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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON THERMAL HYDRAULIC PHENOMENA

+ + + + +

RESEARCH ACTIVITIES RELATED TO RESOLUTION OF GSI-191

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TUESDAY,

JUNE 13, 2006

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The subcommittee meeting convened at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, at 8:30 a.m., Graham B. Wallis, Chair, presiding,

MEMBERS PRESENT:

GRAHAM B. WALLIS Chairman

JOHN D. SIEBER

OTTO MAYNARD

RICHARD B. DENNING

THOMAS S. KRESS

ACRS STAFF

RALPH CARUSO Designated Federal Official

NRR STAFF PRESENT:

RALPH ARCHITZL

T.Y. CHANG

MICHELLE EVANS

ERVIN GEIGER

PAUL KLEIN

WILLIAM KROTIUK

SHANLAI LU

MIKE SCOTT

PAULETTE TORRES

ROBERT TREGONING

MATT YODER

ALSO PRESENT:

CARL ENDERLIN

PNNL

ANNE FULLERTON

NSWC

MARC KLASKY

LANL

BRUCE LETELLIER

LANL

BILL SHACK ANL

A-G-E-N-D-A

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

8:32 a.m.

CHAIRMAN WALLIS: The meeting will now come to order. Good morning.

This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Thermal Hydraulic Phenomena.

I am Graham Wallis, Chairman of the Subcommittee. Members in attendance are Tom Kress, Bill Shack, Mario Bonaca, Jack Sieber, Otto Maynard and Rich Denning.

The purpose of this meeting today is to discuss the progress being made and having occurred in the resolution of generic safety issue 191, PWR Sump Performance. Today the Staff will present the results of its research program associated with chemical interactions of coolant and debris within a containment during a loss of coolant accident.

That's all we're going to hear about?

We'll hear a report from the Staff on their continuing review of the industry response to Generic Letter 2004-02. The Subcommittee will hear presentations by and hold discussions with representatives of the NRC Staff and other interested persons regarding these matters.

The Subcommittee will gather information, analyze relevant issues and facts and formulate proposed positions and actions as appropriate for deliberation by the full Committee.

We understand that Dr. Shack has a conflict of interest and will not be participating in the Committee deliberations on this matter.

Ralph Caruso is the Designated Federal official for this meeting.

The rules for participation in today's meeting have been announced as part of the notice previously published in the *Federal Register* on May 22, 2006. A transcript of the meeting is being kept and will be made available as stated in the *Federal Register* notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

We will now proceed with the meeting. And I believe that Michelle Evans of the NRC Staff is going to begin. Please go ahead.

MS. EVANS: Okay. Good morning. My name is Michelle Evans. I'm the Deputy Director for Engineering Research Applications in the Office of Nuclear Regulatory Research.

I'd just like to take a moment to thank the Subcommittee for the opportunity to be here today to continue the dialogue regarding the work that we've done to support NRR in their review of the PWR sump issues.

We last updated the Subcommittee in February of this year. And the intent today is to continue the dialogue and provide information regarding the sump related research that has been completed since that time.

I'd like to introduce Rob Tregoning. He's the group lead in our office for the sump related research.

Rob?

MR. TREGONING: Thank you, Michelle.

I wanted to echo her sentiments and just thank the Committee for taking time out and allowing us to present the results of our research to provide you an update of activities that have gone on since our last chance in front of you in February and in March of this year.

As Michelle mentioned, I'm Rob Tregoning from the Office of Research. I want to briefly here to kick us off, provide an overview presentation. And there's not much technical in this presentation, but

what it's meant to do is to provide a framework for the very technical presentations that you're going to be hearing over the course of the day. So really just provide an overview, let you show how the various pieces of these various research facets really fit together into a global scheme.

So the global objectives of the presentations you're going to be hearing today, they're all going to follow a very similar format and try to present similar information, albeit on disparate technical topics. You'll be hearing about the motivation, the objectives and goals for the research initiatives that are supporting the Generic Letter resolution.

Here I talk about the Generic Letter instead of GSI-191 because that is the major regulatory activity that licensees are trying to satisfy as we move forward here. It's certainly an integral part of GSI-191, but that's why I'm normally referring to the General Letter here meant as a euphemism for GSI-191 as a whole.

The second objective will be to provide an overview of the associated technical areas of research and discuss how these programs fit together. I'll be doing that in this talk.

For talks that you heard in February, what you'll hear today it'll essentially be a status report for those research programs. They'll outline their objective, motivation intended regulatory use. They will briefly describe the technical approach, although we've tried to be brief in these areas since in many cases you've heard this information already. And what we really want to do is focus on the salient results that we've achieved since February. So the bulk of those presentations that you've heard already will be summarizing those important results and observations and analyses that have occurred since the February time frame. And then we'll also discuss plans and schedules for remaining work.

CHAIRMAN WALLIS: Is there going to be any remaining work? We've heard many times that all this is supposed to finish in spring 2006.

MR. TREGONING: There's remaining work associated with each of these programs in terms of reporting and analysis, in some cases. And you are going to hear about some programs specifically in the area of downstream effects which have been initiated since the February meeting. The downstream effects presentation will not be held today. That's the only research related talk that will be held, actually,

tomorrow. But that is one activity where we do have ongoing efforts.

CHAIRMAN WALLIS: But the other work is essentially being wrapped up and it's just a question of finishing the reports, is that right?

MR. TREGONING: At this point the programs that you're hearing today or that you'll be hearing about today, most of the active testing has been completed in those programs. And we're finishing reporting and analysis and at this point trying to do assess where we go next.

MEMBER DENNING: I'd like to pursue a little bit this question of GSI-191 versus Generic Letter.

MR. TREGONING: Yes.

MEMBER DENNING: In terms of the question of when does GSI-191, when will it be brought to resolution versus this response to the Generic Letter?

CHAIRMAN WALLIS: Yes. I hesitate to tackle that question given that the responsibility for GSI-191 rests within NRR. I know they have a schedule associated with closure. So Mike Scott's going to come up and illuminate us on what the schedule is, I believe.

MR. SCOTT: Good morning. This is Mike

Scott with NRR.

We have a schedule, as Rob mentioned, for resolution of the Generic Letter first and resolution of the Generic Letter is one of the activities that supports resolution of the Generic Safety Issue. Because some of the plant full responses are not going to be coming in until early in 2008, we're currently planning that the GSI would be closed if the information is available to support that closure in mid-2008.

MEMBER DENNING: Thank you.

MR. TREGONING: But just as a matter of process, the Generic Letter would be closed and resolved first as one of the integral steps to closing GSI-191.

The fourth objective is there are a few programs that we'll be talking about over the course of the day that we did not discuss in February. So those presentations will be more complete self-contained presentations where we go through the approach objectives in greater detail.

The philosophy, I'll just quickly touch on this. I covered this in February. This slide's just up here to remind the Committee what we discussed in February.

The motivation for all these activities we certainly recognize that research was necessary in a number of important technical areas to acquire knowledge so that we could adequately resolve this Generic Letter.

CHAIRMAN WALLIS: Well, I'm not sure that you've reduced uncertainty. I thought the last report you gave us seemed to indicate that you had increased uncertainty. You had raised new questions. You've shown that you get a bigger spread in data than we'd seen before. And so it's probably not quite right to say that research is reducing uncertainty. It's giving you more knowledge.

MR. TREGONING: Right.

CHAIRMAN WALLIS: And that doesn't always reduce your uncertainty.

MR. TREGONING: Well, if you look at the wording there it was very carefully done. It was to reduce uncertainty associated with the resolution. And I would argue the more knowledge that you have, the better you're able to resolve issues.

CHAIRMAN WALLIS: Well --

MR. TREGONING: Regardless of it, the knowledge that you gain may increase the amount of uncertainty that you have with respect to a given

technical parameter. But I agree with your point.

CHAIRMAN WALLIS: And if you believe a correlation and then you do some work which shows that it's invalid, it would seem that you would increase the uncertainty.

MR. TREGONING: It would increase the uncertainty with using that correlation, certainly. That's correct.

CHAIRMAN WALLIS: Right. Okay. Well, we can go on.

MEMBER KRESS: The motivation for doing the work remains the same.

MR. TREGONING: Yes.

MEMBER KRESS: It's just --

MR. TREGONING: Well, the phenomena that we've investigated it's certainly been qualitatively understood for quite some time. I think going back to the LANL work and previous effective debris sequencing was a well known phenomena. I think we've quantified it in a way that had not been done in any sort of rigorous manner prior to that. So, that would be the distinction I would like to make, yes.

CHAIRMAN WALLIS: Important work.

MR. TREGONING: Again, the broad objective of this work, we've tried to focus on technical areas

that initially did have the highest uncertainty. Although I hesitate using the word "uncertainty" again at this point. And we've tried to take direction from the ACRS Staff and industry to identify areas where generic evaluation and research sponsored activities we thought would have the most impact. So that's been an objective behind all of the various activities we've conducted.

We discussed this in February but it's worth highlighting. Many of our studies are parametric or scoping in nature at this point to evaluate important variables over ranges of representative conditions.

CHAIRMAN WALLIS: Now that doesn't tell me that you arrive at any method for predicting anything. You're just going to evaluate important variables? You're not going to be able to reach a conclusion about predicting performance?

MR. TREGONING: You know, the second bullet is a necessary step I would argue in laying the basis of the groundwork behind the --

CHAIRMAN WALLIS: Right. But my impression is that it's going to be left to industry to predict performance. You are just looking at important variables and you're saying "Yes, we found

out that this has an important effect and this has an important effect, and this has an important effect and this other thing is rather unimportant." But you're not going to pursue it to the point where you say now we have a predictive tool

MR. TREGONING: That's largely correct, although we are doing work in correlation development where that's the goal. However, the correlation development work has mainly been targeted as a tool for Staff to use for confirmatory purposes and not for the industry to use for the solving the problem. So you're largely correct in your analysis.

The goals, and this just amplifies the point that you just made, Dr. Wallis, is that most of this research the goal has been to conduct confirmatory research for the Staff use in making sure that the independent review and assessment of the licensee's submittals for this Generic Letter are technically acceptable. However, even though that's been a primary goal, certainly all the results of our research we've strived to make them publicly available as quickly as possible so that the industry and other stakeholders could use them in developing their solutions as they move forward.

Technical area of studies on the next

slide, there's four main areas of study. And what I've highlighted here are programs underneath that that we discussed in February.

CHAIRMAN WALLIS: But what are you confirming in this confirmatory research?

MR. TREGONING: We are confirming that the path that industry chooses or the technical resolution that they propose to use will be is that we can confirm that it's technically acceptable and that we have independent testing and analysis to buttress any claims that the industry --

CHAIRMAN WALLIS: But I'm telling you you're doing that before you even know what they're going to send you?

MR. TREGONING: It's true that we haven't seen final submittals with respect to the Generic Letter. But we've certainly seen lots of preliminary information from the industry.

And, Paul, did you want to -- he's coming up here. I didn't know if you wanted to add something at this point.

MR. KLEIN: Yes. I'm Paul Klein from NRR.

One of the things you'll hear from me tomorrow, Dr. Wallis, is that part of NRR's approach will be to step back, look at all the available

information and try to make a determination with research on whether additional testing might be necessary. And part of that input will be trying to evaluate industry's approach.

CHAIRMAN WALLIS: You know, from looking at the workshop in May that just occurred two or three week ago, it appeared that NRR had learned a lot of the questions to ask from this research. And that seemed to be one of the main roles of this research was to establish which technical questions need to be asked and need to be responded to by industry.

MR. TREGONING: Yes, that has been a contribution.

CHAIRMAN WALLIS: Okay.

MR. TREGONING: So in the area of chemical effects we discussed in February, and then previously in July of last year we talked in great depth about the integrated chemical effects test that was conducted by Los Alamos National Lab. We also heard about some chemical specification prediction work that was done at CNWRA.

In February in head loss we talked about chemical effects head loss testing at Argonne, and in particulate head loss testing at PNNL. The big distinction between the two is that Argonne is truly

focusing on chemical effects where PNNL is largely focusing on standard particulate and fibrous debris.

In February in downstream effects you heard about the sump screen pass-through of debris, otherwise known sometimes as screen bypass as well as HPSI throttle valve performance work that was conducted at LANL. And then you heard the beginnings of the coating transport work being conducted at NSWC.

Now this time in May it's going to be a slightly different lineup. We don't have any presentations planned on ICET, per se.

CHAIRMAN WALLIS: Could I go back to this --

MR. TREGONING: Okay.

CHAIRMAN WALLIS: -- effects of ingested debris on reactor core cooling?

MR. TREGONING: Yes.

CHAIRMAN WALLIS: I wasn't aware that NRC was doing any work on that.

MR. TREGONING: We weren't in February. SO this is something, this is --

CHAIRMAN WALLIS: But you are now?

MR. TREGONING: But we are now.

CHAIRMAN WALLIS: Are we going to hear about that?

MR. TREGONING: Yes. I won't be covering it today, but it will be covered tomorrow as part of the downstream effects.

CHAIRMAN WALLIS: We'll hear about it tomorrow?

MR. TREGONING: You will hear about that. That's the only research activity that we will not be covering today specifically.

CHAIRMAN WALLIS: Thank you.

MR. TREGONING: So today we will not be hearing about ICET or the chemical specification prediction work. There has not been a lot of new activity, or at least enough new activity in either of those areas to warrant presentations. However, as always, if you have questions we'll be happy to field those.

What we will be discussing in the area of chemical effects is some ICET follow on characterization work that has been conducted at LANL. You've heard a little about this in July as well as in February. But we want to give a full blown treatise today.

And then you'll also be hearing about our peer review activities in the area of chemical effects.

Now in the area of head loss you will hear updates on the ANL and PNNL work. There's additional testing and analysis work that will be presented today.

There's also a new program that we only touched on in February, some static drain column testing work conducting at LANL that's going to provide, I think, some very interesting complimentary information to be coupled with the work that's ongoing at ANL. It's a different philosophy and approach to head loss testing than ANL has embarked on.

And then in downstream effects, as Dr. Wallis mentioned, tomorrow you'll be hearing about the effects of ingested debris on core coolability.

And then also today you'll hear some results associated with the coatings transport work.

MEMBER DENNING: Rob, before you move on to that, it's pretty clear that the Staff's approach towards the resolution of this issue is going to rely very heavily on large integral tests that have conditions that are clagged together glugged together, in a sense, to include the effects of chemical effects. And I was wondering whether you had looked at -- and it's going to be very difficult to assess whether these really represent reality or whether they

don't. Have you considered the possibility of some additional testing that would try to look at that these integral tests in an experimental way to try to determine where the ranges of applicability or are you going to rely totally on the work that's already been done?

MR. TREGONING: We've certainly discussed among the Staff, as anything, the possibility of doing some, say, larger scale integrated confirmatory types of tests. It's something that we haven't chosen to proceed down that road at this point because all the issues associated with those integral tests have not shaken out.

Whenever you embark on integrated testing, you know some of the concerns that you've raised with respect to the industry testing, we would have the similar concerns if we embarked on our own program making sure that they reasonably representative and that we were understanding all the various integrated effects that were occurring in the test.

So we've chosen more typically to look at single effect; transportability, head loss, chemical effects with the hopes that we can integrate those in a little bit more rigorous way by understanding the pieces. Now that doesn't mean downstream if there is

some compelling issues that we're just not able to come agreement or, again, provide a rigorous technical evaluation of some of the outstanding questions that we have on the industry approach, that we wouldn't revisit that decision again. But as of now, we have no plans to do any integrated testing along those lines that you mentioned.

CHAIRMAN WALLIS: Well, the sort of questions I have about industry, just to show some out: (1) There seems to be an assumption that all concretes are the same. We know that concretes can be quite different depending on what aggregate goes into them and so on.

And there seems to be an assumption that the precipitate of some chemical always has the same physical properties, whereas we know that precipitates can be very different depending on how they're formed.

So there's a whole lot of questions like that which much be occurring to you guys.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: We can have a discussion about that later on about some of these things which are sort of being assumed or appear to be being assumed by industry which maybe you need to do some confirmatory work to check on.

MR. TREGONING: Tomorrow at the NRR presentations that is some slides that they'll be discussing about the integrated testing and some of the issues we've raised.

The issues you raised are certainly a subset of many issues --

CHAIRMAN WALLIS: For example, I guess.

MR. TREGONING: Yes. Many questions that we have.

I know in August there is at least a plan at this point to bring in or to attempt to have the vendors come in so that we can have a more detailed discussion in front of this Committee in terms of the various testing approaches that are being undertaken at this point.

Can I move on?

CHAIRMAN WALLIS: Sure. We've seen this one before.

MR. TREGONING: You've seen this one before, so I'll move right through it. It's just essentially I wanted to show that there's been a lot of collaboration between the various labs that we have working on this. It's a true consortium. We've tried to go to many labs so that we can conduct many programs simultaneously and try to achieve the benefit

of a large number of people thinking about these issues simultaneously.

These messages there, I've essentially one method associated with each presentations that you're going to hear subsequently. There's not a lot of technical information given in these messages, but they're just there to set the stage for what you're going to be hearing about and to try to provide a bit of a take home set of summary points.

You're going to be hearing in the first presentation today that we have achieved some in depth technical understanding about aluminum precipitate formation. And this was the precipitate, just to remind you that we saw in the ICET 1 and 5 tests within containment pool environments. So I think we know a lot at this point of how these aluminum products conform and what some of the conditions of formation are.

In the next talk, which is the ANL head loss talk, we have certainly demonstrated some very important implications associated with observed ICET products. And we've actually measured head loss associated with surrogates intended to mimic those products that we observed in ICET.

The new talk on peer review I think you're

going to hear a number of interesting issues that we haven't discussed previously in front of the Committee. And we're using this external peer review quite extensively to not only review and comment on the research that we have ongoing, but really to help us identify and priortize outstanding issues in the chemical effects arena.

Finally, when we look at the PNNL work and the head loss correlation development work you'll see once again that we're continuing to identify and quantify important phenomena that effect head loss for particulate and fibrous debris systems. And we'll show you how we're using the test data as well as developing correlations so that we can try to provide an analytical tool that the NRC Staff will have to evaluate some of the solutions that industry is development that ensure that the head loss will be acceptable, for some of these engineered some modifications.

Finally, not so much today but tomorrow you'll hear about research that we've initiated to determine if adequate core cooling is maintained due to ingested debris within the reactor core.

And then also today you'll be hearing that the coating testing has qualified several important

metrics for assessing chip transport to the sump screen. And also you'll see some results from PNNL where they actually made some head loss measurements of coating chips. And that was an area that hadn't seen much, if any, data to try to make that assessment. So there'll be some valuable new data there.

CHAIRMAN WALLIS: Rob, last time we meet, before we met we received some reports. These very quick look sort of reports, but at least they gave us technical information. And I was thinking about how we're going to evaluate this work or write any letter on it. Really until we see a report which has information in it which you finally are going to hang your hat on, it's very hard for us to evaluate what you've done.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: And we can sort of look at what you present and say, yes, things look okay and this thing is an interesting new twist or something, but we can't give any really definite statement about how good we think the work is until we see the final product. And that is going to be sometime in the future, isn't it?

MR. TREGONING: Yes. I think what you'll

see with each project starting in August from about August through October we'll probably have one or two new reg. reports a month coming out on these various projects.

CHAIRMAN WALLIS: So how are we going to advise the Commission about the usefulness of your work until we get all that stuff? We won't be able to do it until, say, next year it looks like.

MR. TREGONING: I think the plans are, and Ralph can elaborate on this a little bit, we plan for a November Subcommittee meeting to be followed up with a main Committee meeting in December. And the strategy at that time was, or the plan is that all of the reports will be done by that. So you'll have six or seven reports.

CHAIRMAN WALLIS: So November with the time when we can review this great stack of information?

MR. TREGONING: Yes. Yes. We realize that it's going to be a challenge.

CHAIRMAN WALLIS: Well, I'm interested in the timeliness here. Because I think that these plants are putting in screens now. And by next year they're going to have them in there. And so if NRR is going to use your work, it needs to come out presumably pretty

quickly.

MR. TREGONING: That's what we're working to do. That's why we're working--

CHAIRMAN WALLIS: They can't really base a decision on some preliminary indication that you have of something. You need to actually state something definitely. Okay.

MR. TREGONING: That's our main activity this summer is reporting and publishing. So over the next several months there'll be a number of reports that are coming out on these various programs.

CHAIRMAN WALLIS: Will these be selective? It's nice to see quick look reports because people then say what they saw. It's a bit like evidence in a legal case. You know, the evidence is the most valuable. When people think about what they really want to say, sometimes it changes. We do sort of see the quick look type report as well as the final report and say "Well, why don't we emphasize this part of the work or something?"

MR. TREGONING: I can tell you the strategy we're pursuing with all of our work:

A. We're trying to present the information as neutrally as we can, and;

B. For instance, with the PNNL report I

know there are plans to incorporate all of those quick look reports as appendices.

CHAIRMAN WALLIS: Okay. So everything is going to be there?

MR. TREGONING: And that's the strategy we're attempting to pursue with all of the reports is that there will be data and more raw information that will be in the appendices.

CHAIRMAN WALLIS: Wonderful. Okay.

MR. TREGONING: So for those that are inclined, there'll be plenty of information to sift through and dig into and really get your teeth around.

Any other questions? I'm accused of being wordy, so once again I've exceeded my time allotment so I don't want to dally any longer and let the real technical stars get up here and talk about what they've been doing.

CHAIRMAN WALLIS: Well, you may be wordy, but you don't usually say much that's irrelevant.

MR. TREGONING: That's a matter of interpretation, I think.

Okay. Next we're going to hear about aluminum chemistry in a post-LOCA environments and T.Y. Chang, who is the NRC Project Manager, is going to come to come up to lead this talk off and as well

as Marc Klasky.

MR. CHANG: Good morning. My name is T.Y. Chang. I'm a Project Manager in the NRC Office of Research.

With me here is Dr. Marc Klasky from Los Alamos National Lab.

We would like to present to you a subject, the title is Aluminum Chemistry in Post-LOCA Environments, which I should add that the environment we're talking about is within the PWR containment.

A little bit of background. The chemical effects issue was raised by the ACRS about three years ago. And in response to this raised concern, the NRC initiated a small scale chemical testing program. This study demonstrated that gelatinous chemical precipitate could indeed be formed when introducing aluminum or zinc salt into a simulated PWR containment pool solution.

CHAIRMAN WALLIS: I was trying to think about the history of this. There were some small scale tests, I think they were done at Los Alamos or New Mexico where --

MR. CHANG: That's the one I'm referring to.

CHAIRMAN WALLIS: -- they had trouble

getting consistent results. I mean, they put a lot of things in a lot of jars with various pHs and things and it was resolved it was zinc or something, wasn't it?

MR. CHANG: The zinc --

CHAIRMAN WALLIS: It wasn't aluminum, it was zinc.

MR. CHANG: Aluminum and zinc both.

CHAIRMAN WALLIS: Getting consistent results. And therefore, I wondered how --

MR. CHANG: The missing link there is from the metal corrosion to the formation of the precipitate.

CHAIRMAN WALLIS: And there was something, it wasn't even consistent. I mean, they'd sort of get sometimes the -- they'd put in these, what did they call them? Little pieces of metal. I can't think of what they call them.

MR. CHANG: Coupon or whatever.

CHAIRMAN WALLIS: Coupons, right.

MR. TREGONING: Dr. Wallis --

CHAIRMAN WALLIS: And then sometimes they gain weight, sometimes they'd lose weight and so on. It was very difficult to get consistent results. And this was telling me something about the difficulty of

doing a good small scale test because industry is now planning a whole lot of small scale tests, right? And they're going to take the results of these and apply them. They'll probably learn from the small scale chemical tests that it was difficult to get consistent results. That's what I remember from that work.

DR. LETELLIER: This is Bruce Letellier from Los Alamos National Lab.

I think that your memory is correct. When we tried to perform a small scale corrosion test to look at how rapidly the materials introduced to solution, the results were rather inconsistent. It depended a lot on the physical property of the sample, whether it was a coupon or granular material. That was one of two objectives for the small scale testing.

The first objective, as T.Y. had mentioned, was to demonstrate or to investigate whether or not chemical products could induce an important head loss effect. And that was demonstrated conclusively in repeated studies.

MR. CHANG: Yes. Let me continue.

CHAIRMAN WALLIS: But in terms of this confirmatory research, I mean industry's got a huge plan where they seemed to do all of independent tests of different coupons and stuff. And what you've

learned is something about how one can do those tests and how one can interpret the results and how reliable it is to extrapolate them and so on. That may be useful information when you evaluating what the industry is doing.

MR. CHANG: Yes. Certainly they are useful information.

That study, that the main finding is that those gelatinous chemical precipitates and induce pretty high head loss across a sump screen.

As I mentioned, that the link from the metal corrosion to the precipitate formation, however, was not demonstrated in this testing. To address this missing link NRC and the industry started a joint program. It was the integrated chemical effects test. In short it's ICET.

This ACRS Subcommittee was briefed by the staff and our contractors on the ICET test twice. Once was in July last year and again in February of this year.

The ICET tests show that this chemical precipitate could form under certain conditions when corrosion and leaching of various materials happened. Those various materials tested in the ICET tank are things such as different insulation materials,

concrete and exposed metal surfaces.

CHAIRMAN WALLIS: Now these precipitates were sometimes web-like?

MR. CHANG: Pardon?

CHAIRMAN WALLIS: They were web-like? Looked like a spider's web? And so this indicates that the precipitate isn't necessarily always a collection of small particles which seem to be being produced in some of these industrial tests?

DR. KLASKY: You want me?

MR. CHANG: Yes.

DR. KLASKY: Dr. Wallis, Marc Klasky from LANL.

I'll speak in a minute about some of the earlier photographs that I think we've presented in the past with respect to the web-like growths. And I'd just summarize by saying that some of those pictures that you might have seen perhaps are just manifestations of the drying process.

CHAIRMAN WALLIS: That's right.

DR. KLASKY: And so I'll talk about that in a minute.

CHAIRMAN WALLIS: Okay. Yes, but the point is that precipitates appear in many forms depending on what you do?

DR. KLASKY: Correct.

MR. CHANG: Correct.

Well, based on our observation of the ICET tests it becomes clear that a better understanding is needed about what kind of condition that aluminum based precipitate can form. And also -- I think I skipped one slide. Let me see. Okay.

What really motivated us to start this study is the ICET test results. Looking at this table we have three ICET tests --

CHAIRMAN WALLIS: Well, let's go back to here. You say temperature and pH. But the form of the precipitate depends on other things such as whether or not you stir things, whether or not this flow of some sort through something else or around the precipitate and so on. It's not just temperature and pH.

MR. CHANG: Well --

CHAIRMAN WALLIS: Yes, you can make precipitate but how it forms and what it looks like depends on a whole lot of things, doesn't it?

MR. CHANG: That's correct. There are a lot of other things. But we think those are the main things that will influence formation of those precipitate.

CHAIRMAN WALLIS: I mean, you can make a

single crystal of something if you do it very, very, very carefully.

MR. CHANG: Correct. Yes.

CHAIRMAN WALLIS: But that's not what you usually get for a precipitate. It's not likely you get one large rock in the sump.

MR. CHANG: Okay.

CHAIRMAN WALLIS: It's conceivable, right?

MR. CHANG: Here we have three of the five tests performed for the ICET. If you look at test number 1 and 4 both have sodium hydroxide as the buffering agent and the pH value of the solution about the same time. However, pretty different results were observed.

For test 1 quite a bit of the aluminum-based precipitate was formed upon cooling. Whereas for test 4 none of those gelatinous precipitate were observed during the test or after cooling.

In addition, you look at test 5. The buffering agent was the sodium tetraborate STB and the pH value was about 8. However, we also observed aluminum-based precipitate formation.

The notable difference between tests 1 and 4 really is the insulation material used. Test 1 used 100 percent fiberglass and test 4 we have 80 percent

of the calcium silicate there and only 20 percent of fiberglass.

so the question then is why do we see such different results from the test? This is something we need to know better.

CHAIRMAN WALLIS: This is something that unless you've done the tests, you wouldn't know. No. No one predicted this would happen this way, as far as I know.

So this shows that you cannot do a test where you simply look at NaOH and aluminum. It depends on the environment of other things as well?

MR. CHANG: That's correct.

CHAIRMAN WALLIS: Which is another message, I think, where some of these small scale tests -- what are you going to put in the small scale tests in order to simulate the environment? Because the small scale test doesn't contain everything that's in the sump. They're sort of separate small scale tests, I think are being planned by --

MR. CHANG: Well, the test actually is a evolution. We started with the small tests --

CHAIRMAN WALLIS: I'm talking about ICET. I'm talking about you learned from this that you can then apply to your analysis of what the industry's

doing. I think they have a whole lot of small scale tests. I mean, many, up to an order of a 100. But I don't think they've put in different insulation, for instance, in each test to see what effect it might have. So unless you happen to do those tests, you wouldn't know this?

MR. CHANG: That's correct.

CHAIRMAN WALLIS: Okay. So how much CalSil do you need? Would 2 percent CalSil have the same effect?

MR. CHANG: For test 4, 80 percent of the insulation was CalSil. But I don't --

CHAIRMAN WALLIS: Suppose it was 2 percent, would it inhibit the aluminum precipitate?

DR. KLASKY: Dr. Wallis, I think I'll touch on those.

CHAIRMAN WALLIS: You're going to talk about this later?

DR. KLASKY: Yes. Yes.

CHAIRMAN WALLIS: Okay. Thank you.

DR. KLASKY: And I think your point with respect to the presence of sort of, let's say, them hidden variables --

CHAIRMAN WALLIS: Right.

DR. KLASKY: -- is valid. And I think

that that is precisely why we're attempting to really formulate a greater understanding. We only have five tests and yet there are many different conditions that may exist. And so I think one has to be cognizant of that fact and try to really extract more information from these five tests than otherwise would be, you know, just laid out on the table.

CHAIRMAN WALLIS: Yes, but you've learned something about the fact that unexpected things happen. That something like insulation can effect what you thought was the primary reaction between NaOHs and aluminum,

DR. KLASKY: Correct. So there are --

CHAIRMAN WALLIS: And that you cannot sort of lightly go induce a whole lot of separate effects tests and assume that when you put it all together, you'd get the same result?

DR. KLASKY: Correct.

MR. CHANG: Based on the observation of the ICET tests, then it becomes clear a better understanding of the condition is for the formation of aluminum-based precipitate is needed. And also, we need to understand better about the properties of those precipitate.

The regulatory application is for the

results of this test -- of this study, rather, is first linked to support NRR evaluation of the licensee responses to Generic Letter 2004-02 in the area of chemical effects. And in addition, the results will support NRR plant audits for evaluating chemical effects.

Finally, for the licensees this will provide information for them to perform plant specific assessment of chemical effects in the post-LOCA chemical environment.

The product of this study will be in the form of a NUREG/CR report. The report, we're in the progress and the publication is expected in October this year.

And with this, I would like to hand it over to Dr. Marc Klasky. He will finish the presentation of this study.

DR. KLASKY: Okay. Thank you, T.Y.

I want to point out another collaborator, Don Chen from University of New Mexico who has assisted in the work.

I think there were a couple of points made, Dr. Wallis, that really sort of are the motivation for this work. And those are that we want to recognize that we've done a limited number of tests, but at the same time we have conditions that

either are interpolations or extrapolations, in some sense, to the conditions that were present in these five tests. So we're attempting to understand the maximum quantity of precipitate, it's susceptibility to change with respect to its properties due to any number of factors that may be present and at the same time really prepare for the subsequent head loss test. So in the end we're after an understanding of quantity of material and its inherent properties that might effect the head loss. That's ultimately the goal.

CHAIRMAN WALLIS: Were you able to predict anything that happens in the sump?

DR. KLASKY: I think the objective of this work is to predict or attempt to predict the quantity of precipitant and its properties --

CHAIRMAN WALLIS: Are you able? I mean, do you end up with something which says yes you can use this and we understand it and you can make a prediction?

DR. KLASKY: I think what we wind up with is a prediction of the properties of the precipitant that could be used to develop a head loss correlation that Bruce, my colleague at Los Alamos, will present.

An outline of the technical presentation, we're going to summarize of the ICET tests in which

we've observed aluminum concentrations that were substantial enough to warrant further investigation. Present some of the measurements that have been conducted, hydrogen production, properties of the precipitant and talk about the passivation of the aluminum coupons, in particular, and some supplemental tests that has been performed to understand the quantity or source term that we might be faced with. And talk about some follow-up analyses that have been performed to characterize the precipitant.

Again, the goal is to characterize it in the sense that we can reproduce it and understand its susceptibility to changes in different parameters. Mixing, for example, or rapid addition of sodium hydroxide to a solution of aluminum.

Then we want to talk about aluminum solubility, what factors affect aluminum solubility. And this is important again because we're trying to predict the quantity and the properties of the precipitate. And we're going to talk about h ow those properties might change as a consequence of aging, for example.

And then lastly talk about the development of surrogates for head loss testing to facilitate head loss testing if you want to do that separate and not

in an integrated loop where you're corroding the metal and then measuring the head loss that results from that chemical product, which in fact what Dr. Shack has performed at Argonne.

We'll talk about some of the difficulties and the variability in the properties of the material.

But first I'll just summarize some of the basic observations from ICET. On the left we see test 1 we basically produced substantial quantities of aluminum or corroded aluminum such that the concentration in test 1 rose to a plateau value of about 350 milligrams per liter over the course of the first 20 days. And subsequently it plateau.

CHAIRMAN WALLIS: This was all in solution?

DR. KLASKY: This was in solution, correct. This is the measured concentration performed by taking daily samples from the ICET solution of test 1.

Test 5, which again was a sodium hydroxide system. Actually, we used sodium tetraborate to simulate, I guess this was the ICET-- or sorry, the ICET condenser simulation or attempt to simulate that environment. We obtained concentration of aluminum of

about 50 milrems per liter.

CHAIRMAN WALLIS: This is aluminum in solution. It's not aluminum that comes out and then reacts with something else and precipitates during the test?

DR. KLASKY: Right. Right here we're just presenting the concentration of --

CHAIRMAN WALLIS: Do you know if there's any aluminum that reacts with something else and then precipitates during the test or is it all in solution.

DR. KLASKY: I'll address that.

CHAIRMAN WALLIS: You'll address that later?

DR. KLASKY: I think we have pretty convincing evidence that the aluminum that was present or that had corroded remained in solution during the course of the experiment. And that's confirmed by the fact that the weight loss from the aluminum coupons was largely consistent with the concentration that we observed. And I'll present it on the next slide, some other evidence to that effect.

Just to illustrate a point that I think you made, Graham, with respect to pH and it's not as simple as just, you know, a single variable. I have plotted the pH of each of the respective tests. And

the upper most curve is in fact that of test 4 in which the pH is the highest. And as we alluded to, or T.Y. alluded to, we did not observe the aluminum placed in solution.

CHAIRMAN WALLIS: So you've got two things at the same time in test 4. You've got CalSil there and you've also got a high pH, is that right? No, you've also got a high pH in test 1?

DR. KLASKY: Correct. So the difference in --

CHAIRMAN WALLIS: It doesn't show -- oh, it's almost the same. Yes. Okay. Yes.

DR. KLASKY: It's a little higher, but they're comparable. But as T.Y. alluded to the difference being that CalSil is in test 4 and so one can basically examine, you know, so what's different in test 4 and what led to, in essence, the lack of aluminum. And that's what we've pursued.

Likewise, test 5 is the blue curve. It has a pH that's comparable to that of test 3 yet there was no aluminum placed in solution in test 3. And there we're talking about a trisodium system, borate system versus sodium hydroxide system.

So I think it's clear that we're talking about more than pH here. We're talking about different

chemical species that behave in a manner which passivate the aluminum surface, others the chemistry is just so substantially different that we have different effects that are taking place.

This just summarizes the five tests and just simplistically whether sodium hydroxide or TSP was present, the pH range and whether fiberglass or CalSil was utilized.

I spoke to this point earlier, and that is the question was asked whether the aluminum might have precipitated in test 5 and test 1. And there's I think some substantiation to the effect that we believe that the aluminum did not precipitate in either of these tests during the course of the test. Rather remained in solution. One piece of evidence is the hydrogen concentration.

We measured the hydrogen above in the head space of the tank. The tank was vented. And just as a safety precaution, hydrogen was monitored each day. And there's substantial variability in the data, but --

CHAIRMAN WALLIS: Turbidity is the opaqueness of the --

DR. KLASKY: Yes.

CHAIRMAN WALLIS: So what is it if it's

not a precipitate that's making it turbid?

DR. KLASKY: Well, let me speak to that. This measurement was performed by allowing the solution to cool.

CHAIRMAN WALLIS: Ah.

DR. KLASKY: Right. This is indicative of turbidity in the tank itself.

The point of this figure, the lower figure, is that basically it reached the value which saturated.

CHAIRMAN WALLIS: So there was no turbidity during the test? It's just after you --

DR. KLASKY: Right.

CHAIRMAN WALLIS: -- lie around for many days it --

DR. KLASKY: Well, the sample was withdrawn from the --

CHAIRMAN WALLIS: By different, the days is during the ICET test, that time span?

DR. KLASKY: Correct.

CHAIRMAN WALLIS: Okay.

DR. KLASKY: Yes. So the sample's withdrawn --

CHAIRMAN WALLIS: And then cooled?

DR. KLASKY: It's allow to cool to reach

room temperature. And the turbidity here is illustrative of the fact that you're basically precipitating. And that's in essence what's occurring here. So the passage of light is impeded or you're reflecting light.

MR. TREGONING: Marc, this is Rob Tregoning. Just a quick point of clarification.

Those turbidity measurements, although they are taken at room temperature, they're taken within ten minutes of extraction from the tank. So the samples don't sit around for any great length of time.

CHAIRMAN WALLIS: They don't settle?

MR. TREGONING: What?

CHAIRMAN WALLIS: They don't settle, for instance? They don't settle?

MR. TREGONING: And Bruce might elaborate here. But I believe, you know, that they shook the vials up so that they could homogenize any precipitate that it formed, yes.

DR. KLASKY: Yes. I guess the point of this is only to illustrate that we basically observed a plateau roughly at the same point at which the plateau was observed with respect to the aluminum concentration. And likewise, in the upper figure the

hydrogen production really ceased at about the same time that that plateau in aluminum was observed, which basically means that aluminum corrosion ceased at this point. That is aluminum corrosion is accompanied by the production of hydrogen and --

CHAIRMAN WALLIS: It seems it ceases at various times?

DR. KLASKY: Well, there is substantial variability in the data, there's no doubt about that. I think I mentioned more perhaps the compelling evidence of the fact that the aluminum did not precipitate is the fact that if you just take the weight loss of the aluminum coupon and put that into the volume of a liquid, that's roughly consistent with the concentrations that were observed during the course of the measurement.

MR. TREGONING: Rob Tregoning.

And just again, point of clarification with the hydrogen measurements. They were done for safety purposes so they weren't done necessarily with a QA consistent qualified procedure. In fact, in many cases we varied the procedure; left the vent open, closed it for a certain period allowed things to accumulate. So there was some tweaking of procedures as well as measurement accuracy that leads to some of

that observed variability in the hydrogen data.

DR. KLASKY: I guess I'll hit it on the next --

MEMBER MAYNARD: Excuse me. The source for the aluminum in these, was it based on quantities that would be typically available or in a containment?

DR. KLASKY: Yes. The surface area of the aluminum was consistent with what would be in a containment vessel -- or sorry, a containment --

CHAIRMAN WALLIS: At least one containment. I think that containment -- aluminum varies tremendously from plant-to-plant.

MEMBER MAYNARD: Right. But I think most of them do have -- there's a program for accounting for how much aluminum. I just wonder if they can correlate that to these results or that's --

DR. KLASKY: These were, I believe, the upper bound. Rob, maybe you can elaborate on that.

MR. TREGONING: Paul's going to.

DR. KLASKY: Okay. Paul's going to.

MR. KLEIN: Paul Klein from NRR.

This would be considered an upper bound. Based on plant survey data there are a few plants that have aluminum levels on the order of ICET. Compared to what was scaled for the test, most plants have

substantially less aluminum.

MR. TREGONING: I'm going to clarify. Upper bound, I always cringe when the word "upper bound" is used.

I think I would agree that in terms of the amount of submerged aluminum that was within this test, which was the largest contribution of aluminum in solution, that that upper bound statement is probably accurate.

I qualify that because we see a lot of information from the plants and it changes quite often in terms of -- even some plants that have reported high aluminum, over time their aluminum levels can fluctuate pretty dramatically. But there are plants out there that do have more aluminum than we put in the ICET tank.

MEMBER KRESS: Right. A typical containment, the bulk of the aluminum --

MR. TREGONING: Yes, that's more typical. But we did --

DR. SHACK: Even in ICET only 5 percent of the aluminum was submerged.

MEMBER KRESS: Okay. So it is --

MR. TREGONING: But the thing that we have seen at least based on the limited plant data that we

have seen is the plants that have high aluminum typically have much less than 5 percent submerged. So that's the distinction.

DR. SHACK: And I'll have a little discussion of that in my presentation.

MR. TREGONING: Yes. We can revisit it then.

DR. KLASKY: So one consequence of having the aluminum, in test 1, in test 5 -- this was initially surprising to us that the lack of silica we had predicated initially that the fiberglass would dissolve under the high pH conditions. And yet we didn't observe that in test 1 or test 5. The silica concentrations were very low. And so we started to think about the interactions of aluminum and silica. And in fact this is sort of the inverse of what happened in test 4.

Test 4 we dissolve the CalSil which led to high concentrations of silica. And consequently we basically precluded or passivated the aluminum surface. So to show that in this next figure that illustrating the silica concentration and showing that in test 1 and test 5 the higher aluminum concentrations in essence permitted the dissolution of the silica or fiberglass, whereas test 4 you see the

CalSil initially produces very large concentrations of silica which presumably had some effect on the corrosion of aluminum. So there is an interaction in this.

CHAIRMAN WALLIS: You don't know how much you need of this CalSil --

DR. KLASKY: So we started studying that and --

MEMBER KRESS: You predict that in advance of running the tests?

DR. KLASKY: No. The predictions of --

MEMBER KRESS: I wouldn't think so.

DR. KLASKY: The predictions of silica, I believe Southwest Research performed those equilibrium calculations. And that was not predicted.

Now it turns out that there is a wealth of information in the literature so this had been studied. Yes, and I'll present this next figure that illustrates that.

So to start to answer Dr. Wallis' question with respect to how much silica one needs to more effectively passivate the aluminum, we performed a number of studies. And I'm only reporting on the study in which we place silica in concentrations that basically were attempted to mimic test 4, but we have

performed these studies in which we've introduced a variable amount of silica.

CHAIRMAN WALLIS: Wouldn't it depend on the form of the silica. I mean, with glass we put in a glass beaker. You got silica in the glass.

DR. KLASKY: The answer to that question is yes, it is very dependent upon the form of silica that's placed into a solution. It requires a amorphous silica to have an effect, which hasn't been studied. But it is a valid point.

So the upper figure just illustrates precisely what we saw in ICET test 4. It formed places a high concentration of silica and the concentration is roughly a 100 milligrams per liter, I believe, or I think we might have placed the upper bound as well. 200 milligrams per liter. One effectively very rapidly passivates the aluminum surface and does not observe aluminum in solution, whereas if no silica is present or a negligible amount of silica is present in solution, the corrosion rate is rather rapid. And calcium has some effect on passivating the aluminum surface, but it's not as effective as the silica itself.

So getting back to the question can one predict this or was this known, the answer is that

there have been numerous investigations of the passivation of aluminum surfaces. And I present one on the bottom figure. And this bottom figure is meant to illustrate that one can basically effect the potential by introducing both silica and aluminum into solution. And so what you see in the baseline is:

A. That the potential is negative 1600. And the more negative the more apt or susceptible to corrosion. By introducing small quantities of silica, one can effective raise the potential and basically in some sense inhibit or slowdown the rate of corrosion.

So this starts to answer Dr. Willis' question about how much one needs by viewing the potentials and the means by which the potentials raised by small incremental amounts of silica one can attempt to get at that.

And we have performed some tests that will be outlined in the NUREG that attempt to address that question of how much silica is necessary to effectively passivate --

CHAIRMAN WALLIS: So you're suggesting that plants should put CalSil in their sump if they don't have any?

DR. KLASKY: Well, not too much because then you've got a different problem. Just enough.

MEMBER KRESS: CalSil injections.

MR. TREGONING: This is the first known benefit of CalSil in this whole sump issue that we've seen over the years.

CHAIRMAN WALLIS: I think it also protects the steel, doesn't it?

DR. KLASKY: I'm not familiar with steel.

So it also turns out, if one examines these figures, that the presence of aluminum in solution also has an effect on the corrosion rate. So there is a feedback mechanism in the system. And that basically speaks to if one performs corrosion tests in a smaller volume and the concentration is larger in the end, one can expect different corrosion rates. So I think one has to be careful in the means by which corrosion rates are measured. And I think that that's part of -- I guess, Bruce, you guys found in the small scale testing that you had performed. Perhaps not in the same context, but nonetheless there is an effect.

So we observed upon cooling from 60 degrees to room temperature that a precipitate formed during this course of cooling. And so first to get a sense of what the elemental composition of this precipitate was or is, we performed ICET analysis. So basically we're just after what is the elemental

composition. Forget about molecular structure. It's a very simplistic chemical analysis, in some sense.

And one can see that there's a large percentage of aluminum, predominately the precipitate is aluminum. And it's accompanied by a rather large percentage of boron as well.

Now this precipitate that I'm -- these numbers that I'm giving here is precipitate that has been washed. I think perhaps previously we've presented elemental compositions in which we did not rinse the precipitate first. And in that case, one sees a lot more sodium and a lot more boron. And this speaks to, I think Dr. Wallis mentioned this film or web-like structure between fibers. Well, this same material basically forms on the precipitate itself. And it's a consequence of sodium borite being very soluble but, of course, if one extracts something from solution, there is surface tension. And then one allows it dry, well you get this material as a consequence as well. And the figure that illustrates this. This was a figure that a colleague of mine, Steve Tippera, he noticed that this material that we call precipitate was actually two different materials. A white material which turns out to be tincalconite or a crystallin material as evidenced by the sharp

residences in the XRD and this amorphous greenish material or amber material which is amorphous aluminum.

So I don't know if we previously presented this, so really we're talking about two different materials and really because we're interested in what's really occurring with the ICE environment, the actual material that we're interested in is the amorphous aluminum material. The tincalconite is only present if one allows material to dry or removes material from the solution and subsequently the material dries. So we're going to focus on the amber or amorphous aluminum material. And I think that perhaps a lot of the remarks we made with respect to the web-like structure and whatnot need to be revised. That is, some of the work that we presented earlier with respect to the web-like structures probably is just a manifestation of removing the fibers from solution.

CHAIRMAN WALLIS: Why would it make a web?

DR. KLASKY: I think it's almost like it -- well, first it adheres just to surface tension. And then basically it's drying. And I guess it's the cracking during the drying process that is leading to that appearance of a web.

One might think that it just cracks and basically separates. But apparently there's enough tension and cracking doesn't break it, I presume.

MEMBER KRESS: I think it's surface tension driven.

DR. KLASKY: I think that's what forms it. I mean, we haven't fundamentally studied the surface tension properties of this, and so I'd just leave it at that.

MEMBER KRESS: Probably not important to this work.

DR. KLASKY: I don't know. I think that really -- Bruce?

DR. LETELLIER: Bruce Letellier from LANL.

Marc, maybe you should explain that you're able to reproduce this artificially.

DR. KLASKY: Yes. Sort of to prove this hypothesis or I should say to lend a little more support, we took fiber strands, dipped them into sodium hydroxide with borate present and removed the fiber, examined the fiber subsequently and observed these same structures.

CHAIRMAN WALLIS: Out into the air that you make these liquid films?

DR. KLASKY: That's right.

CHAIRMAN WALLIS: And then they dry?

DR. KLASKY: That's right. We also washed the film or the fiber as well. And once we did that, of course, it disappeared. So it's certainly a manifestation of just removing it from--in my opinion at this point is what I'd include.

Now to understand the nature of the precipitate, I wanted to understand what the consequences of drying has on the precipitate itself, that is the aluminum material. And so one can see in the figure that the material is comprised of a very large percentage of water. Just four hours of drying at 60 degrees leads to a weight loss of upwards of 90/95 percent. So we're talking about very hydrated material which has a consequence in terms of head loss. So we're not just talking about -- you know, we're talking about a small amount of aluminum basically assuming a rather large volume due to the associated water that's present. And I think in developing a head loss correlation that has to be recognized. We're not talking about just aluminum. We're talking about the whole hydration sphere itself or an effective diameter that's much larger than just the small amount of aluminum that one places into the solution.

CHAIRMAN WALLIS: What is tincalconite?

DR. KLASKY: It's just sodium borate. I It's I think --

CHAIRMAN WALLIS: That's sodium borate. And boehmite is a oxide of aluminum, isn't it?

DR. KLASKY: Boehmite is the --

CHAIRMAN WALLIS: Aluminum.

DR. KLASKY: What's that?

CHAIRMAN WALLIS: Isn't that an oxide of aluminum or --

DR. KLASKY: Yes. It's ALOH and the tincalconite is the sodium borate.

CHAIRMAN WALLIS: And Boehmite is from Germany and bauxite is from France.

DR. KLASKY: I'm going to give a little more information about the precipitate of the least qualitative. We performed a number of observations using TEM. And what we have here is just an illustration that it almost looks like the precipitate is comprised of --

CHAIRMAN WALLIS: When you're finished all this are you going to tell us what this means for sumps? It's a lot of detail you're going into here, but you've got to -- the interest really is what does this tell us about sumps. Are you going to get there?

DR. KLASKY: Okay. The answer to that question is we're first attempting to understand what the material is. And then the attempt was to, in essence, reproduce the material so that Dr. Shack and others could basically have confidence that the surrogate that they're using was indeed representative of what might be expected --

CHAIRMAN WALLIS: Well, if you take this stuff out, you tell me it's a gel. And you take it out and you dry it and you get a powder or something like that?

DR. KLASKY: Correct.

CHAIRMAN WALLIS: Then if you put it in water again does it make a gel again or does it stay a powder?

DR. KLASKY: This solution is very slow.

CHAIRMAN WALLIS: But it would stay a powder? It wouldn't necessarily go back to a gel?

DR. KLASKY: It would stay -- if one allows it to -- I mean, it's a powder. It's an unstructured -- it's not a crystal, right. There is very little crystal indication. But it's going back into solution --

CHAIRMAN WALLIS: Is it a gel in the sump or is it a powder, or what is it?

DR. KLASKY: It's a gel in the sump.

CHAIRMAN WALLIS: A gel in the sump.

DR. KLASKY: Right.

DR. SHACK: That's because he shows that enormous amount of hydration.

CHAIRMAN WALLIS: That's right. And I was just wondering about whether the way that industry is going to make this stuff is going to duplicate the gel also.

DR. KLASKY: Well, I'm going to speak to that. Because let's get to the chase.

CHAIRMAN WALLIS: Surely.

DR. KLASKY: The bottom line is is the material that one produces representative of what is produced under an environment in which corrosion is basically leading to the formation. And so when -- one has to be very careful. I think we'll show you can produce it, but it takes a lot of care. It's not as simple as just mixing the two test tubes together and saying I've mixed aluminum with sodium hydroxide and I'm done. So the onus is on the person making the surrogate to prove that the surrogate is representative. And I think we're attempting to come up with some metrics by which to judge the adequacy of that surrogate. So, yes, it's long-winded, but that's

the --

CHAIRMAN WALLIS: But that's important I think. That's an important conclusion.

DR. KLASKY: I won't spend too much time on these, but we've also used NMR to characterize the properties of both the solution and the precipitate itself. And this figure basically shows, if one goes to the literature, that the chemical shifts that are observed in the aluminum 27 and the boron 11 NMR are consistent with the fact that aluminum coordinated with boron. And so we know something about the real structure now of the precipitate; that is aluminum coordinated with both trigonal and tetrahedral boron.

CHAIRMAN WALLIS: Now this boehmite doesn't have boron in it, does it?

DR. KLASKY: The boehmite itself?

CHAIRMAN WALLIS: Yes.

DR. KLASKY: Boehmite has boron absorbed onto the surface.

CHAIRMAN WALLIS: But boehmite itself doesn't have boron in it?

DR. KLASKY: No, it does not.

CHAIRMAN WALLIS: No.

DR. LETELLIER: I'd like to interject. This is Bruce Letellier from LANL.

Although Marc's presentation is extremely detailed, it does illustrate how difficult it can be to speculate an unknown material. I mean, that was one of the Staff's initial requests that we identify exactly what products were formed. And, in fact, it can be very difficult especially in an integrated test environment that's less well controlled than you might expect for a --

CHAIRMAN WALLIS: Well, this does concern me about this May meeting, this workshop. It seemed that NRR was asking questions learned from your tests. And industry seemed to have sort of rather easy going answers. They're saying, well of course, we'll just make this stuff and it's going to be okay. And it's not going to be perhaps quite as easy as that. Maybe that's just the impression I got from looking at the --

DR. SHACK: You'll get a simpler minded answer from me. So you can --

CHAIRMAN WALLIS: Well, you think it's going to be okay.

DR. SHACK: We'll come back and review this question.

CHAIRMAN WALLIS: Okay.

DR. SHACK: Because obviously it does it

arise in the context of my --

CHAIRMAN WALLIS: You're going to sort it all out for us?

DR. SHACK: Well, I'm going to give you my take on it.

CHAIRMAN WALLIS: Okay. Okay.

DR. LETELLIER: I'd like to say that numerous investigators have created a variety of chemical products and they all appear to give adverse head loss effects. But comparing the degree, trying to quantify the comparison is the challenge.

CHAIRMAN WALLIS: Yes. Especially if they differ by an order of magnitude.

MR. TREGONING: But I will say many of the points that Marc's raising with respect to difficulties of creating surrogate, I think Bill is going to hammer home with some applications to show how these manifest themselves in head loss tests and some of the peculiarities and differences that you really need to keep in mind when you're doing these sorts of tests if you want to get something that's meaningful at the end of the day.

DR. KLASKY: I think the way of illustrating the difficulty or one of the problems illustrated by these two figures that I've presented

on the solubility of the aluminum, one is a crystalline structure gibbsite and the other is for a amorphous aluminum. And basically for any given pH there's two orders of magnitude difference in solubility. And initially, I'll just recount a conversation I had with Dr. Shack, that I had basically performed some calculations and was very careful in using the best thermodynamic data available. And by that I mean, let's say data that had been scrutinized and really taking mean values and standard deviation and thermodynamic data and came up with predictions that could be up to a factor of 2 different than what other investigators have presented in textbooks, for example. And investigators have found that the solubility is very susceptible to small changes in the way it's produced mixing the rate at which one reagent is added to another.

So it's not --

CHAIRMAN WALLIS: So this solubility is dissolving of something? It's not the point where it gets saturated and precipitates? That's something else, isn't it?

DR. KLASKY: Prior to the highest concentration --

CHAIRMAN WALLIS: Does the amorphous of

the aluminum reach the saturation and then a gibbsite is way far -- way far away from it?

DR. KLASKY: That is correct.

CHAIRMAN WALLIS: What is gibbsite? Do you make ladders out of gibbsite? I have no idea what a gibbsite is.

DR. KLASKY: It's just crystalline aluminum.

CHAIRMAN WALLIS: What do you make ladders out of?

MEMBER KRESS: Aluminum, hopefully.

DR. KLASKY: Aluminum.

CHAIRMAN WALLIS: But what is it?

DR. KLASKY: Aluminum metal.

CHAIRMAN WALLIS: So --

DR. SHACK: Gibbsite is just another ALO(OH) --

CHAIRMAN WALLIS: Oh. Because you're talking about your solubility of aluminum it says here. So you're talking about solubility of aluminum oxides?

DR. KLASKY: Correct. Correct.

CHAIRMAN WALLIS: Okay.

DR. KLASKY: So that the point here is that the concentration of aluminum in both test 1 and

test 5 we believe was below the solubility limit. And so the point is what basically terminated the production was not reaching a concentration limit, but rather passivation of the aluminum surface. So if one wants to really get a good estimate of the total quantity of aluminum put into solution, one has to really understand the corrosion or the passivation of the aluminum surface.

CHAIRMAN WALLIS: I would think these ladders are passivated already, aren't they? They've been around for a long time.

DR. KLASKY: There is an oxide layer that is present, but if you place that ladder in the wrong pH regime, then one may not have a ladder anymore, or a smaller ladder.

DR. SHACK: But it's those solubilities going up by orders of magnitude.

CHAIRMAN WALLIS: That's right. I noticed the log scale there. Yes.

DR. KLASKY: So that's sort of the conclusion with respect to the source term, that the source term is really driven by that corrosion and basically terminates due to passivation. And really we're not faced with approaching a solubility limit in the ICET like environment.

CHAIRMAN WALLIS: Does it also dissolves readily with the low pH?

DR. KLASKY: Yes. That's right.

CHAIRMAN WALLIS: And what's the lowest pH you get with the boric acid? Before it gets to the sump? It sprays out over the containment, it forms pools, it does all kinds of --

MR. KLEIN: Paul Klein from NRR.

It's probably in the range of 4 to 4½.

CHAIRMAN WALLIS: So if we look at 4 here, we've orders of magnitude difference in the solubility from -- gibbsite, I didn't know about -- yes, there are orders of magnitude, too, for the other one, whatever.

DR. KLASKY: Yes.

CHAIRMAN WALLIS: The amorphous one. Yes.

You're always talking about the sumps here, but aren't there pools of acidic water that are hanging around in the containment on their way to the sump?

MR. CARUSO: And could there be rivers of sodium hydroxide, concentrated sodium hydroxide flowing down ladders?

CHAIRMAN WALLIS: From sprays or something?

MR. CARUSO: From sprays?

DR. KLASKY: What is the upper pH? 12, is it?

CHAIRMAN WALLIS: It always seems to be assumed that you've got some mixture in the sump, but there are regions where there -- there are high or low pH regions in the containment depending on what you're doing locally.

DR. SHACK: But that generally exists only for a relatively short period of time.

CHAIRMAN WALLIS: I mean, it could be in a blanket of hot fiberglass dammed up there dissolving something --

DR. SHACK: If it's dammed up, you don't worry about it.

CHAIRMAN WALLIS: But then you turn on some sprays and it's released, you know. So it's not -- anyway.

DR. LETELLIER: Dr. Wallis, one important implication of solubility is that you're not creating a precipitate. It actually provides a reservoir to keep that material in solution.

CHAIRMAN WALLIS: Until you've put it in the sump maybe. Until you cool it down in the heat exchanger.

DR. LETELLIER: Until you experience a temperature change.

CHAIRMAN WALLIS: Right.

DR. LETELLIER: But on the other hand, the important conclusion Marc has that the plateau in aluminum concentration is due to passivation. And that's very important because otherwise you could have a heat exchanger that generates a pump of aluminum hydroxide precipitate continually removing it and replacing it.

DR. SHACK: Right.

DR. LETELLIER: So Marc's identified a very important physical mechanism that provides a measure of safety, if you will. That's the benefit of adding a little bit of CalSil is to passivate the aluminum surface.

MEMBER MAYNARD: And there may be some small pools, but typically the containment's designed so that the water will all flow to the sump. So you obviously have some equipment and some stuff, I-beams coming up where there may be some small pools.

CHAIRMAN WALLIS: There could be drains that get locked.

The effect though of barriers now they're putting in some containments to at least catch the

fiberglass, to catch the insulation.

Okay. Can we move on.

DR. KLASKY: Yes. So the point is is as well that the solubilities are very sensitive to the conditions in which the material are mixed, made and can vary substantially.

In addition, I think another point that needs to be made is that within the sump we're talking about times on the order of 30 days or a month. And during this period the investigators have shown that aging has an affect on crystallizing the aluminum materials or precipitate that formed. And yet, when we examine the ICET precipitate we observed an amorphous form months after its formation. And that sort of led us back to why is the aluminum remaining amorphous, why is the solubility as high as it actually is. And the answer to that question is born out by the fact that we have a very high percentage of boron absorbed onto the aluminum surface. And so what this does is really preclude the crystallization and it keeps the solubility high, which is of course a good thing in that sense that we basically aren't precipitating aluminum. And this has been illustrated by a number of different experimental investigations in which the structure of the aluminum precipitate

have been examined by XRD. For example, we observe after aging, we get more of a crystalline material than if in fact the initial aluminum is amorphous and it just basically crystallizes and, hence, the solubility decreases at that point.

CHAIRMAN WALLIS: Now the head loss correlations use something like area per unit of volume, which translates really as particle size.

DR. KLASKY: Right. And that's what I'm going to get to now.

CHAIRMAN WALLIS: It must be dependent upon history here quite a bit, right?

DR. KLASKY: Yes. So I'm going to get to the particle size, and I'll do that by talking about the development of surrogates.

So after that examination of the ICET material, the question that we had and Argonne was wrestling with at the time, I think this was even prior to Argonne starting, was can we develop surrogates so that we could perform separate head loss testing and didn't have to wait 20/30 days to produce the material and run it through a loop.

And so what we did was examine the dissolution of two different types of aluminum. One was aluminum nitrate crystals and the other was just

dissolving aluminum metal coupons just in the same manner that ICE basically dissolved aluminum. But we're attempting to produce a surrogate and basically use our analysis tools to basically determine whether we can produce a surrogate that has the same properties. And now I'll get into the --

CHAIRMAN WALLIS: How about the gel characteristic that you talked about earlier?

DR. KLASKY: I'm going to talk about that.

CHAIRMAN WALLIS: You're going to get to that? Oh, you're going to get to that. Okay.

DR. KLASKY: So in fact this is what we did in terms of compromise. So we used XRD and compared the amount of crystalline or the crystalline size cell dimensions. And you can see that the matches is rather good. That is, that the cell dimensions a, b and c match remarkably --

CHAIRMAN WALLIS: Unreasonably closely.

DR. KLASKY: Yes.

CHAIRMAN WALLIS: And extraordinarily closely?

DR. KLASKY: Yes. Which, I mean, was rather remarkable. We were very happy with that.

CHAIRMAN WALLIS: How can you measure so accurately?

DR. KLASKY: That's the -- I'll leave that to --

CHAIRMAN WALLIS: Those are --

DR. KLASKY: XRD analyst. The decimal places are remarkable, I'd agree with you. I guess in one case, b, 12.259 versus 12.264. But that's about as --

CHAIRMAN WALLIS: Well, in every case 3.6817, 3.6821. And I'm not sure you can measure that accurate --

DR. KLASKY: We will have to get back to you with respect to the accuracy of the measurement.

MEMBER DENNING: Help me again. This is a crystalline structure, that's what the a, b, c stand for?

DR. KLASKY: Correct. The cell size dimensions of measuring a cube, and these are the dimensions.

CHAIRMAN WALLIS: Well, I guess this is down to the atomic level. You'd expect them to be the same there, wouldn't you?

MEMBER DENNING: Yes. I'd like to suggest that the accuracy is indicative of a specific crystalline form.

CHAIRMAN WALLIS: That's right. It's the

same thing.

MEMBER DENNING: The signatures are very unique.

CHAIRMAN WALLIS: It's the same thing. It's the same stuff.

DR. KLASKY: And that was the confirmation that we were hoping for.

CHAIRMAN WALLIS: Right.

MEMBER DENNING: But it's the crystallite size critical to us. And that is that that 29 and 20 represent the equivalent of an aerodynamic mass mean diameter or something like that if you were thinking of -- what does that represent?

DR. KLASKY: No. I'll present the actual hydrodynamic rating is the equivalent of that in a different measurement.

CHAIRMAN WALLIS: Well, they have a cubical thing? Are they long fibers? What do they look like.

DR. KLASKY: The individual -- leads you to believe that it's an octahedral boron -- sorry.

CHAIRMAN WALLIS: Not of the structure level, but of the particle size. They're random sized shaped particles like gravel?

DR. KLASKY: That I can't answer.

CHAIRMAN WALLIS: Course gravel or something?

DR. KLASKY: The assumption in deriving the effective radius or diameter is that they're spheres. I mean that's --

CHAIRMAN WALLIS: It's area to volume ratio or something?

DR. KLASKY: Yes. That's based on the life scattering. You know, you're assuming a hydrodynamic radius and using the assumption that it's a sphere.

So we also compared the hydration or we used TGA analysis to basically determine or make a comparison between the surrogate and the ICE material. And the upper curves illustrates the results of the surrogate and the lower curve represents the ICET material, the gel or I think we called it the gel. And you can view the results. I mean the fact is that they're rather good agreement as well obtained from the comparison between the surrogate and the ICET gel in terms of the hydration -- loss I should say.

MR. TREGONING: Marc, this is Rob Tregoning, NRC.

Point of clarification. On the slide before you talked about two different surrogates. You talked about surrogate induced by aluminum nitrate

crystals as well as corrosion from aluminum metal. Which surrogate are you talking about as providing a good comparison in this slide, which of those two?

DR. KLASKY: You got me there, Rob. I'm going to have to get back to you. I don't recall which figure corresponds to this data.

CHAIRMAN WALLIS: Well, the surrogate is the first one, isn't it?

DR. KLASKY: No, there were two. We produced surrogates in two different --

CHAIRMAN WALLIS: But the real surrogates you want to use is the -- you don't want to have to dissolve coupons. You want to just dissolve stuff.

DR. KLASKY: Correct. And I believe the answer is the aluminum nitrate crystals.

MR. TREGONING: Okay.

DR. KLASKY: But I need to check that.

MR. TREGONING: That's an important point, obviously.

DR. KLASKY: Yes.

MR. TREGONING: Because here is baseline is the actual ICET 1 precipitate. So that's what was actually formed in the experiment.

DR. KLASKY: Rob, I believe, it's been some time, but I believe we were successful by

dissolving the crystals. I need to confirm that.

MR. TREGONING: WE'll certainly clarify that within the NUREG.

CHAIRMAN WALLIS: What are we looking at in the top graph there?

DR. KLASKY: Say that again.

CHAIRMAN WALLIS: What are we looking at in the top?

DR. KLASKY: The red curve is the weight loss and the blue is the derivative of the red.

CHAIRMAN WALLIS: The derivative?

DR. KLASKY: Right.

CHAIRMAN WALLIS: So we're not looking at -- okay. We're not looking at a comparison there?

DR. KLASKY: No.

CHAIRMAN WALLIS: No.

DR. KLASKY: You have to compare the red curve versus -- in the upper graph versus the green curve in the lower graph or figure.

For example, at 300 degrees the ICET we have a weight percentage of 80 percent.

CHAIRMAN WALLIS: The derivative is the blue?

DR. KLASKY: Correct.

CHAIRMAN WALLIS: Well, it should be zero

when it's leveled off, shouldn't it?

DR. KLASKY: Well, that's certainly true.

CHAIRMAN WALLIS: It doesn't look right. Oh, it is zero. Ah.

DR. KLASKY: You had me going as well.

CHAIRMAN WALLIS: Okay.

DR. KLASKY: Okay. So sort of the bottom line now. So we want to use this --

CHAIRMAN WALLIS: The bottom line is surrogates are okay?

DR. KLASKY: I think the bottom line is that we were able to produce a surrogate that did have comparable properties of ICET by dissolving the aluminum nitrate crystals into solution. But, you know, I think the caveat is one can also produce surrogates that are not okay. If you titrate and are not careful, concentrate aluminum hydroxide into a -- sorry. Concentrated sodium hydroxide into an aluminum, you will basically form a precipitate that is very crystalline in nature and not representative of what is observed in ICET. So the fact that we're able to do it doesn't basically drive home the point perhaps that it -- it's not always the same.

CHAIRMAN WALLIS: You haven't shown us here what happens if you do it badly?

DR. KLASKY: Correct. We haven't shown that.

CHAIRMAN WALLIS: You ought to show that really.

DR. KLASKY: We just showed that you can do it.

CHAIRMAN WALLIS: One of the way you can go wrong. You're showing you can do it, it doesn't mean to say that anybody --

DR. KLASKY: That's right.

CHAIRMAN WALLIS: -- who does it is going to get the right answer.

DR. KLASKY: And maybe that's more important.

CHAIRMAN WALLIS: That's more important, right.

DR. KLASKY: Yes, that's correct.

MEMBER DENNING: But let's understand this slide better because I think it really is important and I'm not understanding. What you're saying is a primary and then an aggregated version. That is that on the right side there, the thing that -- now that's half micron basically, is that what that is?

DR. KLASKY: Correct.

MEMBER DENNING: So that's an aggregate.

DR. KLASKY: Yes, let me talk about this slide. So we're using dynamic light scattering to determine the particle size, if you will, and distribution within both the surrogates and also ICET.

For ICET we only could measure the particle size of solutions that had previously cooled. Now with dynamic light scattering there are large limitations to --

CHAIRMAN WALLIS: So you don't get any of these things until you cool it, do you?

DR. KLASKY: Correct. Well, hold on a second. What this shows with the surrogate at 60 degrees is that particles or these colloids do exist at 50 degrees. So when you say you don't get anything, that's really I think a misnomer.

CHAIRMAN WALLIS: The solution is in colloidal form?

DR. KLASKY: Correct. That's right. And these colloids have diameters on the order of we're seeing 30/50 nanometers and also we're also seeing 500 nanometers.

CHAIRMAN WALLIS: Well, they haven't precipitated yet. They're just --

DR. KLASKY: Correct. They're suspended.

CHAIRMAN WALLIS: So how do you know the

properties of the precipitate depend perhaps on how you cool it?

MEMBER DENNING: Now wait a second. I'm not sure I'm understanding what you're saying. It still is a precipitate, yes?

CHAIRMAN WALLIS: That's what they say.

MEMBER DENNING: I mean it's --

DR. KLASKY: I think the answer to that question is difficult in this. We're talking almost semantics. It is not in solution. We have --

MEMBER DENNING: Right.

DR. KLASKY: -- particles or we can observe particles. They're not settling --

MEMBER DENNING: Because they're extremely small?

DR. KLASKY: Correct. Right. Or they have a settling time associated with them perhaps.

CHAIRMAN WALLIS: But right at the beginning you said the aluminum was in solution or in the beginning of the whole talk?

DR. KLASKY: Well, maybe that I --

CHAIRMAN WALLIS: You mean it was in colloidal form?

DR. KLASKY: That's right. It's inclusive of colloids as well. That's right.

MEMBER DENNING: Okay. And that figure that you're showing corresponds to the first bullet, the top bullet?

DR. KLASKY: Correct. Yes.

MEMBER DENNING: Okay. Now why is it so bimodal? I mean I would expect --

DR. KLASKY: A continuous --

MEMBER DENNING: A continuous.

DR. KLASKY: Right. If it's an aggregation. But there are systems in which there are two stability points. People have seen that. So it's not impossible. But I think we'd have to do a lot more work to conclusively prove that point that it is, you know, it almost looks like its quantized that these two -- around these two points, 50 nanometers and 500 nanometers.

MEMBER DENNING: Now, but isn't the $64 question the second bullet and what size distribution looks like there? I mean, are you going to show us something like that, isn't that the question?

DR. KLASKY: No. The answer with respect to ICET at 24 degrees we basically observe particle sizes that were roughly 50 nanometers.

MEMBER DENNING: Fifty nanometers?

DR. KLASKY: Fifty nanometers.

MEMBER DENNING: I mean, it's extremely small.

CHAIRMAN WALLIS: And no 500 nanometers?

DR. KLASKY: Correct. Correct.

CHAIRMAN WALLIS: An awful lot of surface area.

DR. KLASKY: It is.

MEMBER DENNING: So if that's true then when these things deposit in the bed, they don't come close to filling voids and stuff like that? They're very small in the deposition in the bed?

DR. KLASKY: Well, I think perhaps, Bill, you saw this when you produced -- you measured head loss. In fact, without seeing any precipitate, right? So presumably that's what you have. At 60 degrees you have these colloids that, I don't know what you measured, but are nanometers in size, tens of nanometers perhaps that basically pack very well and --

CHAIRMAN WALLIS: What do they do in a bed? Do they actually cluster in some way?

DR. KLASKY: I --

CHAIRMAN WALLIS: Are they sticky, do they stick to each other?

MEMBER DENNING: Purely sticky. But if

they were stuck together in the volume, then you would see big things. You wouldn't see --

DR. KLASKY: Right. But also there's an interaction, a repulsion as well. And that repulsion between particles is dependent upon the solution pH.

CHAIRMAN WALLIS: Charges, are they? Exactly. Right.

DR. LETELLIER: Bruce Letellier from LANL.

You can speculate about a number of mechanisms, but particles that small can always impact themselves on the surfaces and increase the hydraulic resistance. In fact, the fact that they're physically hydrated raises some interesting questions about energy dissipation through vibration in a colloids type of form. You don't actually have to agglomerate physically large particles that can fill the void space --

CHAIRMAN WALLIS: But they do when they flow through a vent. They could deposit like snow on a power line. I mean --

DR. LETELLIER: Very much so.

Marc, you did have a slide that showed the agglomeration of larger clumps out of small constituents. That one.

CHAIRMAN WALLIS: Well, this is very

interesting. I just can't see how to make the connection to what happens in a sump. That seems to be a long way from what happens in a sump. This is very interesting stuff, but I suspect that if you can't make the very close connection to a sump, it's not going to be very useful to NRR and they're going to just accept some global test done by industry as okay. Because they don't quite know what to do with this stuff.

DR. KLASKY: I guess what my thought is that, you know, you've characterized in a sense a useful parameters in terms of the diameter to be used in a head loss correlation. And I had done some preliminary investigations in terms of using this type of work. I think -- I'm trying to think where they were from.

But anyway, they had basically developed head loss correlations via this type of a mechanism of measuring the particle size diameters and basically running it through --

CHAIRMAN WALLIS: How do they expect a history effect? If you run this stuff through a fiberglass bed, that it would so deposit and you'd go around and more of it would deposit and it would sort of take quite a long time before you'd reach any sort

of equilibrium with bed structure?

DR. KLASKY: That's in fact these basically filtration models. In fact, precisely what happens.

CHAIRMAN WALLIS: It would go through the reactor many times before it --

DR. KLASKY: There's a collector efficiency. And one can calculate as well the collector efficiency and there's a fundamental theory I think that was first developed that lays --

CHAIRMAN WALLIS: Then you've got charged particles. It's not the same as just collecting --

DR. KLASKY: They've examined the effective colloids and --

CHAIRMAN WALLIS: They have?

DR. KLASKY: Yes.

CHAIRMAN WALLIS: Okay.

DR. KLASKY: So there is a substantial amount of literature on filtrational colloids. So it's not like we're stepping into something that's totally new.

CHAIRMAN WALLIS: Okay.

DR. KLASKY: So I think your question of so what do you do with all this stuff is the $64 question, right? That's --

MR. TREGONING: Well, I want to weigh in a little bit on this, if that's okay.

Rob Tregoning from Research.

I think one of the things, this is fundamental, very building block technology that I would argue is really necessary to understand what the true implications are. I think will Bill Shack gets up here next he's going to show what some of the head loss implications can e for some of these materials.

But more importantly, what Marc's really doing I think when we look at these various surrogate tests is he's providing metrics that people that want to run tests, if they want to try to recreate the type of precipitate we found in the ICET tests, he's giving them very definitive metrics and ways that they can go about achieving that in demonstrating that the implications in terms of what the ramifications are for their specific head loss can be, you know, defined in that sense. So that's a very important point. I think it gets lost sometimes in --

CHAIRMAN WALLIS: Well, NRR is going to look at some industrial tests and say did you use a surrogate which has a 29 Armstrong -- they're just going to say this is to complicated. We're going to accept whatever they did?

MR. KLEIN: Maybe somewhere between the two.

This is Paul Klein from NRR.

I think we'd be looking for some help from the Office of Research on what might be the most appropriate way to try and generate surrogates and then ensure that industry's approach of generating surrogates is consistent with that.

CHAIRMAN WALLIS: Well, what concerns me is that there seems to be a move to cut off all this research and say it's finished. Really, the research ought to be going on while you're asking questions of industry. And as question come up from these industrial tests and there's some doubt about whether the surrogate has the right particle size, then you can turn to these guys. Because they still have a contract. And you can say tell us some more or do some more. I mean, the idea that you can just cut it off and then you're going to accept what industry gives you leaves a huge gap somewhere in the middle it seems to me.

MR. TREGONING: This is Ron Tregoning, Office of Research.

One of the things that we've been very sensitive in this area is that I would say up until

this point, the Office of Research has had a prime lead in uncovering much of the technology associated with this, especially chemical effects if not other areas as well. And there's been a decision that given that it's industry's issue ultimately to resolve, we want them to show a more prominent lead role in some of these technical issues and working them out. It doesn't mean that we're just washing our hands and walking away.

CHAIRMAN WALLIS: Well, look at the history of this. You will see that there's sort of discoveries that have been made every few months, which were very important. And were not anticipated as a result of doing exploratory research, not confirmatory research. And now industry is going to want to close this off by doing the minimum research possible. And it seems to be a completely different approach.

MR. TREGONING: That's certainly --

CHAIRMAN WALLIS: So I'm a little concerned about this dichotomy here.

MR. TREGONING: One of the things that we've been trying to do is make sure that we identify what amount of research is necessary for them to do and what type of research so that they don't do that,

minimize the research and then essentially, you know, move on at that point.

MEMBER DENNING: Marc, before you wrap up on this summary and conclusion could you give me a physical picture of your concept of you've got a fibrous filter bed, let's say, and we produce this precipitate. In the precipitate depositing somehow into that filter bed and causing additional head loss --

DR. KLASKY: Right.

MEMBER DENNING: -- what's your concept of how that's happening? Do you think that these particles are small are these masses are small and they're attaching themselves to fibers distributed throughout that filter bed?

DR. KLASKY: I think it's -- I guess the concept is analogous to a cake filtration model in which, you know, basically you're developing -- these particles are basically attaching on the outer fiber strands and basically that's, as they circulate, they're just growing. Aggregates are growing as a function of time.

MEMBER DENNING: You've got this huge mass of fibrous material and a much smaller mass of -- am I right? A much smaller mass of --

DR. KLASKY: But I guess what you're saying is, yes, you do have a very large surface area of fiber. But the question is how much of that fiber is actually active in this process. In other words if the particles basically all agglomerate at the top surface, then that is effectively the surface area over which these particles are being --

MEMBER DENNING: That's the question.

DR. KLASKY: I think that's correct.

MEMBER DENNING: The key question is is it at the top most surface and we are getting a kind of debris bed of this flocking material or whatever it is and then the fiber or is it distributing throughout the fiber and --

DR. KLASKY: The answer to our question, and this goes back. I did this about a year ago. I looked at that. And my conclusion was it was almost a surface coating. The depth of penetration was rather small.

MEMBER DENNING: Well, I'm struggling if these particles are that small, this 50 nanometers, that you're going to do that or whether they'll just go through and attach someplace else?

DR. KLASKY: You know, I think more work needs to be done. I'm just speculating really.

DR. SHACK: I mean it is true that we get high head loss with absolutely no visible cake like structure on top of the bed. I mean we don't see a thing. The head loss just goes up.

CHAIRMAN WALLIS: Well, some of these new designs have a screen with a hole size of 32nd of an inch or something and then they follow it up with a bypass capture which has woven, woven metal with a much tighter holes. It's quite conceivable to me that the woven metal will catch different kinds of little tiny fibers that might be more effective in catching these guys than the course fibers on the big screen. You can ask more questions then there are every answers to until you do the research.

MEMBER DENNING: Let's see if Bruce has a comment on that. Do you have a comment on this?

DR. LETELLIER: What you propose has actually been observed sort of unintentionally in one of the integrated tank tests where we were looking at CFD models. We had a primary capture screen of a 1/8th inch mesh. And, in fact, when we overflowed the tank accidently we discovered there was an internal screen much, much smaller that was capturing everything that penetrated. All of the rust particles, all of the very, very small particulates. So indeed it does

occur.

CHAIRMAN WALLIS: But then what do these things do?

DR. LETELLIER: That wasn't capturing. It wasn't capturing chemical --

CHAIRMAN WALLIS: Do things attach more preferentially to concrete fiber or concrete particles or the fiberglass or what? Maybe they don't attach to fiberglass at all. They're charged particles, they're not --

MEMBER DENNING: They're really charged -- or is this a polarization question?

DR. KLASKY: No, there's a charge. I mean, I think the point of zero charge is -- I'd need to get back to you. But there's a tremendous amount of literature on this topic?

CHAIRMAN WALLIS: They repel each other. So when they stick to a fiber, do they lose their charge or what do they do?

MR. TREGONING: Ron Tregoning, Office of Research.

Let me suggest that we table some of this discussion. Because I think you're going to hear more in Bill Shack's presentation. And then we're going to have some more of these issues comes when the peer

review presentation is raised. So, you know, that might be a more appropriate time to revisit some of these issues. Because there's not only these issues, but there are other ones that are associated with this that will be --

CHAIRMAN WALLIS: What we seem to be learning is that we don't have it all wrapped up where we can now write a textbook and there are no m ore questions, right? That seems to be the case. Is that a valid conclusion?

MR. TREGONING: Well, assuming that you're writing a textbook on aluminum effects --

CHAIRMAN WALLIS: Well, you can't just write a handbook which says this is the way you can do everything. We understand everything. You can't quite do that yet.

MR. TREGONING: No. We could not write a text book on everything. That's true.

CHAIRMAN WALLIS: Oh, you can't? Well, okay. So you're proposing that we end this presentation now and take a break?

MEMBER DENNING: Did you have any other points?

MR. TREGONING: I don't know if he wants to go through his summary conclusions quickly.

DR. KLASKY: I think the conclusions, we've spoken to --

CHAIRMAN WALLIS: Then we've gone through the conclusions, have we?

DR. KLASKY: I think so.

CHAIRMAN WALLIS: I think so. Because we discussed them along the way as well. And it seems to me that these aren't sort of solid conclusions hat help me as an engineer design anything.

DR. KLASKY: I think the most important point may be that, yes, we can produce surrogate. We think we have produced surrogate that can match the properties of --

CHAIRMAN WALLIS: But there's nothing on here that says use equation 10B to predict something.

DR. KLASKY: Well, no, but I think the attempt was -- one attempt was can we produce a surrogate to be used in subsequent head loss testing. And the answer is yes.

CHAIRMAN WALLIS: But that is a positive thing. You can.

DR. KLASKY: But more importantly we can, and maybe we didn't answer it here, there are many ways to produce the wrong surrogate. And maybe that's --

CHAIRMAN WALLIS: You didn't tell us that, though?

DR. KLASKY: That's right.

CHAIRMAN WALLIS: You didn't show us how to make a bad surrogate.

DR. KLASKY: One way to do is correctly and there are many ways to do it wrong or incorrectly.

CHAIRMAN WALLIS: Okay. Are we ready to conclude as a Committee, have questions about these conclusions?

Is the Committee convinced that everything is solved by now?

MEMBER MAYNARD: Well, I don't think everything will ever be totally resolved and I don't think we'll ever be to a point where you can just plug in numbers into an equation and come out with a design. I think the goal here is to get the information and to see if we can get where we get reasonable level of assurance that the health and safety of the public will be protected. I don't think we'll ever get to the point where all questions are answered and there's a textbook answer.

CHAIRMAN WALLIS: No. The question is whether we have adequate information to evaluate the performance of the sump and then it's effect on the

cooling of the reactor core.

Okay. So I'm ready to take a break until quarter to 11:00. Okay. Take a break for 15 minutes.

(Whereupon, at 10:33 a.m. off the record until 10:50 a.m.)

CHAIRMAN WALLIS: Let's come back into session.

We're going to hear from our colleague Bill Shack. I notice that to get the work done he needs quite a few colleagues to help him. But he's going to present it very clearly to us in his usual way, so please go ahead.

Oh, we're going to hear from Paulette first? All right. So, go ahead.

MS. TORRES: Good morning.

My name is Paulette Torres. I'm a chemical engineer working in the Office of Nuclear Regulatory Research. I'm the Project Manager of the chemical effects head loss testing program, and I'm going to be presenting a few introductory remarks today.

Next to me is Dr. William Shack. He represents Argonne National Lab, and he will presenting test results.

The objectives and motivations of the

presentations are unchanged since last February ACRS meeting. The chemical effects head loss testing is a confirmatory research activity. The tests measure head loss associated with simulated ICE environments. The program was designed to improve our understanding of the effects of important variables on chemical products information and head loss.

This project was motivated by our need to understand how chemical byproducts form in some specific environments can effect head loss.

When the project was begun we had little information on head loss associated with chemical byproducts.

CHAIRMAN WALLIS: I really wish you folks would call it pressure drop. Because head loss sounds really dramatic. But anyways.

MS. TORRES: You already familiar with ICET program LANL. ICET demonstrated that chemical byproduct can form in the chemical environment in a containment water pool after a LOCA. Those byproducts could possibly plug the sump screen. In turn, sump screen plugging can head loss sufficient to fail the ECCS recirculation function. However, the head loss associated with chemical product was not evaluated in the ICET program. So the head loss

testing program at Argonne is investigating the head loss across a sump screen that results from the combination of containment debris and chemical products form in a post-LOCA sump pool.

The chemical byproducts chosen for head loss testing are consistent with those observed in the ICET program. The results for the head loss in TSP buffered environments were presented in last ACRS meeting and those were representative of ICET-2 and -3 environments. And the results were documented in a series of quick look reports.

Since February Argonne's study of the head loss associated with chemical products observed in sodium hydroxide and sodium tetrabirate buffered environments. Both environments contain dissolved aluminum. During this talk we're going to present the results of those two ICET environments.

In terms of the regulatory applications the research at Argonne will provide the Staff with information to help review the responses of Generic Letter 2004-02.

All of the chemical head loss testing at Argonne is now completed and currently we are in the process of documenting testing results with a projected NUREG/CR report released date of September

2006.

At this time I would like to turn it over to Dr. Shack.

MEMBER SHACK: Thank you.

Most of you are familiar with the Argonne test facility. It's a fluid loop. We have essentially a horizontal sump screen. And essentially we've been trying to measure a local head loss if you're trying to relate this to the overall integrated problems. We're not intending by this loop to represent a sump configuration. What we really want to do is have a controllable bed geometry and understand the head losses across those beds.

The perforated plates I'll be talking about today, we've used two types of perforated plates. The one for the test today has a 40 percent flow area with staggered 1/8th inch holes. It is our understanding that is the kind of sump perforated plate that's used for most of the modern sump screen designs.

CHAIRMAN WALLIS: I think the newer ones are having smaller holes.

DR. SHACK: Well, there may be newer ones --

CHAIRMAN WALLIS: The anticipated ones

now.

DR. SHACK: This was as of September or so.

CHAIRMAN WALLIS: As of when? Yes.

DR. SHACK: As of then.

Just a brief review for some of the work that we did in the past with the ICET-3 environments.

These are plants that use trisodium phosphate for pH control after an accident. In the ICET-3 test in which the TSP buffer was used with a CalSil insulation, calcium phosphate precipitates were observed to form. We had extensive discussions with the Subcommittee on these environments in February. We noticed that the head losses from the chemical products were greater than we got from an equivalent amount of the CalSil just as precipitates. So, in fact, the chemical effects were worse than simply the precipitate loss, which is bad enough with CalSil.

One of the other interesting concerns was there's some question about just how these will form, where the dissolution of the CalSil will take place and where the formation of the chemical product will take place. We did some tests where we assumed that there would be a significant amount of dissolution before the bed was built. And then we looked at a

test in which there was no significant amount of dissolution before the bed. And what we did find is we had the dissolution before the bed was built, which we think is the more prototypical situation since you don't start resump recirculation immediately. You've got a high head loss quickly. If you waited and let the chemical reaction occur after the bed formed, you essentially got to the same total head loss, it just took you a little longer as the chemical process went on.

We noted that for a given CalSil loading, the head loss could be highly non-linear, a monotonic function to the fiber loading. You could saturate thinner fiber beds to the precipitate.

And although ICET itself probably has a nonprototypical level of CalSil, again although it was established with industry cooperation, even for more realistic concentrations of CalSil down to .5 grams per liter, you can get the 75 ppm of calcium. You've got plenty of phosphate where essentially calcium limit in these things, you've added enough phosphate as your TSP buffer, you're going to take up all the calcium that you can dissolve whether it's from the CalSil or the concrete and turn it into calcium phosphate. But with relatively small amounts of

CalSil you can get fairly large amounts of the calcium phosphate precipitate.

The next interesting set of environments are ICET-1 and ICET-5. And ICET-1 is a sodium hydroxide buffered sump with primarily NUKON insulation. And ICET-5 is a sodium tetroborate buffer with NUKON.

CHAIRMAN WALLIS: Now, Bill, last time when we were talking about the previous tests we got some quick look reports to look at. I haven't seen any on this stuff.

DR. SHACK: No. Since we're working on the final NUREG, you know we haven't produced an intermediate quick look report.

CHAIRMAN WALLIS: Haven't had those? Yes. So there's nothing that I can get to?

DR. SHACK: There's nothing that you can get your hands on here.

CHAIRMAN WALLIS: All right.

DR. SHACK: As Marc discussed, what's characteristic of both of these environments is that we got significant dissolved aluminum levels, about 375 ppm in ICET-1 and about 50 ppm in ICET-5. When we cooled the ICET-1 solution, we produced visible precipitates and the volume and rapidity of the

formation of these sort of increase.

CHAIRMAN WALLIS: You say "when we." You mean when they?

DR. SHACK: They. The scientific community.

CHAIRMAN WALLIS: You didn't redo the experiments?

DR. SHACK: We didn't redo the experiments.

The cooling of the ICET-5 essentially produced much smaller amounts of precipitates, but they did do it.

We have these aluminum hydroxide emulsions of various forms. I think Marc convincingly demonstrated that these are amorphous forms, and I take the simple minded approach. That I have a factor of 400 difference between the amorphous form and the crystalline form. If I've got this much aluminum floating around in this solution, it's got to be in the amorphous form. Because if it was in the crystalline form, it would be in a rock. You know, an extraordinarily super saturated solution.

And again, coming back. One of the surprising things was, you know, if you looked at other products that could be forming. One of the

candidates beforehand was essentially the silicates. And as Marc pointed out, one of the fortunate things here is in the tests where you have lots of silicates, you have little aluminum, which is the ICET-4 kind of environment and in a test like ICET-1 where you have lots of aluminum, it inhibits the dissolution of the NUKON to form the silicate. So we don't really see significant amounts of the aluminum silicates.

We were kind of curious about how much aluminum it took to inhibit that NUKON dissolution. And if we took a little bit -- you know, if we had a more realistic submerged aluminum level, would we get the same sort of benefit. And we did some small scale soaking tests where we had a greatly reduced ratio of NUKON volume to aluminum surface area that we thought was much closer to prototypical. And in fact, that still inhibited the silicate distribution. So although we saw some formation of the aluminum silicates, we still believed that it primarily is aluminum hydroxide type solutions that are going to be the principal chemical product that we're going to have to worry about.

And again, ICET-1 had a very specific kind of submerged aluminum level. You know, they got an amount of dissolved aluminum that's a function of

their environment and the amount of aluminum they had available to corrode. And the actual plant levels of dissolved aluminum with the same environment would roughly scale with the amount of aluminum exposed, which is plant specific.

We tried to look at sort of a range of aluminum levels that we might expect to get. And again, the estimates here of the amount of aluminum, and you're looking at your aluminum essentially as the amount of volume of sump fluids you have so it's sort of surface area per volume of sump fluid. In good English units its foot squared per cubic foot.

Then, of course, you're interested in what fraction of that is actually submerged because that's going to be corroding for the whole mission time. And then which portion of that is only wetted during the spray period. Because that's only going to be corroding for, you know, maybe four hours or so while the sprays are active.

And I just went through some calculations here. And, again, you can see a wide range in ratios of sort of aluminum to the volume that's submerged. Most of them tend to be fairly small. We have one --

CHAIRMAN WALLIS: That doesn't look right. I mean 3 feet squared by -- it seems to me that -- I

mean if I have a cubic foot of water and I try to put three square feet of aluminum on there. This just doesn't sound typical. I mean, the plant must be made of aluminum. It doesn't make any sense.

DR. SHACK: Ralph is going to explain this.

MR. ACHITZL: Ralph Achitzl, NRR.

And I'm not positive, but I believe there is at least a couple of plants that have aluminum RMI. Aluminum R would have a very high surface --

CHAIRMAN WALLIS: With a lot of aluminum at each -- that's where it is.

MR. ACHITZL: I think. I'm not positive. I believe --

CHAIRMAN WALLIS: That must be where it is.

MR. ACHITZL: I believe a very high plant would be an aluminum RMI situation.

CHAIRMAN WALLIS: So the sump is full of this stuff then of aluminum debris? I mean, literally full. And that amount is a huge amount.

MR. TREGONING: Yes, I think we do have plants -- at least some evidence that Plant R does have aluminum RMI.

CHAIRMAN WALLIS: Insulation. Okay.

DR. SHACK: Now it may also be true that Plant R doesn't have sodium hydroxide, a buffer. I just took these results from the survey. I have no idea whether these plants actually use sodium hydroxide as a buffer or not. I was using this more as a representative range of aluminum levels that I might find in plants that do use aluminum hydroxide as a buffer and seeing what kind of levels I would get.

MEMBER KRESS: This is with no passivation?

DR. SHACK: And in here I've assumed no passivations. This is a slightly conservative assumption.

The one day total is in fact typically dominated by the amount of aluminum that you have available to be wetted by the sprays. So what you have going for you early on, you've only got four hours but it's occurring at high temperature and relatively high pH. So you can get fairly large contributions. And, in fact, I may be underestimating these because I used an activation energy for corrosion from the Center data. The industry data for aluminum corrosion has a somewhat higher activation. So it would roughly double my one day totals if I used essentially the results of the industry.

CHAIRMAN WALLIS: Well, that number of 3.36 feet squared -- looks higher than the aluminum in any ICET experiment.

DR. SHACK: No.

CHAIRMAN WALLIS: Number R? Is it really true? They actually managed to stuff in that much aluminum?

DR. SHACK: I only know what is reported in the survey. It's not all submerged. You know --

CHAIRMAN WALLIS: Oh, it is. Okay.

DR. SHACK: You know, if you look at -- although again the fraction that's submerged in plant R is extremely large.

CHAIRMAN WALLIS: Mmm. Okay.

MEMBER KRESS: What temperature did you assume in these calculations?

DR. SHACK: What I did was a temperature history. I have essentially sump time temperature profiles for a Westinghouse 3 and 4 loop plants and Westinghouse ICET condenser plants. So B and W and CE plants became 3 loop Westinghouse plants for these time temperature histories.

MEMBER KRESS: Well in your calculations did you stairstep the temperatures?

DR. SHACK: I stairstepped, yes.

MEMBER KRESS: So it's an activation energy and --

DR. SHACK: I mean, I did a continuous temperature history, yes. I just integrated the differential equation.

MEMBER KRESS: Oh, you took the differential equation?

DR. SHACK: I just did the differential equation and just integrated it over the time temperature history with the corrosion rate essentially decreasing with the temperature.

MEMBER KRESS: Is that a reasonable thing to do? Because most of these activation energies are done at constant temperatures.

DR. SHACK: Well, yes, we always do that with activate -- you know, we measure them in an isothermal situation and then we apply them to a rapid transient situation.

MEMBER KRESS: That under estimates or over estimates? It considerably over estimates, doesn't it?

DR. SHACK: I'm not going to make any general conclusion about that. That's what I did. If you don't like it, you have to do another calculation.

MEMBER KRESS: Okay.

MR. TREGONING: Hey, Bill, Rob Tregoning.

Another point of clarification. Did you look at any contribution due to condensation effects beyond the spray phase of four from --

DR. SHACK: No.

MR. TREGONING: Okay. So no aluminum outside, there's no contribution --

DR. SHACK: No aluminum outside. Once the spray stops --

MR. TREGONING: It's done?

DR. SHACK: -- it's done.

MR. TREGONING: Okay.

DR. SHACK: Because I think the condensing solution would be relatively neutral pH and a relatively neutral pH doesn't dissolve a whole lot of aluminum.

MR. TREGONING: You don't think there could be left over films that effect the pH of the condensate?

DR. SHACK: You know, I'm not sure how long it would wash off and how long it would take to dilute that. But it's certainly true that I did not consider any corrosion during the condensation stage.

CHAIRMAN WALLIS: The 5000 parts per million of aluminum in the sump is a large mass, is it

not?

DR. SHACK: Yes. Well, clearly you're not going to get there. You know, if this plant was using sodium hydroxide and if all these numbers were true --

MEMBER KRESS: It'd hit the sump right away.

DR. SHACK: -- you know you'd turn to Jell-O here before you got the 5000.

CHAIRMAN WALLIS: The sump would turn to Jell-O?

DR. SHACK: The sump would turn to Jell-O.

The conclusion here is that the ICET-1 level is expected to be conservative, and we sort of expect most plants with sodium hydroxide buffering to be at about 100 ppm or maybe a little lower. But a 100 ppm is certainly a level we have to consider is roughly what I wanted to get to from here.

Because of again, the ICET-1 submerged volume of aluminum is pretty conservative for everything except the infamous plant R, we think the ICET-5 aluminum level is similarly conservative, again with most plants that have the sodium tetraborate buffer would not have 50 ppm aluminum. They'd be more like 15 ppm aluminum.

MEMBER KRESS: The designation of your

plants, is that a code to it or can I look at it and figure out which plant it is?

DR. SHACK: No. I think this is meant to be blind.

MEMBER KRESS: Blind?

DR. SHACK: Now, again, as we point here the pH of interest -- and again I've only looked at the neutral pHs because the acid pH is a very short phase of this whole process. And we're up on the alkaline region here for almost the whole duration. So my curve did go back up again the way Marc's did if I showed the whole pH range. But I'm only interested in this range from 8 to 10, which probably covers the pHs that I'm really interested in.

And, again, I have an amorphous solid of some form in equilibrium here and I'm looking at the amount of aluminum that I can have in solution. At about 9.6 or so, I'm at about 1-- ppms. So at the kind of retemperature conditions at a 100 ppm I'm just about at that solubility limit. And that's not inconsistent with what was seen in ICET-1.

If you look at the turbidity results in ICET-1, they start going up about the time you get to a 100 to 200 ppm of solution.

Although this data doesn't take into

account the boric acid. You know, there's no boron complex in there. This is just literature data on solubility in water. It doesn't seem unreasonable to compare with the ICET-1 results that sort of indicate that about 100 ppm at room temperature you're starting to have things come out.

CHAIRMAN WALLIS: How does this compare with your 5000 on the previous slide?

DR. SHACK: Well, at 60 C or 140 F you'll see I can have about a gram and I can have a load of --

CHAIRMAN WALLIS: Because you haven't got 5000?

DR. SHACK: I haven't got 5000, no. As I say if I had a sodium hydroxide in that plant with that amount of aluminum, you know, I'd never get here. But as Marc pointed out, we can dissolve an awful lot of aluminum at high temperatures in these plants.

The other thing I wanted to point out in my simple minded way is that if I look at this equilibrium solubility, I can have essentially an equilibrium between an amorphous solid and a crystalline solid. And if my crystalline solids, if it's a gibbsite, it's about a factor of 400 in solubility between the amorphous form and the

crystalline form. With Bayerite it's more like an order of magnitude. And again, these re essentially pure water results. The solubilities can be influenced by the borates, organics and, as Marc mentioned, we just have uncertainties in the basic thermodynamic data. If you look at different literature results, you can get different predictions of the solubility.

We used essentially the aluminum nitrite additions to generate our emulsions. And what I'm trying to show here are essentially some colloids that we generated with 100, 200 and 370 ppm additions. So this gives me aluminum levels of ICET-1 down to the 100 that I think is sort of typical of most plants.

And the difference in the two photographs, one is taken with the flash on so you get more realistic looking appearance here on the right.

CHAIRMAN WALLIS: We're just seeing a precipitate, is that we're seeing?

DR. SHACK: Yes. Here you can get a better measure of the volume of precipitate with the 375 down to the 100. And over here, you know, it looks sort of solid white. But over here you get this more gel-like colloidal kind of suspension.

CHAIRMAN WALLIS: Well, a large fraction of

what's in the jar.

DR. SHACK: At 375, yes, you do get a substantial amount of stuff. It's a lot filmer and a lot more difficult to see at 100 ppm, but it's certainly there.

CHAIRMAN WALLIS: That's after it's cooled?

DR. SHACK: Yes. Marc showed his ICET-1 after the 4 months at ambient temperature in which he had a 10 micron particle distribution.

DR. KLASKY: Marc Klasky, LANL.

That figure that shows the particles that are roughly what? Ten micron or so, I think we need to discuss that further.

The measurement technique that's utilized I don't think can very accurately predict particle sizes that are greater than a micron. And it has to do with the fact that the concentrations-- you have to dilute the solution, otherwise you got multiple scattering events and this technique requires that you don't get multiple scattering events. So you dilute it, and the fact of the matter is when you dilute it, then at very low concentrations you have the opposite effect. That if you look at a volume, basically the fluctuations, which is what we're measuring, are

dominated by what's flowing into that control volume.

So at the concentrations that are really necessary to make that measurement, they're really dominated by, in essence, noise. So we can through it, but I think that those measurements above a micron are very uncertain. And so we choose to basically delete them. We attribute them to noise and not actual particles. And, in fact, we have in a series of tests we actually filtered the solution at about a micron and we still observed those particles which, you know, gives you that indication that they're not real.

I'll just leave it at that.

MEMBER KRESS: But does that apply to the bottom curve also, to the second mode? Would that be noise?

DR. KLASKY: I can't see the scale on that.

DR. SHACK: No. This is your half micron particle.

DR. KLASKY: Okay. Okay. Generally I would just leave it at anything above a micron is very suspicious and warrants much further investigation with dynamic light scattering.

MEMBER KRESS: Yes. Well, I'm still trying

to understand why you get a bimodal distribution of colloids, but --

DR. KLASKY: The polydispersity of the system, that's another issue which also is difficult. And I don't know that I'm prepared to answer the polydispersed nature of the system at this point.

MEMBER DENNING: You've never attempted to use different cooling rates to see whether that impacts the size distribution or things like that?

DR. SHACK: No, we haven't done that.

Now, again, this is particle size distribution for some of our simulation products after being cooled to room temperatures. When we just take the product and we look at it, we get a peaky sort of distribution, not quite bimodal the way Marc gets it. But we do see a kind of peak distribution.

CHAIRMAN WALLIS: I don't quite understand this. I thought we were told this morning that these were nanometer size particles and now you're telling me they're micrometer sized particles.

MEMBER KRESS: Divide by 100.

CHAIRMAN WALLIS: No. But I mean, the scale is a 1,000 or a 100 different or something. I mean, this morning we heard they were all itty-bitty teeny-weeny nanometer sized particles.

DR. SHACK: We believe that we're looking at agglomerations here, you know, in these. Again, when we essentially agitate these with a little ultrasound, you'll see we suddenly have aggregated a lot of these particles. You know our --

CHAIRMAN WALLIS: But they're still microns, aren't they?

DR. SHACK: We're down to micron sized particles, which are not so different from Marc has half micron particles. And, again, I believe he's actually done some agitation of his also in a flow systems.

So when we look in the TEM we sort of see the similar thing; that there are nanosized salmon eggs and then they sort of sifted in these agglomerations. Just how sticky the larger agglomeration is isn't clear to us and exactly what particle size you should be using isn't clear because, again, you do go all the way -- you know, it's clear from the TEM that the fundamental particle is sort of nanosized --

CHAIRMAN WALLIS: The nano is 10 to the minus 8 meters, is that it.

MEMBER KRESS: That's 9.

CHAIRMAN WALLIS: Nine. So it's a factor

of 1000.

DR. SHACK: Yes.

CHAIRMAN WALLIS: So when you're talking about 30 nanometers -- I thought that was right. This is .03 microns. It's very much smaller than you're showing here, isn't it?

DR. SHACK: Yes. Yes. But, again, if you recall Marc's TEM pictures, he has essentially salmon eggs in clusters. So we're looking, you know what we're seeing here are the clusters. And what we've done hasn't disaggregated the cluster completely. We can break those clusters up in some ways.

So I know it's very difficult to come up with the exact notion of what a particle size is here because it may depend on the agitation. It may depend on the flow rate as you're going through the bed as these things impact together and can agglomerate or disagglomerate.

CHAIRMAN WALLIS: The basic size of the unit would seem to be this 30 nanometers. And the fact that they make a cluster is okay. But the basic size of the unit --

DR. SHACK: The basic size of the unit.

CHAIRMAN WALLIS: -- is this tiny size.

DR. LETELLIER: This is Bruce Letellier,

LANL.

Even the basic size is rather ill defined because it's a hydrated sphere. And those spheres interact with their neighbors. And so it's hard to find a discreet definition of size, at least from the point of view of fluid flow through a packed bed. It just doesn't behave the same. And it's my opinion, perhaps speculation, that these agglomerations rather than filtering the flow, they obstruct the flow. They represent a blockage and those interspatial spaces between nanometer sized particles do not participate. It's stagnate. But that's speculation at this point.

CHAIRMAN WALLIS: Well, they could wonder around the loop until they agglomerated enough to get stuck.

DR. LETELLIER: Of course. And I'm sure they do.

DR. SHACK: But it just makes it hard to come up with a fundamental partial size to deal with here.

MEMBER DENNING: Well, what's the fiber diameter for NUKON? Was it 1 micron?

DR. LETELLIER: Seven.

MEMBER DENNING: Seven microns?

CHAIRMAN WALLIS: This raises questions

really, as we had this morning. What's representative for a test in using a surrogate? What would you say is representative spectrum of particle sizes?

DR. KLASKY: Mar Klasky, LANL.

One thing I'd say, and Bill maybe you'll get to this or you've already alluded to it, and that is you've observed head loss prior to seeing actual precipitate, which sort of lends itself to this notion that the particles in fact at that point are too small to see. So what's visible? A micron. So the aggregates apparently are small enough up to that point to still cause substantial head loss, right?

CHAIRMAN WALLIS: Well, what I'm getting at is how is NRR going to evaluate all these different tests? How are they going to decide that this particle size that shows up in the test is representative of the plant? It seems to me the plants are all so different and all these vendors are going to test, filter test their screens for each plant, aren't they? How are you going to know that it's a representative test? Are you going to say look at the particle size and say here that it's like ICET, so it's okay or what are you going to do?

MR. KLEIN: Paul Klein from NRR.

I think probably what we intend to do is

work with Westinghouse since they're building the fundamental building block of generating the particles and assure ourselves that we're comfortable with the direction they're providing to the screen vendors on how to generate the chemical surrogate.

DR. SHACK: Yes. I think you need to return to this after we look at some of our head loss results.

CHAIRMAN WALLIS: Okay. So sometime later in the year, maybe November or something, NRR is going to say we now know how to have some criteria for evaluation for these tests?

MR. KLEIN: We can discuss that tomorrow.

CHAIRMAN WALLIS: Okay. But from what I've heard so far, it mostly seems to be in the future. I mean, you know some of the questions to ask, but you don't know what specific physical criteria you will use yet? Okay.

DR. SHACK: Now our first attempt at a simulated test we ran to match the 375 ppm aluminum in ICET-1. and in this particular case, as Marc suggested, perhaps we were not careful enough in making our surrogate. And when we introduced our surrogate, what we in fact show was a snowfall. You know, if you recall my previous graph, 375 ppm should

be soluble at 140F. You know, I should be able to put in up three times that amount.

Now what I had when I did my introduction, of course, is when I introduced my aluminum nitrate I lowered the pH. As you'll notice again, that's a log scale, so my solubility is dropping exponentially. As I drop the pH locally, I form a product. So we saw heavy snowfall.

Now within 2 minutes that snow disappeared and we could see nothing in the loop.

CHAIRMAN WALLIS: So blue is the temperature?

DR. SHACK: Blue is the temperature.

CHAIRMAN WALLIS: Red is the?

DR. SHACK: Red is the head loss or pressure drop.

CHAIRMAN WALLIS: Pressure drop. I noticed you called it by an appropriate term.

DR. SHACK: We tend to say pressure drop, that's true.

But what's interesting here is that in this particular test the pressure drop started to increase even at 140^{o}F, even before we started lowering the temperature. But, again, I think you would have to argue that there's some questions about

this test. We created too much of the precipitate to begin with. But, again, you're getting this head loss with no visible build up of product on the bed. All the snow that we have generated has dissolved and disappeared.

CHAIRMAN WALLIS: Well, the pressure drop originally, without adding this, from the fibers alone looks like something like .2?

DR. SHACK: Point 3. Yes.

CHAIRMAN WALLIS: Three. So it's gone up by a factor of 20 when you finish the test?

DR. SHACK: Yes. Well and in fact if I could keep my velocity moving, it would probably be up even more than that.

CHAIRMAN WALLIS: Are they constant here?

DR. SHACK: I'm trying to keep the velocity constant. As my pressures are building up here, I can't control the velocity in my loop any longer. So, you know, this is a minimum increase in head loss that you're looking at here. I'd have to show you my velocity versus pressure drop profile so that you could know exactly where --

CHAIRMAN WALLIS: But it's roughly .1, roughly through this whole test?

DR. SHACK: Right. You know, that was the

attempt was to keep it at .01.

CHAIRMAN WALLIS: Until the near end. Okay.

DR. SHACK: And again, this is a picture of the snowfall --

CHAIRMAN WALLIS: -- by a factor of 20. That means that essentially all the pressure drop is due to this aluminum?

DR. SHACK: Yes. Again, there's the snowfall, definitely nonprototypical. But again, you can see it dissolve.

CHAIRMAN WALLIS: Well, if there's a snowfall, it's pretty big size particle?

DR. SHACK: Yes. You know, again, there are agglomerations of things, but you know fundamentally they dissolve, again they disappear. But we're still getting pressure drop.

Now later we did a repeat test with the 375 ppm aluminum that representative of ICET-1. Here you can see the aluminum addition at high temperature. We had a very small snowfall here and you see this little tiny spike in pressure. Now that may somehow be associated with the snowfall. But, again, that dissolved and we're seeing no pressure drop here, although we're now decreasing temperatures. We're

going along. We're down to 100 degrees C --

CHAIRMAN WALLIS: There's no visible precipitates circulating?

DR. SHACK: There's no visible precipitates circulating.

Now one of the difficulties between the last test and this test, of course the previous test was run in a Lexam chamber, which is wonderful because you can see what's going on. Unfortunately, in the presence of the sodium hydroxide it cracked to bejesus. And so we've switched now to a PVC chamber, which is says it's clear, but what it really means is it's kind of a smokey looking plastic. So, you know, there undoubtedly could be some changes in turbidity here that we can't really see through the PVC. It's just not clear enough.

But, you know, we're coming down. We're sitting here at 100 C and then it begins to take off. And again, once the pressure begins to increase. But there's no visible product in this test. However, if we --

CHAIRMAN WALLIS: Did you look at the bed afterwards and see what was there?

DR. SHACK: Yes. When we stopped this test, we just let the loop sit overnight before we do

anything. We collect, essentially, a large amount of Jell-O above the bed.

CHAIRMAN WALLIS: Above the bed?

DR. SHACK: Above the bed. So it's been circulating in the loop. You know, the aluminum hydroxide is a transparent thing in a very fine precipitate colloidal form, so --

CHAIRMAN WALLIS: The Jell-O on top of the bed or inside it?

DR. SHACK: Well, it's -- I'm sure it's inside the bed, but it's also stacked up.

CHAIRMAN WALLIS: Well, there's enough of it to be stacked up on top of it?

DR. SHACK: On top of the bed. I mean, we're talking 6 inches. You know, we're not looking at thin cakes here. We're looking at a quite substantial bed. Zero flow. And it just collects there.

CHAIRMAN WALLIS: And when you have a flow, doesn't it compress then?

DR. SHACK: Well, if we could actually start the flow with the Jell-O on top, we could probably compress it.

CHAIRMAN WALLIS: The Jell-O completely jams it up solid?

DR. SHACK: The Jell-O has completely jammed it up solid. Yes, we know. WE were jammed up as far as the head went long before we could see the Jell-O. But at least we knew there was a large mass of precipitate in the loop that we could not visibly see, but we could collect at the end of the test.

MEMBER KRESS: In the process of this aren't you generating a large amount of hydrogen?

DR. SHACK: No. No. There's no corrosion going on here. You know, we've just added the aluminum as aluminum nitrate and so we're just looking at the precipitation product.

MEMBER KRESS: But in a real plant you would be generating hydrogen?

DR. SHACK: You would be generating hydrogen.

MEMBER KRESS: And would that not tend to break up this cake? I mean, there's a lot of hydrogen.

DR. SHACK: No, but the hydrogen's being generated at different -- you know, it's being generated off where you're putting the aluminum into solution. The cake, if it was building up, would e building up on the sump screen, you know, in a very different location.

MEMBER KRESS: Okay.

MR. TREGONING: Although we have looked at effects of hydrogen or we've talked about effects of hydrogen and hydrogen peroxide on the redox potential of the solution. And that can certainly affect agglomeration as well as other aggregation types of mechanisms.

CHAIRMAN WALLIS: Well if you had all this hydrogen made in the sump, it was sort of attached to the particles or it's in the water, and then you recirculate it, presumably the hydrogen could collect in the reactor.

DR. SHACK: No. The hydrogen partitions off, I assume.

CHAIRMAN WALLIS: Well, but I mean where does it do it? Does it do it in the sump or does it do it in the rector? Some of it comes out in the reactor. In the head? No, it doesn't go out the break necessarily unless it's carried -- it could go up into the head. It depends upon a lot of things. Where the break is and the flow rates and everything.

Okay. So hydrogen might be a downstream effect to think about. When we get to downstream, NRR, we might think about what the hydrogen is doing.

DR. SHACK: Yes, downstream of this

presentation.

CHAIRMAN WALLIS: It's not your concern.

MEMBER KRESS: But what's your p1 and your p2?

DR. SHACK: Those are two different transducers. One of them, the p1 transducer is about 3 inches from the bed above and below. The p1 transducer is about 12 inches below the bed. So you'll see the p1 is typically or a noisier signal than the p2. But they should basically read just about the same. You know, we get transducer drift in occasional tests. But it's just sort of a check to make sure we're not getting too much transducer drift to have both of them there.

CHAIRMAN WALLIS: But you don't look at the effect of hydrogen bubbles on the pressure drop in the bed then?

DR. SHACK: No.

CHAIRMAN WALLIS: You don't have hydrogen bubbles? But if they were small enough, they could presumably get trapped in the bed?

DR. SHACK: Yes. I really wouldn't expect them to be there. I would have expected them to --

CHAIRMAN WALLIS: Yes, but when you do experiments in sort of flow through porous media and

look at the pressure drop, you got to be very careful that you don't have gas in there because it really increases the pressure drop.

DR. SHACK: We tried to repeat the test with 100 and 200 ppm, and these were our first attempts at doing that. And --

CHAIRMAN WALLIS: Didn't make enough Jell-O?

DR. SHACK: Well, we didn't see any real effect on the pressure drop. You know the variation we see here is almost the -- sort of the temperature variation of the head loss, you know, as we heated up the pressure drop drops, as we cool it down again the pressure drop goes back up.

These were probably just too short term a test.

We then went to a longer term test. We added the aluminum back here at 257 minutes. Things were going along, basically nothing was happening. So we added nitric acid to the loop to decrease the pH by .2 units. And, again, that makes a difference in solubility. You'll see that again as soon as we did that we began to see rapid increases in the pressure drop. Again, no visible product. You know, you couldn't see anything happening except once you

dropped the pH to .2 units, the pressure drop went up. We didn't have to add very much. Just a drop at .2 units.

And, again, it gets more instructive to look at this in terms of temperature. You see the cycles here. We didn't want to leave the cooling on overnight so that the loop would heat up and cool down here in cycles. That didn't seem to have too much effect. Once we get to having formed product, there's a significant correlation between the temperature drop and the pressure rise. That is, as we changed the temperature, we could put the product in and out of solution.

CHAIRMAN WALLIS: Well, this is many days of --

DR. SHACK: This is many days.

MEMBER KRESS: That's the difference between day and night?

DR. SHACK: There's a difference between day and night. When we didn't have enough nerve to leave everything on overnight. You know, as we were approaching the end of the program and we didn't worry about whether the loop would be there the next week, we got nervier about, you know, letting everything run and taking our chances. So you'll see some more

controlled tests coming up.

MEMBER KRESS: What's your source of heat? Steam?

DR. SHACK: No. Electrical heat.

MEMBER KRESS: Electrical heaters?

DR. SHACK: Electrical heaters.

We did a third test with a 100 ppm. And this time we're going to add some nanometer particles to try to essentially initiate the precipitation. We thought we'd get --

MEMBER KRESS: What kind of particles were they?

DR. SHACK: Aluminum oxide. This is what we used in our patented Argonne sunscreen that we make money from.

MEMBER SIEBER: Oh.

DR. SHACK: Our first application of nanoparticles, revenue generating for the Lab.

MEMBER SIEBER: Is that an outgrowth --

DR. SHACK: It's an outgrowth of earlier research work. We take our money where we can get it.

MEMBER SIEBER: It sounds like some kind of hair dressing.

DR. SHACK: It's sunscreen.

Again, it didn't do very much. But if we

waited long enough, and again these are time scales that are long compared to a --

MEMBER KRESS: A LOCA?

DR. SHACK: -- laboratory test, but it's certainly not within the mission time of the sump, you know as the sump is aging and cooling down, you know, these times. And, again, we began to see a rapid increase in head loss. No visible product here during the test. And in this case after we let the loop settle overnight, we didn't collect any visible product on top of the bed. However, when we took the samples out and we let them age for a while, we did collect a colloidal product --

CHAIRMAN WALLIS: Well, you seemed to have dropped the temperature also. It's not just a question of aging.

DR. SHACK: No. No. You know, we have gone down to a low temperature.

DR. SHACK: Is that's what's caused, do you think, this rise in pressure drop or is that --

DR. SHACK: Oh, no, no, no. The --

CHAIRMAN WALLIS: What's causing the pressure drop to take off?

DR. SHACK: It's essentially, I think, a precipitation process that --

CHAIRMAN WALLIS: And it takes a week to start?

DR. SHACK: Yes. Well, it takes a week to agglomerate, to build up.

MEMBER SIEBER: Build up, yes.

DR. LETELLIER: Bruce Letellier of LANL.

I think Bill's right. It's just the competition of bed penetration versus increasing filter efficiency. We've talked about impaction and agglomeration on particles. And eventually that reaches a crises point where it's very -- it becomes a very effective filter and the head loss increases rapidly.

CHAIRMAN WALLIS: So you're going to require this --

DR. SHACK: No. There's still a question I think of whether there is an additional aging thing goes on. If you look in the ICET tests, although they got to the 375 at day 15, if you look at the data right you see an increase in things like the viscosity that goes on after that. And in essentially their readiness to form the precipitate increased, although they'd gotten to a constant level, you know, in one case they had to wait a little bit before the precipitate would form when it started to cool down.

In other cases, it's almost the instant they took it out of the loop and it began to cool at all. So I think there is a couple of processes going on here.

One is an aging and development of the colloidal suspension itself. In this case I think the larger one is probably the build up on the bed, but you know there are multiple processes going on here.

CHAIRMAN WALLIS: So is there a predictive tool which can tell you when this is going to take off, this pressure drop? Sometimes it happens in less than a day, sometimes it takes a week, sometimes it seems to take more than a week if you look at these different graphs.

MEMBER KRESS: Yes. In that test it looks like you get a change in mechanism in phenomena at around 7500 minutes.

CHAIRMAN WALLIS: Yes, but on the next figure it goes to --

MEMBER KRESS: It's a sharp change there. And, you know, if it were just a build up -- if it were just a build up on the filter, you expect a smooth transition.

DR. SHACK: No, I think it's a kind of a threshold thing that once you begin to get the loss, then you can really begin to build it up.

MEMBER KRESS: Yes, but I wouldn't expect to see a break like that.

DR. SHACK: Well, I mean we saw that with the 375. You know, all these tests are in the sense that it's going along very, very slowly. But once it begins to take off, it takes off.

CHAIRMAN WALLIS: So what does this mean for a sump again? I mean, are these guys going to have to do a test for months until they see if it takes off?

DR. SHACK: Well, I mean my first conclusion is I really don't want my sump to have a 100 ppm of aluminum in it.

CHAIRMAN WALLIS: Because you know that in about a week you might have this phenomena. But suppose you had 50 ppm? Do you have to wait two weeks or I mean do you have any idea.

DR. LETELLIER: I can substantiate the aging process. In this afternoon's talk we'll look at solution from test 5 which had about 50 ppm. And after 60 days post-test it did show signs of measurable head loss.

CHAIRMAN WALLIS: That's two months.

DR. LETELLIER: After the ICET experiments.

CHAIRMAN WALLIS: But these contractors who are designing and testing these screens for 69 plants are going to have a lot of difficulty running tests that take weeks for each plant. Especially if there are lots of different tests they have to run.

MR. TREGONING: Well, if you go back, aging is certainly an important issue.

CHAIRMAN WALLIS: Maybe they just don't have any aluminum, so they don't worry about it.

MR. TREGONING: Well, there is a small temperature drop that occurred that does coincide with the onset of --

CHAIRMAN WALLIS: But we're speculating about things. We're not saying --

DR. SHACK: No. Again, we can go back to our solubility curves. Again, if you look at the one I have a pH curve and I have a temperature curve. Obviously, you know, the lower you drop the temperature the less aluminum you can tolerate. The higher the sump that remains. So this --

CHAIRMAN WALLIS: Well, what I'm learning here is what I've learned from all these presentations is that when -- and RES does the research, they learn something. And they learn something we didn't know. And so when you stop doing RES doing research, you

stop this learning process.

Okay.

DR. SHACK: Okay. We went to --

CHAIRMAN WALLIS: And these are significant things.

DR. SHACK: -- the ICET-5 environment. And, again, the thing that I want to focus on here is that ICET-1 and ICET-5 are equally aggressive in terms of having a large amount of submerged aluminum. So you know it's a little unfair to look at comparing the dissolved aluminum levels. What you really want to say is I have a sodium hydroxide environment and a sodium tetroborate environment with the same submerged aluminum levels, I get different amounts of aluminum. Since I'm at different pHs, that doesn't necessarily mean I'll be better off. But when we look at the tests we do see some interesting results.

And in this particular case we went along in the test and, again, after 5 days or 6 days, you know, nothing was happening. So we thought we'd go through with our pH kick again. You know, that did wonders the last time we tried it, so we --

CHAIRMAN WALLIS: Nothing happened.

DR. SHACK: --decreased the pH .2 and, again, nothing happened. We actually also added some

nanoparticles about that time. So we gave it the double kick; the nanoparticle addition and the pH kick. And nothing really happened.

Again, the test program was coming to an end so we thought we'd sort of see just how much aluminum we could tolerate. So we added enough aluminum to get to almost 100 ppm total.

CHAIRMAN WALLIS: Wow. This was only 100 ppm, though?

DR. SHACK: Yes.

DR. SHACK: Well, a 100 ppm is more than enough. And now we're at 100 ppm at a pH of 8.5. If you go back to my solubility curves --

CHAIRMAN WALLIS: But previously 100 ppm took a week before --

DR. SHACK: But that was at a pH 9.6.

CHAIRMAN WALLIS: Different pH.The velocity of the fluid --

DR. SHACK: Again, this is really unfair. I mean, this is a plant R kind of submerged aluminum level.

CHAIRMAN WALLIS: This is all at .1 feet second?

DR. SHACK: This is all at .1 feet per second.

CHAIRMAN WALLIS: Don't know if these colloids might stick much better at .2 or .01 for the second? We have no idea.

DR. KLASKY: Marc Klasky, LANL.

Bill, I think an issue here is we showed or previously I showed the hydration associated with the amorphous form. And I guess the question that comes into play here is the aluminum that you're adding and the means by which you're adding aluminum into the system, do you think on the surface you're forming a gibbsite or a crystalline structure or how exactly is the aluminum introduced to basically preclude that initial formation of the crystalline material?

DR. SHACK: If it was an equilibrium with the crystalline form, I would expect an equilibrium concentration of about .2 ppm of aluminum. It's such a night and say difference between the amorphous and the crystalline one that I think I have to be primarily in equilibrium with an amorphous solid to have anything left in solution.

If I was really forming significant amounts of the crystalline material, I'd be precipitating aluminum like a rock. And it would be coming out. So, you know, I'm arguing from that basis

that it has to be primarily amorphous, otherwise I would have no aluminum left.

DR. KLASKY: Right. I guess my point, though, is that the crystalline form, although it precipitates more readily, doesn't have the hydration and has much less of an effect on the head loss or pressure drop. If you are precipitating, that which precipitates first may have very little effect. It's the hydrated form that has the large effect on the pressure drop. There's a difference in precipitate A and B. I guess that's the point that I'm trying to make. It's very much dependent upon how large this effective diameter is. And if it's crystalline, it's not very large. And so one could put a lot more aluminum, presumably, on the bed as opposed to something that ties up a lot of water.

I guess we need to talk.

DR. SHACK: That's true. But, again, I wouldn't have anywhere near this much aluminum in solution if I was in equilibrium with a crystalline solid. It would come out like a rock. And so I think it has to be primarily an equilibrium with the amorphous form.

CHAIRMAN WALLIS: Your bed is NUKON fibers?

DR. SHACK: NUKON fibers.

CHAIRMAN WALLIS: NUKON fibers. So we know nothing about the effect of having some concrete debris or something to which these particles could cling. We know nothing about other mechanism?

DR. SHACK: If you added precipitate, you know that would --

CHAIRMAN WALLIS: But you tried to add precipitate aluminum, but there's all kinds of other stuff in the sump.

DR. SHACK: No, there's no precipitate aluminum here.

CHAIRMAN WALLIS: You added --

DR. SHACK: Oh, the nanometer aluminum. Yes.

CHAIRMAN WALLIS: You added precipitate aluminum.

DR. SHACK: Yes, the 30 nanometers.

CHAIRMAN WALLIS: But there's a lot of other stuff. I think the point is that whether or not this stuff sticks to the bed would seem to depend upon what else is there.

DR. SHACK: That's probably true.

CHAIRMAN WALLIS: Now there's a long way between this and predicting anything about a sump.

You've just shown that there can be a large effect.

DR. SHACK: Yes. Well, I think what we've shown up until we added the extra aluminum is that for a given amount of aluminum the STB buffer is much more forgiving than the aluminum -- than the sodium hydroxide buffer. For a given amount of aluminum the sodium tetraborite's a more benign environment.

We went off to look at a head loss test with the sodium tetraborate environment and CalSil. Now we didn't add any aluminum to this test because we knew from, essentially, the integrate ICET test that when we had the CalSil in any significant amount we got very little aluminum. And so we left the aluminum out of this test.

And so we essentially now got 15 grams of NUKON, 15 grams of CalSil in a STB buffered environment. The head loss we're seeing here initially, and again I don't know whether this is a test-to-test variable or not, is higher than we saw in the same sort of NUKON CalSil loadings in a different buffer. So it's about twice as high. You know, I would have expected to see something on the order of 1 to 1.2 psi for the immediate pressure drop in the CalSil/NUKON mixture that we have here.

And I didn't mention that we'd done some

other tests. There are sorts of interesting things that go on here. When we go simple aging tests of the insulation in the different buffers, we see different behaviors.

CHAIRMAN WALLIS: But you're suggesting, possibly, that the buffer changes the pressure drop through CalSil/NUKON --

DR. SHACK: Yes. What we've done here is essentially presoaked the NUKON in the buffer solution for a half an hour, which may essentially affect the way that it can disagglomerate--

CHAIRMAN WALLIS: Which is what happens in reality?

DR. SHACK: Which is what happens in reality. You know, if you disagglomerate the fiberglass even more, you allow the bed to build a little bit differently and so that you could conceivably get a different head loss. We see a different behavior of the NUKON in a long term soak test with the STB buffer.

CHAIRMAN WALLIS: That makes sense. It must mean that you can't use a correlation for NUKON and CalSil without saying something about what buffer you've got and how long you've soaked it for and all that kind of stuff.

DR. SHACK: Perhaps.

CHAIRMAN WALLIS: Perhaps. But as an indication?

DR. SHACK: It's an indication.

Now, the good news is, is that as you go along here, the CalSil just benignly dissolves and you end up at a head loss that's really much more typical of what you would expect from a pure NUKON loading. So as the test proceeds --

MEMBER SIEBER: By that time the pump is shot.

DR. SHACK: Well, yes. Obviously, it's this head loss that concerns you.

CHAIRMAN WALLIS: Benignly dissolves? I mean it's now going around the loop forever more.

DR. SHACK: There's calcium and there's silicates.

CHAIRMAN WALLIS: Unless it deposits somewhere else in, say, a cool region or something

DR. SHACK: Yes. On the fuel; who knows?

Summary slide. Again, we observed high head losses in environments with the sodium hydroxide buffer for aluminum levels of 375 and 100 ppm.

The formation of the precipitate or the build up of the head loss is time dependent.

We had one test that we think is nonprototypical where we got high head loss even at 140°F. We think we really do have to decrease the temperature to get these kind of head losses. We really believe the solubility of the amorphous aluminum at 140 is high enough that we shouldn't have seen that.

The solubility, again the amorphous and crystalline phases is by a factor of 400 for gibbsite. The behavior we see in the ICET-1 environment is pretty consistent with the literature data for amorphous materials. These high head losses can occur with no visible precipitates and, again, very small changes in bulk properties like viscosity. You know, if we do the test with 275 ppm dissolved aluminum, we do see an increase in the bulk viscosity, but it's like a factor of 2. You know, it's nowhere comparable to what we see in the head loss --

MEMBER KRESS: You actually measure viscosities?

DR. SHACK: We measured viscosity in the sense that we take a sample of the solution, we shake it up and we pour it through a viscometer.

MEMBER KRESS: Okay. That'll do.

CHAIRMAN WALLIS: Well let me ask you

something. You've got this stuff which dissolves aluminum, right. And there's a certain equilibrium solution. And then you catch this aluminum gel in the bed. That presumably means that the water now has less aluminum in it.

DR. SHACK: Yes.

CHAIRMAN WALLIS: So doesn't it mean it would try to dissolve some more aluminum somewhere else? Because it no longer has --

MR. TREGONING: Unless it passivates.

CHAIRMAN WALLIS: Because some of it's now trapped in the bed.

DR. SHACK: Yes. You know, the ICET-1 seems to indicate that the aluminum will in fact passivate.

CHAIRMAN WALLIS: Oh, it will. So you won't dissolve anymore?

DR. SHACK: You won't dissolve anymore.

MR. TREGONING: And that was a key finding.

CHAIRMAN WALLIS: That it's passivated, not because it's saturated?

MR. TREGONING: Because the levels in ICET-1, as Marc demonstrated, were well below the solubility limit for --

CHAIRMAN WALLIS: But you understand the kinetics of passivation of the aluminum?

MR. TREGONING: I'm sorry, could you repeat?

CHAIRMAN WALLIS: Understand the kinetics of this passivation of aluminum so you can predict it?= in a plant?

MR. TREGONING: Part of Marc's work, he didn't present this in much detail today given the limited amount of time, but he's got a substantial section in his NUREG report that discusses rate controlling factors for passivation of aluminum.

MEMBER KRESS: Does it have to do with forming Al3OH?

MR. TREGONING: I'm sorry, Dr. Kress, could you repeat?

MEMBER KRESS: It have to do forming Al3OH?

MR. TREGONING: Well, the passivation determines how much aluminum you'll get into solution.

MEMBER KRESS: I understand that. I'm trying to figure out why it quits dissolving is because you get a surface layer -- Al3OH, probably.Okay.

CHAIRMAN WALLIS: Well, I'm hoping to

finish at 12:00. Will that work out?

DR. SHACK: I'm on my last bullet.

CHAIRMAN WALLIS: Good.

DR. SHACK: Again, the STB buffer seems more benign than the sodium hydroxide or the TSP.

For the same submerged area that produced high head loss in the sodium hydroxide buffer, no significant head loss observed in 11 days in the STB buffer.

Again, interaction with NUKON/CalSil mixtures produced much lower head loss than observed in corresponding tests with TSP.

MEMBER MAYNARD: I've got a question. The applicability of this test to containment sumps that you'd find in a plant in that you have a fairly small screen area it looked like for your flow loop, whereas in a containment you're going to have a fairly large screen area. It's going to be vertical probably also with a top open. Flow is probably going to coming in from the side. I'm not sure what this gelatinous solution would do. Would it settle out more o the floor or how --

DR. SHACK: That's the thing, because we don't see any settling at all. This thing is infinitely transportable. You know, it's going to go

wherever the fluid can go. The sort of product per volume, the product per unit screen area that we have in our tests are roughly prototypical of what we expect in the plant.

You know, the geometry is different. You know, it's horizontal rather then vertical. But to the extent that you're looking at a local effect and you see the head loss going across there, it seems to me applicable in that effect. It certainly doesn't take your overall design, if you made a much larger screen area, you know, that would have an effect. But these things really do have to be scaled to a kind of a mass per unit screen area kind of scaling.

MEMBER MAYNARD: Kind of going back to the question of what happens if it's filtered out. There's other things in the basement of a containment that this could be attaching itself to besides the screens. Got all kinds of metal support, other concrete and other things. As some of this attaches to other things, does that make room for more aluminum to be formed or --

DR. SHACK: Because again it's a passivation process, we don't think that you'll -- you'll be limited by essentially the amount of corrosion that you can sustain on the surface of the

aluminum. And taking it out, perhaps, in a heat exchanger would not really create more product.

MEMBER KRESS: I have a problem with your must use screen scaling concept, and that is how do you ever solve what mass per screen you ended up when you took off? How did you know what mass screen that represents?

DR. SHACK: I can't -- yes. That's what I would need if I was going to have a model.

MEMBER KRESS: Okay.

DR. SHACK: If all I'm looking for what ppm can tolerate in my sump, I can give you a ppm chemical concentration per unit area of screen that you can use as a kind of a guideline.

CHAIRMAN WALLIS: How could you do that? Because it depends what's on the screen?

MEMBER KRESS: Yes.

DR. SHACK: Well, if you're going to argue that, yes, if I had concrete precipitate -- you know, that my 50 ppm although it looked benign here, really would have been different --

CHAIRMAN WALLIS: But if plant R had nothing but aluminum reflective insulation and no fibers, nothing, you get this sump and it's got all this colloidal stuff in it, it just goes right through

the screen and through the -- nothing happens. So it's not a problem, even if it's 500 ppm.

MEMBER SIEBER: Presumably.

CHAIRMAN WALLIS: But we haven't done that test. We don't know. So it all depends on the synthesis of everything that's going on. You've looked at part of it, and it's very interesting. But every plant is different. I want us to figure out what's a reasonable test. Okay.

And this work is finished now?

DR. SHACK: Except for the reporting.

CHAIRMAN WALLIS: It's finished because the money has run out? It's not finished because you've solved anything, is that true?

What was your task? To explore these things or do you come up with predictive methods?

DR. SHACK: No, we were to explore these things.

MR. TREGONING: When we started this work, there was no information on any sort of head loss indications --

CHAIRMAN WALLIS: Try it and see what happens.

MR. TREGONING: Chemical.

CHAIRMAN WALLIS: How can exploratory

tests be confirmatory? Not confirming anything, just seeing what happens.

MR. TREGONING: Associated with the ICET products.

CHAIRMAN WALLIS: Anybody else? We'd like to stop 12:00. You can go on as long as you like, but my schedule, being a schedule-driven Committee of course, was that we should stop at 12:00. Is that okay with the Committee?

MEMBER KRESS: Yes.

CHAIRMAN WALLIS: Okay. Thank you very much, Bill. That was very, very interesting. You did a great job.

MEMBER KRESS: And I'd like to congratulate the speaker on such elicit presentation.

CHAIRMAN WALLIS: Right. I noticed that you put the commas in just the right place. Thank you.

We will take a break until 1:00.

(Whereupon, at 12:01 p.m. the hearing was adjourned, to reconvene this same day at 1:03 p.m.)

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

1:03 p.m.

CHAIRMAN WALLIS: Please come back into session.

We're now going to hear about the peer review of chemical effects. Rob, are o going to start us off? Go ahead, please.

MR. TREGONING: Thank you, Mr. Chairman.

Again, this will be a presentation on the chemical effects peer review given by myself and Paulette Torres from the Office of Nuclear Regulatory Research.

The objectives the peer review itself, there are really two specific objectives. One was to review the technical adequacy of Research sponsored activities related to chemical effects in PWR sump environments. So we've only asked them to focus on those programs where we're specifically looking at chemical effects.

Secondary objective is to have the peer reviewers recommend not only improvements, and they've done this all throughout the year, recommend improvements to the Research programs that we have ongoing. But also just as importantly, or maybe more importantly, identify additional important technical

issues for consideration that we're not currently addressing. So we'll discuss some of these subsequently in this presentation.

Programs we asked them to review. There were four specifically.

We had them look at the ICET test at LANL, some of the follow up testing and analysis that Marc Klasky presented today, specifically looking at the aluminum sodium hydroxide and sodium tetraborate systems. To look at the chemical specification work. Again, we're not discussing this today, but we discussed it in pretty good detail at the last meeting conducted at the Center for Nuclear Waste Regulatory Analysis. And then also looking at the ANL chemical effects head loss testing, which you've just heard about.

My slides are heavily process driven. We talk about the process that we went through in the peer review, and that's intentional because we're still getting results. So I want to try to get through the process related slides as quickly as I can so we can get to some of the nuggets that I'm sure you're most interested in.

Why did we decide to do peer review for this, and that's always a legitimate question. There

was really two reasons. One, it's NRC policy. In fact, it's something that's become more focused, a more rigorous look at using peer review. And where it's warranted, external peer review to make sure that the programs that we are undertaking have sufficient quality.

And part of the policy as well is to make those results visible. And you'll be hearing how we're going to be doing that as part of this process.

Chemical effects in the sump pool were certainly a logical review topic. It's a relatively new research area. I think BP indicated that it was really 2003 when we started seriously looking into these issues. And there's an aggressive resolution schedule associated with GSI-191 in 2007. So relatively short time frame to try to identify and then resolve issues.

And as we've discussed not only today but in prior meetings, that it's a technical area that has quite a bit of complexity. So we thought some independent review was necessary.

And then finally, as we've discussed, the issues that we're talking about could have some effect on the resolution path. So we've been trying to use peer review to identify those as quickly as we can at

this point in the process.

Quickly, the intended regulatory use. This is very similar to what you have seen in prior presentations. Again, all of the information that were developed in this program will be used by NRR in evaluating the Generic Letter responses from the licensees and in conducting audits to ensure appropriate resolution.

Specific applications that NRR will be using as well as Research, they need to evaluate the adequacy and uncertainty of results that we're giving them within the proper context. So the hope is that the peer review will help them assess the results that we are giving them from our sponsored activities.

We want to help identify or help NRR identify outstanding chemical effects issues. And we also want to provide them with a measure of assessing the fidelity and the robustness of some of the industry sponsored testing in this area.

I've presented this table before. I just wanted to refresh your memories. This is the group of external peer reviewers. I'll get into a little bit how we selected them subsequently. There are five peer reviewers. They are from industry as well as academia, as well as national laboratories. So we

tried to get diversity of affiliation as well as diversity of technical expertise. We have gel experts, filtration experts, experts in analytical and experimental chemistry, corrosion and metallic corrosion, experimental testing, electrochemistry. We tried to really cover a range of technical areas that we think are appropriate within the chemical area.

And we looked for diversity. We didn't want five people with the same background. We really wanted, again, distinct backgrounds so they could interact as a group and try to look at the issue in a holistic a manner as possible.

So I've talked a little bit about the peer reviewers and the approach. And that was the first step in this process.

We spent quite a lot of time here. We spent probably three or four months gathering recommendations to try to assemble a panel that covers a range of technical expertise and also has a diversity of experience. So we took recommendations from a number of different people; staff, contractors, industry, the nuclear industry itself to arrive at the final panel of five.

We had provided, we actually started the activities back last fall, October 2005. We initially

provided documentation to the peer reviewers of all the various research activities that we had. And the formality of that documentation varied as a function of the program.

ICET where the testing and the documentation was relatively mature, we were able to provide the completed test data reports that were available at the time. For other activities like the ANL work, which was really just initiating at that time, the documentation was more along the lines of presentations and sort of informal status reports and things like that.

After the initial documentation we had a kick-off meeting in October. The purpose of that meeting was to identify the important technical issues and questions that we wanted them to assess in their review. We also during this meeting through presentations summarized the additional documentation we provided them. And, again, we also as well as summarizing what we had done, we also discussed the plans and philosophy behind the phase of research that was ongoing and then planned for the near term over the next several months at that kick-off meeting in October.

Then in December we asked all of the peer

reviewers to provide us with a preliminary assessment. This was essentially their quick look reports to use. What we wanted them to do was give a pretty quick cursory review to try to identify any major deficiencies or any major course changes that we should undertaken in that time. So that was one thing that we wanted them to do.

The other thing we wanted them to do in this assessment was identify issues that they needed additional information in so that they could complete their review. And also raise some technical questions that we could try to address to help them with their formal review.

After their preliminary reports, we followed it up with a second peer review group meeting. This was conducted in March of this year. That meeting we presented our research activities that we had undertaken between the October meeting and the March meeting. We attempted to address their questions and clarify remaining issues during that meeting.

And then the third thing we did at that meeting was actually conduct a PIRT process to help with identifying outstanding issues.

So where are we in the process? We're

really at the stage now when we're gathering and receiving their final assessments. I say June 2006, so we are in the process of that. We've gotten first drafts of the final assessments from all the peer reviewers. And we're in the process of going back and iterating with them as necessary to get final documents.

The PIRT process we're still awaiting results from some of the peer reviewers. So we're really right in the middle of that. But the intent is by the end of this month their assessment, both their formal written reports as well as their PIRT assessments should be completed.

We expect that there's going to be two products that will evolve out of this work.

One, there will be a NUREG summarizing the peer review process and the significant findings from the formal reviews. And we will have or document in their entirety the individual peer reviews in the appendices. So the NUREG itself it just going to provide a summary of important findings as well as describe the process that we followed for the peer review. And again, the actual individual peer reviews will be contained in the appendices.

The second product that we're working on

is a Research Information Letter. And that will be focused on summarizing the PIRT process and outstanding issues.

So there will be nothing about the PIRT contained in the NUREG, but the PIRT aspects of the peer review will be covered in this RIL and Research Information Letter.

I know this is old hat to many of you, but I thought I should have at least a few slides describing how we conducted the PIRT or how we have been conducting the PIRT since it is ongoing.

This first slide is, again, for anyone that's not familiar, just a brief introduction to the PIRT process.

PIRT stands for Phenomenon Identification and Ranking Table Process. A PIRT is, again as most of you are aware, it's based on expert opinion. You enlist the various experts, you provide them with background information. And there's a structured process that you follow through for a formal PIRT.

The outcome with the PIRT is you're really seeking informed opinions. And you're looking at opinions in two areas: (1) You're looking at ranking important issues and providing the rationale behind that importance ranking, and; (2) You're trying to

assess the amount of knowledge that's associated with a given issue. How well do we really understand it and the rationale behind that.

Ideally what you're looking for, you're looking for areas that come up as being both high importance and where there's a large amount of technical uncertainty. Those are the areas that potentially are ripe for additional research. If they're high importance yet they're well known, those are areas that are ripe at that point for more of an engineering analysis and assessment of the issue.

And the final point behind a PIRT is, again, the outcomes are documented. So it's a formal process.

This last bullet, I think, is important. It's not meant to be an end all, be all final step in the process. The outcomes are fluid. They're very much based on information available at the time of the PIRT. As you learn more, issues that have been raised in a PIRT are either validated, shown to be inconsequential or changed. So, again, it's meant to be sort of essentially a status report of what the state of knowledge is with respect to the issues that are developed as part of the PIRT.

Again, this is just a brief flow chart

that depicts at least what's classically outlined as the nine steps associated with the PIRT. I don't want to go through all of these steps, obviously. But I've highlighted in orange here, that's typically information that you provide to the reviewers before you get into the assessment or even identifying phenomena. So these are things like the objectives, what the issue is, the background information, how you're going to evaluate the importance of issues. These are all things that are determined up front, normally by the sponsor, but it tends to be iteration involved. Because, again, the experts themselves in many cases have a hand in defining those things.

I know when we had our meeting in March where we presented this, we had some preliminary evaluation criteria that we proposed. And we actually modified that and iterated that based on information that we got from the peer reviewers.

Same thing with background information. I mean there's some initial background information that you supply, but quite often the panelists themselves will identify other background information that's pertinent.

So once steps 1 through 5 are completed, the blue steps 6 through 9 are really the essence or

the actual product that evolves from the PIRT process itself.

At least 6 through 8, and I've got a bit of a typo. Six through 8 I would argue are really completed by the expert panel. NRC Staff is doing step 9, which is the documentation process.

I mentioned that these steps are often irritative. And, again, it's important to document not just rankings but rationale.

So that;s the generic process.

MEMBER SIEBER: Do you have colleague meetings with the experts to do all this?

MR. TREGONING: We had a kick-off meeting that was 2½ days where we covered a lot of the initial ground work for the PIRT as well as initial brainstorming to identify issues.

We've had prior meetings in collaboration to lay the ground work, provide background information.

Now since that initial PIRT meeting we've had two conference calls with the peer reviewers to go through various stages of the PIRT process. Both to clarify issues, make sure that there's common understanding and agree to how we're going to conduct the evaluation. So we've had -- again, I think there's

been two conference calls. There's another one this week because in fact by the end of tomorrow I'm expecting to have initial assessments from all the members. And then what we'll do at that point, the next conference call will be to synthesize the various results and summarize them and identify issues where we have consensus as well as also look at issues where we don't have consensus and try to understand why. So there's a number of follow on interactions.

MEMBER SIEBER: So you product the final report, do you interpret that as a consensus?

MR. TREGONING: No. No. I wouldn't interpret it as a consensus report.

MEMBER SIEBER: You wouldn't?

MR. TREGONING: No. What you will get will be a distillation of -- again we're asking for individual assessments from each expert. We're not asking the panel to come up with consensus recommendations.

MEMBER KRESS: The PIRT will be a consensus in terms of rating priorities, won't it?

MR. TREGONING: We ask for individual ratings and priorities. We have to be very careful about --

MEMBER KRESS: You'll have to come up with

a rating --

MR. TREGONING: Well, we the Staff will distil the information. It various, for instance, there's five peer reviewers. Let's say there was one issue that one reviewer rated very highly, of extreme importance. Four of them said were not -- was not an issue at all. One of the purposes for the conference call is to try to explore that. And try to assess, maybe the person who thinks it's incredibly important is the one that's correct. So we'll try to have some discussion to whatever we think is the right way to go.

If there are any issues, I can assure you that we'll tend to error on the side of presenting an issue versus sort of squelching an issue. So if there's lack of consensus, we might certainly indicate that in the PIRT document, but probably raise the issue all the same.

MEMBER SIEBER: So the Staff is the controlling process as opposed to your peer reviewers?

MR. TREGONING: Yes. The Staff is in charge of the process. LANL, Bruce Letellier has been helping out as well. So we've elicited some contractor support. But we've specifically tried to make sure we were in charge.

We have to be careful because, you know in terms of getting consensus recommendations from panel, there are very specific statutes and mandates that we have to work within to make sure that we don't violate any of those mandates. So --

MEMBER SIEBER: We do, too.

MR. TREGONING: Yes. We're well aware of many of those.

MEMBER BONACA: Well, the peer review, it's very focused on the chemical effects. And to what degree are they informed of all the other aspects of GSI-191?

MR. TREGONING: Yes, that's a good question. And in fact when we've had some of the brainstorming issues, they'll raise issues like what about particulate, you know what about precipitate that's formed here, how might that effect things. And we've tried to provide a context to let them know what issues are important, yet we've really strived to maintain a focus on the chemical effects area. Because that's the area currently with the most uncertainty with respect to GSI-191.

MEMBER SIEBER: Okay.

MR. TREGONING: So they've been informed, yet asked to sort of recuse themselves from commenting

as much as possible.

So let me briefly go through how we tailored the PIRT process. And I think we've covered a lot of this based on Dr. Sieber's comments. So let me just quickly go through this.

WE had a brainstorming session to identify technical issues. We documented that brainstorming session. And we had the peer reviewers review it to make sure we were both accurate and complete. And then we developed from that a finalized issues list. And then we had a subsequent conference call to make sure that we all had common understanding as to what the issues were and that we knew how we were going about the ranking, what the criteria was for doing the ranking.

So now step 2 is really the step that we're currently in, that's when the panelists are completing their initial PIRT assessment. As I mentioned, these are independent and they're going to be documenting rationales.

What's going to happen later this week we'll be summarizing some of the initial assessments. And again as I indicated, one of the things we'll be doing in that summary, we clearly want to identify highly ranked issues that have consensus, but we also

want to identify and explore other possible highly ranked issues that don't have a clear consensus. We want to understand why we don't have consensus ranking.

And there'll be, again, subsequent panel discussion. So to either get some sense that there's na agreement on the consensus, those highly ranked issues and like I said, explore reasons for a lack of consensus.

We will give the peer reviewers if they're so inclined, a chance to iterate their tables and provide us with final tables based on all the discussion. I doubt that there'll be much, if any, irritation, though.

The next four slides I want to delve into some of the comments and results that we've gotten from the peer reviewers. And the way I've structured it is this first slide is some general comments just with respect to the four program technical areas that we asked them to review. So this is outside the PIRT, but this falls within the purview of their formal assessments.

With respect to the integrated chemical effect testing, I think what we've generally heard from the peer reviewers is those tests within the

limitations of the assumptions that were made were generally representative of what might be expected in a containment environment with multiple materials. And I think, again, there's been agreement that several important contributing materials and interactions have been identified by this ICET tests.

With respect to the ICET follow-on bench scale studies that Marc Klasky talked about, there has been, again I'll say, an understanding that the work has addressed some of the implications of the ICET byproducts which formed and provide us some indications as to the effects of change sin critical parameters like temperature, concentration, pH and time.

The specification prediction work, again, we didn't hear about this this time. I think one of the things that people would agree that it has done, is it has identified capabilities that a code would need and limitations of commercially available codes for predicting specification in these environments. However, again, I think there's fairly universal agreement amount the peer reviewers that much more rigorous code development would be needed in order to really develop an adequate assessment tool --

MEMBER KRESS: Yes. I remember. These

were chemical equilibrium codes?

MR. TREGONING: Yes. Yes.

MEMBER KRESS: Were they relating to kinetics in that second bullet there then?

MR. TREGONING: There's certainly been a discussion about kinetics And we've had a number of discussion with the peer reviewers. Initially some peer reviewers were of the mindset that we should look at developing a full kinetic model. I think as we've had further discussions, there's been a realization that full kinetic model might be very difficult to achieve and --

MEMBER KRESS: Under these conditions, yes.

MR. TREGONING: We may be better served doing, you know, pseudo-thermodynamic, you know sort of an informed thermodynamic equilibrium type of model. And by informing, by informing developing thermodynamic constants in conditions that more closely simulate the environments that we're concerned about. And also with precipitated species that are along the lines of the products that we've observed in these various tests.

So the idea would be to try to focus the thermodynamic development and experiments that you

would do.

With respect to the ANL work in chemical head loss testing, again there's been an understanding or an agreement that we have identified implications for some of the ICET products. And those tests have been valuable with respect to that.

One of the things that they've, I'd say, been generally critical of is I think they'd like to see more smaller scale tests in parallel so that more parameters can be identified and evaluated in parallel versus what we chose to do initially was focus on more a larger scale loop development. And, again, one loop versus several smaller loops. So that's a recommendation that we got from several of the peer reviewers that they thought would be helpful.

MEMBER SIEBER: And one of the problems is that you don't vary one variable at a time? Such a small set of tests that you're running, it's hard to pick out --

MR. TREGONING: Yes, that's true.

MEMBER SIEBER: It's only a matter of time --

MR. TREGONING: Well, the philosophy has been not just at ANL, but also LANL to some extent is the smaller scale studies have been used more, not in

terms of looking at head loss implications, but in defining parameters which are important for the bigger testing, either the integrated testing of the ICET type testing or ANL, and Bill can speak better to this than I, they've used bench scale studies to try to identify what parameters they should try to study in the bigger loop. So there has been, again, at least the objective from the beginning to try to couple as much as we can, smaller scale testing with the larger scale testing that's gone on at each of these labs. But, again, I think to be fair the peer review comment was really focused along the lines of they'd like to see more smaller, sort of head loss loop apparatus to get more information relative to head loss with respect to all of these parameters in a much cleaner way.

I mean, the way Bill did his scoping tests. Quite often he's varying many things within the tests.

MEMBER SIEBER: Right.

MR. TREGONING: I think they would have been happier seeing, you know, single effects sort of tests on a smaller scale to look at one change at a time and evaluate that before -- you know, to try to muddy or confound the issue by looking at several

things in any one test.

MEMBER KRESS: You don't gain much in either time or money by going to much smaller scale than what Bill had, I think.

MEMBER SIEBER: No, you don't.

MEMBER KRESS: I mean, you got to have a pump, you got to have a heater, you got to -- you just don't gain much I don't think.

MR. TREGONING: There's certainly trade-offs. But, again, you know I look at these recommendations of, you know, let me design how I'd love to do these experiments if I had no constraints, okay, be they constraints of time or money. And I think it's valuable to look at them in that context.

MEMBER DENNING: Should we interpret that last bullet as a criticism of the way the experiments were performed or as an indication of their feeling of the need for more testing than has occurred?

MR. TREGONING: Yes. Someone else may want to weigh in here that's been privy to the discussion. But I would say more likely the latter. You know, although again there's been -- I don't want to minimize the former point. Because, again, those tests are difficult to interpret because there is some confounding aspects going on in several of the tests

that make it hard to really see what's happening. But I think it was really more focused on the latter; the need to more fully evaluate the effects of certain parametric changes.

So this next slide would, again, along the lines of what I just talked to Dr. Kress about, if the peer reviewers were conducting in a vacuum and trying to solve this issue, this is how I think they would progress based on the comments that they've made.

They would argue that we do need to do more parametric studies on a smaller scale to completely address chemical effects. And we'd use these smaller scale studies to evaluate outstanding issues. I'm going to cover some of these outstanding issues in a subsequent slide. And also to look at plant-specific variability within critical parameters.

I think they recognize that they had a pretty close collaboration among the analysts and the experitmentalists from the various labs. But the main point behind the second bullet that if you wanted to develop applicable models, which is again what their focus was, you'd need a much closer collaboration than we had where the analysts would be informing the testers what actual tests they wanted to do. We haven't followed that model. Again because our tests

have been more scoping in nature.

So if the focus were on model development, this close collaboration between analytical development, the characterization of the byproduct and then the head loss testing, it would all need to be very closely coupled. I think several peer reviewers recognized that fact.

Okay. And this next slide, I figured I'd throw this up and we'd all have a lot of fun with it. I mean, this is meant to be a bit of a laundry list of preliminary issues that many of the peer reviewers have raised.

Now I say it's preliminary because again, this will be refined once the PIRT process is complete.

MEMBER KRESS: The first recommendation is that throw that first bullet out.

MR. TREGONING: Okay. That's fine with me. That's a major complication I wouldn't have to deal with.

MEMBER KRESS: Yes, right.

MR. TREGONING: So this list is not meant to be encompassing in anyway, and it's to provide you a bit of the flavor of some of the classes of issues that we've discussed and talked about within the peer

review group.

So I really wanted to focus on sort of four areas of additional study here that have been a theme as part of the peer review discussions.

One that we've had a lot of discussion about were the coupling of radiolytical effects and the redox potential of the sump environment. That's been a major area of discussion in the sense that ICET or none of the tests that we've done have looked at all at radiolytic effects.

MEMBER KRESS: Where is the radiolytica coming from?

MR. TREGONING: There's a variety of potential sources. The ones that we focused on would be crud that's released from the inside of piping due to the thermal transient. So this would be nickel ferrite, magnetite, nickel oxide, hematite, activated species that would be released due to the thermal shock transient from the inside of piping. Again, build up or scale that accumulates over time.

And the rationale behind that is, you know, when plants are shutdown normal pressure, temperature transients you do see a certain amount of crud release, you know, comes out as black water. And there's been a lot of speculation about those effects

and how they might alter the chemistry and the byproducts that we're seeing in these environments.

And, again, Bill or Marc or Bruce may want to jump in at any point here because they've been privy to all of these discussions. In fact, Bruce looks like he can't wait any longer.

DR. LETELLIER: I think one of the reasons that even small amounts of radiolytic products are important is because of the sensitivity of the chemical systems to the hydrogen peroxide. So that's part of the concern.

I mean, we can discuss the radiation levels or quantities of crud that are released, but even a small amount of hydrogen peroxide can have a large effect in the system.

MR. TREGONING: In terms of preparing the redox off potential of the system. And there's been a lot of discussion about the redox of the containment environment could vary from over a volt, which is tremendous in terms of what the implications are in terms of chemical product formation.

So that's an area that we've had a lot of discussion and at least several peer reviewers have indicated that if you really want to understand what's happening in your environment, you have to have a

really accurate sense of the what the redox potential and how the radiolytic effects might be effecting the redox potential in your containment.

Another area, we've touched on this before so this is certainly not a surprised, the ICET tests by in large were, in fact they were isothermal tests. And there are certainly concerns. We know in the containment environment we don't have an isothermal environment. In fact, we go through a temperature cycle, any little control value of liquid first gets cooled down at the heat exchanger and then gets heated back up in the core. So we talked a lot within the peer review of the effects of thermal cycling on both product formation due to standard solubility, precipitation at lower temperatures as well as retrograde solubility; scaling or plating our at higher temperatures on areas within the reactor core itself.

And one of the concerns here, and it was raised earlier by Bruce, without passivation thermal cycling potentially sets up a continuous source term development for chemical product.

Another important area, and Marc touched on this a little bit, is the solution pH or point of zero charge. This is an important measure for

determining the susceptibility of product to go from nanoscale colloids to actually agglomerate and form bigger particles, bigger agglomerated particles that may have implications beyond just impaction on the fiberglass in terms of how they might effect head loss. And that could significantly effect if substantial gel core formation actually occurs.

A fourth area which we've touched on a little bit, I think we talked about this in July at this Subcommittee meeting, was the effect of coatings and other organic materials.

And, Marc, I don't know if you presented this in July or not, but Marc has done some work on effects of organics on effecting essentially acting as chelating agents and effecting agglomeration of some of these aluminum products. And has been able to demonstrate that for certain organics that he can impede precipitate formation through certain levels of organic elemental additions. So the effects of organics as well as coating.

MEMBER KRESS: Is that considered possible mitigating strategy, to add chelating agents in there?

MR. TREGONING: It's a possible strategy. I don't know that it's been seriously considered at this point. It's something that's been discussed.

You know, when we try to assess the chemical environment we want to be careful that we're not making changes in one area that are having adverse effects in a different area. So it is a possible mitigation path, though.

And then so I talked about chelating agents. There's been at least one peer reviewer has an alternative hypothesis that while you may have some organics that instead of acting as chelating agents, may act to bound or accrete solid particles together and actually promote agglomeration instead of discourage agglomeration.

And then we've talked about effects of hydrolysis on coating materials to produce particles or gels.

So again, this list isn't meant to be inclusive, it's just really to provide you a flavor of some of the things and some of the issues that have been banied about within the peer review discussions.

So my next slide is, you know the obvious question that you're all having is so what? What are you going to do with this information? And this next slide is meant to really outline the strategy that we have initially. And I'll say this slide is really broader than just these issues. It's really how we're

approaching sort of the entire Research philosophy behind GSI-191 resolution.

One of the things that we think is important is that the issues that are developed, not just here but with respect to all of our research, they need to be resolved within the context of the resolution plan. And the way that's been outlined, and the way we're proceeding to date, the industry has been given the lead and the ultimate responsibility for the resolution. And the NRC's role has been very clearly articulated as verifying that these resolution strategies adequately assess outstanding issues. So this gets at your point where our role has never been defined as developing the methodology or the textbook for solving these. But really to focus more on ensuring that we ensure adequate safety of the modified sump designs that the industry is going to be promulgating.

MEMBER SIEBER: How closely has the industry been involved in observing the research and tests and experiments that you folks are doing? Now if you're going to ask the questions of them based on what you're learning now, if I were a licensee I would try to figure out exactly what it is you're doing so that I knew what questions you were going to ask.

MR. TREGONING: That's a good question. And I'd say it depends on the test program.

The ICET program was a joint program.

MEMBER SIEBER: Yes.

MR. TREGONING: So that was all mutual collaboration and it was set within a formal framework. The other programs, we have to be a little bit careful because we have other stakeholders other than industry so we can't give industry preferential knowledge prior to giving other public stakeholders. So our strategy in these other programs like the ANL work is conduct tests and then try to disseminate the information as quickly as possible so we can get -- in ANL's case we had a number of quick look reports. In the area of ICET, when ICET was developed we never intended to release all these individual data reports associated with each test. We were just going to have a summary report when it was done. But we thought it would be more product to get the raw information out there so that plants and people could --

MEMBER SIEBER: That's the stack of stuff that we have.

MR. TREGONING: Yes. That big stack that you had was never envisioned at the beginning of the program as being output. But as we progressed we saw,

look, we're seeing things here and it's incumbent upon us to get as much of the information out as quickly as possible.

MEMBER SIEBER: But it lacked the analysis. It was just reports.

MR. TREGONING: It was reporting day. I mean, look, you sacrifice something in in --

MEMBER SIEBER: That was fast?

MR. TREGONING: For speed, right. Right.

So the summary report that's being developed for ICET will provide some of that analysis that was lacking in the data reports.

MEMBER SIEBER: Well, I would think that if your overall plan is to be successful --

MR. TREGONING: So I would agree.

MEMBER SIEBER: Out designing sump screens that aren't going -- maybe they're going to do it twice, and that's not good.

MR. TREGONING: No, that's my concern. And I share that concern.

Another thing we've done other than providing sort of quick written products, is we've had a number of public meetings. In fact, I think we've averaged over the last two years a public meeting every two months. And typically the way these public

meetings are structured is Research has a role, or we go up at the public meeting and present what we've done of interest in the prior two months.

When ICET was going on, we were presenting what we were finding with ICET. When the Argonne work has been going on, we've been presenting findings as we get them. So that's another mechanism we've been using to get information out quickly.

And at these public meetings quite often we take a lot of feedback from industry on how we might refine the tests or do them better in ways that are more representative of their actual environment.

So I would agree with you that as much as possible we need to get feedback and an understanding that our testing is looking at things that are reasonable and representative.

MEMBER SIEBER: One of the troubling things is the Generic Letter-- but the basic ground work is not finished. And I guess I just have to hold my breath and wait and see. Because it's sort of a concern to me that when you rush and doing a lot of things in parallel with the opportunity to come to a wrong conclusion someplace -- it's difficult to reverse.

MR. TREGONING: Again, we are doing things

in parallel. And when you do things in parallel, the need for communication becomes more important and you potentially run the risk of exactly what you're saying. But I think NRR probably may want to address this a little bit more further.

MEMBER SIEBER: Well, I think it's more of an NRR problem than yours. I guess the other question that strikes me is that you're going to learn a lot of things. A lot of them are specific to PWRs, you know the boron and the chemical species and all. There's some things that apply to other kinds of plants who have already done the sump work. To what extend do you expect to find some startling new mechanism that would have an impact on BWRs, for example?

MR. TREGONING: Yes, actually, that's an excellent point. I would argue that --

MEMBER SIEBER: They think they're done.

MR. TREGONING: Yes. Certainly the focus with the chemical effect work and the environmental focus has been on PWR environments.

MEMBER SIEBER: Well, that's where a lot of the chemistry issues are.

MR. TREGONING: Right. And we expect, this is informed speculation or maybe ill-informed speculation at this point. But I would argue that the

PWR complexities are greater. However, that doesn't preclude additional artifacts in the BWR environments. And one of the things that we're looking at NRR has actually taken the lead on this, and it's not just the chemical area. I mean, we've learned things in a lot of areas. The information that we didn't have when the BWR resolution was achieved.

So one of the things that NRR has taken the lead on is to go back and start to revisit BWR resolution and see if there are any issues that have arisen since the resolution time which may have implications. So that's something that we're looking at working closely with NRR to try to see if there are issues.

In fact, we do have -- there's at least a plan in the budget to go back and start to evaluate BWR chemical effects.

MEMBER SIEBER: I agree with you. The two types of plants are very different from the standpoint of chemistry. But there may be thermal-dynamic -- that you'll learn about in the process of dealing with PWR sumps that you may want to think about for broader applicability. So I think what you're doing is what I would expect a reasonable assumption of folks to do.

MR. TREGONING: Okay. The second point on

this slide that I think is important, we've talked about today, you know prior to this presentation within this presentation, there are many possible issues that we don't fully understand. We certainly don't understand to the point of having rigorous technical models developed that can predict head loss due to this set of conditions. But I guess one of the points that I'd like to make, and Professor Wallis has stressed this I think pretty elegantly so far today, is that many of these remaining issues are very plant specific. And the importance of these issues will be a function of important plant specific parameters, the design margin that they have for their ECCS system and the mitigation strategies that they've adopted.

So we need to while we're considering which issues are important, we really -- unfortunately, this is just the nature of the beast; we really have to identify I think for each plant what set of issues are most important and to try to understand and build up in that way for the fleet wide which issues are really important.

Is it aluminum corrosion? Is it calcium phosphate formation? You know, to me it's not quite clear yet which plants. There may only be one plant where we have to worry about substantial aluminum

corrosion and precipitation. And if that's the case, we take a very different approach and strategy to dealing with that plant than we would if it were a whole fleet of plants.

MEMBER SIEBER: Well, one of the difficulties that I think you're going to have is that you are developing models. You are investigating so you can come up with a list of phenomena or a per process or something like that. But you don't have a way to take the individual characteristics of a given plant and translate that into this plant will operate successfully or not. And I think that piece is missing still. And maybe the industry is going to come up with the magic formulas, but I don't see the evolution of models coming from this research work.

MR. TREGONING: Yes. I would say in some areas I would agree with you that that's certainly the case. In other areas, like for instance I think the TSP issue, I think we've gained really quite a significant level of technical understanding in terms of, you know, CalSil levels that can potentially cause issues. I think there's metrics that can be developed or can be utilized to really determine if you have a problem or not and can be used for screening.

So there's some issues I feel like we're

sufficiently far advanced enough where we can make a definitive statement. There's others where there is significant uncertainty. That's why understanding which plants are potentially affected by those issues and how many plants are there is in my mind a vital importance to really determine how best to proceed with fully resolving those remaining issues.

MEMBER SIEBER: On the other hand since you're limiting the number of tests and experiments that you do, where you're varying more than one variable at a time from any given test, it's not clear to me that you're going to be able to make that distinction for every plant that's out there as to what's important, what is not, what combination of things. For example, maybe in a test you vary two things, but the plant in its operation and predicted accident response doesn't have one of those variables. You're not going to know --

MR. TREGONING: Right.

MEMBER SIEBER: -- what's important there unless you do additional work or they do additional work.

MR. TREGONING: Yes.

MEMBER SIEBER: So there are some pitfalls here.

MR. TREGONING: Yes. No, I agree.But by the same token, it's really impossible for us to do research to look at 69 different plants uniquely. So that is one area that we're relying on the plants for given the information that's out there to try to do as rigorous assessment as possible, as well as buttress that assessment with some additional experimentation as necessary to demonstrate technical acceptability.

CHAIRMAN WALLIS: Let's look at one plant. Let's look at plant R which has all the aluminum in it. That's one plant.

MR. TREGONING: Plant R potentially has issues.

CHAIRMAN WALLIS: That's one plant.

I'm not sure how you use what we heard this morning to evaluate whether or not they've solved the GSI-191 problem.

MR. TREGONING: Well, GSI-191 is a lot broader than anything we've talked about so far today.

CHAIRMAN WALLIS: Yes. But how you going to use what you heard this morning to assess something when they come in and say our screen is now ten times as big, we've done these tests, and we've found all this stuff, and the pressure drop is acceptable? And I'm not quite sure how you use what you heard this

morning to assess that.

MR. TREGONING: Well, you know, let me try to answer that. Again, I would argue we don't have a specific set of criteria that plant R comes in and says okay, I've checked off 1 through 5 so I know I don't have a problem. However, let's use plant R.

We know plant R has a lot of aluminum, that raises a concern. We've seen results here and results at LANL that lots of aluminum potentially leads to head loss. So then we start asking questions of plant R.

We find that plant R is not a high pH plant. That's a good thing. We know with work at LANL and ANL and other places that the lower the pH, the less aluminum you get in solution. And secondly we learned that plant R is not a sodium hydroxide plant. They're a TSP plant. Even better. So that makes it --

MEMBER SIEBER: Or about to become one.

MR. TREGONING: Or about to become one. So although I believe plant R is a TSP plant, but that's just my own --

MEMBER SIEBER: Okay.

MR. TREGONING: I don't know that for a fact, but I have a sneaky suspicion that they're a TSP

plant.

So I mean that's the sort of evidence you're looking for to try to build a case of how concerned are we for plant R. Now some plants I think we'll be able to make a very good argument that we don't have concerns. There'll be other plants that we probably don't have enough information to make that kind of logical argument to eliminate any concerns. And those are potentially the plants where we have to do some additional analysis, testing, study to try to figure out what issues are.

But the way I look at this right now we have an issue with 60 plants --

CHAIRMAN WALLIS: So are you going to do some more analysis and testing when you find out what other questions you need to answer?

MR. TREGONING: Paul might want to jump in here because I'm stealing some of his stuff.

MEMBER DENNING: But let me jump in here before we go much further. And that is, I think that a basic decision is made that we were not -- I mean I think that the NRC has made a basic decision. They're not going to go down a pathway that's the normal pathway of developing models that could be applied.

I mean at some point, I've heard said this

is too complex a problem for us to really have first principle models and this kind of stuff, particularly in the chemistry area. And so although what we were dealing with three years ago or so, or was kind of a model oriented approach, since we got into the chemistry problems, now the industry has taken a position of going very large scale integral tests and somehow covering the inputs with the thought that you can do that with an integral test.

I mean, the normal way we deal with 69 different plants or 104 different plants is we develop models, we have experiments that validate those models and we use the models to do our safety evaluation. We're going a different pathway here, or at least I perceive that a large element of what we're doing is a different pathway, a very empirical integral test pathway for this difficult element of the problem. And I'm not convinced that really is a successful pathway when we start to -- I mean, if you didn't have the ACRS, you'd be okay. But you know we're going to ask questions that are going to be very difficult to answer without models.

And, you know, you talk about the things are on the good side and things that are on the bad side as far as -- obviously, there are things that are

on the good side and the bad side. But can we really go down this integral test pathway in a conservative manner? Because I'm sure that at some point here what the industry is going to have to do is say you know I can't cover all this, but I'm doing it conservatively. And are we going to be able to answer all those regulatory questions when we've gone away a pathway we normally go, which is model development and a validation? Even if those are fairly approximate models, we're not going down that pathway for this part as I read everything that you've said.

MR. TREGONING: It's true.

Paul, did you want to weigh in?

MR. KLEIN: I was afraid you were going to say that.

MR. TREGONING: Well, you were sitting there.

MR. KLEIN: I think the key would be if we can convince ourselves that the approach even without our lack of model development is conservative. And we will be an iterative process. I think Rob is correct there'll be some subset of plants where it might be easy to determine that, some it might be relatively easy to determine they have a real issue that they need to address, to either switch a buffer or do

something different. And then there may be a third subset that we perhaps may need more confirmatory research to help us address those and verify that the industry evaluation are appropriate and conservative.

CHAIRMAN WALLIS: I'm not sure they're going to be conservative. They can just simply say we've put into our test facilities all the sorts of things we expect to see in the plant. The tests are representative. We simply take the test results and use them in the plant. That's without having to understand anything at all.

MR. KLEIN: I don't know that we understand all of the different vendor approaches at this point. There's five different strainer vendors and potentially four different approaches to evaluating chemical effects. I believe some of them will be doing chemical effects testing in multifunctional loop similar to what ANL has done trying to develop a factor to apply for head loss and chemical effects. I don't believe everyone will use an integrated test where they add simulated chemical surrogate and say they've addressed the issue.

MR. CARUSO: Do you intent to resolve this on a vendor specific basis or on a plant specific basis? Because we read some things that say you plan

to look at what the vendors do and maybe audit a few plants and then say the rest of the plants are okay. But then I keep hearing well you've got to consider each individual plant and their aluminum and their chemistry and their sump configuration.

MR. TREGONING: I don't know if you want to jump in, Mike?

MR. SCOTT: Yes. Mike Scott, NRR Staff.

We are in fact going to be resolving the issue on a plant specific basis. Now, as we'll talk about tomorrow, the process will include audits. And we're not auditing every plant. What we're planning to do is audit a representative sample. And we'll show you those criteria tomorrow.

Now that doesn't mean that the plants that are not getting audited don't get a further look. Everybody's package that they -- all the licensees will turn in supplemental responses to the Generic Letter and/or responses to the Staff's RAIs that we sent out here a couple of months ago. And all of those will be evaluated as part of addressing the Generic Letter. So everybody will be evaluated.

Now what we're expecting is that, obviously, if five plants use one vendor's testing program, then their approaches are going to look

fairly similar, although not identical because as you all have pointed out many times, each plant is different.

Does that answer your question, Ralph?

MR. CARUSO: Does that mean you're going to write an SER? You're going to write 69 SERs?

MR. SCOTT: It doesn't mean we're going to write 69 SERs. We're going to do close out packages for 69 plants associated with the Generic Letter.

MR. TREGONING: Okay. So the last slide here before I turn the floor over is -- I'm here the rest of the day. I'm not going far.

What are our research plans or how do we plan to proceed forward? We've talked a little bit about this. What's our strategy.

We'll certainly be completing the Research Information Letter in the very near term to identify or raise some of these potential outstanding issues.

And then the second bullet is important. We're going to be working with industry to monitor their direction and progress over the next several months to take that issue list and really try to determine based on, again, where the plants are heading with their modified sump solutions to prioritize which of the issues are really most

important and which are the ones can have a dramatic impact on their engineering solutions.

And then those that we identify, we'll be looking at coordinating with NRR and industry so that we can assess and resolve whatever remaining issues result.

Status for the peer review. I think I mentioned this, so I won't go over this. The formal review reports have been received. And the PIRT assessments have been completed. We'll be --

CHAIRMAN WALLIS: Well, we heard this morning, I'm sorry to interrupt you, that the final reports are not available until November. How can these people peer review the work until it's been really pulled together in a report? I mean, they give comments now, same as we can. But until they see the final product, what are they reviewing?

MR. TREGONING: Yes. We have reports that are starting to be published anywhere from April to October. I think in the chemical effects area, yes, we have nothing published after October.

They've certainly seen the -- in many cases they've seen preliminary versions of reports. They've been presented presentations with essentially the guts of the report and had a lot of discussion in

terms of this is what information is going to be in the report.

They won't be formally reviewing the reports themselves as much as they are the programs that are associated with those various reports.

CHAIRMAN WALLIS: But the thoughts change. I think of ICET. We heard about ICET a long time ago.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: And you still don't have a final report as far as I know.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: And you're still saying that -- now you're saying that well our interpretation of the web-like structure has changed as we've learned more. When we originally heard reports you were saying oh we've discovered some new stuff which might clog the screens. You know, we've seen all these pictures of these webs. And now you're saying well that's an artifact of how you process the stuff afterwards.

So, you know, until you've really decided what your final report will say, you don't know quite what it is that's got to be reviewed.

MR. TREGONING: Well, you know, I would agree with you some sense. But in another sense let

me recall the discussion we had last July when we presented these films. And we all discussed that they looked pretty odd, and I think you were the one that raised hey you ought to look at surface tension, maybe these are surface tension phenomena. And subsequent work did show that these were phenomena that occurred via drying.

Marc's precipitate distribution that he put up this morning and the trimodel distribution, that raised a lot of discussion at the peer review level. Enough discussion that Marc's gone back and looked at the technique a little bit. And I think while you heard today that at least one of the peaks he thinks is an artifact of the way the tests were conducted. There's been a lot of discussion like this.

CHAIRMAN WALLIS: Yes. That's the whole point. I mean, it's going around and around. But then you're going to publish a peer review NUREG.

MR. TREGONING: Yes.

CHAIRMAN WALLIS: It's better based on the latest information, not what they heard from him the first time around.

MR. TREGONING: But essentially his analysis, the analysis that he's doing in his report

is finished so there's not any new information that's going to come up in the report.

CHAIRMAN WALLIS: But different interpretations, apparently, of the size distributions.

MR. TREGONING: Yes, as a result of consultation with the peer reviewers.

CHAIRMAN WALLIS: Right.

MR. TREGONING: So you're right. We didn't ask them to actually look at all of the final products from each of these project.

CHAIRMAN WALLIS: Are they being asked to say that what you've done is adequate to review?

MR. TREGONING: No. No. No, not at all. No.

CHAIRMAN WALLIS: So how do you know that you've done enough work to form a basis for resolving this GSI?

MR. TREGONING: Yes. We wouldn't even attempt to task the peer review --

CHAIRMAN WALLIS: You don't know if you've done enough work.

CHAIRMAN WALLIS: I don't think we'll know that.

CHAIRMAN WALLIS: We won't know that, will

we?

MEMBER SIEBER: We won't know it until the reviews are over. The one who will determine it is NRR.

CHAIRMAN WALLIS: So we could write -- you know, we're reviewing your work, too. And what we're going to write is a letter. We could simply write that we've had a very interesting meeting with you. You've told us some things that were new information for us, but until it's all pulled together, we can't really assess what it's worth.

MR. SCOTT: If I can jump in again. And we're actually in the same position that you are, Dr. Wallis. We're not sure right now if the approach that we have taken will be enough in and of itself to reach closure on the GSI. That's why when someone asks when you going to close the GSI, said well 2008 if we have enough information at that time.

Once we start to get the Generic Letter responses in and, as I will discuss with you tomorrow, those are going to start coming in towards the end of this calendar year, we're going to have a better idea of the approach that the industry has taken in response to the actions that we have requested of them. And that may, depending on how that comes out,

require additional actions or refocus.

So we're not prepared to say to you today or tomorrow that we see that this absolutely, positively going to result in closure of the GSI without further work. We don't know yet.

CHAIRMAN WALLIS: Because also what Westinghouse is doing, there are some WCAPs.

MR. SCOTT: That's correct.

CHAIRMAN WALLIS: We have got some, I think, well we haven't talked with Westinghouse, we haven't reviewed them thoroughly --

MR. SCOTT: Right. We just got two. We just got two of those in the door.

CHAIRMAN WALLIS: That's part of the package. And since we heard here that industry is taking the lead, that may be one of the more important parts of the package.

MR. SCOTT: That's correct. A lot of the industry's approach for downstream effects and chemical effects relies on these WCAPs. And as another thing we're going to bring up is that Westinghouse or the PWR owners group is writing a third WCAP to address in core downstream effects.

CHAIRMAN WALLIS: So I just don't know. Have they written these WCAPs without the benefit of

the sorts of work that we heard about this morning?

MR. SCOTT: I can't speak to those.

Paul, do you want to weigh in on the chemical ones?

MR. KLEIN: The chemical effects area they've been very cognizant of the work ICET and also the early work at ANL. We released two information notices with attachments that provide --

CHAIRMAN WALLIS: Well, let's take the work that Bill Shack talked about. I've been surprised that that's been incorporated into a WCAP yet and yet it seems that some important phenomena have been identified.

MR. KLEIN: I think the WCAP that we have in for review currently is not -- there's not much in it related to head loss. It's more in chemical model trying to predict what might form.

CHAIRMAN WALLIS: You have to read a long way before you get something that's useful. But what I found in one of them was how you actually produce some surrogate mix. Isn't that what one of them is up to? How do you produce surrogate set of particles?

MR. KLEIN: Yes.

CHAIRMAN WALLIS: And that's all?

MR. KLEIN: Well, they would argue there's

more. They've done some corrosion testing.

MR. TREGONING: Yes, corrosion as well as looking precipitate formation. They've done some filterability testing as well as --

CHAIRMAN WALLIS: And a lot of small scale tests in separate jars, but not nothing at synthesis between different things.

MR. TREGONING: Yes. That's correct. That's correct.

CHAIRMAN WALLIS: We haven't heard about that. Are we going to hear about that in August, is it? Is that where we're going to hear about that, Ralph?

MR. CARUSO: Yes.

CHAIRMAN WALLIS: We have a presentation from industry?

MR. CARUSO: A whole bunch of them.

CHAIRMAN WALLIS: Okay.

MR. TREGONING: I don't know, is the Westinghouse testing on the agenda for August? Not to my knowledge.

MR. CARUSO: I could ask the WOG to come in and talk about what they're doing. So it's the members of the WOG.

MR. TREGONING: Two days sufficient?

MR. CARUSO: Well, as long as we need to.

CHAIRMAN WALLIS: Well, we've written some fairly strong letters about this issue. I think we might this time simply wait and see. Say, look, we've heard a bit and we're going to see what else comes along before we reach any conclusion. I'm not sure yet. But a lot of this seems to be sort of so incomplete, I'm not sure I want to write anything about it at all yet, personally.

MEMBER SIEBER: I tend to agree with you. And I guess I'm not totally satisfied with the approach that you're taking, but I can't think of an alternative that will work in any reasonable kind of time frame. So if I had a better idea, I certainly would tell you, but --

MR. TREGONING: I certainly would welcome that.

Any other questions with respect to the peer review before we move on to the next presentation?

CHAIRMAN WALLIS: Well, we haven't seen what they've said yet, have we?

MR. TREGONING: Sure.

CHAIRMAN WALLIS: We haven't seen their reviews written down?

MR. TREGONING: No. You have not seen the formal review. And that'll be part of this August 2006 NUREG. Seeing in great detail what each of them have said.

CHAIRMAN WALLIS: What you've told us is it's happening. You've told us some of the sort of things, some of the conclusions. And we don't have much of substance to review yet.

MR. TREGONING: Not at this point. But when the NUREG comes out in August, certainly you will at that time.

CHAIRMAN WALLIS: Okay. But thank you.

So should we be moving on to drain column testing, is that it? Probably should move onto it. If it takes too long, we may need to take a break in the middle of it. And maybe Bruce can move along rapidly.

DR. LETELLIER: Maybe.

MR. CHANG: Good afternoon.

Again, I'm T.Y. Chang, NRC Office of Research.

With me here is Dr. Bruce Letellier from Los Alamos National Lab.

Today we would like to present to you a study that was being performed at Los Alamos. The

study is about the head loss caused by chemical precipitates and the study is being done by using a very simple set up that is the gravity driven drain column.

CHAIRMAN WALLIS: No pump?

MR. CHANG: Pardon?

CHAIRMAN WALLIS: No pump?

MR. CHANG: No pump. All right.

Under the agreement between NRC and the industry head loss implications are not evaluated in the ICET tests. In order to address the head loss caused by chemical precipitates a strategy was conceived to place fiberglass insulation specimens in the form of pucks in ICET test tanks during tests 3, 4 and 5.

Afterwards, the post-test fiber pucks were evaluated for head loss using a very simple gravity drive drain column. Bruce is going to go into much more details about the test later on.

The primary objective of this study is to do work that was not done in ICET tests, that is to evaluate the head loss caused by ICET generated chemical products.

And there is a second objective that is to develop a head loss correlation for flow through

porous media from the fundamental principles. And this will permit us to do the comparison to some of the existing correlations, such as the NUREG/CR-6224 correlation and also there are various one being developed by our colleague, Dr. Krotiuk of the Staff. And he's going to talk about that right after this one. It's about the modified Ergun equation.

MEMBER KRESS: Do these tests duplicate what was done at Argonne, add to it or what?

DR. LETELLIER: Compliment.

MR. CHANG: Complimentary.

CHAIRMAN WALLIS: And Argonne didn't correlate that data with any equations.

DR. LETELLIER: We'll get into the details of the study, but in fact this exercise used actual test articles from the ICET experiments solution in fibers. And Argonne was a surrogate with a much different apparatus. We'll compare the two.

MR. CHANG: Similar to the presentations, the regulatory applications of the study is mainly to support NRR review of licensee responses to Generic Letter in an area of head loss caused by chemical products.

And also it will provide, too, for the NRR to perform audits in this area.

And thirdly, this will provide the licensees information for their assessment of the head loss caused by chemical products.

The end products, again, will be a NUREG/CR report. It is in progress. And the publication is expected in October this year.

Now I would like to hand this over to Bruce.

CHAIRMAN WALLIS: Now in the NRR presentation at the workshop the statement was made that NUREG/CR-6224 is unsuitable for the kind of material that's in a sump.

MR. TREGONING: I'm sorry. What was the--

CHAIRMAN WALLIS: NRR stated in the workshop on May 24th, whenever it was, that this correlation was unsuitable for use in NUREG/CR-6224.

MR. TREGONING: Ralph, you want to take that, or -- I'll be happy to answer it, but you can take it.

MR. ACHITZL: Ralph Achitzl, NRR. I'm speaking a little bit for Shanlai Lu. He's not here right now.

CHAIRMAN WALLIS: Because he's the man who said it, yes.

MR. ACHITZL: Well, we went through ESE

-- actually speak of the devil -- he just walked in.

Well, let me say what we did in ESE and Shanlai Lu basically had the difficulty of whether you could use the correlation or not. And we ended up saying you could use it for scoping because of temperature effects and other effects, debris, that didn't correlate well in the CalSil data. The ESE was written that you couldn't use that alone as the basis like we did on the boilers. So what was being said at that point is you must do individual plant specific testing, but you could use the correlation for scoping. I think that's the gist of the comment that was made at that.

CHAIRMAN WALLIS: You could use it as sort of a guide --

MR. ACHITZL: As a scoping tool.

CHAIRMAN WALLIS: A guide, but you couldn't use it alone to predict things. You had to have it backed up.

MR. ACHITZL: You had to have it backed up with testing.

CHAIRMAN WALLIS: So plant specific tests. Yes.

MR. ACHITZL: Or different correlations. I guess it wasn't ruling out different correlations.

MR. TREGONING: And that's what the SE said. So there was no new position in the May meeting.

CHAIRMAN WALLIS: Okay.

DR. LETELLIER: So the Committee has not seen any of this work except for some minor illusions to it in the February briefing.

As T.Y. said, this was intended to compliment the ICET program by providing some preliminary head loss indications of actual materials that were resident in the chamber, either fiberglass that had been incubated, so to speak, or cultured in the solution or the solutions themselves to look at changes in -- or latent precipitates that might be present.

Just really quickly acknowledge co-authors, two of our graduate students who have successfully matriculated in their master's level. And Will Roesch turned to the dark side. He's now consulting for industry with Alion Corporation.

The presentation we're going to look over the apparatus, just talk about the equipment that's involved. A very simple look at the samples, the fiber samples that were fabricated to essentially avoid the complication of bad compression during the head loss measurement. We tried to avoid that problem by

precompaction.

We'll look at the rinse procedure, in particular how we attempted to control temperature.

The test matrices will give you an overview of all of the items that were surveyed. And in essence this was suited to a survey type of exercise where we could execute rinses very rapidly and collect a lot of data for a variety of test objects.

The theory development deserves a little bit of study because it's not a constant velocity system. You need to understand how to convert the velocity measurements that are -- velocities that are observed into a head loss effect and how ultimately that's converted into a inference of the hydraulic parameters that you might be assuming for a head loss correlation.

And finally, hopefully, we'll spend most of our time on representative results looking at the baseline loss characteristics of the empty column itself, looking at the clean fiber samples which serves as a baseline for all of the products that were present. And then look at ICET-5 which was investigated the most thoroughly. We actually archived about 100 gallons of solution at the test

temperature so we could work through a well designed test matrix.

CHAIRMAN WALLIS: This is the one that has this aluminum in some sort of colloidal form, is that it?

DR. LETELLIER: Test 5 is representative of the aluminum --

CHAIRMAN WALLIS: That stuff has been stored for a long time. Is it still in the same form as it was when it was ICET.

DR. LETELLIER: That's a very good question. The material was tested within 30 to 45 days post-test. So it was rather mature compared to the surrogate material that Argonne has tested. It does represent in some sense a continuation of the ICET exposure condition with the exception of the corrosion products that were present.

The whole concept was inspired by a couple of problems. First of all, we had made the decision not to do any institute pressure drop measurements as part of ICET, but yet people were clamoring for that information. Everyone's curious. So there was an opportunity because we had been extracting samples periodically, we thought if they could be arranged or configured in a manner convenient for testing, we

should continue doing that.

Just some simple studies. Remind you that the drain time kinetics are all that you really need to know to understand the hydraulic properties. For an empty column --

CHAIRMAN WALLIS: This is presumed to be something on its side here that's --

DR. LETELLIER: Did it work?

CHAIRMAN WALLIS: You have gravity that's horizontal in New Mexico? Hey.

DR. LETELLIER: Did we all see that? It drains in approximately 1½ seconds from top to bottom. But if you put clean fiber in place, it's much slower and it could be photographed digital imagining. We actually instrumented this with a pressure transducer that I'll show you.

CHAIRMAN WALLIS: Oh, there's a weighted ball on top of this water that pushes it through or something?

DR. LETELLIER: That was just a visualization technique so we could see the top of the water.

CHAIRMAN WALLIS: Actually, there seems to be more head than just above the screen. Because it actually keeps going. When the ball reaches the

screen, it's still moving. So presumably there's a siphon or something that's sucking it out as well as pushing it.

DR. LETELLIER: Well, I think gravity's doing the work. And whether it's pushing or pulling is a matter for philosophical debate.

CHAIRMAN WALLIS: There is the one that's below the screen that's sucking it out, too.

DR. LETELLIER: This is a schematic of the design. And what you're referring to is what I call the exit chamber. And it was important to have a continuous flow of water through the bed. We didn't want instabilities to form and allow air pockets to percolate back through the bed.

Some configurations do show that behavior. And if I were doing this again, we would make the exit column much longer.

It also has a throttle valve for controlling the flow so that we can have velocities that are in the range of interest. Under gravitational free fall this drains in about half a second. So we need something that's more moderate, but yet we don't want to burn all of the potential energy in the throttle. We need something that's primarily burned through the sample so that we have good

fidelity data.

We instrumented the column with a single pressure transducer that's located a few inches above the bed. The sample itself is inserted in a two inch cylindrical ring in this coupling here.

CHAIRMAN WALLIS: You could also take photographs. You could take a movie so you know the velocity from the movie.

DR. LETELLIER: The very first tests were done that way. It's extremely labor intensive but in fact it would be my preference, it would be much more reliable. I'll show you what challenges we had with data analysis with an electronic pressure transducer.

We also eventually when we moved to --

CHAIRMAN WALLIS: Frame-by-frame you can read a video very quickly.

DR. LETELLIER: If it's automated.

MEMBER SIEBER: With graduate students.

MR. TREGONING: Rob Tregoning.

The challenges we wanted to do testing at elevated temperature. And these are fairly long slender columns and we were trying to minimize the amount of heat loss we had during the test as well. So there were sort of balances that we made.

CHAIRMAN WALLIS: Oh, so you covered them

with insulation?

DR. LETELLIER: That's right.

CHAIRMAN WALLIS: Ah.

DR. LETELLIER: This is shown without the thermal insulation.

CHAIRMAN WALLIS: Okay.

DR. LETELLIER: But there are clever ways around that, magnetic tracers and other pressure taps.

This was honestly a poor man's head loss loop, but I think it was very successful. It's serving its intended role.

We did instrument this with a temperature probe just below the sample. We made every attempt to preserve the temperature of the rinse solution and the temperature of the fiber sample. We did that through constant temperature storage ovens.

Approximately 3.7 liters passed through this sample in each rinse. And, of course --

CHAIRMAN WALLIS: A gallon.

DR. LETELLIER: Approximately a gallon. And, of course, the length of the column is the maximum driving pressure. We had about six feet of head available. In combination with the throttle valve we had test ranging, velocity ranges from .05 to 1 foot per second, which is perhaps a little higher

than prototypical some screen designs. But the intent here is to quantify the parameters of the correlation which are debris specific not velocity specific.

There's our test article. It's a 2 inch diameter ½ inch thick precompressed fiber puck. There's about six grams of fiber once we compacted it and --

CHAIRMAN WALLIS: Got a hole in the side there where your arrow is?

DR. LETELLIER: Yes. There are some occlusions, none of these observed to pass all the way through, but there is a variety of uniformity. And that was the reason it was important to us to characterize the baseline. I'll show you those results.

We started out with 7 grams of fiber, but just the manufacturing process we lost about one gram. So when we calculate effective porosities, it's best to use the dry mass of the fiber that's determined post-test.

The test articles that were exposed to ICET actually dissolved partially. Some the order of a tenth of a gram might have been released to the test solution.

And some of our blanks experienced some

agitation during storage. So all of this contributes to some interpretation of variability. Despite being precompressed to about seven times the manufactured density, it's still 92 percent porous. There's a great deal of open space.

The fiber samples were supported underneath by a retaining screen and also on the top. And so you'll see some pictures of test articles that have a waffling pattern on top.

Generic rinse procedure includes preparing all the bottles. Again, the intent is to maintain the temperature as well as we could manage in a manual fashion.

We would actually fill the lower chamber before inserting the sample so that we could contribute to constant temperature. And then we would fill it from the top using this diffuser arrangement. And the intent is simply to avoid direct water impingement from disturbing the bed.

Manually opening the valve at the bottom initiates the test. We had automatic data acquisition from the pressure tap and the throttle valve was closed before we actually exposed the fiber sample. The intent here is that we could repeat multiple rinses over a full cycle. And typically we would rinse

each puck five times.

Photographs, dry weights constituted the data record. Okay.

The text matrices first of all, consist of the performance data. The basic flow losses inherent drag coefficients for the empty column including all of the retaining screens and rings. WE also did some studies using pure sand beds that are suspended on filter paper just for comparison to theory. We did some studies with sand loaded fiber for comparisons to handbook type of values. And, obviously, a rather extensive characterization of the clean fiber variability.

For the test articles we had fiber samples that were exposed to ICET-3 environments beginning on day 4 following the observation of primary precipitate. If you remember test 3 is the calcium phosphate condition. So most of that had settled, the water clarity was very good by test 4. Sorry, by day 4.

For test 4 the samples --

CHAIRMAN WALLIS: Rinse with clean water. ICET the fibers have some calcium phosphate in there?

DR. LETELLIER: That's correct.

CHAIRMAN WALLIS: And it stays there when

you rinse them?

DR. LETELLIER: That was one of the questions. We could study both -- well, for test 3 and 4 we could only study the release. We could study the shedding of that material through multiple rinses.

For test 5 we actually archived solution. So for the test 5 fiber we rinsed it with both test 5 solution and clean water so we could look at the differential.

For test 5 by that time we were excited about some potential value here. We had a well designed test matrix and we had 12 samples total to examine.

Test 5 was representative of the aluminum hydroxide chemistry. And remember from test 1 we had observed a visible precipitate when it was cooled. We never observed a visible precipitate in either test 1 or test 5 at the test temperature of 6°C. So when we determined test 5 we archived a 100 gallons of solution in the oven until they were ready for use.

We also looked at some rapid cooling of that test 5 solution over a 20 degree temperature drop to hopefully explore the idea of a temperature of a heat exchanger before rinsing that through clean fiber. And we had some very cursory examines of

calcium phosphate that we prepared in a surrogate manner. So all total we have over 60 rinses with replicates of many different configurations.

The cooled test 5 solution was examined in a nested incremental test matrix so we could look at five different samples that were rinsed an increasing number of times. And in that way if the trends are good, we could look at both accumulation and shedding by looking at differences. And I'll show you some of that information.

So in essence, this matrix looks at a set of nested replicas. There are five reps of a single rinse, there are 4 replicas of 2 rinses, et cetera.

The measured viscosities, I think Bill alluded to this earlier, test 5 solution at 60^{o} at the end of the test was slightly higher than clean water as far as measured viscosity.

The cooling rates that we were able to induce just using ice baths and immersion are almost 3 degrees C per minute. And, honestly, I don't know how that compares to a heat exchanger. I expect it's rather slow compared to the heat exchange in the plant. But in fact we measured about the same viscosity at 40 degrees. What was interesting is we did observe a visible precipitate at that temperature.

And in fact by the end of our testing, even the archived solution at 60 degrees had some evidence of settling at the bottom of the containers.

A data typical data record looks like this. This is a pressure trace over 5 consecutive rinses. And you can see that the column is filled. We check out the operation of the data acquisition and it's rinsed.

What's important to preserve is to look at the time delay in comparison to the temperature. Again, we had a target temperature but we weren't completely effective at preserving that.

So the total data analysis follows a history. First of all, you have a presumed or well developed head loss correlation with some free parameters that are determined by the material properties. What we get from the drain column rinse is a pressure trace that has to be converted into a velocity. So that's the first step. Given the velocity, given the kinetics of the system, then we can calculate the delta p, the head loss measure.

CHAIRMAN WALLIS: Well, in theory given if you understand the porous media, you can predict the whole experiment.

DR. LETELLIER: You can, that's right.

CHAIRMAN WALLIS: You don't need to know anything except that you saw this height and you let it go.

DR. LETELLIER: That's true except as you pointed out, that delta p is a more accurate definition of hydraulic loss than simply the head, the static head. So given the sure stress, dissipation and other irreversible effects it's not sufficient just to know the height of the bed. It's essentially a comparison between the free fall velocity history and the observed history.

CHAIRMAN WALLIS: Well, you know the pressure because of the -- you know the height, you know that velocity gives you another rate of change of height. So you're in pretty good shape.

DR. LETELLIER: That's true. This illustration just recounts the data analysis steps that were executed. Intuitively it's all very obvious.

Once you have a delta p then you still have a statistical step of inferring the value of the parameters from the data and then you can feed that back to refinements of the head loss correlation.

The velocity history simply records the conversion of potential energy to kinetic energy. Not

much more to it.

CHAIRMAN WALLIS: I don't know what you mean by kinetic energy and this stuff is just oozing through the bed.

DR. LETELLIER: Well, you saw an example where even clean fiber will drain 6 feet of head in about 8 to 12 seconds.

CHAIRMAN WALLIS: Kinetic energy maybe in that case, but most of it's on a -- if you've got particles in there, do you worry about the kinetic energy?

DR. LETELLIER: Some terms dominate, others do not. When we added calcium phosphate, we would wait onwards of ten minutes.

CHAIRMAN WALLIS: So you don't worry about kinetic energy at all?

DR. LETELLIER: Not at all. But, of course, the terms are there.

The pressure tap cam be converted into a velocity simply by using an unsteady Bernoulli equation. You have to account for the possibility of acceleration, and that's what this reduction shows you. There are two terms. When you initiated the rinse there is some initial acceleration. It's in free fall, for example. And then when it's draining

steadily, it's simply the static head that drives the flow.

Because there is noise in the data, and I'll show you an example. There are various physical constraints that are useful for obtaining a realistic velocity profile. For example, the instantaneous acceleration at any point in time cannot exceed --

CHAIRMAN WALLIS: You can also throttle with the ball valve and run a slower experiment.

DR. LETELLIER: We certainly could.

CHAIRMAN WALLIS: You know the pressure drop characteristics of the valve, presumably, so --

DR. LETELLIER: We certainly could. There was some initial design effort to choose a throttle velocity that would get us in the right range. But this was a time compressed experience, so we collected as much information as we could with the apparatus we had.

Obviously, the derived velocity cannot be greater at any point than the velocity that you would have simply under free fall. And eventually the cumulated displacement can't be greater than the height you started with. So these are all attributes that can constrain the numeric solution.

This is what I'm talking about with regard

to the initial acceleration where the bed essentially drops in free fall for just a fraction of a second.

CHAIRMAN WALLIS: You can open a valve that quickly?

DR. LETELLIER: That's a good question. The sampling we made about 30 to 50 milliseconds. And we saw very regular patterns that looked like this, even from the photographic data which are only 200 frames per second. So in my opinion we were resolving some of that initial acceleration phase. There are a lot of proposed improvements to the equipment, and an electronic actuated valve would be one of them. But the noise, I call it noise, it's more of an extraneous information because when you do open the valve, there are acoustic pressure waves that bounce back and forth from the valve to the surface. And they are being resolved by the pressure transducer and they can travel back and forth many, many times within the sampling rate.

So we tried several numeric techniques to smooth this out. Simply using a repeated rolling average to smooth the data seemed to help. Backwards differing to reference the previous velocity which starts out to be zero.

CHAIRMAN WALLIS: By the first few data

points?

DR. LETELLIER: You can. And, in fact, that was the most effective. We only used the data where the acceleration was negligible.

I really think the photometric data would be more reliable, but it's very labor intensive to do it manually and it's complicated by the thermal insulation. So there's some improvements that could be made.

CHAIRMAN WALLIS: Rather than counting droplets or something like that graduate students used to do --

DR. LETELLIER: The Millikan oil drop experiments? That's before my time I think.

A simple energy balance on the drain column eventually identifies which terms are important. This is cast in terms of the cumulative energy expended. So the first term is how much potential energy has been liberated, if you will. The second term is how much kinetic energy has passed through the nozzle of the outlet. And the last term is a combination of how much kinetic energy remains in each section of the drain column at any point of time.

And the beta factors are to account for some complexities in the flow patterns, which you may

or may not be willing to ignore. But once you have an expression like that and you know the velocities, you can evaluate it any point in time.

The cumulative energy that passes through the column in terms of a the delta p across the bed is equal to that expression. And although we know that delta p is largely driven by the static head, it's not exactly equal to pgh because of our expression for cumulative energy accounts for the finite thickness. It includes irreversible acceleration effects within the bed and the flow irregularities. So there's a minor difference.

Given the delta p and your favorite head loss correlation, which is shown here as the two term model, a linear and a quadratic velocity model, it can simply be plugged into that expression and set up as a matrix equation. Remember we have a data vector of pressure measurements at a very high temporal resolution. So we're not just measuring the steady state delta p at a given velocity. We're concurrently or sequentially measuring delta p over an entire range velocities for each test.

If you know the velocities, the only thing left to determine are the coefficients a and b. And of course, those could be complicated expressions of

porosity, specific surface area, all of the geometry factors that Bill is going to discuss next.

And, of course, we need at some point correct the data by assuming that the total pressure drop is really the contribution of the column plus the debris. But the contributions of the column are very, very small compared to even clean fiber.

Any desired correlation could be fit to this data. You could dream up any complex physics that you like and still fit it to the same set of data. This is a linear model, obviously.

CHAIRMAN WALLIS: But you have trouble if you have -- well, I guess you have enough data points you can fit an infinite number of variables?

DR. LETELLIER: Almost This is a highly over determined data set. In fact, because we have an independent estimate of porosity, we have the dry mass, we really only have one free parameter here.

So the fits themselves, the values of these parameters are supported by the range of drain velocity in a single sine. But once you know that you can evaluate the correlation over a common range. And that turned out to be the best basis for comparisons.

Some of the performance data to take a look at, for the sand-bed comparisons we ran 5 rinses

each. We used --

CHAIRMAN WALLIS: Sand at 10 grams and 7.5 grams or different sand?

DR. LETELLIER: Two different quantities of -- it is different sand.

CHAIRMAN WALLIS: Of different sand? Otherwise, you expect ten to be twice five, but it isn't. Then is twice 7.5 in terms of pressure drop, right? You look at 4 and 8 and velocity of .6, one is twice the other.

DR. LETELLIER: That's a good observation. I hadn't noticed. It is a sieved, it is size graded between a range of 7.5 --

CHAIRMAN WALLIS: It has a different --

DR. LETELLIER: 105 microns.

CHAIRMAN WALLIS: A simple test would be to have the same size two different thicknesses and see.

DR. LETELLIER: I'm not sure what you mean by the same sand.

CHAIRMAN WALLIS: Well, if I used the same sand five grams and 10 grams, I expect twice the pressure drop with 10 grams. And it's sort of a check on whether things were reproducible and all that.

DR. LETELLIER: Sure.

CHAIRMAN WALLIS: Anyway, the theory works out.

DR. LETELLIER: Quite nicely.

CHAIRMAN WALLIS: It's very linear, isn't it?

DR. LETELLIER: This, in fact, this was fit just to the linear term.

CHAIRMAN WALLIS: Right.

DR. LETELLIER: Over particle ranges. In fact, one of my questions is you can see the precipitate Reynolds numbers are portrayed in the bottom panel. And over the range of 1 to 6 the linear determinant transition for flow around a sphere, there is a significant amount of data in the turbulent range and yet the data set matches the linear term remarkably well. And --

CHAIRMAN WALLIS: So it's real turbulence. It's just when the inertia terms --

DR. LETELLIER: Start to dominate.

For empty-column baseline I'd just point out some comparisons in the linear coefficient. For sand it's on the order of ten to the eighth in the appropriate units. For clean fiber it's a little bit higher. Yes, the coefficient for the empty column even when we had the 200 mesh sieve is four orders of

magnitude lower. So the hydraulic resistance of the drain column is negligible.

I do want to stress that this is preliminary information. We've spent a lot of time in organizing the data for presentation in some draft deliverables. So it may change by the next time you see it. But I did want to present the attributes of the information.

We had a specific study done with what we considered to be identically prepared fiberglass samples. But there's a lot of information that we can use to help judge repeatability. For this set of four samples we executed five rinses each with 40 degrees C clean water. And the parameters that we derived for each of the rinses were simply averaged in an arithmetic way.

CHAIRMAN WALLIS: Each rinse presumably gave the same answer?

DR. LETELLIER: Well, I'm sure there is some variability.

CHAIRMAN WALLIS: Or pretty close? Much less than the variability between samples?

DR. LETELLIER: That's the point, exactly. It would be appropriate for us to pool or lump all of the data for a single test article into a single

correlation. That's something we can examine.

I want to go back and make sure that these were all evaluated at a common temperature. Because there is temperature variability between our tests. We need to rebaseline them for an accurate comparison.

CHAIRMAN WALLIS: What looks a bit strange is that the higher curve is more linear than the lower curve, and you'd expect that higher head loss would have more of the squared terminate, wouldn't you?

DR. LETELLIER: Of these five tests that lower curve was the only one that exhibits significant quadratic behavior.

CHAIRMAN WALLIS: Well, it's strange. You think it would be the higher one that would do that.

DR. LETELLIER: In fact, you can tell that I've truncated. I've clipped this because that quadratic behavior starts to dominate at higher velocities.

CHAIRMAN WALLIS: Yes.

DR. LETELLIER: And that was purely -- it's somewhat an artifact of the range that was chosen for the data presentation. I need to go back and determine exactly what range velocities existed in the test data. And the final report will show that as end points so it's clear where we're extrapolating beyond

the fits.

As I mentioned the nested ICET solution matrix will yield additional direct comparisons for this issue of repeatability. But for now I would suggest a kind of a rule of thumb of a 50 percent variability. And that was estimated simply at the .1 foot per second velocity. There's a range from the median of about plus or minus .5 feet per second. That's quite a large margin, but it's important to know that to help us interpret the next plots.

CHAIRMAN WALLIS: Actually, the red one is about a quarter of the green one or something at that point. There's a significant different velocities.

DR. LETELLIER: That's right.

CHAIRMAN WALLIS: So you have to wonder what's going on right down there.

DR. LETELLIER: My first exercise would be to go back and make sure that they're being presented on an equivalent basis. But there are many reasons for possibility variability. You noticed one, the uniformity of the fiber puck itself is probably the biggest one.

CHAIRMAN WALLIS: Yes, there's a hole in it, it would be squared.

DR. LETELLIER: None of those occlusions

were observed to penetrate he entire puck. In fact, they were pressed with considerable force and extruded onto the walls of the pipe. But the preparation procedure, PANL has struggled to come up with a suitable recipe for very regular fiber mats. And this was done much more quickly than their study.

There was an initial decision made whether we should simply immerse a fiberglass filter. That would be a suitable median for incubating chemical products. But the decision was made to use a representative debris type. Even poorly homogenized fiberglass was judged to be preferable over an artificial substrate.

We had a number of different rinse solutions. Notably clean water and three different temperatures, room temperature, 40 degrees, 60 degrees C. And then by comparison the test 5 archive solution which was tested at 60 degrees. Now these have been rationalized to a common temperature and as expected water behaves like water, the theory explicitly factors the viscosity. And so they behaved very regularly.

CHAIRMAN WALLIS: It looks as if the viscosity is over twice as much. I thought earlier you said it was not so different.

DR. LETELLIER: It does, and that's one of my questions to be resolved. But even with the factor of 50 percent variability on the fiberglass behavior, this is pretty clear evidence that there's something unique about the test 5 solution.

CHAIRMAN WALLIS: This is presumably the same puck, isn't it with test 5 and with water?

DR. LETELLIER: No.

CHAIRMAN WALLIS: It's not the same puck?

DR. LETELLIER: No. They're all unique fiber samples.

CHAIRMAN WALLIS: Okay.

DR. LETELLIER: That's why it's important to understand the variability of the blanks.

The cumulative test matrix has somewhat inclusive results, but the trends are I guess intuitive. What we're looking at is a set of data that represents -- at the bottom is a single rinse. The green is actually two sequential rinses. The red is three. The black is four. And the top is --

CHAIRMAN WALLIS: Is it all with clean fibers, not deposits in them?

DR. LETELLIER: That's correct.

CHAIRMAN WALLIS: Yes.

DR. LETELLIER: Clean fibers. There's a

unique or a brand new blank used for each of these--

CHAIRMAN WALLIS: It is strange the way you got that top curve is so linear and then there's a very parabolic curve coming up to it. That top curve is very linear.

DR. LETELLIER: Yes. The parabolic curve is actually not part of the cumulative matrix. It's put in there for reference. And it's simply clean water at 40 degrees. And it had -- I'm not sure Will choose this particular example, except to show that it is within the data range. It is within the range of variability.

CHAIRMAN WALLIS: It's not corrected in any way, so how does it manage to get such a high pressure drop compared with the other stuff?

DR. LETELLIER: That's something to be--

CHAIRMAN WALLIS: I mean it doesn't make sense somehow.

DR. LETELLIER: Something to be investigated.

CHAIRMAN WALLIS: It looks very strange.

DR. LETELLIER: The entire data set needs to be scrubbed for consistency so that all the analysis methods are done the same and it's presented in a rationalized basis.

DR. SHACK: Okay. I'm losing the curves Is the top curve the clean water curve?

DR. LETELLIER: The top curve is the result of ICET-5 solution with five sequential rinses. It's the accumulation of --

DR. SHACK: It's the T5 curve?

DR. LETELLIER: That's correct.

DR. SHACK: Okay.

CHAIRMAN WALLIS: That parabolic one looks almost exactly a parabola if you look at the numbers. It's very much a square.

DR. LETELLIER: I'm certain that it is.

CHAIRMAN WALLIS: But for one of these A zero and the other one B zero.

DR. SHACK: Now which one is the clean water curve?

DR. LETELLIER: The parabolic one.

CHAIRMAN WALLIS: Parabolic is strange.

DR. LETELLIER: And the only reason that it's portrayed here is to remind you that the variability in the fiber blanks is at least 50 percent. So this sequence wasn't necessarily conclusive by --

CHAIRMAN WALLIS: Is this because of the Reynolds number transition that higher viscosity can

actually lead to a lower pressure drop?

DR. LETELLIER: I wouldn't think so at these velocities, but it's worth looking at.

CHAIRMAN WALLIS: Okay.

DR. LETELLIER: Presumably because there are some trends in this data, had we run six, seven, eight cumulative rinses perhaps the pressure drop would have increased.

DR. SHACK: Where is your head loss --

DR. LETELLIER: The pressure tap is not too far above the bed. It's located reasonably close to the top of the fiber sample. There's a lot of information to assimilate and present as part of closing out this study. But we have some significant findings.

DR. SHACK: Now when you say the cooled solution, is this the supernate or you've kind of mixed everything up?

DR. LETELLIER: This was test 5 archive solution which had no visible precipitates. It was drawn off of the top of the tank so it was a supernate in that macroscopic sense. And it was stored immediately.

DR. SHACK: But as you stored it you saw precipitates. Now you then supernated it again?

DR. LETELLIER: It was minimal. It was just a residue in the bottom.

DR. SHACK: Okay.

DR. LETELLIER: I guess the reason I mentioned that is it was significant that the solution had matured to that point even at 50 degrees.

CHAIRMAN WALLIS: I don't see any data with the puck filled with deposit. Isn't that what the whole point of the experiment was to test pucks that had actually deposits in them?

DR. LETELLIER: Indeed. And that information is just not here, this specific.

CHAIRMAN WALLIS: Not there yet? Well, you're giving us rather trivial information so far compared with the real thing --

DR. LETELLIER: Well --

CHAIRMAN WALLIS: -- which is what happens when you have the deposit in the puck. You don't have any of those?

DR. LETELLIER: Let's see.

CHAIRMAN WALLIS: You have a backup slide. I thought you were going to tell us that the pressure drop was ten times as much or something when you had the deposit in it.

DR. LETELLIER: No. This bullet here on

the summary partially addresses your curiosity.

CHAIRMAN WALLIS: Yes.

DR. LETELLIER: We did test number 5's test 5 solution and there was no significant difference between the exposure duration whether it was in the tank for 20 days or 30 days or 10 days was rather irrelevant.

CHAIRMAN WALLIS: So is the idea that the puck picks up deposit or not?

DR. LETELLIER: That was one of the questions.

CHAIRMAN WALLIS: That was the idea it might do?

DR. LETELLIER: I have not presented the data for test 3 and 4 fiberglass.

CHAIRMAN WALLIS: It looks as if it did not pick up deposits?

DR. LETELLIER: That's right. The most significant difference here was in the behavior of the test solution, not in the fiberglass sample.

CHAIRMAN WALLIS: But the behavior of the test solution is a little bit irrational if you look at those parabolism straight lines -- and so you still have to work it out?

DR. LETELLIER: Clearly so. What I was

hoping to show here is that there was some evidence of increased head loss for test 5 solution beyond the variability of the fiber samples. This is not extremely high fidelity data. There's --

CHAIRMAN WALLIS: There is nothing like the Argonne tests where they got huge changes in head loss because of deposits in the bed?

DR. LETELLIER: That's true.

CHAIRMAN WALLIS: You don't have any evidence of deposits in the bed, apparently?

DR. LETELLIER: We have visual evidence from the ICET samples.

CHAIRMAN WALLIS: Does it have any effect on the pressure drop?

DR. LETELLIER: I'm sorry, I just can't present that test 3 and 4 fiber right now.

CHAIRMAN WALLIS: So this is ongoing work?

DR. LETELLIER: Yes. It's within a few weeks of having a draft NUREG for comment.

CHAIRMAN WALLIS: Your boys get a thesis out of it?

DR. LETELLIER: They did.

CHAIRMAN WALLIS: Yet it isn't finished yet, is it? It was all anomalies and --

MEMBER MAYNARD: Feeling more of it's been

resolved, he thought he would present it today.

DR. LETELLIER: The same questions were asked during the defense, I assume you.

CHAIRMAN WALLIS: Well, put me on the thesis committee. Spared that.

DR. LETELLIER: I keep telling my students that their defense just prepares them for future ACRS briefings.

CHAIRMAN WALLIS: I tell my students I'm going to be so hard on you that when you get to the Committee it's going to be a breeze.

DR. LETELLIER: Just some overall observations about the whole concept of the drain column methodology.

One thing that I very much like about it, it naturally permits the correlation to be made over a range of Reynolds numbers. You obtain that naturally because of the time dependent velocity and you don't have to worry about those complications for preserving constant velocity pumping conditions. Because we can recover the bed immediately you have an estimate of the in situ porosity. You know the dry mass that was actually tested at least at the end of five rinses.

And because you can execute these into

discrete volumes, you have a very high data return per sample investigated. You can actually do some differentiation here to examine accumulation and shedding.

CHAIRMAN WALLIS: This is all in principle, but then if you look at all the curves you got and evaluate the a's and the b's, you might get something which looks like sort of a random walk. I mean, the a's and the b's may not make any sense when you look at them.

DR. LETELLIER: There's clearly some improvements that could be made. An automated level height detector, for example --

CHAIRMAN WALLIS: But can you draw any useful conclusions? A simple experiment, a nice idea. But the thing is can you draw any conclusions from analyzing the data?

DR. LETELLIER: I think that the jury is still out on this particular data set, but the technique in general, I think the bed variability could be reduced with very little effort. For example, using filter paper as an alternative.

The precompaction exercise I think was very effective, an important simplification for this study. And within the 3 or 4 weeks of time that we

had it was ideally suited for a survey type of examine.

So that's the status report that we have to offer. As T.Y. said, the publication date is October time frame.

CHAIRMAN WALLIS: That concluded, is it time we took a break? We take a break for 15 minutes until 3:15.

Thank you very much.

(Whereupon, at 3:02 p.m. off the record until 3:21 p.m.)

CHAIRMAN WALLIS: Okay. We all ready? Do we have a presenter? We're going to hear from PNNL, too.

Please come back into session. We'll move with the question of particulate head loss testing and correlation development.

MR. KROTIUK: I'm Bill Krotiuk and this is Carl Enderlin from PNNL. And we'll be talking about the head loss testing and modeling, that effort that we are pursuing at the NRC and at PNNL.

Okay. First we'll talk about the head loss testing aspect. The work that was being done at PNNL was to do some confirmatory head loss testing using typical debris and insulation. Insulation

debris and coating debris. And we wanted to characterize various items, composition, distributing of debris in a bed, fluid temperature effects. And we designed a facility to have certain characteristics that we wanted. And ultimately the data would be used in developing an improved head loss calculational method.

CHAIRMAN WALLIS: Now you showed last time that the head loss could vary by almost two orders of magnitude depending on how you built up the bed.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: Is that going to be included in the calculational method?

MR. KROTIUK: I will address that, yes.

CHAIRMAN WALLIS: Thank you.

MR. KROTIUK: The head loss modeling addressed two items. One is the calculation of the pressure drop itself and the other is the compression of the insulation debris that would be accumulated on the screen or perforated plate that we were testing with.

The motivation really for the testing and the modeling is to provide testing -- sorry. Is that previous testing indicated the further need to evaluate the effects of particulates that were mixed

with fibrous debris in the debris bed itself.

We wanted to address concerns regarding the appropriateness of certain characteristics of the previous testing.

And with regard to the modeling, we wanted to address possible deficiencies in the older CR -- the NUREG/CR-6224 correlation. And ultimately we also wanted to be able to include the coatings debris effects in the head loss calculation and also in the analytical modeling.

The regulatory applications, we wanted to support the 2004-02 resolution of that item. And we wanted the additional head loss testing data to evaluate licensing submittals and to provide insights on how the variations and the reconcentration could affect plants head loss. And ultimately in the modeling we wanted to give a calculational tool that could be used at least to give the estimates of head loss across the debris bed.

The current status is this: Is that we've completed all scheduled insulation and coatings testing in May. I completed the development of the computational model in May also. However, there are certain parameters that have to be verified and worked on a little bit more in the model using the test data.

And since the testing was just completed in May, I still have to go through some of the latest tests and update some of the semiemperical multipliers.

I'm going to present some results with what I believe are the current anticipated values of those, but they may change as I look at more of the data.

We ultimately want to have the modeling, NUREG and the testing NUREG released in October. That's the current schedule.

Let's go here.

MR. TREGONING: Just while this is coming up, just logistics. There's a different package for the next presentation. It's the one entitled PNNL Activities Associated with Head Loss Testing for Sump Screen Debris Beds in Support of the Resolution of GSI-191. So we'll do this -- this package will be next, and then we'll go back to the package that Bill was just working through.

MR. KROTIUK: Okay. Go ahead.

MR. ENDERLIN: I'm Carl Enderlin from Pacific Northwest National Laboratories. Tom Michener is also present, and I've listed other members of the test crew here.

We're going to talk about just a quick

review of the setup and capabilities, the measurements we've taken in a review of debris loading sequence results. These initial review I'm going to breeze through rather quickly unless there are questions on them. This is all material in those first two bullets that was presented back in February.

Then we're going to talk about there were some issues on that February presentation, and those have been resolved. And that's the description of Series 2 test conditions and measurements that were taken.

Overview of the test procedures, just to give an idea and to clarify some things that might be slightly different than the initial test program.

Then we'll talk about results, basically four cases. Results of the NUKON only bed, CalSil only, the CalSil and NUKON combined and a few tests that have been performed on coatings. And we'll do a brief summary of initial findings.

We have two test loops that data has been taken from, both a bench top loop which is a 4 inch screen but doesn't have the fully developed up and down stream flow that the large scale loop has. The large scale loop is a 6 inch diameter test section. We're measuring the delta p across the bed from

approximately 10 diameters upstream to 10 diameters downstream. And we're using an array of 4 pressure transducers so that we've always got several transducers on line. And the data that I'm reporting will be from basically the lowest pressure transducer on line at time.

The in situ debris bed height measurements were taken for all these tests using the optical triangulation method we presented back in February. We have a filtration system that was used for these tests. And I'll discuss in the overview of the procedure of when the flow or the loop flow is filtered.

And then our debris injection system is different than some of the other tests as we used a closed system that gives us controlled dilution and introduction method as far as flow through that debris injection chamber.

This is the slide that Graham Wallis referred to about showing several orders of magnitude difference in the head loss that was obtained. What I'll do is just briefly talk about the different cases.

There were actually four cases that were looked at. We've got the premixed. This means the

material is prepared individually and then before being put into the loop, the particulate and the fiber is combined together.

We have the case where we inject and make a NUKON bed first, get a steady state or based on our experimental criteria for steady state bed formed, and then CalSil material is introduced into the loop.

And the last which is the time delay. And if you look at that, there's actually three cases there. One of them you only see one data point because it was such high head loss as the bed was created before the bed actually reached a steady state. So it's at the pressure at low flow in the bench top loop in which we actually formed a bed.

In the time delay case CalSil is introduced into the loop, allowed to pass through the screen and then there's a time delay which for these were on the order of about 11 seconds. And we've done some additional work. That time delay is basically dependent also on what your loop volume is. But in that the CalSil is allowed to pass through the screen so any fiber material from the CalSil that wants to hold up on the screen, then the NUKON is passed through. And without a lot of the things I talked about last in February, what you have is a different

building process that's going on. Whereas with the NUKON bed first, that same building process or plugging process goes on in just a top layer of the NUKON. Here you have the CalSil able to go to the preferential flow path through the fiber bed as it's being built.

So what we've shown here is basically by the loading sequence we can have a significant impact on what the head loss is.

MEMBER SIEBER: Carl, what's your circulation time in your bench top loop? Your repeat time?

MR. ENDERLIN: At .1 feet per second, I believe it's on the order of 30 seconds. Actually, I think it's closer to a minute. I wouldn't take that to the bank, though. There's numbers I know, that one I don't know off the top of my head.

So this load sequence along with the preparation method of the material that I saw showed to be extremely influential on how or what head loss we measured given the same debris loading on the screen. Everything here, data we've seen, is material that was presented in February.

So now based on those issues these are the conditions we use for what we refer to as a Series 2

test. The debris constituents for all the data that I'm going to show here forward was premixed prior to introduction of the loop. We've used the perforated plate for all these tests. This is a 5 mesh screen, which is what the Series 1 tests were done on and is the material that LANL used. This is a 41 percent flow area and the square openings are listed at 1.28, They're roughly 8th inch opening screen. Perforated plate is an 8th inch hole and is a 40 percent flow area. So flow area through the two is roughly the same. This is the material that is the same, cut off the same sheets that Argonne has.

Testing in the Series 2 has been formed at multiple temperatures. Not all cases have been performed at the elevated temperatures. The temperatures were approximately 21 degree, 54 degrees and 82 degrees celsius.

During debris bed formation, the bed was allowed to build while holding the approach velocity at .1 feet per second. In the Series 1 tests we had built some beds at .2 feet per second. Started at .2 and allowed the velocity to decade down to approximately .1. So here we're using a constant bed formation velocity and adjusting pump speed as needed to maintain that velocity.

Following the initial debris bed formation the loop is pressurized to maintain any gas in solution. We were using approximately 2½ atmospheres was sufficient for our work. And so we reached steady state, pressurized the loop and then verified that steady state still exists based on the criteria that I'll talk about in two slides.

Okay. Following that there was this case of any material that may settle due that we're now at .1 feet per second bed formation. So the first ramp up is conducted and then the entire loop is filtered. So we run through a 10 micron filter and that happens for approximately 20 circulations. It's highly dependent upon when the bed reaches the steady state. So we take a measurement, then we go through the filter system and again, to monitor it until we see a steady state reading. But the minimum criteria was put on that was for 20 minutes, which is roughly 5 at .2 feet per second if it were filtering, that is, five circulations through our large scale loop.

Okay. We have three ways we've characterized the beds as far as visual observation. We have a complete formed, which means all the screen is covered. We have channeling form, which after the bed was created and we've gone to higher velocities it

appears that we see individual ports cleared but you don't see any screening material. And the last one is an incomplete bed. That usually means either I can see screening material or I have a sufficient number of channels that have never been covered.

This is just a quick summary of the number of tests that have been done in both combined bench top and large scale loop. Now some of these, such as NUKON only are in the bench top looking at repeatability and evaluating debris preparation. But this just gives you an idea of the number of tests that have been done. And I'm not going to present results from all of these at the moment. All of these tests will be covered and listed in the final report.

Overview of the test procedure, I've covered some of that in talking about those that are criteria or initial conditions for the Series 2 tests.

Our debris constituents are prepared individually for each test immediately prior to injection in the loop. So a loop is up running, steady state flow has been achieved. At that time the debris constituents are prepared, mixed together and introduced into the loop.

The flow rate through the loop, the injection lines, is maintained at a steady value and

it's the same for all of these tests. So there's no variation in the bed formation except for when we get to the coating tests there were some higher velocities had to be used.

Bed formation was conducted for a minimum of 1 hour, 7 complete loop circulations. And this is different than we showed in February where we were going up to 3 hours based on some debris preparation. But basically when we show the results here, we'll do it for the second ramp up. We don't consider the bed completely formed until we've gone through the first ramp up in velocity and then applied the filtration to try to reduce the chance for additional mass to being added.

Our criteria for assuming steady state, this criteria was the differences for a ten minute difference of a one minute average. This was taken for bed formation and are at peak velocities. At the low velocities or intermittent velocities in the ramp up and the ramp downs we used a 5 minute criteria.

Again, filtering was performed after we had this first ramp up and achieved a steady state at .2 feet per second. Then we put the filtering on without changing the velocity and adjust the pump speed to maintain the .2 feet per second speed.

Okay. WE're going to first talk about NUKON results. The pictures of the beds we were able to get very repeatable NUKON beds. The appearance of them is all pretty much the same and you can see that there's a rim.

Again, our screen, if someone has a screen there, the collar. The idea of those screens is the idea of the test section. So the rim is formed because there is no lip inside of the test section.

I've shown the bottom picture to show that even with the prepared fiber as it is, with the NUKON beds there is no material that we can see passing through or hanging through the screen.

Okay. As we go through these if anyone's of interest or looking at these later, the first number in the test IDs is the date, the second four digit number gives us what the total debris loading is. So 27.03 grams per meter squared. And that is the target value that's been introduced, not what was on the screen.

Okay. Now what we're showing is just for the ambient or 21 degree case. What we've done is on these velocity ramp ups and ramp downs, our velocity sequence at an individual temperature consists of 14 velocities. I'm sorry. Several velocities are

repeated.

The sequence is that we formed the bed at .1 feet per second, pressurize and then we take another .1 feet per second, then we do the ramp up to .2 feet per second, take a steady state reading, filter and go to another .2 feet. Stay at .2 feet per second and take a second reading.

CHAIRMAN WALLIS: It doesn't seem to extrapolate to the origin. Is there some error in one of the readings or something that makes that happen?

MR. ENDERLIN: I'd have to look further on that to see. Should we be going through zero --

CHAIRMAN WALLIS: There seems to be a zero error or something.

MR. ENDERLIN: All --

CHAIRMAN WALLIS: They should be linear near the origin anyway, so it looks a little odd that it doesn't extrapolate to the origin.

MR. ENDERLIN: I'll have to look further in that.

MEMBER KRESS: Screen loss has an effect.

CHAIRMAN WALLIS: Maybe there's a screen loss or something --

MR. ENDERLIN: Well, we've measured the screen by itself and this is not -- oh, yes. The

other thing that can be in there is the temperature correction. There has been no temperature corrections for the loop --

CHAIRMAN WALLIS: No flow, there should be no head loss, shouldn't there?

MR. ENDERLIN: Correct. But if the temperature inside my loop is different than the temperature on my DP manifold, I'll have an offset.

CHAIRMAN WALLIS: You might have an offset?

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: Okay. That's probably it --

MR. ENDERLIN: Yes. And this has not been corrected by temperature.

CHAIRMAN WALLIS: Okay. It looks like an offset.

MR. ENDERLIN: Yes. And, again, when we see the elevated temperatures, the maximum worse case we didn't correct it this time just so we could be consistent with the data. In the report that'll be addressed and the uncertainties will be addressed. An absolute worse case would be a 5 inch correction if we add the max temperature difference that we could have in our two legs of the DP meters for the full height

of the leg. But none of the material that is being presented has had any temperature correction done.

This shows a full velocity sequence at the 21 degree C. And what we can see is that .1 feet per second and the .2 feet per second you're getting an increase in head loss with each ramp up each time you're returning to the velocity.

The .05 is a one time we are based on pipe flow in the area of potential transition between lament or turbulent flow. I believe the Reynolds number for that is on the order of 2000.

The other points at the 21 degree C lie outside of that. And at .02 feet at the elevated temperatures we'll be in the potential area of transition flow.

Now what we're looking at is temperature effects. Okay. And the very first thing we have to point out is the process is that we make a bed at 21 degrees C, we run through our velocity sequence. Then we hold at .1 feet per second while we raise the temperature. In this case to 54 degrees C. Run through the same identical velocity sequence without additional filtering. Then we hold at .1 feet per second, raise the temperature again to 82 degrees C.

So we're seeing a flow history through all

of this.

CHAIRMAN WALLIS: It goes up and down, it doesn't go consistently in one direction?

MR. ENDERLIN: Correct. And that's the question of in this slide we have not isolated by making a bed running it at 21 degrees C, making a new bed. So there is a flow history that has been shown both in the bench top to potentially have an effect here. That the evaluation of 54 degrees C may have been due shifting of the bed.

CHAIRMAN WALLIS: Because you would expect it to go down because there's less --

MR. ENDERLIN: Correct. The viscosity has gone down. All of the cases of 82 degrees C we do see lower than 21. But the point is is I haven't looked at it, I don't know if Bill has. If the proportionality here or the difference between them can be fully accounted between the 21 and the 82 degree case just due to viscosity. The thing to keep in mind is that in these results we have a flow history that can be effecting.

MR. TREGONING: Yes. And you've taken thickness readings, too. So if there's something that affected compaction --

MR. ENDERLIN: Yes.

MR. KROTIUK: -- at these different temperatures, that theoretically could be accounted for?

MR. ENDERLIN: Yes. Which speaking of which, we're now to our next slide. What we're going to show is just a quick video showing the different bed heights. What you'll see the video are these 16 points at which data was taken.

On the left are some examples of analyzed photos. Once we take the photo, we don't get the bed height in real time. So they're not in order. They're selected pictures that we took. And so you've got eight cases in which you've got photographs that have been analyzed, but you're going to see 14 pictures in this.

This will go through it rather quickly. I'll talk about this just a little bit. The movie then goes to a slower mode showing the actual velocity. And we can terminate that and move on.

CHAIRMAN WALLIS: Slowly compacting the bed?

MR. ENDERLIN: Correct.

CHAIRMAN WALLIS: Every time you go up and down?

MR. ENDERLIN: Right. And this is all at

the 21 degree C. So it's a question of we have in the bench top we've looked at head loss for some long, like 15, 18 cycle tests but we don't have the optical triangulation down there.

So that's showing the screen. There we go.

Okay. So now what we're seeing is if you look at the .1 feet per second case, and I'll terminate the movie when I'm done talking here, but the rim height goes .72, .71 and ends at .62 on the rim height. You see a lesser effect on the body height. But you're getting on the order of 20 percent change.

If you look at the .02 case, we see the same thing that the bed is continuing to relax as we go down in velocity.

The NUKON, straight NUKON bed, the results are much cleaner. If you add CalSil, the effects of the CalSil can change based on the ratio what we see here.

CHAIRMAN WALLIS: It looks like I can see the screen through the bed, is that right?

MR. ENDERLIN: No, no. Those lines are the lines used to get the optical triangulation. We're projecting that parallel lines.

CHAIRMAN WALLIS: First I see here is the bed swelling and collapsing and swelling and collapsing?

MR. ENDERLIN: Correct.

CHAIRMAN WALLIS: It's hard to see.

MR. TREGONING: It's better with the fast one.

CHAIRMAN WALLIS: Oh, it does go up and down a bit. Yes. It does go up and down. Okay. Okay.

MR. ENDERLIN: So now I'll talk about the NUKON and CalSil beds combined. If we look at the photographs first, these are two different beds and, yes, the pictures do look the same which is what we are trying to show there. So in visual appearance and in initial height, the bed on the left has 75 percent CalSil based on the CalSil to NUKON ratio, I think is how the NRC has been using it. The other one is a one-to-one ratio or a 100 percent CalSil.

What we see, though, and we've got 56 percent of the material that we injected was retained, the other 69 percent was retained on the other bed. We've applied the filtering. We're working to do mass balances by analyzing those filters, but I don't have the dried results presented here of the filters. So

we're attempting to capture and look what our mass balance is or how much that material is still in flow. That becomes a critical issue from the standpoint when I report a head loss or delta p, how much more mass is being added to the bed.

So what we're going to see on the next one is that the one on the right gives us much higher head loss or pressure drop. However, as we went from .1 feet per second and increased to .2 feet per second, we appear to have saw some channeling formed with the higher CalSil. So a NUKON of just that made a very repeatable bed. As we add CalSil what we see is that handling the beds afterwards or the potential for channeling is the head loss goes up but the actual bed integrity goes down. And when you see that, you also start to see a little bit more variability in the head loss. If I was to make the bed on the left, I would get less variability in the answers than the one on the right. If I make a NUKON bed only, then the head losses that I measure are very repeatable.

So here what we're comparing basically the ratio of CalSil to NUKON and we see the temperature effects. The blue hallow squares are the data take in at 21 degrees C. The red triangles and diamonds are the data at the 82 degrees C. We can easily see that

we see that the head loss goes down with temperature and goes up significantly with the slight addition of CalSil from 75 to 100 percent.

Now in showing an example of the CalSil only beds, we did significant testing just to try to find a CalSil loading that would give us a complete bed. Without running through all these cases, I can go back and forth, you'll notice the mass retained is 13 percent with a 1.92 grams. 13 percent with the 2.37. So I'm increasing over a base. I look at the next slides, I'm still at 13 percent. Those first three were made in the small scale or bench top loop and the large scale I'm at 10 percent. So we're never able to make with the tests we did a complete CalSil bed. But one thing that's significant here is that the CalSil is not filtering itself. Okay. You're always getting the same proportionate amount of CalSil. You would think as you built up more CalSil, you'd have the ability to capture. And if you visually look at it in the holes you have, it's hard to believe that all the CalSil with the low velocity is making over to those holes.

CHAIRMAN WALLIS: Well, what do I see on the left? I see a whole -- screen on the outside with nothing on it at all?

MR. ENDERLIN: That's from the bench top loop, it just doesn't have the welded collar.

CHAIRMAN WALLIS: A small -- you've taken the 4 inch and put it on the 6 inch or something?

MR. ENDERLIN: No, no, no. The 4 inch bench top loop configuration has two gaskets that clamp it.

CHAIRMAN WALLIS: Those are gaskets. Okay. Okay.

MR. ENDERLIN: And that loop wasn't intended to go to as a pressure --

CHAIRMAN WALLIS: There's some bypass holes in it or something that --

MR. ENDERLIN: If you look at that -- the actual holes in the center of the bed, the one on the left, both of them the bed is made to the wall of the test section.

CHAIRMAN WALLIS: Yes.

MR. ENDERLIN: One is 4 inch, the other one is 6 inch. But the important thing to take away is no matter how much CalSil I put in there, it's not filtering itself, which was something we hadn't expected.

CHAIRMAN WALLIS: So you kept on putting some more and eventually it would?

MR. ENDERLIN: That's what you would expect, but we're at loading that are much higher than what were initially put in the matrix --

CHAIRMAN WALLIS: First if it's going around the loop --

MR. ENDERLIN: Well, or the question it's passing through or as you build up a pressure drop, you're feeding CalSil out the bottom of the plate as it's being replenished. So this becomes a question of do I have some ability that I'm not holding my CalSil, I just have a hold up problem or am I actually, the same particulate is passing through the larger CalSil. But regardless, the first assumption is that we should have a proportionate amount of the fiber and larger sized particulate that is being held up. And the other material, I mean, there's 87 percent of that material, the 90 percent is still flowing through my loop. In all these cases these are numbers that were gone to .2 feet per second and we hadn't imposed any filtration yet.

MEMBER DENNING: This is a constant velocity test?

MR. ENDERLIN: These did get ramped up to .2 feet per second. The bed was allowed to form for a long period of time at .1 feet per second.

MEMBER DENNING: Yes. Then presumably what happens is that the pressure drop -- I mean, if it tries to cover the whole screen, then the pressure drop is enough through those individual holes that some of them are going to blow through?

MR. ENDERLIN: Yes. But on these we've never -- these holes existed from the beginning. That's the definition of an incomplete debris bed is that we just cannot get that -- at any velocity from bed formation. At .1 feet per second we don't get the entire screen covered.

CHAIRMAN WALLIS: Eighty-one grams in the loop you only caught 8 grams on the screen, is that what I read there?

MR. ENDERLIN: Right. Ten percent. And if we go back --

CHAIRMAN WALLIS: And the rest of it is going around the loop somewhere?

MEMBER DENNING: On around. It's in the core.

MR. ENDERLIN: Right. And the thing is we're looking at 1.92 grams up to, you know -- we've increased this a factor of four and we're still getting roughly the same approximate amount, relative amount.

MEMBER DENNING: But a little bit of NUKON would change it?

MR. ENDERLIN: Yes. Just a little bit of NUKON would change that drastically. And then as we saw, if you made a NUKON bed and let some CalSil from the loading sequence, you'd have head losses much greater than what I'm showing with pre-mix conditions.

CHAIRMAN WALLIS: So it's the pressure drop that's pushing this stuff through the holes, probably? Because if you got too big a pressure drop, it pushes --

MR. ENDERLIN: And again the question comes is it pushing it just through the holes or is this proportionate amount of small CalSil material entering the bed having some hold up and being allowed to exit.

CHAIRMAN WALLIS: Some CalSils have more fibers in them than others, don't they?

MR. ENDERLIN: Correct.

CHAIRMAN WALLIS: Now you think they would make a better bed.

MR. ENDERLIN: Yes. All of our testing is done with one lot of CalSil. And it's the same lot that Argonne's using. The manufacturer reports 4 percent. It could be as high as 8. The fiber is put

in purely for structural strength. If you go back 20/25 years your fiber content can even be something different of what they've used in there. But the fiber is getting added purely through --

CHAIRMAN WALLIS: You are retaining more. As you put more CalSil in, you're retaining more?

MR. ENDERLIN: Right, but the same relative amount. So if we thought of it as just a size distribution --

CHAIRMAN WALLIS: So you put in 500 grams, you'd probably be able to cover the bed, cover the screen, wouldn't you?

MR. ENDERLIN: We would think.

CHAIRMAN WALLIS: It's hard to tell.

MR. ENDERLIN: Four times and still see -- I would have at least expected to see 13 percent, 20 percent, 30 percent, 40. I would have expected to see a larger proportionate amount to be retained with the high CalSil.

MEMBER DENNING: The velocity through the open holes is really high, right?

MR. ENDERLIN: Yes, it is. The question is if you look at a flow distribution across this, is that enough to get all the -- I mean, when we look at a bed on the left, and again part of the reason you

have that is because the elbow isn't as far up the stream as with the large scale. Am I really able to get all the CalSil to come down and traverse over to those holes? You know, these are still pretty low velocity for a 4 and 6 inch pipe. And if you turn this thing off and look at some of the settling rate, again I don't know at 13 percent we've got some particle size data to go look at, and that'll be in the report is if we just assume the top 13 percent is captured. But, again, it's a question is is just captured or do you have a hold up problem going? So you have stuff constantly being deposited and it's being forced through the hole.

CHAIRMAN WALLIS: Well, it's settled down onto there, then you'd have trouble blowing it through the holes. As long as it's flowing through the hole, that's one thing. But if you stop the loop and start it up again --

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: -- it might be different.

MEMBER DENNING: You might have a problem, yes.

MR. ENDERLIN: Yes.

MEMBER SIEBER: Do you know anything about

the velocity profiles on any of the screens?

MR. ENDERLIN: In the vicinity of the screen if we go back to look at the NUKON bed, that rim on the NUKON bed will pretty much give you what the velocity is. The pressure drop has to be the same through there and you're not getting as much compression at the rim. In the holes and stuff, no. I mean, we'd have to do some LDV or something to get that with tracer particles.

Once you have the incomplete bed in the holes, I don't know what it is right there. But I've got over 20 L over Ds of straight pipe upstream.

MEMBER SIEBER: It would be shifting as time goes on through this whole thing?

MR. ENDERLIN: Yes.

MR. TREGONING: This is Rob Tregoning, Office of Research.

The other point I'd like to make we don't worry so much about NUKON with what the underlying screen looks like, whether it's perf plate or wire mesh. And largely we don't have to worry about that because NUKON is so efficient at forming a bed and capturing regardless of the screen parameters.

With CalSil it's a little bit different consideration. And if for situations that moved to

potentially smaller hole size that require less bridging distances, potentially CalSil would be with shorter fibers, would potentially have more of a chance to clog or form a complete bed by itself. So the exact per plate you tested is certainly more of an issue when you look at CalSil only loading than it is with NUKON.

That was just a point that I wanted to make sure was made.

MEMBER SIEBER: But as the screen starts to load up, the velocity and the places where it's thin or there's some open spots, would increase rapidly which might keep those areas pretty clear. So the overall size of the screen and the total flow would have bearing on how much clogging you got. With just different geometry you may get --

MR. ENDERLIN: Well, there will be head loss data in the report and there have been quick looks that have been written on these for Bill Krotiuk. But at this time I'm not presenting head loss data due to the incomplete bed. The most significant result was we keep CalSil and we can't make a complete bed. And, again, just to remind, our purpose was to obtain data for developing the correlation.

So I'm showing some brief results from the -- we've done four coating tests. On the left hand side you have the AA material --

MR. KROTIUK: That's an altered coating.

MR. ENDERLIN: And on the left is the zinc epoxy. I believe that's a two ply coating.

CHAIRMAN WALLIS: Well there's a mixture of two things, is that what it is?

MR. ENDERLIN: Correct. What we had was through basically screen sieving we refer to as the quarter inch particulate, which I believe is through roughly an 8th inch to half inch screen. That material is referred to as chips. And the other material is processed, the size characteristic on that have not been completed at this time. They will be done by the Naval Weapons Research Center using the same method they did for the transport tests.

But on these this is basically a 50/50 by mass. The AA material or the bed on the left formed a fairly complete bed but due to the structure of the chips, you know if you look in it, you can see -- instead of necessarily seeing straight through channeling when it's formed, you can see that there are some open paths basically in and around chips.

The AA material was much more likely,

especially at elevated temperature, to adhere to each other and you could see some change in its structure with temperature.

The ZE material we didn't see any change or cohesiveness change with temperature. Again, we had limited -- less work on that. But the ZE material, I'll also point out, is denser. So the question is if I went to the same volume of AA material, would I have got different results. But for the mass loadings on the matrix, we came nowhere near to forming a complete bed.

MEMBER SIEBER: Did you examine the physical characteristics of the chips, like are they flat?

MR. ENDERLIN: No.

MEMBER SIEBER: How they would stack up or are they curved?

MR. ENDERLIN: The ZE beds are flat. The AA, those chips tend to have some curl. Much more has been done by Anne Fullerton and --

MEMBER SIEBER: Like a potato chip?

MR. ENDERLIN: And we're using the same material as them, so we didn't want to duplicate what we got from her.

MEMBER DENNING: Did you do mixed with

fibers or are you going to do mixed with fibers?

MR. ENDERLIN: We have not any with fibers and currently our testing is completed.

MEMBER DENNING: Because it seems to me that that's really the issue is the chips mixed in with the fibers and the impact on the delta p across that more realistic debris bed I think. I mean, the fact that -- because I wouldn't anticipate a chip only situation. And now the question is how do you put these together.

MR. KROTIUK: The only thing I can tell you is that we have not addressed that at this point in time.

MEMBER DENNING: Well, I think that in--

MR. ENDERLIN: It was something of interest, it's just in prioritizing the test matrix, and as Professor Wallis pointed out, at the end of funding that one hadn't been done yet.

MEMBER DENNING: Yes. It's interesting if you look at the WCAP that talks about production of how they're going to produce the chemical debris, they talk about how to take different components of the debris and add the resistances together in ways that I think are clearly erroneous. And so I was just kind of curious as to how we think we would do it. But --

or how the NRC thinks it will do that. It has come up with, you know, when you have mixtures of things, how you then come up with a resistance.

MR. KROTIUK: I'll address that in the second part of this.

MR. TREGONING: Rob Tregoning.

I just wanted to clarify a little bit the philosophy behind the coatings test. I think you're going to hear subsequent to this Carderock work and, again, the Carderock work built on some very early LANL work where we looked at measuring the transportability of coating chips vis-à-vis coating particulate. I think what you're going to see is that most of these chips, except for the alkyd systems, have very limited transportability within the flow rate regime that we'd expect globally within a containment pool. Now that doesn't mean that there locally might be some higher velocity locations that might transport this. So that's why we really were focused on the alkyd system primarily.

Zinc, we did look at a zinc chip but I think most of us are more concerned about zinc based particulates. And we would make the argument that if you can predict one particulate, you know the hydraulic parameters, you can essentially use that to

predict another similar particulate assuming you know the size distribution and the amount of loading and things like that.

So we wanted to look at chips here and we wanted to look at alkyd chips primarily, because they there were concerns that they might be pliable enough that even these large chips would bend and create a contiguous bed that would result in relatively rapid head loss, even in the absence of a NUKON bed underneath it. So that's really what these tests were intended to study to see if coatings by itself, coating chips by itself could lead to situations where head loss was maybe unsustainable. And there are a few plants that we are concerned about the amount of coating debris they might potentially have in their sumps. So that's why we looked at those cases first, you know, absence of any NUKON additional filtration bed underneath that coating debris.

MEMBER DENNING: But once again, there are more tests that clearly could be done even at this point, and yet the test program stopped.

MR. TREGONING: Again, at this point until we know how big of an issue for specific plants coating chips are, it's not clear what a suitable matrix would be. So you're right. There are certainly

more tests that could be done, but at this point we've chose to try to assess what plant conditions are most representative. And then if additional testing is needed to assess those conditions, we'll revisit that at that time.

Do you want to weigh in?

MR. LU: Okay. Shanlai Lu, NRR.

And the reasons we asked for this two tests that we want, as Rob just mentioned, we were trying to figure out whether this coating alone can form a bed which can cause a significant head loss. And another reason is we want to study the mixing of the mixture of the coating chip with other fiber and the particulates. So that's not the purpose of this test.

MR. ENDERLIN: The purposes of these initial tests was to first answer that question.

MEMBER DENNING: Yes. But I don't see the question will coatings alone block it as being a particularly important question relative to will coatings in combination with fibers totally block -- significantly block it. Because I think in these cases where there is fiber there, it's going to be there. And almost every plant, even in my plants, have a fair amount of fiber.

MR. LU: I think you're right. And from the testing of the relationship we actually did observe that we have mixed coating chips and the fiber in the particulate. So the reason we want to separate that from that mixture is we want to study whether the coating chip alone can constitute in a former bed which can surprise us. And I think the results is not.

CHAIRMAN WALLIS: Well, the problem with leaves in plants where they, say, take sugar out of sugar beets and so on, is that you get enough of them stacked up, then they compress each other. And that's how the thing clogs up. If these are flexible of leaf like coatings, you've got a very thin layer here. But if you had several stacks, then they sort of lay over each other, you know, and they sort of block -- you can imagine how they do it, especially if they can compress it a little bit.

MR. LU: Well, the object --

CHAIRMAN WALLIS: I'm not saying that you're really testing in this experiment. Now these are pretty hard coatings, they're not coatings that are like leaves. They're actually --

MR. KROTIUK: The alkyd is pretty flexible.

MR. ENDERLIN: Yes, the alkyd by itself chips on the order of over a quarter inch, were able to roll and pass through the 8th inch holes.

CHAIRMAN WALLIS: Well, if you have a thicker layer of these things, it might well be that they --

MR. ENDERLIN: Well, we're about to show the head loss data, and the bed on the left did create some head loss. I'd say there's just some channeling observed. It wasn't significant. What I'm trying to point out is with the debris you can look from the top, just a top view.

CHAIRMAN WALLIS: Well, I'm thinking of a layer, which would be, say, an inch thick. That doesn't look like an inch thick there. A bed which was quite thick with this stuff layered across it. And it's not randomly oriented. It's oriented so it's lying flat, isn't it?

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: And that would tend to block the holes.

MR. TREGONING: Can you comment on how many layers approximately of chips make up sort of the center of the bed on the left? Have you done any sectioning at this point or is that still pending?

MR. ENDERLIN: I've got notes, observations. I wouldn't want to talk off my head. I mean, you know, there are places there are two to three chips on top of each other. The thing to understand is if you look at the bed on the right, if you looked across it it's very flat.

CHAIRMAN WALLIS: Fuller than that.

MR. ENDERLIN: If you look at the bed on the left there's a very 3D, it's a very rough surface. Okay. Those chips are not oriented at all laying flat. They were chips able to past through. If you picked that bed up, any of the AA beds, you see material. The NUKON was very clean on the bottom. This has material hanging through the holes. And, again, we're looking at an angle and you can see what appear to be a few holes, but it's because I've taken it at an angle. If you look dead on, there's one or two and we're not sure it wasn't in bed retrieval.

The point I was trying to get at is if you look at NUKON or a NUKON/CalSil looking down, you can see there's no channeling. If you see that with one of these beds made of coatings, then look from the side, you can see there are clear paths even though from the top. The bridging effect creates a lot of difference of where there's holes big enough --

CHAIRMAN WALLIS: For a vertical screening it might be quite different --

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: -- in terms of the orientation of these things. And if it were one of these commercial screens which has all kinds of strange shapes, you'd have to test that by itself?

MR. TREGONING: That's a good point to keep in mind. When you see some of the coating data subsequently with respect to alkyd transport, you'll see that there's some very strong distributions in terms of -- as a function of thickness where transportability is more or less likely. And I think that's a very relevant point to consider.

CHAIRMAN WALLIS: Well presumably with this coating being sheets of stuff, you wouldn't correlate it with a equation for spherical particles. You wouldn't correlate it with the usual type of sand type filter equation?

MR. ENDERLIN: Right. I mean using the leaf example from the food industry and stuff, you can get packing fractions that are much higher.

CHAIRMAN WALLIS: Quite different, right.

MR. ENDERLIN: Yes.

So here again, the blue represents those

done in ambient 21 degree C. The ZE coating because it was so incomplete, we didn't take the time to go to the higher temperature. Again, you can see that we see the temperature effect of a reduced head loss and we still see some head loss from that AA bed.

CHAIRMAN WALLIS: Well, this seems to be like the other tests, it's sort of try a few things and see what happens. It's not a comprehensive program to establish a method of predicting anything.

MR. ENDERLIN: This portion of it.

CHAIRMAN WALLIS: Right.

MR. KROTIUK: Dr. Wallis, I think when you're talking about coatings that could be a valid comment. However, if you notice in one of the figure that Carl had shown, is that the number of tests that we had run with the CalSil and the NUKON are quite substantial. So I think we have much more information on that.

MR. ENDERLIN: This is just a brief overview of some of the summary of the findings that we have.

One thing we've seen clearly is that the loading sequence or how the material arrives at the screen and the degree of preparation basically can dominate. Given the same debris loading we've shown

substantial differences. However, we have shown that we can control these and get repeatable results and look at these different parameters or different variations independently.

The NUKON only debris beds, they yield very repeatable beds, repeatable head losses. They're influenced by time of flow. What we've seen is that 217 grams meter squared we made beds less than that, but we didn't look for the absolute minimum. But at that loading we consistently get complete debris beds.

We were unable in our testing to get a CalSil to form a complete bed, although we do have some significant head losses from those beds.

CHAIRMAN WALLIS: Well, I think Los Alamos did in one case get a CalSil only bed.

MR. KROTIUK: That's correct. Yes. I remember reading that.

CHAIRMAN WALLIS: So it is possible. And maybe with smaller sized holes, which are now going into screen, it would be more likely.

MR. ENDERLIN: Well, it could be smaller sized holes, another function is just a dilution of the material. We've seen, you know, if you take a test loop that has higher dilution from the injection point, we've seen that can have a bearing on making

it.

DR. LETELLIER: Bruce Letellier of LANL.

I'd have to say that that statement about a complete bed was somewhat qualitative in nature. What we observed was very similar to PNNL. What surprised us was the fact that the very small particulates could actually bridge the gap. And that was the first time it had been observed to induce substantial head loss. But it wasn't uniform in the same sense that the NUKON beds have been.

MR. ENDERLIN: Through the optical triangulation we've shown that the bed continue to contract and relax even with significant number of cycles. So we're not getting to a completely, you know, precompressed bed that's going to stay at one height. We've been able to take elevation measurements for the purpose of the correlation.

For most cases the head loss does increase with increasing temperature. Again, in our test to date that hasn't been isolated as a single parameter tested so that we can check what the relative difference is in temperature. There's always a flow history effect there.

CHAIRMAN WALLIS: And all that we've heard today no one has asked the question what would it take

to do enough work to have a predictive tool. You found out a few qualitative things here, which are very interesting, but you don't have a way of predicting the pressure drop for coatings on a screen.

MR. ENDERLIN: Our scope was to obtain data for others to work with making a predictive tool.

CHAIRMAN WALLIS: You sort of tried a lot of things and the results are interesting. But NRR has perhaps learned a few questions to ask of industry. But you don't have a predictive tool. So any predictive tool that's going to be used has to come from these industrial tests?

MR. KROTIUK: Now are you talking about for the coatings?

CHAIRMAN WALLIS: The coatings, yes. We haven't talked about your theory yet.

MR. KROTIUK: No.

CHAIRMAN WALLIS: But for the CalSil, are you going to give us a predictive tool for the CalSil and NUKON?

MR. KROTIUK: For the CalSil and the NUKON, yes.

CHAIRMAN WALLIS: You're going to give us a predictive tool for that?

MR. KROTIUK: For the CalSil and the NUKON

I have --

CHAIRMAN WALLIS: Are you going to present that today or is that going to be in the future sometime?

MR. KROTIUK: Yes. I'm going to present that today.

CHAIRMAN WALLIS: So you're going back to your script?

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Okay.

MR. ENDERLIN: And another one, the last finding that I have listed here, is that the 5 mesh screen and the perforated plate for the purpose of the NUKON, NUKON/CalSil didn't really have any bearing. We saw the same head losses for the same debris loadings.

MR. KROTIUK: And just go to the end, I guess.

Now if we go back to this one we can continue.

MR. ENDERLIN: Eight in the previous presentation will be the next slide.

MEMBER SIEBER: That up there says 7.

MR. ENDERLIN: Right. And now it's 8.

MR. KROTIUK: Okay. So now I'll talk about the head loss modeling itself. And this is

basically an effort to use the data that is available to come up with the analytical tool. I based the head loss on the classical form of the Ergun Equation. And it does take into account the compressibility for -- accounting for the irreversible and elastic behaviors.

Now this is important. The method that I've developed uses two approaches to modeling the debris bed. One uses a single homogeneous calculational control volume and the other uses a -- breaks the debris bed into two control volumes. So therefore, it's heterogeneous. But within each of those control volumes you have a homogeneous -- it's considered homogeneous.

What I'll do is I'll try to describe the method and then show you some comparisons with data.

This is -- I wanted to -- had a thought process here in developing the models and the reason why I have a one volume and a two volume model is that I tried to look at the possible configurations of debris within a bed. And what I could come up is that there are four basic configurations.

One, you could have a bed that is completely -- in this case these are all fibers and particles. Let's talk about that first. If we have a bed that consists of fibers and particles but are

unsaturated, and what I mean by unsaturated is that if you have a mass of fibers that mass of fibers in a debris bed could only accommodate certain mass of particles. And I'm defining that as a saturated bed.

MR. ENDERLIN: Interspatially.

CHAIRMAN WALLIS: That's not true, though.

MR. ENDERLIN: Interspatially you mean.

CHAIRMAN WALLIS: It depends on how compressed the bed is. If you have a very fluffy bed, you can put more particles more in. If you compress the fiberglass, you got less space to put the particles in, presumably.

MR. KROTIUK: Right. But --

CHAIRMAN WALLIS: so the particles go into the spaces in the fiberglass?

MR. KROTIUK: Yes. So this is within the spaces.

CHAIRMAN WALLIS: Another question, which is if you squashed the fiberglass enough, presumably you can't squash it any more, or is that -- I don't know. Does it yield in some way or --

MR. KROTIUK: The void fraction is pretty high, so above 90 --

CHAIRMAN WALLIS: It keeps compressing forever as you squash it?

MR. KROTIUK: I don't think so.

MR. ENDERLIN: No.

MR. KROTIUK: No.

MR. ENDERLIN: -- can't the drag to the bed to compress it. I mean, if we look at the pressures --

CHAIRMAN WALLIS: You don't have enough force to do it?

MR. ENDERLIN: Right. If we look at the bed heights based on -- you know, we've got data for two atmospheres applied to it.

CHAIRMAN WALLIS: But it seems to me that the saturated concept it okay, but it must depend upon -- and if you have a very fluffy bed of fiberglass, you could put more particles in there and then you compress it --

MR. KROTIUK: Right, and it's --

CHAIRMAN WALLIS: But you can't because the particles are taking the stress rather than the fiberglass.

MR. KROTIUK: Yes. So the saturation conditions for the particles in the fiber bed is a function of the density of the fiber bed. So it's actually a density --

CHAIRMAN WALLIS: So it's not just one

mass to mass ratio?

MR. KROTIUK: Right.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: It's a density of particles to density of fibers.

CHAIRMAN WALLIS: Thank you. Okay. Okay.

MR. KROTIUK: Okay.

MEMBER SIEBER: At a given compression?

MR. KROTIUK: At a given compression.

So let's just look at this first column here. So I'm calling this a homogeneous unsaturated bed. And I'm saying that for this type of condition you would use the one volume approach. And it will give us a best estimate number. And especially this approach was also used for, like, if you have a case with just all fiber bed, you know, you'd use this case also.

What I found is that this will give you have a mixture -- okay -- let me just go to this column here first and then I'll come back to here.

For this case I'm calling it a saturated condition. This is a case where it's -- you have a thickness, a debris bed that has particles -- I'm sorry. That has fibers in it and it has for its conditions the maximum amount of particles that can

be accommodated with this bed.

CHAIRMAN WALLIS: So a void fraction in these two cases is very different?

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: If you look at NUKON by itself, the void fraction is 97 percent or something. When it's saturated with particles, the void fraction is presumably 40 percent or 50 percent.

MR. KROTIUK: It is lower, yes.

CHAIRMAN WALLIS: Very, very different. That's what makes the pressure drop so big.

MR. KROTIUK: That's correct.

So all I'm just saying is that this second case here is really the limit, you know, as you add more and more particles to the fiber bed, you reach a saturated condition. And for both of these cases you basically would -- if you had either of these two cases, you would use the one volume approach.

Now, if you had now a case whereby you had, again, a mixture of fibers and particles, as time would go along --

CHAIRMAN WALLIS: Excuse me. If you had a gel, you could presumably fill up everything solid.

MR. KROTIUK: That's a possibility. Okay.

Okay. For this case is really somewhat of

a -- this third case is really a continuation of this first case. For instance, say you start building the bed and you have a homogeneous mixture of fiber and particles. And it's evenly distributed, just --

CHAIRMAN WALLIS: So all the particles, and you squash all the particles in the --

MR. KROTIUK: So as time goes along, the particles squash and you would have basically a saturated layer here with fibers on top. Now this is a type of situation where I'm saying we would use the two volume approach.

CHAIRMAN WALLIS: That's a conservative or thin bed type of analysis?

MR. KROTIUK: That's right.

CHAIRMAN WALLIS: That's with all the particles in one layer?

MR. KROTIUK: This is a thin bed type of analysis.

CHAIRMAN WALLIS: Squash them as much as you can.

MR. KROTIUK: And the methodology that developed gives you the upper bound of what this calculation to do.

CHAIRMAN WALLIS: Isn't it worse to put the saturated particles in fiber on top of the fibers

so they squash the fibers, too. It depends which is on top, how much of squash?

MEMBER DENNING: In your model does it make any difference? I understand that you're thinking about how it's formed, but does it make any difference in your model as to which comes first, the fiber on top or --

MR. KROTIUK: Actuality the way the model is built, the order of this -- you know, whether I have the fibers on the top or the bottom doesn't really matter.

CHAIRMAN WALLIS: It doesn't make it how much is compressed because if the pressure drops in the black layer, if it's on top, but then it squashes the fibers below it. If it's below, it doesn't squash the fibers.

MR. KROTIUK: Yes, but then if you have the black layer with the particles on top, the particles will tend to try to migrate to the --

CHAIRMAN WALLIS: But your models doesn't let them do that. It legislates that they're stuck there.

MR. KROTIUK: Yes. It legislates. Because my model is a conservative model to try to give you an upper bound calculation.

And then the fourth condition that I posture could exist would be a case where you completely saturate the fibers with particles and then you have a layer of particles on top. And, again, this is handled by the two volume approach.

CHAIRMAN WALLIS: They're close packed or --

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: -- mass the particles on top?

MR. KROTIUK: Right.

Now in the testing that I've looked at, and I haven't looked at all the --

CHAIRMAN WALLIS: I suppose there's a worse case, another case which is you have the fiber clean and then all the particles. So you have a yellow and a gray. And you put all the particles in one layer and the fibers in another.

MEMBER DENNING: The particles can't penetrate the fibers.

MR. KROTIUK: The particles --

CHAIRMAN WALLIS: You put the fibers on the bottom and the particles on top. You can do that, too.

MEMBER SIEBER: That would give --

MR. KROTIUK: That's the third column essentially.

CHAIRMAN WALLIS: No.

MR. KROTIUK: No. You're saying to have a case where you have a separation with fibers on the bottom and the particles on top.

CHAIRMAN WALLIS: Right.

MR. KROTIUK: I mean, that could be handled by the two volume approach.

CHAIRMAN WALLIS: That could be. It's probably the worst case of all, isn't it?

MR. KROTIUK: But I didn't -

MEMBER DENNING: What is the answer? Is it worse or not worse?

MR. KROTIUK: I'm sorry.

MEMBER DENNING: If you take all the particles out, put them on top of the fiber --

MR. KROTIUK: Right.

MEMBER DENNING: Is that --

MR. KROTIUK: This is the worst case.

MEMBER DENNING: -- or is it worst to have particles interspatially within the fibers --

MR. ENDERLIN: Well, the fourth one would be --

MR. KROTIUK: The fourth case is the worst

case.

CHAIRMAN WALLIS: That's the worst?

MR. ENDERLIN: The packing factor. If you think in terms of spheres. If you have a mono disperse, I mean a poly disperse particles, then you could pack those tighter. But if you take fiber with particles I can get the lower void fraction --

CHAIRMAN WALLIS: Actually it compacts more than particles alone?

MR. ENDERLIN: Right.

CHAIRMAN WALLIS: Ah. Okay.

MR. KROTIUK: So in the testing, as I said, I've tried to identify these at least four regimes in the testing that has been done. And I've identified a fair number of cases with this situation. I've identified one case that's probably this, and then one that's saturated.

CHAIRMAN WALLIS: You could actually make this happen. You could actually build up one and then put the other on top of it --

MR. ENDERLIN: That's the load sequence data that we've showed you.

MR. KROTIUK: That's the load sequencing, right.

CHAIRMAN WALLIS: But then you could now

do it systematically and the load sequence before was just sort of seeing what happens with these things. Now you could deliberately make these --

MR. ENDERLIN: The load sequence we did was to deliberately make these.

CHAIRMAN WALLIS: A few points. I'm saying if you want to do a comprehensive --

MR. ENDERLIN: Oh, right.

CHAIRMAN WALLIS: -- testing of a correlation, you'd probably want to take, say, the fourth regime and make a 100 data points out of it, not three.

MR. ENDERLIN: Yes.

MR. KROTIUK: This is just a review for what I previously presented. This is basically the equations, the general form of the equation that I'm solving the Ergun Equation. Basically it has a viscous component and a kinetic component.

The viscous component has these multipliers in them which are dimensional permeability functions which came out of literature. They're based on the Happel approach. And the multiplier is a function of whether you have fibers or particles.

This kinetic approach is, again, taken out of literature. This multiplier here, which is an

empirical B and a C --

CHAIRMAN WALLIS: Now we're going to have such low velocities through these screens that it's going to be viscous.

MR. KROTIUK: Right. And that's what I put down here on the bottom is that currently looking at the data that I've had is that this term, the kinetic term, really accounts to less than 4 percent of the total pressure drop.

MEMBER KRESS: You need both eta and X or chi, or whatever that is?

MR. KROTIUK: I'm sorry, say again.

MEMBER KRESS: Eta and the void ratio and porosity, aren't they one-to-one correlated? Do you need both of them?

MR. ENDERLIN: Are they independent?

MR. KROTIUK: No, they're not independent. If you know one, you know, they're related.

CHAIRMAN WALLIS: Those are two different things, aren't they?

MR. KROTIUK: One is a void ratio, one is porosity.

CHAIRMAN WALLIS: But one is the void fraction, the fraction of the space occupied by liquid.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: And one is a fraction of the space occupied by particles versus the fibers or something, or is it not?

MR. KROTIUK: The void ratio is the volume of the void over the volumes -- of the occupied volumes.

CHAIRMAN WALLIS: Not saying what kind of solid it is?

MR. KROTIUK: Not what kind of solid it is.

CHAIRMAN WALLIS: So X and epsilon R --

MEMBER KRESS: One-to-one.

CHAIRMAN WALLIS: It's the same thing?

MEMBER KRESS: No, they're not the same thing.

MR. ENDERLIN: They get defined in terms of each other.

MR. KROTIUK: You could define X in terms of epsilon and vice versa.

CHAIRMAN WALLIS: Okay. You can. All right.

MR. KROTIUK: Yes. I'm just using both of them.

And to review the compression model that

I have, is that as probably saying is, you know, during testing we have velocity increases followed by decreases. The assumption that I made was that for the first compression is a nonrecoverable, irreversible process and all of the further on compression are elastic with constant compressible.

CHAIRMAN WALLIS: Which means that's when there's no pressure, the bed is infinitely thick?

MR. ENDERLIN: But there's no water in it either, so --

CHAIRMAN WALLIS: One over zero is infinity, so that's the trouble with this P to the end correlation. It obviously blows up when you get no pressure drop.

MR. KROTIUK: That's right. But we don't have that situation.

MR. TREGONING: Fortunately that's not the case.

CHAIRMAN WALLIS: Well, it does. I mean if you had it upside down, the stuff coming up from the bottom. You know the bottom -- when it's falling down from the top, it's own weight squashes it on them. But if it's coming up -- if you had a filter on top and you're flowing upwards, then you can eventually get very, very disperse --

MR. KROTIUK: You lowered the velocity, yes.

CHAIRMAN WALLIS: -fiber bed down there, which it's barely held up -- it's a fluidized bed.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: So it could have a huge X.

MR. KROTIUK: X.

CHAIRMAN WALLIS: There's no pressure essentially until you get enough pressure to begin compressing it. So you can have a hugh bed.

You do all these experiments with the stuff raining down and resting, then gravity makes the bed.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: If you do it with upflow, then you have a fluidized bed below which can have a huge void fraction.

MR. KROTIUK: That's correct.

CHAIRMAN WALLIS: That's where your P of zero comes in, even if particles aren't even touching each other.

MR. KROTIUK: But then I don't think you would be using this approach --

CHAIRMAN WALLIS: Especially with

horizontal. I mean, the gravity isn't helping you compress the bed, then you can have a very fluffy bed.

MR. KROTIUK: I mean, for a fluidized bed you wouldn't be using this type of an approach.

CHAIRMAN WALLIS: Oh, yes, you can use Ergun to some extent.

MR. KROTIUK: You can use Ergun, but this compression --

CHAIRMAN WALLIS: Okay. Go on. Yes.

MR. KROTIUK: The key thing is that I wanted to point out here is that both of these equations have this parameter N which is a material of --

CHAIRMAN WALLIS: It's a quarter.

MR. KROTIUK: -- property from the -- excuse me?

CHAIRMAN WALLIS: It's a quarter. It's been .2 and .25, isn't it from the experiments?

MR. KROTIUK: Yes. I calculate it about .23.

CHAIRMAN WALLIS: Okay. Well, you did a pretty good job.

MEMBER MAYNARD: A quarter is not worth what it used to be.

DR. LETELLIER: Canadian quarter.

MR. KROTIUK: Okay. Now in order to do the calculation there are certain parameters I need. One is the one that I just spoke about was N, which the material parameter. The other thing is that I have to have an initial starting bed thickness for the calculation. I have to know the debris material properties.

CHAIRMAN WALLIS: There's no pressure drop or what?

MR. KROTIUK: I'll define that in a minute.

CHAIRMAN WALLIS: Well, it will be at some pressure drop.

MR. KROTIUK: Yes. It will be at a given velocity. I say at .1 foot per second.

And then I have to have my material properties. This could be densities of the solids and all, and specific surface area. Now this for the debris, this would be calculated from the test data.

I have to know my material masses on the bed itself. In other words, the debris masses that are in the debris bed for CalSil/NUKON.

CHAIRMAN WALLIS: Is a bed which is formed by raining down on to gravity the same as a bed which is formed by particles arriving in a flow?

MR. KROTIUK: If you have a --

CHAIRMAN WALLIS: The structure the same? I mean because some of these beds are formed by putting in a screen and letting stuff rain down on its own weight, right?

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Just like a snowfall?

MR. KROTIUK: Sure.

CHAIRMAN WALLIS: And it's not clear to me that those particles raining down with a particular orientation they take are going to be the same as taking, say, a vertical one and letting flow drive the particles in. They may come in this way instead of that way.

MR. ENDERLIN: I mean it depends on the velocity you're using.

CHAIRMAN WALLIS: I mean, you make quite a different structure depending on how it's made.

MEMBER KRESS: Well, do you want a particle fraud number to --

CHAIRMAN WALLIS: No. This --

MR. ENDERLIN: Well whether it's stratified flow and whether you're exceeding the settling velocity. You're letting it rain.

CHAIRMAN WALLIS: No, it's the orientation

of the particles.

MR. KROTIUK: Yes. But depending upon the velocity, the way you can --

CHAIRMAN WALLIS: If a particle like this under gravity it tends to fall like this.

MR. ENDERLIN: Right.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: If I have a particle in a flow, it tends to come this way. The flow orients -- does it orient it this way.

MR. KROTIUK: Correct.

MR. ENDERLIN: But if you do the horizontal flow with a high enough velocity --

CHAIRMAN WALLIS: I'm just saying that the bed may form differently depending on whether it's formed by gravitational settling or by the flow bringing the particles in.

MR. ENDERLIN: Correct. But I mean it's a function of basically the velocity and the flow regime.

CHAIRMAN WALLIS: The flow tends to take the particles to the places where there are holes, too.

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Whereas gravity doesn't

do that.

MR. ENDERLIN: Yes. If they're stratified, it'll be completely different also.

CHAIRMAN WALLIS: There's something different about making a bed from a flow than there is making it from just laying --

MR. ENDERLIN: I don't think it's vertical and horizontal as much as the flow conditions at which you --

CHAIRMAN WALLIS: Well, I --

MR. KROTIUK: Yes, I think the flow conditions --

MR. ENDERLIN: There are some flow conditions in a horizontal -- in a vertical screen horizontal pipe you can't get to from a vertical flow pipe.

MR. KROTIUK: Yes, there are variable --

CHAIRMAN WALLIS: IF they're not round particles, then all beds aren't the same.

MR. ENDERLIN: Correct.

MR. KROTIUK: The other parameter that I wanted to know is what I defined previously was the saturation condition for a particle concentration in a fiber bed.

And the other comment I'm going to make is

that because the kinetic term was so small relative to the head loss is that I haven't concentrated too much on determining the factors for B and C because of the relative small magnitude of their components of the pressure loss.

MEMBER KRESS: And that means you've ignored them? IS that what that means?

MR. KROTIUK: I didn't ignore them, but I just used the values that were published in literature.

Okay. First, using the Series 1 test data, I have not looked at the new test data that Carl had alluded to, is that again, N is .23 which is consistent with what was previously said.

CHAIRMAN WALLIS: What I got for fiberglass?

MR. KROTIUK: You know, I don't remember. It's around there, though. It's around there. Maybe it 225, I don't know.

CHAIRMAN WALLIS: Well, okay. Close enough.

MR. KROTIUK: Now I have to have that starting point, as I said. So I defined a starting point as a bed thickness of essentially .1 foot per second. And using the data that I had again, for just

the Series 1 tests, I have to factor in the Series 2 tests and I'll modify this. And then using just a basic definition for void ration, which is essentially the same thing -- you know related to porosity, you could come up with a relationship for tat initial bed thickness of this fashion, which uses the mass of say in this case NUKON and CalSil and the density of CalSil and NUKON and the flow area of the screen itself. And then there's two empirical factors which I call X_{NUKON} and X_{CalSil}. And if you look at the data, you could come up with the conclusion that X_{NUKON} and X_{CalSil} are these numbers, 12.% and 19.1.

And, again, this is just based on the data.

CHAIRMAN WALLIS: And it's best to use a reference length which depends on the mass of the stuff?

MR. KROTIUK: Right.

CHAIRMAN WALLIS: Right. That's the way to do it.

MR. KROTIUK: So that's what that is.

CHAIRMAN WALLIS: Not to try to do it by measuring something or as manufactured or anything like that.

MR. KROTIUK: Right. The other thing is

is now -- was the material property itself.

Looking again, this is just my current assessments using the Series 1 test data, is that for the NUKON fibers I came up with a value for S_v of 176,000, Comparing it to the NUREG-6874 the previous value 171,000. So for NUKON fibers that's pretty close.

CHAIRMAN WALLIS: That's pretty okay, but the CalSil varied all over the place?

MR. KROTIUK: Right. And that's the second column here. The CalSil, the recommendation was something of 600,000 feet to the minus one if you had a mixed debris bed and for a thin bed it was recommending --

CHAIRMAN WALLIS: That was just taking the worst case they got as a conservative value?

MR. KROTIUK: Right.

Now, again using the data I calculated what the value should be for the CalSil particles, and I calculated a number around 179,000.

CHAIRMAN WALLIS: Maybe this 880,000 was because they had nonhomogeneous bed?

MR. KROTIUK: That would be my suspicion.

CHAIRMAN WALLIS: And they treated it as if it were homogeneous.

MR. KROTIUK: That would be my suspicion, yes.

And then I'm basically for any fiberglass fibers in the CalSil, I'm using the same numbers for the NUKON fibers, which was consistent for what was previously done.

The values for the material densities are basically the same as was previously determined.

CHAIRMAN WALLIS: What do you do for aluminum oxide hydroxide?

MR. KROTIUK: I haven't looked at that yet.

CHAIRMAN WALLIS: Maybe you can't treat it this way.

MR. KROTIUK: You might not be able to, yes.

DR. LETELLIER: A question. Bruce Letellier from LANL.

Bill, how do you rationalize the recommendation that the NUKON fibers have the same specific surface area as the CalSil given that they're just radically different physical forms. I mean, when I think about the flow path, the hydraulic surface area basically, I have a hard time imagining that they have the same effective metric. Do you have any

thoughts on that?

MR. KROTIUK: I noticed that myself, Bruce. And at this point I don't, and I was hoping to address that after I looked at the rest of the data that was available.

MR. ENDERLIN: The thing to remember is that we're not as manufactured. He's gotten these off the experimental data. So our degree of preparation has a bigger influence.

DR. LETELLIER: I'm just thinking about the SEM photos that we see of raw CalSil is such a complex surface. I mean, perhaps it's a very valid conclusion of the data that the effective drag surface that really participates in the flow just coincidentally looks very much like fiber. But physically they look drastically different. So I've always wondered myself just how much of that visual porosity effects the drag coefficient.

MR. KROTIUK: You know one other thing I was thinking of doing also is, you know, one of the reasons I had Carl run that case with the all CalSil bed is because I wanted the data with an all CalSil bed to really give me a good handle on what this number was. But with the bypass hose that developed, again, I haven't looked at that data in detail, but I

don't know whether I'll be able to --

CHAIRMAN WALLIS: This is the number that correlates your data, is that what it is?

MR. KROTIUK: That's the number that correlates the data, right?

MR. TREGONING: It correlates to the Series 1 data.

MR. KROTIUK: The Series 1 data, right.

MEMBER KRESS: As best I remember that specific surface area wasn't really a ratio of the volume to the -- area to the volume.

MR. KROTIUK: It's really not. It's really more of a --

MEMBER KRESS: You've backed it out of the data --

MR. KROTIUK: That's right.

MEMBER KRESS: And it wasn't really that ratio. It was somewhat related --

CHAIRMAN WALLIS: Whatever the coefficient is in the --

MEMBER KRESS: Whatever the coefficient was, yes.

DR. LETELLIER: But on the other hand it has to be related to the flow area divided by the wetted parameter or volumes to area ratio.

MEMBER KRESS: It's something like that.

DR. LETELLIER: So there is an association with an effective length scale.

MR. ENDERLIN: The thing to always keep in mind is when you've got the porus media is what fraction is actually --

DR. LETELLIER: Exactly.

MR. ENDERLIN: -- flow passing through.

DR. LETELLIER: Exactly. And, Bill, if you --

MR. ENDERLIN: The hypothetical is that you have 100 percent, all particles see flow.

CHAIRMAN WALLIS: Kind of an indication of the inverse of the particle size, isn't it?

DR. LETELLIER: The effective size, yes.

CHAIRMAN WALLIS: Right.

DR. LETELLIER: The hydraulic diameter essentially.

MR. ENDERLIN: And from experimental data it's shown that it can have an effect.

DR. LETELLIER: I think the better conditions for measuring that might be a packed column under Darcy flow, just like they do geologic samples.

CHAIRMAN WALLIS: So we're going to move along now and you're going to convince us that

everything is fine.

MR. KROTIUK: To suggest with regard to the material masses in the debris bed, this is just again from the Series 1 data and I think you've seen this before, is that this for instance on the NUKON, this is the kilograms per meter squared that were added to the loop and this was actually put into the bed, and then this is for the CalSil values also. These are all NUKON tests. These are NUKON/CalSil. Again, this is all Series 1. And it just shows that, for instance, the CalSil, not all the CalSil that is added to the loop is deposited into the bed.

A larger fraction of the fiberglass does get deposited into the bed.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: Let' see now. This is my saturation correlation. And as I previous indicated and as Dr. Wallis has indicated, this is kind of an effect that was observed is, you know, called the thin bed effect.

This is my correlation, it's completely empirical. It was developed using the Series 1 data. And it relates essentially the density of the NUKON in the debris bed volume to the density of the CalSil in the --

CHAIRMAN WALLIS: This is the maximum density possible, is that what it is?

MR. KROTIUK: This is the maximum density possible that I was able to determine using the text data from Series 1. I want to expand this to include the Series 2 data, but I haven't done that yet.

CHAIRMAN WALLIS: Those are things cubed or is that a 3 and 2 are sub --

MR. KROTIUK: That's a cubed square --

CHAIRMAN WALLIS: They're cubed?

MR. KROTIUK: Yes.

MEMBER KRESS: That's just a --

CHAIRMAN WALLIS: That' the cube -- it's a curve?

MR. KROTIUK: It's nothing more exotic than that.

CHAIRMAN WALLIS: Dimensionally, could all these things have dimensions --

MR. KROTIUK: Yes, might. So this is all done using metric dimensions, kilograms per meter squared.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: And just to show you what it looks like when I plot it up, the red here is the actual data or what I determined was from the data.

This is the curve fit line. And I tried to develop a curve fit that gave me an upper limit. So this curve fit includes the data, plus or zero minus 4 percent.

CHAIRMAN WALLIS: To go through the origin?

CHAIRMAN WALLIS: This is postulated. I've only had the data up here. And I just do that there because I -- with the Series 2 test data I will have data down in this area, but I just plotted it up.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: This solid line is really the only data I have right now.

CHAIRMAN WALLIS: Okay.

MR. KROTIUK: Now what I'll do now compare some of these calculational methods, two cases that I've chosen from the Series 1 tests. In the process for the NUREG report itself to look at the Series 2 test also.

The Series 1 tests were only run, as Carl had mentioned, with the metal screen. But the Series 2 tests were with the perforated plate. But as Carl observed, there was a minimal difference between the results of the two, you know, was observed during testing.

CHAIRMAN WALLIS: Particles saturated by

density is pretty low, isn't it? I mean if they were closely packed particles and the density is bigger than water?

MR. KROTIUK: I mean it's not like someone said --

CHAIRMAN WALLIS: You would expect to get a density of 1,000 or something like that.

MR. KROTIUK: Yes, but it's not like a--

CHAIRMAN WALLIS: For a sand bed, a sand bed would have a density of a 1,000 or something, wouldn't it? And it's 2.5 material density and a void fraction of .4, it's going to have a density of 1500 kilograms per -- a sand bed. You try a shovel full of sand, is heavier than water, so -- so this stuff isn't very compacted, is it?

MR. KROTIUK: From the data this is what you --

CHAIRMAN WALLIS: So it's not very compacted?

MR. KROTIUK: It's not compacted. This is pretty large void in it.

CHAIRMAN WALLIS: Still a large void fraction. Okay. What's the void fraction of raw CalSil? I mean, the stuff that I had in my hand that was the piece of insulation didn't seem to have a void

fraction of --

MR. ENDERLIN: Much higher when it's wet.

CHAIRMAN WALLIS: Much heavier than this. It was much heavier than this, wasn't it?

MR. ENDERLIN: The dry CalSil, no, not at all. When you -- if you prepared it by the motor and pestle and wet it --

CHAIRMAN WALLIS: Just pick it up. Pick up a piece of CalSil insulation. What's it's density?

MR. ENDERLIN: Nothing.

CHAIRMAN WALLIS: Much more than this.

MR. ENDERLIN: No, I'm not so sure on the--

CHAIRMAN WALLIS: You're not. You say it's that light.

MR. ENDERLIN: Yes.

CHAIRMAN WALLIS: Well, okay. We should move on. But I guess we can think about it. I think you know what it is somewhere --

MR. KROTIUK: Okay. I've chosen one test which is a NUKON only bed, so this is a bed that was run for its Series 1 that has --

CHAIRMAN WALLIS: By itself?

MR. KROTIUK: All by itself. Just NUKON by itself.

CHAIRMAN WALLIS: Well, that should work out.

MR. KROTIUK: This is the data in black. And the solid black line is the initial compressions, so the first velocity increased. The dotted lines are the subsequent velocity transients.

The pink line here or red line is the results of the one volume model --

CHAIRMAN WALLIS: The hysteresis?

MR. KROTIUK: Excuse me?

CHAIRMAN WALLIS: You can predict the hysteresis?

MR. KROTIUK: To a degree I'm able to do that.

CHAIRMAN WALLIS: How do you do that? I mean, that's not in your theory.

MR. KROTIUK: It is the theory because of the compressibility function.

CHAIRMAN WALLIS: But then doesn't it bounce back to where it was before.

MR. KROTIUK: Not for the first one. If you remember, the equation for the first one is one cycle. The first cycle is --

CHAIRMAN WALLIS: You don't let it bounce back?

MR. KROTIUK: Yes.

CHAIRMAN WALLIS: Oh, okay.

MR. ENDERLIN: After the first cycle.

MR. KROTIUK: So, I mean, the match up is a fairly good match up between the predictions and the text data for this case.

I just put here for comparison, this is a NUREG-6224 correlation using the same input parameter. And it significantly under predicts the measured pressure drop.

MEMBER DENNING: Do you know why? I mean, I know the compression is part of it, but certainly not down at these low approach velocities. Why doesn't the NUREG predict better?

MR. KROTIUK: You know, you're not the first one who has asked that of me, and I'm sorry I haven't gone back and taken a look at this, so I can't answer that question.

MR. ENDERLIN: Well part of it, it relies on the manufacturer as manufactured link goes. It don't seem to make any sense.

MR. KROTIUK: One of the things that I could say, and let me just go to the next graph, this is the debris bed thicknesses as a function of a velocity. The black here is the data and the red is

the one volume model. And these are the predictions for the NUREG.

CHAIRMAN WALLIS: The NUREG is way off or significantly off.

MR. KROTIUK: So if you're significantly off in your thickness, you affect your pressure drop significantly.

CHAIRMAN WALLIS: Well, NUREG has a bogus theory anyway, and it makes dependent on the gradient instead of the overall stress.

MR. KROTIUK: So I mean so the thickness is very related to the pressure drop. So this is probably some indication.

Now the other case that I want to look at is the NUKON/CalSil bed, and I choose one case there. And for this case I have here in the solid line is the measurements from the test data. The blue line is the one volume model predictions and the pink line is the two volume model prediction. And this is what I'm trying to --

CHAIRMAN WALLIS: One volume is a short of lower bound and the other is --

MR. KROTIUK: Exactly. Because that assumes that you have a homogeneous situation. So that gives you the lower bound. And my postulation

here is that when the testing was started you had somewhat of a homogeneous situation. As time went along and you went through the velocities cycling that you started to move -- change the composition of the bed itself and the particles started to redistribute themselves in the fiber and you started to approaching a two volume type of situation or the condition that I assumed for the two volume situation. So in that situation you're approaching this two volume model.

So what I'm trying to indicate here is that the one volume and the two volume approach give you the upper and the lower limit of the expected pressure drops.

CHAIRMAN WALLIS: If you wanted to be conservative, one could say always say a two volume model?

MR. KROTIUK: That's correct.

Then finally just --

CHAIRMAN WALLIS: I think industry claims that they're getting pressure drops much less than predicted by the NUREG report.

MR. KROTIUK: I can't comment on that.

This is for the NUKON/CalSil bed. This is the comparison of the model predictions with the test data.

CHAIRMAN WALLIS: Is this for a layered bed, two layers?

MR. KROTIUK: Right.

CHAIRMAN WALLIS: All right. Well, you got a new degree of freedom, you can presumably predict more things.

MR. KROTIUK: Well, because I have a two volume model.

CHAIRMAN WALLIS: You can assume different amounts in different parts of the bed and so on.

MR. KROTIUK: Right. I mean if you -- one of the things that we are doing also and as part of the testing is that Carl is sectioning, and he mentioned this in February. We are doing section of beds to actually look at the relative distribution of the particles in the fiber beds. But we don't have that data yet to report.

So the summary is is that one volume model could provide the best estimate for a homogeneous bed and the two volume model will give you the bounding case for a heterogeneous bed.

And then I'm going to be using the Series 2 data to essentially optimize the parameters that I spoke about --

CHAIRMAN WALLIS: In this -- where they

got two orders of magnitude. I didn't see any two orders of magnitude in your graph.

MR. KROTIUK: Which?

MR. ENDERLIN: The delay test.

CHAIRMAN WALLIS: That blue spot. The blue spot was at a 100 times bigger than what you get my extrapolating the homogeneous bed, it was a factor of 100. You have a factor of 2 or something by making it --

MR. ENDERLIN: For the cases analyzed. The question is --

MR. KROTIUK: For the cases analyzed.

CHAIRMAN WALLIS: For the cases you're analyzing. But interesting ones are the ones are the pressure drop is hugely different.

MR. KROTIUK: Well, I could tell you that I didn't report it here, but I did look at the case that I said was a Series 2 case that I believed that had the particles essentially on top of the saturated fiber particle layer. And in that case it completely blocked up the test. In other words, there wasn't essentially any flow that could go through. The pressure drop was very high. And my calculations show that the pressure drop is very high for that case also. But, again, I haven't finished the assessment of

all that.

CHAIRMAN WALLIS: Well, I thought that they were able to -- you can't make it homogeneous then because you've got too many particles.

MR. KROTIUK: You can't. Yes, there's too many particles. You can't make it homogeneous.

MR. ENDERLIN: Well, if you go to a lower ratio.

MR. KROTIUK: If you go to a lower mass, yes.

MR. ENDERLIN: You get a lower ratio.

CHAIRMAN WALLIS: The graph with the blue square up here.

MR. KROTIUK: Right.

CHAIRMAN WALLIS: Was for the same upper ratio.

MR. ENDERLIN: That's a 50 percent. Yes. That's a 50 percent CalSil to NUKON.

CHAIRMAN WALLIS: All right. And that's not saturated

MR. ENDERLIN: And what we saw is a significant difference is if we were at a 25 percent CalSil to NUKON.

MR. KROTIUK: But the key thing is is that I will be looking at those cases.

CHAIRMAN WALLIS: You'll be looking at that stuff more.

MR. KROTIUK: I just haven't completed all that work yet.

CHAIRMAN WALLIS: Because are you still going to keep doing this work?

MR. KROTIUK: For the next two months or so to finish all the assessments, yes.

CHAIRMAN WALLIS: Then you're going to write a report which we can see?

MR. KROTIUK: The report is -- I'll have the final report available in October.

MEMBER DENNING: Now if we had some real concept of what was going on with chemically produced gels you could include them in your model presumably?

MR. KROTIUK: If I had some information there.

MEMBER DENNING: Are you giving some thought to what that might be? I mean I think this morning we were struggling with just what the physical model really is of what's happening with it.

MR. KROTIUK: Well, I mean I think that if you think about it, you might be able to include it. But I haven't really given much thought about doing that right at this point in time.

MEMBER DENNING: Well, it might be interesting to think about.

CHAIRMAN WALLIS: Well, we've got a letter from the EDO. We wrote a letter saying that it was unrealistic to stop research in spring of 2006 because of the unanswered questions that were still there. And he wrote back and said the development of these sorts of models, predictive tools, was for the future and in order to get timely resolution of GSI-191 we didn't need that sort of thing. But you seem to be still working on it.

MR. KROTIUK: Well, up until -- for the next couple of months, yes.

CHAIRMAN WALLIS: So maybe you will get some predictive tool. It's not going to be something that's way in the future. Maybe just be a couple of months away.

MR. KROTIUK: At least maybe for NUKON/CalSil, where there could the extrapolation of something else may be questionable.

MR. TREGONING: And again, the jury is still out on how well we're going to be NUKON and CalSil. I think we've been encouraged so far by some of the work we've seen that Bill's done and Bruce as well. We didn't present this today, but there's also

some correlation --

CHAIRMAN WALLIS: Now is industry doing something similar or what are they doing? Are they just going to use the NUREG-6224 or something or are they doing something similar?

MR. LU: I think they stay away from the correlation relevant work. Most of the licensee design to perform prototypical at last testing. So they consider that that pass is very difficult.

CHAIRMAN WALLIS: So they're not going to do this kind of thing? They're going to rely on the test?

MR. LU: Yes. There is only plant which relies on the NUREG/CR-6224 with plant specific correlations developed and also with very conservative assumption on the debris distribution on the strainer. That's the only plant that we saw in our pilot audit --

CHAIRMAN WALLIS: Well, what the industry is going to do is not really going to make use of these research results. They're going to do their own tests and use those results to predict what happens in the plant. Isn't that what they're going to do?

MR. LU: Yes.

CHAIRMAN WALLIS: Okay.

MR. LU: You're correct.

MR. TREGONING: Let me clarify that, and I think Shanlai might want to jump in.

It's really vendor specific. Certainly a lot of the vendors are doing these prototypical tests, but I know at least one group is conducting a lot of these closed vertical loops --

CHAIRMAN WALLIS: Yes, because I read some of those things and it looked as if many of these groups had this huge sort of room with a flume and all that stuff. But they also had bench top tests. So they had something that looked very much like the LANL or Argonne tests with a loop and a 6 inch diameter thing. So they're probably doing fundamental work as well, they just haven't shown us. But they could try to do the same thing as these guys are doing. But we haven't seen any results of that.

MR. LU: Yes. They do have a loop, a vertical loop very similar to Argonne test loop. And actually that's their plan to study the chemical effect of head loss. And a bump up factor on top of the prototypical head loss testing. So it's not geometrically the shape of the complex geometry of a strainer the head loss data, the measure for the normal -- they are going to add up a bump factor. And

based on a test loop and similar to the --

CHAIRMAN WALLIS: They got a factor of safety sort of thing, you multiplied it by --

MR. LU: No, no. The four chemical effects to address --

MR. TREGONING: So it's essentially a multiplier.

MR. LU: Yes, the multiplier, right.

CHAIRMAN WALLIS: Okay.

MR. LU: But by the baseline the head loss is not based on the correlation.

CHAIRMAN WALLIS: The safety, you say there's going to be a factor of two more to that --

MR. LU: That's right. That's right. You're right.

CHAIRMAN WALLIS: Okay. So we should probably move on then. I'm sorry we're taking so long, but it's very interesting.

Are you going to reach a conclusion now?

MR. KROTIUK: No. That's it.

CHAIRMAN WALLIS: That's it? That's it?

Well, you've come some distance since you last talked to us, and that's very good. Thank you.

This going to go in the open literature some day?

MR. KROTIUK: Yes.

MR. TREGONING: Yes, NUREG.

MEMBER KRESS: You could also put it in a journal.

CHAIRMAN WALLIS: That's what I meant. That's what I meant, put it in a journal.

MEMBER KRESS: I think it's worthy of a journal article.

Now you folks were here last time we met with you. Are you going to tell us something new this time? Well, introduce yourselves and go ahead. I don't think we'll take a break. We'll just go on.

MR. GEIGER: Hi. My name is Ervin Geiger. I'm with the Office of Research. With me is Anne Fullerton with the U.S. Naval Surface Warfare Center. And they're the one that's conducting our research on the transportability of coatings.

We were here in February and we presented a pretty good test outline and also presented some preliminary observations as a result of the testing. And today what we're going to do is just do a brief overview of the program for those who weren't here the last time. And then we're going to present some new information.

The research is the transportability of

coatings. And the objective was to characterize the transport behavior of coatings in a water under stagnant and uniform flow conditions.

Again, the motivation was that the current safety evaluation assumes that all unqualified and failed qualified coatings transport to the ECCS sumps. And what we were trying to do is come up with a more realistic assumption.

And also the background is that currently nuclear power plants have had a number of incidents of failed coatings qualified and unqualified which prompted us to evaluate what the transport characteristics are.

The intended use of this test program or the results will be to provide information to aid staff to assess the license's responses to Generic Letter 2004-02. And also it's for plant-specific analyses of coatings debris transport.

That is the testing that was complete back in January and the report is currently in draft form ready to be issued for review.

The schedule is that we are right now planning to issue this for publication in early fall.

Reviewing the research concept. What we're doing is characterizing the transportability of

coatings. And we're looking at different coating systems. We looked at five coating systems which were representative of coatings that were found in containment buildings of U.S. nuclear plants.

We tested debris size ranges from 2 inch down to 1/64th inch. Sort of captured the size range anticipated.

Debris shapes, we looked at the shapes that we generated were random in their outline, but we tested flat chips and also in a curled condition to simulate chips that might have curled off the wall.

We looked at different densities. The alkyd was from a density of near 1 gram per, I guess, centimeter to the heavier zinc epoxy systems and the we tested a 6 coating epoxy system.

We looked at debris thicknesses. That again, we looked at a one coat, about a 3 mil alkyd system and then we also looked at a six coat epoxy system.

And we looked at a range of water velocities from zero up to 1 1/2 feet per second.

We tested, like I said, we tested five coating systems. We did three qualified and two unqualified. We had a zinc primer with two epoxy topcoats. We call that a ZE systems. We looked at an

epoxy primer and the epoxy topcoat, which is the E2. And then we looked at a concrete coating system which consisted of a epoxy sealer and epoxy surface and two epoxy topcoats.

The unqualified systems were an epoxy six coat application. The epoxy itself was qualified, but the 6 coat system was not. That was to simulate continued maintenance painting without removing the previous coats.

And we also did an epoxy -- oh, I'm sorry. An alkyd topcoat, which was just a single layer. That alkyd, that's the one that was also tested at the other tests. And that is a very -- it is almost like a cellophane body to it, so it's very pliable.

Test overview. We did two types of tests. We did a quiescent test, which consisted of a time-to-sink test. And we tested for an incipient velocity which was the time required for debris that we dropped onto the surface to actually break the surface and start to sink. That was to see how long, let's say after the accident a chip fell on the surface, how long it would take for it to sink.

CHAIRMAN WALLIS: The chip it's washed off the wall it's not as if it has to get wetted when it falls into the sump, is it?

MR. GEIGER: Yes.

CHAIRMAN WALLIS: It's probably ready to sink by the time it's been washed down the wall in a stream of stuff?

MR. GEIGER: It depends. Well, you could have two. The failure is more. One, it could be washed down by the spray. And also, just from the -- if you can imagine in a LOCA just the occurrence of steam of things moving it could fall from the wall.

CHAIRMAN WALLIS: But if it's being carried in the flow, it's already essentially broken the surface, hasn't it?

MR. GEIGER: If it's carried in the flow, yes. And we have also did tests where what we did is we wetted it and tried to see what happened. And we found that some cases the wetting, the initial wetting did not really influence how long it took to sink.

Then we did bulk. We measured time, how long it would take for the bulk or 80 percent of the debris to sink.

And then we did a terminal velocity test where we placed the chips underneath the water surface and just timed what the velocity was, the terminal velocity. And that was so we could sort of calculate how long a chip that was under the surface, let's say,

would be sinking depending on a certain velocity how far it would go.

And then we did a transport test where first we did a tumbling velocity test. We wanted to see what type of velocities it would take to actually start a chip moving that was resting on the floor. That was an incipient tumbling velocity test. And then at what velocity would 80 percent or more the chips move along the floor. And that was the tumbling velocity test.

And based on those numbers then we based our steady-state velocity test, the observations we had during that test.

And so what we learned was, the first observation that we made was that at up to .2 feet per second very little of the coatings actually exhibited any tendency to transport very far. Okay. Actually to the end of the flume. And so we did a test that, we initially were going to test at .1 feet per second, but what we're looking for is to try to get a more upper bound number that could be used. You know, the higher the velocity it would be, the easier it would be for plants and us to look at water velocities and less with transport. Less plants would have a problem, I should say.

And then we looked at how far they would transport at the tumbling velocity. And these were tests just by placing the coating chips immediately under the surface into a moving stream and then observing how far they traveled down the flume.

And with that, I guess I'll let Anne discuss some of the testing specific.

MS. FULLERTON: Okay. We showed this the last time. This is just to review the different coatings and sizes that we tested. As Erv said, we went from the 1/32nd to 1/64th inch size up to the 1 to 2 inch size.

We tested five different coating systems. It was the alkyd, which is ALK, that's the flimsy one that looks kind of like a garbage bag. The zinc, which is heavier, that's the highest density coating we tested. And then there were three different epoxy coatings, the E2, E6 and E3C.

This is the matrix for the quiescent tests, so again we did the time-to-sink test for both dry and presoaked chips and the terminal velocity test --

CHAIRMAN WALLIS: This is alkyd, it's like a piece of plastic or a piece of garbage bag or something?

MS. FULLERTON: Yes, it's like a garbage bag.

CHAIRMAN WALLIS: I would think that would be something which would be pretty bad for screens. I mean, layers of that stuff would be pretty bad for screens.

MS. FULLERTON: It would just get stuck on there. Yes.

MR. GEIGER: And that's, you know, they show.

MS. FULLERTON: And then the terminal velocity tests were done only with the presoaked.

I should also mention that we did a thermal curing test in the quiescent testing just to see if there was any effect for heating the chips. We did 120 degrees for 2 days and a 150 degrees for two weeks. And the effects were minimal. So we didn't include that for the transport testing portion.

This is the matrix for the tumbling velocity tests. Again, we had five different coatings the same size range, and we did incipient, what was the velocity for the fresh chips to start moving and what was the velocity for the bulk of the chips, which was defined as 80 percent of the chips to start moving.

We also did the steady-state transport tests and those were done for 0.2 feet per second as well as the tumbling velocity that was determined in the tumbling velocity tests.

Now we have some results from all the tests, but first the quiescent testing. The chart that you see at the top is for the 1 to 2 inch flat chips. And this is the time-to-sink test. And what you see along the Y axis is the percentage of chips that sank. And you have the type of chip along the bottom.

So the reason that zinc is missing from this one is because we actually didn't have flat zinc chips. The zinc chips came in an already curled shape.

CHAIRMAN WALLIS: The alkyd is zero sinking?

MS. FULLERTON: Alkyd never sank for dry or presoaked. And from the other observation you can see there's not a huge difference between the dry and presoaked percentage of chips sinking.

MEMBER KRESS: That means its density was less than water?

MS. FULLERTON: Right. It was actually just about the same. Yes, just about one. So it would float.

MR. GEIGER: With a gravity of 1 to 1.5.

MEMBER KRESS: Oh, yes. I see.

MEMBER DENNING: If the water level stays above the screens, then it doesn't even enter in or is that not true?

MR. GEIGER: I guess I would think --

CHAIRMAN WALLIS: Unless it's caught on something else. Mixed up with all the other stuff.

MS. FULLERTON: The water's just still. There's nothing that would force it in.

CHAIRMAN WALLIS: Probably tangled up with all the fibers and the CalSil and everything else. It's not as if it's by itself.

MR. YODER: This is Matt Yoder from NRR.

And I'd just like to interject here the reason that we use these light alkyd chips, there are two reasons actually. At the time that we developed the test matrix we did not have data to show how these kind of chips would fail. We now know based on the EPRI testing, which I'll talk about a little bit tomorrow during NRR's part, that these things are going to fail as fine particulate. Okay. So that eliminates some of the concern about these things transporting and not here in the stream.

The other reason that they were included

in this matrix is because they are very light and we thought that would bound the lower end of chips that you could see in a plant.

MR. GEIGER: Well, I could see the tendency for these things to float. As the water would start to move, they would just -- you know, like leaves on the surface, you know.

MS. FULLERTON: And then the lower chart there are some of the terminal velocity test results. So we have -- there's a bar graph for each of the different sizes. So the dark red is the smallest size we tested. The dark blue is the 1/8th to 1/4 inch. The yellow is the 1 and 2 inch curled. And the light blue is the 1 inch to 2 inch flat.

So you have the different coatings along the X axis and the terminal velocity in feet per second along the Y axis.

It was hard to correlate the terminal velocity with density. You'll see in the next slide there's the chip weight per unit area had a greater influence. So this was the weight per unit area along the X axis and grams per square centimeter and terminal velocity again in feet per second along the Y axis.

So you can see that there's higher

terminal velocities with the increasing chip size and weight per unit area.

CHAIRMAN WALLIS: Well this is some sort of average velocity, because these things don't fall regularly, do they? Don't they sort of wobble around as they fall?

MS. FULLERTON: Right. This the average. We tracked all of the -- I think about a 100 chips we tracked and took the average terminal velocity.

MR. GEIGER: That's of each size --

CHAIRMAN WALLIS: This is individual chips, this isn't group chips?

MS. FULLERTON: Individual chips. Not group chips, yes.

MR. GEIGER: I think it's five or ten at a time.

MEMBER MAYNARD: I've got to go back on the previous one. Why do you even have a thermal velocity for the chips that don't sink?

MS. FULLERTON: Well, they weren't dropped on the surface. These were placed --

CHAIRMAN WALLIS: Once you pushed them in, they sink.

MS. FULLERTON: -- just below the surface so that they --

MEMBER MAYNARD: But once they get below the surface they will sink?

MS. FULLERTON: Yes, they will sink.

MEMBER KRESS: Right here.

MEMBER MAYNARD: Okay.

CHAIRMAN WALLIS: Surface tension or something.

MEMBER KRESS: Surface tension.

MEMBER MAYNARD: Okay.

MS. FULLERTON: Right.

MR. TREGONING: As Graham mentioned in an actual LOCA environment it's expected with water entrainment to be much more, you know, you won't have these quiescent conditions on the surface. You'll have mixing. You'll have mechanisms for causing entrainment. So the time-to-sink tests with dropping them on the surface were important, but that doesn't necessarily imply that in a LOCA if you had chips, that they would not be submerged.

CHAIRMAN WALLIS: Yes.

MS. FULLERTON: These are some of the tumbling velocity test results.

CHAIRMAN WALLIS: They're picking up from the base?

MS. FULLERTON: Yes. This is when we put

the chips on the bottom. Now, this is not the quiescent tank anymore. The quiescent tank was water is not moving, it's a big vertical column. This is a long tank, so it's 30 feet long with a 3 foot by 3 foot cross section.

And what we did was place a number of chips on the bottom and a starting area, and then we'd slowly increase the velocity of the water in the tank.

CHAIRMAN WALLIS: As feet per second?

MS. FULLERTON: This is feet per second, yes. Feet per second.

CHAIRMAN WALLIS: So many of these chips were never, if they were on their own, might just lie on the bottom of the sump forever?

MS. FULLERTON: Right. Because some of the velocities are pretty high to pick those up.

One of the things to observe is that shape effects the tumbling velocity. The curled chips have a pretty low tumbling velocity because they have those edges sticking up, so it was a lot easier for them to get picked up at lower velocities.

For the large chips --

CHAIRMAN WALLIS: Now, I'm looking for these very small chips.

MS. FULLERTON: The very small chips are

the ones with the vertical lines. So the --

CHAIRMAN WALLIS: Oh, they're the ones on the -- they're the hardest to pick up, are they, is that right? They're the hardest to pick up? The small ones are most difficult.

MS. FULLERTON: Sometimes. There are some differences. So for the zinc they were harder to pick up. Now that's possibly because the zinc, like I said before in the large chips, is curled because that's the way it came. When you get smaller pieces, they're flatter.

CHAIRMAN WALLIS: They lie in a boundary layer on the bottom. They're very hard to --

MS. FULLERTON: Right. And they're the highest density so --

MR. GEIGER: We tried to draw some correlations between the densities and shapes and things. But because we didn't have anything consistent that we could -- you know the test inputs were all over so that we didn't have any consistent parameters that we could compare over the range to come up with a real, you know, trend.

MR. GEIGER: There's a lot of things changing at one time. So if you look at the black bars, those are the biggest chips. You can see there

is an increase in tumbling velocity with density. So the two, the higher densities are -- but then the E6 -- E6 has the most layers. So E2 is a two coat epoxy, E6 is a six coat epoxy. So it did take a higher tumbling velocity to pick that up.

MR. GEIGER: The E6 system and the E3C system were actually the heaviest chips.

MS. FULLERTON: Right.

MR. GEIGER: When we looked at the density times the thickness, they were pretty heavy.

MS. FULLERTON: Right.

CHAIRMAN WALLIS: This is just about velocity of the water you're looking at --

MS. FULLERTON: Both velocity of the water, the average velocity over the cross section.

CHAIRMAN WALLIS: The turbulence on the bottom depends upon whether the water's coming? If you had a bend, for instance, you would expect that the secondary flows and things would probably stir things up --

MS. FULLERTON: Sure. And you'd have different.

CHAIRMAN WALLIS: -- in a different way than in a straight flume?

MS. FULLERTON: Right.

MR. GEIGER: And these tests were primarily to see what they would be in like a steady stream. And when it's applied to unique plant condition layouts, then those would all have to be evaluated. You know, some plants you have a large open area in front of the strainer area, so where the approach velocity would be pretty uniform. And then you have other plants where the location of the strainer is such where there would be a lot of turbulence from water coming out in the RCS and so on. So there's no many different unique applications that it would be difficult to model them all in this test.

MS. FULLERTON: So then the last kind of testing that we did in the long flume was the transport testing. We tested -- now here we didn't have the chips on the bottom. We introduced the chips just under the surface and we did this for two different velocities. One was the 0.2 feet per second--

CHAIRMAN WALLIS: They sank to the bottom before they made it to the end of the flume, is that it?

MS. FULLERTON: Some did, some didn't. That was kind of what we were looking at.

CHAIRMAN WALLIS: Some of them did.

MS. FULLERTON: In the 0.2 feet per second most of them fell out. Most of them fell out right away. The alkyd, there was very few that made it to the end. This is actually for the one to two inch flat chips. And you can see two percent of the -- so there's a bar for each type and you can see the colors show you where they ended up. And I should just define, there's the front section. That was within the first three feet of where we introduced them.

The middle section, that's --

CHAIRMAN WALLIS: What does the blue mean in that?

MS. FULLERTON: The blue? Well, I'll get to that.

So there's the front section which is the light blue or it's the one on the bottom.

CHAIRMAN WALLIS: The light blue -- in other words, the end section is the ones where you got zero?

MS. FULLERTON: Right. The end section is the zero, so there's none of that color. So the there's the three sections of the tank and then we actually had a filter system at the end of the tank. So there was the bottom of the filter, the middle of the filter and the top of the filter.

CHAIRMAN WALLIS: This tank is not prototypical of any of any particular plant?

MS. FULLERTON: No. This is a very generic long rectangle and a filter at the end, yes.

MR. GEIGER: The was 30 feet long and the particles, the chips were introduced 8 feet into the tank. And then there was --

CHAIRMAN WALLIS: Thirty by what other dimensions?

MS. FULLERTON: Three foot by three foot was the cross section.

CHAIRMAN WALLIS: Because some of these changes that the industry is using have a much -- or a different aspect ratio. They're not very long, they're not very long flumes, right? They have sort of a big room and they toss stuff in and then they have a filter, a screen.

MS. FULLERTON: Right.

CHAIRMAN WALLIS: They do not have a long flume like this.

MS. FULLERTON: No. This was actually -- we had the acrylic flume built to those dimensions and suspended it another circulating water channel that we had so that we could contain the chips.

So, yes, only about 2 percent of the alkyd

chips got to the end. Nothing else did. Most of the stuff fell out in the middle section with a little bit of it falling out pretty much right away as soon as it was introduced.

Yes. This is the same type of chart for the smaller debris, the 64th of an inch to 32nd of an inch. And it's very similar. The other sizes were also very similar. Basically all you're seeing is that it fell out in the middle section and didn't transport very far.

MEMBER SIEBER: Are the tank velocities typical in and around the sump in about --

MS. FULLERTON: The 2 feet per second was what the NRC had advised us to use for the --

MR. GEIGER: Most of the testing that was done was at .1 feet per second, I think, which is probably the more typical of approached velocity.

MR. YODER: Matt Yoder from NRR again.

These tests were around .2 and what you see the replacement strand is they're going in, you're more like .02 -- .002 to much lower velocity at the strainer surface itself. So there will be areas within containment that are higher flow, channeling flow and whatnot, but --

MEMBER SIEBER: But the closer you get to

the sump --

MR. YODER: -- the closer you get to the sump, you're talking about much lower velocities than these.

MR. TREGONING: For unblocked screen there's velocities --

MEMBER SIEBER: It sounds like you could almost --

CHAIRMAN WALLIS: Well, you got to be careful because some of these screens -- what's the velocity based on? I mean, you get these screens which are sort of multiple things. The velocity is often based on the area of the screen, but there's sort of the superficial area of the flow coming towards the screen is much less. So it's a real question of what you use for a velocity.

MR. GEIGER: I think what the plants would have to do is they have to look at it go a certain distance beyond the screen and take the area across the flow area and look at what velocity is there and not necessarily behind the screens.

CHAIRMAN WALLIS: But I think some of the vendors are looking at using CFD, I think what happens there. I think the Swiss people in their presentations.

MEMBER MAYNARD: Well, I think it'll vary quite a bit from plant-to-plant. Because I think you're going to find some where the basement area is really going to be quite large compared to what the overall screen surface area is. So the velocity throughout most of the room would be very low. You might find some others, though, to where the screen size dominates the space.

CHAIRMAN WALLIS: Some these designs, the screens fills almost every available space.

MEMBER SIEBER: But they've said the velocity will still be low.

MEMBER MAYNARD: Yes. I think it will be low, but it will vary I think. But I think for all of them it's going to be a very velocity.

MR. GEIGER: That's how we thought the .2 feet per second would be something that would --

MEMBER SIEBER: Bound it?

MR. GEIGER: -- would be --

CHAIRMAN WALLIS: You see we don't know what criteria NRR is going to allow, whether or not you have to consider coatings --

MR. YODER: Matt Yoder again.

I can say that from some of the vendor testing that we've seen even if you get coatings that

approach the screen and actually impact the screen, often these flows are so low that these coatings just fall right to the floor when they're in the chip form. So a licensee from NRR's perspective may be able to take credit for lack of transport of coatings if they can prove that those coatings fail in a chip form. Now that's yet to be seen. They'll have to provide data to show that.

MS. FULLERTON: So then we also tested the debris --

CHAIRMAN WALLIS: Excuse me.

MS. FULLERTON: That's all right.

CHAIRMAN WALLIS: So if you are saying if the coatings were fractured to micron size and so is chips, then they might well go to the screen? And there's something in between --

MR. YODER: Then I think they would behave just like CalSil debris or any other particulate debris. We would assume that they would transport and then --

CHAIRMAN WALLIS: Right. All right.

MS. FULLERTON: Okay. So we tested also in addition to the 0.2 feet per second at the tumbling velocity that was determined from the previous testing.

CHAIRMAN WALLIS: This is much higher?

MS. FULLERTON: Much higher. And it's different for each coating and size. So if you look along where we have the type listed in parenthesis there's the tumbling velocity of that particular --

CHAIRMAN WALLIS: 1.4 feet a second then?

MS. FULLERTON: Right, 1.4 and down to the ALK, which was 0.5. So still kind of low. So the alkyd, most of the ALK debris and the 2 coat epoxy E2 transported to the end of the flume and wound up in the bottom of the filter at the end. Most of the 3 coat epoxy fell out in the middle section. And most of the E6 fell out of suspension at the end of the tank before reaching the filter.

MEMBER SIEBER: They were the heavy ones or not?

MS. FULLERTON: The E6 was the heavy. Yes, that's the 6 coat epoxy ones, the very heavy.

DR. SHACK: The alkyd made it all the way to the top of the filter?

MS. FULLERTON: Some of it did, yes

MR. GEIGER: The 15 percent made it to the top 3 inches of the filter.

MS. FULLERTON: Most of it wound up in the bottom, and that was the only one that ended up in

either the two top sections of the filter.

DR. SHACK: And you had nothing in the middle?

MS. FULLERTON: That was the 2 inch. This is the 64th of an inch to a 32nd of an inch debris. It's the same type of plot.

Again, you have the coatings is in the parenthesis, the actual tumbling velocity of that size and coating. Most of the ALK and over 50 percent of the E2 and the zinc transported to the end of the flume.

CHAIRMAN WALLIS: We have the same question for you we've had for everybody else. And this looks very fascinating, very picturesque, what's going to be used for making any sort of a prediction?

MS. FULLERTON: Well, I think the most important thing that we learned from this was that nothing transported at the 0.2 feet per second.

CHAIRMAN WALLIS: But if you can sort of say something is zero, that's a useful conclusion.

MS. FULLERTON: Something is zero.

CHAIRMAN WALLIS: But nothing gets to the screen. And so dismiss it, say you don't have enough velocity for anything to get to the screen. But when you start to saying well how much gets there, then we

don't know how to make the prediction, do we? We just know that for your particular flume and your particular stuff, you know 54 percent went somewhere.

MS. FULLERTON: Yes. I'm not sure that we saw --

CHAIRMAN WALLIS: There's no equation, there's nothing that --

MS. FULLERTON: I'm not sure that we saw a trend. We looked at the different trends with respect to size and shape --

CHAIRMAN WALLIS: But there's no predictive tool of any sort being tested or validated here?

MS. FULLERTON: No. In this experiment.

CHAIRMAN WALLIS: So all qualitative sort of in terms --

MS. FULLERTON: That's right.

CHAIRMAN WALLIS: -- of how it applies to a real plant?

MS. FULLERTON: Right.

MEMBER MAYNARD: Well, I think it does establish some velocities that may not be the exact one, but that if you blow that you probably don't have an issue, but if you're above that, then you have to --

CHAIRMAN WALLIS: We might establish some zeros for you.

MEMBER MAYNARD: Right.

MS. FULLERTON: Right.

MR. TREGONING: And in terms of coatings, that's a very -- it's very important to establish those metrics. Because some plants are looking at a potentially incredibly large debris of coatings that they have to consider. So these metrics would be --

CHAIRMAN WALLIS: Square feet?

MR. TREGONING: Yes. These metrics would be quite valuable in terms of helping refine what really they realistically have to deal with.

CHAIRMAN WALLIS: And the size of these coatings --

MEMBER KRESS: Yes. It puts a lot of importance on the generation size that you have to--

MR. YODER: As I said, you know, the characteristics of the coating debris is the most critical thing here.

MEMBER KRESS: Yes.

MR. YODER: I think what this shows us is if you can prove that it failed these chips, you know it's not transporting it at .2, it certainly will not transport at .02 feet per second or point .002 feet

per second as we're seeing from a lot of these vendors and a lot of the licensees.

CHAIRMAN WALLIS: But if you can get a zero, that's useful. Right.

MR. YODER: Correct. But as I said, still some testing remains to be done by the industry to prove that your coatings will actually fail in this size range, in this chip range. Now it may be that some percentage fails like this and another percentage fails as fine particulate.

CHAIRMAN WALLIS: So you're quite sure it might well be, you know, you're pretty darn sure that below a certain or above a certain size and for certain kinds of coatings and certain velocities nothing gets to the screen? But apart from that, you probably have to assume that everything else does.

MR. YODER: That has been our stance that unless somebody comes in with testing that shows that, let's say zinc because it's a very dense particulate does not transport, then that might be an exception. But other than that, we would assume that the particulate coating debris makes it to the sump and that it's considered for head loss implications and downstream implications.

CHAIRMAN WALLIS: Good.

MS. FULLERTON: So in conclusion for the quiescent tests, the ALK debris of all sizes remain on the surface indefinitely and have the lowest terminal velocity. So when we dropped it on the surface, it never sank and it also had the lowest terminal velocity. And again, the density was very close to that of water.

Terminal velocities increased with chip size and chip weight per unit area.

In the transport testing the shape affected the tumbling velocity. The curled chips had the lower tumbling velocity because they had more surface area to get picked up with the lower velocities.

At the bulk tumbling velocity, most of the ALK debris transported to the end of the flume.

When the 2 inch chips were in the bottom section of the filter, then the smaller chips were mostly collected in the center section of the filter.

At the bulk tumbling velocity with the exception of ALK of E2 about 30 to 100 percent of the debris depending on the type did not reach the filter. So it was only ALK and E2 that had the higher percentages of debris reaching the end of the flume.

At .2 feet per second there was only a

very small percentage of ALK and E2 that traveled the length of the tank. Most of the debris did not transport.

And so for the range of the coating systems and the debris size tested in this experiment, the velocity of .2 feet per second seems to be a good threshold for debris transport, so things aren't transporting at .2 feet per second.

I think that's all we have. Have any questions?

CHAIRMAN WALLIS: Any questions from the Subcommittee? If not, then you've helped us to gain a bit of time. We get back to where we should be. And if the wrap up in terms of concluding remarks is short, we'll be right on time.

MS. FULLERTON: No pressure.

CHAIRMAN WALLIS: Remarks?

MS. EVANS: Yes, I have 30 seconds, okay.

CHAIRMAN WALLIS: Well, you can take as long as you need.

MS. EVANS: I wanted to thank the Subcommittee again for the opportunity to come here today to make our presentations.

The focus today has been on the research and results that we've seen since our last meeting in

February. As we mentioned in here, the Staff and our contractors are going to be focused over the next several months in completing the review and documentation of the test results that we have.

We expect that that documentation would be completed by November, which would be in time for the next Subcommittee meeting. So I would expect that most to all of the reports would be available at that point in final form.

We recognize that there may be a need to do additional research, okay. Tomorrow I think you're going to hear a little bit about some work that's being pursued since our last meeting in February in the area of downstream effects. Today we heard about numerous questions and concerns that have arisen as a result of the research that we've done. The importance of many of those remaining issue is very plant specific. It's a function of the plant specific parameters, ECCS design margins and also the mitigation strategies that the industry uses.

At this time we don't view that pursuing research for any of these concerns is the immediate action to take. Our focus is on completing the review of the results and documenting those results at this point in time.

The industry's got the lead and the responsibility to address the outstanding issues that are applicable to their plant specific design. Okay. And we've got, I guess, the NRC's got the lead to verify that the resolution strategies address the applicable outstanding concerns.

So as we go forward and move through the resolution process, you know, the Office of Research and NRR, we'll continue to evaluate the need for additional testing.

CHAIRMAN WALLIS: Thank you.

MS. EVANS: Yes.

MEMBER DENNING: It is time for us now to have discussion?

CHAIRMAN WALLIS: Yes, I think we ought to have a discussion on what we heard today. And then we'll have another one tomorrow on what we hear tomorrow. Because it's very different.

MEMBER DENNING: I think particularly with the research here.

CHAIRMAN WALLIS: Because it's very different in nature of what we're going to hear tomorrow.

MEMBER DENNING: Right.

CHAIRMAN WALLIS: What I'm wondering is

really whether this material is mature enough for it to be evaluated by the full Committee or whether what we need to do is simply make a sort of Subcommittee report. A lot of this -- we had a letter. We've had a couple of letters on this stuff. The letter we wrote last time I don't think has change in my mind by what I saw today.

MEMBER DENNING: Well, since I'm a short timer --

CHAIRMAN WALLIS: Yes, why don't you go ahead.

MEMBER DENNING: I'll say that I can write the letter.

No, I think that the reason that there might be some urgency to have the full Committee hear this and to write a letter is that I think that the approach the industry is going down has some substantial risk associated with it that when NRR really gets into the evaluation, there are going to be issues that are going to be raised that can't be addressed well because we just don't have a sufficiently good fundamental understanding of what's happening.

What I've seen over the past couple of years here is that I think Research has done a

terrific job and certainly the highest praise to Rob for his presentations and what -- and then what all his contractors have done. But I see us still on a fairly steep learning curve. And you know, I keep hearing advances and understanding and I think that if Research continues to work aggressively on resolving these issues and developing a better fundamental understanding in some areas like clearly we don't have a very good understanding of how this gelatinous debris really behaves, that it decreases the risk that down the road here when the tests are really performed by the vendors that we'll get into a box where the NRC just can't approve the results because of lack of understanding.

So I would certainly encourage a continued aggressive -- obviously important to document these results. But I would encourage continuous aggressive research in a couple of these areas to help our modeling capability. And perhaps also to do some experiments that would potentially challenge the way that industry will do its large integral experiments, not from a point of view of showing them up, but just making sure that we really do understand how appropriate the assumptions are that they'll be making.

So that's kind of my feeling. That it's too soon to stop the research and that the risk of failure at the end of this is increased by limiting the research now.

CHAIRMAN WALLIS: Bill, you can't say anything.

Mario, do you have some comments.

MEMBER BONACA: Well I agree with what Richard is saying. It seems to me that the biggest issue is how does all these things come together. And when you put them altogether does it really provide you the answer that you want or does it cover all the bases. And then that's really where it's hard to do.

I think that probably we need to see what the industry is doing, however, because they may have a plan to do that. And maybe this information is sufficient for the NRC to provide some judgment on what is being presented. So I cannot prejudge what the industry is going to submit to us and is going to do, you know. But I guess what strikes me is there a lot of good information here. But I think about how we put it all together to address the issue, the problem that we have, I don't know how you would do it. There are so many uncertainties there and so many unknowns.

CHAIRMAN WALLIS: Jack?

MEMBER SIEBER: My turn?

CHAIRMAN WALLIS: Yes.

MEMBER SIEBER: The first question you asked was should there be a full Committee meeting based on what we have heard today and probably will hear tomorrow. In my opinion I don't think we're far enough along right now for that. But I think a Subcommittee report would be important, one that's perhaps a little more detailed than the standard Subcommittee report. Put a little more detail into that.

I think overall the Research Staff and its contractors have done a pretty good job except there is not a lot of testing that will really define what the analytical models should look like. And of course there's time constraints, there's budget constraints and they all enter into that. And so one has to ask the question will NRR and NRC management know enough from the tests that they have to be able to say that a licensee's proposition is good or not good -- sometimes they vary more than one parameter at a time and sometimes it wasn't clear which variation was causing -- and they have that ambiguity involved and not deeply involved in all these things, but that

ambiguity is involved it seems to me more difficult to accurately model what's going on. And that's one of the problems that I saw that would be satisfied by additional research to separate the variables and on the other hand, the Generic Letter is already out there. Designs are occurring. There's a balance that has to be achieved.

I don't think we need a full Committee letter.I think the Staff's done a good job for the time and the money that they have. The question is how long do you drag it out and do you have enough where you can reach some reasonable.

So that would be my conclusion. Maybe after tomorrow's --

MEMBER BONACA: It seems to me, I mean that again, I mean the biggest questions I have is regarding on how does all this come together in a convincing way that it works.

MEMBER SIEBER: Yes.

MEMBER BONACA: And I think until I have some applicant that comes in with an approach and proposes something, it's hard for me to say what additional research the NRC needs to do. Because I think probably that first attempt to license an approach will raise a number of questions that says,

you know, you should know more about this and this and that. And, you know, maybe then that's why I still have questions in my mind about what additional research should we recommend at this stage when we know that the industry is coming in with some proposals there that will then test our questions of what research.

MEMBER SIEBER: Well, the interesting thing is that the Staff is not designing the drains.

MEMBER BONACA: That's right.

MEMBER SIEBER: They're resolving the problem. So what they're doing is gathering information about areas that they need to know more about.

MEMBER BONACA: Right.

MEMBER SIEBER: While the licensees are out running around trying to analyze the problem and design the screens to the proposal. Assuming that that occurs and these proposals come in for that, maybe this body of knowledge will help the staff to understand well enough what the issues are and understand the licensee's approach to tell whether it's a good approach or a bad approach.

CHAIRMAN WALLIS: Well, I think tomorrow we have to ask NRR, and now they've all gone home,

what they have learned from this research, whether it's enough. If it's enabled them to establish any sort of criteria or process or something that is going to help them evaluate what industry submits.

MEMBER SIEBER: I think that's a good--

CHAIRMAN WALLIS: And if they think this is a complete set of work for that purpose, why is it complete. And my suspicion is that they're going to forget it all and they're going to simply have some qualitative idea of the questions to ask, and then they're going to do everything based on what industry submits.

MEMBER SIEBER: Well the difficulty with that approach is that the process is going to really -- because the structure isn't there. And I guess that can happen, but I --

MEMBER MAYNARD: I think it would be difficult to cover this subject at a full Committee meeting in the current state of things in the time frame that would be allowed. I think a Subcommittee report would be beneficial, but I'm not sure what material you would choose to present and not present. And I also think there's still some valuable information to come from some later efforts finding out what the industry is proposing and what they're

doing I think is important.

I do believe some additional research is going to be needed. I'm not sure that at this point without knowing what the industry is going to propose and without even evaluating what they've got right now, whether just continuing the same research is the right thing to do or whether it needs to set back, regroup and take a look at what the industry's proposing to see where the focus the future research effort.

MEMBER DENNING: You know I think there, I mean I think I know what industry is proposing. And I could be wrong, and I heard a little bit today that was different about a version that's a little more analytical. But I think we clearly know that the industry is headed towards integral tests to fill this gap of the proof test.

MEMBER MAYNARD: Yes. And I think that -- well, I'll get back to where I really think the research probably needs to focus in the future in just a minute here.

I do think that a lot of information has been gained. And I think that from what's been done, the Staff may have enough to develop some criteria to at least screen out and determine that there's going

to be some plants and some groupings that's okay, some that definitely have a problem. And there's still going to be some in the middle I think that's going to be difficult without some additional research there.

From my own perspective I think this gelatinous material is the one that is most bothersome. And I think where the research needs to focus on that in that area, a decision needs to be made that we really try to understand it and it's effects or that we really try to understand what it takes to make sure that we don't get that.

And that's really what I'm talking about, focusing where we go with the research as to how much you try to understand what it may or may not do versus you really focus on what you need to do to make sure that you don't get that. And so I think that's some of the decisions to be made. I do think it's going to take some more research, though, to be able to come to a conclusion for all the plants, but there may be a population of the plants to where there's enough information now. I don't know.

MEMBER KRESS: Well, on the need for a full Committee, let's make it unanimous, let's do a report. I think it's called for.

I know --

CHAIRMAN WALLIS: I thought we were standing with not in favor of --

MEMBER KRESS: Well, I don't know. Maybe it's not unanimous.

MEMBER MAYNARD: Try to keep pressure on.

MEMBER KRESS: The plurality.

But I know it's not NRC's goal or at least they've expressed such to have a fully predictive model. But I think we're getting close. For a long time I thought that would never be possible, but now I think we have some real hope. And the reason I say that is let's look at the issue of typical effects. I think it's a delta function. I think you can neglect it up to a certain level of amount in there, and then it goes up. So Research out to focus on where is that point and where you have to worry where it has to go infinite or wherever it goes. And you can rule -- see it's better or not.

I really felt the modeling that we heard on the NUKON and the CalSil with the various layers showed a lot of promise. And I think continuing along those lines you either have the gel in there or you don't and then you could fall back on this porous layer of modeling,

With the transport, I think the need there

is for better information on the generation size, and I think there's a need for some actual modeling there, some physical modeling of how it transports and gets there rather than just it's either going there or not. I think there's a need for some modeling there. And I think you can put all that together then and have a finally definitive model that you can judge what the industry comes in with. And I would actually encourage the staff to think along those lines.

One last item, I thought the PIRT was a good move, but if it were me, I would definitely try to some sort of argument to argue away the radiolytic effects. Because you're going to get tangled up in a real mess there. But, you know, that's just my own opinion.

MEMBER DENNING: Usually you do a PIRT when you're going to do some research.

MEMBER KRESS: Yes. So basically that's all I have to say.

CHAIRMAN WALLIS: Well, I was looking at what I what I learned today. We had, first of all, a presentation on the aluminum chemistry and what I learned from that is that it is possible to make surrogates that looks something like the ICET and probably atypical of what might be found in a plant.

But I also heard that it's also possible to screw up. It didn't seem to be really definite criteria about how you do it right. So I was left uncertain there. I'd say yes we know that there's hope that these surrogates really will be good, but I wouldn't hear enough about -- I was told that it was possible to make them in some other way which was not suitable, and I didn't really know how one would judge whether or not industry had made them in a suitable way. I have sort of a box into which they had to fit. So I didn't have criteria there for evaluating, so I wasn't sure how NRR would evaluate when --

ANNOUNCER VOICE: This conference is showing no activity. If you would like to continue the conference, please star 1 now.

CHAIRMAN WALLIS: No, we don't want to continue the conference.

MEMBER DENNING: Is that a criticism of the Chairman?

CHAIRMAN WALLIS: Yes. It's a voice from somewhere.

MEMBER KRESS: I didn't hear anything.

CHAIRMAN WALLIS: Bill Shack showed that it's possible to get leaps in pressure drop like forming gels. And he showed that if you didn't have

enough aluminum, it was more difficult you had to wait longer and so on. Again, this didn't lead to any kind of predictive tool for what's the pressure drop when you do get a gel or when does this leap occur. So, again, it was a bit like showing yes there is an effect that needs to be thought about, but I didn't know how I was going to evaluate an industry submittal on this kind of thing.

MEMBER KRESS: I thought you needed to know when the leap occurred.

CHAIRMAN WALLIS: Right, you need to know when it occurs and why and to how much time does it take and --

MEMBER KRESS: Yes, forget the pressure drop.

CHAIRMAN WALLIS: -- how much aluminum do you have and so on. So, yes, we had some idea of the kind of thing that happens, but not much of an idea of how to predict it.

And the peer review I don't really have any comments on until I see the peer review. I think that we need to see what these people think about the work, and we didn't really see that.

Gravity driven, well again this seemed to be work in progress. It's an interesting simple

experiment, but we didn't see conclusions drawn from the results there.

Bill Krotiuk has made some progress in this region model. The thing that I missed was how it ties in with sort of this factor of two orders of magnitude when he's shown a factor of one order of magnitude. And I felt that, yes, they had a lot of promise but it order for it to be validated for use it had to be based on a broader range of experiments. It's got a lot of promise, but it wasn't something I'd go out and use to predict.

MEMBER DENNING: Could I inject a comment there, Graham?

CHAIRMAN WALLIS: Yes.

MEMBER DENNING: You know even though it looked like -- I think there's some unexplained reason as to why there's such a discrepancy between that and the old NUREG. It really was only a factor of 2 in pressure drop. And I think --

CHAIRMAN WALLIS: That was for the NUKON alone.

MEMBER DENNING: Yes, that was for the NUKON. Right. Right.But if we could get a factor of two, I would be happy.

CHAIRMAN WALLIS: But you know there was

this blue square point which --

MEMBER DENNING: Yes, I know. Yes.

CHAIRMAN WALLIS: -- causes the -- he didn't actually explain that.

MEMBER KRESS: Well, he did mention that he thought it was a completely blocked filter.

CHAIRMAN WALLIS: So I think what we said in our last letter still applies. I mean are there questions that Research has made a lot of progress, but it isn't all buttoned up to the point where you can predict stuff. It seems to me it's exactly the same state of affairs as we had in our last letter. I don't really see that we want to have a presentation which leads to the same letter we were at last time. I'd be happy with a Subcommittee report.

MEMBER DENNING: As long as we get a letter back --

CHAIRMAN WALLIS: I thought we might be further along. I think we might well now send the reply to the EDO that was held up by the suggestion of a Committee member last time.

I think by November when we get all these reports we do owe a letter evaluating what's been learned from it all. And it may well be that when we hear from NRR tomorrow we may hear enough that we will

want to hear them present to the Committee. What is it they've learned from all this research, how are they going to evaluate the industrial work and so on. We haven't heard that yet. And I assume we're going to hear something about that tomorrow, but I'm not sure what we're going to hear.

MEMBER KRESS: That was a good wrap up.

CHAIRMAN WALLIS: So that's the way it is. I feel like Walter Cronkite or somebody.

I think we may want to think about this a bit more, overnight and so on.

MEMBER KRESS: Well, we have the presentation tomorrow anyway.

CHAIRMAN WALLIS: We another presentation tomorrow, too. Right.

MEMBER SIEBER: We may change our mind after tomorrow's --

CHAIRMAN WALLIS: We may change our mind tomorrow. But if the Commission is hoping that we are going to write a strong letter saying as a result of what we heard today we've changed our mind and everything is fine and they're on a course to great success, I'm not sure that's what they're going to get. It's going to be the same letter as last time. There's been a lot of progress, a lot of good work and

we don't know how it all comes together.

MEMBER MAYNARD: Again, I'll be interested in what they say tomorrow because that's where the real key is is how do they intend to use this information to then determine what criteria they're going to use to accept or deny --

CHAIRMAN WALLIS: I think it's a bit naive to assume that industry is going to raise all the questions. It seems to me that the key advances in understanding or awareness of phenomena which needed to be considered has not come from industry. It's come from Research and from ACRS. I'm a little weary of waiting for industry to determine all the phenomena that need to be considered.

MEMBER BONACA: You know, I made a comment regarding -- I wasn't suggested that, but I was suggesting that that at their first attempt of putting all these pieces together, I think some of the weaknesses will show up and will point for additional work to be done in Research. And I'm not sure, I'm not here long enough, this is the first Committee I attend, but I'm not sure that all the basis are fully understood to make the judgment now.

CHAIRMAN WALLIS: Well we don't know. We've got what? Five or six vendors who present

stuff.

MEMBER BONACA: That's right.

CHAIRMAN WALLIS: It may well be that they will present results which are incompatible.

MEMBER BONACA: And they understand that are some WCAPs which have not reviewed yet, so we don't know what's in there.

MEMBER KRESS: We still have to hear about the downstream effects also.

MEMBER BONACA: Yes.

CHAIRMAN WALLIS: And when we do get all this stuff written up, we do get all the WCAPs and we get all the reports from all this research and we get submittals from vendors, I'm alarmed about what it would take to really review it thoroughly.

MEMBER KRESS: A good predictive model.

CHAIRMAN WALLIS: I mean, it might be a full time job for somebody.

Anyway --

MEMBER DENNING: It's not your job.

CHAIRMAN WALLIS: Let us stop at 6:00 or before 6:00. Then we can talk about what we're going to do after that.

So we'll close for today.

(Whereupon, at 5:59 p.m. the Subcommittee

adjourned.)