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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMITTEE

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SOURCE TERM APPLICABILITY PANEL

MEETING

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THURSDAY

DECEMBER 13, 2001

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ROCKVILLE, MARYLAND

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The Panel met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B1, 11545 Rockville Pike, Rockville, Maryland, at 8:35 a.m., Brent Boyack, Facilitating.

PANEL MEMBERS PRESENT:

DR. DANA POWERS, ACRS

DR. THOMAS KRESS, ACRS

BRENT BOYACK, FACILITATOR

DAVID LEAVER, EPRI

JAMES GIESEKE, CONSULTANT

BERNARD CLEMENT, CEA FRANCE

JEAN-MICHEL EVRARD, CEA FRANCE

ALSO PRESENT:

JASON SCHAPEROW

MOHSEN KHATIB-RAHBAR

JOCELYN MITCHELL

PATRICK BLANPAIN

STEVE NESBIT

LARRY LOSH

HAROLD SCOTT

ED LYMAN

* indicates unintelligible word(s) due to accent.

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

(8:35 p.m.)

MR. BOYACK: Good morning. Just a couple of items that I wanted to check on as we begin, and then we will return to our Source Term Applicability work.

I just wanted to go ahead and reaffirm that the date for the third and final meeting of the panel, we agreed yesterday that that meeting would be held in February, the last week, February 26th, 27th, and 28th. That is Tuesday, Wednesday, and Thursday.

And the idea here I think for setting the meeting this far ahead is so that those of you who need to make arrangements that involve non-reformable tickets and everything, this is a short day.

So I just wanted to look at Charlie and Jason, and make sure that this day is all right with the NRC.

MR. SCHAPEROW: We may have an issue with it. Can we get back to you later today?

MR. BOYACK: Yes.

MR. TINKLER: Well, from what I know right now, it would not be -- and not that my involvement is critical, but it would be a real bad meeting for me to have, because it is the start of the EUSAFE Program,

and their first meeting is scheduled for that week.

MR. BOYACK: Just to go ahead and give you an idea of where you are. If you look at the week of the meeting before, you will see that there are ACRS meetings; and the week of the 11th for Tom and also Dana. It is a holiday the next week.

I can't remember if there was anybody else. Then we get into February, and I was trying to keep the meeting to the first part of the month, and the first week was--

MR. CLEMENT: Well, with the NRC, Mark won't be involved with that.

(Discussion off the record.)

MR. BOYACK: So a Wednesday, Thursday, and Friday. I think that possibly could work, but let's just check everybody's calendar. Now, the discussions from the audience, you have to use the mike.

MR. CLEMENT: Is it a firm date for --

MR. TINKLER: I believe it is.

MR. CLEMENT: What this means is that assuming now that this week that has been previously scheduled is still available, it would leave within the period that we initially had blocked out, two weeks, the first of them beginning the week of the 18th of February, and there was a suggestion that we

meet on Wednesday, Thursday, and Friday.

Is there anybody for whom that would not be acceptable?

DR. LEAVER: My wife.

MR. SCHAPEROW: Shall we have the meeting out in California?

MR. BOYACK: Okay. David, what do you see there? Then there is this other possibility. While David is looking at that calendar, the week of March 11th. What is the situation there with everybody?

MR. LEAVER: The week of the 11th is a better week for me, and if push comes to shove, I could do it then.

DR. KRESS: Dana, there is some suggestion that we go down to Turkey Point on Wednesday and Thursday of that week?

DR. POWERS: Yes, I think we have our Turkey Point visit then.

MR. BOYACK: Are you talking now about the week of March?

DR. POWERS: Yes.

DR. KRESS: Yes.

MR. BOYACK: All right. It looks like, although it is difficult to David, that really leaves the only available date here of -- and you suggested

Wednesday, and Thursday, and Friday? Is everybody else all right with that?

DR. POWERS: I think for us that work for a hard working laboratory, we don't even get that holiday.

DR. KRESS: I don't even know what holiday it is.

DR. POWERS: I have no idea what it is either, but I'm sure we don't get it.

MR. LEAVER: I'm not sure that we get it either, but we might.

MR. BOYACK: Well, those who do work at progressive laboratories do celebrate President's Day, and we do celebrate it on Monday, the 18th, although I can travel on Monday, the 18th. Again, it is not a big thing to me. It is just a question of when you would rather go.

If you would rather meet on the 19th, 20th, and 21st, that's perfectly all right with me.

DR. POWERS: It is mox-nix.

MR. BOYACK: I am the only one who has a holiday, right? And you guys don't travel.

DR. POWERS: Oh, I am sure that they get the holiday.

MR. SCHAPEROW: We get every holiday.

DR. POWERS: And on President's Day we have to travel. As an independent Federal Agency, you probably shouldn't celebrate that holiday.

MR. TINKLER: Well, we celebrate Washington's birthday.

DR. POWERS: Oh, that's an idea.

MR. LEAVER: Well, if we do the 20th, 21st, and 22nd, can we adjourn early enough Friday?

MR. BOYACK: Well, what I propose is that we just go up to the middle of the week, which would be the 19th to the 21st. Would that be all right with everybody? February 19th through the 21st, Tuesday, Wednesday, and Thursday.

MR. LEAVER: Yes, that's fine.

MR. BOYACK: Then that's okay. Now the idea here is that nobody is going to cross-program, and we can go ahead and make reservations and things like that.

Now, the next thing that I would like to do before we actually get working is just to ask a question that would help me with the documentation. If you look up on the screen here, this is one of the typical tables.

It was the last one that we were working on for the Tellurium group, and what I have done in

all the tables that I have generated thus far is I have just entered your initials, and it seemed to be a good way because we were taking individual comments.

The question is the final report. There are perhaps three options that occurred to me. The first is that as I sit down I have tried to identify what I think would be the key points. We strip any reference to individuals, and it just becomes a view of the panel.

The second one is that we leave the names there and you get to clean up your own live transcription of your comments, but they are identifiable.

I guess the third approach is something like they did on one of the elicitations, where they become Expert One, Two, Three, Four, and Five. Does anybody have any questions or comments, because I am going to ask if there is a preference for how to document this. Any questions?

MR. GLESEKE: Well, I think that as it appears now there is a lot of just nonsense in there, like same as X, Y, Z, or you know, those kinds of comments.

MR. BOYACK: Sure.

MR. GLESEKE: I see one up there by my

name, for instance. I mean, it is the same as some reference. Does that mean anything?

MR. BOYACK: Well, my preference on a claim document is to summarize the information, and put it down, and let the panel review it, and comment on it, and help me to correct it, and get it right. But then it doesn't have specific names. David, do you have any comments on this?

Well, the one thing that you lose there is that you lose something that might look more like a minority opinion, and we would have to figure out a way to do that.

DR. POWERS: Well, you just say that the majority opinion was this, and there was a view that--

MR. BOYACK: We could do that.

MR. LEAVER: I think there is some value to different views, or in some cases even contrary views to document that and not lose it. But I think the way that Dana described it would be an okay way to do it. I don't think it makes sense to have the same as so and so.

MR. GLESEKE: And then there is a lot of lines in there like this.

MR. LEAVER: I think it is kind of less important on who said what, but to capture the

fundamental information that may be of use for people in the future when they are trying to figure out what we did, and what they want to do.

DR. POWERS: Well, for instance, there is a line in there that says when we looked at the JAERI data, and, oh, I will get you the reference to that very JAERI data, and things like that.

MR. LEAVER: Exactly.

DR. POWERS: And we can dress that up and things like that. It is like progressives, and you need to know what data the panel was looking at, and the fact that they didn't have whatever was known in five years.

MR. LEAVER: Exactly.

MR. BOYACK: All right. And again, the way that this will happen is that when I send this information to you, I will send it in two forms, two electronic forms. One will be a PDF file, which you can't work on, and one will be a Word file, Word 6 or something like that.

And then if you choose, you can work electronically on the file in color, and I will put these instructions in to just send it back to me marked up that way, whichever way it works for you. You can mark it up and fax it back to me. I will take

it any way.

Okay. Well, with that, before we actually go back to Tellurium, are there any other comments of an overview nature that you wish to offer? If not, when we concluded yesterday, we had moved on to the Tellurium group.

After we finish Tellurium, we have the Barium Strontium, Cesium group, and Lanthanides to complete by noon. We had gone through on Tellurium, the gap release, and actually slipped in a value of a half-a-percent.

We had some differing opinions on the early vessel, and I guess those range between 15 and 35 percent. And I don't think -- by that time it was late, and we had not really decided on how to come to a view, but we just heard the comment from Dana that sort of said that what we might do eventually here, and the majority opinion was to state the value again, and another opinion was to enter that.

Well, enter my password. I don't know what my password is. Oh, I'm sorry. I am curious about it why it came up. So I would propose that the way that we handle this is since I--I don't see any way to continue on in a protracted discussion, which would lead us to a point where we would sort of

negotiate a value here or a value there. That is my opinion.

You may offer a different opinion, but I suggest that what we do on the early in-vessel is enter what we call the majority opinion about the release fraction, which I think was in the area of 30 percent.

Then we would make a statement that there was another opinion that the release fraction was in the area of 10 to 15 percent. I don't know any other way to do that.

MR. LEAVER: Well, there was a comment--I don't know if you documented it, but I think at the end of that discussion that Dana had a comment.

MR. BOYACK: Is that here?

MR. LEAVER: I thought I saw it in the thing that you handed out. Maybe Dana can remember what his comment was.

DR. POWERS: Well, my comment was that even if we take the lowest value, the 15 percent value, you are doing radical surgery of this, this fraction, and you are really --

MR. SCHAPEROW: That comment is right there under notes, the very last sentence.

MR. BOYACK: Yes, it is. Down at the

bottom here, if you take a look.

MR. LEAVER: Yes, and I think we kind of broke up right at that point.

MR. BOYACK: Right, that's where we are. So the question is would the rest of the panel accept an entry of 15 percent, and if not, we could go to the majority and minority statement, because I think everybody else was in the 30 percent range.

DR. POWERS: It seems to me that if you have got a majority opinion of around 30 percent, then you might as well put that in.

MR. BOYACK: Right.

DR. POWERS: I will comment that before we met that the number that I had written in for that one is a change as it was 15 percent, and when I did it without the discussion.

DR. KRESS: I would be interested in how you arrived at that 15 percent.

DR. POWERS: I think I arrived at it, Tom, thinking of the release is a hundred percent from the tool, and Bernard reminded us that the PHEBUS result, which is a fairly oxidizing environment, but not deliberately so -- it is not huge -- was a more quantitative release, and that they got very much higher penetration.

And more troublesome than that, they get higher penetration when the codes are calculated for this steam generator tube. It is like the codes are overpredicting what the amounts and it is an aerosol/physics problem.

And we never thought that. We always that the codes were giving us a lower bound on the deposition because they don't take into account all the subtleties and things like that.

And it just reminds us that it is a bit more complicated than you think, and I don't think I was taking that -- and so when I listed to you and Jim, and Bernard talking, and you reminded me of the FPT-1 results, I moved it back up.

MR. LEAVER: Let me ask a question of Bernard. What was it in PHEBUS that -- you had this period of between 6,000 and 6,500 seconds, of which you had release occurring?

MR. CLEMENT: Which release?

MR. LEAVER: Your release occurred over a period of somewhere between 6,000 and 6,5000 seconds. What was going on, or what was the form of the bundle? Had the bundle melted and slumped, or was it just kind of sitting there with steam going by?

DR. POWERS: The jump in the release that

you get out at the end there.

MR. LEAVER: At the end?

DR. POWERS: That is a relocation that is taking place

MR. LEAVER: So relocation occurred at around 5,500 or 6,000?

DR. POWERS: Yes. They had several of them, but you will recall that he is giving a pretty fair release all the time, and then it kind of jumps.

MR. CLEMENT: It jumps, and we have a relocation of hot material.

MR. LEAVER: Do you have your integrated release chart, and maybe we could take a look at it?

MR. CLEMENT: Too many slides. What you have at the end is movement of hot materials.

DR. POWERS: Old time slides.

DR. KRESS: It's a sad day when George passed away.

DR. POWERS: Oh, did he? I didn't notice that.

(Discussion off the record.)

MR. CLEMENT: You are speaking about this?

MR. LEAVER: Yes.

MR. CLEMENT: And then it seems that the * should be roughly the same for all the * inventing

material lower, because it is higher because there is -- this * is higher.

MR. LEAVER: At the end, yes.

MR. CLEMENT: But it is roughly the same rate for all three items * and the difference in slopes comes from what happens at the beginning, and --

MR. LEAVER: There is a little bit of a delay, about a 200 second delay, yes.

MR. CLEMENT: So, after that --

DR. KRESS: Actually, that is the kind of results that we got at Oak Ridge when we used fuel that didn't have clad on it and it was highly oxidized. The ones with clad on it was the problem.

MR. LEAVER: Was this -- well, what kind of a fuel bundle was this? Was it a typical PWR?

MR. CLEMENT: Yes, it was okay. 20 watts coming from the R-3 reactor. So short watts coming from the R-3, and --

MR. LEAVER: Okay. And it was fully clad?

MR. CLEMENT: Zircaloy full cladding and the *.

MR. LEAVER: And it was heated how?

MR. CLEMENT: Heated by neutronic heating --

MR. LEAVER: Heating from what?

MR. CLEMENT: Neutronic heating from the -- the UO2 is heated. But only if this is the PHEBUS, but if we look at the VERCORS, we have three pellets that are zircaloy -- and heated by high frequency -- and we have roughly the same rates for the material.

DR. KRESS: And that is basically what I have in my model.

MR. CLEMENT: I mean, it is a -- * same tendency, and recently they choose * .

MR. LEAVER: The FPT-2 has been done, right?

MR. CLEMENT: Yes. FPT-2 has been done, and it gave the results for the material.

DR. POWERS: And it is the same.

MR. CLEMENT: And they are the same.

DR. POWERS: Even though that one was much less steam. It actually went in earlier, in 20 minutes of the test.

DR. KRESS: The only thing I would ask is --

MR. LEAVER: And then FPT-2 was a more --

MR. CLEMENT: It was a period of producing where it was much lower. Typically, it was .5 instead of 1.5. You have things in a period of about 17 or 18

minutes, and all the steam flow was consumed by zircaloy oxidation, and plus nearly all the zircaloy oxidized, and * . And so the Tellurium release was * during this period.

DR. KRESS: Where did you get your fuel?

MR. CLEMENT: From Belgium --

DR. KRESS: From ER3?

MR. CLEMENT: ER3.

DR. KRESS: Yes, I wondered, because about the only thing that I could reconcile the difference between the Oak Ridge and that was the type of clad, but that is what we used. We used Belgium ER3.

MR. CLEMENT: There are many sources of ER3 used.

DR. KRESS: I was wondering.

MR. CLEMENT: We made a study at the beginning of the safety program to be sure it was okay, because many people that they *.

MR. LEAVER: So in terms of inert materials, the only inert materials in this experiment was the clad?

MR. CLEMENT: No, not the clad. It was also *

MR. LEAVER: Oh, in PHEBUS you had --

MR. CLEMENT: Yes.

MR. LEAVER: So these are the finger piece --

MR. CLEMENT: They is a rod in the center of the --

DR. POWERS: Oh, he just has a rod that goes down and out right in the center of the package.

MR. LEAVER: In a BR3? I mean, it is out of Westinghouse, basically a Westinghouse type report?

DR. POWERS: Basically, Westinghouse, yes.

DR. KRESS: And the VERCORS didn't have any.

MR. CLEMENT: The VERCORS didn't have any.

MR. LEAVER: Yes, the VERCORS did not.

DR. KRESS: And it is hard to discount that.

DR. POWERS: And it is very difficult to conform.

DR. KRESS: Okay

MR. BOYACK: I think there has been a good review, but we need to move on. Let me tell you how I propose to handle this. What I will do is that in any case where you have a difference of opinion, you will see that there is a footnote here, and we will refer down to the bottom, which in this case that says that I recently had --

But basically this is a note to the majority opinion. However, one member of the panel felt that lower release rate of 10 to 15 percent was more applicable. So that will be explicitly called out when they look at that number, rather than just somewhere down in the text, okay?

Now, the one that we are working on now, the Tellurium, we have a total of five groups left, or 4-1/2, and Tom drew the short straw for being the first speaker on Tellurium, and then we will go around, and there is four more groups and four more leads. So we will just go in order.

And what I put in for the early release was 30 percent. So, now we are going to move to the next release, and it is your turn.

DR. POWERS: I arrived at the 30 percent number by a release that was the fraction of normal fuel and high vertical fuel, and so what is left that didn't get released in the pool is 40 percent. And that is going to get released in the same sense that some of the other volatiles and from residual fuel that was in the core, it will get released from that.

The vessel had fails, and everything that goes down and does the core concrete interaction will get -- and so I would be putting about a 40 percent in

the -- release.

MR. BOYACK: All of which will be released when it reaches the concrete block?

DR. KRESS: Yes, or in residual fuel, the fuel that is left up in the vessel after the vessel failed, and the fuel has been released from that, and so I counted that as excess.

MR. BOYACK: Okay. So that the statement that I need to capture is the release from the residual fuel core.

DR. KRESS: Interaction. I know that I said interaction, but --

MR. BOYACK: I didn't realize that I was so literal on the transcription. Thank you. Okay. Jim.

MR. GLESEKE: I'm trying to reconstruct. I went through all this logic yesterday, but I can't remember. I think I had --

MR. BOYACK: Do you need a moment to reconstruct?

MR. GLESEKE: Yes. Let me take a few minutes.

MR. BOYACK: Dana.

DR. POWERS: I'm behind Tom. I can't help but relate an antidote. Dick Vogle bet me that

Tellurium would not be released during a melt concrete NR action because he was in love with the reaction of Tellurium with metals.

And so I took advantage of him, because when you do a melt concrete interaction after the test results, you smell rotten eggs. And the reason is that the gypsum out of the concrete is being reduced down to sulfur, and sulfur is being vaporized as H2S.

So I knew damn well that Tellurium was going to come up, and so I laid a bet with him for a case of Scotch, and he has never paid off.

MR. LEAVER: Let the record show.

DR. POWERS: Let the record show that he has never paid off.

MR. BOYACK: Do you need to continue on for a moment?

MR. GLESEKE: Yes.

MR. BOYACK: Okay. Bernard.

MR. CLEMENT: Okay. We have got in our estimation a number of Tellurium in the future to be released by cerium * interactions.

MR. BOYACK: Yes, you basically have a complete release from either the fuel and residual.

MR. CLEMENT: Yes.

DR. POWERS: We will catch up with you.

We are just slow, that's all. You have to remember that we are colonials.

DR. KRESS: I didn't get the complete release for two reasons. One of them was not all of the fuel got real hot, because peripheral fuel was pretty cold, and because a lot of the fuel was still at the low burn up levels, and doesn't get released as fast at the low burn up. So that is the reason that I --

MR. CLEMENT: Okay.

But just what we are saying is that roughly * of Tellurium, either fuel remaining in the vessel, or the fuel remaining in the vessel after lower pressure, and *.

DR. KRESS: But it all gets released, that's for sure.

MR. GLESEKE: I'm coming up with about the same number of 35 to 40 percent.

MR. BOYACK: So, see TK, the -- David.

MR. LEAVER: Okay. I wouldn't disagree with that, but I think this is a -- well, this is probably a similar kind of situation to what we had for the other volatiles, which is the release really is from what is left in the vessel. I don't think that much Tellurium is going to go down into the

container.

MR. BOYACK: Yes.

MR. LEAVER: Yes, and the way that Tom expressed it was that it was very slight.

DR. KRESS: I think you are basically right given these --

MR. LEAVER: Right.

MR. BOYACK: Okay.

MR. LEAVER: So we would want to footnote this in the same way that we did the other volatile releases, indicating that this isn't really the next vessel release. It is a faster -- release.

DR. POWERS: There is a bunch of data from Elrick and Company, and from the Brits, saying, gee, you vaporized this collodium and you ran it down and as soon as it sees metal it reacts, and that lent a lot in the trap melt there to a very high deposition of velocity for Tellurium.

And since that time as it moved towards more sophisticated chemical models, where the gas stays, and you have these control rods boiling off, you end up with tin tellurides, and silver tellurides, and things like that.

Should we note that that's why we get relatively high transmission through the piping

system?

MR. LEAVER: Well, we are getting there.

DR. KRESS: Well, yes, when we get late releases and --

DR. POWERS: Yes.

MR. LEAVER: Well, that's where we are now isn't it?

DR. KRESS: We --

MR. LEAVER: There is no transport section of 1465.

DR. KRESS: Oh, yeah, the earlier vessel has to include the fraction.

MR. LEAVER: And you have already done that.

DR. KRESS: Yes, I have already done that. In fact, basically I said --

MR. LEAVER: But maybe we need to capture Dana's point just as a point of information.

MR. BOYACK: And that is?

DR. KRESS: Well, the earlier vessel number is 30, and 60 percent got released, and the rest of it -- and so that is what happened to the other and that's where it had to go. So we are saying that -- out, and that is a number that we probably ought to talk about. It used to be that --

MR. BOYACK: Okay. Since I was not tracking the discussion at times, does somebody want to give me a statement that I could enter in?

DR. POWERS: I think I would just note that because of the fuel rod -- I mean, the control rods and other metals in the system, chloromas transport as Tellurides through the reactor cooling system.

MR. CLEMENT: Not as elementary Tellurium.

DR. POWERS: Not as elementary Tellurium, and consequently it doesn't chemically react with the piping system.

MR. CLEMENT: I would agree that there is some suspicions *

DR. POWERS: Yes, and that's all we know. I wouldn't argue with that.

MR. BOYACK: Okay. So basically what happens is that because of interactions with the control rods, and because of chemical reactions with control rod materials --

DR. POWERS: Because of chemical interactions with vapors from the control rods, and other things, and it would be tin from the clad as well.

DR. KRESS: You see, Telluride is --

DR. POWERS: It is a place in Colorado, and you can ski there in the winter time, or you can get doped up there anytime. I think that Telluride is permanently floating about two feet off the ground.

MR. BOYACK: Okay. If we move on to related vessel.

DR. KRESS: The question is why did we choose the number of 50 percent?

DR. POWERS: Well, it is just like the iodine and the season of its particular deposition.

DR. KRESS: Yes, but I don't think we are dealing with deposition.

MR. LEAVER: Because it is mis-mash?

DR. POWERS: I don't think so.

MR. GLESEKE: The French experiments down are less of a hold up.

DR. POWERS: Yes, they got real hold-ups.

DR. KRESS: And that might be because the bulk of the aerosol --

DR. POWERS: It is hard to argue that they didn't have it, because of the melting down fuel.

DR. KRESS: Well, but the aerosol concentration is --

DR. POWERS: That is almost exactly right. It is almost exactly right.

DR. KRESS: -- outside the flow rate.

DR. POWERS: It is almost exactly right for concentration in the piping system.

DR. KRESS: And how about delay times?

DR. POWERS: It is pretty well glomerated up. We don't see a whole lot of evolution in the aerosol. It is zipping along pretty fast, and the flow rates are high.

DR. KRESS: And we will still stick with the 50 percent.

MR. BOYACK: Charlie.

MR. TINKLER: I just want to make a request here. I understand the logic as you go through the tables of the different phases. But for the benefit of future users of this document, it might be nice if we really think that some of these releases are laid in-vessel, and we put them under laid in-vessel, as opposed to putting them under ex-vessel. Just so that when people go through this document --

DR. POWERS: The problem, Charlie, is you have got about 10 hours, and if we think it is concentrated in the front end of it rather than spread over the whole 10 hours, we put it in the ex-vessel.

MR. TINKLER: Well --

DR. POWERS: And there is a footnote that

says why.

MR. TINKLER: Then maybe you should consider some modification of the duration for the laid in vessel.

MR. LEAVER: Or break it into two phases, or something like that.

DR. POWERS: Well, effectively, you have that.

MR. LEAVER: It's just that you call it ex-vessel, and that is misleading.

DR. POWERS: Yes.

MR. TINKLER: I can see this now. Three years down the road --

DR. KRESS: Nobody is going to remember it.

MR. TINKLER: -- why in the heck did they put that under -- oh, yeah, and did they do it for other things, and --

DR. POWERS: There is a footnote on the table. I mean, what do you want?

MR. TINKLER: I can understand.

MR. SCHAPEROW: Actually, this is a very big departure from what is in 1150. I mean, 1150 has a Tellurium release -- with more concrete interaction. I don't think anybody here was on that particular

panel for that issue, but I personally would like to see a little more clarification in this areas, too, but I am not -- I am an in-vessel, but I don't know too much about that stuff.

DR. POWERS: All you can do is sit here and create another column, and augment the amount of work by 20 percent.

MR. TINKLER: Well, I appreciate that, but I don't want to do that. To the extent -- and I want to raise another issue here, too, because I know that we have talked about this, and I am not proposing that I know the answer to these questions, but to the extent Tellurium is influenced by metals -- and I know, Dana, that you have already addressed that.

The scaling and preservation of zircaloy to fuel passes, and PHEBUS is something that you might want to look at a little more carefully. We have the ratio of zircaloy to fuel may be a little different in some other reactors than it is in PHEBUS.

We have plans for zircaloy grid space, and we have plans with other zircaloy. We have lots of --

DR. POWERS: They have zircaloy grid spaces.

MR. TINKLER: Yes. So you re confident that the zircaloy --

MR. CLEMENT: For the zircaloy --

MR. TINKLER: So you are confident that the zircaloy mass relative to the UO2 emphasis ratio is good in PHEBUS?

MR. CLEMENT: At this time, it is * * the same in the reactor right now *.

DR. POWERS: But when you are talk about material mass, they are very good. I mean, that was a focus in the design of the test, was to get the --

MR. TINKLER: I know, I can remember that.

DR. POWERS: And the relative amount is about right.

MR. TINKLER: And I remember the concerns about some of the -- and this is different, but concerns that the Swiss had that they wanted more upper internal steam separators, dryers, lots of steel on the top of the vessel. That is not --

MR. CLEMENT: Speaking of volume with reactors obviously.

MR. TINKLER: I understood that. That is a difference.

MR. CLEMENT: Well, the * is okay.

MR. TINKLER: But do you -- so you think the steel masses are scaled well enough, and the effect of a small bundle relative to a larger core is

--

DR. POWERS: That is more problematic.

MR. CLEMENT: You could argue that you are getting more oxidation of metallic surfaces, and perhaps PHEBUS would --

DR. POWERS: You would play hell trying to make that argument.

MR. TINKLER: I am just bringing that up, and this is not in any way a criticism of PHEBUS.

MR. CLEMENT: No, no, I understand that.

MR. TINKLER: I am the biggest fan of PHEBUS in the world. I wish we ran three tests a year instead of -- you know --

DR. POWERS: So do they.

MR. TINKLER: I would like to have more high burn up tests, and air ingression tests, and everything else. I am just -- the arguments about unreactive metals, unoxidized metals, and the argument about tellurides, that is an excellent point, and would make some of my discussion moot.

Is the panel going to read on chemical form of Tellurium?

MR. CLEMENT: The point also I think is related to oxidation processes, because even Tom * zircaloy is not * and sometimes with cladding and not

being able to continue any more * and then all the activity on that has not interacted with such metal.

I mean, it is really * processes and has been * of the core that are cold than the Tellurium that is left in the upper part to escape. And that is really into the degradation processes, and this is where it is quite not so easy to make -- to come from a small bundle to * and many effects are taking place.

DR. POWERS: In fact, I would argue that as far as getting the Tellurium in a known reactor form, the large bundle is going to lead you more toward that than a small bundle, because rather than having a control release and then kind of tail off, you are having control rods going off all the time in a large bundle.

And so there is always kind of a constant amount of silver and cadmium coming through the piping system at the same time. And zircaloy reacts with those things.

MR. TINKLER: I was just going to say that originally I think we had a little too much silver, the ratio of molds of silver to molds of iodine at once upon a time. So we were getting an exaggeration of the silver iodine to --

DR. POWERS: Well, that was true in zero.

MR. TINKLER: But not -- okay, because it was -- yes, you are right on that one. With respect to the chemical form of Tellurium, could we anticipate that this would decay to iodine in its --

DR. KRESS: It doesn't matter whether it is in its chemical form or not. It will be iodine or --

MR. TINKLER: It will be iodine in its molecular form or --

MR. LEAVER: I was just talking with a couple of more guys, and there is six isotopes in Tellurium that are among the group that is considered for design basis the 60 radionuclide considered for design basis access.

And three of them -- there is one 27-M, and 127 and 129-M, and 129, and those are of course would not be interesting even if indicates iodine, because iodine would be non-radioactive.

Then there is 131-M, which is interesting, and 132, which is interesting. 131-M has a 25 minute half-life, and as to 131, and that is the main source of iodine.

DR. KRESS: But your question is that this is going to be iodine or -- and I don't think it disassociates.

MR. LEAVER: We did some dose calculations to get the effects of this, and it is not truly what one might expect, and the 131 is a big contributor, and we assumed that it was as I-2, and you are using a little more I-2 than you would have otherwise.

DR. KRESS: I would have assumed that it was iodine.

MR. LEAVER: You would have?

DR. KRESS: Or behaved like iodine.

MR. LEAVER: I mean, you have this clump of stuff, right? A couple of micron particles of something -- and the stuff decays and becomes iodine. Why is it going to stay --

DR. KRESS: It is iodized.

MR. LEAVER: Why is it going to stay as an iodide?

DR. POWERS: I think that's not how I would go about attacking the problem. I would put it in containment, and I know that when it decays the recoil alone will rip that compound completely apart.

MR. LEAVER: The what?

DR. POWERS: The recoil will rip the compound apart.

MR. LEAVER: The recoil?

DR. POWERS: The decay recoil. If I shoot

an electron out this way, something has got to go this way.

DR. KRESS: The whole thing goes out.

DR. POWERS: Oh, no, it would bust the bond.

MR. CLEMENT: To compare that to your --

DR. POWERS: And then I would run it into the iodine chemistry model, and let it do what it will do what iodine does. From that point on it will do what iodine does, okay?

Whatever state that iodine is born in, it isn't going to last very long. And you have cesium iodide, and I-2, it doesn't make any difference. You go so quickly to a steady state that it wouldn't make any difference.

MR. BOYACK: Could we go ahead and move on.

MR. LEAVER: Well, these are important points.

MR. BOYACK: That's why I waited.

MR. LEAVER: I thought I had reached the point, and if I am wrong --

MR. BOYACK: I think that is acceptable.

MR. LEAVER: Okay.

MR. BOYACK: Okay. Laid-in vessel.

MR. SCHAPEROW: I guess I wanted to know the application of it. We are still not quite sure why we would have -- coming out as a result of -- and I don't know if anybody had a chance to go back and look at the question.

All three of the experts have Tellurium, have 25 percent Tellurium, from other interactions.

MR. LEAVER: That may be because they didn't release it from the vessel.

MR. SCHAPEROW: Oh, okay.

MR. LEAVER: I mean, we may not have released it all.

DR. KRESS: Does it --

DR. POWERS: Well, it is the one thing that we can predict. That is the only product that I am very confident of Vanessa predictions, because we specifically validated that because, by god, I was going to get my case of Scotch if it killed me, and Dick Vogle was not going to give me the horse laugh.

DR. KRESS: It depends on the type of concrete that you used.

DR. POWERS: No.

MR. SCHAPEROW: One of the things also that I see in the beginning from where it seems the message that I seem to be getting is that we insert --

associated with the heat in the releases for fission products, and generally it is the difference between regular fuel and high burn up fuel.

I am not hearing much talk about, well, this is so important for my burn up.

MR. LEAVER: I don't think that this is an issue --

DR. POWERS: Hang on. As we move down the list --

MR. SCHAPEROW: Maybe some of the other items are more important, but at least for -- at this point it doesn't seem to be.

MR. LEAVER: Well, it is probably worth noting that what we have been discussing here for the last couple of hours on zirconium is really not -- we would have had this discussion whether we were here for high burn up purposes or not. Is that fair to say?

MR. KHATIB-RAHBAR: I have a comment on this zirconium issue in the vessel part. The argument is that in the PHEBUS you predicted a complete release of Tellurium because most of the zircaloy was gone either due to oxidation or relocated.

And the other argument that Dana was giving is that Tellurium is going to be in metallic

forms because of all of the control rod material. So presumably that will not settle on surfaces as it would otherwise, and would not react, for example, on metallic constituents like tin on a surface or whatever, to form things that would not ge retained in the vessel.

It would behave like cesium iodide or cesium hydroxide, or what have you. If this Tellurium is going to come very late, in the very late in-vessel phase, you may not have a lot of control on the material left inside the core.

Most of the stuff is probably gone by them.

MR. CLEMENT: You may not follow to the surface.

MR. KHATIB-RAHBAR: Well, you have a lot of surfaces inside the vessel, and there is a lot of steel inserted.

MR. CLEMENT: Yes, and the surface is oxidized.

MR. KHATIB-RAHBAR: I'm sorry

MR. CLEMENT: The surface is oxidized.

MR. KHATIB-RAHBAR: Well, you are assuming you have lost enough steam always, but you typically have steam derivatives in most of these accidents. So the concern that I have is that you probably are

putting too much Tellurium into the containment.

DR. POWERS: We have not gotten to the ex-vessel late, or the late in-vessel you have.

MR. KHATIB-RAHBAR: But you are releasing a hundred percent of it though.

DR. POWERS: And you only have 30 percent on the viping system.

MR. KHATIB-RAHBAR: So 30 percent is still retailed of the total.

DR. KRESS: Yes, but it is not going to be there.

DR. POWERS: It's not going to be there very long.

MR. KHATIB-RAHBAR: But the argument, the way that you have it, you are going to release the rest of it anyway most likely, and so you are going to put most of it into the containments.

DR. POWERS: Eventually we are going to have it pretty much in the containment. Wait until Tom launches --

DR. KRESS: Yes, but wait until --

DR. POWERS: We have not gotten to that part yet. But he is going after it.

DR. KRESS: I probably would put a .25 in the -- or .02 in the halogens, or something like that.

DR. POWERS: Well, the number that I wrote down was .2.

DR. KRESS: Yes, .2.

DR. POWERS: Because?

DR. KRESS: Because the .05 is predicated on the fact that there is not very much there in the first place in that particular thing, and if there is a lot there, it is not latched on chemically to the surfaces. It is latched on to something else, like an aerosol, and it is going to behave like cesium hydroxide.

So it is about the same amount on there as the cesium hydroxide, and so the .2 seems to make a lot of sense.

MR. BOYACK: Okay. There is -- at present and is not related to surface, but rather it has bonded to aerosols and more readily to --

DR. KRESS: Released.

MR. BOYACK: Okay. Jim.

MR. GLESEKE: Twenty percent.

MR. BOYACK: Okay. Dana.

DR. POWERS: Well, once we open up the piping system, and while the oxygen portion will go up, we oxidize all the Telluride TEO, which is highly volatile.

MR. BOYACK: To what?

DR. POWERS: TEO.

MR. BOYACK: TEO?

DR. POWERS: Yes, and telluric acid.

DR. KRESS: What is the chemical formula of telluric acid?

DR. POWERS: It depends on which one you are worrying about.

DR. KRESS: I was going to see what it would be.

DR. POWERS: You are just testing him.

MR. BOYACK: No, I don't know the formula.

DR. KRESS: Okay.

MR. BOYACK: And I don't plan on learning it either.

DR. POWERS: Oh, come on. It is good for your soul.

DR. KRESS: There is probably about five of them or six.

DR. POWERS: Yeah, there is a bunch of them.

MR. BOYACK: Okay. Bernard.

MR. CLEMENT: Okay. We did not study the * of Tellurium. We did not consider it because of uncertainty, and just because we have already put 17

percent into the containment.

DR. KRESS: It amounts to the same thing.

MR. CLEMENT: But we agree with * and maybe some mechanisms.

DR. POWERS: Well, once again, you see the importance of doing the PTA work and PHEBUS.

MR. BOYACK: Okay. David.

MR. LEAVER: I would have nothing to add to those comments.

MR. BOYACK: Okay. Jim, you realize now that you are on the hook for the next lead here.

MR. GLESEKE: What is the next lead?

MR. BOYACK: On the barium strontium group; and David, you will have the lead on the noble metals and so on.

DR. POWERS: It is the noble part. It is appropriate.

MR. BOYACK: I don't think I am going to call in.

DR. POWERS: It only leads to dissention, right?

MR. BOYACK: Okay. Well, how easy can it be? There is only a couple of numbers. Things change, right? Okay. I see, that you have moved to the last of the stack.

DR. POWERS: Oh, I have been demoted.

MR. BOYACK: No, that means that you get to really just cap off the discussions and summarize it and make sure that everybody else has got it right.

DR. POWERS: You have to understand his personality. He likes to take potshots at everybody that has gone before.

DR. KRESS: I love it.

MR. BOYACK: Okay. Jim. Release for barium strontium for high burn up fuel.

MR. GLESEKE: I think I am going to have to leave it real small, I think. Like zero. Zero is a very small number.

MR. BOYACK: So in our little --

DR. POWERS: Say yes.

DR. KRESS: Negative numbers are real small.

DR. POWERS: They are not small.

MR. GLESEKE: They are.

MR. BOYACK: Okay. Do you want me to go through the whole thing, or do you want to come back and take it a phase at a time?

MR. GLESEKE: I am thinking maybe one more. That is kind of what we did with the last one. I think this is going to be up from what we talked

about before. I mean, from the 1465. He looks at Tom's calculations, for instance, with effective burn up. And where his low burn up was .02, and this may be four times that much.

MR. LEAVER: Are you just talking about just due to burn off?

MR. GLESEKE: Yes.

DR. KRESS: And you had better correct that to fuel that has high burn off.

MR. GLESEKE: yes, maybe you need to back that down some because of the percent of the amount of fuel, and --

DR. KRESS: I know that when I make that correction that it takes it down to six, .06.

MR. GLESEKE: Yes. I was going to say .05, and .06, that's about right.

MR. BOYACK: So, 6 percent. Okay. So let's go back to Dana then.

DR. POWERS: Zero is applicable.

MR. BOYACK: Okay. Ont he early in-vessel?

DR. POWERS: I believe there are two competing factors that are arising here. When you raise the diffusion coefficient for post-strontium and barium, but you increase the oxygen potential and that

has the effect of decreasing the volatility of barium and strontium.

So I don't see a bases for changing the value. I see the need for experimental investigations of high burn up fuel.

MR. BOYACK: Okay. So the first is the increase in diffusion coefficient for these species, and the second is the oxygen potential.

DR. POWERS: Yes, the oxygen potential, and to depress the volatility of both strontium and barium.

MR. BOYACK: Okay. I see no reason for changing -- well, it lacks a basis really to change it.

DR. POWERS: You don't have a bases to change it.

MR. BOYACK: Okay. Let's see.

MR. CLEMENT: Applicable

MR. BOYACK: Sorry?

MR. CLEMENT: Applicable.

MR. BOYACK: Thank you. All right. Okay.

MR. CLEMENT: And barium should be treated in a different way than strontium, because there is experimental evidence that they are not released in the same way with the same amount from the fuel.

DR. KRESS: That's right.

MR. CLEMENT: Barium is much more volatile than strontium from experiments.

MR. BOYACK: Which is much more volatile?

MR. CLEMENT: Barium.

MR. BOYACK: Okay.

DR. POWERS: Even from a chemistry thing, it has to be because the barium is huge, and the strontium just about fits in the vacancies of uranium dioxide.

MR. CLEMENT: If you look at VERCORS and HI/VI experiments, we derive the release of barium from fuel of 50 percent of inventory, and what is true from containment of 10 percent.

MR. BOYACK: Okay. So, this was which?

MR. CLEMENT: From barium, because we observed important deposits above the fuel regions in experiments. PHEBUS doesn't give you the same reasons. PHEBUS gives you very low areas of barium, and we have to take into account HI/VI and VERCORS experiments, and that's why it retains high areas for barium.

MR. LEAVER: You have to take into account HI/VI?

MR. CLEMENT: Yes. HI/VI from that, yes,

from Oak Ridge.

MR. LEAVER: From Oak Ridge.

MR. BOYACK: Well, you did get about twice as much barium released in FPT-1 as you did strontium didn't you?

MR. CLEMENT: If you want to put it as negligible.

MR. BOYACK: Well, I have got some numbers and maybe they are not right. But it is a factor of two is it not?

MR. CLEMENT: Well, it is very small. Okay. For strontium, we get 10 percent release from fuel, and 2 percent through containment.

MR. BOYACK: FPT-1?

MR. CLEMENT: That is the general judgment from all the available data. That is our own definitions, and that is the release from fuel in the --

MR. BOYACK: Ont he barium, was that a 10 percent release of barium from the fuel, and then a 10 percent delivery, or --

MR. CLEMENT: Fifty percent from the fuel.

MR. BOYACK: Okay. So that is what it was.

MR. CLEMENT: And 50 from the fuel and

then 10 from the containment.

MR. BOYACK: Thank you. I had that wrong.

MR. LEAVER: What are these numbers if this is not correct for barium?

MR. CLEMENT: Yes, yes, but they are very small.

MR. LEAVER: For FPT-1, do you have those?

MR. CLEMENT: They are small.

MR. LEAVER: This is the release from containment?

MR. BOYACK: Tom, with what you are doing for this test, what if any of the elements in the middle of sentence, is it capitalized or not?

DR. POWERS: The names are non-capitalized.

DR. KRESS: The names are not capitalized.

MR. BOYACK: Okay. Thank you. Okay. David.

MR. LEAVER: On the barium and strontium, I am just confirming with Bernard that the FPT-1 releases in the containment were of the order of a factor of 5 to 10 less than -- and we are talking about in-vessel release now, early in-vessel, and a factor of 5 to 10, and less than what we have currently in NUREG 1465, which is where we have 2

percent.

For FPT-1 the releases in the containment were tenths of a percent, and noting also TMI was a tenth of a percent, and the SFD-14 was about one percent.

And also noting that to the extent that there is a slight increase in oxygen potential, that would tend to suppress the release of barium and strontium.

So I would say probably we are high. Our 2 percent number is probably high, but I certainly don't see any basis for changing it other than I guess we could lower it, but if we are not in the lowering business, then we would leave it the same.

DR. POWERS: Maybe what you wanted is -- well, it seems like it might be a little high to me, but it is not very much high.

MR. LEAVER: Yes. If it is high, it is probably only a factor of two high.

DR. POWERS: Yes.

MR. LEAVER: So it is not a huge effect.

MR. CLEMENT: At that point, I believe we should note that I believe that * different experimental results.

MR. LEAVER: Between barium and strontium?

MR. CLEMENT: Yes. We have got out of five experiments, there was high release of barium, and you take away the five experiments, and there is no release of barium.

MR. LEAVER: Right.

MR. CLEMENT: And it is worthwhile to say whether we have a need to understand really why.

MR. LEAVER: Right.

MR. CLEMENT: And not to say that I choose one or the other.

MR. LEAVER: No, I think that's right. It has always been in my mind that barium and strontium, that there probably should have been a difference, but I don't think -- you know, we are trying to decide what to do with 1465, and so we could split them up and make them two different numbers.

But I think that the .02 is probably high, and so what we wouldn't want to do is leave strontium the same and raise barium. We might want to reduce strontium and leave barium the same.

But I am not sure. The difference is modest enough that I am not sure that it is worth the trouble of creating an extra fission product group, which is what you would be doing.

DR. POWERS: Well, they have not stayed.

When we get down into noble metals, all of the problems that we will run into is the different elements, because they have unique and different chemistry.

MR. LEAVER: Right.

DR. POWERS: So you are averaging over those things, and you have to bear that in mind. And the penalty that you pay in systems level codes and writing another category is a lot.

So you don't like to -- I mean, we went from essentially noble gases, and iodine, and particles, to this.

DR. KRESS: The question that I would have is at these levels does it make any difference?

MR. CLEMENT: The system that would * for strontium, and also --

DR. POWERS: Originally it was that you would just take the average of them, and --

DR. KRESS: Well, you know, the release rates are -- even if you had them differentiated, they are so low that I don't think that it makes a lot of difference, in terms of consequences.

MR. LEAVER: I think that one perspective on this is that we are sort of trying to do -- I guess get things as right as we can get them based on the

information that we have here.

And I think if we were to present -- if we were to present, or cut this release of strontium in half, that would have a non-trivial effect on -- well, with the strontium, I don't know what percent of the dose is, but in the stuff that we handed out, it is about 10 percent of the dose.

DR. KRESS: Well, the thing about strontium is that it hangs around a long time, and it can have environmental effects, and those are overlooked in things like dose.

MR. LEAVER: Well, maybe -- well, i would say again what I said earlier, that there certainly is some evidence that we maybe overdid it a little bit on strontium, and less than in barium.

But if we were to reduce the strontium, it would have some effect, and that we are trying to get it as right as we can get it, but I think that is probably worth considering.

We seem to be changing things, and there is no rule that says we can only change them in one direction, or at least I haven't heard it.

DR. POWERS: I don't know about that one either.

MR. LEAVER: I think it would be a

significant thing if we were to break up the strontium and barium, but I think that if there is a difference there that we shouldn't just push them into the same number if we don't think that is the right way to do it.

And people who do dose calculations could handle this without any problem. You might need a little reprogramming, but it is not a big deal.

MR. SCHAPEROW: For the record, barium and strontium, or different groups, drafted NUREG 455 in '92.

DR. POWERS: I remember that, yes.

MR. SCHAPEROW: And the strontium was less, and they were merged on the basis of in-close.

DR. POWERS: I think the argument is always an ungrouping, and the argument goes that if there are uncertainties, and the values are as big as the differences, or commensurate with the differences, then that when it comes to official product release --

MR. SCHAPEROW: They are separate and --

MR. LEAVER: Okay. Let's continue on with the discussion, and first with Tom Kress on gap release.

DR. KRESS: The same.

MR. BOYACK: Okay. And then we come down

to the next phase --

DR. KRESS: My fission product release model does have barium and strontium separately, and the strontium gets released less than the barium, but it is about half, and I link them together because half is well within the uncertainties.

So I put them together and the analysis that I get with the model tells me the same thing that Jim did, that it ought to be about 6 percent. I have to qualify that and say that the database that underlines the barium-strontium release does not really have this oxidation potential change in it, because the tests will run in such a way that you wouldn't get this kind of oxidation potential.

So to worry about just a tiny bit about it, and I think that the release of strontium and barium occurs later than a lot of the other materials, and it is during a time when you are reducing environment according to my model.

So I am going to go ahead and stick with the 6 percent.

MR. BOYACK: So did you say you predict the barium-strontium separately?

DR. KRESS: They are close enough together that --

MR. BOYACK: They are close enough together.

DR. POWERS: Tom, your 6 percent is released from the fuel and --

DR. KRESS: The 6 percent is the release of aerosol that transmits all through that, and the 6 percent was the release.

DR. POWERS: So if we assume it was transmitted as an aerosol, would that be consistent with --

MR. GLESEKE: I had mine down to 3 percent.

DR. POWERS: Well, 2 percent and 3 percent look an awful lot alike.

DR. KRESS: I have to admit that I overlooked that in this particular one, and 6 percent is the release from the fuel.

MR. GLESEKE: I had mine down to 3 percent.

DR. KRESS: So that gets me back to David, and 3 percent is a lot like 2 percent. So you might want to adjust that. So, 3 percent is close enough to 2 percent, and in fact if I separated the strontium out it would be 2 percent.

MR. BOYACK: Okay. And so --

MR. GLESEKE: And I didn't put that up here either. I have it here in my calculations.

MR. BOYACK: Okay. So you lose your identity.

MR. GLESEKE: Great.

MR. BOYACK: But that's all we are going to go anyway. And then there was the debate about whether or not to consider to stay combined, which at the present time we will just keep them combined at the two percent?

DR. POWERS: That's right. My argument is that I will intend that Barium is always more volatile than Strontium, and my uncertainly is in the absolute release fraction is so high, and that the difference between the two is that they just don't seem very important at this point.

MR. BOYACK: Okay. All right. Let's see. We now move to the ex-vessel phase. Jim.

MR. GLESEKE: I don't see anything about burn up effects.

MR. BOYACK: Dana.

DR. POWERS: The release of barium and strontium excess was directly proportional to the amount of zirconium metal that comes ex-vessel. If it has impact on that, it will adjust the number. I have

no reason to think there is an impact on that, and so I just stayed with what they have.

MR. BOYACK: Okay. Would you like to have that comment in there?

DR. POWERS: Yes, I think it is important, because they need to understand how high burn up affects the core degradation process, and how much metal you have coming out of this vessel.

MR. BOYACK: I can use it to capture three word phrases, but this one went on a little longer. So, if you would just --

DR. POWERS: Three words? Okay. Release of barium and strontium ex-vessel is proportional to the amount of zirconium metal becomes ex-vessel. If high burn up affects the amount of ex-vessel metal, it will change the release fraction in proportion.

DR. KRESS: High burn up fuel has been in the reactor longer and has had a chance to oxidize.

DR. POWERS: That's right. I mean, that is the potential for a severe accident. In the case of a design basis accident, it is legislated that thou shall not change that number very much.

DR. KRESS: But we are dealing in design basis.

DR. POWERS: Okay. And I don't know how

things are going to go, but what it all says to me is we have got to understand how high burn up fuel degrades if there is a difference. I mean, that is the test.

I have the fundamental doubt about the adequacy of the existing models to high burn up fuel. I think they quantitatively lack consideration of foaming potential.

MR. BOYACK: Okay. Very good. Bernard.

MR. CLEMENT: We agree with Dana that the release of barium and strontium from * depends on the existing amount.

MR. BOYACK: Okay.

MR. CLEMENT: I have got some values from the thermal-dynamic calculations, saying that 10 percent of the strontium present in the core that is directing * , and 2 percent of volume present in the core be *. This is to be applied to our reinspections to full volume, and we release 50 percent and 2 percent of the remaining 50 percent. So, 1 percent of barium.

And for strontium, it is 10 percent of 90 percent remaining, and so 9 percent of strontium. We have got some more thermal dynamic calculations, and this is purely a calculation.

DR. KRESS: Is that not with the Wechel Code?

MR. CLEMENT: The Gemini Code.

MR. GLESEKE: And that is 10 percent of the --

MR. CLEMENT: Ten percent of 90 percent.

MR. GLESEKE: Ninety percent?

MR. CLEMENT: Nine-zero, not 19. So, 9 percent.

MR. BOYACK: You will notice that the job of the court reporter is safe. Are you going to keep your day job?

DR. KRESS: I don't know if you have read any of her transcripts.

DR. POWERS: I think the logic that has been used in the French study and the logic that we used in ours are identical. We just can't get the fractions coming down, that's all. It depends strictly on what you do on the in-vessel.

MR. CLEMENT: Yes.

DR. POWERS: On the ex-vessel, we agree exactly on the physics ex-vessel, and we are all working with the same database, and technology base here. We do use different codes, radically different codes. But the way that we get to our numbers is

about the same.

MR. BOYACK: Okay. David.

MR. LEAVER: There may be an effective burn up, but my instinct is that it is not a large effect, and so I would have no basis for changing the importance of that. If anything, you would think that higher burn-up would tend to oxidize a bit more, and so there would be a little less metal coming down and it might reduce the number, but probably not a big effect.

DR. POWERS: Well, the one thing that you can't imagine is that high burn up increasing the amount of metal available, and it is just very difficult to see that.

MR. BOYACK: Right.

DR. POWERS: But I see the potential of cutting the amount of ex-vessel Tellurium by a factor of two. I mean, it certainly exists.

MR. BOYACK: Of course, you were will remember though that the high burn up in only on a portion of the core. So it is probably no a huge amount. If you predicting 50 percent zinc oxidation, and then you do it with what we call with a high burn up core, where some portion of the core is 65 or gigawatts, I would be surprised if you went from 50

percent oxidation to 75.

DR. POWERS: I can see that.

MR. BOYACK: Well, that's why we need to figure that out.

DR. POWERS: You need to run a test. It won't happen if our candling models are good, and if they are good for everything, then you are right. You can't get up to 75 percent.

I can't imagine it happening if you go to a foaming kind of scenario.

MR. LEAVER: That's a fundamental change, and you don't get into the degradation process.

DR. POWERS: That's right. That's right. And that's why you can't do this calculation. You have to do it experimentally at least once, and see if you need to qualitatively change the codes, and then you let the codes do their thing.

MR. LEAVER: But I think it would tend to promote oxidation as if things kind of stay up there for longer.

DR. POWERS: Well, that's what he does.

MR. LEAVER: Oh, he does, yeah.

DR. POWERS: Actually, what happens is the fuel stays up there all foamed up, and it forces the steam to go where there is a code in the vessel, and

it keep it there so that there are long interaction times.

MR. BOYACK: Okay. Tom, it is your time to wrap it up.

DR. KRESS: I have no basis to change it.

MR. LEAVER: That is a nice succinct way of saying it.

MR. GLESEKE: That's a good way to wrap it up, Tom.

MR. BOYACK: So this stayed at 10 percent. Okay. Laying in vessel. Jim.

MR. GLESEKE: Zero.

MR. BOYACK: Okay. What is the phase that I use here then?

DR. POWERS: Applicable.

MR. BOYACK: I need to use a little more wording.

DR. POWERS: You guys at Los Alamos never get to the point.

MR. BOYACK: Okay. Dana.

DR. POWERS: I can't see anything that raises the volatility of barium and strontium in this system. I see lots of potential for lowering it. It is kind of hard to drop it down below zero.

MR. GLESEKE: My logic exactly.

DR. POWERS: Sigma.

MR. CLEMENT: I mean, there are * on all the different * processes. As you have seen then, we consider elements such as barium and strontium, we see from the experiments that there is rather high deposits above the fuel zone.

So we have material available for resuspension, and * many any late emission. So in that case * . But maybe, for instance, some steam surge, if we adjust the water on the core and things like that, and we think they should be investigated.

MR. LEAVER: Well, a quench test is one of the things --

MR. CLEMENT: Yes, because you release a rather fraction from the fuel, and you deposit a fraction in the * structure of the vessel *

DR. POWERS: And you get high pressure steam.

MR. CLEMENT: And in the containment, you may have mechanisms in terms of quenching that * .

DR. POWERS: Either a high pressure steam or a high pressure hydrogen. Either one of them --

MR. CLEMENT: Yes.

DR. POWERS: -- will drive vaporization. If you have a mechanism for getting that, you can get

it and I think it is something to pay attention to when you go experiments.

MR. BOYACK: Yes.

MR. CLEMENT: So we think we need to --

MR. BOYACK: Okay. So I have entered your information about testing. David.

MR. LEAVER: I would have no change to the NUREG 1465 number.

MR. BOYACK: Thank you.

DR. KRESS: I certainly agree with the French position that these things are stuck on those aerosols, and I know that is chemistry is going to react on the surfaces, and so they could be resuspended if you have ways to do it, and they could be revolatilized if you have got hydrogen or steam going hot enough.

At the moment I have no basis for evaluating any of that. So I have no basis for changing the number, although I think those need to be investigated.

DR. POWERS: Hydrogen suppresses the volatility.

DR. KRESS: Air ingression --

DR. POWERS: Hydrogen or steam could jack it up.

DR. KRESS: Yes.

DR. POWERS: But what most likely happen is that it will just react further and bury itself down in the air and oxide as barium and strontium flow rate. All that work to leave the number alone. No fun at all.

(Discussion off the record.)

MR. BOYACK: Okay. All right. Dana, are you ready to go?

DR. POWERS: Okay. We are going to take it as applicable here, and it is not zero, but it is so small that at this stage of resolution you might as well just call it zero.

MR. BOYACK: Dana, do you want to continue on?

DR. POWERS: Sure. And in here we come to the trouble with grouping. Within this category we have ruthenium, molybdenum, and palladium.

DR. KRESS: And technetium.

DR. POWERS: And technetium. And technetium by everything that I have been able to do is a relatively non-volatile material. And because our oxygen potential is in the fuel is going higher is a relatively volatile material.

And you will see from the PHEBUS results

that you get movement of ruthenium even though they are not particularly strong in oxidizing environments.

So you have got to kind of mentally integrate all those findings together for this category. Based on doing that, and recognizing that in the piping system that you have fundamentally different processes, and you have molybdenum moving along as the trioxide or the cesium molybdate.

And ruthenium coming out as an oxide vapor, and then getting reduced down to a metallic particle. So it is an aerosol physics. You have got some substantial integration of the two.

But I would say that the preponderance of information coming out now suggests that the value that we have is too small in this category. That when we are talking about a quarter of a percent here.

And based on that, I propose to raise that up to about one percent.

MR. BOYACK: Okay. If you are ready to go, I am going to take you right through them.

DR. POWERS: Okay. The one element that we do an atrocious job in the Vanessa modeling to monitor the release is ruthenium, and universally under-predicted. I think we would do better for molybdenum and the palladium metals and what not.

But when we have looked particularly at the results from Argon on the experiments, we just underpredict there. Ruthenium releases by about 3 orders of magnitude and what not.

I think the people and the panel that we are doing this evaluation are aware of that under-prediction, and so I suspect that the number actually reflects that. So I am not going to change it at this point.

We are going to learn from the experiments and other sorts that we actually get release and movement of the use of materials when they are deposited on the primary piping system, but they are susceptible to volatilization in the oxidizing environment that exists in the laid in vessel.

And so I am going to propose that we have about a four percent revaporization of these materials.

DR. KRESS: Four percent of what was on there?

DR. POWERS: Yes.

DR. KRESS: Which was one percent?

DR. POWERS: No, no. It is more like 10 percent of releases, and I only got about 2 percent through, and so I have about 8 percent of the

inventory. So I am going to get half of that off.

MR. BOYACK: I am not sure that I got enough of that.

DR. POWERS: I think you got enough.

MR. BOYACK: Okay.

MR. LEAVER: Just a second. I was going to clarify. You are saying that the retention of this group in the RCS is of the order of 80 percent, aerosol deposition?

DR. POWERS: It is a combination of vapor and aerosol, both.

MR. LEAVER: Some vapor condensation?

DR. POWERS: Yes. The palladium is moving around as the trioxide and as a cesium update. The ruthenium probably moves around -- it comes off as one oxide, dioxide, trioxide, and even the cesium luthinate.

And as soon as it gets the opportunity and converts into a metallic particle and deposits as an aerosol at that point, and it is reasonably efficient.

I mean, I suspect that Bernard is going to show you some stuff about it from around the top of the bundle and things like that. And so I think that there is a lot of deposition that goes on here.

MR. BOYACK: Okay. And do your numbers

add up? To get a one percent release to containment, you would need if you had a 10 percent release of the fuel, you would need 90 percent retention.

And if you had a 5 percent release of the fuel, it would be -- well, somehow you want to end up with one percent.

DR. POWERS: If I said one percent, I should have said two percent.

MR. BOYACK: Would that change any of the figures or just the comment?

DR. POWERS: Well, I should have said 2 percent and these fractions should be raised to 2 percent.

MR. BOYACK: Okay. Thank you. Okay. Bernard.

MR. CLEMENT: Okay. The first one, the *.

MR. BOYACK: Okay.

MR. CLEMENT: The second one is that we have a problem of grouping. There is a problem of grouping here. * probably find this very volatile.

MR. BOYACK: And those figures were for what, molybdenum?

MR. CLEMENT: So the figures we got from experimental -- is for molybdenum, and 90 percent release from * and 70 percent from containment, and

the same for --

MR. LEAVER: And the FPT-1?

MR. CLEMENT: It is mainly * . And the same for technetium. For rhodium, 10 percent from * and 2 percent to containment.

MR. BOYACK: That's your severe accident?

MR. CLEMENT: Yes. Rhodium seems to be more released than the ruthenium.

MR. BOYACK: What was that last --

MR. CLEMENT: Rhodium. And 30 percent from fuel, and a total of 6 percent to containment.

DR. POWERS: It does not get industrial use as much as some of the others.

MR. CLEMENT: I think --

DR. POWERS: It is break time.

MR. BOYACK: Okay. So you are again showing these problems -- which is then referred to as trying to deal with -- but having a significant difference.

MR. CLEMENT: Yes, there are significant differentials in * and also in the consequences for each agreement.

DR. POWERS: The technetium not the biggest problem.

MR. CLEMENT: No, the technetium is not

the problem.

DR. POWERS: It is pretty high. But palladium isn't very much of a problem.

MR. LEAVER: Not too much.

DR. POWERS: But ruthenium is a pole buster.

MR. LEAVER: Yes.

MR. BOYACK: Okay. For the ex-vessel.

MR. CLEMENT: Looking for the ex-vessel for what we consider exists or that * . Except that our calculations indicate that we will be negligible, and so for the time being there is on reason to change your values.

MR. BOYACK: Okay. And finally the ex-vessel.

MR. CLEMENT: Okay. There is again this same problem with the reauthorization, suspension, and so on. As I stated for the previous group, and I have stated here, we have a lack of data and a need for experiments. Just because *

DR. POWERS: Yes, that is absolutely correct. What you are saying is that with experimental we know that we can get it off, and we know that we could get it on, and it would be nice to know whether it comes back off again.

MR. BOYACK: So you know that it is created and deposited. Not created, but --

DR. POWERS: Released.

MR. BOYACK: Released. I think after we get through with this, we will take a break. David.

MR. LEAVER: I really think that this is a case where the right thing to do is to split this group up, because I think the numbers are substantially different for molybdenum and technetium and for ruthenium. Those being -- well, I don't know how important rhodium is.

DR. POWERS: It is about seven --

MR. LEAVER: Do you have data for rhodium?

MR. CLEMENT: No.

MR. LEAVER: I didn't think so. The FPT-1 really does show a significant higher release fraction for -- and release to containment for technetium and moly. So I think it is difficult to -- I think you can justify using the 1465 number for earlier vessels.

MR. BOYACK: Okay. You are in a different category than I am.

MR. LEAVER: I have no change on the gap. Sorry. Ruthenium, I guess I don't see a basis for changing the ruthenium number. If you look at FPT-1, it was .005, and the 1465 number is half of that.

So, I guess the factor of two is within our uncertainty. I suppose that you could double it. But I think I would say that the ruthenium number is pretty close.

MR. CLEMENT: For which that is containment.

MR. LEAVER: And that is .005. I note that again for what it is worth, I tend to -- I'm sorry, go ahead.

MR. CLEMENT: Okay. We have a high value for containment because you consider it a hot leg breaks, and the --

MR. LEAVER: Well, the hot leg breaks are part of it, but certainly we need to balance that with as we said before different grid locations and sizing. So I think for ruthenium that I wouldn't have a complete basis for changing what is in 1465.

But I would suggest that we recognize moly and technetium, and I don't have any data for rhodium, and so I am not sure where it fits, as being different, and as being more volatile. And I think a number like maybe 5 percent for those, and create a new group.

MR. CLEMENT: That means that it is not worthwhile because * .

MR. LEAVER: Well, I don't know. You don't have any data on rhodium, and so I am not sure what --

MR. CLEMENT: * .

MR. LEAVER: And I don't know how important rhodium is radiologically. I just don't have that information with me.

DR. POWERS: My recollection is that it is not wildly different from ruthenium.

MR. LEAVER: I suspect that if it was a substantial release fraction, you probably would have --

MR. CLEMENT: But I think that what is most important and maybe a radiological * rhodium is -- ruthenium. That means in fact what are the radiological consequences in times of release of * is important.

MR. BOYACK: You could keep rhodium with you.

MR. CLEMENT: Yes.

MR. LEAVER: And also I am just going to add that once could certainly debate what the right release faction and containment is for technetium and moly. I would be tempered somewhat in this judgment again by the TMI accident, and just sort of bear that

in mind.

And that is perhaps why I sort of arrived at a number like 5 percent. It is kind of an in between kind of number. It is not quite as high as was observed with PHEBUS, but it is a lot higher than what is in NUREG 1465 today.

MR. BOYACK: So your 5 percent -- is 2 percent?

MR. LEAVER: right.

MR. BOYACK: And that's because it was splitting?

MR. LEAVER: Right.

DR. POWERS: I have integrated them together and he wants them split.

DR. POWERS: Okay.

MR. BOYACK: Okay.

MR. LEAVER: Again, just recognizing that there seems to be something fundamentally different going on, which I think we understand perhaps a little bit, because the release fractions are greatly different.

MR. BOYACK: Okay. Let's see. The ex-vessel. I am not sure about that. I guess I am not sure why would we not see the same effects going on ex-vessel for technetium and moly as we saw in the

core.

DR. POWERS: Keep them because we have got a huge amount of steel that keeps them in a metallic state.

MR. LEAVER: In the ex-vessel?

DR. POWERS: Yes. And when we tend to have a model, we calculated it and it was truly negligible. What we left out of the database didn't include cesium ruthenate, and cesium -- or the corresponding sodium and potassium.

So when -- they did the experiments up in Argon, where they delivered -- they put these into the charge that we never did and --

MR. LEAVER: That's because you were in New Mexico.

DR. POWERS: Yes, and they actually got some release. They got releases up around -- well, maybe around the one percent level, and maybe a little less than that. And then we just don't calculate with the codes, because we have got it all down on the metal and keep it producing and release into the environment.

And the rutheniates and -- have just enough vapor pressure to give you enough release protection, and I think the number on the table

actually reflects that, because if you had just gone on calculations, it would have been a release fraction of 10 to the minus 6 or something.

MR. LEAVER: Well, I would say that is a little more detailed explanation than what you said when you dictated yours, and so I would say let's leave the number the same. I have no basis for changing it in light of that comment.

MR. BOYACK: Okay. In-vessel.

MR. LEAVER: No change.

MR. BOYACK: Okay. Tom.

DR. KRESS: Well, I guess I have to add comments or dissenting opinions on this one. This is a place where I think we have assented out of the sublime in to the ridiculous, and we need a paradigm shift.

The ability to predict things at this level is just not there. You can't do it, and to put the differentiation between noble metals, and cerium, and lanthanides, and to try and differentiate between all those things is just not a thing to do for design basis accidents.

And those numbers up there that are already there have no basis in fact. They are just put up there --

DR. POWERS: Are you talking about noble metals?

DR. KRESS: Yes, all of them. Noble metals, cerium, and lanthanides. What I think happens is that you get some -- and especially with high burn up fuel, you get the potential for some decrepitation of the fuel itself because of the grain structure.

And even in ex-vessel core concrete interactions, you get bubbles splattering off the fuel, and you get the fuel itself transmitting. So there is a minimum level of release that just goes with the fuel itself. This doesn't have anything to do with vaporization or chemistry, or anything.

And what you do is you just put a ground floor on the release of those things, and I would put it at about one percent to two percent levels, which is what I think those numbers come from in the first place, both ex-vessel and in-vessel.

And I would lump all of them, but I didn't bother putting them in my model because of this. The noble metals and cerium, and the lanthanides, and so I think the release mechanisms are just different for those, and they have to do with the way that fuel gets kicked off of its -- the grains get kicked off the fuel.

And I would just stick them all the same, both ex-vessel and in-vessel, and I would make them all about 2 percent or something.

MR. BOYACK: Okay. So on the gap --

DR. KRESS: Nothing on the gap. There is nothing there. I am talking about the early in-vessel and the ex-vessel.

MR. BOYACK: So, the --

DR. KRESS: I would leave them all about .002, and I get rid of that damn five, and do it on both ex-vessel and in-vessel, and all the noble metals, I would group them all together, and I would also group the cerium and the lanthanides, in with that.

MR. BOYACK: And do you see the need for testing?

DR. KRESS: Well, always. I have never argued against more testing.

MR. BOYACK: I guess it is hard to argue against testing. So you would see then another release from down here at 0.2 percent time; is that what you told me?

DR. KRESS: Yes, something like that.

MR. GLESEKE: Two percent wasn't it?

DR. KRESS: No, no, .2.

MR. BOYACK: Let's see. And where did you get the .2?

DR. KRESS: It was just pulled out of the hat, out of the air.

MR. BOYACK: No, what is that for?

DR. KRESS: For all of it. Just for ex-vessel and in-vessel.

MR. BOYACK: For each one?

DR. KRESS: For each one, and I think if I look at the amount of uranium that gets transmitted in in-pile tests, and in the tests that we do with the annealing fuel, it would be about -- it would give you about that level of release just from the fuel itself. There is no vaporization, and nothing. Just fuel particles.

MR. LEAVER: But that's almost like staying in the fuel. I mean, the stuff just falls down.

DR. KRESS: No, no.

MR. LEAVER: Well, where does it go?

DR. KRESS: It is grains, and these grains are very small, almost like an aerosol with a --

MR. LEAVER: When you say very small, you are talking what?

DR. KRESS: Microns. Aerosols.

MR. LEAVER: How many -- well, aerosol-sized?

DR. KRESS: Yes, 4 or 5, to 10.

MR. LEAVER: Well, with 10s, you are getting big.

DR. KRESS: Well, I mean, it is aerosol size. But you can deal with that later. So you just lump it in with the aerosol. And what happens with the core concrete, is that your bubbles of CO2 comes up and breaks at the surface and just kicks fuel up.

And here is some fraction of those that are real small particles compared to the aerosols, and it is about the same level. I have no basis for predicting how many, and so I just put a floor on it, and say it is about that many.

DR. POWERS: Right.

MR. BOYACK: Laid in-vessel. Did I miss something, or was that --

DR. KRESS: Laid in vessel and it is such a small amount that it is nothing even worrying about, and I just would give it at zero. But I think it jus travels like an aerosol, and half of it gets re-released.

MR. BOYACK: All right. Okay. Jim, it is

your cast.

MR. LEAVER: Can I ask Tom a question?

MR. BOYACK: Okay. Go ahead.

MR. LEAVER: How do we account -- you are looking at low percent release data from certain experiments, and Tom is saying that mechanical release is an aerosol from the fuel at a low percentage, fractional percentages. And the French data show 90 percent molybdenum in the fuel.

DR. KRESS: That has to be a vaporization process.

MR. GLESEKE: That's right. That's right, but where does the two glide between .2 and 90, and when we are looking at the data here --

DR. POWERS: The French are right.

MR. GLESEKE: What?

DR. POWERS: The French are right. I agree with Tom. When you get down into the cerium and lanthanides, I am sympathetic to the point where you -- well, we can measure, and we are measuring, and we are seeing differences from what we presumed with our models.

And here is a pox on your houses, and make it a constant level, and I don't think that is a bad view for the cerium and lanthanides. But I think the

noble metals we can do things with, and it is measurable, and it looks like it is actually important in discussing things like cesium, which are very important.

MR. CLEMENT: Even for the cerium, if we look at measurements, for instance, of neptunium and plutonium, you can see that there is a lot of difference between the reasons that neptunium is much volatile than plutonium, and this is actually measured.

So, for instance, you look at neptunium, and plutonium, and uranium. Neptunium is more volatile than uranium, and uranium is more volatile than plutonium, which means that not only they can be made from small grains *, and other mechanisms.

DR. KRESS: I would just trying to add on the others, because the releases are low. Int he case of moly, I may have to retract what I said. I don't like to go in the face of experimental --

DR. POWERS: And what we need to do is pay attention to the noble metals and not -- well, is because of the -- and you really want to know where that ruthenium is, because ruthenium is a bad --

DR. KRESS: And none of my data deals with

air intrusion that I have.

DR. POWERS: And what we never suspected, and I think that it is becoming more and more obvious, and I think it gets worse and worse as you go to high burn out, is that the ruthenium and moly would move around the way we see them.

And I think it is simply because as you burn you are driving that and you are losing the ability to buffer the oxygen potential around and striking that uranium.

And you start getting more oxidizing, and these things start moving around, and it gets worse and worse on you. And you get this, and there is a lot of evidence in getting cesium updates so that you are pulling the moly, as well as pushing it.

And then you say, okay, now what happens if I put this stuff on the primary piping system, and let air into this vessel, and watch them moving around. If it was just moly, I would say, so, but when I look at ruthenium toxicology, and ask guys to do consequences to tell me how that relates to things that I am more familiar with, they come back with a scary moment data.

MR. BOYACK: Did you get your question asked?

MR. GLESEKE: Yes. Okay. Starting with Tom, I will go along with the consensus there.

MR. BOYACK: Okay. Are you going to bring clarify to vessels?

MR. GLESEKE: I'm afraid not. What I see is a wide range of experimental results, and a lot of unknowns in terms of chemistry and phenomena; from aerosols, where there will be a residual aerosol, and all the way down through these last groups.

And I can see Tom's point there, but it looks to me like there is a lot of vaporization, and because of the wide diversity in the experimental numbers, I can't see breaking this into pieces.

And there is so much uncertainty in the numbers that to divide them into separate groups, I think you are kidding yourself no more than you do. So, I would be included to lump them, at least at this point in time until there is more data available, and that we are dealing with a firm basis for splitting them.

And I would go with some mid-range percentages similar to Dana and Dave's numbers on those, maybe 2 to 5 percent early in-vessel.

MR. BOYACK: Okay.

MR. GLESEKE: And small ex-vessel fraction

of a percent here. Maybe a quarter of a percent. And with a laid in-vessel, a few percent again.

DR. POWERS: Four is a good one, right?

MR. GLESEKE: Four is a good number. You can define four as a few.

MR. BOYACK: Can somebody tell me how to make something out of this mess?

DR. POWERS: Just take Jim's numbers.

MR. BOYACK: Okay. What we had talked about was whether to see there was a -- and we talked about majority, and the minority numbers, and there was at most one or two people. And so what about this? Let's see, we have got 2 percent.

MR. LEAVER: Can we have a little discussion on this before we try --

MR. BOYACK: Sure.

MR. LEAVER: Let me just throw this out in light of Jim's and Tom's comments on this. The PHEBIS FPT-1, which seems to be something that people feel is important, the results that we get, and it is new, since 1465, and it is probably the best fission product measurement experiment that we have done.

So we are paying attention to it. I will have some other comments about that later. But there is a factor of 500 difference in their release to

containment for ruthenium versus these apparently more volatile noble metals.

I just don't see how -- what bothers me a little bit here is -- and maybe a lot, is the notion that you are see from moly and technetium higher numbers. So why not just raise the whole group.

But the problem with that is that if you make ruthenium 4 percent or 2 percent, that really is higher than what the data suggests, unless you have some other reason for thinking that.

And how you will have a pretty significant impact on dose, and not that that is not a reason to do it, but I think we need to be sure that we know what we are doing if we are going to do that, and I just don't think that is --

MR. CLEMENT: Well, if it is a question of the volatility of moly, the * other -- out of other experiments is the first point; and the second point is that it is quite logical --

MR. LEAVER: Right. It is logical. You raise the outside potential that --

MR. CLEMENT: And higher volatility, and I think it is clear that it is much more volatile.

MR. LEAVER: Especially with a little bit higher oxygen potential.

MR. CLEMENT: Yes, of course.

MR. LEAVER: So there is a reason for that.

MR. CLEMENT: And the radiological consequences of moly and oxygen is not at all the same as for --

MR. LEAVER: And you need even higher oxygen potential to see the same effect --

MR. CLEMENT: It is a fact that radiological consequences are not all the same. There is other reasons for separating molybdenum from ruthenium, and the radiological consequences we don't care so much about moly. But for ruthenium, we care very much.

MR. LEAVER: Right.

MR. GLESEKE: They do have 2 percent of the containment on ruthenium

MR. LEAVER: Let's see. Half-a-percent is what I had.

MR. CLEMENT: What we give as a number I am just entering as a containment. It is not the number from one single experiment. It is a number derived from the FPT-1 experiment and other experiments, and our own experiments -- and this is just one piece of the feather.

MR. GLESEKE: Oh, okay.

MR. LEAVER: I was just talking about the H1/V1 number. The number that I think Bernard was talking about was kind of a regulatory conglomeration.

MR. GLESEKE: There were more experiments than one.

MR. LEAVER: Yes.

MR. GLESEKE: Well, yes, but FPT-1 to me is probably more meaningful numbers for our purposes. Yes, it is .005, and I am not sure whether that is a basis for changing a .0025 number. It is within a factor of two, and so how significant is that relative to our uncertainty.

That is how I arrived at keeping it the same, but I think the factor of 500 really to me is -- and in effect as Bernard points out, there is some physical basis, chemical basis, for why we think the moly is going to be more volatile.

And breaking them up does give you a more adequate picture, and in this case it is probably worth a significant enough effect that it is worth doing.

MR. BOYACK: Maybe the first question that we have to deal with is literally this question of breaking up the groups, and I am not exactly sure how

one resolves this, except to look at the users. My users are the NRC, and Charlie, any comment?

MR. TINKLER: Yes. There is a way to compromise on this, and that would be to reflect the much higher volatility of some of the noble or what we once called, and still may be called, transition metals.

And then retain the other radionuclides that we have separated into separate groups, and then we combine those. I mean, we could reflect a different release fraction for lanthanides, and then combine lanthanide and plutonium, and all the cerium and lanthanites, into one group.

I know that is another option. If we think that ruthenium has a higher volatility as is reflected in the vaporization phenomena, and you separate that out, and then we combine all the others because we think they are perhaps at least to a large degree mechanically -- due to decrepitation and other factors.

And as far as the NRC is concerned, we don't have a view that thou shalt retain these particular groups, and historically the original 1465 panel struggled over this question a little bit on ruthenium versus Lanthanidium.

The large difference in radiological effects is clearly one of the reasons why you might want to make a distinction between .2 percent and 5, or 10 percent, or whatever you decide, for anthanithium. I mean, there is a large difference there.

MR. CLEMENT: I agree with you for a separation of them and other groupings, but maybe we have some * . For instance, if you look at the VERCORS --

MR. LEAVER: Can we take a short break?

MR. BOYACK: In just one minute.

MR. CLEMENT: Okay. If we look at the results from the tests, for instance, on neptunium, and uranium, and plutonium. We have got here 6 percent neptunium, and 2 percent uranium, and .2 percent plutonium. So, exactly one decay between these three elements. So we are against them because they are a grouping.

MR. TINKLER: Yes, I understand, but in this case, I would attach a little more significance to some of the integral data when we are talking about releases to containment, and for the less volatile stuff, if it migrates a little bit to the upper part of the bundle, well, that is important.

And it reveals a difference, but as far as my release -- to defining my release to containment, I guess I am not sure that I can reflect that level of discernment in the table

MR. BOYACK: Please come to the mike.

MS. MITCHELL: Jocelyn Mitchell. I wanted to speak from the point of view of off-site consequence calculations. We are preparing to be able to put into the off-site consequences uncertainty distributions, and if you see that different chemical elements have a more uncertainty, or others have less uncertainty, you might want to divide them.

Maybe they are the 50th percentile, or their mean value might be the same. But if they are big uncertainty differences, you might want to have the ability to keep them separate so that when you get to the off-site consequences that you can put in different uncertainty distributions on them.

MR. BOYACK: I have had multiple requests for a break now. Let's take 10 minutes and come back at 11:00 and try to work through these.

(Whereupon, at 10:50 a.m., the panel meeting was recessed, and resumed at 11:04 a.m.)

MR. BOYACK: Let's consider a little bit about where we are and where we need to go before the

end of the morning. We have in my view maybe until 12:30, and then a very short lunch -- a quick run down and a quick run back, and then to go ahead and cover two more areas.

DR. KRESS: Bring our lunch back with us?

MR. BOYACK: We could do that, sure.

(Discussion off the record.)

MR. BOYACK: All right. Given all of that, we have had some major issues that have come up since we dealt with this noble metals group, and it seems clear to me that we are not going to be able to deal with them in the course of this meeting, and then also cover the rest of the items.

These primarily have to do with whether or not to separate and reform the groups. And then even within that, there may be some differences in the values.

Now, I guess if we reform the groups that maybe some of those differences would narrow, but I don't think we can do that within this time frame that we have.

What I would suggest is that we go on to the next two, and that is that we don't firm the values up right now. What we do is go on and have the dialogue about the next two groups, and record that

information.

And then between now and the next meeting, try to figure out what to do with it. So that is my position. Let's see. Bernard, did you end up with the overhead that had our groups?

MR. CLEMENT: Sorry, no.

MR. BOYACK: IT is the one that we were marking up with the changes in values.

MR. LEAVER: Yes, you do.

DR. POWERS: There you go. The third group.

MR. CLEMENT: Oh, sorry.

MR. BOYACK: Maybe I ought to check and see if you have --

DR. KRESS: Maybe he fixed the values up.

DR. POWERS: He corrected them. He corrected them. He got them right.

MR. BOYACK: David, you started to make a comment before? Were you just going to make a comment or just put up the slide?

MR. LEAVER: I was just going to put the slide up.

MR. BOYACK: Okay. Is there any disagreement or other perspectives on continuing? So what we have to do is -- well, just so there is no

question about that value.

So when you get your copies, which you will be provided, and I think before you leave today, you will be able to get those to you. All right. So that moves us on to the cerium group.

(Discussion off the record.)

MR. BOYACK: So now we are moving to the cerium group. So with that in mind, I wonder, Bernard, if you would be willing to provide your knowledge on the cerium group.

MR. CLEMENT: Okay. So we start with neptunium, and first of all, there is no reason for changing that.

MR. BOYACK: Thank you. There we go.

MR. CLEMENT: Then neptunium for our measurements, and mainly from VERCORS and also from PHEBIS. We derived the value of 10 percent release from cerium, and 2 percent from *.

Okay. For plutonium, it is one percent from * and 2 percent to the containments. For cerium --

MR. BOYACK: And what is cerium?

MR. CLEMENT: Cerium? CE.

MR. BOYACK: Thank you.

MR. CLEMENT: We did not have direct

measurements, but * rather than with other cerium, and I will speak later on on lanthanum, but from this analogy, we come out with a figure of 10 percent from fuel, and 2 percent from containment.

MR. BOYACK: That was plutonium?

MR. CLEMENT: That was cerium. It is the measurement of lanthanum in VERCORS, and an analogy between lanthanum and cerium.

MR. BOYACK: Anything else?

MR. CLEMENT: No.

MR. BOYACK: Okay. Then as we go along to the ex-vessel.

MR. CLEMENT: Okay. We don't -- well, no. We don't think we will have a significant release from an MCCI.

MR. BOYACK: Okay. And the late in-vessel.

MR. CLEMENT: The late in-vessel is the same remark as for various groups. As far as we have important deposits, and species whose volatilities can change, depending on the oxidation degree and so on. And we need more experiment on that to reduce the * on these processes.

MR. BOYACK: Okay. Thank you. I am assuming that everybody has read NUREG 1465 values?

DR. KRESS: I think everyone has.

MR. BOYACK: Okay.

DR. POWERS: You had better check that with the guys from LSU. They seem to do better on these things. Okay, Dana.

MR. CLEMENT: I looked at * that maybe in high burn up fuels and so I am going to * of these groups.

MR. BOYACK: Okay. So basically --

MR. CLEMENT: That is not easy to quantify now, but we know that the morphology of the fuel has high burn up will not be the same, okay? So the access from oxygen from the steam to the fuel, or from the fuel to the steam, may really change, and we know that the volatility of species, depending upon their oxidation state -- for instance, the volatility of species for plutonium is plutonium dioxide, and so it has to go to dioxide and monoxide.

For uranium, it is thee oxides, and so it is dioxide and trioxide. And then we need to have more on the effect of morphology of the high burn up fuel on the release of these species.

MR. BOYACK: Okay. Thank you. All right. David.

MR. LEAVER: Well, the PHEBUS -- the FPT-1

release of plutonium was somewhat less than 1465, the 1465 number for VERCORS for the cerium group, and the neptunium was somewhat higher.

So I guess I think -- I have -- these numbers are all so small that I just don't really know -- I don't feel too comfortable throwing numbers around because they are all so small.

But I think qualitatively that the neptunium number from FPT-1 would make me want to think that perhaps we are a little low on 1465, even though the plutonium release is less than what we have for 1465.

So I maybe would double that number just to reflect that that FPT-1 data, and make it 10 percent. And I would have no basis for changing the ex-vessel. I'm sorry, 0.1 percent.

And that was ex-vessel. Was there any comment on any question about the vessels? I would say it is applicable.

MR. BOYACK: Okay.

MR. LEAVER: Thank you.

DR. KRESS: The high releases of neptunium and ruthenium even in the VERCORS experiments gives me room for pause, because 2 percent would be a significant biological effect in a design basis space.

MR. LEAVER: You are talking about ruthenium?

DR. KRESS: Yes, and I am talking about neptunium also. That has a severe biological effect. Cerium also does. So, I worry about those numbers.

I still believe that all the other things ought to be capped at something like one-tenth of a percent, but I am about to change my mind about neptunium, and ruthenium, and cerium, and we might want to separate those out as separate species, because each of them have biological effects that are severe, and apparently there is experimental evidence to think that the releases might be in the order of magnitude more than the .1 percent.

So I would kind of focus on those, and I say that we need to understand and have more experimental data to understand the PHEBUS results and the VERCORS results.

But right now I would still say that until we get that data I think I would still look for a cap on all of those at .1 percent. I don't think I can differentiate between .1 and .0025.

So I am sticking with my number, just .1 percent across the board right now, and with the qualifier that I think we need to go back and recheck

the results for neptunium, cerium, and ruthenium. Those are the ones that I worry about.

MR. BOYACK: And that is because of the health effects?

DR. KRESS: Yes, and I am not sure that my .1 percent properly captures the results of the VERCORS in places.

MR. BOYACK: Okay. And the next vessel?

DR. KRESS: The same, .1 percent.

MR. BOYACK: And the laid in vessel?

DR. KRESS: I don't have any reason to worry with changing.

MR. BOYACK: Okay. Jim.

MR. GLESEKE: Well, for not necessarily all the same reasons, I am going to go along with the numbers that Tom has here. I would think it would be important to point out that the neptunium numbers from the French experience may be a big deal if they are closer to right than the lower numbers. And we are looking at 2 percent for the containment, and that would be a significant deal. I would be inclined to go with Tom's numbers on this one for the time being.

MR. BOYACK: Let's see. I am not sure what those are.

MR. GLESEKE: About a tenth of a percent

in the in-vessel.

MR. BOYACK: And for the same reason, the health effects?

MR. GLESEKE: Yes.

MR. BOYACK: And the ex-vessel?

MR. GLESEKE: That can be a tenth of a percent also for NUREG 1465.

MR. BOYACK: Okay. I would like to introduce the clean up man for the survey group now.

DR. POWERS: Over the last decade there has been quite a lot of work noted at the national laboratory in the high country in the southwest by an investigator named Oscar Krikorian, looking at the vaporization of plutonium at relatively modest temperatures.

And this has been an effort that he has worked on for at least a decade, and maybe a decade-and-a-half. And she has indeed found that within water vapor there are vapor species for plutonium that will lead to lower temperature vaporization.

And I think this lends credence to the assessment that the French have made for plutonium that is quite at odds with the thinking at the time of the source to include package and the time of the development of 1465.

And I think we just have to say that here we have two lines of investigation, totally independent, that are leading to a similar result that says indeed under these accident conditions you can get non-trace releases of some of these elements.

I have conceptual difficulties with estimating cerium release based on an analogy to lanthanum, and I just basically wouldn't do it. I think a better analogy is between cerium and plutonium.

All together, that leads me to be very supportive of the idea of an overall 2 percent in-vessel release of the cerium group to the containment.

The results they have obtained from the neptunium I find remarkable. I don't doubt them, but the neptunium chemistry is an area that I just am not very familiar with.

I did an assessment in connection with the development of the Vanessa Code, and my recollection is that indeed the thermal dynamic data would support a higher volatility for neptunium.

Currently, my mental integration, biases, thinks heavily toward the plutonium because I think that Krikorian is giving us a much richer understanding of the vapor chemistry of plutonium that

would have us say, yes, this is not an irrational -- it is not a fluke of some particular experimental thing, or that you should get some substantial release.

So I am supportive of adopting plutonium release as representative of the cerium group.

MR. LEAVER: Plutonium release for PHEBUS or --

DR. POWERS: When they did their assessment, PHEBUS is one point in space. They have these VERCORS tests which -- and I have been able to examine them only to the extent of view graphs being put up on the viewing table.

And it would be nice to have a close examination of these tests, because there are things that go up and down in them that I don't quite understand.

Again, I doubt them, and our history of these out of pile tests is that there are things that we don't understand about them. But you also see some consistent trends.

And I also know that the French, when they make these assessments, use heavy use of the Gemini code in developing an understanding. I have spent innumerable hours with some of the originators of this

code name, and generally appreciate its quality that they have exacted on that.

I state again that frequently we find ourselves in completely -- in harsh debate, but I think they have found something. Furthermore, it seems to me that these trends that they observed and their experiments to date, that it only gets excesuated as we move toward high burn up fuels, where there is the propensity to have somewhat more oxidizing conditions in the fuel.

And as Bernard points out, indeed even greater access of oxidizing gases to the microstructure. So I am saying let's accept what you have done for plutonium as representative, and flag it, and say let's look further at this neptunium business, because Tom tells me that neptunium is a real bad actor.

And cerium I know has the -- is our decay heat carrier for the long duration. So we need to pay attention to what it is doing just as a heat source.

The French have taken a roughly 80 percent deposition along the release path, which I think these materials are transmitted primarily in the aerosol form, and if we consistently looked at what we did with aerosol, we basically took a 50 percent

deposition.

And I am not going to go into that kind of detail. I am just going to be supportive of the 2 percent to containment.

MR. GLESEKE: I think they have a tenth of a percent of plutonium load, Dana, and not to quibble over numbers.

MR. CLEMENT: Yes, you always take * deposition, and just because when the contents just above the fuel, there is not so much aerosol in which they can *. So the lowest volatility of elements, the highest will be the position by vapor condensation on the walls.

If you take a highly volatile element, and the condensation will find a huge population of aerosols to be transported. That is the reason. That is also supported by measurements of deposits in the upper part above the * section and the * section, and that is the idea.

And concerning cerium, I don't remember if I have ever known why people have made the analogy with *, and they were probably some reasons that I don't know.

DR. POWERS: Well, the basis for doing it is that the predominant vapor species from lanthanum

sesquioxide is LAO, and the predominant vapor species from cerium is CEO, and they are very similar in their properties, and that's how they would do it.

MR. CLEMENT: In VI-5, there was a measurement of 2 percent of the release of cerium, one for one, and that is the only value that we have got as a measurement.

DR. POWERS: Well, my bottom line is that it is 2 percent to the containment.

MR. GLESEKE: Now, that's a neptunium number?

DR. POWERS: Yes, 2 percent to the containment.

MR. BOYACK: And the name of the researcher, was that Krikorian?

DR. POWERS: Krikorian, yes; K-R-I-K-O-R-I-A-N.

MR. BOYACK: I was distracted by somebody speaking here. Could you give it to me one more time?

DR. POWERS: K-R-I-K-O-R-I-A-N. The noted Lassel (phonetic).

MR. BOYACK: Oh, I did want to get that in.

DR. POWERS: Yes, I figured you did.

MR. BOYACK: I assumed that may be what

was going on when it was an unnamed lab in the southwest..

DR. POWERS: Well, no. A famous laboratory in the high country of the American Southwest.

MR. BOYACK: And the ex-vessel.

DR. POWERS: The ex-vessel number here again is composed of two components. One component is a mechanical release due to the bubbling, the bubble bursting at the surface, and then another component that is a vaporization release that is directly proportional to the amount of zirconium present.

If that amount of zirconium metal present is changed radically by going to high burn up fuel, you will see some change in the cerium release, but at this point I am comfortable with the numbers that exist.

MR. BOYACK: Okay.

DR. POWERS: And I think it is .5 percent.

MR. BOYACK: Oh, I'm sorry. I was just looking at the other values.

DR. POWERS: No, .5 percent.

DR. KRESS: .5 percent.

MR. BOYACK: .5. I've got it. Thank you.

DR. POWERS: And for the late in-vessel

release, I think any of this is a resuspension release. I don't see a vaporization route here, and our understanding of resuspension hinges very much on physical form on the surface.

I think that these will be among the most resuspendable of the fission products that drop on the surface. But right now I think that our assessment of that suspension is that it is small.

MR. BOYACK: Like?

DR. POWERS: Like zero. I would not change the existing value.

DR. KRESS: Way to go.

MR. BOYACK: Way to go. Pardon me while I try to find out where I am. Oh, there it is.

DR. POWERS: These guys up at Los Alamos, they lose tape drives, and they lose -- they just can't find things, you know.

MR. BOYACK: It is our way of keeping --

DR. KRESS: Look behind the xerox machine..

DR. POWERS: The xerox machine.

MR. BOYACK: It is an employment thing that we engender for the NRC, or not for the NRC, but for the FBI and other security forces.

DR. POWERS: Security forces?

MR. BOYACK: Yes. And I think thinking, David, because you get to lead off on this.

MR. LEAVER: I forgot about lanthanide. I guess I would see no basis for changing the numbers in 1465, and that is based on that the FPT-1 data --

MR. BOYACK: Now, just to make sure where we are, which phase are you in?

MR. LEAVER: Early in vessel, and there is no change on the basis of the FPT-1 data, and very similar results for some of the earlier tests with SFD.

MR. BOYACK: S what?

MR. LEAVER: SFD-14.

MR. BOYACK: Okay. So that is applicable. Ex-vessel.

MR. LEAVER: I would have no basis for anything on NUREG 1465 on that.

MR. BOYACK: And in-vessel?

MR. LEAVER: That is a factor. That is a factor.

MR. BOYACK: And late vessel?

MR. LEAVER: No basis for changing the number.

MR. BOYACK: Okay. Tom. Is there anybody who would change the gap --

DR. KRESS: Hang on. I want to change some things that I have done before, because I was saying a cap of all of these things at .1 percent. I think that is only in the early in-vessel release. I would make all of those .01, or .1 percent.

But for the ex-vessel release, I would keep those at .005, because it is a different mechanism, and it is bubble bursting like Dana said, and it is more like .005. So I would make all three of those at .005, and the ex-vessel, and all three of the early in-vessels at .001. And I would not change the late-endings.

MR. BOYACK: I am just a little slower here, because now we are going back to talking about previous --

DR. KRESS: Yes. I had them all at .001.

MR. BOYACK: So, for the moment here, if you could just give me this entry, and then I will go back and --

DR. KRESS: .001 and .005., no change to the NUREG 1465. I actually think that number was probably based on the bubble bursting in the first place.

DR. POWERS: It has a little bit of a component in the vaporization, or the zirconium metal

that is pleasant.

MR. BOYACK: Now, how many of these are there that we need to go back to?

DR. KRESS: The noble metals and the cerium.

MR. BOYACK: The noble metals and the cerium. Okay. And this was now .1.

DR. KRESS: What are you looking at, at the end-vessel?

MR. BOYACK: I am back at noble metals, in-vessel.

DR. KRESS: Okay. It is .1; and ex-vessel is .005, or .5.

MR. BOYACK: Five percent. I should have been consistent on this, but I wasn't. And then we were all right on the --

DR. KRESS: And the same thing for the cerium.

MR. BOYACK: Okay. So, now let me get this together. Now, for cerium --

DR. KRESS: And I have .1 percent and that is okay, and down there I would use .5.

MR. BOYACK: Five?

DR. KRESS: Yes. Thank you.

MR. BOYACK: That is just part of the

service out at Los Alamos. I am trying to keep my piles straight here. One moment.

(Brief Pause.)

MR. BOYACK: This gave you a lot of time to think, Jim.

MR. GLESEKE: And it should go pretty quickly shouldn't it. Just to inform you of the logic. I used Tom's total numbers here, and --

MR. BOYACK: That is his calculations.

MR. GLESEKE: His calculations of the correlation of data, and I adjusted that for some loss of core practice, and lo and behold we come out at .002, which is amazing because that is the number that is up there for the NUREG 1465. So it stays the same.

And I will go along with NUREG 1465 on the ex-vessel, and the same for in-vessel.

MR. BOYACK: Okay.

MR. BOYACK: Zero, point, oh, two.

MR. POWERS: Zero, point, two percent?

MR. BOYACK: Zero, point, oh, two release fraction.

MR. POWERS: Okay, all right. I'll just try to be consistent. Okay. And do you want to provide a basis?

MR. BOYACK: You're going to hear it

shortly.

MR. POWERS: Okay.

MR. BOYACK: And so you can say CDC/JME up there.

MR. POWERS: Here?

MR. BOYACK: Yeah.

MR. POWERS: CDC, CDC/JME. Okay.

MR. BOYACK: And for the ex vessel, I'll stay with the NUREG number.

MR. POWERS: You say the same?

MR. BOYACK: Yeah, the same way.

MR. POWERS: I have this projector going in my ear, and I just --

MR. BOYACK: And for the laid in vessel, I don't see a lot of perturbation. I see resuspension, but my understanding, resuspension is fairly low level. So I'll stick with the 65 number.

MR. POWERS: So this was CDC?

MR. GIESEKE: SED.

MR. KRESS: The first C is SED.

MR. GIESEKE: It's not three initials. It's a word "see."

MR. POWERS: As in "see saw Marjorie Daw" (phonetic)?

MR. BOYACK: See what?

MR. POWERS: CBCA, right?

PARTICIPANTS: No, no, no.

(Laughter.)

MR. KRESS: It's supposed to be BC.

MR. POWERS: "See" and then BC.

MR. BOYACK: Evidently, what is that when you flip on I? Am I dyslexic? I've got dyslexic ears, I guess. So this is CBC?

MR. GIESEKE: There you go. Now you're okay.

MR. BOYACK: Hardly took anything, did it?

MR. KRESS: Have you heard of the organization called DAM, D-A-M?

MR. BOYACK: Tell me.

MR. KRESS: That's Mothers Against Dyslexic.

(Laughter.)

MR. BOYACK: Now that I see BC/JMB, what am I supposed to do with this?

MR. POWERS: Listen.

(Laughter.)

MR. BOYACK: Oh, I'm sorry. Now I finally got it.

(Laughter.)

MR. BOYACK: I am really slow, yeah.

Bernard, I have been told to listen to you.

MR. POWERS: Intently. Let me put the "intently" listen.

MR. CLEMENT: We've got the two loops because we initially thought as everybody that all of this stuff had very low volatility. In fact, when we performed the VERCORS HT-1 experiment, it was a burn-up of 14. When they performed it up to 3,000 Kelvin, we measured the significant releases of lanthanum, europium.

MR. BOYACK: So the two groups are lanthanum --

MR. CLEMENT: No, no. The first group, you've got -- we put lanthanum, europium, and praeseodyminim, Pr.

MR. BOYACK: Br?

MR. CLEMENT: Pr. It was not measured, but you can do an analogy.

MR. GIESEKE: Yeah, yeah, not a bad analogy.

MR. CLEMENT: Okay. So from these few results, we've put ten percent release from the fuel and two percent to the containment. In fact we measured eight percent that we are using.

For the others, zirconium, and neurodeme (phonetic), there are others. Okay, but we have put one percent from *, .2 percent to containment.

MR. BOYACK: And .2?

MR. CLEMENT: Point, two to containment.

MR. BOYACK: All right. Thank you.

MR. CLEMENT: Okay. I address those. Okay? The same as --

MR. BOYACK: Okay, and laid-in vessel?

MR. CLEMENT: Laid-in vessel, I don't know if we could have some resuspension or not for the staff. That's again the point. We are in the resuspension. We don't give any number, but just point out that as far as we have important requisites, we've got this problem of uncertainties in resuspension for the latent vessel. We're saying this is a point to be addressed then.

MR. POWERS: Right, but it has to do with what we find out about the physical form of these deposits, and as we learn a little more about it. In some cases the deposits down in the low temperature regime, they move around with a breath of air, and others you can't chip them off with a hammer.

MR. BOYACK: All right.

MR. LEAVER: You certainly don't see that

type of result from PHEBUS.

MR. CLEMENT: No.

MR. LEAVER: So there's something else going on.

MR. CLEMENT: Yes. I don't know if it's an effect of burn-up of temperatures, but something is going on.

(Pause in proceedings.)

(Whereupon, the foregoing matter went off the record at 11:49 a.m. and went back on the record at 11:51 a.m.)

MR. BOYACK: As you know, as we dealt with the last three groups, no metals, cerium group, and Lanthenides, we ran into this issue where the data uncertainties, this regrouping, and what I'd like to do is just have your thoughts on candidate ways of dealing with this, not a resolution, just options.

MR. KRESS: Well, first off, I would be tempted to look at neptunium and cerium and lanthanum separate from the other groups because of their biological effectiveness, and from the fact that it looks like in some cases they get released enough to worry about.

If they didn't get released enough to worry about, I wouldn't worry about them otherwise,

but they seem like in some cases they get released enough, and their biological effectiveness is severe and can make some differences.

So I would be tempted to look at those as separated out from the groups.

MR. BOYACK: Okay. So this was a case where you would separate them out. What else? Anything else that might occur to you or is that -- you can do this more than once.

MR. KRESS: Well, I would think all the other groupings are pretty much okay because it doesn't make much difference anyway, and that would be my feeling.

MR. BOYACK: Okay. Jim, what do you think? Any thoughts about how we handle this, the fact that we have these --

MR. GIESEKE: Well, I think it's important to note that at least in the case of neptunium, the French data suggests that that's significantly different than some of the other elements in that grouping. So it warrants a special look.

I just supported what Tom said, I guess.

MR. BOYACK: Okay. Dana?

MR. POWERS: I guess you're asking the question really of what do you do about the fact that

some people say ten and other people say .1.

MR. BOYACK: Yeah, yeah. So what do we do with the variability?

MR. POWERS: And I think you go with a voting mechanism with careful attention in the text outlining the minority opinion. I think you need to end up with tables with numbers in them that people can use, but I think you have to put the explanatory part in there that says overall this is based on incomplete information, a snapshot in time.

It could be that the outlier view is closer to reality than the majority.

MR. LEAVER: Your point about ending up with a table that people can use, when we started this, there was some talk about writing a -- do you consider this a PIRT exercise, a PIRT report, which would be like a NUREG CR report similar to the ones that you've done on the reactivity insertion of the three that you get?

If we do that, then maybe we don't need a table. Maybe it's a range. I don't know, but then maybe it's RES' job as the issuer of 1465 to reissue 1465 using this information that we provide. I mean that's another process here.

I'm not sure what the process is, but --

or whether we're expected to come up with a table.

MR. BOYACK: Let me just comment that the very first contact I had referred to this work as a PIRT activity, and I carry within my mind a certain definition of what that means, the processes and the approach.

And we had a pre-meeting, came together, sat down, and discussed what was really the objective of the NRC activity, and that was to come up with these tables, these revised tables.

And so you may see that I don't use the word PIRT. In my correspondence I call this the source term applicability panel rather than a PIRT panel, and that's the reason why. I don't think it's the PIRT.

MR. LEAVER: Yeah.

MR. BOYACK: I understand the point that you've indicated, and so what I've done is to put down as one of the options to consider whether the NRC would take the results of the panel, which may not have a single value, and then come up with --

MR. LEAVER: Well, in some cases it may; in some cases it may not.

MR. BOYACK: Yeah.

MR. KRESS: Can you change that word

"early" in the top line? I didn't say that or at least I didn't mean it. It seems to be released to a greater extent than previously filed.

MR. BOYACK: I can change it.

MR. KRESS: Yeah.

MR. BOYACK: Sure. Okay. Let's see. So any comments about options, things we might consider?

MR. CLEMENT: Yes, I think that some elements should be separated. So I agree with neptunium, cerium, and lanthanum, and they would add the volatile ones, molybdenum and technetium.

MR. BOYACK: Okay. So was all of that a single group?

MR. CLEMENT: No, no, no, no, no. This should be separated according to their volatility and their radiological importance. I don't give the separation right now of these other three important factors.

MR. BOYACK: Let's see. The volatility was the second.

MR. CLEMENT: Biological effects, biological hazards, you know.

MR. BOYACK: The list of species. So let me go ahead.

MR. CLEMENT: So neptunium, cerium,

lanthanum, molybdenum, technetium at least.

Okay. Then the second point is I fully agree with what's said that we should identify what are the sources for the variability within the values. This is quite very important. I think we do not have time to discuss in detail all of what were always sources of variability. What I would propose is when you look at all what we have said, you identify the most important sources of variability and maybe asking panel members --

MR. LEAVER: To confirm that.

MR. CLEMENT: -- to confirm that and to send in written form what are the reasons why, not for all of the variables that were given, but I think that you can identify after the meeting what are the main sources of the main differences between the members of the panel, and you could ask each member of the panel to give more detail in written form about why stay with that.

That could help to document these variability.

MR. LEAVER: While he's typing, what are we doing on getting copies of the material that you presented? Is that happening? Are you going to send it out next week?

MR. BOYACK: They'll send that to me early next week.

MR. LEAVER: Okay. That's going to happen next week. Okay.

MR. BOYACK: And the one thing, I think we said Power Point is what you had the material in, but if you can also in addition create a PDF file. The one thing I've found out is the PDF files almost always work and sometimes --

MR. CLEMENT: Yeah, yeah, yeah.

MR. BOYACK: -- we have trouble with other files.

MR. LEAVER: Is that why you do it, PDF?

MR. BOYACK: Absolutely.

MR. LEAVER: Is it a smaller file?

MR. BOYACK: No. It's platform independent. That's why it seems to work better. Since I generate on a MAC machine, that becomes important.

MR. LEAVER: If you got after that reading, something like that.

MR. BOYACK: Right, and those can be downloaded pretty often off the Internet.

Okay. Is there anything else then? I think that sort of captured what I wanted.

Charlie, what we're going to do is in a moment take a break for lunch, come back a few minutes before one, and so the question I have is is there anything else that you wanted us to discuss for a few minutes or are we kind of covering what you wanted?

You'll note that we haven't done anything on BWR yet, for instance.

MR. TINKLER: Right.

MR. BOYACK: And we had some discussion yesterday based upon Ralph Meyer's presentation. Maybe that was the first day, that the BWR fuel is moving in appearance and characteristics towards a PWR fuel, and there was some thought that we might be able to go through the BWR --

MR. LEAVER: Pretty quickly.

MR. BOYACK: -- pretty quickly, which maybe would be the first section of the final meeting

MR. TINKLER: Well, I mean, to the extent you can talk about the BWR fuel, you know, as we went into the implementation of the alternate source term, revised source term, we thought again about whether or not the distinctions in the source term between the PWR and BWR were really worth the effort, were really worth the distinction, frankly.

Revised system level calculations only

serve to convince us that variability in sequences was larger than the variability in the source term between the two reactors, and that, you know, making distinctions between 30 and 35 percent, which was an averaging process once upon a time, just really has questionable value to us, okay, and whether or not it's, you know, a distinction without a difference, frankly.

MR. BOYACK: Yeah.

MR. TINKLER: You know, I'm not encouraging you to spend lots of time on it if you want to, but you might consider whether or not it's just -- considering, as we say, the preponderance of evidence maybe and the uncertainty associated with all of this, we just adopt the same source term for Ps and B.

MR. LEAVER: You recognize if we do that, you're talking about a 30 percent increase in iodine release.

MR. TINKLER: I understand, but do we really think that, you know, there is that much difference at this point?

MR. LEAVER: Yeah.

MR. TINKLER: It's just something to consider. I know that it's going to have some impact

on some calculations, but do we really think there's a difference within the range of uncertainties?

It's something you might want to debate among yourselves, you might want to consider.

MR. BOYACK: Okay, all right. What I suggest we do then now is eat lunch.

MR. LEAVER: That's an idea.

MR. BOYACK: Unless there is any real objection to that particular approach.

MR. KRESS: Why don't we get back at 12:30?

MR. BOYACK: And?

MR. KRESS: You said we'd get started at 12:30 or did you say shortly before one?

MR. BOYACK: We have speakers coming in at one to talk to us on this particular question.

(Whereupon, at 12:03 p.m., the meeting was recessed for lunch, to reconvene at 1:00 p.m., the same day.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(12:57 p.m.)

MR. BOYACK: Let's go ahead and resume our meeting.

We're grateful that we've been able to have some individuals come and talk to us about MOX fuel, and I don't know whether one of you will take the lead in introducing all of that.

Steve Nesbit. So I'll turn the time over first to Steve and have him introduce his colleagues and also the course of the meeting.

Now, I should inform you that we have a court recorder. We're going to have a transcript of the meeting. There is a microphone --

MR. NESBIT: Whatever I say can and will be used against me.

MR. BOYACK: -- microphone here on the pulpit, and if from the audience you wish to speak, if you'll use the microphone over at the side.

And, Steve, thank you.

MR. NESBIT: Thanks.

MR. BOYACK: And if there are handouts, I can take care of getting those out.

MR. NESBIT: Okay, great. Okay. Thanks for giving us the opportunity to come and participate

in this meeting this afternoon.

I do have some handouts, and if you can distribute those, I'd appreciate that.

I'm Steve Nesbit from Duke Power, and I'm the mixed oxide fuel project manager for Duke Power. WE also have with us today Patrick Blanpain from Framtome in France. Patrick is going to give the second presentation of our two.

Larry Losh from Framtome USA back there next to Patrick, and we may be joined by Patrick Rhodes from Department of Energy. I don't see him here yet.

And we were asked to come and discuss with your panel aspects of mixed oxide fuel that might bear upon the applicability of the NUREG 1465 source term to that fuel, and we've tried to put together a couple of presentations to address those issues.

We certainly encourage you to ask questions, and if we can't answer your questions today, we'll try to get back to you at a later time.

The purpose of my presentation is to give you a general overview and background of the program that we're involved in to use mixed oxide fuel and to address a few of the points that I think were raised in your first meeting, which I got from reviewing the

transcript there.

Some of this information, maybe all of it, you're familiar with. I'll try not to go into excruciating detail on it, but I do want to give you a general overview of what we're trying to do here.

The Department of Energy has the responsibility for managing surplus plutonium from the United States Weapons Program, and in September of 2000 the United States and Russia signed an agreement to dispose of significant portions of that plutonium, 34 tons of weapons grade plutonium for each country.

By the terms of the agreement, the majority of the plutonium will be disposed of by conversion to mixed oxide fuel in use in existing nuclear power reactors. Pursuant to that agreement, actually a little before that, DOE contracted with a team of companies known as Duke Cogema Stone and Webster, or DCS, to provide MOX fuel fabrication and irradiation services.

As a general overview of the project, the way it's supposed to work is this. The material, the plutonium is currently in the form of pits stored at Pantex in Texas, a DOE facility there. It's to be transported by DOE to a facility to be developed at the Savannah River site called the Pit Disassembly and

Conversion Facility.

That facility will turn the classified weapons into unclassified plutonium oxide powder, which will be transferred to another facility on the Savannah River site, also not yet built called the MOX Fuel Fabrication Facility. That's the subject of a current license proceeding in front of the Nuclear Regulatory Commission. They're applying for construction authorization.

This facility will make mixed oxide fuel. It will be transported by DOE to four nuclear power reactors, McGuire Units 1 and 2 and Catawba Units 1 and 2. There it will be used in a manner very similar to existing uranium fuel and eventually it will be -- the eventual in-state would be a geologic repository.

Who's who in this team of companies? Duke Cogema Stone and Webster, imaginatively named after Duke Engineering and Services, Cogema, and Stone and Webster. These are the three main partners.

Cogema, of course, is the French fuel cycle company that has a lot of experience in the field of plutonium and mixed oxide fuel and also another partner or another subcontractor through Cogema is Belgonucleaire, which is also an experienced MOX fuel fabricator, and Electricity de France, which

has a lot of experience using mixed oxide fuel.

Other subcontractors include Nuclear Fuel Services, who are working in safeguards and security; Duke Power, which is responsible for using the fuel; and Framtome AMP, which is responsible for designing the fuel.

Some key milestones. I mentioned that there's a construction authorization request before the Commission. That was submitted in 2001 for the MOX fuel fabrication facility. The goal was to start MOX fuel fabrication facility construction 2002 or 2003; receive a license to possess and use nuclear material at the facility in 2004; begin a lead assembly irradiation program in one of our reactors in 2004; complete the MOX fuel fabrication facility construction and start-up and testing and start making mixed oxide fuel in the 2006 and 2007 time frame, and ultimately start to use the fuel at McGuire and Catawba 2007 to 2008.

Moving on to some facts about what MOX fuel is and isn't, MOX fuel is, as you're probably aware, a blend of about five percent plutonium oxide with about 95 percent uranium oxide, depleted uranium oxide in our case, and I will point out that like conventional low enriched uranium fuel, MOX fuel

pellets are primarily uranium.

Fission power from MOX fuel comes from Plutonium 239 primarily rather than Uranium 235. Other than the fuel pellet material, a MOX fuel assembly is essentially the same as a uranium fuel assembly.

MR. POWERS: When you say "a blend," you don't mean a blend on the atomic scale, do you?

MR. NESBIT: I think you're asking about a question that pertains to the microstructure of the pellet itself.

MR. POWERS: Yes.

MR. NESBIT: Mr. Blanpain is going to address those issues.

Using mixed oxide fuel in light water reactors, typically this is being done on an industrial scale in Europe today. There's 35 reactors using mixed oxide fuel.

You typically load a mixture of mixed oxide fuel assemblies and conventional uranium fuel assemblies in a core.

If you look at the EDF program, they have 30 percent MOX fuel assemblies in the core. The European experience base actually goes up to 38 percent in German reactors. Our plans for the fuel is

to use approximately 40 percent MOX fuel assemblies and the remainder conventional uranium fuel assemblies.

We designed the MOX fuel assembly in terms of plutonium concentration to have essentially the same reactivity as the uranium fuel.

MR. BOYACK: Does the plan to burn this how many tons, 34 tons of plutonium in these four plants and no others?

MR. NESBIT: The current plan is that. In fact, there's currently 25 tons of plutonium allocated for MOX fuel. Those 25 tons would be used in those four plants.

What kind of plutonium is in the MOX fuel? Well, in the European programs, the plutonium comes from recycled or reprocessed commercial nuclear fuel. As a result of that, the isotopic mix is approximately 60 percent Plutonium 239, more than 20 percent Plutonium 240.

The material that we're going to be using is weapons grade plutonium. It will have 93, 94 percent Plutonium 239, and the remainder Plutonium 240. Obviously the material we're using, the weapons grade material was produced in a special manner optimizing or maximizing the amount of Plutonium 239

in the mix.

I've got some analytical comparisons I'm going to show between different mox fuel types and uranium fuel that shed a little bit of light on differences and similarities between the fuel.

This first slide is a nuclear slide, and I'm a nuclear engineer. So I actually understand this. What you have got on the Y axis is K infinity or a measure of the reactivity of the fuel assembly as a function of burn-up on the X axis, and of course, as the fuel is burned, the reactivity or the energy content decreases.

The purple or blue line here is a nominal low enriched uranium fuel assembly. The red line is a reactor grade MOX fuel assembly, and as you can see, compared to the uranium fuel assembly, the reactivity in the reactor grade MOX assembly drops off much slower.

The third line is the weapons grade MOX fuel assembly, and as you can see, the nuclear behavior is between the other two, the reactor grade MOX and the low enriched uranium fuel. So the behavior is bounded by the other two.

Total plutonium content shows a similar story. Again, these assemblies have a similar

reactivity at burn-up of 20 gigawatt days per ton. This is the amount of plutonium in a fuel assembly as a function of burn-up. The uranium fuel assembly starts with none and builds up to around one percent as the fuel assemblies burn.

Reactor grade starts with more because the plutonium is not as high grade as the weapons grade plutonium, and it decreases with burn-up. The weapons grade decreases with burn-up.

Two points. Weapons grade is between reactor grade MOX and low enriched uranium fuel, and second, as you burn it up in terms of content of the fuel material, the weapons grade becomes increasingly close to low enriched uranium fuel.

The fuel that we're going to use in our program is shown here, and it's a proven design in the United States. It's the Mark BW Framtome design that has been used for years, and in fact, for years in the McGuire and Catawba reactors.

The difference, of course, is the fuel pellet material will be five percent weapons grade plutonium in the form of MOX fuel. There is advanced alloy M5 cladding for the fuel. I think you all are probably familiar with that; intermediate flow mixing grids to make it compatible with the co-resident

uranium fuel, and another difference here between uranium and MOX fuel, We've got radial plutonium concentration zones within an assembly. I've got a picture here that's going to show that a little better.

Most pressurized water reactor fuel assemblies have all the pins are the same enrichment uranium. That's not uniformly true. There actually is some uranium fuel that I'm aware of that's zoned in two zones.

With MOX fuel, MOX fuel has a much lower thermal neutron flux than uranium fuel, and therefore, when you put a MOX fuel assembly next to a uranium fuel assembly, the edge pins in the MOX fuel assembly see a high neutron flux leakage from the adjacent uranium fuel assembly.

So we zone the fuel assembly. We put low concentration plutonium in the corners and on the edges and next to the control rod guide tubes, and that way that keeps the power profile across the assembly flat.

Talk briefly about the reactors that are going to use the mixed oxide fuel in the United States program. McGuire Units 1 and 2 were started up in 1981 and 1984. They're located near Cornelius, North

Carolina, which is north of Charlotte.

Catawba Units 1 and 2, located near Clover, South Carolina, which is southwest of Charlotte, and Catawba units started up in 1985 and 1986. All four units are operated by Duke Power.

All four units share the same designed primary system and core, for the Westinghouse pressurized water reactors; core power level of 3411 megawatts thermal and 193 fuel assemblies in the core.

And all four reactors employ an ice condense containment system.

I want to talk briefly about our planned fuel management with mixed oxide fuel. We're currently on 18 month cycles at these reactors. Our intent is to stay on 18 month cycles so that we'll keep the same cycle length.

We usually have batches around 80 or 84 feed. In other words, we load 80 or 84 fresh fuel assemblies for each reload, and we anticipate that we'll keep about the same feed batch size.

For the MOX fuel, we are going to impose a 50 gigawatt day per ton burn-up limit on the fuel. Now, our uranium fuel is licensed to either 60 or 62 gigawatt days per ton burn-up, but we're putting this limit on there for the MOX fuel to make the burn-up

that we're going to use consistent with the burn-up that's been achieved in European reactors.

And Patrick Blanpain is going to talk a little bit more about that in a minute, too.

We're going to discharge the mixed oxide fuel assemblies after two cycles. Most of the low enriched uranium fuel will run for three cycles. What we expect, based on our preliminary core designs is that the typical mox fuel assembly burn-up will be about 40 gigawatt days per ton.

The maximum fuel assembly burn-up will be in the area of 45 gigawatt days per ton in order to keep the rod burn-up under the 50 gigawatt day per ton limit.

MR. POWERS: How do those limits compare to the limits the French have on their MOX?

MR. NESBIT: The French, interestingly enough, don't have specific burn-up limits for their fuel. They have fuel management licensed by their regulatory authority. It's a little different.

The French typically see burn-ups in the range of 36 to 40 for their MOX fuel assemblies. However, other countries in Europe which use mixed oxide fuel typically see burn-ups in the 40 to 50 range.

Patrick is going to have a slide that shows that, but the Germans and the Belgians for years have been using mixed oxide fuel to burn-ups in the 45 gigawatt day per ton range.

I should add that that's also on a production basis, and that there's lead assembly programs in various countries that are taking the fuel burn-up well above 50 gigawatt days per ton.

Here's typical uranium fuel management that we use in our cores. The shaded assemblies, this is a quarter core slice. The shaded assemblies are the feed assemblies. These are all uranium assemblies. We use a low leakage pattern, one or twice burned fuel on the exterior. This increases neutron economy, and it also minimizes the fluence (phonetic) at the reactor vessel walls. It's a typical checker board pattern with concentration feed assemblies in the so-called "ring of fire" here.

This is a busier slide here that tries to illustrate our MOX fuel core management as we envision it today, but the point that I'll make is that in terms of patterns of feed assemblies, it's almost the same as the uranium patterns. Again, the checker board feeds in the middle.

In this case, the feed LEU are shaded

light green. The feed MOX are shaded dark red. The once burned MOX are shaded light red. Again, checker board in the middle, the "ring of fire," the once or twice burned fuel on the exterior.

Shift gears a little bit here. One of the issues that I think was discussed at your first meeting I would like to talk about briefly: radionuclide inventories or, I guess, the starting point for your source term work. Framtome specifically has done some work on the applicability of the scale code system for mixed oxide fuel and they benchmark scale, 4.4 against MOX and LEU fuel rod measurements.

And what they've seen is that when you compare agreement between the code prediction and the measurement or the C over M, if you will, calculated over measured, that the results are comparable for MOX and LEU fuel. For most isotopes you're within plus or minus ten percent.

I should mention when I say scale that includes the code origin that some people might be more familiar with the code system by that name.

Oak Ridge has done a lot of work with scale. In fact, they're the people who developed the code and maintain it. They are participants in the

ARIANE Program, which has recently been completed in Belgium, managed by Belgonucleaire actually all over Europe.

And there has been benchmarks there to LEU and mixed oxide fuel rods. They benchmarked other LEU and MOX fuel rods, and again, they've come to the same results that Framtome has, that the results are generally comparable in terms of accuracy between MOX and LEU fuel.

So when you exercise the codes against two fuel types what happens, what you find out is generally MOX fuel has slightly fewer fission products than uranium fuel, and it has more actinides than uranium fuel, and that's due to the fact that you've got the initial presence of some plutonium there. So you are already starting higher up on the atomic number scale.

Framtome did force some comparisons of radionuclide inventories using scale. I forgot to capitalize it. Sorry. And they compared a couple or three different fuel assembly types actually, low enriched uranium, weapons grade MOX, and reactor grade MOX at a 45 gigawatt day per ton burn-up point with no decay time.

So this is right at 45 gigawatt days per

ton, no shutdown time. What kind of fission products do you see?

And I just pulled some samples out here. These are some radioisotopes that are important to dose calculations, certainly not all, but I just want to show the general pattern.

Again, the same radioisotopes are there. They're in different concentrations depending on what fuel type you've got. What I've done is I've normalized everything to the low enriched uranium value here. So you start here with weapons grade MOX, Krypton 87, about 68 percent of the activity; Ruthenium 106, 74 percent higher activity; Iodine 131; Xenon 133, about the same; Xenon 135, about 60 percent higher in MOX; Cesium 137 about the same; Cerium 144, about 17 percent lower.

Integrated over all of the fission products, you're about 98 percent of the low enriched uranium fuel at that point, at that snapshot point.

Reactor grade MOX is very similar to weapons grade MOX.

Actinide activities, as I mentioned, weapons grade MOX and the reactor grade MOX are significantly higher than the low enriched uranium because you just start out with more plutonium there,

two to three times higher for these type of isotopes.

Another issue that may bear on your work is decay heat. There's two real contributors to decay heat in fuel. One is fission product decay, and that is higher enriched, low enriched uranium fuel. There's actinide decay or the heavy elements. That's higher in mixed oxide fuel.

As a result, when you look at a total decay heat comparison between the two fuel types, short-term decay heat is lower than mixed oxide fuel. It's dominated by the fission products, and that difference is what makes the difference there.

Long-term decay heat is higher in mixed oxide fuel. After many of the fission products have had a chance to decay off, then the actinide contribution is more important, and the MOX fuel has a higher decay heat. I guess the point that I would leave you with here is that MOX fuel has lower decay heat during the time frame of the kind of accidents that I think you're considering for the purpose of your work on source terms, and I have got a plot here that shows that a little more quantitatively.

This is a comparison, again, a ratio of MOX to LEU decay heat. Total decay heat for two fuel assemblies irradiated to 43 gigawatt days per ton.

One is weapons grade MOX. The other is low enriched uranium fuel.

So in the vicinity of 80 days or a little longer, you cross over the 1.0 point. So before -- excuse me -- hours, 80 hours. Before 80 hours, the delay heat from the MOX fuel is lower. Afterwards it would be higher.

I've tried to give you a brief overview, and Patrick Blanpain is going to give you a much more detailed and technical overview of the MOX fuel pellet and the characteristics there.

I will leave you with this. As a part of our program we have been working developing reports, submitting them to the NRC. I'll call your attention to several that you may be interested in. The COPERNIC topical report from Framtome covers a COPERNIC code which is used to predict fuel mechanical behavior. And the MOX part of that was submitted to the NRC in August of 2000.

D.C. Hess has developed a fuel qualification plan, which describes the work that we're doing in order to qualify in mixed oxide fuel for use in United States reactors.

Revision 2 was provided to the NRC for information in April 2001, and I would add that it has

a lot of information that we're covering today in there.

Duke Power submitted a topical report on nuclear analysis methods for MOX and uranium fuel in August of 2001, and that's before the NRC for review right now.

There's a Web site that the DCS Company runs that has information on DCS and also has links to other Web sites that are out there that have plutonium MOX fuel information.

So that's about it for me. If anybody has any questions they want to ask now, you're welcome to, or if not, I'll turn it over to Patrick.

MR. BOYACK: Any questions?

(No response.)

MR. BOYACK: Okay. Why don't we proceed with Patrick's presentation then?

MR. BLANPAIN: Good afternoon, ladies and gentlemen. The purpose of my talk is a general overview on the MOX fuel fabrication, on the MOX fuel experience in Europe, and on the MOX fuel behavior in the reactor.

First is the introduction to the stages of MOX fuel use in Europe.

The first commission in 1972 in Germany,

and in 1987 in France, fuel recycling is a form of MOX fuel, has reached an additional maturity.

The production capacity used by Framtome ANP was 180 ton per year heavy metal with the MIMAS process. The two French Cogema plants in Kederash (phonetic) and Machood (phonetic), and the Belgian at Desser (phonetic) operated by Gelgonucleaire.

More than 1,700 fuel assemblies have been delivered by Framtome in Bay of Fragema to 20 French, two Belgian, and three German pressurized reactors. A little more than 1,000 fuel assemblies have been delivered by Framtome NP GMBH, formerly Siemens to 11 German and three Swiss PNBWRs.

Now, a few words about the MOX pellet fabrication. Through the MIMAS process, which means the micronization massive plants where you're starting with UO2 powder and Pu2 powder. First, we are performing a primary blend with Pu2 powder and Pu2 powder at a ratio that's about 20 to 30 percent of * stock in UO2.

That primary blend is micronized with a ball grinding (phonetic) to micronic size.

MR. POWERS: What kind of balls are you micronizing?

MR. BLANPAIN: Please?

MR. POWERS: What kind of balls are used for the micronizing?

MR. POWERS: I think it's uranium balls.

So that primary blend -- also with the primary blend we can recycle the scraps, and that is one of the interests of this process. We can reuse all of the scraps of the fabrication in the primary blend.

So the primary blend is then mixed with fresh U2 powder to constitute the secondary blend, which is mechanically mixed, and then the process is famous for U2 fuel pressing, centering, and different dried grinding compared to U2 where we use wet grinding control, and then the MOX pellets.

And the results is that * structure. That's the fresh MOX pellets as fabricated. It is micrographed after chemical etching to reveal the grain boundaries. That scale is ten microns.

So we can hear what age of plutonium, the plutonium * uranium. So you can see here are some quite white, large grains that's plutonium rich particles. So with plutonium enriched zone here, we can see the gray, some pure uranium particles or grains.

So it's easy to see something here that

need to be more easy. And you saw the micrograph after special attract or review, the plutonium sustained edge chemical attack, and we can see the plutonium oxide in white.

So that what is called plutonium enriched particles, but what is important is that the plutonium compound in those particles is the content of the primary blend between 20 and 30 percent of Pu2.

You can see also that we have plutonium in the U2 matrix also, and here on the right you have a careful analysis of the plutonium distribution into pellets. On the Y axis there is the total, percentage of total plutonium in the pellets, and on the X axis the particle size in micron from zero to 100 microns.

And this was mixed by EPNE (phonetic). So we can see from that graph we have two curves because two -- the * obtained with a U2, AOC U2 border or ADU U2 border. There is small differences in that zone.

But we can say that 50 percent of the total plutonium is included in particles larger than eight microns. That means also that 50 percent of the plutonium is included in very small particle size, smaller than eight microns in this region.

And also it's about 25 percent of the total plutonium is included in particles larger than

30 microns, to give you those large particles, size 20 to 30 microns.

So that fuel is not so intelligent in radiation use, but most of the plutonium is spread in very small particles. And the plutonium content of those particles is the content of the primary blend. So it's charged through EPMA, and these are the plutonium through the pellets. It will glow if there's a plutonium content to plutonium rich particles. That's the size of the particles, about 60 microns here. It's a large one.

So if we have zero plutonium here in the U2 grain and then the particles with present content of about 25 percent, which was the plutonium content of the primary blend.

Then out of the particle there's another small particle, back to the U2 metric, and then the next one.

MR. SCOTT: You mentioned on the --

THE REPORTER: Could you use the microphone?

MR. BOYACK: You can go over here.

MR. SCOTT: The question had to do with the dimension on the bottom of the graph. Was that your just arbitrary scale? It was over 100.

MR. BLANPAIN: Here? Sorry. That scale, it's micron.

MR. SCOTT: Micron.

MR. BLANPAIN: So it's the size of the particles between -- it's roughly 40 microns.

So as I said, this is the MOX use in Europe. So we're using MOX U in light water reactors up to 1,300 megawatts electric and are using different * designs. For example, for the PWRs from 14 by 14 to 18 by 18 array.

They're using in Europe different neutronic design in fuel management. They're using a total * content up to seven percent, enriched assembly, and given to U5 enrichment to more than four percent. That would be in Germany, for example.

Different core loading schemes are used, one third annual or one fourth annual.

As mentioned before by Steve Nesbit, recycle rate is usually 30 percent of MOX fuel assemblies in the core, but 50 percent of MOX assemblies in the core is licensed in Germany, but in fact, with maximum use it's 38 percent.

We are using different U2 matrix, depleted, due to natural U2, natural U2 that's formally in Germany, but not used among it for the

most.

The discharge assembly burn-up are usually around 45 * and up to 54 gigawatt day * assembly was reached for individual elements.

In France the MOX fuel can operate in neutral load model without any restrictions, and so the conclusion of that is no rods, no MOX rod ever failed for a mox specific reasons. I mean for reasons linking to the fuel itself or to the plutonium content of the fuel.

That table shows in more details the MOX used in Europe. Here is reactor type, B or PWRs, and different countries, Germany, France, Belgium and Switzerland.

The initial year of loading from 1996. The total number of assemblies delivered at the end of 2000 and the max assembly burn-up at shields and the time. This year delivered fuel assemblies reached 80 and 58 * in the German * water reactor. The rough burn-up in the assembly was 63.

In France the discharge burn-up is usually around 37, 40, but it is on assemblies which * 58 gigawatt days per *. Also in Switzerland.

And that is the actual situation of discharge burn-up with the burn-up experience for the

Fragema deliveries in Belgium, Germany, and France. In blue we have France. You can see that average discharge burn-up is around 37, 38 with lead assemblies at higher burn-up here. That corresponds to a rough burn-up of about 60 gigawatt days per ton metal.

In Germany and Belgium the numbers are quite higher, in green and yellow here. We show burn-up around 43, 45.

That was our experience, and now some more about the design and on the performance of the MOX fuel. First, the mechanical design of the fuel assembly structure is identical for uranium and MOX fuel assemblies.

MR. POWERS: That suggests to me that you anticipate the swelling of the fuel to be the same.

MR. BLANPAIN: I will show you some examples.

I submit the design of the fuel assembly first. Okay? So FOU to fuel meter variable prediction of the thermal critical behavior of the fuel rods. So we are making a design of the MOX fuel with adequate description of the MOX specific properties.

The design models and codes are

continuously clarified by comparison with measurements, and that is continuing with high burn-up, and for the design code, we have for the moment the same level of * as for uranium fuel.

In France, the current 17 by 17 fuel rod design accommodate high efficient * of the MOX fuel by an increase of the plenum volume and the lower area of initial pressure, and that was * mainly to accommodate high efficient gasoleaks (phonetic) due to the load follow effect.

In Germany, they don't do that because they have a larger plenum than the 13 by 13 designs. They have two plenums.

So some examples of the physical properties of the MOX fuel we use in all codes. The thermal properties, a more important one, and the specific heat of the MOX fuel compared to U2 fuel. You can see U2 in the red and the MOX fuel in black and blue. It's going from an open literature and showing it the same tendency.

The larger difference between MOX and U2 is the thermal conductivity. The thermal conductivity of the MOX fuel is a little bit lower than the U2 fuel. It depends, of course, on the plutonium content in the pellets.

Here we have the thermal conductivity of temperature. The dashed line is U2 and the lower using for the MOX fuel in blue. There is about a difference of five percent for the six percent of plutonium in the penthouse (phonetic).

MR. POWERS: There is something that I don't quite understand when you show heat capacity as a function of temperature. For a heterogeneous material, surely as you get up in the upper range of that temperature you have plutonium dissolving into the lattice.

MR. BLANPAIN: Yeah.

MR. POWERS: And that becomes a time dependent phenomena, and there must surely be some sort of heat of solution that's reflected in your measurements, you know.

MR. BLANPAIN: Yes, we have a * of plutonium in the metric has a very, very high temperature above lattice, 1,800 degrees. No, there's no asset to that, but it was measured, I know, because we have performed measurement at very high temperature.

Finally, to the thermal expansion of fuel U2, fuel U2 and mixture of * U2 with the same BA load of course.

Again, the fuel -- the thermal conductivity and the fuel temperature. That's just the reason of the experiment.

The final experiment is MOX and U2 rod flex with a central thermocouple evaluated in the same condition in the same reactor, and on the Y axis we have the central comparator as a function of the power. In blue we have the MOX fuel and in red the U2 fuel.

You can see that small difference, and typically we have a difference of 50 degrees at 200 watt per centimeter, cell * temperature at beginning of life.

Also reasons for experiment about the fuel central departure, but at high level, that's a watt *, 50 gigawatt days per ton. It also is a rod pre-irradiated in a commercial reactor and then refabricated with central thermocouple and irradiated in the Alden reactor.

Here we have the comparison. Then we have the central temperature, assumption of power. The experimental points are in blue here. and the * are the calculations made by design code. That's the percentage here and the two bounds, the upper and lower limits.

And that is the prediction at high burn-up, and what is interesting to note is that the fuel * degradation with burn-up in MOX fuel is the same as for U2 fuel. It was demonstrated in the Alden experiments.

For fission gasoline, I'm going later on the resource, but that is the comparison of the prediction and the measurement with COPERNIC on 56 commercial fuel rods irradiated to four radiation cycles, typically burn-up to 50, 53. I'm show a quite good prediction of the codes.

Now I will show you some regions from partial examination of irradiated fuel. We have a quite large experience feedback through surveillance and analytical programs. We have about 100 commercial fuel rods, to a rod burn-up of 60.

We have performed a lot of power ramp testing and also instrumented * irradiation which have been launched through a national or international program to assess the particular interaction, fission gas release, fuel central temperature, * specification, and so on.

Those experiments were published in the open literature.

But as the main reasons of those programs.

So the MOX fuel has the same behavior as U2 fuel concerning the fuel rod growth, the cladding variance of the formation. That means the fuel swelling in normal irradiation conditions. So the irradiation of the fuel density is the same.

The cladding water wide corrosion, there is no MOX effect on the cladding and no mutual spectrum effect on the corrosion. As already mentioned, the pellet solid swelling is the same. It is quite normal because it is a cell fission (phonetic) product in the same amount of fission product in the U2 metrics.

Also with no difference concerning the zirconium oxide internal layer. That's zirconium oxide internal layer gross with burn-up after the contact of the pellet and the cladding, and then stabilized with thickness of around ten, 12 microns as in high burn-up U2 fuel.

Roughly the fission product and activity release of failed rod, of failed MOX rod is the same as for U2 MOX rod. So we can see some differences in the ratio of some isotopes. So that allows us to know if it is a linked MOX assembly or U2 assembly, but the activity release is the same.

And those results were published in the

literature.

So we're seeing the somewhat high efficient gasolines than U2 fuel at high burn-up. That means about 40 gigawatt days per meter, leading to a higher fuel rod internal pressure at the end of life. I will explain that later.

The MOX fuel shows a better cladding behavior due to the high creep property of the pellet. So it is interesting for us in France because the MOX fuel is automating (phonetic) versus the plant maneuverability. So there is no limitation for the * operation.

And also there is no AW release in normal pressurized water reactor irradiation conditions. As you know, the MOX fuel produce more radium than U2 fuel. This is another magnitude of difference mainly due to the decay of the Curium 242.

Helium release was observed in non-precise fuel rod, and it's also observed in boiling water reactor fuel rods, but in the case of pressurized fuel rod, there is no helium release because helium * pressure is higher outside than inside the pellet.

So fission gas release data. After fission gas release, at the induction of rod burn-up for commercial fuel rods irradiated in EDF reactors in

France. The MOX data are in red. They're compared to some U2 data, whereas it is also because it's due to quite high enrichment. That's in higher power than the standard U2.

And you can see that fission of radiator (phonetic) of the MOX is generally higher than for U2, and mainly at high level, above 42 gigawatt days per ton here.

What's interesting, that at high burn-up this is three cycle fuel rods, annual cycles, and four cycle fuel rods. There's no fission rate enhancement due to the burn-up alone. It's, of course, normal because the power during the four cycles is decreasing.

We had obtained very recently new fission release data after five irradiation cycles. They are here.

And loss MOX rod while irradiated, power level similar to the U2 fuel rods. So we can see that they are in the U2 range.

Other data provides by our German colleagues. Here we have the fractional fission * as a fraction of the rod power during the second irradiation cycle because it's the high powered cycle, and there have been the open triangles, U2, and here

the MOX fuel. There's data above 100 for * and showing roughly the same * at relatively low burn-up.

At somewhat high efficient gasoline we have observed in the ADS plant, is explained first mainly by the neutronic properties of the MOX fuel leading to the higher linear heat rate at medium and high burn-up. That's the first factor.

I know so the MOX fuel presents a different paired radial power density distribution leading to the higher fuel temperature at high burn-up, and that's higher power, high burn-up. It's explained by the K infinity showed by Steve Nesbit during his presentation.

So due to the physical properties, due to the relatively lower thermal conductivity already mentioned, what is important is the typical threshold for fission gas release is the same for MOX fuel and for U2 fuel that was demonstrated in different experiments. These are Framatome Halden reactor projects, and that tendency remains the same at high burn-up.

Another factor we could explain high efficiency gasoline is the oxide microstructure. It's the presence of plutonium rich particles of 230 percent of Pu2 during the * process can change and

make an inefficient gasoline.

And due to the local high burn-up zone, which leads to the formation of dense pore populations. I didn't go into more details about that.

So as a result of EPMA measurements in MOX fuel, mainly in the plutonium enriched particles, that's a nine concentration, as I mentioned, for the burn-up as a function of the average paired burn-up of 50, we have a local burn-up of 102, 150 gigawatt day per ton of metal in the plutonium enriched particles.

And what is interesting also to note, it's no big difference, is that it's normal between the periphery with radius and the center of the pellet. That's the nine. It's the difference increased slightly with burn-up. It's due to the fluid depression in the pellet of high burn-up.

Here we have a typical radial cut of a MOX pellet at high burn-up. It is 50, showing the typical cracked bottom as U2 fuel, but what we can see here, the black spots enhanced due to the chemical etching, but it is plutonium rich agglomerates.

And on magnification of that microstructure, it shows it on the large plutonium agglomerates, 20 microns. That's why it's a big one,

but periphery, we have the formation in the plutonium rich zones if porosities of brittles (phonetic). It's like a *. At big radius, it goes out at center where the temperature is higher. We have * coalescence, and so we have the presence of larger pebbles, and that's pretty centralized. Sometimes we have a big coalescence of the purpose leading to the whole surrounded by smaller porosities and metallic precipitates.

And here we have an X-ray map of plutonium and xenon in the set time of agglomerates. Here we have plutonium mapping. You can see in white plutonium rich zone with porosities in black. And to the right side is xenon mapping. We cannot see anymore xenon in the plutonium rich particle, mainly due to the cutting and polishing of the pellet. So the gas escape the porosities.

But it is interesting that we have a lot of xenon around, such as in red, higher with xenon, higher across red around the agglomerate, of course, is due to the fission in the U2 around agglomerates.

Actually now also we have plutonium also in -- this is quite a solid solution in the pellet, but also we have xenon around due to the fission spike, the * and recourse, but our high burn-up with

quite a lot of xenon in the U2 matrix.

We have homogenization of the fuel with the burn-up.

Also the result of EPMA analysis, but through the pellets, both the pellet's periphery and the pellet's center, in green here is the calculated prediction of xenon, and the red spot, the measured xenon. We can see a quite large expression, of course, is due to the porosities because we cannot see xenon porosities, but EPMA cannot see xenon in pulse and bubbles, and also we have cracks in the fuel *. So about half of the gas escape in the detection. It's the same for U2, but particularly for the MOX fuel.

But what is interesting, that that fuel rod release -- I don't remember -- but five to six percent of gas, and we have the depression of xenon in the center of pellet exactly as can be seen on the U2 fuel. So it is the same fission gasoline mechanics.

Going back to the helium in the fuel, the graphs show a helium balance in commercial figure of MOX fuel rods, and yet here, the helium volume t the permit (phonetic), and the open square here is initial volumes with uncertainty on the as fabricated fuel rods.

And the blue squares are the final volume of helium which is lower than the initial volume. That means that a part of the helium was absorbed by the fuel. It's well known we have exactly the same reasons for U2 fuel.

And also after power ramping. So we have an example here. The initial volume of the rod before ramp testing and in green the final volume after testing. So we have no helium release doing standard less two power ramps.

And the last results about fission gas release obtained for weapon grade MOX fuel. This is the data from Oak Ridge, the Oak Ridge National Laboratory. And those data are compared to commercial data coming from Germany. It's the same data I presented before.

The fractional release is a function of average pour (phonetic) during the second irradiation cycle. So we have German data, and here the data from the short rods irradiated by Oak Ridge at Aidelle (phonetic) with weapon grade fuel, and a quite large amount of gallium, around the one ppm.

And here after 20 gigawatt days per ton metal and sufficient gasoline resource, and after 30 gigawatt days per ton metal. So we have a very good

correspondence with other fission data. So we can say that there's no gallium effect on the fission gasolines.

And as a conclusion of my talk and our major and long-term development. So for economical reasons, MOX fuel will perform as efficiently as U2 fuel, with the gas burn-up and * of flexibility because the burn-up, the key balance to uranium fuel assemblies as applied in Germany, Switzerland, and Belgium. It's the maximum assembly discharge burn-up around 50 gigawatt days per ton of metal.

In France that U2 and MOX parity will be achieved near 2004 because today the maximum burn-up arrived is 52, and it's due to a MOX -- 52 is the name of the project.

The licensing process is underway. It's a quite constant process because it concerns the 20 MOX license plans in France, and the rod burn-up will be 60 in that fuel management.

So that we are thinking about the future, that disparity must be studied on a long-term basis, and the objective of EDF is to further increase the U2 fuel assembly discharge burn-up.

The next step is six days of degraded pattern meter (phonetic) assembly by 2010, and

research and development to prepare the future MOX product is still ongoing.

Thank you for your attention

MR. BOYACK: Thank you.

Any questions or comments?

MR. POWERS: I thought it was a very nice presentation, the introduction.

MR. BOYACK: Yes, it was.

There was a third presentation, and then we can open it up.

MR. POWERS: I have to leave.

MR. BOYACK: You have to leave? Okay.

Any other questions or comments here?

MR. NESBIT: That's it for us.

MR. BOYACK: That is? Okay. Very good. I just wanted to make sure I had that right. Okay.

Then you've escaped us without a lot of questions. Again, the presentation was very nice and very much appreciated.

MR. NESBIT: Thank you.

MR. BOYACK: Would you come up to the microphone if you have a question? Would you come up to the microphone?

MR. SCHAPEROW: Brent, would you like to pass around the sign-up sheet again? I think there

are a few more people that weren't here earlier.

MR. BOYACK: That would be good because I didn't pass it around at all.

MR. SCHAPEROW: Well, I think the very first day when we started the meeting you did.

MR. BOYACK: Yeah, okay.

MR. SCHAPEROW: If you could say your name.

MR. LYMAN: Yeah, I'm Ed Lyman from the Nuclear Control Institute.

I have questions actually for Steve Nesbit, but they pertain to the presentation we just saw.

I just want to establish for the record now we saw the pellet homogeneity distribution of the MIMAS fuel that's used in france, and I just want to compare that to the specification which DCS has issued in the dual qualification plan, and it looks to me like those are not the same, and that the DCS specification is less strict as far as concerning the size distribution of the agglomerate.

So it just --

MR. NESBIT: Well, the purpose of our presentation here wasn't to get into a detailed comparison of fuel specifications and information that

you may have gleaned or inferred from this presentation, and I'm not going to stand up here and try to respond to that question.

Do you have any other questions?

MR. LYMAN: But it's important because if you're going to argue that the French data is pertinent to this discussion, you want to know if the parameters are the same for the two types of fuels.

MR. NESBIT: The specification that we're using for weapons grade MOX fuel is fundamentally the same as the specifications being used in Europe today for reactor grade MOX fuel.

MR. LYMAN: But that -- well, I have the specification here. It says it's 95 percent of the plutonium enriched particles shall have an effective diameter of less than 100 microns, and the mean of the plutonium enriched particle distribution shall be less than 50.

Now, if you look at the distribution that was just shown, it shows a much smaller number, a much smaller fraction as an agglomerate size of 100 microns. So it's --

MR. NESBIT: Well, but that's actual results of manufacturing process versus the specification. The specification provides upper

limits for acceptable parameters. It doesn't provide "you shall not be below this."

MR. LYMAN: Well, considering that Westinghouse recommended back in '94 that improvements be made for the U.S. MOX program, in fact, limits on plutonium agglomerates should -- the average should be limited to ten to 15 microns. Why don't you start with the specification that's at least as good as what the French are achieving and try to improve it?

MR. NESBIT: We're using the same specification.

Anymore questions?

MR. LYMAN: Okay. The other question I have is you keep maintaining that the maximum fuel assembly burn-up is typical of European experience, but from the graph that was shown, just by eyeballing this it looks like no more than about ten percent of the fuel assemblies that have been burned have burn-ups greater than about 40 gigawatt days per ton.

And so it just looks to me like a maximum burn-up of 45 is outside of the envelope of the bulk of the experience.

MR. NESBIT: It's clearly within the experience as shown by that graph, and I would further add that if you were to look at uranium oxide fuel

burn-ups in the United States, you'd see a similar shape. b ut that doesn't mean we can't and don't take uranium fuel to burn-ups of 45 or 50 or 55 gigawatt days per ton.

MR. LYMAN: Well, but there are issues associated with high burn-up MOX fuel performance that may be more critical than high burn-up LEU fuel performance, and so I think it's even more critical that the experience be supported for the --

MR. NESBIT: Your opinion is noted.

MR. LYMAN: Okay.

MR. BOYACK: Okay. Thank you.

Let's see. If there were no other comments or questions, first off, I want to thank our guests for bringing in the MOX material. It was very, very much appreciated.

What will happen, for your information, is we will be meeting, if I can find the slide, we will be meeting in the third and final panel meeting here at the NRC February 19th to 21st, and that will be the meeting that focuses on the MOX source term applicability, February 19 to 21.

And I have you, Steve, I have you on the distribution list. So you'll receive the E-mails that have to do with that, but I wanted to make sure you

were aware of when we meet again.

MR. NESBIT: Do you anticipate a desire for any more participation from our team at that meeting or is that yes/no or is that to be determined later?

MR. BOYACK: Maybe I don't have the answer because I basically facilitate and rather than deal with the technical aspects of the meeting, but, Charlie, do you have any insights as to an answer to that question?

MR. TINKLER: Well, since that's the meeting at which we will be discussing release fractions, to the extent anyone has an interest in hearing those deliberations I would encourage you to attend, you know, if it's --

MR. BOYACK: And we --

MR. TINKLER: You know, if you think you're going to have an interest in that, I would encourage anyone to attend.

MR. NESBIT: Well, I'm sure we will. I just wondered if --

MR. TINKLER: No. Right now I guess I don't see a need for presentations as part of that. Okay? All that will be panel deliberations.

MR. BOYACK: But we also welcome and

accept the comments coming from the --

MR. TINKLER: Oh, sure, sure.

MR. BOYACK: -- the participants, too.

Okay. Well, very good. I think we have concluded the business that I had arranged for ourselves. What I will be doing is momentarily going upstairs adjacent to make copies as we did yesterday of the materials that we generate through the course of our discussions today, and certainly if you want to stay here for a few moments.

The other alternative is I'm going to be providing these as PDF files in any case to people, but we will go upstairs and make quick copies if you want to stay here for a few moments, and we'll be back. It takes us about 15 minutes to get them out of my computer through the copying machine and back down here to you.

PARTICIPANT: If we're leaving do we need escorts or do we need to wait for you?

MR. BOYACK: Okay. The question was whether we need escorts, and anybody that's not an NRC person needs escorts.

MR. SCHAPEROW: It says on your badge whether you do or not.

MR. BOYACK: With that, I will adjourn the

meeting.

Thank you very much.

(Whereupon, at 2:16 p.m., the meeting in the above-entitled matter was adjourned.)