerged coupons is, say, 3 centimeters a second. That's over a big area. Flow velocity in the pipe is much bigger.

MR. LETELLIER: I'm sorry. You're asking -- maybe you're asking two separate questions.

CHAIRMAN WALLIS: The circulating water goes around the loop, then it comes out of a pipe and goes into the tank. Is that what I see on the side of the tank there, that pipe that's squirting into the tank?

MR. LETELLIER: Let's look at Page Number 9, where there's a good picture of the submerged sample rack. Yes, in the lower left you can see the distribution --

CHAIRMAN WALLIS: I can't see anything.

MR. LETELLIER: If you look at the screen you can follow the arrow. This is the distribution --

CHAIRMAN WALLIS: Is it some sort of a manifold thing that --

MR. LETELLIER: That's correct.

CHAIRMAN WALLIS: It's just a pipe?

MR. LETELLIER: It's a pipe.

CHAIRMAN WALLIS: With holes in it?

MR. LETELLIER: With holes drilled.

CHAIRMAN WALLIS: But the velocity in those holes is much bigger than the velocity that you're quoting us through the rack.

MR. LETELLIER: The outlet velocity at the holes is less than or equal to 3 centimeters per second. By calculation.

CHAIRMAN WALLIS: Well, there's a huge area between the coupons.

MR. LETELLIER: That's true.

CHAIRMAN WALLIS: There's a huge flow area between -- so the velocity between the coupons is very, very low.

MR. LETELLIER: That's true.

CHAIRMAN WALLIS: Okay. So I was confused. When you say flow velocity over submerged coupons, what velocity is that?

MR. LETELLIER: It's between 0 and 3 centimeters.

CHAIRMAN WALLIS: No, what velocity is it, which velocity are you talking about.

MR. LETELLIER: Which velocity are we

talking about. It's very difficult to design to achieve a minimum required flow velocity. That's the reason for zero being the bound. As the test facility design evolved, we had many discussions about what upper bound we should achieve. And it --

CHAIRMAN WALLIS: No, I wanted the definition. You take a flow rate and divide by an area or something? How do you get this velocity?

MR. LETELLIER: Essentially that's true.

CHAIRMAN WALLIS: Flow rate divided by area. And what's the area that you use?

MR. LETELLIER: The area necessary to achieve 3 centimeters a second.

CHAIRMAN WALLIS: No, no. What particular area does it correspond to in the tank?

MR. LETELLIER: The area of the holes in the distribution header.

CHAIRMAN WALLIS: It's the holes in this --

MR. LETELLIER: That's right.

CHAIRMAN WALLIS: -- which has nothing to do with the velocity past the coupons necessarily.

MR. LETELLIER: It provides the upper bound. That's the only relationship.

CHAIRMAN WALLIS: Very different from the velocity --

VICE CHAIR RANSOM: What kind of velocity do you get if you divide by the cross-section of the tank?

MR. LETELLIER: The maximum velocity in this tank exists at the drain outlet where it's a 2-inch diameter.

VICE CHAIR RANSOM: -- just the volume average, or area average velocity of flow of the header through the tank to the outlet. It must be something very low.

CHAIRMAN WALLIS: It must be a trickle.

MR. GISCLON: Bruce, may I address the committee?

MR. LETELLIER: Yes, please.

MR. GISCLON: During the initial startup phase, this was a concern. And we didn't -- personally, I didn't feel it was appropriate that we had real very turbulent conditions within the tank even before the coupons were loaded. We wanted to have this 0 to 3 centimeters per second, and how do you achieve that. Basically we fill the tank with water, put some streamers out there, and adjusted the flow, and actually measured the velocity, the horizontal velocity coming out of those distribution headers with a timer and stopwatch using vegetable dye. And the horizontal velocity coming out there about to the point of where they mixed and went down to the tank was within this 0 to 3 centimeters per second.

CHAIRMAN WALLIS: So what's the --

MR. GISCLON: What it was when it goes down through the coupon racks, and with the coupon racks is certainly going to be something smaller.

CHAIRMAN WALLIS: What's the dimension of the tank?

MR. GISCLON: The tank is 4 feet square.

CHAIRMAN WALLIS: 4 x 4? So it's 16 feet squared.

MR. LETELLIER: All of these considerations were part of the design process. We can provide the --

CHAIRMAN WALLIS: No, but I think this is really important. You're telling me that the velocity that matters here is 3 centimeters a second. The velocity past the coupons is a trickle compared with that, isn't it?

MR. LETELLIER: Except that it's a transitional flow between -- across the plates towards, accelerating towards the outlet.

VICE CHAIR RANSOM: Where is the outlet?

MR. LETELLIER: In the center of the bottom of the tank.

CHAIRMAN WALLIS: I get four thousandths of a foot a second when I take the flow rate and divide by the area of the tank. I get four thousandths of a foot a second, which is nothing.

MR. LETELLIER: It's very small.

CHAIRMAN WALLIS: Very small. And so you put in something here which is very much apparatus-dependent.

MR. LETELLIER: I'm sorry?

CHAIRMAN WALLIS: It's very much apparatus-dependent, the way that the velocity, the flow happens in this tank. And it's a very, very small velocity over most of these surfaces.

MEMBER SIEBER: Well, if you look at the sump itself, you find a similar condition. You know, it's got tremendous area, and the flow rate is relatively small in the coupons, so basically scattered throughout the actual equipment.

MEMBER DENNING: It looks like what's flowing by the coupons, is that flow rate really going to be driven by this exit?

MR. LETELLIER: I believe so. It's

sitting about 14 inches, 18 inches above the outlet. And so there is an acceleration across the plates.

MR. TREGONING: Well, the other thing too, there's a good bit of turbulence in that tank which will affect --

CHAIRMAN WALLIS: There's bubbles.

MR. TREGONING: Well, we've seen in some of the tests, you know we've had some chemical products which has acted as flow tracers, and we've noticed quite a bit of secondary flow. So that will, you know, between the acceleration in the drain pipe and the secondary flow, I think the implication would be the flow across the coupons would be higher.

CHAIRMAN WALLIS: But Tim said something about the specifications for the test was a certain velocity, right? Didn't he? I can't find where it is.

MR. LETELLIER: Only the range.

CHAIRMAN WALLIS: But he mentioned 0 to 0.1 feet a second, or somebody did. And I asked about Reynolds number. It says flow velocity over submerged coupons, and to me that means the flow past the surface of the coupon. And what you actually got is something far less than that past the coupons themselves.

VICE CHAIR RANSOM: My question is, is that coupon centered over the outlet?

MR. LETELLIER: Yes, they are.

VICE CHAIR RANSOM: So the outlet is under the --

CHAIRMAN WALLIS: So it's a smaller area down there than the 4 x 4?

MR. LETELLIER: Two inch outlet that matches all of the stainless steel piping.

MEMBER SIEBER: It's essentially a point, though.

MR. LETELLIER: That's right.

CHAIRMAN WALLIS: So there's a point sink down there?

MR. LETELLIER: That's right.

CHAIRMAN WALLIS: So the coupons further away get less velocity than the ones near the drain?

MEMBER SHACK: And the header is only on one side. There's no --

MR. LETELLIER: Headers are on both sides. They're opposing injection.

CHAIRMAN WALLIS: Oh, there are two headers.

MR. LETELLIER: Parallel injection on both sides.

MEMBER SIEBER: The flow area --

MEMBER DENNING: -- a much smaller impact on the flow than drain.

MR. LETELLIER: I would agree with that. Not having a CFD calc.

MR. CARUSO: Why would the flow even go through the coupons? Why wouldn't it just bypass the coupons and go directly down to the bottom and into the drain?

MR. LETELLIER: The nozzles are oriented parallel to the surface of the water to have some momentum effect so that it's actually intended to pass through the top and downwards through the rack.

MR. CARUSO: How far away are they from the sides of the?

MR. LETELLIER: Well, they're 12-inch square coupons, and the tank is 4 feet in cross section. So they're approximately a foot and a half, 18 inches away from the header. Fourteen to 16 inches away from the header.

MEMBER DENNING: Have you considered ways of trying to determine what those velocities are?

MR. LETELLIER: As John mentioned, we've actually done some flow tracers, some ribbon studies to confirm that it's a very low velocity. However, it's much more difficult to measure that velocity. We have talked about performing CFD calculations of the tank. It really seemed like a level of effort that would delay the initiation of the test. Now, that type of work characterization can be done at this point to answer some of these questions. Keep in mind that I think my principal concern with the design was to have a system, a chemical system, at the plates that was not diffusion-limited. That we had enough advection to remove any chemical corrosion products, and to thoroughly mix them in the recirculating solution.

CHAIRMAN WALLIS: I just wondered where Tim got his spec of 0.1 foot a second from, and how that got translated into what you actually designed. Because it doesn't seem --

MR. LETELLIER: That's 3 centimeters per second. That's the upper bound.

CHAIRMAN WALLIS: I know, but of what velocity? Did he say he wanted 0.1 foot a second past the plates, or did he say he wanted it somewhere in the tank, or what?

MR. LETELLIER: The range specified across the plates was in the range --

CHAIRMAN WALLIS: Across the plates. To

me that means flowing through between the plates.

MR. LETELLIER: That's right. Zero to 3 centimeters.

CHAIRMAN WALLIS: You made some effort to get this 3 centimeters a second through the plates?

MR. LETELLIER: It's a range. We made some effort to guarantee that the flow velocity is in that range across the plates.

CHAIRMAN WALLIS: Out of the holes in the distributor? Well, all the details are in your report, so I can go to it and look these things up?

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: And we'd be happy to provide --

CHAIRMAN WALLIS: So we can sort that out.

MEMBER SIEBER: I guess that the guiding principle is that you aren't trying to create an erosion, you're trying to create a chemical cell where you're replenishing the chemicals at a rate at which they are reacting, or presumed to react, which is a very slow velocity, in my view.

MR. LETELLIER: In containment, you might expect the highest velocities to exist beneath the outlet of the break. Beneath the outlet of the break would be the highest velocity in containment, where your erosion potential is greatest. Some of them, the modern retrofit screen designs, are considering very, very low approach velocities of 0.005 feet per second. And so that's one of the reasons that this is an appropriate range. We did not want to exaggerate the higher end.

CHAIRMAN WALLIS: But they have to analyze the existing plants.

VICE CHAIR RANSOM: There is one thing that I'd be concerned about in this tank. Apparently there are no baffles at the bottom under the coupon samples to prevent flow from just bypassing the coupons? And I'll tell you where I'm coming from. The major resistance to flow in that tank is the viscous shear on the coupon plates themselves, and you have a foot and a half on each side that's just free volume with no resistance. So the flow naturally is going to want to flow down through the free volumes, over to the outlet and out, and bypass the coupons. Now, some will go in the coupons undoubtedly, but I'm wondering if you shouldn't do a little analysis to show just how much flows past the coupons versus how much actually bypasses the coupons.

MR. LETELLIER: That's of particular

concern when you start thinking about the fiberglass blankets which introduce a much higher flow resistance. And that will come up in our discussion of test results when we look at exams of the exterior versus the interior of the fiber blankets.

CHAIRMAN WALLIS: It will be interesting to see if Victor's right, and 99 percent of the flow bypasses the coupons. We're going to have to investigate this.

MR. LETELLIER: They present a very small cross-section in the present orientation. They're intended to provide a very low cross-section.

Let's move on to Page 12, I think, is where we left off. We don't have to go through these in detail. It's simply provided here so that you're aware that we are attempting to replicate the accident sequence as closely as possible, depending on whether the pH control is provided from an induction tank, introduced as sprays, that's the sodium hydroxide, or whether it's intended to imitate dry chemical, tri-sodium phosphate baskets, or whether it's intended to imitate the melting ice in the sodium tetraborate systems.

We had to reach a number of compromises about the durations.

CHAIRMAN WALLIS: Excuse me. This distributor nozzle is below the water level?

MR. LETELLIER: That's correct. The spray nozzles, however, are in the upper corners of the tank. One of the most significant differences between our tank and the power plant is that spray water is provided from an external storage tank. And we do not have an isolated additional volume. So when we introduce our sodium hydroxide into the spray, some fraction of that is immediately recirculated, it's immediately diluted and recirculated. That's of most concern if you're worried about what spray environment the upper containment materials experience.

Let's move on. You can read these and ask more detailed questions as you read the reports.

CHAIRMAN WALLIS: This boric acid and NaOH presumably interact pretty quickly, don't they?

MR. LETELLIER: I'm sorry, what was the question?

CHAIRMAN WALLIS: Well, if you say you added boric acid and NaOH prior to spray initiation, you just pour them in somewhere, or you pour one in first and then the other? Presumably they interact chemically pretty rapidly.

MR. LETELLIER: Yes, acid-based reactions are almost instantaneous.

CHAIRMAN WALLIS: Right. So it depends how you put them in, what you get.

MR. LETELLIER: Remember that the boric acid, the hydrochloric acid, and the lithium hydroxide are introduced to the pool to establish the baseline reactor cooling system water.

CHAIRMAN WALLIS: Do you pour one in first and then the other, or what?

DR. HOWE: In Test 1, the sodium hydroxide, and lithium, and hydrochloric acid went in first, followed by the boric acid.

CHAIRMAN WALLIS: Now, why was that? In reality, the boric acid washes things down first.

DR. HOWE: Well, the first point I want to make is that all these chemicals went in before the latent debris, before the coupons, so they --

CHAIRMAN WALLIS: Before the coupons. So all the reaction has occurred already.

VICE CHAIR RANSOM: So you do establish the mixture, and then put the coupons in.

DR. HOWE: That's correct.

CHAIRMAN WALLIS: Oh, well that's ….

MR. LETELLIER: Remember, this is the RCS water. The HCL, boric acid, and the lithium, that is the effluent from the break.

MEMBER SHACK: It's not the sodium hydroxide.

MR. LETELLIER: That's correct.

CHAIRMAN WALLIS: It's not the sodium hydroxide. All right.

MEMBER SIEBER: It's separately pumping.

MR. LETELLIER: And that replicates the spray scenario.

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: Well, that gives you a base pH, doesn't it. The NaOH overwhelms the boric acid.

MR. LETELLIER: That's correct. The pH from the spray induction tank approaches 12, so the upper coupons are exposed to a very high pH environment for the duration of the spray only.

MEMBER SHACK: But -- I mean, so your approximation here is you've got the pH 10 from the get-go. In reality, you have a pH of 7 for 30 seconds, and then the sprays come on, and you reach pH 10 in some fairly short order I would assume.

MR. LETELLIER: There is a proportionality that exists between our pH just prior to spray, and our target pH after everything's been mixed.

DR. HOWE: The pH at the beginning -- and I'm going to talk about Test 1 here -- the majority of sodium hydroxide is added in the base solution, and then additional sodium hydroxide was added during the first 30 minutes of spray. The pH of the base solution was about 9, and the spray that was added, the sprays had a pH that was calculated near 12. Once that additional sodium hydroxide was added into the solution, and diluted into the total volume, the pH ended up around 9.5.

MEMBER SIEBER: Which is --

DR. HOWE: So over a 30-minute period, the pH rose from 9 to 9.5, due to the addition of additional sodium hydroxide.

MEMBER SIEBER: And that's pretty close to, but a little shy of, RCS pH.

MR. LETELLIER: That's our target pH for the containment pool.

MEMBER SIEBER: Yes, right.

MR. LETELLIER: The sump pool.

MEMBER SIEBER: Well.

CHAIRMAN WALLIS: How many gallons --

MEMBER SIEBER: The temperature is different than it is in your reactor coolant system, but that accounts for the difference in pH.

MR. LETELLIER: That's true.

CHAIRMAN WALLIS: How many gallons are in the tank?

MR. LETELLIER: Two hundred fifty gallons.

CHAIRMAN WALLIS: So it's 25 gallons per minute, so 10-minute residence time. So you're actually doing a lot of mixing in the piping.

DR. HOWE: The residence time in the piping was about three or four seconds.

MR. CARUSO: Why did you add the HCL?

MR. LETELLIER: To account for the possibility of degradation of electrical cabling. There are chlorides that might be involved as part of the accident sequence.

MR. CARUSO: Okay. Okay. That wasn't simulated reactor coolant.

CHAIRMAN WALLIS: Hope not.

MR. LETELLIER: Tests 1 and 4 have the similar pH control system, and then the next slide, Page 13, describes the initiation for Tests 2 and 3, which are the tri-sodium phosphate system. The very bottom bullet on Page 13 describes the proportionality in the debris types, 80 percent cal-sil by mass, and only 20 percent NUKON. When we're examining fiberglass only, there's approximately 4 cubic feet of fiberglass debris that we have to arrange in various blankets and sacrificial samples.

CHAIRMAN WALLIS: This is before it's compressed? As manufactured.

MR. LETELLIER: Yes. This is manufactured volume. So moving to Page Number 14, we can walk through some of the daily activities that are conducted to monitor the tank. The very first step is to check hydrogen level as a safety test. We do this by extracting some vapor from the head space. It's not intended to be a quantitative quality assurance data point. However, it has provided some anecdotal information about the rates of corrosion. We have never approached a safety concern of lower flammability limit, but there is strong evidence of hydrogen generation.

MEMBER SIEBER: And that comes basically from the aluminum?

MR. LETELLIER: It comes from a metallic oxidation process.

CHAIRMAN WALLIS: Do you see bubbles on the -- you don't see any hydrogen bubbles in the tank?

DR. HOWE: A little. In the first test, which was the highest pH, and where we saw the highest hydrogen generation, there was -- you could see very fine bubble formation on the aluminum coupons for the first several days, first period of days.

CHAIRMAN WALLIS: Because presumably it's formed on the surface, and it has to get from there to the place where it's vented. So it gets in the form of a bubble.

MR. LETELLIER: It is interesting to note that when we did our design calculations using conservative corrosion rates followed by the industry for hydrogen generation, we would have predicted a flammability concern within 16 hours, and we've never come close to that.

MR. KLASKY: This is Marc Klasky from LANL. In some separate bench scale tests that we've performed, depending upon whether the oxide layer has developed, we actually took aluminum where there was no oxide layer and very rapid bubbling occurred. So it truly is a function of that oxide layer developing.

MR. LETELLIER: These samples have all been exposed to air for --

CHAIRMAN WALLIS: So if you had really clean surface, you'd probably get a lot of this initially.

MR. KLASKY: Yes, we did that, and in fact it was just turbulent. And it looked like we were boiling the solution, actually.

MEMBER SIEBER: That's the way it works.

MR. LETELLIER: Water extraction is our primary means of interrogating the tank. There are two samples that are extracted, 100 milliliters taken directly from the sample line while it's hot. It is filtered in place to a level of 0.45 microns, and the temperature is maintained throughout the bench activities which we'll describe. There is an additional sample that's taken that's unfiltered, and it's not temperature controlled. And the initial intent of having both filtered and unfiltered was for fractionation, so that we could have some understanding about the suspended versus the dissolved materials, and then we would -- if we sampled it online, thereafter we would not have to be worried about temperature control, because the ICP analyses are a destructive exam that re-homogenizes all of the constitutes.

Dynamic viscosities are measured at the 60 degree test temperature, and also after they've cooled to room temperature. We have constant temperature water bath.

CHAIRMAN WALLIS: It's mostly the different shear rates?

MR. LETELLIER: The dynamic viscosity is performed in a capillary viscosimeter.

CHAIRMAN WALLIS: At different shear rates? Different velocity?

MR. LETELLIER: No.

CHAIRMAN WALLIS: How would you -- someone mentioned non-Newtonian, but you didn't have the different shear rates. How will you detect non-Newtonian behavior?

MR. LETELLIER: The shear rate viscosymmetry is performed at Sandia National Lab across town. It's not actually performed on the bench as you see it here.

CHAIRMAN WALLIS: Okay. But it's performed somewhere.

MR. LETELLIER: That's correct. The turbidity is also measured at both temperatures. And one thing that we learned during Test 1 is that we have to establish a protocol for cooling, because we actually produced a visible precipitant that affected the turbidity and the viscosity. So now at least there's a time limit, and a temperature target for a window of reporting the data. We can be consistent throughout the tests.

The filtered and unfiltered samples are

sent offsite for ICP elemental analysis. This was part of our QA efforts to have a qualified wet chemistry lab provide that service. Part of our daily activities also include pulling sacrificial coupons of fiberglass, and additional higher volume filter samples on a particular schedule. And as I mentioned before, water is added as needed. After every 5 gallon increment, water is added back to the tank. I think the maximum was in the range of 12 gallons for any single test.

CHAIRMAN WALLIS: These sacrificial coupons are much smaller than the other ones, aren't they?

MR. LETELLIER: They're approximately 10 to 20 grams of fiberglass that are packaged in a 4 x 4 inch square stainless steel envelope.

CHAIRMAN WALLIS: That's the 4 x 4 inch square postulated in the NEI's document?

MR. LETELLIER: No, this is simply a convenient method to extract an in situ sample.

CHAIRMAN WALLIS: But then, so these are fiberglass. You have samples of other materials that you extract? Or just the fiberglass?

MR. LETELLIER: Just the fiberglass. In particular, we're looking for evolution, a time evolution of some of the products that you're going to see next. Originally we had scheduled extraction on Day 15, and then of course Day 30. Based on the evidence we observed, we accelerated that schedule to have a Day 4 sample, and some of our beaker studies show even earlier evidence.

CHAIRMAN WALLIS: Now, let's talk about the fiberglass. Your fiberglass is in sort of a bundle or something in this, encased in --

MR. LETELLIER: I describe it as a pillow.

CHAIRMAN WALLIS: It's a pillow, a fiberglass pillow. And velocity's in the tank are very small. So any sort of driving head for flow through the fiberglass is very small. So you'd expect the velocities in the fiberglass to be minute.

MR. LETELLIER: That's correct.

CHAIRMAN WALLIS: Essentially stagnant in there. So how do any kind of chemical products get out?

MR. LETELLIER: These are, I think the interior, internal fiberglass blankets are diffusion dominated.

CHAIRMAN WALLIS: But if they were in a pool, there might be smaller bits of fiber more distributed, and having much more access to what's going on in the pool. The center of this pillow may not even know what's going on in the tank.

MR. LETELLIER: That's a fact, and in fact our test plan has evolved to accommodate that observation. In addition to the larger pillows, we also have constructed a small box, a so-called bird cage, where that material can reside in a less compact configuration.

CHAIRMAN WALLIS: Or you could put the pillow closer to the injection, or you could deliberately inject some liquid in the manifold that aims through the pillow.

MR. LETELLIER: Yes. In the most recent test, we've actually suspended one of the envelopes directly in front of --

CHAIRMAN WALLIS: So you've thought of these things. Of course when you start varying all these, you get a lot of tests.

MR. TREGONING: The other thing we do is we have insulation wrapped around the drain collar screen, which as you mentioned, is the highest flow area in the chamber itself. So we've got a large number of samples that we've added as tests have gone on to try to interrogate some of these effects.

MR. KLASKY: One additional remark is that aside from Test 1, the predominant dissolution has been the fiber itself. We've observed the concentrations of the silicon to be on the order of 100 milligrams per liter, 2 through 4. So the driver could in fact be the dissolution of the fiber itself.

MR. LETELLIER: At the same time we don't think that the flow conditions in the test are atypical. There are certainly regions in the plant that experience the same types of stagnant flow conditions.

The various sample types are itemized on Page 15.

CHAIRMAN WALLIS: Well, I think this raises a question I raised earlier, that out of all this, if you're going to get a tool for prediction, it would seem that there's an awful lot of plant-specific conditions that are not -- even if it's a typical test. But there's a lot of variability between plants. And I'm not quite sure how from all this you get a tool that's usable in every plant.

MR. LETELLIER: Let me point out one of the conditions that is not being simulated here, and that is accumulation of fiber across the sump screen. When you think about the impingement velocities, that is one test condition that we have not reproduced.

CHAIRMAN WALLIS: Well let me ask, who's responsible for developing a tool for use in plants, and what does that person need? Somebody must be responsible for designing and producing a predictive tool someday, otherwise we're nowhere.

MR. LETELLIER: I can only respond by saying that the limited or sparse sample matrix has been intended to represent major classes of plants.

CHAIRMAN WALLIS: This is not part of your work scope. So maybe NRR will respond to this later in the day? Who's responsible for developing a tool?

MR. KLEIN: We'll address that question later today.

CHAIRMAN WALLIS: Thank you. And then the other thing is hen is it likely to be ready. Okay.

MEMBER SIEBER: Well, these tests are limited to chemical effects.

CHAIRMAN WALLIS: Yes, they're sort of try-it-and-see tests.

MEMBER DENNING: That's true, but if you think about where the fibers are, the fibers are in a location prototypically where there's going to be significant flow going past them. That's not necessarily true of the other stuff. But the fibers are going to go to the screen and be there, regardless of whether we're trying to measure their delta P or not. There's going to be significant flow-through. You don't agree Bruce?

MR. LETELLIER: Well, there would be some amount of fiber on the screen, undoubtedly. But in some of our integrated flow tests there are also -- there will be large piles of fiberglass in quiescent areas of containment that may be very typical, prototypical, of our test conditions.

CHAIRMAN WALLIS: But if you use the guidance that's available, I think you find that in some of these plants a lot of the fiberglass gets through a screen.

MR. LETELLIER: By regulatory, that's a requirement.

CHAIRMAN WALLIS: And then you do have higher velocity there with existing screen. And so the fiberglass map becomes conceivably a chemical reactor.

MR. KLASKY: One thing I'd add in terms of trying to develop a predictive tool. I think to supplement some of the ICET work, we've been conducting bench scale tests that really, I think, consist in developing the mechanism for the chemical dissolution and the formation of product. So I think it complements some of the work that's been done with the IC work.

MR. LETELLIER: We've tried to modify and tailor the test plan as much as possible without perturbing the primary intent. We've tried to, in particular, examine the different flow regimes to the extent possible within the constraints of the MOU.

On Page 15 is itemized the various sample types that we extract, and how we tag them and refer to them in the reports. Fiberglass blankets, we've talked a lot about those. Those are the largest volumes present. The sacrificial coupons are much smaller quantities in basically small envelopes. They are intentionally placed in what I would call higher and lower flow regions. However, all of them are sandwiched inside of the larger blankets. So in essence, it is a very quiescent stagnant condition. Water samples, a primary sample type, which we archive. For any one of these tests you can line them up and look at evolution of products. Filter paper is associated with both the daily samples and the high-volume extractions. When I say high volume, that's intended to be 2 to 4 liters of water. It's not like we drain the tank and reintroduce it. We have observed visible precipitants in Test 1. That becomes a sample type of particular interest. The sediment on the floor, which its primary constituent is the latent debris, the crushed concrete. Any chemical products that are formed and happen to settle there, and also the fugitive fiberglass. That's a very heterogeneous sample type.

The drain column, which Rob mentioned, is essentially a fiberglass jacket around a cylindrical screen that's inserted in the drain. It's 2 inches in diameter, and it stands about 14 inches tall. It's intended to preclude the debris from simply falling in the hole and fouling the pump. But because that experiences some of the higher water velocities, we took that opportunity to introduce a fiberglass jacket. The top of the drain cylinder is completely open because we didn't want to obstruct the flow because of debris accumulation. So that becomes a sample type.

The metal coupons are somewhat overlooked. We're finding a renewed interest. They are the source term for all the corrosion products that drive the interesting chemistry. But we have only begun to examine the surface phenomena involved with those.

CHAIRMAN WALLIS: But you store them somewhere in an inert atmosphere?

MR. LETELLIER: No.

CHAIRMAN WALLIS: Just leave them lying around the lab, and they get green?

MR. LETELLIER: They are individually cased in plastic Ziploc bags, and they're archived, and photographed, and weighed pre-test and post-test.

In Test Number 1, we had production of enough precipitant when the effluent cooled that we deemed that a special sample type called sludge. There are a number of scientific sample types descriptive terms that come to mind. We have seen evidence of residue on the tank walls and in the pipes. We have extracted samples of those.

CHAIRMAN WALLIS: You said you had enough of it. Does that mean you had a handful, or a pocketful, or a truckload-full?

MR. LETELLIER: For Test Number 1, there was approximately 17 gallons of combined liquid in semi-solid sludge.

CHAIRMAN WALLIS: Seventeen gallons?

MR. LETELLIER: Yes. There's a significant quantity --

CHAIRMAN WALLIS: Well, that isn't a truckload, but it's a fair amount.

MR. LETELLIER: And please understand that this material was observed when the tank was drained and the solution was cooled, the ambient temperature. We began observing -- well, we're straying into the results.

CHAIRMAN WALLIS: Seventeen gallons.

MR. LETELLIER: There's quite a bit of it.

CHAIRMAN WALLIS: Well, then that's something like this.

MR. LETELLIER: Five buckets.

DR. HOWE: This is again, just to clarify what the source of this is, we have the 250-gallon tank. At the end of the test, we've removed all the metal coupons, all the fiberglass, everything else, then we drain the liquid from the tank. After it was drained, it cooled to room temperature. When it cooled to room temperature what was in the water precipitated.

CHAIRMAN WALLIS: There were 17 gallons of sludge in there?

DR. HOWE: Then we decanted off the supernatant after the 250 gallons had precipitated.

CHAIRMAN WALLIS: Seventeen gallons in 250 gallons.

DR. HOWE: Exactly.

CHAIRMAN WALLIS: That's -- whatever, a

fifteenth or something.

MR. LETELLIER: Solid fractions.

DR. HOWE: So we'll get into what those precipitants were.

CHAIRMAN WALLIS: One-fourteenth. So how many gallons are in the sump? One-fourteenth of that of sludge? Now you're getting up to truckloads.

DR. HOWE: That's right.

MR. TREGONING: Again, the case to be made here is that the sludge formed upon cooling. So it wasn't --

MR. LETELLIER: Let's return to this. I'll show you a photograph --

CHAIRMAN WALLIS: I'm sorry, I'm going ahead, but it's ….

MR. LETELLIER: The last sample type I want to mention is the importance of providing baselines of clean material. Because we're trying to diagnose chemical products that are plated and deposited on various substrates, it's important for us to examine fiberglass, have a composition analysis of our dirt, our soil, and concrete. We've introduced nylon mesh as a sample container, and of course our metal coupons.

CHAIRMAN WALLIS: To go back to the -- the fiberglass has this original coating on it? You put that in when you start? Do the pillows have this original coating?

MR. LETELLIER: It's part of the manufacturing process, so it's provided.

CHAIRMAN WALLIS: So that's what goes in. It's not -- okay. But is it aged in some way? Is it heated so that might have been driven off?

MR. LETELLIER: A portion of it has.

CHAIRMAN WALLIS: Right.

MR. LETELLIER: John explained that earlier.

CHAIRMAN WALLIS: Yes, that's right. So it looks to me as if you could take a break when you've finished this part of your talk.

MR. LETELLIER: Slide Number 16 you can read at your leisure. It's simply an inventory of the various diagnostics and analysis techniques that we've brought to bear on this issue. The very first one, SEM with complementary EDS, is a very useful survey tool. Almost all of our sample types are processed through this lab. Some of the others are more specialized. They're only applicable to crystalline products, for example. So not every single sample type goes through the full battery.

ICP, inductively coupled plasma spectroscopy, is our primary means of inferring the elemental --

CHAIRMAN WALLIS: Now, you're looking at the materials in solution, and you're looking at the sludge. You're not examining the original sample coupons? Some of them may even grow rather than dissolving, depending on what the chemical reactions are. Are you examining those coupons at all?

MR. LETELLIER: Yes. Those sacrificial fiberglass.

CHAIRMAN WALLIS: Those ones, but the metal coupons?

DR. HOWE: The metal we've looked at as well.

CHAIRMAN WALLIS: The pictures show that they're discolored, and grow warts or whatever you want to describe them.

MR. LETELLIER: For example, one of our key findings in Test Number 1 was a 25 percent weight loss in the aluminum coupons.

CHAIRMAN WALLIS: That is a lot.

MR. LETELLIER: Before and after.

CHAIRMAN WALLIS: How thick are those coupons?

DR. HOWE: One-sixteenth of an inch thick.

CHAIRMAN WALLIS: Twenty-five percent loss.

MR. LETELLIER: However, in some of the other tests, the mass balance is somewhat less conclusive. We have reasons to suspect there are deposition processes as well, and that's motivation to examine the surface effects on these plates.

CHAIRMAN WALLIS: Because I mean, hopefully you were thinking of removing maybe one-thousandth of an inch, or something. And we're talking about something much more substantial than that.

MR. KLASKY: The pH is high enough.

CHAIRMAN WALLIS: Now is the weight loss uniform over the plate, or has it lost more on the edges than the middle, or something?

MR. LETELLIER: I don't recall evidence of severe pitting. It's more or less uniform.

CHAIRMAN WALLIS: It's more uniform all over? The thickness loss is the same at the bottom as the top, and the middle? When you take a micrometer, do you measure the same thickness everywhere?

MR. LETELLIER: So that concludes the first portion of the talk. The remaining slides illustrate results.

CHAIRMAN WALLIS: I'm sorry, 25 percent loss of aluminum in a plant is enormous. Just thinking out loud here. The ladder, I have a 25 percent of an aluminum ladder, that's --

MEMBER DENNING: That's a lot of hydrogen, too.

CHAIRMAN WALLIS: That's a lot of hydrogen too, yes.

MEMBER SIEBER: That's why they don't use very much aluminum.

MR. LETELLIER: It's also worth noting that that's the weight loss on the submerged samples. In the atmospheric samples, which comprise the high, percentage of aluminum had much less --

CHAIRMAN WALLIS: Make sure that the scaffolding doesn't get submerged.

MR. ANDREYCHEK: That's a point said previously, that the aluminum would be a hydrogen generator, and particularly with submerged. And plants generally control it fairly carefully, with the exception of the one that was noted earlier.

CHAIRMAN WALLIS: Now, this reflective metal insulation, that's mostly stainless steel.

MR. ANDREYCHEK: The RMI is -- there's two types. RMI is a -- typically has a band of stainless steel outside. Some of it is aluminum sheeting inside.

CHAIRMAN WALLIS: Some of it is aluminum sheeting, which is ideal for dissolving if it gets carried down to the sump. It's in a foil sort of form?

MR. ANDREYCHEK: Correct.

CHAIRMAN WALLIS: Maybe it all disappears, then?

MR. ANDREYCHEK: Well, it could -- yes. It would tend to become a source of hydrogen generation, which is accounted for in hydrogen generation calculations that plants make.

CHAIRMAN WALLIS: So the sludge on the bottom of the sump would then burp hydrogen from time to time, which would be alive. It wouldn't just be --

MR. TREGONING: I don't service that --

CHAIRMAN WALLIS: -- bubbling up hydrogen.

MR. TREGONING: I don't see that the hydrogen is --

MR. ANDREYCHEK: It continues bubbling, it's a more --

CHAIRMAN WALLIS: Well, you've got to remove that much aluminum, you must be making a lot of hydrogen, just looking at the models involved, and the volume involved with those models.

MR. ANDREYCHEK: We didn't see -- even in this test we didn't see hydrogen bubbles forming per se.

CHAIRMAN WALLIS: It has to get to the surface somehow. And if you're dissolving that much aluminum.

MR. LETELLIER: Wait a minute, I think we need to be clear. Just to restate what Kerry said, we did see hydrogen bubbles on these aluminum samples for this test. However, Test 1, the amount of aluminum that we lost in the submerged specimen was only for Test 1, and it was an outlier with respect to all the other materials by far.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: Predominantly the weights of the other materials varied at pre- or post-test. I'll state within a gram, within a handful of grams.

CHAIRMAN WALLIS: Why did Ralph tell us that nothing interesting happened except in Test 3?

MR. LETELLIER: No, something interesting happened in every test.

CHAIRMAN WALLIS: Ah, okay. So Ralph was wrong. He said look at Test 3, and we should've looked at Test 1.

MR. LETELLIER: I think he may have just been ill-informed.

CHAIRMAN WALLIS: Okay.

VICE CHAIR RANSOM: Test 1 was just the high sodium hydroxide that resulted in the aluminum hydroxide high pH.

MEMBER SHACK: Not a surprise.

MR. LETELLIER: Yes, that wasn't a surprise.

VICE CHAIR RANSOM: Now you know what all that sludge is, that 17 gallons of sludge.

MEMBER SIEBER: You also know how to make party balloons float. Put aluminum foil --

MR. LETELLIER: The next portion of my talk provides a survey of results. And there are some high-level overview slides, and there are also many, many detailed presentations.

CHAIRMAN WALLIS: I think we heard a comment from above on your findings just then.

MR. LETELLIER: A high-level observer? Commentator? So this can be a very flexible presentation to satisfy your.

CHAIRMAN WALLIS: Well, I think you're on time, aren't you? You planned to not reveal anything in the morning?

MR. TREGONING: We may be about a half hour behind, I think.

CHAIRMAN WALLIS: You're half hour behind? Because I thought you were not going to -- you were going to get to the results this afternoon.

MR. TREGONING: We were going to start into them this morning.

CHAIRMAN WALLIS: Oh, you were. So you're a little bit behind. Okay. So should we break down now for an hour, or do you want to wait till 1:00?

MEMBER SHACK: Forty-five minutes? Oh no, we're going to meet somebody.

CHAIRMAN WALLIS: Yes, we're going to meet at 12:30. So we could meet at quarter to 1:00. Is that fine? Helps us get things done, since this is very interesting, and you've put a lot of time into it. We will take a break then until a quarter till 1:00. Thank you.

(Whereupon, the foregoing matter went off the record at 11:53 a.m. and went back on the record at 12:58 p.m.).

CHAIRMAN WALLIS: Okay, let's come back into session and continue where we left off this morning.

MR. LETELLIER: We'll be continuing with the presentation of survey of results for the ICET tests 1 through 4. As I explained before lunch, there are some high level overview slides, and there are numerous data reports where we get into the trends and possible explanations for the behavior. So I'll leave it to your discretion and level of interest how we proceed.

Let's start with the findings, and the concerns regarding principles.

CHAIRMAN WALLIS: Why did you in this figure you show us here put another figure on top of the one point which is obviously way out of line?

MR. LETELLIER: This is purely intended to be a collage of representative results.

CHAIRMAN WALLIS: Yes, but there is a point which is down at about 30 or something which is hidden behind that picture.

MR. LETELLIER: We're going to examine each of these in detail in later slides.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: It's just to pique your interest, basically, and obviously it has.

The first of our findings relates to deposits in the fiberglass. Before lunch we talked about velocity regimes. We have observed chemical products deposited in what I would term quiescent fiberglass conditions.

CHAIRMAN WALLIS: Now this is the fiberglass that's down at the bottom of the tank? Is that what it is? Or is the fiberglass in the pillow?

MR. LETELLIER: These are the 4-inch square coupons which are nestled --

CHAIRMAN WALLIS: The ones you took out.

MR. LETELLIER: -- on the interior of the large pillows. So they're almost approaching zero flow velocity. They could be considered internal or interior fiberglass conditions. The second bullet recounts our observation of visible precipitants in Test 1, and raises some questions about possible production of this material in a heat exchanger.

CHAIRMAN WALLIS: Now, there are some -- there's some fiberglass near the drain, which has higher velocities. Do you see the same sort of thing there?

MR. LETELLIER: Are you referring to deposits in the fiberglass?

CHAIRMAN WALLIS: Yes, just looking like the ones you've got here.

MEMBER SHACK: The web structure.

MR. LETELLIER: They're a lot more difficult to interpret because the fiberglass collar also collects a lot of the sediment, the fugitive fiberglass. It's a hodge-podge of everything.

MR. TREGONING: But just for clarification, we did not have the fiberglass deposit for this particular test. As a result of this test, we installed it for all subsequent tests.

MR. LETELLIER: Rob means that the drain collar --

MR. TREGONING: The drain collar, yes.

MR. LETELLIER: -- for Test Number 1.

VICE CHAIR RANSOM: Are these precipitates coming out of solution as you lower the temperature?

MR. LETELLIER: That's essentially correct. In essence they're not visible to the eye at 60 degrees, but upon cooling of even a few degrees, then they start to appear. And they accumulate substantial quantities in the bottom of these sample vials. We suspect that the precipitant is related to the sludge that's produced at the end of the test, that may in fact be one and the same thing, but we wanted to examine them independently.

CHAIRMAN WALLIS: It's hard to imagine a web growing. I mean, how does it form? Does it grow from the ends and know how to meet in the middle? No.

MR. KLASKY: They have observed like dendrite type structures.

CHAIRMAN WALLIS: Which then get filtered out. It's pretty hard to imagine a web unless you've got a spider in there or something to transport it.

MR. KLASKY: But it's adhering to --

CHAIRMAN WALLIS: But if you were making -- if you were sort of spinning these things in the solution and then they meandered and stuck on, that would --

MR. KLASKY: -- particles adhering to the fiber itself.

VICE CHAIR RANSOM: The implication of this is that the solution is saturated, I guess, to start out with, or near saturation. And as you cool it, you go through the saturation point, and begin to precipitate material.

MR. LETELLIER: Well, except that we see evidence of evolution, or growth continuing from Day 4 through Day 15 through Day 30. Let me clarify these figures. As I said, they're just for illustrative purposes only, but the lower panel that shows the film-like structure, this is actually a desiccated sample from fiberglass in Test 1. That's what I referred to as the deposit inside the fiberglass. The center panel is an exam of some of the semi-solid precipitant. This is a high resolution TEM image. They key finding for this is it looks like an agglomeration of much, much smaller particles, on like a 10 nanometer type of scale of a cellular arrangement.

MR. CARUSO: Do you have a good idea what temperature these precipitants formed at?

MR. KLASKY: Let me add one thing with respect to the temperature. I think with respect to Test 1, the real visible precipitant was seen upon cooling. In bench scale tests, however, we've observed the precipitant at test temperature, 60 degrees. And even in Test 1, upon placing the samples in an oven at 60 degrees some time after a few weeks, precipitant was seen at test temperature. So, it may be more than just basically a change in the thermodynamic -- or I should say the temperature. It could be phase changes as well, and I'll elaborate in my talk later.

CHAIRMAN WALLIS: This 17 gallons of sludge we heard about before, is that something which appeared on cooling, or was it there -- it wasn't there until you cooled?

MR. LETELLIER: That's correct. It wasn't visible, certainly, until we cooled it.

CHAIRMAN WALLIS: Was it detectable?

MR. CARUSO: I'm just wondering if this liquid is drawn through an RHR heat exchanger and cooled down, is it going to precipitate inside the heat exchanger?

MR. LETELLIER: It very well may, depending on the point of maturation of the solution itself in relationship to the accident sequence. When the heat rejection systems are active versus how long during the corrosion that the tool has --

CHAIRMAN WALLIS: So you're not looking at the deposition in the sump? You're looking at deposition in the heat removal system?

MR. LETELLIER: It's one possibility that's the point of production. There are two concerns.

CHAIRMAN WALLIS: I'm thinking about the plant.

MR. LETELLIER: Well, there are two concerns with production of this material in a heat exchanger. One is it may physically obstruct the flow channels. Second of all, it may be reintroduced to the pool as a transportable particulate.

CHAIRMAN WALLIS: Right, right.

MR. LETELLIER: And we've performed some re-dissolution studies, rather qualitative in nature, but we're asking the questions does this stuff continue to survive in this present form. The evolution of this material changed over the course of the test. We should get into this later.

But even as early, Kerry, as Day 5, Day 8? Maybe sooner, there was some visible evidence of precipitant in the bottom. As the test progressed, it would precipitate sooner, with even less cooling, and it would generate even more of this product. So if you line up the 30 sample bottles, there's a continuous growth in quantity.

DR. HOWE: And to answer your question, by the end of the 30-day test, the test temperature was 60 degrees Celsius. By the time it cooled just a couple of degrees it would precipitate. I mean, it was very close to the saturation point. So it would start cooling, and it would precipitate right away. And as it cooled from there down to room temperature, we would get that entire quantity to precipitate.

MR. LETELLIER: So, as Marc Klasky alluded to, there's some speculation that perhaps the tank itself would have reached that saturation point if we had run another five or 10 days. There's corroborating evidence both in some of the archival sample bottles that we retained after the test, and also in some of his bench scale studies that not just the saturation point, but the phase change of these materials could induce the precipitation.

VICE CHAIR RANSOM: Out of curiosity, have you talked to any civil engineers who do a lot on water purification, where they use alum, for example, and you form a floc, and it tends to take sediment out of the water? I mean, is this a phenomena similar to that?

DR. HOWE: Yes.

VICE CHAIR RANSOM: Do you understand the mechanism of why does alum form at a pH of 7. Basically it'll form a floc that settles out too.

DR. HOWE: Right.

VICE CHAIR RANSOM: I'm not sure I understand all that, but.

DR. HOWE: So what we do in water treatment is we typically add an aluminum sulfate, salt aluminum sulfate, and it would be very soluble. As soon as we add it to the water that's -- I'm sorry. What we do in water treatment is add an aluminum sulfate salt to water. Aluminum sulfate being very soluble, as soon as it goes in the water that's buffered around a neutral pH, the aluminum combines with hydroxide. We form a very insoluble aluminum hydroxide floc. That aluminum hydroxide is very efficient for aggregating particles, and so it's used in the clarification process to make our drinking water potable. It's also a very -- so it's very efficient at pulling particles out of water. It's very efficient to filter out of the water, which is very good for water treatment, and very bad if you're trying to keep your sump screens clogged.

MR. LETELLIER: Some direct evidence of that process we believe exists during the first 24 to 36 hours of the test where the water substantially clarifies, the turbidity drops very rapidly, as soon as we start introducing ionic species, the corrosion products, essentially.

The third bullet for overall findings. We have seen some internal deposition of calcium silicate on internal pipe surfaces. I'll show you photographs of that. That raises some concerns about fuel heat conduction fouling of internal components. This is a concern for some of our follow-on test work where we're trying to design circulation systems to handle large cal-sil quantities. And we've never observed this effect in a room temperature normal, just a standard tap water environment. However, we did observe it in this particular test. So it may be encouraged or exaggerated by the chemical environment.

DR. HOWE: But just to be clear, that wasn't observed in Test 1. That was observed in --

MR. LETELLIER: Test Number 3.

CHAIRMAN WALLIS: No calcium silicate.

DR. HOWE: Right. It was 3, Test 3.

MR. LETELLIER: Test 3 was our first experience with calcium silicate. The fourth bullet is there's some speculation that the calcium present in this insulation debris can lead to passivation of aluminum surfaces, and actually inhibit corrosion. It may be one of the only good things I can say about calcium silicate. It does substantially affect the chemical system.

MR. TREGONING: And that was an observation for Test 4. So, Bruce is summarizing some of the principal observations for all the tests here.

MR. LETELLIER: These will all be repeated later.

CHAIRMAN WALLIS: Could you tell us what this web-like substance is?

MR. LETELLIER: We'll get into that. We do have some EDS exams of elemental composition.

MEMBER SHACK: In your internal pipe coating, is that calcium silicate or calcium phosphate? Or you don't know what it is?

MR. LETELLIER: We did recover some scrapings from that, and we've done the exams. I just can't cite the composition right now.

The fifth bullet, evidence of colloids in aluminum based test conditions. That refers back to some of the microglobular constituents of this center frame on the image where very, very small particles are able to grow and actually sequester large amounts of aluminum that are neither dissolved, truly dissolved, or in suspension. They sort of exist in this amorphous third phase.

CHAIRMAN WALLIS: These are little particles of aluminum?

MR. KLASKY: The primary structure is about anywhere from 10 to 50 nanometers.

CHAIRMAN WALLIS: Something that condenses? It's not particles actually break away from the coupons?

MR. KLASKY: Well, the source of aluminum is obviously the metal coupon. And it's -- basically the aluminum is undergoing hydrolysis reactions. And then basically --

CHAIRMAN WALLIS: Mixes together and makes a particle later on.

MR. KLASKY: That's right. That's right.

MR. LETELLIER: Some of this information is rather speculative at the moment, but the reason that we're interested in it is to answer the questions that you pose. How can these deposits control films, how does that behave in a filtration system when it's under flowing conditions. We believe that's the first step towards the predictive capability.

The next bullet cites the observation that the presence of aluminum can inhibit fiberglass dissolution via its effect on solubility. And that -- there's evidence cited for that in the literature, and also more recently Southwest Research has measured that in a recent dissolution test.

CHAIRMAN WALLIS: Do you get aluminum silicates in this?

MR. LETELLIER: Oh, yes.

CHAIRMAN WALLIS: You do.

MR. LETELLIER: Fiberglass itself is an aluminum silicate.

CHAIRMAN WALLIS: Yes.

MR. LETELLIER: The next bullet --

CHAIRMAN WALLIS: There's not much aluminum in fiberglass.

MR. KLASKY: About 4 percent. I should say, AL_2O_3 is about 4 percent. So aluminum's 2 percent.

MR. LETELLIER: As we try to piece together all the various attributes explaining the behavior of the four tests, there's increasing evidence of time dependent phase and solubility influences on the exact products that are formed. This refers back to the sensitivities that Rob mentioned this morning, which has some implications for how we develop surrogates, and how do we test the substitute materials.

If we have time later today, Rob Tregoning will talk about the last item, the Tri-Lab consortium to investigate these issues of identifying the species involved, chemical species, how are they formed, and how they affect head loss.

The rest of the talk is organized first of all by test. There is an overview slide for each test, and then there are a set of pairwise comparisons. Not all possible pairwise comparisons, but chronologically we were able to line up Tests 1 and 2. Later on we would line up Tests 3 and 4. And through this process, we can discuss similarities and differences that lead us to final conclusions.

Again, as a reminder, Test Number 1 was a constant temperature, 140 degrees F, target pH of 9.5. This was a sodium hydroxide controlled system, with 100 percent fiberglass debris. There's no calcium silicate present. We've talked several times, the initially cloudy water from the latent debris quickly agglomerated and clarified. You see that in the turbidity trends. This is basically the water clarification process that you mentioned.

We did observe the white precipitant that was formed upon cooling. And I've already discussed how that process accelerated both at the precipitation temperature and also in the quantity over the course of the test.

The viscosity was virtually constant throughout the 30 days at the test temperature. And there was a noticeable increase in the dynamic viscosity upon cooling, but most of that 50 percent increase is simply the temperature effect of the water. At this point we refined our protocol for measuring this because of the possible interference of the visible precipitant. There was some non-Newtonian shear rate dependence observed for this material at room temperature.

VICE CHAIR RANSOM: Is that a thixotropic effect?

MR. LETELLIER: It's a shear thinning effect that turns this into a Bingham plastic flow.

VICE CHAIR RANSOM: Right.

DR. HOWE: So if we're talking about thixotropic in terms of there being a threshold shear stress at which there's no flow, I don't think that was observed.

VICE CHAIR RANSOM: But it dropped off instead of being linear. It dropped off.

DR. HOWE: Yes.

MR. LETELLIER: So there's a lot of speculation about how much influence the semi-solid particulate loading has on that measurement. And if you could filter it effectively, is there truly a liquid viscosity change. We're not prepared to speculate.

VICE CHAIR RANSOM: I imagine you're aware that silicon dioxide is a common material that's added in very minute amounts to make a thixotropic mixture for gels. A wide variety of applications. Interesting thing is, at least you would tend to believe that they wouldn't have much effect on a shear flow. In other words, if you tried to filter it, or pump it, or something like that, they have very minor effect. But they'll sit there as a gel, which is sort of interesting. I'm wondering how do you distinguish between this precipitant and a possible gel of that type?

DR. HOWE: I think so far the word "gel" is being used rather loosely here. And I don't think we used in the strict sense of, you know, a structured solid phase with a movable liquid phase within that. I don't know that we're seeing that kind of gel formation in what we've observed here. We do see this viscosity change during the precipitation. That viscosity -- and we've discussed it among ourselves, whether that viscosity is a function of the measurement now we're measuring a two-phase system rather than a single liquid phase, and how that's impacting the measurement of viscosity as opposed to a true change in the liquid viscosity. And those are issues that need to be resolved.

MR. KLASKY: I'd just add, some of the bench scale tests where we've been able to isolate the -- I'll call it gel, certainly do look like the appearance of a viscosity change. I mean, you simply move it and it doesn't respond in the same manner that water responds. But these are bench scale tests, and the applicability to the ICET is still being investigated.

CHAIRMAN WALLIS: What is water glass?

MR. KLASKY: Silicon dioxide.

CHAIRMAN WALLIS: It is, isn't it.

MR. LETELLIER: Next up --

VICE CHAIR RANSOM: You use a drum style viscometer? Is that what you use for measuring viscosity?

DR. HOWE: There was two different instruments used. We did some work bench scale where the experiments were taking place, and that was done with a capillary viscometer, which measured strictly Newtonian fluid viscosity. There was also samples that were transported offsite, where they had -- I never saw the instrument, but I believe it was a drum style where they could vary the shear rate. And I don't remember what the type of instrument was called, but it measured viscosity as a function of shear rate.

VICE CHAIR RANSOM: How significant were the changes that you saw in the non-Newtonian behavior? Are they minor?

MR. LETELLIER: I believe we have that

figure later in the talk.

VICE CHAIR RANSOM: Okay.

MR. LETELLIER: Some other overall findings. There's some evidence that we recovered sediment from the bottom of the tank in excess of the initial mass that we added. However, the mass balance is not extremely accurate, and it's very difficult to estimate the proportion of retained water. But we are looking at that as a reservoir of production and accumulation.

Early in the test, up until recently there was an unexplained lack of silicon in solution, because early testing in beakers had led us to believe or expect a very high dissolution rate. Now it appears that the presence of aluminum can be very important in the solubility of the dissolution rate.

CHAIRMAN WALLIS: Did you do some tests without aluminum?

MR. LETELLIER: We will see tests where the aluminum concentrations are quite low, and the silicon concentrations are much higher.

CHAIRMAN WALLIS: And you still have the same coupons of aluminum in each test? Although some plants don't have aluminum in the same amount. There's a big range of aluminum.

MR. LETELLIER: There's a variation, of course.

CHAIRMAN WALLIS: And if they have aluminum in their insulation, aluminum foil, then that's one thing. Some of them don't have that at all.

MR. LETELLIER: I've been very concerned about aluminum reflective metallics, and I've asked the question many times. I don't believe there's anyone in service that uses aluminum reflective metal. It may still be an open issue.

CHAIRMAN WALLIS: I thought we heard earlier that there were some.

MR. GISCLON: John Gisclon from EPRI. The reflective metal insulation on the piping, it's our understanding that that's all stainless steel jacket and stainless steel foils on the inside of it. There are some older forms of RMI which are around reactor vessels, which may have aluminum foils inside of them, in pressurized water plants.

MEMBER SIEBER: The casings themselves, those are stainless?

MR. GISCLON: Yes, sir.

MR. LETELLIER: So one of the questions would be whether or not that's a legitimate location for debris generation, whether it would be exposed to the chemical environment.

CHAIRMAN WALLIS: This is around the reactor vessel, so it's not somewhere that you really want to go and replace the insulation?

MR. LETELLIER: I'm sure that there are good rationales for why it has not been changed. We've already seen some of the photographic evidence for membrane-like coatings on the fiberglass surfaces. We'll examine that some more. The other frame of that collage illustrates some amorphous semi-solids in suspension.

We also saw a stabilized aluminum concentration after 16 to 18 days, and a corresponding decline in the qualitative rate of hydrogen generation.

CHAIRMAN WALLIS: But it was still producing hydrogen? Presumably you're producing aluminum, and then getting rid of it somewhere?

MR. KLASKY: It could be that the concentration of the aluminum is such that in that timeframe, basically at that point we don't see the continued corrosion. Effectively this oxide layer has terminated the corrosion.

CHAIRMAN WALLIS: So qualitative decline

could mean a decline to almost zero?

MR. LETELLIER: Yes, essentially. That's what was observed. Within the accuracy of our safety examination.

MR. KLASKY: I think the idea that we weren't producing any more aluminum can be realized by the quantity of weight loss in the coupons that roughly corresponds to the concentration that we observe. Meaning, we didn't see a weight loss that would indicate that we were precipitating.

CHAIRMAN WALLIS: So you've saturated something?

MR. KLASKY: No, I don't think we saturated. I think we passivated the aluminum coating.

CHAIRMAN WALLIS: After dissolving a quarter of it?

MR. KLASKY: Roughly.

MR. LETELLIER: Roughly the mass balance of inventory of aluminum in the water is roughly equal to the mass that was lost from the coupons. And we have not seen evidence of a repository of aluminum in the sediment, for example, and no strong indication of aluminum in the fiberglass deposit.

MR. KLASKY: Yes, I was going to say, I

think there are two other indicators that we did not precipitate. The filtered and unfiltered water samples were equivalent in concentration, so we were not able to remove any particles.

CHAIRMAN WALLIS: So you didn't saturate. It's conceivable that at a different temperature, where things proceed at different rates, you might.

MR. LETELLIER: One of the very immediately attributes of determining the solubility is what phase this material is present in. I have to defer to Marc to explain this, but there's substantial difference between amorphous aluminum and crystalline aluminum with regard to its equilibrium solubility with solutions.

MR. KLASKY: I have a number of slides that present evidence of phase transition from the more soluble amorphous to the crystalline structure, and the corresponding changes in solubility. The changes in the crystalline behavior basically are accompanied by changes in pH, and we've observed changes in pH following the test and certainly in our bench scale test.

CHAIRMAN WALLIS: Now, if it's just aluminum that matters, you could do bench scale tests with aluminum alone.

MR. KLASKY: That's --

CHAIRMAN WALLIS: Do all kinds of exploratory stuff.

MR. KLASKY: That's exactly what we've done.

CHAIRMAN WALLIS: Have you explored temperature effects?

MR. KLASKY: We've looked at temperature. And as I indicated, even at our test condition, at 60 degrees, we've seen sizable quantities of precipitant. We've added the boron to explore the role of boron in that regard. I have a presentation that sort of addresses it.

CHAIRMAN WALLIS: Did you go to 80 degrees or something?

MR. KLASKY: We did not. We did not raise the temperature.

MEMBER SHACK: Did you examine the surface of the aluminum plates, and characterize those in any way?

MR. LETELLIER: We did. We've conducted SEM and EDS exams. We're contemplating an XPS surface interrogation. But we had no motivation to do that until at this point now we're trying to think about the competition between the corrosion rate, the introduction of product versus the passivation, and explaining the equilibria, or the pseudosaturation behavior in terms of physical phenomena. We have a huge backlog of samples of that type that would be very interesting to study.

So this slide encapsulates the principal, the overall findings of Test 1. Now we'll get into some of the more specifics. Page Number 20, this just shows you a picture of pre-test fiberglass so you have a mental conception of what it looks like when it's clean. It's a manufactured product, a very regular diameter, about 7 microns in diameter. Looks like spaghetti straws.

The next figure, Page 21, illustrates the nature of the deposits that we've found. Between Day 15 and Day 30, there is some evidence of growth or more complete deposition. But keep in mind how difficult it is to take a sub-sample of 4 cubic feet. We're looking at very large pillows, and we're able to take a few strands in tweezer sizes. And so the spatial sampling is quite sparse.

CHAIRMAN WALLIS: Well, 15-day sample looks as if it could be something that's growing out in sheets from each individual fiber. And 30-day looks like something which is in some mysterious way created windows in those sheets.

MR. LETELLIER: I don't want to mislead you into thinking that --

CHAIRMAN WALLIS: No. This isn't necessarily typical.

MR. LETELLIER: -- these two forms are exclusive.

CHAIRMAN WALLIS: Because there are other parts that look more complete in terms of membranes in that picture.

DR. HOWE: I think to look at the 30-day sample, we need to be clear about what we're looking at. What's shown there is a lot of cracking between --

CHAIRMAN WALLIS: They may have been physically moved apart.

DR. HOWE: Well, again, the scanning electron microscope operates at an extremely vacuum. And so the samples are necessarily dried out completely as part of the analysis procedure. And so what you could envision here is that this is a complete continuous membrane that then cracked, and you know, pulled together, and all these cracks formed as part of the imaging process. So I don't think we can assume that those cracks existed in situ. I mean, so you could --

CHAIRMAN WALLIS: You may have --

DR. HOWE: -- picture this as a progression.

CHAIRMAN WALLIS: -- process here.

MR. TREGONING: I think you have some evidence later from some of the bench scale testing that you looked at an environmental SEM which was hydrated that confirmed that.

MR. KLASKY: Yes, let me speak to that. We did a series of fiber soaking tests, as we called it, where we have the aluminum in boron, and allowed the fiber to soak for roughly a month. And basically only approaching a month is where we saw evidence, and as Rob pointed out, even under environmental SEM which does not require the desiccation of the web. So we were able to produce something that looked very similar to what evidently occurred in the ICET environment.

VICE CHAIR RANSOM: What is this material that's depositing?

MR. LETELLIER: The next slide shows you sort of a rough composition table that's been corroborated by several different diagnostic techniques. It's primarily based on the EDS samples of replicate samples from various fiberglass patches. But we've got similar evidence from ICP and XRF, that it's dominated by oxygen 46 percent, with constituents of aluminum, sodium, and boron. Those four peaks appear continually.

VICE CHAIR RANSOM: Is that consistent with like Al_20_3, or?

MR. KLASKY: Bruce, let me just clarify one thing. The table on Page 22 is precipitant.

MR. LETELLIER: I'm sorry.

MR. KLASKY: Okay, so let's be clear. The table that Bruce was referring to refers to the elemental composition of the white participant.

CHAIRMAN WALLIS: The stuff that's --

MR. KLASKY: Yes, the webbing is much more difficult to characterize. It's on Page 23, yes.

MR. LETELLIER: So this -- I'm sorry. Thank you Marc. This is based on EDS exams that oxygen, sodium, and boron are present in that film.

CHAIRMAN WALLIS: That's not so terribly different. Looks as if the --

MR. LETELLIER: There's an absence of aluminum in the film.

MR. KLASKY: Let me just speak to the difference. In the soaking test, we looked at the growth with and without boron. That is, with aluminum present, only when aluminum was present did we see the web-like structure. And it's sort of peculiar in that aluminum isn't a constituent of the material itself, but yet it was required for the webbing to occur. So that's still something we're trying to understand. But with respect to collaborative methods, we also have performed XRD analysis, and determined that this composition of the sodium, boron, and oxygen is in fact consistent with some of the sodium borates. In fact, tincalconite was a crystalline structure that was observed with XRD.

MR. LETELLIER: The task of identifying and reporting a chemical species is daunting. Needless to say, it's a very complex chemical system, even in Marc's beaker studies where it's a challenge to decide what chemical species is being formed when you know exactly what the ingredients were at the initial condition. So we're working on a phenomenological explanation of product formation, trying to be consistent with the observations and narrow down our choices.

CHAIRMAN WALLIS: How well bound is this stuff to the fiberglass? And if you had much flow through this, would it detach these webs?

MR. LETELLIER: That's an important question. In my opinion, we have no direct evidence at the moment that these webs can form under flow. We've argued about that a lot. There's no particular reason that it cannot form, but we've not demonstrated that. We do have some, I guess, bench-top experiments, if you will, to study that, which will be mentioned later.

CHAIRMAN WALLIS: Maybe you get something else formed when you have flow. Not webs, but streamers, or whatever.

MR. LETELLIER: It's possible. So let's move on to Page Number 24, the kinematic viscosity.

CHAIRMAN WALLIS: This is the dynamic viscosity?

MR. LETELLIER: Yes. That's right. This is the dynamic viscosity as it increased over time, the course of the test, 30 days, at room temperature. And this was reported at, help me Kerry, at 10 minutes of cooling. This was reported under the protocol that we established.

VICE CHAIR RANSOM: This was a capillary viscosimeter?

MR. LETELLIER: Yes.

DR. HOWE: So what was happening here was, as the test progressed, and as we started seeing more precipitant in our solution that had been cooled 23 degrees, that was correspondent to this increase in viscosity that we see over the progression of the test.

VICE CHAIR RANSOM: How does that viscosimeter work? Just gravity head, or?

DR. HOWE: Yes.

VICE CHAIR RANSOM: Okay. So it's always fixed in terms of what shear you're measuring it at.

MR. LETELLIER: I apologize. I do not seem to have the shear rate viscosity reported here, but it is present, and it is reported in the Test 1 report, which you have access to.

CHAIRMAN WALLIS: And this is the mixture of water and what, primarily?

DR. HOWE: The precipitant -- I'm sorry?

CHAIRMAN WALLIS: What is the stuff which has been created which is in this water that is flowing in the viscosimeter?

DR. HOWE: It's the water with the precipitant that's shown on Page 22.

CHAIRMAN WALLIS: It's particulate matter.

DR. HOWE: Yes.

CHAIRMAN WALLIS: And you think the

viscosity change is due to this particulate rather than the actual change in the liquid.

MR. LETELLIER: It is possible. But if you look on the next page, Page 25, the definition of particulate is problematic. Again, we're talking about a chemical precipitant that can exist on a cellular level approaching 10 nanometers, and can become agglomerated into larger effective particle sizes. So this material that's shown in the SEM photos is essentially visible as a white chemical floc. And it is present, it produces upon cooling, it's generated upon cooling and it does affect our viscosity readings.

CHAIRMAN WALLIS: Is it white because it has a lot of reflective surface area?

MR. KLASKY: Actually, I think it's white because of the boron. What we see is the -- if you try to form the gel just with aluminum, it's much more clear. The boron gives it much of its white appearance.

Just one more thing I was going to say with respect to the viscosity, the aluminum is hydrated, and so there is that interaction with the solution itself. So it wouldn't surprise me to note some change in viscosity.

CHAIRMAN WALLIS: Is it the structure that makes it white, or the chemical composition?

MR. KLASKY: I think the boron, we believe to be absorbed on the surface of the aluminum. So it's a specific absorption onto a surface in the inner sphere complex with boron.

CHAIRMAN WALLIS: So it's white the same way that popcorn was white at Davis-Besse?

MEMBER SIEBER: Same stuff.

VICE CHAIR RANSOM: What are the pore sizes in the filters that you're using?

MR. LETELLIER: Our daily filter samples are 0.45 micron, which essentially these products pass through it happily.

VICE CHAIR RANSOM: 0.45 will pass through the filter, right?

MR. LETELLIER: Yes. We've never seen any notable difference between the filtered water chemistry and the unfiltered water chemistry. They're essentially identical.

VICE CHAIR RANSOM: Except there seems to be some difference in viscosity, though, at times I guess, from this plot.

MR. LETELLIER: Keep in mind that the water is filtered directly from the sample tap at test temperature. So at the moment of filtration, it is not present as a semi-solid. But upon cooling, it quickly appears.

VICE CHAIR RANSOM: You mean, even after you've filtered it.

MR. LETELLIER: After it's filtered.

VICE CHAIR RANSOM: You still may get some precipitant forming.

MR. LETELLIER: In fact, you can decant this solution, it will continue to precipitate when you disturb the equilibrium.

VICE CHAIR RANSOM: It would be useful probably to have some error bars, or some measurement of the uncertainty in these measured values, possibly even including that kind of an effect.

DR. HOWE: These particular results you're looking at, there is a lot of variability because the viscosity measurement happened relatively soon after water was taken from the tank and cooled to 23 degrees C. But it was not the exact same amount of time, and what we found was a very temperature and time dependent production of precipitant. So if the test had occurred one minute later one day than it did the previous day, there was variability in that viscosity measurement.

MEMBER SHACK: Yes, I mean, up until Day 12 everything was tight.

MR. LETELLIER: Right.

VICE CHAIR RANSOM: What is the uncertainty under controlled conditions?

MR. TREGONING: I think you're seeing it in that plot. You're seeing it --

VICE CHAIR RANSOM: Well, no. He's giving some reasons why there would be this variation.

MR. TREGONING: There was an attempt to have controlled conditions by specifying the time at which measurements were taken after cooling.

VICE CHAIR RANSOM: But usually the instrument will have some known one sigma type variability.

DR. HOWE: The graphs aren't in here, but on times when we were doing viscosity measurements on liquid, where we didn't have this precipitation confounding the results, the results were the reproducibility was well under 1 percent. It was a quarter percent, or something like that.

VICE CHAIR RANSOM: A quarter percent? One sigma value?

DR. HOWE: There was -- I mean they're not in this set here, but we have nice straight lines of viscosity over the 30 days where every data point falls right on the line.

MR. LETELLIER: I believe this Test Number 1 is the only case where we saw any change in the viscosity, room temperature viscosity.

MR. TREGONING: And the results aren't shown either here, but for Test 1, the viscosity at 60 degrees C was also flat with relatively little scatter. So it was only the room temperature viscosity results for Test 1 that display this behavior.

CHAIRMAN WALLIS: Do you have any idea of the volume fraction of solids? When you've got this viscosity of 1.7. Any idea what volume fraction of solids that corresponds?

MR. LETELLIER: That's a very good question. It's very important to the determination of head loss correlations. It's apparent that this material is obstructing a volume that's much larger than its mass might indicate because of its hydrated properties. We are contemplating a carefully controlled thermogravimetric analysis to try and determine that. We essentially take a very accurate measurement of mass, and heat it in a controlled matter, and note the transitions in --

MR. KLASKY: The exit therms of where the water is.

MR. LETELLIER: Yes, the exit therms, the latent heat of evaporation, for the free water that's present, the hydrated water, and finally the bound.

CHAIRMAN WALLIS: Have you used something like -- this isn't appropriate to use something like an Einstein equation for spheres, you find you've got 20 percent suspended solids, which is a tremendous amount. But I imagine these things are vonal or something, more effective than spheres.

MR. KLASKY: Right, I think that's another important point in the filtration model, that we probably don't have spheres. We have irregular objects that obviously are porous as well.

MR. LETELLIER: We do have some ideas for pursuing that question. We recognize that it's important.

MEMBER SHACK: Just again, the protocol. You bring everything to 23 C, but you get there in a certain amount of time. And so all the tests are done at 23 C in this figure, it's just that at some times it takes you 10 minutes to cool to 23 C, and sometimes it takes you 12 minutes to cool to 23 C, and that kinetic effect is what's really introducing this.

MR. LETELLIER: It's the competition between graduate students and chemical production.

CHAIRMAN WALLIS: They wear sneakers, and they run around the lab.

DR. HOWE: But you're -- just to follow up, you're cooling quicker than 10 minutes, right? Wasn't it cooling the temperature and then holding for a set amount of time?

CHAIRMAN WALLIS: Well, if you hold does the viscosity change?

DR. HOWE: Yes.

CHAIRMAN WALLIS: So this plot is --

MR. LETELLIER: It does not represent a maximum. It represents a --

CHAIRMAN WALLIS: Oh, so you can move it around by holding it for longer.

MR. KLASKY: It's perhaps a not quite accurate definition of viscosity.

CHAIRMAN WALLIS: So ought to put a point on here, Joe, Jim, and Pete, and all that kind of stuff.

VICE CHAIR RANSOM: This is the 10-minute viscosity.

MR. LETELLIER: That's right. We established the protocol simply for uniformity. So continuing to Slide 25. We've talked about the agglomeration of very, very small colloid scale particulates. We've already presented a table of compositions for this material which does show the presence of aluminum. To summarize findings for Test 1, Page 26, the aluminum concentrations saturated at approximately 375 milligrams per liter. After -- we'll look at a curve after 15 - 17 days. The weight loss of aluminum we've already talked about. That's fairly consistent with the observed inventories of aluminum in the water.

CHAIRMAN WALLIS: This weight loss is 25 percent, roughly?

MR. LETELLIER: Yes. I avoid the word "solution". Speaking of aluminum in solution, there's very careful definition of where it resides in the physical system. And we believe that it may be bound in some of these colloid-type particles which is important to know what phase it is present in because it affects solubilities.

Silicon concentrations --

MEMBER SHACK: Just -- you have aluminum here saturated at 375, but the argument seems to be the more recent one is that you're essentially passivating the aluminum.

MR. LETELLIER: Again, that word is to be avoided, but when you examine the curves you see that it flattens out, and there's obviously a competition between processes occurring. To determine exactly what the processes are is the challenge.

MEMBER SHACK: It makes a big difference. You know, if you think you're taking saturation and you're running it through a heat exchanger you get one result than you do if it's really passivating.

MR. LETELLIER: That's very true, and that's why it's important for us to examine the reservoirs, like the sediment on the tank floor. Because if it truly is -- if corrosion is continuing that you've reached a production depletion balance, then you should continue to increase the aluminum in the sediment. We don't see clear evidence of that. We have better reasons to argue in favor of passivation.

CHAIRMAN WALLIS: How does solubility depend on temperature?

MR. KLASKY: The calculations I've made with respect to amorphous phase would indicate that the solubility would be upwards of one gram per liter.

CHAIRMAN WALLIS: And then how does it --

MR. KLASKY: That's at 60 degrees. At 25

degrees, it's closer to about 50 or 60 milligrams per liter.

CHAIRMAN WALLIS: It dissolves more at higher temperatures?

MR. KLASKY: Correct.

CHAIRMAN WALLIS: And that continues up?

MR. KLASKY: I forget what the enthalpy is, but using van't Hoff we can calculate it. But I guess the point is that not only does the solubility change, but changing temperature also induces phase transitions, which cause the solubility to change. So you don't just simply have, you know, a temperature effect with a single phase. You have transformation from an amorphous phase to a more structured phase as time and temperature are changed.

CHAIRMAN WALLIS: And the solubility of the hydrogen is very low, is it? So that almost all hydrogen produced is immediately forming bubbles?

MR. KLASKY: I'm sorry, repeat that?

MR. LETELLIER: We talked about the changes in turbidity, changes in viscosity, and we've also noted the qualitative behavior of hydrogen generation. I don't believe it's a coincidence that the hydrogen rate fell to zero at approximately the same time point that our aluminum concentrations leveled off. The precipitation we've discussed.

The last bullet is perhaps important to note. These photographs of deposits in the fiberglass for Test 1, those deposits were primarily concentrated near the surface of the flocs. The interface between the fiber and the stainless steel. If you broke it open, those deposits were not as prevalent.

MEMBER SIEBER: Do you have a reason why you think that happens?

MR. LETELLIER: We don't have a specific explanation, but in order to investigate that in later tests we built additional confinement structures, like a box arrangement, where the fiber can reside in an uncompact configuration that's not compressed against stainless steel. We've also introduced nylon mesh as a sample holder to remove the conductor insulator type of concerns.

CHAIRMAN WALLIS: I have a question of the court reporter. Is it okay if we go one more slide here? Okay. We'll have a short break for five minutes or something after your observations.

MR. LETELLIER: So, the summary of Test 1, there are several corroborating facts listed here in a long sentence. First of all, we have turbidity that's monotonically decreasing at 60 degrees. We have aluminum that increased to about 350 milligrams per liter and then leveled off, did not change thereafter. We have no appreciable difference between filtered and unfiltered concentrations, and we see that the concentration of aluminum in the water, the inventory, is consistent with the weight loss. Therefore, given the evidence, we think that most of the aluminum remained suspended in the water, and it simply passed through the filters, and that the aluminum did not settle into the sediment bed. It's largely present in the water.

CHAIRMAN WALLIS: But this would mean that you'd be cooling the reactor with a solution of aluminum?

MR. LETELLIER: Heavily loaded. Depending on the explanation for why it leveled off. But keep in mind that this is an isothermal system, and if a heat exchanger perturbs that equilibrium, you may be in a continuous production mode, where you precipitate this floc and make room for more dissolved aluminum, so that corrosion is no longer inhibited; that you're suddenly in a mode of continuous production rather than an equilibrium.

CHAIRMAN WALLIS: Well, if you're boiling it off in the reactor, are you producing aluminum around all the nuclease sites?

MR. LETELLIER: It's out of scope for this project, but it's a relevant question.

MEMBER SHACK: If it had been passivated, that won't happen.

MR. LETELLIER: If it's passivated, that seems to be the leading hypothesis at the moment.

CHAIRMAN WALLIS: Yes, but if you're boiling it.

VICE CHAIR RANSOM: Did anyone think to include zirconium or zirconium alloys as a metallic species in these tests?

MR. LETELLIER: We did consider it. Largely considered to be an impervious metal, something benign in the presence of reactor cooling system water. It is not present in the test matrix.

MR. ANDREYCHEK: Ralph? We did look at zirconium as a species. It doesn't participate in the chemical reactions that we would have expected to see, and that's based on input from our fuels people.

MR. LETELLIER: You'd have to ask yourself how much different the accident environment, in particular how much higher the pH might be compared to the RCS system where it normally resides. Our explanation for these observations at the moment is we cannot rule out the presence of a colloidal size particle that does not or would not affect the bulk viscosity, but yet it does not settle, and it does not affect the turbidity which we measured. But yet it provides a reservoir to maintain the very high aluminum inventory.

CHAIRMAN WALLIS: Colloids might presumably stick on the filter cake.

MR. LETELLIER: There may be some deposition mechanisms. But this allows you to achieve a measurement of aluminum in excess of the saturation point that you might infer from a handbook value. When you look up aluminum, you're more likely to find a value for crystalline phase. That's not what we have here. And we seem to have an explanation that permits that behavior.

VICE CHAIR RANSOM: Certainly the not settling would be typical of a colloidal suspension, but not affecting bulk viscosity, and the effect on turbidity are not -- I mean, classical colloidal suspensions, I think you would see effects in both of those, but not in the tendency to settle, of course.

MR. LETELLIER: Are we at a point for a break?

CHAIRMAN WALLIS: Yes, I think some of us may need a quick break. Can we come back at 2:00? Can you remember that on this clock here? Is that okay?

(Whereupon, the foregoing matter went off the record at 1:52 p.m. and went back on the record at 2:00 p.m.).

CHAIRMAN WALLIS: Let's go back into session. And we're off. On the record.

MR. LETELLIER: The presentation continues with a comparison of findings between Test 1 and Test 2. And I'd simply remind you that Test 2 was a variation of the pH control system. Test 2 uses the tri-sodium phosphate. Again, both of these have 100 percent fiberglass debris.

This is presented in sort of a counter-comparison of results side by side. First of all, the Test 1 water samples. At room temperature, the precipitants were visible after the first several days of the test. And this photograph illustrates what was present in all of our sample bottles. This material is easily agitated. Even after months post test it is not congealed into a solid, semi-solid body. You can shake this up and it takes a couple of days to resettle out of suspension. The amount of this precipitation apparently increases with time over the course of the test. You can line up these bottles of equivalent volume, and there's more, and more, and more, and more. And again, the precipitant seems to form more quickly with cooling as the test progresses.

CHAIRMAN WALLIS: And if you heat it up again does it disappear into solution?

MR. LETELLIER: Not entirely. Our examination of that is somewhat qualitative in nature. Some of it does re-dissolve, but not to the same extent that it precipitates with temperature.

MR. KLASKY: Yes, let me just speak a little to that point. The quantity of re-dissolution I think is a function of whether the material has really crystallized at that point. If it crystallizes it's not going back. So it's, again, a time dependent problem.

MR. LETELLIER: At the test temperature of 60 degrees, this precipitant is not visibly apparent, although the TEM samples of a water drop show that there are sub-visible amorphous particulates present.

The next slide -- Kerry's reminding me that the TEM images are also desiccated because they're performed under high vacuum. And the point is that --

DR. HOWE: They're also at room

temperature. So you're observing the particles at room temperature, not at test conditions.

CHAIRMAN WALLIS: So you don't really know what's there at test condition?

DR. HOWE: No, but bench scale tests we've performed with dynamic light scattering to understand the colloidal particles at 60. So I'll present that.

CHAIRMAN WALLIS: You will present that. Good.

MR. LETELLIER: That's one of the major challenges is to examine the in situ condition.

CHAIRMAN WALLIS: Right.

MR. LETELLIER: The temperature is the most difficult thing to control with some of these refined diagnostics. Page 29 shows a similar information for Test 2. Essentially nothing to report. No precipitants were visible at either room temperature or test temperature.

VICE CHAIR RANSOM: Test 2 you didn't put as much sodium hydroxide into it.

MR. LETELLIER: There was no sodium hydroxide. This was a TSP system.

VICE CHAIR RANSOM: Okay.

MR. LETELLIER: Both of them had fiberglass as the only debris type.

MR. KLASKY: I think one thing I want to point out. With respect to the aluminum, the solubility at the pH of 7 is very low. So you wouldn't perhaps see it. The sensitivity of ICP is on the order of percentages of milligrams, so it would be difficult to observe with this measurement.

VICE CHAIR RANSOM: Well, the pH of 10 is a chemical reaction that's going on, is that right?

MR. KLASKY: With respect to the hydration? The hydration?

VICE CHAIR RANSOM: With respect to the aluminum. Whatever is putting the aluminum --

MR. KLASKY: The aluminum, what's putting it into solution is dissolution. And what's driving dissolution is that the concentration gradient. So the solubility is high enough such that you can establish its, you know, concentration gradient, whereas where you have solubilities on the order of tenths of milligrams, you really don't have a concentration gradient to speak of.

MR. GISCLON: I think what Marc's referring to, and I guess I'm just going to try to clarify, aluminum hydroxide, the solubility of aluminum hydroxide is very pH dependent. At pH 10 it's reasonably soluble. At pH 7 it's very insoluble.

MR. LETELLIER: Slide 30, Page 30, shows a comparison of the aluminum inventories between the two tests. This is a figure that you had questioned before.

CHAIRMAN WALLIS: Yes, that's okay. We see the whole thing now.

MR. LETELLIER: Yes, you can see the entire scale with all of the data presented. There is a fair amount of scatter in the data, but it's not too difficult to imagine a plateau of some kind, if not a maxima with some kind of decline, which is also a possibility. This matches very well the semi-qualitative hydrogen generation behavior, which seems to stop about Day 17 as well and fall to zero. In Test 2, the concentration of aluminum --

CHAIRMAN WALLIS: The right-hand one is aluminum? It doesn't say, does it. It was all silicon? It's all silicon.

MR. LETELLIER: Page 30 is aluminum.

CHAIRMAN WALLIS: There's nothing there.

MR. LETELLIER: For Test 2 there was none observed.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: And again, Marc and Kerry have just described the very sensitive solubility at low pH.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: If corrosion was occurring, it did not appear in the water. It may have deposited somewhere else.

CHAIRMAN WALLIS: So because there was no aluminum you could get some silicon? Is that it?

MR. LETELLIER: That's shown in the next page, on Page 31. For Test 1 there was a lot of aluminum present. We argue that the silicon levels were very small. Somewhat surprising at the time, it seems to have a physical explanation. In Test 2, there was no aluminum, and that allowed the silicon to enter the system. It also shows some evidence of a saturation type of behavior.

CHAIRMAN WALLIS: Everything is saturated to Day 20 so far.

MR. LETELLIER: There was a lot of debate about the duration of the tests, whether 10 days was enough, or 60 days was necessary.

CHAIRMAN WALLIS: Well, it's a lunar cycle is what it is.

MR. LETELLIER: Coincidentally, I think we've been able to capture some important behavior within this time scale. Now, whether or not there's more to be learned and investigated on longer scales is an open question. But there's plenty to keep us busy over the 30-day course.

Page 32 shows a comparison of the deposits in fiberglass. Again, the panel in the upper left shows the clean fiber for comparison. You've already seen deposits for Test 1 on Day 15 and Day 30. Because of the extent of deposits on Day 15, we instituted a sample extraction on Day 4 to try and catch this deposition even earlier. And indeed it is present. There are deposits present in the Test 2 chemical system as early as Day 4, if not sooner.

MEMBER SHACK: But in Marc's bench scale experiments, he needed aluminum.

MR. KLASKY: With respect to --

MR. LETELLIER: That's sodium hydroxide.

MEMBER SHACK: Ah, sodium hydroxide solution.

MR. LETELLIER: Again, keep in mind, Test 1 is sodium hydroxide high pH, Test 2 is tri-sodium phosphate with a neutral pH.

CHAIRMAN WALLIS: So you got these sort of web-like deposits in both tests.

MR. LETELLIER: Physical attributes are

different. They visibly look to be different. In particular, the deposits for Test 2 do not appear to involve the surface of the fiberglass. Underneath this deposit it appears to be pristine fiber. The same thing could not be stated as conclusively for Test 1. You don't know if these crust-like deposits are growing out of the surface, or if the silicon is participating. At least it's my opinion this looks like an external deposit.

The other attribute of Test 2, important attribute, is these deposits were formed throughout the fiberglass. It did not show the surface dependence that we noted in Test 1.

CHAIRMAN WALLIS: Now, if these deposits had material from the fiberglass in them, would that be detectable?

MR. KLASKY: With respect to EDS, we can attempt to focus the beam --

CHAIRMAN WALLIS: Just on the deposits?

MR. KLASKY: Exactly. And --

CHAIRMAN WALLIS: It can get that fine?

MR. KLASKY: Yes. Yes.

MR. LETELLIER: Easily.

MR. KLASKY: And you know, whenever you see the silicon present, you know you can question as to whether the beam in fact hit the fiber. But as Bruce stated, the uniformity of the deposits is consistent, that is in Test 2, consistent with the notion that the material is emanating from the fiber itself. That is, we have the high silicon concentration in solution.

MEMBER SHACK: But he just said it looked pristine?

MR. LETELLIER: I acknowledge the contradiction. We do have a high silica inventory in solution. The fiberglass is not the only source of silicon. The crushed concrete is a source, the soil itself in the latent debris.

MEMBER SHACK: Now, what did your collar specimen look like? Did you look at it for this deposit with the higher velocity?

MR. LETELLIER: Yes, we did. But again, that fiberglass collar around the drain is confounded by the presence of high quantities of particulate, of grains of sand to fugitive fibers. It's much more difficult to make those interpretations, although I would not hesitate to say that similar deposits are present.

DR. HOWE: I think the other thing we need to consider here is mass balance issues. You know, we've got 100 milligrams per liter of silicon in solution. We've got roughly 1,000 liters in the tank, so that comes to 100 grams, or 0.1 kilogram of silicon out of the -- how many pounds did we put in? How many kilograms of fiberglass are in the tank?

MR. LETELLIER: About 10 pounds.

DR. HOWE: So the point being we could get this much silicon in solution without noticing a big change because there was so darn much fiberglass in the tank.

So, Slide 23 presents a slide-by-slide comparison. For Test 1, it's important to note that we did not have a Day 4 sample for comparing the deposits. That's evidence for continuous quality improvement. We're trying to tailor fit the protocol to match what we learned. Again, the surface effect is noted there, within a few diameters. That's where the highest concentrations of deposit present. Substance encompasses all of the fibers. That is it grows around them. It also spans the spaces between fibers. And less of this material was found in the center or the interior portions. I wouldn't say it does not exist. That's a strong statement.

CHAIRMAN WALLIS: So you didn't change the flow pattern in any way.

MR. LETELLIER: No. In fact, again I'd reiterate that these sample coupons are quite quiescent, approaching zero flow at the interior of the blanket. In Test 2, we saw the substance evident as early as Day 4, if not sooner. It was dispersed. It has a much different morphology. It's not an encrusted substance. It looks like it might detach easily under flow, but that has not been tested.

CHAIRMAN WALLIS: So one wonders --

VICE CHAIR RANSOM: Is it a sodium borate also?

MR. LETELLIER: I have to admit that Test 2 has not been examined as thoroughly. We are generating data much faster than it can be diagnosed. So I hesitate to speculate at this point.

CHAIRMAN WALLIS: Do you do bench-top tests with fiberglass alone to get the similar effect? Then metals and metal coupons aren't participating at all.

MR. KLASKY: The answer to that question, upon soaking of just fiber with sodium hydroxide, no we did not observe any webbing. I have some pictures of that.

CHAIRMAN WALLIS: And with TSP?

MR. KLASKY: We did not try TSP.

MR. LETELLIER: It hasn't been examined yet. The next slide showed some of the EDS spectra for these deposits. Here's a partial answer to your question. For Test 2, the spectra is much more complex. It's dominated by oxygen and silica. Silicon is present, as is phosphorous, which is not surprising. Don't be confused by the golden plate.

CHAIRMAN WALLIS: Yes, I was wondering about that.

MR. LETELLIER: We've reinvented alchemy for our testing. The gold palladium alloy is a sputtering coating placed on the samples to prevent electron charges from building up. So it's an external constituent there. It's present in almost every spectrum.

On the left-hand side, that spectrum for Test 1, that's the classic. Four constituents appear over and over, boron, oxygen, sodium, aluminum.

CHAIRMAN WALLIS: That certainly looks like webs.

MR. LETELLIER: It evokes various thoughts, from parachute nylon to saran wrap. It's a very --

CHAIRMAN WALLIS: Something out of Harry Potter.

MR. LETELLIER: And again, I'd remind you that these are desiccated samples, and simply the fact that this retained its continuity without cracking tells you something about the surface tension involved. It's a structurally robust material.

MR. CARUSO: So those are very thin.

MR. LETELLIER: Keep in mind that the diameter of the fiber is roughly 5 to 7 microns, and so yes, these are very, very thin films. Now, it doesn't mean to say that it can't build up to thicker quantities. As you survey a given sample, you will find locations where you can look at the edge, the thickness of this sample, and infer a much thicker deposit.

CHAIRMAN WALLIS: You have no idea the strength of these webs?

MR. LETELLIER: I think one of the primary objectives of ICET was to look for the presence of --

CHAIRMAN WALLIS: But presumably you had -- let's take the sample and do something to it, and you cut it up or something, and the webs are still there. So they must be fairly tenacious.

MR. LETELLIER: Again, keep in mind the spatial scale here. This is maybe 200 micron square, and this is a very small spot on a tweezer-size sample that came out of the --

CHAIRMAN WALLIS: But it's all webs. All the spaces have webs in them.

MR. LETELLIER: Yes. Microstructural properties are difficult to measure. I would much prefer to test the head loss properties under flow.

CHAIRMAN WALLIS: As long as we're getting at it. And if each one of these membranes can take a pressure drop of somewhat, you may not be able to get much through that at all.

MR. LETELLIER: Well, one idea that may be mentioned later is to incubate coupons that are pre-configured for head loss testing. And in fact, in Tests 3, 4, and 5, we have introduced samples like that. My initial concept is to introduce the sample in a simple drain column, put a static head, and open a valve, and simply do the kinematics to measure velocity through clean fiber versus loading fiber.

CHAIRMAN WALLIS: Maybe there's a yield stress or something.

MR. LETELLIER: There may be, and there may be evidence. Maybe something to learn about how it flushes, or it's released from the system as well. Those types of studies could be performed relatively quickly, and we're working out a draft procedure with the industry.

VICE CHAIR RANSOM: Is there any significance to the difference in scale between those two plots? I mean, the ordinate?

MR. LETELLIER: Actually, I think the images are distorted, but the scales are very comparable. They extend to 10 --

VICE CHAIR RANSOM: One shows going zero to 200. The other one is zero to 2k.

MR. LETELLIER: The ordinate is simply a bin frequency number. It's a counting number. What this is evidence of is a difference in the collection time, and also in the intensity of the signals.

VICE CHAIR RANSOM: No significance, huh?

MR. LETELLIER: It affects your interpretation of accuracy and the percent errors that you would assign to each peak. The last step, and you will read this in the test report, the last step to interpreting these spectra is to convert the peak intensities to a percentage of composition. But that's -- there was a lot of variability involved with that process. It involves the linearity of the detector response function. It involves the self-absorption of the signal within the sample. There are a number of calibration fudge factors that are involved. It also requires the operator to select which of these peaks that they wish to fold into the mass balance. And so you will see quite a bit of variability in what we call semi-quantitative compositions.

ICET 3. This is the overview. Now, I'd remind you to look back at the test matrix. Test 3 is a tri-sodium phosphate test that now contains 80 percent cal-sil by mass. That's an enormous quantity of this material. It's 45 to 50 pounds of material, and you will see what a burden that places on a tank. The turbidity again was initially very high, exceeded our measurement limits shortly after the cal-sil was added. It decreased to within measurement range just prior to initiating the spray phase. For your reference, Time Zero is when we initiate the spray. That's when 30 days starts.

This has already been noted anecdotally. After 30 minutes into the TSP injection, the turbidity actually increased, and a visible white precipitant was seen, observed to swirl through the tank. After the hydrochloric acid mixture was injected, the turbidity came back down at the conclusion of the 4-hour spray phase, and then within a day it was quite clear. This white precipitant seems to have been generated, or perhaps it could have been generated as a calcium phosphate compound. I'm going to let Kerry elaborate on this, but this precipitant partially covered everything in the submerged region. There was a significant quantity that --

CHAIRMAN WALLIS: There was a snowstorm of this stuff?

MR. LETELLIER: Essentially it became the perfect flow tracer. You're asking about velocity tests. You could see the eddies and the internal current.

CHAIRMAN WALLIS: Does it settle out, or is it always?

MR. LETELLIER: Eventually it did settle out.

CHAIRMAN WALLIS: You get fairly big piles of it? But you say in fairly large quantities. That doesn't -- what does that mean?

MR. CARUSO: Did you take any videos of these?

MR. TREGONING: You can't -- there's not enough visibility into the chamber to really have any sort of meaningful video. But there are copious amounts of pictures that are taken, especially pre- and post-test.

CHAIRMAN WALLIS: But fairly large quantities.

DR. HOWE: That is a little bit of a difficult question to answer because what went into the tank was 40 pounds of pulverized calcium silicate, so we ended up with, you know, 8 or 10 inches of deposits of cal-sil on the bottom of the tank. And if we added some quantity of this to the top of that, it's hard to distinguish.

CHAIRMAN WALLIS: But you say fairly large. So what does that mean? That must mean something to you.

MR. LETELLIER: It means that you could visibly distinguish the presence of the white deposit compared to the cal-sil debris.

CHAIRMAN WALLIS: So you've got a blanket of this stuff covered the cal-sil.

MR. LETELLIER: Yes. Including the dust.

CHAIRMAN WALLIS: It was an inch or two thick or something?

MR. LETELLIER: No, it's --

MR. KLEIN: If I could jump in, I think we did make one measurement at the end of the test, and near the center of the tank by the drain collar was about an inch and a half thick of this deposit on top of it.

MR. LETELLIER: But it is mixed with particulate. I mean, that's an important point to keep in mind. It's not fully segregated by any stretch of the imagination.

MEMBER SIEBER: The purpose of -- just to stay on point, the amount of cal-sil that you added -- what was available in a typical containment.

MR. LETELLIER: The amount of cal-sil debris was intended to represent the amount of cal-sil debris that might be formed, might be generated, according to our best information about debris generation, and also according to our best information about particle size. You'll see in the next slide that some of the debris was introduced as inch to two-inch cubes, large pieces. Some of it was pulverized into a free-floating particulate.

When you mentioned spatial scale, I would have you note that because the cal-sil bed was so deep, certainly not 100 percent of that was available to participate in the chemical system. It was sequestered, if you will, by -- it self-shielded from participation in solution chemistry.

Next page, 36, again discusses the white precipitate, and how it evolved. As Paul Klein mentioned, this white shiny substance had the texture of a face cream. It was almost a very finely divided solid, if not --

CHAIRMAN WALLIS: More like talcum powder, or it was finer than that?

MR. LETELLIER: Opinions will vary. Again, these are the scientific adjectives that we apply to what we observe.

CHAIRMAN WALLIS: Something like sunscreen?

MR. LETELLIER: Yes.

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

MR. LETELLIER: Yes. Like lotion. And at the center, here's the drain column. That's where you would expect the largest accumulations.

CHAIRMAN WALLIS: Now, this cal-sil is immobilized during this? It just settles down, and that's mostly after this? The turbidity came down, that means the cal-sil's all settled out? So you're not trapping this face cream in your cal-sil?

MR. LETELLIER: Well, even before the sprays are initiated, a substantial portion of the cal-sil is present on the floor. But the water is very turbid with the suspended residue. This flocculent was observed within tens of minutes, so certainly it was combined with the suspended cal-sil.

MEMBER SHACK: But again, the cal-sil goes in at the very initiation of the test like four hours before the spray?

MR. LETELLIER: Before the spray. Then the coupons are loaded, the racks are loaded. The pool is continuously circulated, but that does not preclude gravitational settling.

CHAIRMAN WALLIS: Especially at the velocities you have. So now you said these tests were at zero to 3 centimeters a second or something. Which was the velocity in these tests? It was always 25 gallons per --

MR. LETELLIER: Always the same.

CHAIRMAN WALLIS: That was the highest velocity?

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: That you intended to test? Or is it the middle, or what?

MR. LETELLIER: We had a phone call over lunch to clarify that issue, and the design criteria was that the range of zero to 3 centimeters per second be present across the metal plates, across the submerged metal plates. So I was incorrect in my earlier statement. The outlet velocity along the manifold was quite a bit higher.

CHAIRMAN WALLIS: Okay. That's what I suspected. But you said -- because it seemed to be awfully low, what you gave me.

MR. LETELLIER: The outlet velocity, which is representative of circulation in the pipe, is approximately 75 or 80 centimeters per second in the 2-inch lines. So there is an acceleration term directly below the coupon rack.

The other finding that's mentioned here in Test 3, this was our first experience with cal-sil, and we actually had a flow meter malfunction on Day 8. As I said, the plumbing layout, the piping layout is flexible enough to valve off that meter, and to isolate it and examine the problem. After cleaning and then reinstallation, the flow meter operated without failure for the remainder of the test, and no additional deposits were apparent on Day 30. So this accumulation occurred very early when the calcium silicate loadings were extremely high. And the next page shows you some photographs of what that looks like.

MEMBER KRESS: That piping wall, could you tell me which way is the vertical orientation?

MR. LETELLIER: Well, these segments have -- these spools have been pulled out for the purpose of examination, but this accumulation occurs in the vertical -- thank you, Kerry? This flow meter is placed in a vertical pipe section.

MEMBER KRESS: Well, I was thinking more of this picture on the right. The reason I'm asking is it looks like it's settled out because of the -- it doesn't seem to have an orientation to it.

CHAIRMAN WALLIS: On the left, too.

MR. LETELLIER: It's a little bit misleading because the gasket is not centered. What you're seeing is the face of the flange. More importantly is to look at the scratch marks on the wall, which is essentially a thumbnail. It's not tightly adhered. It is loosely accumulated. It's easily dislodged. But on the left-hand panel, if you can see the three-pronged struts of the flow meter, it has visibly accumulated on that obstacle.

CHAIRMAN WALLIS: It does stay stuck at these 80 centimeters a second then.

MR. LETELLIER: Yes. Now, one of the interesting things is we -- Oh, this is one and a half inch pipe in this test line.

CHAIRMAN WALLIS: Is it even higher here?

Several feet a second?

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: We have not observed this type of accumulation for calcium silicate loadings at room temperature in normal tap water. We've done extensive head loss testing. Some of the other laboratories are building complementary head loss loops. They're thinking about their cleaning procedures, and testing at room temperature. They have not observed the same level of accumulation. That might lead you to suspect that the chemical environment is important at this point.

The next test is impressive. The next slide illustrates just how much calcium silicate debris is present. And these monitors don't do the photographs justice.

CHAIRMAN WALLIS: Insulation bag.

MR. LETELLIER: Let's see if I can find my way through this mess. Keep in mind that the bottom of the tank has a sloped bottom. The lowest point near the center drain is about 18 inches from the bottom of the coupon rack. And you can see that the cal-sil with the sample bags on top is almost that deep. It's a good 12 to 15 inches deep, full of this cal-sil particulate.

CHAIRMAN WALLIS: Insulation bag is a fiberglass pillow? Is that the same thing? So an insulation bag is a fiberglass pillow, is a same thing is it?

MR. LETELLIER: Yes. On the left-hand panel to the left, you can see that there is a stainless steel bag that extends about three feet long. If you watch the monitor, the arrow can illustrate that, from the bottom to the top. This is a mesh bag containing fiberglass.

CHAIRMAN WALLIS: Am I supposed to see an arrow somewhere? A mouse arrow.

MR. LETELLIER: It's a mouse arrow.

CHAIRMAN WALLIS: Tiny little thing. Oh, that one. Okay.

MR. LETELLIER: That's the bottom, and this is the top of the sample bag. On the right-hand side, if you can distinguish from the resolution, you can see the large pieces of cal-sil present underneath the stainless steel. This represents the larger pieces that would be generated during debris generation. There's just an enormous amount of this material.

CHAIRMAN WALLIS: There seems to be

something stuck on the coupons too.

MR. LETELLIER: There's an arrow on the right-hand panel that points to the white deposits on some of the metal coupons.

CHAIRMAN WALLIS: They're more evident in the left. Okay.

MR. LETELLIER: They're the same, actually.

CHAIRMAN WALLIS: Same stuff.

MR. LETELLIER: They're the same stuff. Keep in mind that this photo was taken after the solution was drained, so there is some residue on all of the surfaces, and there are corrosion products present in the samples.

The presence of cal-sil confounds our interpretation of chemical deposits. It's a very messy type of debris. However, it's becoming apparent that it's important in the chemical system. Now, whether it requires this much actually as a mass fraction in order to examine, I have my doubts. But nonetheless, this is representative of the current regulatory assumption for cal-sil debris loadings.

The next image shows you what the raw calcium silicate looks like close-up. Again, in the past we've talked about the fact that calcium silicate has microporosities on almost every spatial scale. It looks like a sponge. The closer you get, the more structure it seems to have.

CHAIRMAN WALLIS: It has some fibers in it.

MR. LETELLIER: And it also has its own fibrous binder to give it tensile strength.

CHAIRMAN WALLIS: That's added to it on purpose?

MR. LETELLIER: Yes. This is part of the manufacturing. And I showed this picture so that when we look at the next frames you can judge for yourself whether or not the deposits are actually calcium silicate contamination, or if they're unique to the chemical reactions.

So the next slide shows you a series of expanded photos for the deposits observed in Test 3. Test 3 is the TSP system of neutral pH with the cal-sil loading.

CHAIRMAN WALLIS: So you've still got something growing out of the fibers or whatever? In sheet-like form?

MR. LETELLIER: I would say physically it's more similar to what we saw in Test 2, the TSP system. And they may have similar origins. My opinion is this looks different than the cal-sil itself. This is actually taken from the exterior of a high flow sacrificial sample. But again, the adjective high and low flow is somewhat misleading because they were sequestered at the interior of a larger blanket.

MEMBER DENNING: I want to make sure I understand what I'm looking at. This is actually NUKON?

MR. LETELLIER: Yes.

MEMBER DENNING: This is the NUKON, but interspersed with it is some cal-sil that by some transport mechanism has gotten in?

MR. LETELLIER: Well, the composition of this deposit is the point in question, whether or not this truly is the cal-sil particulate that was freely suspended, or whether this is unique to a chemical reaction.

VICE CHAIR RANSOM: How did you introduce the fiberglass cal-sil mixture? Your 80 percent cal-sil, 20 percent fiberglass.

MR. LETELLIER: Again, the fiberglass is always sequestered in stainless steel --

VICE CHAIR RANSOM: Oh, so they're separate.

MR. LETELLIER: They're physically separated. Nonetheless, there is an enormous surface area in both of these products, and they do commingle from the point of view of chemistry. But it's not as if we stirred them up in a bucket and poured them together.

MR. TREGONING: Just a question, point of clarification. I know in Test 3 there were some samples of insulation added after the cal-sil, the bulk of it at least, had settled. And part of the reason for that was to try to discern differences between cal-sil particulate loading and other products which make more. Can you comment at all on any differences between at least visible evidence between fiberglass that was added after the bulk of the settling had occurred versus these samples, which were in the tank from the inception?

MR. LETELLIER: Test 3 was our first attempt to introduce prepared coupons for potential examination. And those coupons have not been examined yet. So I'm afraid --

MR. TREGONING: It's still being studied.

MR. LETELLIER: Nonetheless, the opportunity exists. We introduced fiberglass after substantial clarification had occurred. On Day 4, we were extracting samples for other reasons, we introduced the additional fiberglass. And so they are not likely to contain the high cal-sil loadings that we see here.

CHAIRMAN WALLIS: Can you explain this? Can you explain again what you mean by bird cage and low flow?

MR. LETELLIER: Slide Number 41, there was always the question from as early as Test 1 whether the deposits were preferentially forming at the interface between stainless steel mesh and the fiberglass itself. And so we built an alternative holder sample cage. We affectionately call it a bird cage. It's essentially a 4 x 4 x 4 inch cube. It's a stainless steel box where we can put in a small amount of fiber, and it is not compressed. It resides in a more natural status that you might expect for a pile of debris on the containment floor. Now it should be hanging -- yes, I guess that is. In Slide 38, the bird cage is placed on top of the sample basket to the left. You can see the square -- or the cube arrangement. However, it's been inundated by the presence of the cal-sil. And the close-up in the right-hand panel shows it as well.

VICE CHAIR RANSOM: So it looks like flow goes through it, but it's mostly stuff sticks on the surface, huh?

MR. LETELLIER: That's my interpretation as well.

CHAIRMAN WALLIS: So that mesh of steel we see there, that's the outside of the bird cage?

MR. LETELLIER: The cage. If you want to attempt to follow the cursor on the video screen, I'll show you. On the right-hand panel.

CHAIRMAN WALLIS: Yes, that's what I thought.

MR. LETELLIER: Yes. That's a close-up of the bird cage, which was placed on top of a much larger.

CHAIRMAN WALLIS: It's hard to tell depth in that picture.

MR. LETELLIER: It's approximately 4 to 5 inches cubed.

CHAIRMAN WALLIS: So it's sitting on top there? It's really above the background there, isn't it?

MR. LETELLIER: Yes, it is sitting on top of the fiber blanket.

VICE CHAIR RANSOM: The fiber blanket looks kind of the same. Why is it so different in ….

You know, here it shows exterior, exterior, exterior, and it doesn't show the same kind of exterior behavior in the bird cage that a normal blanket does.

MR. LETELLIER: You're referring to Slide 41?

VICE CHAIR RANSOM: 41.

MR. LETELLIER: Comparison of SEM images. Again, spatial heterogeneity is difficult to explain. It exists, and it does skew, or -- well, it affects your interpretation of these findings. And I would never claim that we have a complete spatial sample.

CHAIRMAN WALLIS: What's called exterior looks quite different.

VICE CHAIR RANSOM: In 38 the exterior looks the same.

MR. LETELLIER: Let's examine the low flow samples. This is a small 4-inch square envelope containing a small amount of fiberglass. When you take off the stainless steel jacket, on the surface of that sample, that's referred to as exterior. That's before you break it open and look inside.

VICE CHAIR RANSOM: Well, where is the low flow over in 38?

CHAIRMAN WALLIS: It's different? It's also a cubed 4 x 4 x 4?

MR. LETELLIER: No. It is a square envelope. It's a packet which has been placed inside of the larger blanket. And it was intentionally located in an area that we thought was higher flow than what we deemed the low flow sample. Again, I have to caution --

VICE CHAIR RANSOM: Well, wait a minute, on 38 when you pointed out this square, which one is that?

MR. LETELLIER: That is the bird cage. That was an alternative configuration to avoid compaction.

VICE CHAIR RANSOM: That's the bottom, right?

MR. LETELLIER: Yes.

MR. TREGONING: Just a point of clarification. Exterior on the low flow samples does not mean that it was exterior to the chemical environment in terms of the solution itself.

MR. LETELLIER: It means the top, the surface layers --

MR. TREGONING: Of that sample.

MR. LETELLIER: That sample.

MR. TREGONING: Which was contained in a larger insulation bag.

CHAIRMAN WALLIS: Which was laid on top of something?

VICE CHAIR RANSOM: Or you said it was buried inside the blanket or what?

MR. LETELLIER: It's very difficult to explain without a figure.

CHAIRMAN WALLIS: Maybe we'll never understand.

MR. LETELLIER: If you can imagine a pillowcase that's quite long, and you put your envelope in the middle, and then fold the large blanket. That's what I mean by inserted in the interior of a larger object. It's essentially --

CHAIRMAN WALLIS: Low flow is protected by the pillowcase.

MR. LETELLIER: And that raises the next question. Please don't be misled by the adjectives of high and low. It is my opinion that all of these fiber samples were at very low quiescent condition.

VICE CHAIR RANSOM: Well, exterior and interior might be deceptive, too.

MR. LETELLIER: Yes.

CHAIRMAN WALLIS: Because it's really --

VICE CHAIR RANSOM: The exterior of the low flow sample might be equivalent to the interior --

CHAIRMAN WALLIS: So what's your conclusion from these figures?

MR. LETELLIER: The reason that the interpretation of interior/exterior is still relevant is because we were concerned about the interface between the packaging, the stainless steel bag, and the sample itself. So that distinguishes between the surface and the true inside.

I need to ask for some clarification of the bird cage samples. The left-most figure actually looks like the top of the stainless steel mesh itself. This type of deposit in the center is very similar to what we see accumulate on a filter paper. In almost no other circumstance do you see that level of homogeneity. The interior is very obviously the inside of the fiber sample that was placed in the bird cage.

CHAIRMAN WALLIS: Now, is this exterior bird cage, is that cal-sil particles, or is that some other kind of precipitant? Is that the flocs?

MR. LETELLIER: The answer is yes. Obviously it was located --

CHAIRMAN WALLIS: One or the other, is that what you mean?

MR. LETELLIER: Both.

CHAIRMAN WALLIS: Both.

MR. LETELLIER: Clearly it contains mixed together. Because of the time scale at which the flocculent was observed to be produced, it was clearly mixed, intermingled, with the cal-sil itself.

CHAIRMAN WALLIS: I'm just sitting here thinking, as I thought earlier today, how does all this detailed stuff help some engineer who's trying to predict something useful?

MR. LETELLIER: Again, what you're seeing is our attempt to struggle with the very complex experimental.

CHAIRMAN WALLIS: Yes, but you know what I'm getting at. At the end of the day there has to be something useful of an engineer.

MR. LETELLIER: That's a fair question. We're trying to isolate the chemical products from --

CHAIRMAN WALLIS: You may be doing the right thing at this stage, but I can see then there's going to be something that follows this before it's useful.

MR. LETELLIER: Yes, there's obviously an important piece of information. This looks alarming. It looks like a potential problem, but we have no direct evidence that it will impede flow. We have no direct evidence it will form under flow. Those are open questions.

MEMBER SIEBER: Until you test, flow test the system, you won't know the answer to that. So that's the next step. Now you know you can form it. You know what the constituents are, and the conditions that are there. You probably classify plants with regard to the debris content, but until you flow test you really aren't going to know.

MR. LETELLIER: Part of the rationale for not testing head loss at this point was that the industry in particular did not, and netiher did the staff. We did not want to perturb the evolution of the chemical system by filtering some of your important constituents.

MEMBER SIEBER: Well, it's better to take it separately, because if you're trying to accomplish some mechanical test, you're going to lose a lot of the chemistry insights.

MR. LETELLIER: Right.

MEMBER SIEBER: And I don't think that's what you want to do.

MR. LETELLIER: And even if we proceeded with those tests, we would have to think very carefully about at what point during the 30 days you would wish to accumulate this material and test its properties. I personally like the strategy of incubating this material, and then pulling out samples time-dependent evolution.

CHAIRMAN WALLIS: It's an integral test where you have a sump and a filter and a reactor.

MR. LETELLIER: Because of some of the time dependencies, that may be the ultimate solution.

CHAIRMAN WALLIS: No, you have flanges, so you can do tests.

MR. LETELLIER: That's right.

CHAIRMAN WALLIS: But I understand some other lab might be involved in doing head loss tests? How do you ship this stuff to them in a state which doesn't get all changed by the railroad, or whatever when you ship it?

MR. LETELLIER: I think Rob is going to address the institutional collaboration a little bit later, but we're working on the interface, and the best way to accomplish that. LANL, UNM, is supporting the characterization and the baseline examination of these products. We're trying to work with Argonne National Lab to decide whether we can generate a legitimate surrogate, whether they have to be grown or incubated in place, whether it needs to be a truly integrated loop where you grow it and test it.

MR. TREGONING: I think a point to remember though, as soon as you remove these products from the loop, or from the test chamber, you destroy them potentially. Or you potentially alter their characteristics.

MR. KLASKY: Solubilities change.

MR. TREGONING: So it's not a transportability question. It's a removal question.

CHAIRMAN WALLIS: It almost has to be an integrated system test where you generate the stuff and filter it all at the same place.

MR. TREGONING: These are questions that we're actively engaged in investigating at this point. And that's certainly a prime consideration and concern.

MEMBER SIEBER: It would seem to me that you would want to generate the material right in the test bed where you're going to flow test it. So there was no disturbance.

MR. LETELLIER: We share your opinion, and I think ultimately we will have complementary apparatus that can test various attributes, if not independent verification. We can pursue issues of particular interest. There's more than enough questions to go around.

CHAIRMAN WALLIS: It would be very interesting to see if this other lab can duplicate your experiments.

VICE CHAIR RANSOM: As a representative of the other lab, just let me get in a few points. You know, one of the things we'd like to be able to do is to control the amount of chemical product. You know, the problem with the current setup is this stuff comes out, it comes out everywhere. If I put in a screen, I really want to know how much chemical product gets to it. I also think they're independent quantities. That is, if I put in 50 pounds of cal-sil on a 6-inch diameter screen, I'm not going to have to look for chemical effects, you know. I'm going to have a bed so deep that my head loss is gone. So it seems to me, you know. We're trying to simulate something that's really very complex. There's the transport of the cal-sil to form the physical bed on the system. There's the development of the chemical products. There's the relative transport of the chemical products and the cal-sil to the screen. And the intent of the Argonne test is to try to control those independently. And that's very difficult to do in an integrated test.

So, now, again, and I'm not sure whether you're going to get to talking about our test today, but we have the problem with the chemical simulant of simulating the chemistry. But I think at least in the Test 3 we can simulate that chemistry, we can control the amounts of chemical products, so we know what the chemical product for unit area in the screen is. It's the kind of quantity that a designer is going to need to know. Now, whether our chemical simulated product is identical to the integrated test product is a question that we have to address.

And the other difficult one, and I think I'm sort of with Bruce, is that, you know, the thing that I think is most hard to simulate is this growth in the bed. But we think we can do that essentially as a separate thing. That is, you age fiber beds as a separate sort of thing. And again, there are many things. In the integrated test, it's very difficult to control the velocity through that bed. It still seems to me a very interesting question of what does that bed look like if I have a 0.01 feet per second flow through it, or you know, a 0.005 feet per second flow through it.

MEMBER KRESS: Couldn't you do that in a side loop? You don't have to -- a parallel line.

VICE CHAIR RANSOM: As Bruce said, there's lots of things to look at here. It's a complex thing. But it's not as simple as building a side loop on the side of his integrated test facility because again, we've got to be able to control both the fiber loading on the screen and the amount of chemical product that's deposited. And those all have to be connected back through an analysis.

CHAIRMAN WALLIS: And then there's the whole loop through the reactor. I mean, the cal-sil deposits on the pipe. Are you going to put in a typical dummy fuel element thing with spaces, and all that stuff in there, and see how cal-sil deposits?

MR. LETELLIER: We're not looking at rate simulations at different temperatures at this point.

CHAIRMAN WALLIS: You know, that's going to be a question too, what happens in the downstream effects.

MR. LETELLIER: And I think what you'll see if we get to it, I mean we realize all these questions are certainly pertinent questions, and how we're attacking it is doing a mix of short-term simulation bench scale testing as well as longer term testing similar to the ICET.

CHAIRMAN WALLIS: The screen designer is

going to try to find a way to design the system which is independent of all this complexity as possible.

MR. LETELLIER: I think we're going to talk later in some of the other talks about what some of the ramifications are.

CHAIRMAN WALLIS: It's going to be an endless task.

MR. KLASKY: I think there's one other point, is that you can perhaps mitigate the chemical effects by adding chelating agents and whatnot.

MEMBER SIEBER: There you go.

MR. LETELLIER: If we can understand what's driving this behavior, the competition between introduction of aluminum, for example, and passivation principle findings, principle explanations may lead to some kind of mitigated system. We already identified some simple things, like removing the constituents, move your scaffolding. Those sort of simplistic arguments may not completely take care of the problem for all plants, but on the other hand it may be an important action taken early.

I think we could finish this up in 10 minutes or so if we push ahead. Page 42 I'm just going to skip over. It's a composition analysis of the bird cage deposits. We do have baselines on the cal-sil, so we will be able to compare those -- or make those comparisons in composition.

Moving to Test 4, overview, on Page 43. I remind you that now we're returning to the sodium hydroxide system, with the cal-sil loading. Surprisingly little deposits on the coupon racks or the insulation, and most of the cal-sil had settled by Day 1. The tank clarity and color remained constant except for that early initial turbidity, and surprisingly little corrosion products are apparent on the submerged coupons. There's no obvious evidence of chemical byproducts in the tank, and no precipitants in the water samples. In fact, there was very little corrosion apparent on any of the submerged specimens, in contrast to Test 1, which did not have cal-sil present. In fact, visible evidence, there may have been more corrosion on the unsubmerged, the suspended racks, than there was on the submerged specimens, especially with regard to the aluminum and the zinc which had previously been the most reactive metals. And again, this may be some evidence that the presence of cal-sil is an important mitigation in surface corrosion.

Again, in all tests there's some apparent chemical byproducts in the insulation samples, but not as prevalent as in Test 1.

CHAIRMAN WALLIS: So you've only had one day of this?

MR. LETELLIER: Well, these are the overall observations. Having a technical difficulty, if you could give me just a moment.

CHAIRMAN WALLIS: So, Test 4 has the darkest hue I see.

MR. LETELLIER: I have to find my place.

MR. TREGONING: I think that Day 1 observation was just put there to contrast.

CHAIRMAN WALLIS: Maybe you could flip through this pretty quickly and just tell us the main things that you noted about Test 4, the most important things you noticed.

MR. LETELLIER: Yes. Page 44 is a much more visible arrangement of the sample bags. You can see the cal-sil blocks. There's much less of a deposit or surface coating present on the submerged racks.

Comparison of Test 3 and Test 4 water samples on the next page show that no precipitants were formed in either of these tests, nor have they appeared as the samples have aged following the test. They've been on the shelf now for quite a long time.

For aluminum in Test 3 and 4 there was a minimal measurable aluminum concentrations. In fact, Test 4 had no visible corrosion of the metal samples, and it was only measurable during the first 24 hours, so it's not presented here. Even Test 3, the figure that's shown is near the limit of detection for the system that we're using.

Page 47 is a comparison of the calcium present in solution.

MEMBER SHACK: Bruce, just a question. In Test 4, did you have the low pH thing initially, or did you add the sodium hydroxide from the first? I mean, was there a time when you had a low pH at all in this test?

MR. LETELLIER: No. It was initiated in exactly the same manner as Test 1.

MEMBER SHACK: 1, okay.

MR. LETELLIER: Where the sodium hydroxide was introduced through the spray system. The cal-sil --

MEMBER SHACK: So there was a low pH then initially, no?

DR. HOWE: Most of the sodium hydroxide went into the tank initially, and then --

MEMBER SHACK: A little bit more came in

through this?

DR. HOWE: Right.

MR. LETELLIER: Did either Marc or Kerry, did you want to comment on the calcium comparison?

MR. KLASKY: Well, I want to first go to the silicon concentration. You see an increase in the silicon concentration in 4 relative to 3. It should be noted that the pHs are different in this system, and there is, again, a strong effect of solubility with pH. Again, it can drive further dissolution, or increase the rate of dissolution.

MR. LETELLIER: I don't want to dwell on these comparisons because it's getting late, and we don't have firm conclusions. Some of this data is only a few weeks old. Test 4 was only recently completed. We're trying to fit the pieces into place to have a self-consistent explanation of these trends. And I believe we're making progress.

CHAIRMAN WALLIS: You seem to get a white substance of some sort every time.

MR. LETELLIER: With the exception of Test 2.

CHAIRMAN WALLIS: There's no white substance at all in Test 2?

MR. LETELLIER: No. If we skip ahead even to Page 50, that shows an increased time resolution of turbidity. We have talked about how cloudy the water is when we introduce latent debris, and especially when we introduce cal-sil. But quickly thereafter the spray phase has ended, the clarity improves rather rapidly. It agglomerates and settles very quickly. Thereafter, the water is largely transparent. It sometimes has an amber color.

Page 51. This is either the red spot on Jupiter or the face of Mars. But in actuality it's the variation in the color observed for calcium silicate. You recall that some of this material was heated to simulate the in-service life of the insulation. And while it's initially light yellow in color, and talc powder in consistency, a change in iron oxidation state turns it pink, a beautiful rose color. And so a portion of the solid product that we introduced was heated, a portion was not. And you see this color variation throughout the test.

Test 4, there was no unique chemical byproduct that was apparently deposited on the top of the surface. In Test 3, that's when we saw this white deposit being formed in relatively large quantities.

CHAIRMAN WALLIS: That's shown on 53. 53, there seems to be quite a low of white stuff on the rack.

MR. LETELLIER: Yes, for Test 3.

CHAIRMAN WALLIS: And that's a rack which is in the liquid or above it?

MR. LETELLIER: These are both the submerged sample racks.

CHAIRMAN WALLIS: Why is it up above the other rack, then, or is that an illusion?

MR. LETELLIER: These have been removed.

CHAIRMAN WALLIS: They've been removed.

MR. LETELLIER: This is post-test, after they've been dried.

CHAIRMAN WALLIS: Well there is some sort of white stuff in Test 4.

VICE CHAIR RANSOM: On the concrete specimen.

MR. LETELLIER: There's always a residue.

CHAIRMAN WALLIS: Okay.

MR. LETELLIER: These final figures as I said are qualitative in nature. Hopefully they've illustrated to you the detail with which we conducted the test. We're trying to examine the samples, and to interpret the results.

CHAIRMAN WALLIS: I think we might move on to Paul Klein. I'm sorry, and I'm just wondering if he can really spend an hour interpreting the usefulness of this, since it seems to be so mysterious. But he'll try. And maybe you don't need a whole hour. Thank you very much, that's very interesting.

Since you have four tests that each show different things, is there enough tests? Okay.

MR. KLEIN: Good afternoon. I'd like to follow up on the last presentation to try and provide some of the staff's interpretations of some of the key observations that were made during the initial four ICET tests. If we go to Page 2.

This presentation is really going to be divided into three areas, a very brief discussion of chemical effects history since it's been covered already earlier today. And then I'll try to provide some of our interpretation about some of the key observations from the test in the middle part. And then finally we'll have some discussion about NRC plans for moving forward on this issue. Next slide.

By way of history, I don't think I'll spend a whole lot of time here. ACRS obviously raised the concern about gelatinous materials. That led to the initial scoping study at LANL. It was conducted in 2003. And those tests showed that if gelatinous material formed, it will produce additional head loss. And those tests of course were not intended to simulate plant sump pool environments. That led to the genesis of the ICET program that we're currently in. And Tests 1 through 4 have been completed as discussed earlier. Test 5 will initiate next week we believe. And in general, these tests were designed to be screening tests that addressed the issue on whether you could see chemical effects in representative sump pool environments. And indeed we found that chemical products can form in these representative type environments. As a result of that, there is additional work that's going to be pursued by both NRC and industry, and Research and industry will have presentations that follow mine that provide more details. Next slide.

Slide 4, the intent here is to show schematically some of the factors that have to be considered in an evaluation of chemical effects. And keep in mind that chemical effects are just one piece of this overall GSI-191 evaluation. And we know that for some of these byproducts we have observed, there is the potential for head loss. If you look at some of the items that are listed here, again we won't spend a whole lot of time on them, but obviously the design of the screen's important, the particulars of the sump pool environment, the species present, the pH, the temperature, what goes into the debris mixture that arrives to the screen and exists within the pool. And that includes insulation, metals, paints, latent debris. There's obviously time dependency effects, since head loss consequences change as a function of time after a LOCA, as well as the properties of what might be formed, how it transports, whether it's particulate or amorphous material. And then another piece of the overall evaluation is what mitigative features there might be used to try and mitigate chemical effects.

The next few slides here, we cover some of the implications from Tests 1 through 4. I think the overall conclusion is that chemical products can form in representative sump environments. And so this issue does have the potential to influence sump head loss. We've noticed through just running four tests that variations within the test matrix, such as a change in insulation material, or a change in buffering agent, has produced significantly different chemical effects. And the real question that leads to is that while the ICET test series is representative of plants, there's no particular plant that matches an

ICET environment. So the question is what you may get under plant-specific conditions, with their own plant-specific mixture of environment.

The next bullet discusses some of the differences in composition that we've seen within the products that are formed. And so it would be important for plants to understand which materials participated in product formation for their given environment. And we saw evidence in some of the earlier ICET tests aluminum was very important. Some of the follow-on ICET tests it looked like aluminum was below detectability levels.

CHAIRMAN WALLIS: When you get a messy problem like this where it depends on quite a few things, and every plant is different, the solution doesn't appear very soon. I'm just wondering how long is it going to take to get something useful out of this? Just my intuition is that it's not going to be tomorrow, and it's not going to be several tomorrows.

MR. KLEIN: I think one of the greatest challenges we face is trying to gather sufficient information to drive the issue to resolution in the near-term.

CHAIRMAN WALLIS: Say you do another four tests and they each reveal something new, and you don't quite know what to make of that. Well, this is --.

MR. KLEIN: And I think we'll discuss in a few slides here --

CHAIRMAN WALLIS: Isn't some schedule where someone says thou shalt resolve this matter by next year or something?

MR. KLEIN: There is an existing regulatory schedule for GSI-191 resolution. And we will discuss in a few slides some of the implications in the near-term with an upcoming response due to the generic letter.

The final bullet on this slide discusses that we've observed products formed at different times. Obviously in ICET Test 3 we saw a chemical product very soon, within the first half hour of TSP addition. In some of the other tests we saw it would appear to be a time dependent buildup of product within insulation samples over the 30 days.

CHAIRMAN WALLIS: I would think that the strategy might be to make the worst product you can, and then show hopefully that the flow just breaks it all off, and it doesn't have any effect on the filter.

MR. TREGONING: That's one potential strategy.

CHAIRMAN WALLIS: Hoping that you could demolish the stuff. These little webs, you know, have no effect on head loss. Well, we don't know yet.

MR. KLEIN: Continuing --

CHAIRMAN WALLIS: What happens when you start flowing stuff through it? I mean, anything can happen presumably. String it out, bond it, tangle it up, whatever.

MR. KLEIN: Well, I think those are important answers that will be addressed, or important questions that will be addressed in upcoming tests.

On the top of Slide 6 here, we've obviously seen formation of amorphous chemical products. And the type of materials that can't necessarily be existing head loss correlation you could apply it to. So it points to the need, as you just discussed, of trying to determine head loss consequences of formation of some of these products. And in addition to the chemical effects, there are some downstream implications with some of the results that have been observed, in particular two bullets listed here. In Test 1 we saw temperature dependence on the formation of the amorphous material. And that certainly needs to be considered for head loss, and also for potential effects on downstream components.

CHAIRMAN WALLIS: These downstream components are which?

MR. KLEIN: It could be anything in the ACCS system. So it could be pumps, valves, reactor --

VICE CHAIR RANSOM: Spray systems I guess, too. Nozzles.

CHAIRMAN WALLIS: There's an RHR socket or something, isn't there? Or isn't something else involved? Or am I confused? There's something cooling this as well.

MR. KLEIN: Yes, there's heat exchangers involved.

CHAIRMAN WALLIS: So does it affect on heat exchangers? It might be more severe than the effect on the reactor. We don't know, but it conceivably could.

MR. KLEIN: Another observation from Test 3 that may have downstream implications is the effect on the flow meter that was observed due to deposits. So the past few slides, we tried to provide a sense of where we currently are based on some of the observations in the ICET. I think the next few slides we're trying to discuss some of the areas that we feel we need to go to gather more information in order to address this issue. And also try to describe what we think are some of the responsibilities that NRC has, as well as industry.

So we look at an overview from the chemical effects evaluations. The ICET results have been very informative. When we initiated this series eight months ago, we were trying to determine if chemical effects were a real issue, if we would see any chemical products at all. And there's a variety of opinions. I think over the course of the first four test we have some answers. However, the ICET tests were not really designed to go beyond that level. And so as a follow-on to those tests, it's obvious that additional testing is needed to understand head loss consequences.

CHAIRMAN WALLIS: How much additional testing is needed, do you think?

MR. KLEIN: Well, we've seen observations of chemical products form. We don't really know what they mean as far as effects on head loss.

CHAIRMAN WALLIS: So first of all, exploratory tests, and then you'll learn what tests you'll really need to do. And then you'll do those presumably.

MR. KLEIN: Well, I think there's a number of questions. One is, you know, the things that were formed within ICET, what effect do they have on potential head loss. And then there's also questions about what might happen if you had a plant unique chemistry that's outside the ICET series, and what factors need to be varied and studied to understand the sensitivity of that.

CHAIRMAN WALLIS: Maybe that gets to bottom bullet. You've done four tests. It's taken a year and a half. And you're going to look at some other variables and do more tests? How many tests do you think you need to do of the ICET type?

MR. KLEIN: Well, I will get to that point in a minute. I think the ICET tests are just one piece of the information that we're trying to gather in order to develop the technical basis. And I think we do understand that although the ICET tests weren't designed to be representative, there are 69 different potential chemistries, and a combination of materials that exist in industry that need to be evaluated.

CHAIRMAN WALLIS: Well, the brute force way is to build duplicates of the ICET tests facilities. Just run them all night.

MR. TREGONING: Yes, I think we need to look at ICET within a suite of work that needs to be done. ICET is incredibly informative and a valuable piece of research. However, you know, there are other questions that might be examined more cleanly in a more segregated type environment. So I think we need a mixture, and I'm going to overstep my bounds here.

CHAIRMAN WALLIS: I'm intrigued here. I mean, I'm involved right now in some industrial development of something we didn't know much about, and it's taken an awful long time. And this looks like that. So you should hope for a breakthrough which will simplify everything.

MR. TREGONING: Well, again, it's not hope or a breakthrough.

CHAIRMAN WALLIS: It would complicate this thing ad nauseum if you're still looking at all the effects.

MR. TREGONING: Well, there needs to be a basic level of understanding, but there are other ways to mitigate as well, I think. So let's, you know. I think we need to approach this, and at least our philosophy is we need to approach this problem from every different avenue that we can. There's not one path that I would say that we should be going down. We should be going down all of these paths at the same time.

VICE CHAIR RANSOM: There seems to be one general path, though, get rid of cal-sil, fiberglass, and aluminum.

(Laughter)

MR. TREGONING: Yes, those would be things that would address many problems.

VICE CHAIR RANSOM: Aren't there some plants that are in pretty good shape from that point of view?

MR. TREGONING: I don't want to speak, but there are some plants that are predominantly RMI type of plants, yes.

CHAIRMAN WALLIS: With no ladders, and scaffolding.

MR. TREGONING: If you had the luxury to remove cal-sil and fiberglass and aluminum, you would solve a myriad of problems with respect to sump blockage, if you could do that.

MR. KLEIN: If we could have the next slide. I think it's important to remember that ultimately it's the licensee's responsibility to evaluate and account for chemical effects. And I think that it's very important that industry work aggressively to try and develop a technical basis to address chemical effects. And I think we're going to hear in a short while about some of their plans to address this issue. But the staff does not plan to issue design guidance to address chemical effects.

CHAIRMAN WALLIS: Ever?

MR. KLEIN: No, no, I don't think that it's really appropriate. As I mentioned before, there are 69 different situations that exist out there.

CHAIRMAN WALLIS: But you might endorse an industry guidance.

MR. KLEIN: We may endorse an industry guidance. I think -- it's their responsibility to develop it, and our responsibility to evaluate their evaluation of chemical effects. And they understand that they're required to provide justification for chemical effects evaluation.

MEMBER SHACK: Let me see if I understand what you did say. You talked about design guidance, and it's not your role to provide the design guidance. You provide evaluation guidance, or you will develop a means to independently evaluate.

MR. KLEIN: Yes.

MEMBER SHACK: Design guidance clearly isn't within your domain, is that what you're saying?

MR. KLEIN: Yes. If we can have Slide 9. At the same time, we do recognize the complexity of this issue.

CHAIRMAN WALLIS: Well, let's see. You might endorse tools. Or you might develop tools. If you develop a correlation for head loss with spider web type stuff, then that would become something which could be used by industry. So you might be developing tools. But not the entire design guidance. Is that correct? Or would you just not even try to develop tools?

MR. KLEIN: I think there's a number of things that we will be trying, and I'll get to that in one slide after this.

CHAIRMAN WALLIS: But you have developed tools before. You developed correlations for better head loss and stuff like that.

MR. KLEIN: Yes.

CHAIRMAN WALLIS: You might well develop correlations for better head loss with chemical effects.

MEMBER DENNING: But if they do that, don't they lose their independent regulatory --

CHAIRMAN WALLIS: Well, they've already had NUREGs on this topic.

MEMBER DENNING: Well, I think they really accepted industry's. I may be off. I'm curious of what you're saying, because the question really affects where do some of these experiments go as far as head loss calculation methods and stuff like that. Is the NRC going to develop its own? Well, one way is to develop a joint capability, and that sounds a little bit questionable to me. Another is industry goes off and does its own, and NRC has an independent evaluation capability. I guess you can address that an hour later.

MR. KLEIN: Well, I think when we set the ICET series up, as Rob mentioned previously, it was somewhat unusual to be partnering with industry, and generating the data. Typically the research products that they work on are provided to staff, and are used as a confirmatory tool to do independent evaluation of what industry may submit to address the topic.

MR. TREGONING: But there is certainly -- there's a number of examples in the past where we've had complex problems such as this, where there's been a giving up among the research community, industry's going to tackle this one, NRC's going to try to provide some information there. So there have been some areas where that type of work has been done. And this program to date has followed more along that model.

CHAIRMAN WALLIS: It depends how far back you go. You go back to LOFT program, I think that was a government program. It was not an industry program.

MR. TREGONING: Well, yes, that's true, it depends on how far back you go, and which specific project you're talking about.

CHAIRMAN WALLIS: How much money you have, and things like that.

MR. TREGONING: But I think there's certainly an interest here. We all recognize the complexity. There's obviously an interest to make sure that both the NRC and the industry are using resources not only most efficiently, but that we're also generating information that can be used to solve the problem.

CHAIRMAN WALLIS: I'd be a bit nervous about this bullet 1 here, where industry I think initially said there aren't any chemical effects. Didn't they get perhaps a surprise from these ICET tests? And they may get other surprises. So if I were industry, I'd be a little perplexed about what kind of assumptions I ought to make about chemical effects.

MR. TREGONING: I can't answer for industry on that, unfortunately.

MR. KLEIN: I think the point of the first bullet here is that there's a GL response due in September. And the staff recognizes that industry won't have developed the technical basis to address this issue fully. So that, some cases they may identify margins, or other steps they're taking to address chemical effects, and then subsequently verify that with testing, and update their submittal in the future. I think some of the things that they clearly do have at this point, they should be able to identify their plant-specific materials, their projected sump pool conditions.

CHAIRMAN WALLIS: This could last a long time. Are they asked to solve GSI-191? Or are they asked to figure out how to calculate chemical effects? If it's the latter, they could say, well, it would take us 10 years before we could do that, so we can't solve GSI-191. Or I thought you were asking them to go and solve GSI-191, despite all the uncertainties.

MR. TREGONING: At the risk of speaking for NRR, they're asked to respond to the generic letter, not specifically to solve GSI-191, but to respond to the evaluation.

CHAIRMAN WALLIS: So there's still some flexibility left in strategy.

MR. KLEIN: I think one of the

expectations we do have for the response is that they're going to describe their overall evaluation strategy, identify assumptions that are being made in the near-term, and provide detailed plans, and a schedule to confirm the adequacy of their assumptions. And the final part of this slide is not intended to be an inclusive list, but it just shows there are a number of options available for industry to address chemical effects. And there will probably be a number there taken, including a combination of options, when applicable.

CHAIRMAN WALLIS: And you probably also need to do confirmatory research so that you have some technical base for evaluation.

MR. KLEIN: If we go to Slide 10, it's not our intent to walk away from the chemical effects issue, and put it all in industry's lap. Really, our role is to try to rely on the information from confirmatory Research work that's sufficient for us to perform an independent evaluation of the chemical effects evaluations that are performed by industry. And we obviously will be continuing to communicate with industry to try and make any testing complementary, take advantage of that as much as possible. But as far as general areas that additional testing is planned right now, and I won't go into these in detail because I think Rob will elaborate in a later talk. But we're going to take advantage of the fact that we have an ICET test loop built. And once we complete Test 5 there may be some additional tests that are of interest to be conducted at that loop, whether that might include some type of head loss measurement, or perhaps not doing an isothermal type test. There's a number of things that have been discussed.

CHAIRMAN WALLIS: Can I ask you about paint coatings? We were sort of assured earlier that paint coatings wouldn't leech, and they were not going to be affected by these chemicals. But if you have a mixture of paint coatings, and whatever this stuff is, and then there could be some synergy between the paint coatings and the fiberglass, and the web-like structure, and all those things. I don't know what it would be, but it could well be some synergistic effect of the whole thing together, and the cal-sil or whatever.

MR. KLEIN: I think within the ICET series, we have only tested the inorganic zinc. We do have some data provided by industry on the qualified epoxy-type coatings. We think it's important to try and perform some testing on some unqualified coatings to see what contribution they may have in the chemical effects area. That might be the type of thing included under the third line.

CHAIRMAN WALLIS: The painting on electrical equipment, for instance.

MR. KLEIN: So in addition to the ongoing work at LANL, we have head loss testing planned at ANL, bench-top testing at a number of locations to try and do two things really. One is to inform the other programs, and then also to investigate items of interest that might be outside what could be conducted in those --

CHAIRMAN WALLIS: I highly encourage you to do that, to try a lot of things bench-top, so you could perhaps eliminate some of the variables.

MR. KLEIN: Yes.

CHAIRMAN WALLIS: Perhaps box in what you need to study in the more elaborate experiments.

MR. KLEIN: And then the final line item is intended -- it's really an attempt to see if there's a model or some way that we might be able to look outside what we can test. And if we're able to develop information from that it would be a very useful tool, although the probability of success with that work might be lower than some of these other programs.

CHAIRMAN WALLIS: So you're not going to take any of this and develop any sort of engineering model from it?

MR. TREGONING: I would unequivocally say that that's not going to be the case.

CHAIRMAN WALLIS: Well, I don't see a task here which says take all this and develop an engineering model.

VICE CHAIR RANSOM: What is the SWRI?

MR. TREGONING: Southwest Research Institute.

MEMBER KRESS: Is that chemical equilibrium code you're developing, or they already have one?

MR. TREGONING: Their first task is they've looked at a number of commercially available codes and tried to screen out for these environments which ones would be most appropriate to use.

MEMBER KRESS: These are condensed state solubility type? I mean, they can go to that detail? Those are generally difficult chemical analyses to make.

MR. TREGONING: Well, the big limitation

of course is they don't consider kinetic effects.

MEMBER KRESS: These are equilibrium codes.

MR. TREGONING: That's the biggest challenge of these codes, is understanding the role of kinetics and how they may affect your --

MEMBER KRESS: That would certainly be useful, though, to get the equilibrium.

MR. TREGONING: They were -- similar types of codes were used to help justify some of the ICET plan. So they had already been useful in that regard. Whether we can extend their usefulness still remains to be seen at this point.

MR. KLEIN: If I can have the next slide, Rob. This slide presents a sample evaluation flow chart. And it's not really intended to be something that directs industry effort, but it's more an attempt to show one approach to trying to evaluate this complex chemical effects issue. And I'll just briefly touch on some of these items here.

If you start at Item 1, one of the first --

CHAIRMAN WALLIS: Hang on one second. Tombstone.

(Laughter)

MR. KLEIN: That's the glass is half empty already in this case. One of the first steps is trying to identify, I think, a plant-specific environment, and then once you understand the conditions in your plant, try to compare that to the type of tests that have already been conducted within the ICET series. And you would essentially try to determine if the ICET tests bound your particular plant. And in most cases, the answer may be no. I mean, it may have a condition that's quite different due to insulation materials, or other combination materials that weren't represented within ICET. In some cases, their particular plant environment may be quite similar to one of the ICET tests. If they are not bound, they would need to do some type of evaluation of their plant-specific conditions.

MEMBER KRESS: Would this test loop -- I guess it's at New Mexico. Would it be available to the industry if they wanted to learn -- if they had conditions that were considerably different than the ICET and they wanted to come in and run a specific test for their conditions?

MR. TREGONING: We have to be careful about that, because if we were evaluating plant-specific conditions, that would put us in a bit of a quandary. So we would have to, you know, that was something that we would try to not encourage at a minimum. And the other thing, you know, just sheer length of time for using that loop to evaluate one set of conditions. You can see it can drag on pretty interminably, potentially. So I don't see that as a very attractive solution.

MEMBER KRESS: They would have to have the same extensive quality assurance that you guys have, which may be a little tough for them I imagine, unless they hired Los Alamos and New Mexico to do it. That would be a conflict of interest.

MR. TREGONING: Potentially, yes.

CHAIRMAN WALLIS: But they could copy your test facility very quickly, I think. Doesn't look all that complicated. If they wanted to build something very similar.

MR. KLEIN: It's not a complicated test setup.

CHAIRMAN WALLIS: Maybe they've done it already, I don't know.

MR. TREGONING: There's been a lot of ground work that's been laid, a lot of good design work and procedure development that's been done by LANL in concert with the NRC and the industry. So there's a lot of prescriptive attributes to this thing that are in place potentially, if people were interested in copying it.

MEMBER KRESS: I thought maybe a memorandum of understanding with EPRI might allow you to allow them to do this.

MR. TREGONING: No. It's not going to that level.

CHAIRMAN WALLIS: Well, if it costs, say, a million dollars per plant to solve the problem, that's a hundred million bucks cost for the industry. They ought to be able to do some research. To save money.

MR. TREGONING: That's usually why, you know.

CHAIRMAN WALLIS: Yes.

MR. KLEIN: I'm going to continue on here. Within the Block 4 - 7 area, I think we're trying to show schematically is some type of time-dependent analysis where you're looking at the chemical effects over your mission time. And at various points in time, you're trying to determine if you have chemical effects that are observed. And if they are, then you need to understand the head loss consequences associated with them.

CHAIRMAN WALLIS: About which we know nothing yet, right?

MR. KLEIN: Of which we know nothing at this point. And the goal, obviously, is to assure yourselves that during the mission time you're -- any head loss from chemical effects is less than the available margin. Using that criteria, you would go through your mission time for your system. And in many cases I think we'll find that very early on is when the point of least margin is, and it may be when plants can account for the least chemical effects. But over time, if you're able to demonstrate that, you know, any type of head loss generated from chemical effects is less you're your available margin, that's a sufficient criteria to show that you've addressed chemical effects, which would take you to Block 10.

If you weren't able to meet that criteria, that your head loss from chemical effects was greater than available margin, then that would tend to drive you into some type of modification that would require assessment following the modification. The idea would be to get through to Block 10, and then from a chemical effects you've demonstrated that there's sufficient margin, and that the question that would maybe drop out of the process, are there any downstream effects that were observed, or potential downstream effects that need to be accounted for.

MR. TREGONING: I guess the only point I'd like to make, I think Paul made this, but I'll stress it a little bit as well. One of the things that is somewhat in our favor is with respect to ICET at least, the observations are for all of these tests that the chemical effects, their magnitude, or their amount of chemical effects tended to increase the potential over time. By Day 30 quite often there was more product, there was more evidence of product in insulation, those sorts of things. We know from many of the licensee designs that their minimum MPSH margin is really right at the point where they switch to recirculation, which is very early, you know, around Day 1, or sooner than Day 1 in the cycle. So that's one thing, at least, with respect to chemical effects that it looks like we may have in our favor. Now, we're obviously concerned about any I'll say early initiating chemical effects of the type that we saw in Test 3 where we saw some of this white flocculent material very early on in the test. But that's an important point to remember, that there's time sensitivity not just to the chemical effects themselves, but also to how MPSH margin increases normally as a function of time.

CHAIRMAN WALLIS: I don't quite know how you interpolate here. You've got Test 1 the way you had, all fiberglass. And then you have Test 4 and the difference there is 80 percent cal-sil. And you got very different results. Now suppose you had 5 percent cal-sil. You have no idea whether you're like Test 1 or Test 4. So you can't sort of interpolate yet. You need more information.

MR. KLEIN: Yes. And I think that's where --

CHAIRMAN WALLIS: I'd expect a little bit of cal-sil, since it's in a big pile at the bottom now, is as good as 80 percent. But I don't know. So maybe a 5 percent cal-sil test might tell you something. We can start thinking about that. Very soon start generating more tests. Well, I don't know yes. Very soon start generating a lot of tests.

MR. TREGONING: ICET is, again, because of the long duration, you want to try to maximize the value of any long-term test you do.

CHAIRMAN WALLIS: Build another one.

MR. TREGONING: What?

CHAIRMAN WALLIS: Build another one.

MR. TREGONING: We could build 69 of

these, potentially.

CHAIRMAN WALLIS: Well, that's too many.

MR. TREGONING: Okay.

CHAIRMAN WALLIS: But if time is a problem, if time is valuable. Well, I'm not going to design your program. I can see all kinds of things that you might do that would be helpful, that potentially could be helpful.

MR. KLEIN: I think that overall, though, there's probably, you know, two main points in this time-dependent analysis. One is what is your least MPSH margin when you consider chemical effects. And in most cases, it's going to be very early on. And then the second question is what's the maximum chemical effect head loss you can generate, and do you have sufficient margin at that point in time to address it.

Let's go to the last slide here. In summary, we feel that a plant-specific approach to chemical effects is consistent with what we've observed in the ICET test series. It's been very --

CHAIRMAN WALLIS: Well, generally, a plant-specific approach to GSI-191 is called for anyway.

MR. KLEIN: And the chemical effects part is no different. We think that additional testing will support a realistic chemical effects evaluation, and we need to address the head loss uncertainties for the type of products that were generated in ICET, and we also need to address the uncertainties that exist within all these different plant-specific environments. And by reducing the amount of uncertainty with testing, it'll reduce the amount of margin that will be needed to account for chemical effects. As more data is generated, and a better understanding is developed for chemical effects.

CHAIRMAN WALLIS: At the moment, we know nothing about the effect of chemical effects on MPSH.

MR. KLEIN: Well, I don't know if I'd say nothing.

MR. TREGONING: I wouldn't say nothing.

CHAIRMAN WALLIS: We haven't got that far yet.

MR. TREGONING: We did small-scale testing. Now again, that wasn't representative.

CHAIRMAN WALLIS: We know that suitable goop can block a screen, but that --

MR. TREGONING: And there has been some testing done internationally that's shown that head loss can have a -- or that chemical byproducts can under certain conditions have a relatively --

CHAIRMAN WALLIS: So there is another base of information, international?

MR. TREGONING: There's been some testing, although we have had difficulties understanding the exact conditions of the test to try to make direct comparisons. We have seen, in some cases, fairly significant pressure drop.

MEMBER DENNING: What's the regulatory status of downstream effects in this? Is there anything related to that in GSI-191 letter -- or does it have any relationship to GSI-191, or is it's own separate issue? I mean, I realize that there it's combined, and the question really relates to also now we've seen some chemistry effects that could have downstream implications that we have to address.

MR. KLEIN: Downstream is considered as part of the GSI, and we have been communicating with the people that are responsible for the downstream effects evaluation to try and keep them informed with what's being developed in the ICET series.

MR. ARCHITZEL: This is Ralph Architzel from NRR. I'd like to clarify a little bit. Technically, the GSI is strictly related to sump screen performance. I mean, as far as the generic safety issue goes, and the path work and its past history. When we were going through the approval process on the generic safety issue, and the resolution path, and what we were doing with a generic letter in the bulletin, downstream effects were added, as were chemical, were added to the resolution strategy to do them on the same timeframe, because it didn't make sense to fix the screen and the suction problem, and with an interrelated problem associated with the downstream effects. So those activities were added to the resolution vehicles of the GSI, but technically it is a separate, if you will, piece that could have been its own GSI, but it's being coordinated in the resolution of GSI-191.

CHAIRMAN WALLIS: Are you through?

MR. TREGONING: Any questions for Paul?

CHAIRMAN WALLIS: No. We've gained a bit of time. And I think what we want to do next is take a break, and then I'd like to come back and hear from industry. And then Rob, you're going to finish up everything for us? In whatever time you've got. Now, if I take a break which doesn't end in an hour, people will sneak on the hour. But I really would like to get us back before 4:00 if we can do that. My watch says it's not yet 20 to 4:00. Can we get back at 5 minutes before? Or earlier than that? What's realistic? Five minutes to 4:00 is realistic? Probably. But don't hang on -- fifteen minutes? From now. We'll start with the presenter from EPRI will be here. And if no one else besides the committee we'll just go for it.

(Whereupon, the foregoing matter went off the record at 3:41 p.m. and went back on the record at 3:56 p.m.).

CHAIRMAN WALLIS: On the record. Are we ready to go again?

MR. GISCLON: Yes sir.

CHAIRMAN WALLIS: Okay. Let's go. Good afternoon, gentlemen.

CHAIRMAN WALLIS: We're officially back in session.

MR. GISCLON: I'm John Gisclon with EPRI (PH) and I've asked Gil Zigler to join me for this presentation. The presentation basically consists of two parts here. One part is aimed at some basic follow-up activities or mainly aimed at the passive strainers and there's another part near the end of the presentation that has to do with the segment of industry that is working on the active strainer part. The second slide please.

So far we've identified chemical effects or byproducts in the ICET test matrix that are involved with as listed here. For Run 1 with the sodium hydroxide buffer and fiberglass, we've seen particulate formation in the sediment. Precipitate formed as the temperature's reduced in the solution. Increase in viscosity that is associated perhaps with the precipitate material and material deposition within the fiberglass matrix. Run 2. Again, particulate material, chemical reaction, byproducts in the sediment. Again, material in the fiberglass. Run 3. Similar situation but with certainly a different buffer and different insulation. Run 4 as was previously explained. The only real byproduct material that you've seen is probably some deposition in the fiberglass. Next slide please.

As previously explained, the current program does not address impact on debris bed head loss. And the GSI-191 resolution, the industry matrix for doing that is being coordinated through NEI through the Sump Performance Task Force and the membership includes various owners' groups, vendors, utilities and EPRI.

To deal with the chemical effects resolution for the passive designs, there's been an industry group that has been assembled which includes five different participants which includes Alion, Enercon and Westinghouse and CCI, Framatome and PCI, General Electric for passive designs and the Atomic Energy of Canada. Alion has basically volunteered to take the lead to develop and provide the industry participants with the data necessary to address chemical effects for the full range of passive screen design.

CHAIRMAN WALLIS: So Alion is providing the data. It means they're doing experiments?

MR. GISCLON: They may do experiments.

CHAIRMAN WALLIS: I don't know how you get data otherwise.

MR. ZIGLER: That is correct, sir. We're intending to do serious experiment with IC Test byproducts that are available.

CHAIRMAN WALLIS: So you'll do tests building on the experience you've heard from Alion.

MR. ZIGLER: Exactly. Our idea now is essentially to conduct a highly controlled vertical loop testing with both temperature controls on it and in an attempt -- The first thing we are actually planning on doing is to understand what we call in some -- the bid morphology of it. We want to see what really happens when you actually put fiber beds in the liquids and the byproducts in it, what has actually formed and then go from there. Once we have an idea from that, then we know where our next path is going to be.

MR. GISCLON: That's perhaps a little bit ahead of the presentation but the basic approach for dealing with the chemical effects is to include a margin or bump-up factor as it were in the net positive suction head analysis or the debris bed head loss determination basically in support of the September mandate that we have for response specifically to be able to deal with the chemical effects. So you --

CHAIRMAN WALLIS: Are you going to be ready by September?

MR. GISCLON: We need to have something.

CHAIRMAN WALLIS: Something by September.

MR. GISCLON: By September.

CHAIRMAN WALLIS: But you won't have much it seems.

MR. GISCLON: We won't have much. We'll have basically some estimates of what a margin should be or a bump-up factor should be that addresses the previously discussed chemical effects and we intend to validate this margin or the bump-up factor through follow-up testing or analysis in accordance with the schedule.

CHAIRMAN WALLIS: I think bump-off factor would be good if you could bump this to be off the screen.

MR. GISCLON: That's --

CHAIRMAN WALLIS: If you bump-up, you can bump-off.

MR. ZIGLER: The bump-up factor is a term that was introduced back in the BWR days on it where very late in the BWR campaign, we found some additional material that we didn't quite know. So we reran the experiments, if you please, with a simple head loss measurements so we could take the previous numbers and multiply it by a k factor, if you please, a bump-up factor. So that's the reason for that terminology.

CHAIRMAN WALLIS: Okay.

MR. GISCLON: I apologize for not picking up on the bump-off factor.

CHAIRMAN WALLIS: That's okay.

MR. GISCLON: We were talking about a self-cleaning strainer and certainly there's a bump-off there. The assumption on margin was to basically apply this so-called bump-up factor or to add a specific quantity of margin for head loss or an NPSH margin specifically allocated for chemical effects. This margin is determined on some preliminary debris bed of adjustment factors as well as comparison of plant-specific parameters with those that are used in the ICE test run matrix.

MEMBER KRESS: What happens if you apply this margin, it's going to be to account for the chemical effects later on, and you come back and chemical effects far exceed this margin? What do you then? I mean that's just the chance you're going to take.

MR. GISCLON: What else can we do at this time?

MEMBER KRESS: Yeah.

MR. GISCLON: We were required to have something.

MEMBER KRESS: I agree. You don't have much between now and September.

MR. GISCLON: I believe that if toward the end of the year something else, more light is shown on it, there may be some time to deal with it, but not much.

MEMBER KRESS: I suspect if it did turn

out that it exceeded your bump-up margin, you would start looking for ways to mitigate it in some way, maybe chemically.

MR. GISCLON: One of the -- I'll ask Gil to respond to that because the selection of the vendors that are involved here will be the vendors that are going to be responsible for helping industry specify the hardware fixes that are necessary to grapple with this.

MR. ZIGLER: Well what has happened is that, basically the genesis of this, is that there are three vendor groups, the Enercon/Alion/Westinghouse/Fenscer (PH) group, the CCI group and the Framatome/PCI group. We call ourselves loosley as a NUREG 6224 user's group because we basically do all of our design pursuant to 6224 head loss correlation. We got together. As I said, why don't we merge our resources since we're using the fundamental correlation and after thinking some more, GE decided, "Hey, me too" and AECL who's also sort of a 6224 correlation user said, "Me too."

So we are basically the vendors of passive strainers and we're all right now with the industry. They are issuing request for proposals for strainers faster than their Xerox machine can issue. So we receive -- Right now, we're in the process of doing the design of something. I think the last count I had yesterday night was about 11 plants that we're trying to put responding to design.

So the answer is yes and basically what is going on is that essentially we are putting the largest passive strainers that are commensurate with the volume and attempt to go from there.

CHAIRMAN WALLIS: So it's not so much a bump-up factor. It's a squeeze-in factor.

MR. ZIGLER: You can use that terminology.

CHAIRMAN WALLIS: All the fits we install. Is that the rules?

MR. ZIGLER: We'll get there.

VICE CHAIR RANSOM: Are these designs based on the NEI guidance?

MR. ZIGLER: The designs that are coming in, essentially what we're seeing is there's basically two categories of design. One is a so-called Chapter 3 of the NEI guidance design. This is where a very conservative debris generation and transport and those are the numbers that come in with very large quantity of debris.

Then you have another category of plants who have done the baseline design and saw, "My goodness. We cannot survive with that and we just don't have the space." So they immediately went into the refined analysis. Those are the plants that are doing more sophisticated debris generation, debris transport using CFD, etc.

So there are a number of solutions going on that are being implemented of very fine design. So you have those two basic categories of plants, but both of them still have considerable analytical margins left in them and we still have very conservative assumptions on it. In other words, for example, whether it's the debris size distribution that affects how it transports, whether it's the issue of erosion or things of this nature. So we always are doing, all the vendors are doing, conservative upper bound estimates and hopefully we will, if the situation comes up that we miscalculated the bump-up factor, hopefully we can go back and redo some of those conservatism to regain margin again.

MR. GISCLON: As previously alluded to, the assumption validation here, the resolution approach includes both a plant-specific analysis and potential testing and the schedule for doing that is in accordance with an overall resolution plan which I'll get to later in the presentation.

Plant-specific evaluation, the first step is to sit down with the plant and compare plant-specific conditions with the ICET test parameters or the ICET matrices. The ICET test plan considered representative containment environments and materials and the plants are going to have to look at the different material types and quantities of metals, concrete, the pool conditions, the buffer, the pH, what the velocity is in the pool, what the insulation material is and what the mix of insulation is. At the end of the day, they will have to be able to identify facilities or details that may not be "representative" in accordance with the matrices that were already performed, or excuse me, the matrices that were developed for the ICET testing.

CHAIRMAN WALLIS: So it's barely a matrix. It's four tests.

MR. GISCLON: Five/six basically.

CHAIRMAN WALLIS: Four tests just about makes the minimum matrix, isn't it?

MR. GISCLON: Step two is to identify and recommend supplemental testing or analysis to address plant-specific representation or gaps in the data. That is to be done with consultation with existing reports such as the Southwest or CNWRA Thermodynamic

Modeling Report which was produced by the NRC.

MEMBER KRESS: But would that be benchmarked for the ICET test in some way to show that it gives reasonable results?

MR. TREGONING: We have plans to do that as part of our research activities and I'll get to this later, but there will be some line simulation testing and then there'll be some post test calibration to look at model sensitivities.

CHAIRMAN WALLIS: This goes to equilibrium concentrations of things, isn't it, this thermodynamic modeling? Do you have any idea now how far away or close you are to equilibrium in your facility?

MR. TREGONING: Mark may want to address that. It's certainly a function of the species that you're looking at for a particular one.

MR. KLASKY: We'll get to that after a few slides.

MR. GISCLON: We may embark on some benchtop or beaker testing, supplemental testing.

CHAIRMAN WALLIS: I'm surprised you haven't done some already. You probably have. It must have been very tempting.

MR. GISCLON: It has been very tempting but we --

MR. ZIGLER: We have done some benchtop testing with atoms like calsil for example where there is this anecdotal evidence that all calsil are like aspirin. You put it in warm water and it disintegrates. We've tested about three different types of calsil in benchtop testing with different kinds of water, with different kinds of chemistry, with the stirrer on it and left it over for three days and it basically keeps intact on it very similar to what we're seeing in the ICET testing for the calsil plants. That's a very valuable observation that the calsil in the ICET test under plant-representative conditions did not become aspirin.

CHAIRMAN WALLIS: Unless you whack it with a LOCA jet.

MR. ZIGLER: A LOCA jet first, that breaks up. We know what it does from the OPG test but the remaining big pieces were our main concern on it.

MR. TREGONING: Just have you evaluate, I'll say, the strength of the calsil after immersion compared to prior immersion. I think of this stuff hanging on a pipe or something that's submerged and then under its own -- potentially or you still worry about maybe --

MR. ZIGLER: Once we saw that the calsil

was not dissolving readily on it, we did what I think was a very conservative test. We put it in a beaker with the magnetic stirrer underneath it and let it sit there for days. Yes, it rounded off the ends, but we didn't see any more than about ten percent if that coming off. I think that's a good mechanical simulation of tumbling, if you please, water and stuff. We're getting off the main track here. Sorry about it.

MR. GISCLON: We're getting into details here.

VICE CHAIR RANSOM: The last slide you have says that GE did tests with simulated chemical effects using a recipe provided by EPRI.

MR. GISCLON: The recipe provided by EPRI was basically the recipe that was used in Test 1 of the ICET series and I will get to that when I get to that slide.

CHAIRMAN WALLIS: We have some comments from the staff.

MR. ARCHITZEL: Ralph Architzel. I would like to get back on, real quick, but I did want to mention that we did go to ACL Chalk River Labs and we did get to look at some of their calsil tumbling tests that they had done and in this case with turbulence over, it will disappear if you go for 30 days and keep tumbling.

But if you stopped it after 24 hours, basically they had numbers like 20 percent with those type numbers. So 80 percent is going to be intact. Only 20 percent would go away. So there is another data point that says it doesn't totally dissolve even if it's in a pool if it's not a turbulent moving condition.

MR. GISCLON: One of the facets of --

CHAIRMAN WALLIS: Sorry. Gil, this was water you put it in or you put it in NaOH or something else that might loosen up some of the bonds in the calsil.

MR. ZIGLER: We did with chlorinated water and NaOH on it. It was just one of those high school level science projects.

CHAIRMAN WALLIS: I think you can do a lot of things to help.

MR. ZIGLER: Right. So we got a lot of insight and eventually we're now in the process of doing a full Appendix B1.

MR. GISCLON: Continuing with step two there, one of the things that we wanted to consider is full application of diversified mitigation strategies and this could include something as simple as removal of problematic sources or materials, moving aluminum scaffolding up, change in the buffer chemical or pH although as has been previously explained, we need to be very careful with that and realize what's happening, installation of traps of interceptors that could trap or intercept debris on its way in the first place or any other innovative approaches that haven't even been thought of yet. But we don't want to be restricted to just conventional, more square feet. The next slide please.

Step three will be to identify and perform the required chemical effects material head loss testing. To do that, we've agreed with NRC and LANL to have some of the archival material from the first four ICET tests released to industry and the material, the different vendors are getting together to decide what to do with that material.

But it could include further material characterization, determination of properties and last but not least, possible testing of the material and some of the fluid in a situation where they could develop head loss data. For example, you could take some of the fluid from some of the tests that was collected, some of the fiberglass archival material, and set that up in a small head loss loop and actually look at it for awhile.

Step four will be the application of results which will be to develop and prescribe impacts to the plant for debris bed head loss, specifically the chemical effects and this goes back to the refinement of the bump-up factor for --

CHAIRMAN WALLIS: Excuse me. I would think you would want to develop what I call this engineering tool where you take all this scientific data and you turn it into correlations or equations or something for design purposes. Before you just go and guess some bump-up factor, you have some idea of how you're going predict things.

MR. GISCLON: It's implicit in that.

CHAIRMAN WALLIS: Right. That seems to me isn't a trivial task to take these scientific observations and see if they give you the information you need to develop a design tool. Maybe they don't yet.

MR. GISCLON: If they don't, we have more work to do.

CHAIRMAN WALLIS: I don't see how they do yet, but maybe with some head loss tests they will. There's no way I could look at those pictures and say what bump-up factor I need.

MR. GISCLON: True. We've indicated that in some instances it may be appropriate to account for changes in viscosity and as indicated before, Paul indicated that the time dependency of these effects needs to be taken into account. In applying this to the screen hardware, we've come to the preliminary conclusion that it may require development of new correlations other than the existing NUREG 6224.

CHAIRMAN WALLIS: This is what I was referring to.

MR. GISCLON: And that's certainly one of the steps. One of the other considerations is to look at the three time-dependent effects which may be going on simultaneously which include the NPSH as the available net positive suction head as the accident progresses and things change within the containment, the time dependency of head loss across a debris bed and the time-dependent nature of chemical effects both. As was previously explained, if there's an initial chemical effect or if there's a long-term effect, you need to account for all of those simultaneously.

VICE CHAIR RANSOM: What were you thinking in the drop-off? I mean there's a containment overpressure credit which would be a positive effect on time and then of course the decrease in need for, or decrease in decay heat, I guess, which is decreasing the need for coolant flow. Is that what you're referring to?

MR. GISCLON: That's the sort of thing we're thinking about. Also the fact that in long-term accident management, the temperature in that sump is doing to decrease. So the requirement is going to change with changing temperatures. So it's a time dependent thing.

CHAIRMAN WALLIS: That might not help you. It might be as you cool it down you deposit more stuff as they found in these tests. So you might want to keep it warm because this hurts your NPSH. The hotter the water is the worst.

MR. GISCLON: Depends upon the environment, sir. If you're operating in a sodium hydroxide and pure fiberglass you may have that effect.

CHAIRMAN WALLIS: Right.

MR. GISCLON: If it's some other environment --

CHAIRMAN WALLIS: So the plants made different operating strategies as well as different designs.

MR. GISCLON: Very plant specific.

MEMBER SIEBER: Of course, you're going to have a audit of NPSH requirement versus time due to the closed reduction that you'll be able to achieve as --

MR. GISCLON: Right. Certainly that's another factor.

MEMBER SIEBER: That's the key thing in my mind.

MR. GISCLON: Right. Reduction in temperature as well as the reduction in flow.

MEMBER SIEBER: Right.

MR. ZIGLER: Those are areas of margin to address Dr. Kress's concern about if we underestimate the bump-ups. Right now, keep in mind that the mission profiles that we're doing those things are had the maximum runout flows on it and the minimum temperatures on it. So we still have issues on it that we could do and we could also do modification of how they actually do the NCPR accident management space and the EOPs about actually reducing the flows because we all know that within 48 hours later we don't need 10,000 GPMs going through the core.

MEMBER SIEBER: On the other hand, if the strategy is to put in the biggest screen that you can put in, you may find that a plant doesn't have a lot of calsil. A lot of mere insulation or uninsulated pipes doesn't generate a lot of debris. You may not have to spend that much money. The bigger question though is how much margin do you feel you really need for a lot of these unknown factors that the testing so far hasn't identified or reduced to any kind of a correlation. Identifying that margin, I think, will be difficult.

MR. ZIGLER: Well, there is a certain category of plants which we finally called them the zero-head-loss plants which are essentially the ones that put in very large strainers and a very low fiber plants on it. And essentially even with operational margin for fiber to forget about jackets and whatever it is that may have been left over, you're still about a factor of two or three less than what's necessary to form a debris that if you place on all those strainers.

So then they have a considerable amount of open area and we all know from testing that if you even have 10 or 15 percent of your total area open, you essentially have no head loss going through your screens on it and so you couple that with those very large screens that have a very low approach for losses in the 0.00-something world on it where you essentially have approached velocities that are starting to become approximating the quiescent fiber deposition and still water on it.

I've run some tests with those kinds of strainers and again it becomes a part of the margin issue that we're talking about. You get to the point where it's almost physically impossible for you to deposit the fiber because you have to cause sufficient turbulence in your water to transport the fiber to the strainer. However, you may have too much turbulence now because your approach velocity is so low.

So this is a situation which I had in testing in Slovakia in the Manon Loop (PH) over there was exactly that. I had four burly Slovakians with compressed air to try to generate turbulence. But as I was generating turbulence and getting the debris to the strainer, it was also knocking the debris out. So you enter this never-ending situation.

So there are margins on it. We also talk about the non-uniform bed deposition issue. That is how you convert from flat-plate data to non-uniform because all the advanced-trained designers from every single one of the vendors rely on very heavy non-uniform bed deposition. That's just one of the tricks of the cards that we have in our pockets.

So yes, if you take chapter three analysis with 100 percent transport using flat-plate, you are in a very high, non-realistically conservative space. But if you now start addressing each one of those issues and applying reasonable judgment, turning on gravity, a few things of that nature, you start now coming into a situation that there is a resolution path ahead of you.

MEMBER SIEBER: On the other hand, all these elements of margin you have to document and have enough technical background and experimental data to be able to show that the margins that you say you have are realistic. It's not an easy problem.

MR. ZIGLER: No.

MEMBER SIEBER: Okay. Thank you.

MR. GISCLON: The target evaluation schedule that is on slide 13, step one, is underway now and again, this is tentative. subject to change through September.

CHAIRMAN WALLIS: If I were developing this, I would want to have very clear deliverables from each step. You said something here about the process you go through. Are you going to have it clear what it is you expect to arrive at at the end of each step and is the final step going to give a handbook for how to calculate all these things to the industry or is there going to be something else?

MEMBER SIEBER: It's too late.

MR. ZIGLER: No, not quite exactly. We're in the process of trying to finalize. This is an upper level tier program. We're just presenting the upper level. We're in the process right now of putting the meat into the skeleton that we've generated. So we're in the process of entering those questions. We already have developed a step-by-step about a 20 or 30 step process for step no. one, i.e. that you look and you gather the information and you see what the scoping, etc., step one. Then step two, we're in the process of developing. So what you see is --

CHAIRMAN WALLIS: You see what I mean. You have to know when you've reached the end of step three. That's when you achieve your deliverables.

MR. ZIGLER: Yes.

CHAIRMAN WALLIS: And it may well be that you need more activity in order to achieve those deliverables that you said you would deliver at the end of step three.

MR. ZIGLER: Yes, the last page.

CHAIRMAN WALLIS: I don't want to manage your research but I think I would want to be clear about just what it is you're going to deliver at each stage and how close you're going to come to giving recipes that industry can use for all these various phenomenon.

MR. GISCLON: That's a very astute observation because as Gil said, some of these steps are still in the process of being developed and certainly at some point in time, hopefully sooner rather than later, we will have nailed this down.

The final slide here, 14, considerations for the plan and in summary, the strategy is predicated on the latest information that has emerged from the ICET testing program. Should new information become available, the resolution strategy could, will and should be revised as appropriate. We feel that it's very important to have regular and meaningful communication with other entities that are involved in this, the NRC specifically and some of the other results that come out.

This is a very aggressive schedule and we feel that having this communication is going to be essential. We've had good communication so far and we want to be able to continue that.

The last slide that I would like to present to you is a summary of an active strainer concept which has been developed and is being marketed by General Electric. The active strainers respond differently to chemical effects than passive strainers do. The first point there is that they are not expected to develop a chemical effects precipitant on the active strainer surface because as you've observed so far, it's being continuously removed from the surface.

CHAIRMAN WALLIS: Where does it go?

MR. GISCLON: It goes underneath the location of the strainer.

CHAIRMAN WALLIS: And it's removed into some chamber and it doesn't come back into the system.

MR. GISCLON: I believe it's just knocked off in the containment.

MEMBER SIEBER: Probably on the floor.

MR. GISCLON: If General Electric is on the phone and can respond to that, I would invite your response.

MR. AMYL: Geoff Amyl speaking. Yes, the issue of where the debris goes, it would go into the containment and settle -- that in more detail that's requested.

MR. GISCLON: I believe that's it. If you have a strainer in a location within the containment, the debris gets accumulated on the strainer.

CHAIRMAN WALLIS: Just makes piles of debris somewhere in the containment where it's not going to be swept back into something.

MEMBER SIEBER: Yeah. Knocked off on the floor.

CHAIRMAN WALLIS: Now are these curves and artists of the cartoon of what might happen or are they based on an experiment of what did happen?

MR. GISCLON: I believe these are based on an experiment that actually did happen and you talked about the recipe that was provided by EPRI after the first ICET test was concluded. As was previously indicated, we had 15 or so gallons of sludge and people asked us where did this come from and we told them what the conditions were of the ICET test and they were able to generate some material which is probably not dissimilar to what this sludge material was. It's my impression that they use this material and basically prove that this acted device could manage it rather well. Geoff, are you still there?

MR. AMYL: I'm still here. John's summary is correct. That was gathered during the product demonstration that was held in January of 2005. Basically, it showed three full loadings of the acted strainer, one of fibrous debris, paint particles and one of calcium silicate and the third one of simulated corticochemicals (PH) effects as a takeaway here is that your head loss, that -- head loss value.

MR. ARCHITZEL: This is Ralph Architzel from the staff. I would like to say the NRC did observe it. I believe that's the same test, Geoff, right, that we observed?

MR. AMYL: That's correct, Ralph.

MR. ARCHITZEL: And we were provided a summary of the testing and it's documented in a trip report where we went. So that's a trip report that's available at least what we observed.

CHAIRMAN WALLIS: This is some kind of a rotating device?

MEMBER SIEBER: Yes, it is.

CHAIRMAN WALLIS: That cleans itself as it goes around.

MR. AMYL: This is the motor-driven plow and brush active strainer.

VICE CHAIR RANSOM: Is it possible to install these in most plants?

MR. AMYL: For the majority of the plants that we have looked at, the answer is yes.

MEMBER SIEBER: Everything is possible.

CHAIRMAN WALLIS: No, but if this solves all the problems, maybe you don't need quite so much information, if all the junk is scraped off no matter what it is, whether it is chemical effects or whatever.

MEMBER SIEBER: That's true.

CHAIRMAN WALLIS: The designer, an astute designer, might look for a solution that's kind of independent of the problem. It works on all problems. Of course, the silver bullet, but there may some solutions which are less dependent upon all these details which you could research forever.

MEMBER SIEBER: On the other hand, if it's active, that means it has a motor. If it has a motor, it needs diesel power. It's a 1A deal, putting in new power in the containment and it has to be single failure proof.

MR. AMYL: All of those points are well taken and are taken into consideration for the design.

MEMBER SIEBER: Yes. If you have extra kilowatts available on your diesel which a lot of folks don't, then the problem is simpler if you end up with an extra two diesels.

MR. AMYL: The loading here is very low, but if there are specific questions on the details, we can address those.

MEMBER SIEBER: Okay. This probably goes beyond the scope of what we wanted to hear but it's not a simple deal to put an active strainer in.

MR. LU: Sean Lu from Plant System. We did observe the GE active strainer design but it does require some minimum ∆P head loss across the rotation device. So not all the plants have that margin to use that. I just wanted to add one point there.

MEMBER SIEBER: Okay

MR. LU: Some plants can afford to use that. Some plants may not. It just depends on the plant-specific condition.

MEMBER SIEBER: Well, it's an interesting concept in any event because it really says that if you are willing to be heroic enough you can continue to run your plant.

MR. GISCLON: Very true. I think that concludes our presentation.

CHAIRMAN WALLIS: Okay. So are some of the members going to be leaving soon? Did I understand that at 5:30 p.m.?

MR. GISCLON: Thank you.

MEMBER SIEBER: Thank you very much.

CHAIRMAN WALLIS: Thank you very informative and useful. So this goes back to our master of ceremonies now.

MR. TREGONING: Yes, I guess. So we have two more talks. We understand the time limit. So we'll try to be as brief as possible. This first talk is much longer because it's focusing on follow-on research activities in the area of chemical effects based on the ICET results. This should be the slide packet that's in your handout.

CHAIRMAN WALLIS: Too many words. Too many words on these slides.

MR. TREGONING: The follow-on chemical effects research. We'll try to summarize quickly. The second slide, based on the ICET results, I think Paul Klein mentioned this, there are several technical areas that we've initiated additional research in. We've addressed some of the results and implications.

The first one which Mark Klasky is going to present will be supplemental testing and analysis with respect to the ICET results. So there will actually results presented for that technical area. The other technical areas have initiated within the last few months. So mainly the strategy is just to present those with the objectives and approach and any preliminary results and status. So we can move to these other programs pretty quickly. The final one is peer review which we're just initiating.

As I think was alluded to on slide three, we've tried to develop a team to look at chemical effects between three different laboratories, Los Alamos certainly, Argonne National Laboratory and then Southwest Research Institute and we're working in a collaborative environment to try to, each laboratory is addressing different phases and different aspects of the challenge. The goal of the challenge is to communicate information amongst all the labs so that again we're not duplicating effort. Peer review which we're initiating now we're hoping will be used to inform all this research activity that we've previously initiated. Next slide.

I'm not going to go through the --

MEMBER SIEBER: Chart.

MR. TREGONING: -- flowchart again. But again I just want to stress that we're dealing with areas in the boxes that are on the left-hand side related to chemical effects for the synergistic chemical effect head loss testing.

CHAIRMAN WALLIS: I don't know how Argonne would produce something like a stimulant of your stuff without actually doing what you did because I don't know how they were create these web-like structures and things in any way than the way in which you did it.

MR. TREGONING: I'm going to defer that if I may. There are some aspects of these tests. Because what Argonne is doing is they're looking at initially doing short-term tests where Los Alamos has been focusing mainly on long-term tests.

Now there might be attributes of the long-term tests that will not be able to simulate in a short-term environment. So that may require us to do some additional work either outside with respect to insulation. I think Bill mentioned earlier having pre-aging the insulation potentially to try to get it to a certain state in time so that we can do accelerated testings.

There are advantages and disadvantages to doing both short- and long-term testing. So the strategy we have is to do a little bit of both and try to marry the results we get from each type of testing to get a full understanding as much as we can of what's going on here.

CHAIRMAN WALLIS: They may have to build a loop like yours to make the kind of stuff that they need to test.

MR. TREGONING: Potentially. I have a slide out of place here. I apologize. We're going to come back to slide five. I want to jump to the slide six first which is -- is going to do additional interrogation of the ICET results and I'm going to turn it over to Mark Klasky.

MR. KLASKY: Okay. Thank you. The motivation for this work is to provide the understanding so that we can basically produce or attempt to produce the surrogate, understand perhaps ways to mitigate the chemical effects and finally to perhaps understand the capture of particles on the (Cough.) class in the formation of the web.

In the next slide, we pretty talked about the test one solubility of aluminum and our belief that we really kept most of the aluminum at least suspended within the solution. If we did not have precipitation that was indicated by a number of different measurements including the hydrogen and also the lack of difference between the filtered and unfiltered concentration. So we had pretty much the hypothesis that the aluminum and boron were along with sodium hydroxide the major actors in this chemical system.

And so in order to really understand the chemical speciation and attempt to produce a surrogate, the next step was to examine the crystalline structure or lack of crystalline structure in this case that is. So we performed XRD analysis. Next slide.

What this illustrates in the case where you have crystalline structure you see very sharp defraction peaks, just Bragg's law scattering. In these two figures, you see the absence of these peaks. So consequently, there really is no structure or uniformity in structure and we refer to this material as a amorphous.

This is important because the solubility of the aluminum is very dependent upon the equilibrium solid phase. Also it turns out that the solubility or the phase present is very dependent upon a number of factors and that includes the means by which one produces the solution that is rapid titration of sodium hydroxide into a solution produces a more crystalline phase in some cases and also impurities have really a very dramatic effect on the phase present in the solution and we'll talk about that a bit more.

Bottomline is that if in fact we do have a amorphous material at least initially within the ICET environment we'd expect the material to be soluble and then at room temperature given the concentrations that we observed in the test about 370 milligrams per liter we would expect precipitation to occur.

CHAIRMAN WALLIS: Does this amorphous phase turn into the crystalline phase given enough time?

MR. KLASKY: Exactly. I'm going to show that in the next figure. Next one. This table --

CHAIRMAN WALLIS: And then it's hard to redissolve. Right?

MR. KLASKY: Correct. So as a function of time, what you have is this phase transformation in which initially it depends upon any number of factors, one of which most importantly perhaps is the pH, you undergo this phase transformation. It's Oswald's Rule of Stages where the first thing that comes out of the most thermodynamically unstable followed by suggestive stable compounds and eventually, of course, thermodynamics works and you wind up with your most stable stage.

The only problem is that reaching that most stable stage takes time. Aluminum is notoriously slow in reaching that final thermodynamically stable stage. So the table here basically, the column that's most applicable is the right most column in which the OH/AL tool is 3.25 and I have a little titration curve to the right. It tells you that at test temperatures it roughly corresponds to 9.5 in the way of a pH.

Basically from this column, if one just had pure aluminum in a sodium hydroxide system for example one would expect a crystalline material to be produced. So the fact that we don't have crystalline material --

CHAIRMAN WALLIS: Crystalline material all of them aluminum hydroxide.

MR. KLASKY: Excuse me.

CHAIRMAN WALLIS: Made up of aluminum hydroxide warrants some expectation and it's in an attempt to explain. Actually, it's deferred to a couple slides. I can't immediately present it but I just want to say something about the pH changes that we observed even in the ICET test.

Upon conclusion of the test, the solution was placed in an oven at test temperature. I think we indicated before that after a number of weeks there was observable precipitant that was formed. So whether it just took that extra amount of weeks, that's a question. We've measured pH and pH in fact increased and the increase in pH is basically explained from the further dissolution or the precipitation of the aluminum hydroxide in solution. That is the aluminate ion combines with the colloid (PH) phase to produce two aluminum colloids plus a NOH and thereby increasing the pH.

We also examined the solution following the precipitation and found that the concentration of aluminum and boron had diminished presumably, of course, that they were in the precipitant and those values, that is the 48 milligrams per liter with respect to the aluminum is again pretty consistent with the idea that we had a amorphous aluminum which in fact we measured with XRD.

With respect to the boron, boron, I think I mentioned earlier, is very readily absorbed onto the amorphous aluminum and so the fact that we basically went from about 2800 milligrams per liter to 2,000 is basically explainable and I'll elaborate on that a little later hopefully in the next slide.

So with respect to why we have a amorphous phase aluminum when a pure just aluminum hydroxide system which has previously shown that we would have crystal in it, that's explainable by examining the role of boron and to understand the role of boron we've performed some more analyses or investigations with nuclear magnet resonance both liquid and solid state and actually we were able to find a coordination between the aluminum and the boron in the solid state at NMR. I have a couple slides on page 13 and 14. It illustrates both liquid and solid state NMR results.

I'll examine just very briefly. Figure 13 is the typical aluminate ion. It gives a shift of about 80.2 ppm, the shift relative to the magnetic field that you apply. The solid state NMR, the point to be noted on the figure to the left is that the aluminate ion which was indicated at 80.2 basically now has moved, it's hard to read, but it's about 60 or so ppm. That shift is attributed to the coordination of tetrahedral and triagonal boron with aluminum.

To further explain that, that figure on the right is actually a boron NMR for an 11 NMR and we can basically conclude that we have both tetrahedral and triagonal boron coordinated to the aluminum. So that point is that we've identified a complex between over-minimum boron and there was some debatable or controversy concerning whether a complex between aluminum and boron did indeed exist. There was conflicting literature that did exist. We feel we are consistent with the more recent French investigations that basically observe coordination between the aluminum and boron.

Why is this important? It's important because it really is the mechanism by which the aluminum is basically precluded from crystallizing. To illustrate that I just have a simple depiction of page 15 of how that works. It's really precludes the assembly of the unit structures and so consequently wind up with this amorphous phase that's retained for longer periods of time as of release.

So with respect to IC, I think we can explain, understand, the phase at least initially that is present attributed to mainly the boron absorption until the amorphous aluminum. There was a question, I think, earlier. Do we have colloids at test temperature?

Well, unfortunately, we didn't retain the test solution from IC test one at 60 degrees. It cooled and so right now the only thing that I can tell is which is surprising that we have large particle because it precipitated. We examined ICET test one with dynamic light scattering and we do see both large particles and particles also at about a micron in size as well.

CHAIRMAN WALLIS: Excuse me. You have H_3BO_3 and then NaOH and aluminum and your only product involved is aluminum hydroxide. You have no other aluminum compounds that are produced.

MR. KLASKY: In general, that is correct. The XRD did examine -- We have a couple other XRDs where we did also find the sodium borates, crystal in structure determined. So with respect to aluminum, we didn't observe any crystal in aluminum compounds. With respect to other species, there are other species.

CHAIRMAN WALLIS: Yeah, but they're very small in trace species.

MR. KLASKY: Well, I think Bruce alluded to it. The uniformity of the debris sediment really doesn't allow for that conclusion. It's sort of a mixed bag. You can look at a given sample and find that a zinc, for example, or a calcium. So it's not a uniform system or a homogenous system.

Given that knowledge that we've obtained from the ICET test one solution, the question is can we produce a surrogate to be used in subsequent head loss tests or particle capture studies, filtration studies and can we mitigate this formation of this precipitant.

So what we've tried to do is to produce systems, basically aluminum boron/sodium hydroxide systems, and examine those systems with a number of different techniques including the NMR and ICP and light scattering. We've produced solutions by a number of different techniques, titration and also the dissolution of metal which is obviously analogous to the situation that we have in ICET.

Included in this development of a surrogate, we also have attempted to grow the web material between the fibers. As I indicated, we were successful in the aluminum boron system. After 30 days, we did produce the webbing. It was interesting that, as I mentioned, it did require the presence of aluminum. We did not see in the system that did not have the aluminum formation of any web-like structure which is peculiar in that it's not a constituent of the web itself or doesn't appear to be.

And the consistency of the precipitant did resemble, and that's just obviously qualitative, that did resemble what we observed in the way of precipitant on -- I think I have a picture of the web that we produced on page 19.

CHAIRMAN WALLIS: Amazing.

MR. KLASKY: So to try to address the question of do we have colloid at 60 degrees, we did the light scattering at 60 degrees and found something that I find pretty remarkable and that is we dissolved the metal and I have two figures on page 20 that illustrate even after 20 minutes, we were able to observe particles at several microns or ten microns, I think. And that certainly contradicts what we observed in ICET where there was no difference between concentrations that were measured between filtered and unfiltered. So in addition to this after a couple weeks, we observed visible precipitant at test temperature.

So you start to ask the question what is the difference between the ICET and this surrogate solution. The answer is perhaps well there are any number of impurities in the ICET system including we have dirt or latent debris as we call it. We also have about, I think, 15 milligrams or so per liter of organic and that can actually affect solubilities as well of aluminum. It's known to do that. So the bottomline is we've had big success in terms of defining a surrogate because certainly in our bench scale test we have observed precipitation at 60 degrees.

CHAIRMAN WALLIS: Now this web-like structure it does look like a sheet of material between fiberglass.

MR. KLASKY: Right.

CHAIRMAN WALLIS: Now presumably, it doesn't miraculously appear. There must be some way it grew or developed from something. Do you have any intermediate stages in its development? Tell us how it was made.

MR. KLASKY: Unfortunately, we didn't see it. We examined it after two weeks and didn't see it.

CHAIRMAN WALLIS: It was suddenly there or something?

MR. KLASKY: After another two weeks, right. We didn't have the high speed photography to view this and of course, this is post test examination where we remove it from solution.

CHAIRMAN WALLIS: Just how amazing how it would be developed.

MR. KLASKY: You know what? I thought about that and that's why some of the work that we've done has examined the colloids and I thought well perhaps you have this attachment and subsequent growth on the fiber itself. So you have these structures that build but it's kind of hard to imagine how you get half way there.

CHAIRMAN WALLIS: Right.

MR. KLASKY: So I don't have an answer. It seems to be more apparent at vertices. So there is that aspect.

CHAIRMAN WALLIS: More at the vertices.

MR. KLASKY: But no, I can't envision a means by which --

CHAIRMAN WALLIS: It's almost as though it could grow in the vertices and then there's some sort of surface tension or something that pulls it across to fill this bound.

MR. KLASKY: Expansion.

CHAIRMAN WALLIS: Right.

MR. KLASKY: So the bottomline is again, we've had mixed success with respect to the surrogate. I think the real question is are we seeing phase transformation and consequently precipitation after two weeks in this what I call "cleaner system" relative to ICET. Is what we've observed in ICET after a couple weeks of, let's say, gestation or heating in post tests, is that a real effect? Are we likely to observe precipitation if we were to have waited for 40 days or thereabouts? Unknown at this point.

Finally, I just want to mention that this understanding that I think is also allowing us to start to think about means to mitigate and by mitigate, I mean, to use chelating agents to basically keep the aluminum in solution. We've done some tests with citric acid and EDTA and been somewhat successful doing that at room temperature.

We just performed a test where we put in about 400 milligrams per liter aluminum with the boron and pH of about 9.5 and after two weeks with citric acid, we do not observe any precipitation and that's at room temperature. That's one means of dealing with this chemical problem which is to keep it in solution. The question, of course, is in that solution do we have particles that could attach as well.

The next step would be to put in the fiber and see what we grow on fiber in that citric acid system. So that's where we are in the current research and tests.

CHAIRMAN WALLIS: Thank you.

MR. KLASKY: You're welcome.

MR. TREGONING: Citric acid, is there enough to affect the pH level?

MR. KLASKY: We performed the test where

we added the citric acid and adjusted the pH such that we were basically looking at a comparable system. And it wasn't a lot. It was one mole of citric acid per mole of aluminum. It's not all that much, a little orange juice.

CHAIRMAN WALLIS: That's right. You could hang some lemons in the containment.

MR. KLASKY: Something like that.

MR. TREGONING: I'm going to jump back to slide five and then jump forward. I just wanted to introduce the chemical effects head loss testing. This is the work that we have ongoing right now at Argonne. I'm going to provide a brief overview. Obviously if there are questions, Dr. Shack is right here to address them.

The objectives of this work are to evaluate head loss associated with chemical byproducts which form at least initially in the ICET environment and understand how relevant changes in that environment leads to different chemical byproducts and their associated head loss. So some of the objectives in this work are certainly overlapping the work that LANL is doing as well.

The motivation is obvious. We have really very little information on head loss and we need to understand how plant to plant variability might affect head loss formation. Let me --

CHAIRMAN WALLIS: It would be interesting. I mean in the great effort to make things better there's a drive to put in more screen area and to reduce the velocity. You might reduce the velocity so much that it's incapable of bursting these tents.

MR. TREGONING: That's potential ramifications. Yes.

CHAIRMAN WALLIS: Maybe it's not velocity. Maybe it's pressure. I don't know how you burst these tents. But presumably if you look at the pictures, it looks as if the fiberglass is filled with this stuff which would prevent something going through there.

MR. TREGONING: In some cases, yes.

CHAIRMAN WALLIS: It appears to be like that.

MR. TREGONING: Well again there's quite a bit of heterogeneity. I think Bruce alluded to this. When you look at the -

CHAIRMAN WALLIS: Yes, but if there's a window filling in every little space in the fiberglass, then it's like a closed cell foam.

MR. TREGONING: Again, you see the small SEM pictures and I don't want to say that's --

CHAIRMAN WALLIS: I have no idea how robust those things are.

MR. TREGONING: Well, you don't have any idea how robust it is and again, while it can prevalent, it's not necessarily that uniform over the entire surface. So you're right. You're looking at things on a microscale and understanding the macroscale ramifications in terms of head loss. That's really what we're most interested in with respect to the fibrous debris deposits what the macroscale ramifications are in terms of head loss.

CHAIRMAN WALLIS: And you haven't done any crude experiments where you simply take this and put into the University of New Mexico head loss test and even though you weren't paid to do so, you could still do it.

MEMBER SIEBER: There you go.

CHAIRMAN WALLIS: And then you write the proposal to do the work having already got the results. That's an old way to operate.

MR. TREGONING: I guess one point I'd like to make is these are joint tests.

CHAIRMAN WALLIS: That's the way Kutataladze or whatever his name, he used to say that's the way he operated in Russia. He would do the test and then he'd write the proposal for the work. You always keep a step ahead.

MR. TREGONING: There are challenges whenever you remove material from the loop and then try to reconstitute it in another.

CHAIRMAN WALLIS: Right.

MR. TREGONING: So if it were that simple --

CHAIRMAN WALLIS: But aren't the other facilities close by? Put it in a plastic bag, run over there and --

MR. TREGONING: It's in an adjacent building.

CHAIRMAN WALLIS: Before it dies you put it in. I'm sorry. But obviously there's some questions about this how you simulate the real stuff in the head loss test.

MR. TREGONING: Again the other thing you have to remember is this testing was done under the framework of an MOU and the MOU specifically precluded the head loss test.

CHAIRMAN WALLIS: Won't let you do it.

MR. TREGONING: It would have been a point of consideration that would have needed to be negotiated.

MEMBER DENNING: At what point is the MOU over? The thing that bothers me and I think it's bothering everybody here is starting up tests at Argonne, and we're not sure that you can develop the surrogates, is a risk, whereas you have Los Alamos or you have a facility in New Mexico that you could just extend probably, take the surrogate materials and use them in some form of head loss test. Seems like the most natural thing to do. I guess the question is is it just the MOU that's preventing you from moving in that direction aggressively in parallel with what's happening at Argonne or are there other technical reasons?

MR. TREGONING: Again, I don't want to pin the blame on the MOU so to speak. I mean there are technical considerations. Ideally, if we were going to investigate this in an integrated manner, we'd have the head loss loop to be consistent with the same loop the chamber is in. So what would happen right now we do have a head loss facility. It's maybe an eighth of a mile away.

CHAIRMAN WALLIS: Pick it up and move it next to it.

MR. TREGONING: When you move the insulation, you have all these questions about how you get the insulation to form a debris bed and how representative is that of the bed.

CHAIRMAN WALLIS: You see what Rich is saying though. I may be even more difficult to duplicate things at Argonne.

MR. TREGONING: It may be and again I think that's why we're looking at strategies where we try to do a couple of things at once, where we try to do some long-term tests.

CHAIRMAN WALLIS: If Argonne is going to build a chamber to generate this stuff, it would be very interesting to see if they could duplicate any of your tests because conceivably they run the matrix and get completely inverse results from what you have.

MR. TREGONING: I wouldn't be that --

CHAIRMAN WALLIS: I'm not saying it's possible. It's such a small database. There must be a lot of uncertainties about whether you can duplicate the condition.

MR. TREGONING: One of the things that LANL has done in subsequent tests is they have placed samples and Bruce alluded to this earlier. We're going to see a slide on this. They placed some samples in the ICET loop to be used in a gravitationally-fed head loss column and as Bruce alluded to, this might provide some quick and dirty information. However, even that has a lot of uncertainty associated with it.

CHAIRMAN WALLIS: Yeah.

MR. TREGONING: So I don't want to predict here but certainly evolving into some sort of integrated head loss.

CHAIRMAN WALLIS: But before you build a $100,000 facility, it might be used to do some quick benchtop tests where you grab some samples and do it very quick, drain through a filter or whatever test to see if you can get anything to flow through this stuff and it takes a certain pressure drop or something, some idea of what you're dealing with. It might be extraordinarily cheap compared with committing yourself to some facility.

MR. TREGONING: It might be. But again, the thing you have to remember is these fiberglass samples that are taken out especially in test one, they weren't keep at temperature.

CHAIRMAN WALLIS: So it's impossible.

MR. TREGONING: You saw the dessication crack.

CHAIRMAN WALLIS: I know.

MR. TREGONING: I just think the knowledge to be gained by that might not be as great as one would like.

CHAIRMAN WALLIS: But I think you might reduce the risk of going with some test with surrogates which won't really duplicate or something.

MR. TREGONING: Again, one of the things we're trying to do is benchtop simulation.

CHAIRMAN WALLIS: Yes, I think you must do it. I would again commend a lot of that even if it's not in the program.

MR. TREGONING: It is in the program. Argonne's been doing -

CHAIRMAN WALLIS: You have permission to do it.

MR. TREGONING: Argonne's been doing a lot of benchtop simulation and LANL as well as you've seen has done quite a bit of benchtop simulation. So if you looked at the industry's proposal, they are essentially proposing to do just that. So assuming we can work out the details, that would potentially be some more information that will be, I'll say, that could be valuable because they're approaching it from a very different way.

But that was one of the first questions we dealt with. How do we want to try to recreate these environments? Do you take the archival material or do we try to create either surrogates or recreate that environment?

CHAIRMAN WALLIS: Well, if you have this stuff in your chamber which is this fiberglass with windows in it and you take it out and you dessicate it and you look at it, presumably it has windows when it's in the chamber. It would seem very simple to take some of it and flow some water through it and see if the windows are still there.

MEMBER SHACK: Let me just argue since I'm the guy with the other head loss loop. There are two things that are going on here. There's the possible head loss you get from what's going on within the fiberglass. There's also the head loss that you could get from the precipitates like the calcium phosphate, like the gelatinous material.

One problem I see with the current ICET test is that if you put a side loop on it and you grew it there, you couldn't control the mass of product that's really over there. It's sort of settles out in the bottom of that tank and how much do I get on it, I don't know.

CHAIRMAN WALLIS: You get whatever you get.

MEMBER SHACK: And you get whatever you get and in the Argonne test, we will have the disadvantage that we're not simulating the long-term growth of the fiberglass but we'll be able to carefully control the amount of chemical product that's deposited on the fiberglass. And we also admittedly started this before the realization of the web phenomenon was really at its peak.

And again one possible solution there is to essentially pre-age these fiberglass beds so that you get the fiberglass bed which you put it in a test loop where you can control the amount of the chemical product. So those become independent variables which I think you need when you plan to scale this back to a plant-like analysis.

MR. LETELLIER: I think we have a very collaborative and complimentary approach to the research, but I think the challenges of quantifying how much chemical product is actually in the fiber bed are exactly the same. It's not as easy as separating the effects simply by controlling the amount of ingredients that you put in the loop. Because as we've talked about, it's very difficult to measure how much resides in the bed and that's what actually induces the head loss.

That's why we're thinking about these techniques for thermogravimetric analysis so that we can look at the volume to mass ratios that are actually represented by that product. I think you're going to have some challenges in that regard as well even if you know exactly how much of this and that you initiated the loop with.

MEMBER SHACK: Well, that's certainly possible. It's easier for me to control in my loop because it's not all going to settle out among all those plates and that thing which goes around in my loop is just going keep going around to a much greater extent than it is in your bed.

CHAIRMAN WALLIS: So what do you expect might happen in the plant? You have this stuff and it accumulates on the screen, but it doesn't have webs in it yet and you're going to run this recirc for 24, 40, 55 days whatever and yet about the 20th days, it's all going to be bunged up with webs. Is that what you think might happen?

MR. LETELLIER: I don't think that we've proven that the webs can actually form.

CHAIRMAN WALLIS: Yeah, but the webs don't form instantaneously in your system.

MR. LETELLIER: It will evolve though.

CHAIRMAN WALLIS: They take days in the plant.

MR. LETELLIER: That's true.

MR. TREGONING: And again as we've mentioned those are relatively quiescent conditions and under following conditions --

CHAIRMAN WALLIS: Right. So you have to somehow simulate conditions for web formation in the plant.

MR. TREGONING: Flow rate's an important variable that we haven't been able to vary. Again, the ICET chamber was never designed to vary flow rate. That's the advantage of when I say flow rate through at least initially through a debris bed.

CHAIRMAN WALLIS: There's nothing in the 6224 correlation that says how you treat or whatever.

MR. LETELLIER: Unfortunately, no. There's nothing to preclude controlling flow rate using the ICET loop. We have a variable speed pump with a design range for that purpose. I mean it could be modified. I think that the series of tests performed under the MOU has given us some clear direction, some clear questions, that need to be pursued. But at the end of this series, we'll have the flexibility to pursue some of those issues by redesigning our loop, by supporting the research effort at Argonne and tackling these from multi-facets. Whether we replicate for independent verification or we pursue separate issues that are complimentary, it will be to the industry's advantage.

CHAIRMAN WALLIS: But it seems to me -- I don't know. I am sort of speculating but the webs could have a certain sort of strength to them and they build up and you need to evaluate. So if you're going to model this thing, you need to have some kind of a burst strength for these membranes or something and then you have to put that together to get the material properties of the whole thing.

MEMBER DENNING: Or they never form. Don't forget --

CHAIRMAN WALLIS: Or they never form if you have enough flow. Right.

MEMBER DENNING: If they're going to burst, you have the largest flow going through at the beginning anyway.

CHAIRMAN WALLIS: Right. Maybe they never form.

MR. LETELLIER: One of the very simple tests that could be done if you're ready to move to that slide, we've actually prepared coupon samples that have been exposed at solution. They are prepared and loaded with those products and they could be exposed to either a flowing loop or a gravity feed drain and then we would post examine them for rupture of those diaphanous sheets that you described.

We can look for evidence of retention. We can do chemical analysis on the effluent. We can essentially discretely simulate a flowing condition.

CHAIRMAN WALLIS: They may never flow and they may never form. It may well be that they form in the fiberglass which is lying on the floor and that doesn't do any harm at all.

MR. LETELLIER: That question could only be answer in a flowing loop --

CHAIRMAN WALLIS: Because if your pillow is on, it doesn't have much flow through them at all.

MR. LETELLIER: That's true.

MEMBER SIEBER: The only way you can gather the information is through a post moratorium (PH) examination as opposed to in-process examination which is less desirable.

MR. LETELLIER: In the next series of ICET tests whenever that is constituted, we could actually have time-dependent samples where we pull them out as a function of development and run them through a test protocol for head loss property. This is the ultimate metric that you're interested despite what it's visible appearance looks like. You really want to know what ΔP is.

CHAIRMAN WALLIS: Well, if this is true that this is forming at the joints, then in the static systems, it leaps across and makes a window between the fiberglass strands. It may well be that in a flowing system it still forms at the joints but then it makes some other structure because of the interact with the flow which we don't know. Maybe it extrudes a thread or something.

MR. LETELLIER: It's quite possible.

CHAIRMAN WALLIS: I have no idea but I don't know how you even examine that.

MR. LETELLIER: One of the principal challenges for head loss testing is bed uniformity. It's very difficult to compare two sets of test series unless you're confident that your bed morphology looks similar. That's one of the real advantages, I think, in pre-exposing a prepared coupon for that purpose. We are working very hard to standardize the bed preparation techniques for use at Argonne based on the experience that we've gained over time. But even there from test to test, you're not absolutely certain that they're same, whether or not a hole in your fiberglass map has skewed your interpretation of the chemical effect.

MEMBER SHACK: But again, we would do these tests where you'd make the bed, you'd do the pretest before the aging and we could do that in the Argonne loop too if you're willing to tie it up for 20 days. There's nothing that says that you can't build a bed and let it sit there with a flow circulating through it at whatever rate you'd like for as long as you like as long as somebody's willing to wait.

CHAIRMAN WALLIS: You might want to build several loops then.

MEMBER DENNING: It seems to be that this uncertainty that's suddenly is facing us. The funny thing that we've seen is the formation of these webs. For the next six months, we're not going to have a clue as to how important they are or how unimportant they are unless we move forward with Los Alamos and the University in doing something. And maybe we're not going to learn anything there.

But you could very well. It could be that they go ahead and in six months they'll come in and they'll show us things and we'll say, "Of course. We should have known all along it couldn't have formed or it broke" or something like that. But I don't see us doing anything in the short-term associated with particular issue although they've taken some data and are taking some data that could be used in that direction with some experience.

CHAIRMAN WALLIS: I would like to know if the webs increase the tenacity with which the fiberglass holds together and if I have asthmatic, fractured fiberglass I can grab pieces and pull it out very easily. But if I have webs in there, does it have a much more tenacious structure? Are they rubbery? I have no idea. Are they like glue? They glue everything together. Or are they just like water and almost no effect?

MR. LETELLIER: It's difficult to speculate what the mechanical changes would be.

CHAIRMAN WALLIS: Have you tried to pull your pillow apart?

MEMBER SIEBER: I'll try that.

MR. LETELLIER: The micromechanical measurements, I really believe, are best approached through a head loss measurement.

CHAIRMAN WALLIS: Yes.

MR. LETELLIER: There are some specialized techniques for doing that type of thing, tensionometers, at those scales, but it just doesn't make sense. We're interested in a macroscopic bulk effect and it should be measured in that manner.

CHAIRMAN WALLIS: What happens to pillows when you finish with the experiment.

MR. LETELLIER: They're normally drained and dried in air.

CHAIRMAN WALLIS: And put somewhere?

MR. LETELLIER: Yes, they're all in storage and archived.

CHAIRMAN WALLIS: No one has done anything with them.

MR. LETELLIER: Well, as John mentioned, we're sharing some of those with industry but they have not undergone further examination. They're in storage.

CHAIRMAN WALLIS: Well, do they still feel like pillows or do they feel like they're something solid?

MR. LETELLIER: They are dried and crusty. They're quite crisp.

CHAIRMAN WALLIS: So they're solid. They're not as squishy as they were when they were new.

MR. LETELLIER: Of course not, they're

not.

CHAIRMAN WALLIS: Why not of course not?

MR. LETELLIER: They're not the same. They show evidence of crystalline chemical.

CHAIRMAN WALLIS: So the web's stiffened. The web has given more stiffness.

MR. LETELLIER: Once it's dried.

MEMBER SIEBER: Well, something gives it more stiffness.

CHAIRMAN WALLIS: So it's more calsil form maybe. Am I holding you up? Are you going to tell us more?

MR. TREGONING: That's what we're here for.

CHAIRMAN WALLIS: That's what we're here for is a discussion.

MR. TREGONING: What we're here for is to have this discussion and again we had many similar discussions. I don't know if we want to discuss the Argonne loop at all.

CHAIRMAN WALLIS: I don't know but it all looks the same.

MR. TREGONING: We'll bypass the loop. Let's go to slide 26, a little bit about Argonne's at least, initial -- metrics that they're looking at.

CHAIRMAN WALLIS: That's a pretty basic loop there.

MR. TREGONING: You said it yourself. A loop is a loop.

MEMBER SIEBER: Don't worry about it.

MEMBER SHACK: So control the temperature is the only real difference between the original and we try to reduce hideout. It's small changes.

MR. TREGONING: There will be some baseline testing without chemical effects to benchmark against prior work.

CHAIRMAN WALLIS: Since you've found surprises, it's almost highly likely that Argonne will find surprises.

MR. TREGONING: Surprises in terms of?

CHAIRMAN WALLIS: What they find that they didn't expect and Argonne has, you could have these peer review people who are smarter then and ask the right sort of questions and make sure that hopefully catch things you might forget or miss or whatever.

MR. TREGONING: Again, we're at the beginning of this and I'd say our strategy is evolving as we learn more and get more information. That's certainly a fair statement and that's how we're going to proceed throughout all of this. We understand that there's some risk involved with some initial short-term testing. But there's also potential benefits to be gained with short-term testing as well.

With long-term testing, we're limited to a relatively small number of conditions if we're trying to actually create 30-day simulations. So there's pros and cons to each testing strategy and how we're trying to proceed is utilize the strengths of each type of testing strategy to their best advantage. So at least in here on 26, initially Argonne is looking ICET 1 and 3 products. Those are the ones that are most interesting in terms of chemical byproducts that have been formed, the face cream, describe structure in ICET and then the variously described precipitate and the pillow structure --

CHAIRMAN WALLIS: Maybe you have a product, this can sell. Face cream product.

MR. TREGONING: We won't be contracting with Noxema here. Their status is they're in the middle of fabricating the loop now and they're looking at conducing shakedown testing.

CHAIRMAN WALLIS: Are there any other situations where web-like structures like this have been observed.

MR. KLASKY: Observed? Dendrite

structures.

CHAIRMAN WALLIS: I don't know what you call a material which is made up of fibers. A fiber map I suppose you call it. Are there any similar situations where web-like structures have been observed with different kinds of chemicals and some idea how they form.

MR. KLASKY: On filters, they form dendrite structures.

CHAIRMAN WALLIS: Dendrite, but that's not a web, is it?

MR. KLASKY: No, I can't say that I've seen in a liquid system structures spin webs or fibers. No.

CHAIRMAN WALLIS: In two phase system, you make foam. That's right away. You have windows and all that.

MR. TREGONING: Moving on to slide 27, Bruce, you might want to hit the highlights. I think we talked about this quite a bit.

CHAIRMAN WALLIS: Yes.

MR. LETELLIER: It's good to get some visual clues as to what we're talking about with some of the samples in these prepared bed configurations. This is one of these benchtop experiments that could be done at little expense that might give you some great added intuition about how to design a more complex loop.

We have exposed fiberglass samples like is shown in the upper panel. They are two inches in diameter so that they conveniently fit inside of PVC piping and they contain seven grams of fiberglass that's been precompressed in order to avoid compressibility issues in the head loss correlation and it's a pretty simple matter to fit this with a pressure transducer and/or videophotometrics.

CHAIRMAN WALLIS: It doesn't look very homogeneous. It seems to have gray areas.

MR. LETELLIER: The compaction is about seven to eight times the manufactured density.

CHAIRMAN WALLIS: It just looks as if there are cracks or regions of low density.

MR. LETELLIER: That may be true and one of the important aspects would be to characterize the variability of clean fiber prepared in exactly the same manner so that you establish a baseline. We have five of these exposed in test three. Those were drained and they're being stored moist in plastic. Five prepared in test four. Those are being stored at temperature in the test solution and we will introduce these to test five as well.

As we iron out a procedure under the MOU with the industry to test these, I think it could be performed relatively quickly. This could give you an opportunity to execute multiple rinses to assess adhesion. You could look at evidence of rupture in a post mortem examination and of course, you would want to dissect one of these to do all the precharacterization of the loadings. The key element still for any kind of head loss correlation is to relate the head loss to the amount of mass present. So this volume-to-mass ratio is probably one of the greater challenges for treating amorphous hydrated materials. It's no longer a grain of sand that you can apply geometry to and come pretty close.

CHAIRMAN WALLIS: It would be interesting to see how big L1, is it, has to be in order for anything to happen at all?

MR. LETELLIER: I've actually done these tests with clean fiber and the results are quite comparable to the 6224 correlation -

CHAIRMAN WALLIS: They might well be.

MR. LETELLIER: -- in the absence of compression and this is six feet of head basically -

CHAIRMAN WALLIS: And if you have the --

MR. TREGONING: It's L4, not L1.

CHAIRMAN WALLIS: If you have the webs, nothing may happen at all.

MR. LETELLIER: In any case, this work is undergoing. It is within scope of the present tasking and it's intended to be complimentary to the work that's being done to design other loops.

MEMBER DENNING: It is within scope?

MR. LETELLIER: Yes.

MEMBER DENNING: Until maybe when you get the MOU work? Sorry, it's in your scope.

MR. LETELLIER: It is in my scope to support this type of work.

MR. TREGONING: Slide 28, we alluded to some of this work earlier, chemical speciation work. The objection of this work is to evaluate the ability of analytical tools to predict high product formation in plant-specific environments. The strategy is to look at number of commercially-available codes and first recommend the best code and attempt to evaluate the applicability of these codes for plant-specific environments and utilize these codes to conduct simulations of the ICET tests for benchmarking studies.

Initially, the simulations will be blind.

So they'll be based on corrosion rate and dissolution rate information that's supplied to the models and then we'll be looking at differences between the model predictions and what we actually see in the ICET test. There are ways to depose test calibration of the results. So there will be some post-test calibration done to look at the sensitivity for how you have to tweak these models to get them to work. If it ends up that there is different tweaking that's needed for every condition, then that's obviously an important consideration.

Ideally, we'd obviously like to have a tool that we felt somewhat comfortable with in terms of predicting speciation that we could give to NRR for evaluation. Now we're a long way away from that obviously.

MEMBER KRESS: This stuff you get precipitating out and even in solution, it's just one chemical compound. I think you have a real chance here. If you had several different species acting together in that, you may have a problem.

MR. TREGONING: That certainly makes it more complex. There's no doubt about that.

MEMBER KRESS: It might depend on rates then and 20 percent takes first and what happens to it.

MR. TREGONING: And again, like I mentioned, the codes don't simulate kinetics.

MEMBER KRESS: You don't have kinetics.

MR. TREGONING: So what you'll do is you'll predict species that are over the side.

MEMBER KRESS: It could very well be that this web-like stuff in there is just one species.

MR. TREGONING: Sure.

MEMBER KRESS: If that's the case, you have a good chance.

MR. TREGONING: I think we've already noted and the analyst noted the web-like material has certainly different apparent elements.

MEMBER KRESS: Different elements but it may be just one species.

MR. KLASKY: I think the real challenge is the time dependent and phase transitions that are occurring and changes in solubility that result from that.

CHAIRMAN WALLIS: Not just that. There's a physical structure of the stuff. You may be able to predict that you have 100 grams in something of stuff. But if it's in a cube, it's very different than if it's in web.

MEMBER KRESS: You'll never get that.

CHAIRMAN WALLIS: Dendrites or whatever.

MEMBER SHACK: I also think those are local environments. It's difficult to decide what environment you're going to put into the code.

MR. TREGONING: Again, if local effects are important, simulating those again is potentially another challenge. So we've talked about slide 29. I'm not going to go over it. We've talked about some of the limitations and the bases for these codes.

I do just want to mention on slide 30 the approach. There are four different codes they've looked at initially, the OLI code which were used in some of the scoping work done for the ICET program. It was evaluated as well as three other codes and it's actually this EQ3/6 code that at least in some type environments initially has performed the best. So that code has been chosen for subsequent modeling.

Again, I think I mentioned this. Any post test calibration of the ICET results that are done will try to mimic kinetic effects by limiting which phase is predicted by equilibrium are allowed to precipitate.

Also at the Center, they're doing, I think we've alluded, some leechability studies and as Mark had mentioned earlier, the presence of aluminum has a significant effect on leechability of silicon in NUCON fibers and that was initially very puzzling to us because the thermodynamic predicts for test one were predicting quite a bit silicon that we just weren't seeing.

CHAIRMAN WALLIS: How much aluminum do you need to do this?

MR. TREGONING: Mark.

MR. KLASKY: There is some work on it but very limited, 1950s work. So I don't think we know the answer to that question. I don't know what Southwest used in their studies to limit the rate. YO have a figure I believe.

MR. TREGONING: Well, their initial rates were based on essentially the blue curve there.

MR. KLASKY: Right, but that curve, how much aluminum is in that one.

CHAIRMAN WALLIS: Right. There must be a decrease in the curve as you put in more aluminum.

MR. TREGONING: Yeah.

CHAIRMAN WALLIS: Maybe it's very small amount or maybe it's not.

MR. TREGONING: No, the aluminum in here was simulating the ICET one amount.

CHAIRMAN WALLIS: So it's quite a bit.

MR. TREGONING: So yeah, 70 milligrams per liter.

CHAIRMAN WALLIS: Right.

MR. KLASKY: So they didn't look at threshold aluminum in any way. They just tried to mimic the ICET one condition.

MR. LETELLIER: Those initial rates were calculated in isolation without carbonates, oxygen, aluminum or any other contaminants. Is that true?

MR. KLASKY: Yes.

MR. LETELLIER: So it's not surprising that they'd see a variation.

MR. KLASKY: There's a scaling. There's a hydration number that actually -- There's some literature and we can look at that with respect to the leeching rates.

MR. TREGONING: Slide 32, we've alluded to this. There is a number, I'll say, other contributing material potentially that we're not simulating in the ICET tests. We want to understand those materials and effects that we're not including how they're affecting the results or contributing to the byproducts that are formed. This is at a very exploratory stage at this point. We've had discussions and we've had issues which we are looking at considering but we're still in the process of developing plans.

We certainly plan on looking at leechability of codings and specifically on qualified coatings, non-epoxy type coatings which we might expect to have some contribution. That's certainly one area where we could have significant debris that could significantly impact. So the coatings is at the top of the list obviously. But we've also discussed contributions from organic materials where small amounts of concentrations might have an effect which we've talked a little bit about today. You mentioned galvanic effects. That's something that we started thinking about other metal contributions, mercury, lead, tungsten.

CHAIRMAN WALLIS: Would you ever get mercury there? Is there some source of mercury?

MR. LETELLIER: There are accelerometers in the power plants, thermometers, any kind of diagnostics might be sources, but we have asked the question. The plants have not responded.

MEMBER SHACK: They probably have mercury switches.

MR. LETELLIER: Right, mercury switches.

CHAIRMAN WALLIS: Yes, might be.

MR. TREGONING: One of the things we'll be looking that we'll be trying to work with industry on especially in the area of organic materials and metals is to try to identify proper concentrations that would expected in containment again because we know trace amounts may play a role. We want to make sure those amounts if we can are representative. Then we've also talked about production rates. These are some other issues that we're considering.

Again our approach for all of these things is to use a combination of analytical predictions, benchtop testing to try to understand effects first and then decide if we need to do larger scale simulations based on that benchtop work.

Finally, the last slide, the peer review which again as Bruce alluded, we had an initial peer review that disbanded and now we are creating a follow-on peer review group. The objectives are to review the adequacy of all the various NRC-sponsored activities related to chemical effects. We will also ask them to provide a review of the ICET testing as well. So there will be both a review objective and a guidance objective to the peer review group.

The structure, we're looking at between three and five peer reviewers. At least one we expect will be as recommended by industry. So we'll have a lot of experience with certainly industry concerns. We're trying to span required technical specialities and also organizational affiliations. So we're really looking for a very broad group within this three to five member team.

The status of assembling this team, we've made some initial recommendations for who these peer reviewers might be. We've initiated contracts to initiate this work and the plans now are to initiate the peer review and hopefully conduct an initial meeting in August time frame with the expectation that they'll need several months to complete their review. That was it.

I have one more talk but it's up to you if we want to sum. It's everything else we're doing in nonchemical effects areas to try to summarize that in five minutes if you want or we can defer it. It's up to you.

CHAIRMAN WALLIS: Do you want to do it? Why not? Five minutes is fine if it doesn't grow to 50 minutes.

MEMBER SIEBER: Four minutes is even better.

CHAIRMAN WALLIS: Four minutes is better.

Is it something that has a handout or not?

MR. TREGONING: Yes.

CHAIRMAN WALLIS: Yes, it's ongoing. Okay.

MR. TREGONING: It would be the last handout in this packet.

CHAIRMAN WALLIS: This is the colors.

MR. TREGONING: I put up the wrong one.

CHAIRMAN WALLIS: It may take you five minutes to get the right thing on the screen.

MR. TREGONING: I have the right one. These are additional activities that we're doing. We're looking at some additional work to do head loss characterization of BWR containment materials in absence of chemical effect. This is follow-on work from some of the initial calsil work that was done at Los Alamos National Lab.

We have a program to look at improved analytical head loss modeling. We have a program to look at evaluating downstream effects and we currently are, at least we're in the planning stages of possibly initiating some coding transport testing work.

So we'll skip slide three. Again, slide four talks about the head loss characterization. The objective is we really want to focus on calsil, coding some of the standard debris types that we have some head loss information on but we're still trying to gain, I'll say, a phenomenalogical understanding of how those materials really affect head loss. So this is follow-on to a lot of the good scoping work and initial data that's been provided at LANL.

The other thing we're trying with this work is we realize that a lot of the historical data has been developed at relatively high flow rates compared to a lot of the modified screen designs. So we are going to be looking at data within that line of flow rate regime. That's a regime that probably hasn't received as much study historically.

Slide 5, I'm not going to cover. Slide 6, one head loss loop is like another. I feel compelled to show them. I guess the main difference this one and past ones is there's a temperature-controlled component because we are interested in looking at what happens at higher temperatures. We're trying to be much more quantitative than we have in the past in terms of quantifying what's actually in debris beds. So we're looking at sectioning techniques and ways to measure even full field debris bed uniformity using ultrasonic sectioning.

CHAIRMAN WALLIS: You're going to measure the compression more accurately.

MR. TREGONING: That's the intent, yes. Provide a more accurate measurement of that.

CHAIRMAN WALLIS: You're going to figure out if there are strata or thin beds within the bed or whatever it is, the sort of questions that came up before and got partially answered.

MR. TREGONING: We'll be looking at a lot of those questions. So the status of that, we are currently fabricating that loop and we've done a number of small scale loop tests to try to standardize procedures for debris mass, balance cleaning, filtration and instrumentation. The schedule with that testing is we expect the fabrication to be completed in August and the shakedown testing and some of the initial tests to be --

CHAIRMAN WALLIS: And you're going to have sort of a suitable pump. You seemed to be limited previously by the head you could generate from the pump. You couldn't actually -- You had a limit.

MR. TREGONING: That shouldn't be a issue.

CHAIRMAN WALLIS: It shouldn't be an issue. Okay.

MR. TREGONING: Although we are, to be honest, focusing on -- Again, we're looking at the modified screen designs where most of the debris thickness are expected to be relatively small.

CHAIRMAN WALLIS: There also seems to be flow fluctuations previously too. Since it's the same kind of loop, you might get that again.

MR. TREGONING: I'm sorry. Get what?

CHAIRMAN WALLIS: Flow fluctuations.

MR. TREGONING: In terms of bypass?

CHAIRMAN WALLIS: It had flow fluctuations and the flow meter was going like this (Indicating.) and the question was what is the flow through this thing when it's traced like that? Do you remember that?

MR. TREGONING: Yes.

CHAIRMAN WALLIS: Especially a low flow is where the fluctuation is comparable with the flow itself.

MR. TREGONING: There will be some regions where there's no doubt we'll be in transition sort of regime. So that might add some additional, I'll say, noise to some of the measurements that we make.

Let me keep going. The improved analytical head loss modeling, this is work that staff is doing in concert with Bruce and Los Alamos. Really what we're doing is trying to look at not only developing possibly new analytical tools but evaluating some of the historical tools that we've developed as well to make sure that we have a good understanding and can bracket again the type of phenomenology that's occurring for some of this fine particulate loading that you can get for calsil and other debris types.

So we're looking at developing and we have developed in some cases more theoretically based models. And it's something that we want to use. We want to use these theoretical models potentially to provide additional evidence that some of the more engineering based models are either appropriate or not appropriate.

Slide 9 I think I've covered just about all of that. Downstream effects, I want to touch on this a little bit. This again is also work that we're doing at Los Alamos. This is work that's currently ongoing and we're close to wrapping it up at this point.

This has been a two-phase objective. The first phase was to evaluate types of debris that could bypass sump screens. The second stage was to take that type of debris, introduce it in a separate loop and model potential effects on HPSI throttle valve blockage and there's been a surrogate valve that's --

CHAIRMAN WALLIS: You really expect to block the valve. Velocity is pretty high through this valve.

MR. TREGONING: We've demonstrated under conditions --

CHAIRMAN WALLIS: That you can block it.

MR. TREGONING: -- that we can block the valve. The thing is you can experience some flow degradation. Block implies fully blocked. So let me clear about that.

CHAIRMAN WALLIS: The stuff sticks to the wall in the valve.

MR. TREGONING: The types of debris that we're thinking about include reflective metallic foils which --

CHAIRMAN WALLIS: -- which is the valve.

MR. TREGONING: And we've seen with fibrous debris as well that there appears some bridging.

MEMBER KRESS: In the plant, the pumps, if you block up this valve, the pumps will -- at the head and try to push it out. Is your pump going to be able to --

MR. LETELLIER: We certainly replicate

2000 psi systems.

MEMBER KRESS: That's what would worry me.

MR. LETELLIER: But we do have some margin. We can get on-the-pump curve at the onset of injection and if we experience blockage, our pump has 50 to 80 psi of margin to look for scouring effects. So it could be self-cleaning as the velocities increase. It could dislodge the blockage.

MR. TREGONING: You see some of that in the experimental traces. There are definitely time dependencies where you can see blockages form at a certain time and then debris appears to shed. On slide 11, there's a picture of our loop which is actually right next to the ICET chamber. They're all in the big testing lab. Most of the testing has been conducted at 75 gpm although there has been a little bit of testing done at 90 or 95 gpm.

The pressure as Bruce had mentioned, they're not at 2000 psi but they're 400 to 450 psi. It's a surrogate valve configuration and there are different valve stems, seed configurations that you swap out and there are three unique geometries with different contact areas and different seed areas. So I describe what those are there.

There are four phases in this testing.

There were some baseline valve coefficient and SHIM tests. SHIM was trying to demonstrate what would happen for a known nonuniform flow blockage, how that would effect the --

CHAIRMAN WALLIS: Now are you cycling the valve in some way? It's conceivable that if you throttle you might then build up debris because it's a throttle valve and it provides a blockage itself. Then when you try to open it, the debris won't move. You've built it up while it was throttled. If you throttled it down to a low flow rate, you're blocking it at a very low area and stuff could easily pile up while it's --

MR. LETELLIER: What would prevent it from opening?

CHAIRMAN WALLIS: No, once you open it, it won't clear the junk which has built up upstream of the valve.

MR. TREGONING: We've looked at -- These are parametric test and what we've done is we've buried the -- For any given test, there's a prescribed opening height. We've buried that over the course of the test, but there's no cycling that's not clearing any -

CHAIRMAN WALLIS: You might need to cycle it and see if you can build up a blockage and see when you open it up if it clears.

MR. LETELLIER: That was our first objective to see whether it was possible to build up a blockage by any possible mechanism. In fact, even when we challenged the valve with very large loadings, we only see a minimal retention. It is possible that some debris can reside in the valve, but we've never seen a long term accumulation, not to the point of blockage.

MEMBER SIEBER: These are globe valves.

MR. LETELLIER: Primarily, a globe valve design.

MEMBER SIEBER: They sort of resist clearing themselves when you have larger pieces of debris getting through.

MR. LETELLIER: Keep in mind. This is only debris that's able to pass through a sump screen which has a nominal dimension of one-eighth to half an inch square.

MEMBER KRESS: Yes, it's pretty small.

MR. TREGONING: And the other reason we haven't --

MEMBER SIEBER: Well, the valve is big enough that it would go through --

MR. TREGONING: One reason we haven't cycled is after each test the valves opened up and we have differential pressure measurement across the value. So we have at least an indirect indication if we've had blockage. But only through opening the valve do we get any direct evidence if we've had any sort of blockage that occurs. So it is important for these scoping tests to understand that what we're seeing is actually some sort of blockage. I think by design we specifically haven't try to clear the valves in these tests.

We've looked at single debris tests. So single debris types that is on just RMI, just calsil, just NUCON and then we've looked at debris combinations. We've also looked at some series of tests where we allow debris, we pulse debris in and continue to let it try to build up over time.

Quickly, the status on those, the confirmatory testing or the testing itself is complete. We're still analyzing results and reporting the information as we speak. I think Bruce alluded to this first finding. Certainly RMI can cause flow obstructions when the maximum debris dimension is larger than the valve opening. Not surprising.

We have seen correlations between the

increase in flow resistance and the number of pieces of RMI we find in the valve after the test. It's fairly monotonic and a fairly good correlation. We have seen that the largest increase in flow resistance was due to a large amount of NUCON in 100 grams and people always why 100 grams. That was essentially as much as we could fit into the loop, the bypass part of the loop.

We're not saying that that amount of material is representative of what might bypass a screen in the plant. These tests were scoping in nature, parametric, and they looked at various amount of NUCON, probably as small as one gram up to 100 grams to try to understand what the effects were.

CHAIRMAN WALLIS: Presumably if you closed the valve down to 1/20th of an inch and then build up NUCON which was 6 feet of NUCON along the pipe you might have a fear of blowing that out when you open the valve.

MR. LETELLIER: It does take some amount of time to pass, so to speak, that amount of fiberglass. We don't envision any operational scenario where that could occur but it was important for us to see if self-scouring was a potential effect, potential phenomena. It appears that is important.

As the velocity is increased, the scouring effect occurs.

I think one of the more important results is that we have shown that small amounts, small fragments, of debris can reside in throttle valve. The throttle valve was chosen because it represents the minimum size of orifice that's present within the reactor internals but it sort of generically represents fuel channels, spray nozzles, other sorts of valve bodies.

MR. TREGONING: I think the last point I'd like to make with that is we're expecting a report to be completed and available sometime in the September timeframe on this work.

A little bit about coating work and again this is planned. We want to look at the transportability of some of the larger coating surface of the sump screen and understand how the physical characteristics of the chips may affect transportability. The motivation behind this work is we have little testing information about chip transportability in sump environments.

The current SE guidance is pretty conservative in this area and we fully expect the licensees because it's so conservative to make alternative recommendations and analyses to try to reduce that conservative. We would like to have some testing to buttress the analysis that we're going to see from the licensees. So we're looking at studying the effects of all the classic variables, coating density, thickness, flow rate, coating size and shape, looking at evaluating both qualified and unqualified coatings from common coating systems.

The current thinking is that we'd be using the manufacturers to actually apply the coatings and and to create the chips. That way we'd have standardized applied coatings in a standardized way so that that wouldn't affect the results. Again, the manufacturers quite often for their own internal R&D have a good bit of experience in creating chips. Then the testing would be typical flume type testing to investigate how single variable changes under study state flow conditions affect transportability.

CHAIRMAN WALLIS: Now you're evaluating pressure drop through chips on screens.

MR. TREGONING: In the PNNL loop, you've all indicated that after we do some of the initial calsil and NUCON testing we want to look at some chip testing as well. That will provide its own challenges certainly in terms of what distributions we want to look at.

MEMBER SIEBER: You have to resolve the problem as to how well the chips transport from -- to the sump screen, how many come off and what's the purpose going to be because they may not represent a significant enough contribution to the debris that causes the head loss to be worth the testing. I think that varies from plant to plant. So you're going to have to bracket that.

CHAIRMAN WALLIS: If they're fine enough, then presumably they're bound to be transported unless you have a --

MEMBER SIEBER: Yeah, then they go right through the system.

CHAIRMAN WALLIS: Well, it depends on what they find when they get to the screen. There's already this bed of webs and stuff.

MEMBER SIEBER: Oh, what a tangled web we weave.

MR. TREGONING: Again, I think a lot of the coating particulates are not that dissimilar from calsil and we certainly have seen experiences with calsil -- in fibrous debris beds.

MEMBER SIEBER: I think you're right. So you have an opportunity to have another chemical product too.

MR. TREGONING: That's why the leechability will be done separately.

MEMBER SIEBER: Right.

CHAIRMAN WALLIS: It looks like a lot.

MR. TREGONING: Yes, a lot of work. That was all that I had.

CHAIRMAN WALLIS: Thank you. It's going to get very interesting when you start to go from this to some engineering design methods using the information that you've produced.

MEMBER SIEBER: And that's sort of the problem. We've going to the engineering design before we finish the research and testing. To me, that's cart before the horse a little bit. On the other hand, I'm glad to see that the work is going on. I feel confident that we're getting results that I expected to see us get.

CHAIRMAN WALLIS: Yeah.

MEMBER SIEBER: And it's work that has to be done, I think.

CHAIRMAN WALLIS: You have to think about the interface and making all these interesting measurements and observations and how am I going to define some parameters out of all this which are measurable which are useable for design purposes. If they're webs and they are important, how do we characterize them in terms of flow resistance or something. There may be no precedent for how this is done. So you may have to be pretty creative.

MR. TREGONING: Again, I'm a big proponent of a diversified strategy in terms of chemical effects. It's important to understand and be able to predict possible head loss. But I think it's as important to look at other avenues that might get it --

CHAIRMAN WALLIS: It seems to me like it's a real adventure and something like an adventure in the jungle. You don't quite know what terrain you're coming up against or kind of animals you might meet. I wish you well in your endeavors.

MR. TREGONING: I think ICET has provided a good focus in terms of what beasts we're going to be meeting in this jungle. I think that's going to serve us well and informing these future endeavors.

MEMBER SIEBER: There aren't very many tests. So the combinations of tests, all the possible combinations really aren't there. All you can do now is draw some preliminary conclusions and all you've proved so far is there are chemical effects.

MR. TREGONING: But I think --

MEMBER SIEBER: Lacking the tests, you couldn't even say that.

MR. TREGONING: I would agree. I think what ICET is going to be and Bruce might want to jump in here, but what I think it's really going to be valuable for at the end of the day is it's going to help industry and the NRC focus on which variables are most important.

MEMBER SIEBER: Right. I agree with that.

MR. TREGONING: We have a lot of different materials that are in the ICET group. I think for any given test there seems to be one or two materials which seem to be more important than others. Now again, those materials which are more important certainly can vary from test to test.

MEMBER SIEBER: The interesting thing is you've established that you now get chemical reactions that perhaps at times form precipitants or gelatinous substances but you don't know whether has any impact on the flow.

MR. TREGONING: That's right.

MEMBER SIEBER: So until you do that next step, you don't know too much except you know you have to do the next step.

MR. TREGONING: That's correct.

MEMBER SIEBER: But overall I think it's been professionally done. Certainly generates plenty of paper.

MR. LETELLIER: Paid by the pound you know.

MEMBER SIEBER: Yes, I know.

CHAIRMAN WALLIS: Are we through?

MR. TREGONING: Unless you have other questions.

CHAIRMAN WALLIS: Is it time for me to thank everybody for your participation and your very open way in which you answer questions? I think we've had a good discussion.

MEMBER SIEBER: I think the update was important to us so that we know what it is you're doing, what direction you're going, whether or not you're making progress. I heartened to hear that the industry is taking an active role that basically is in coincidence of what the staff is doing through the memorandum of understanding or otherwise. I think that's important too.

CHAIRMAN WALLIS: Maybe there'll be some competitive aspect to see who gets there first.

MEMBER KRESS: I was particularly

interested in the active screen. This may be such an impactable problem going through our observations to a real predictive model that the active screen might circumvent all that.

CHAIRMAN WALLIS: If you have enough power.

MEMBER KRESS: I just say it's important.

CHAIRMAN WALLIS: Right. That's what you'd love to have is some solution that's independent of all the messiness.

MEMBER KRESS: All the uncertainties. But I suspect you guys are going to be faced with multiple solutions probably first off bigger screens and then maybe changes to materials in the plants and probably maybe some active screens, maybe even some mediation process like added chemicals and I don't see how you're going to deal with that yet. If somebody comes up with a way to add another chemical in and say this prevents all these chemical effects.

MEMBER SIEBER: One thousand gallons of nitric acid ought to do it.

CHAIRMAN WALLIS: The active screens sounds good. The stuff that comes off them has to go somewhere.

MEMBER KRESS: It has to go somewhere.

That's part of the design.

CHAIRMAN WALLIS: If you have 75 truck loads of it, that would be a pretty big pile somewhere.

MEMBER KRESS: Yeah, that might be part of the design problem.

CHAIRMAN WALLIS: And you might have to have some place to --

MEMBER KRESS: You may have to have pickup trucks going in.

MR. LETELLIER: I'd be aware of is that the active strainers tend to exacerbate your screen penetration problem. Your downstream effect may become worse.

MEMBER KRESS: It might be --

MR. LETELLIER: There's no longer a filter media in place to capture it on the screen.

MEMBER SIEBER: Yeah, and plus you're reducing the size of the particles in the process of scraping them off. It's going to get more transport.

MEMBER KRESS: That's right.

CHAIRMAN WALLIS: So the hay elevator that takes this stuff and lifts it up and dumps it over a wall into some chamber where it's stagnant and doesn't go anywhere. It's not absurd. You have to put it somewhere where it isn't going to do any harm.

MEMBER KRESS: Get it out of the flow stream.

MR. ANDREYCHEK: Service wire screens.

CHAIRMAN WALLIS: Somewhere, yes.

MR. ANDREYCHEK: Just like a service wire screen. You pick it up and put it somewhere else.

PARTICIPANT: That's not the design. The design --

CHAIRMAN WALLIS: Can we come off the record now? We're just having a general discussion with the audience. I would like to declare the formal part of this meeting closed. Anyone have any objection to that? Thank you all very much. Off the record.

(Whereupon, at 6:04 p.m., the above-entitled matter concluded.)