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NUCLEAR REGULATORY COMMISSION

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SOURCE TERM APPLICABILITY PANEL

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TUESDAY

FEBRUARY 19, 2002

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ROCKVILLE, MARYLAND

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The Panel met at the Nuclear Regulatory Commission, Conference Room 4-B-6, One White Flint North, 11545 Rockville Pike, at 8:38 a.m., Brent Boyack, Moderator, presiding.

PRESENT:

BRENT BOYACK, Moderator

BERNARD CLEMENT, Member, Consultant

JIM GIESEKE, Member, Consultant

DR. TOM KRESS, Member, Consultant

DAVID LEAVER, EPRI Designee, Polestar Applied Technology, Inc.

DR. DANA POWERS, Member, Sandia National Laboratories

JASON SCHAPEROW, Member

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

(8:38 a.m.)

MR. BOYACK: Let me now begin by welcoming you to the third and final meeting of the Source Term Applicability Panel. The reason I emphasize the word final is just as we begin our activities to encourage panel members to realize that we have a good deal of work to do before we get to the end of the meeting. On the top of your agenda is --

DR. POWERS: I will remind you of the discussions with the Russians. He knew that it was going to be a very difficult negotiation, and so we announced to the press that this was easy, and that only obstructionists would cause it to go more than a day long.

And the Russians felt then obligated to show they weren't objectionists, and so the negotiations -- and that is what you should do here. Is say we all have a little bit of work to do, and this should go very quickly if you are not obstructionists, you see.

MR. BOYACK: I have a problem with short term memory retention, but I do have enough memory retention just basically to say that those are great words, and I choose to adopt them.

All right. So we don't have very much work to do, but you will be an obstructionist if you go ahead and prohibit us from getting to the end, but let's go ahead and talk about the objectives.

There is one primary meeting objective for each of the three major portions of work that we have before us, and one supporting the meeting objectives for each of those.

So the first thing that we want to do, and we are going to schedule two hours to do it, is to complete the extended source term applicability tables for high burn up of the PWR fuel with Zirlo cladding, and undergoing a low pressure scenario.

That is the very first thing, and the supporting objective to take a small amount of time, and try to quickly move through the development of an approach for addressing the situation in which we might have different conclusions by individual panel members regarding individual BWR release fractions.

And I call out the ex-vessel release of the cerium with one example, and so we will have two hours for that. We will take a break, and then we will move on to the next item, which is to go through the process for developing extended source term applicability tables for high bred BWR fuel.

We have only allocated a couple of hours to this; one hour before lunch, and one hour afterwards. In our prior meetings, there were some points made by panel members that to me suggested the possibility that there really wouldn't be to many differences between the BWR and the PWR fuel.

I don't know whether that is the case or not, but what we can do, of course, is have the initial dialogue to come to an understanding, and hopefully a common understanding amongst the panel as to whether or not the BWR fuel would have any significant differences than PWR.

DR. POWERS: What I have come to learn is these new BWR fuels are going to use erbium as a burnable poison, rather than gadolinium?

MR. BOYACK: Okay.

DR. POWERS:

DR. KRESS: Quite frankly, I know nothing about what erbium does to fuel.

MR. BOYACK: So there is an example. Maybe what we can do is get the list of challenges down, and the NRC can direct us, but my understanding is that the higher priority activity would be to make sure that we get the MOX fuel source term.

And we will limit our BWR discussion if we

happen to go through MOX quickly, which I guess would be a surprise, and we can always come back to BWR.

DR. KRESS: Well, the differences we might see in the BWR fuel, of course, are two. The rates of heat-up, and the fission product release are sensitive to rates of heat up, and you may have some difference there.

Plus, they are sensitive to how much oxidation of the clad you get, and with the channel boxes around, you will get a different oxidation rate for the BWR fuel. Those two things --

MR. LEAVER: Lower power density in --

DR. KRESS: Lower power density.

MR. LEAVER: But you would have more Zirc around.

DR. KRESS: Yes. So those two --

MR. LEAVER: They would, but I don't think we are smart enough to --

DR. KRESS: No, I don't know if we are smart enough to figure that out and the difference.

MR. BOYACK: So, let me start the earlier discussion here and that we are basically saying that the approach that I envision is right at the start of the discussion on BWR, to go ahead and list these facts that would have an influence, and at the end of

that time the panel basically says it would be so speculative that we really could not do too much more, then we will go on.

We will have listed the factors, and we might list research areas that would be helpful to go ahead and deal with the uncertainties that are associated with the different fuel and then go on.

What I would like to do like I say is just go ahead and do that. So the second supporting objective is to develop an approach for preparing extended source term applicability manuals for BWR fuel.

Now, if we can get that done briefly within a few moments, or a few hours actually here in the meeting, fine. But if not, if we can get enough of the information down that would be of value to the NRC.

The third and major time elements of our meeting will be associated with developing the extended source term applicability tables for MOX fuel.

And I have indicated a low pressure snarl (phonetic) again. We will first specify the characteristics of MOX fuel for which the source term applicability is to be considered.

We had presentations at the last meeting about that, and I have a slide on that, and where we can look at that, and adjust it as necessary, and then use that as a basis for going ahead and working on the source term applicability for the MOX fuel.

So that is the plan. So we have a few hours, and a break -- an hour before lunch, and two hours for PWR, a break, an hour before lunch for BWR, and then an hour after lunch.

And so the rest of the time is associated with the MOX fuel. Is there ny comments about that?

MR. LEAVER: I have a question. Is there going to be some information made available on any experimental results on MOX by anybody beyond what we already have, which is not much at this point? Is Jason still here?

MR. SCHAPEROW: I am. I don't have any handy that's for sure. I mean, there is some out there that I understand.

MR. LEAVER: Yeah, I guess the French had some, but the results were quite qualitative in the presentation that Bernard and Jean Michel made at the last meeting.

It was interesting, as it talks about earlier release of fission gas, and that was about it,

and even that was -- I guess there were some conflicting results between the two tests.

But nothing really other than -- well, we can talk about it, but I mean, how do we come to conclusions with basically no data at all. Not much. I guess that is my question.

MR. CLEMENT: To make a short comparison, but not with absolute values for all the elements. It would remain qualitative with the classification of various elements, depending on their release. We can make a short presentation with a few view graphs.

MR. SCHAPEROW: You are going to have to figure out the availability of the French --

MR. LEAVER: I was leading to that, yeah. I suspect that there is some.

MR. SCHAPEROW: I guess we will have to discuss it with the NRC what the available is, and whether it is publicly or not.

MR. LEAVER: Yes, whether NRC is participating in that.

MR. SCHAPEROW: I know that we have a current international agreement with them to cooperate in this area.

MR. BOYACK: When we come back after lunch, about one or two o'clock, I guess it is, and

begin the discussion. Then the individuals -- what is the name of -- is it --

MR. SCHAPEROW: They will be here this afternoon when we start the MOX work.

MR. BOYACK: They will be here this afternoon also around two o'clock when we go ahead and come back to the discussion. So the questions are entirely appropriate.

MR. SCHAPEROW: Absolutely.

MR. BOYACK: And we will have that discussion as soon as we begin that activity. So the question -- well, the first question that I have is already I am standing and I can feel the heat sort of coming down.

Is there anything that sets the thermostat in this room, or is it just me?

MR. GIESEKE: No, it is hot in here.

MR. BOYACK: I tend to run cold real quick, and so when I am starting to sweat it means to me that -- well, jus be aware that it is getting warm. Now, Jason, I think you had a few comments to make also.

MR. SCHAPEROW: I brought with me something that I put together quite a while ago actually for communication purposes. Salesmanship

obviously, but I think it might be useful.

And maybe just to remind everybody and I guess not everybody has been involved in the application as much as developing it. So this gives kind of an thumbnail sketch of what I put together on the application of the source term.

This is just a little summary of what we use the source term for. We use it for containment integrity, and off-site dose, and we do off-site dose calculations to ensure the containment is okay, and leak-tight enough, and to make sure that we have proper EAB boundary.

But the thing that we sold it on was more realistic, more realistic release rate, and more realistic physical forms, and chemical forms. And as you are probably quite well aware, we did a rebaseline study to basically change out the old source term.

And we did lots and boatloads of calculations with a revised source term to see how the dose would change. All the scenarios that I had this design basis leak rate, and that is in the application.

Use the design basin leak rate, together with the early in-vessel releases, and if have had four pilot plants so far with the revised source term.

We have had two more plants that have been approved, and I think there is three more in the process and with applications in with NRR.

But this is my attempt to draw a schematic of what we do with those.

DR. KRESS: Those aerosols are too big.

MR. SCHAPEROW: Radioactive aerosols. Number 1 was the source term, the early extent of the containment, and again we just used the early in-vessel release for the design calculations.

Number 2 was the containment sprays, and in some cases we allowed natural deposition of aerosols in the containment. Again, we only used the design leak rate, because this was a licensing calculation.

DR. KRESS: How do you know whether --

MR. SCHAPEROW: Apparently it could make a difference.

DR. KRESS: It could.

MR. SCHAPEROW: But we always tilt towards conservatism most of the time, and that's it. And that poor fellow is standing in the middle of the flow.

DR. KRESS: That is a big person.

MR. SCHAPEROW: But we took the picture

from standing near the --

DR. KRESS: Oh, that is a perspective. Good graph. I like that.

MR. LEE: Jason, just one more comment. Now we have about a dozen or so source --

MR. SCHAPEROW: Oh, okay. Good. Glad to hear. This is a voluntary thing. The industry feels that this is benefiting them in both cost and safety, and they are coming in and they really like it. They were a little nervous about it at first.

Dave has worked on a couple of the applications, and he can tell you how industry people felt about applying it. And I am kind of curious as to how -- as to what it was like when people first heard about the source term, and wanted to apply it. And if you want to share anything with us.

MR. NOURBAKHSH: Jason, just a question. When do you decide we are using in-vessel releases on in-vessel dose calculations?

MR. SCHAPEROW: Well, actually, NRR made a licensing decision that we would just use in-vessel for licensing dose calculations.

MR. NOURBAKHSH: Because EAB analysis --

MR. SCHAPEROW: That's correct. So only in-vessel.

MR. NOURBAKHSH: And of course cap (phonetic) release?

MR. SCHAPEROW: Yes, I included that.

DR. KRESS: One of the things that we have is whether we use the first two hours or the worst two hours, too.

MR. SCHAPEROW: That's correct. The NRR goes towards conservatism and wanted to use the worst two hours because the release really didn't happen until a bit later, and they wanted to capture that. They wanted the dose to reflect the big release.

DR. KRESS: The previous use of the source term specified the first two hours. So that was in the log.

MR. LEAVER: But it was artificially introduced at time zero.

DR. KRESS: Yes.

MR. LEAVER: That happened because of AP600 and it was really hard to get the source term in there faster than about an hour, or an hour-and-a-half.

DR. KRESS: That's right.

MR. LEAVER: So they wanted to have zero to two hours, in which only about 30 minutes was --

MR. SCHAPEROW: There was quite a debate

over that. That's right. I remember Len Soffer got up and debated against NRR on that.

MR. NOURBAKHSH: How come you can't make the time realistic and how it impacted each time?

MR. LEAVER: Well, I mean, they did a bunch of those calculations and in AWR, we did a bunch of calculations. But I think there is maybe -- well, maybe Jay Lee or Jason could speak to this.

But there was a desire to have a somewhat stylized accident that was representative or typical, rather than considering -- I mean, we considered a spectrum of accidents, but you wanted to have something that you could define as a design basis, and two hours is a pretty reasonable period for release.

It could be more and it could be less, but it is not unreasonable. The TMI was a little bit -- about an hour-and-a-half or something like that.

DR. KRESS: The regulations would allow what you said, and if somebody wants to come in with a different source term. If they can justify it, that's allowed, if they have the database, and the release model, and the calculations.

MR. SCHAPEROW: Well, there is a lot of that done, where a lot of sequences were analyzed in coming up with the current one, saying this is

representative of the risk important sequences that we see.

MR. BOYACK: Let me summarize where my records show that we got to last time. What I have done is that this is the PWR releases in the containment, and the black are the original NUREG 1465 values.

And the red values are from our source term applicability panel meeting of December 11th through the 13th of last year. The blue, which does not show up here, are question marks, which are values remaining to be specified by the panel at this meeting.

And of course if there is any comments about any of the earlier values that we needed to address. Now, I sent out to the panel members the tables that go along with each of these values.

You will recall that at the last meeting that we had generated on-line if you will tables, and we kept track of the individual's names, and then we went ahead and came to these values.

And there is a table for each one of these individual entries, and my action item from the last meeting was to go ahead and produce a summary table, and take out the names, and just try to summarize and

come up with key factors.

And that is always subject to error, and so those were put out with a chance for all of the panel members to revise and correct. Now, Jim Gieseke said he has some work on tables, and presumably I will receive those before the end. I think a lot of those are editorial.

MR. GIESEKE: It is on big deal in there, and most are just words.

MR. BOYACK: Right. But that is a start. So what I will call editorial and a few things that may modestly change the meeting. And then there are other issues that are bigger.

So I had an e-mail from Dave Leaver on 2/12, and he had some comment on the summary tables, and so he will present those as we go along here in a few minutes.

And basically what he wanted to do was that he wanted to revisit some of the values and talk about those a little bit, and he was asking why do we have some of the values that we do.

I think that is certainly an appropriate activity. So what I would like to do is spend the next few moments -- well, first, I will come back and ask if there is any other approach things.

But what I would like to do is just go through, and I think we will go through these values one at a time, and see if we are still okay, and are there any major comments that need to be recorded so that I can revise the table.

And not so much the editorial comments that Jim had, and unless there is some major ones amongst that. Those I can take by way of mark-up, because it won't require the whole panel to review.

But if there are indeed issues of the values, or the rationales that I wrote down on the table, then those we want to talk about. Now, I think there are one or two here that Jim had, and that Dave had some comments on.

And we get down to this point, and we have the rationale, but there were differences of opinion where these question marks are. And we would like to go ahead and try to get values.

Now, there was a proposal at the last meeting that said if after a modest discussion we can't come to a single value amongst the panel members, then what we have to do is go ahead and list the number of the majority of the panel members, and then make a clear indication in the information that comes along with that, that there were one or more

other views.

Now, the shortcoming of that, just so you are aware of it, is that ultimately -- and if you look at the report, the draft report, and you get to the conclusion section, and what you see is just a table with the values.

So you lose those -- in the table you lose those other views. Now, in the text there will be comments that indicate that other situation. But people get to the point where they don't read the text. They just look at the table.

So I want to point out that out as one of the realities that we would encounter by going through that process. Now, before we start all of that, I would just like to entertain any comments from the panel members about what I said about the process.

MR. LEAVER: It sounds okay.

MR. BOYACK: All right. Well, the first thing I am going to do is see if I can have any better luck than I do last time in starting up the projector.

You may recall the very first time I tried that it failed.

DR. KRESS: Move it to the left.

MR. BOYACK: No matter how many times I try this, it doesn't always work.

(Brief Pause.)

MR. BOYACK: Okay. So let's go ahead and begin. Let's just take these in order, and I am going to ask given the constraints of time that if you have just minor mark-ups, if you will go ahead and mark up a hard copy and give that to me.

If you don't have a hard copy, Jason will provide a hard copy. But that would keep us just dealing with the major issues. So the first thing I would like to ask is we have the values shown on the left screen of the original NUREG 1465 values, and the values that the panel agreed up.

Are there any questions about any of those having to do with duration or -- and I will give you a moment just to look through the write-up that I prepared, but not very long. The hard copies would be better.

(Brief Pause.)

MR. BOYACK: All right. Well, for the moment then, since that is going to be harder, let's just take a moment and go through the table and talk about -- see if there is any comments on the values, because we will get to a point where Dave Leaver will raise his hands and say, yes, I have got one on that.

So the duration, we went to 4/10s of an

hour on gap release, and 1.4 hours on erbium vessel, ex-vessel, two; laid in vessel, 10. Any comments about the values of those times, as opposed to justification, which will be written down below?

(No response.)

MR. BOYACK: What we will do is provide you with hard copies. And I am going to continue on just for a moment here, and go to noble gases. Thee should not be too much difficulty or question about the values, because those seem to be fairly firm by the panel members.

We had .07, a little longer on gap release for noble gases, and 0.63 hours for the early in-vessel, and .3 ex-vessel, and zero, late in-vessel.

DR. KRESS: I have one comment. I have .3 on the .63 is kind ridiculous.

MR. BOYACK: Is kind of a what?

DR. KRESS: Ridiculous. It implies more knowledge than we know. It should either be .6 or .7, or .5, or something.

DR. POWERS: You are going to run into a problem that you will make noble gases .1.

DR. KRESS: Yeah, okay.

DR. POWERS: I mean, that is the only reason it is 0.63.

MR. LEAVER: Well, let me say that the only reason that we did that was because we wanted the sum to --

DR. KRESS: The sum to be .1.

MR. LEAVER: Or the sum of gap and early in-vessel, which is .7. I guess I have a question, and I sent this question to Brent about .07, and what we said, if I read this correctly, is there was a French test for high burn up fuels that had a value of 5 percent at 60,000 megawatt days per ton.

What test is that? It is right here. And I looked through all the information that we had, and didn't see --

DR. KRESS: Well, I think VERCORS was five wasn't it? One of the VERCORS tests was --

MR. LEAVER: Was it a VERCORS test?

DR. KRESS: Yeah, it was at that level. That I remember, but I don't remember which one of the tests though. I think it might have been five.

MR. LEAVER: Okay. Maybe that is true. Let's assume that's the case, and then I guess the question then is what do are we doing here? If we have a test at 60,000 megawatt days per ton, with a 5 percent release, and we are talking --

DR. KRESS: Why then is it seven?

MR. LEAVER: Why then is it seven? Well, first of all, 5 percent as far as I know, is a pretty bounding number, and 5 percent was the original number in 1465.

And I think it was certainly an estimate, but it was -- it tended to bound at least the data that I have seen that had been collected by examining spent fuel rods.

And we also have to remember that this is a high burn up situation, and only about a third of the fuel assemblies have the high burn up. And this is a core-wide number that we are coming up with.

DR. POWERS: Are we supposed to be integrating the core loading pattern?

MR. BOYACK: You are asking me?

DR. POWERS: Yes.

MR. BOYACK: It is probably better to go ahead and have Jason answer that, because I don't know the answer.

DR. KRESS: We mentally integrated using this one-third as a rule of thumb last time.

MR. LEAVER: Yes.

DR. KRESS: I remember that. We ran that through all of this I think to some degree.

DR. KRESS: Yes, and if we just start with

the one-third type.

MR. LEAVER: Or if we wanted to, we could use the one-half. But clearly it is something of the order of a third, or a half of the fuel that would have this high burn up and not more, because you refuel every 18 months or every two years.

DR. KRESS: Well, what we are talking about is 70.

MR. LEAVER: What is that?

DR. KRESS: Seventy, I think, is what the high burn up was. So I think the number might be -- instead of 60 giawatt days per ton, I think we are talking 70.

MR. LEAVER: Well, that could be. What is the burn up target that we are shooting for? That is a good question, I guess.

DR. KRESS: It was 70, and the reason for that was that I think it is allowed already up to 70. Dana, do you remember?

DR. POWERS:

DR. KRESS: I think it is 62.

DR. KRESS: Okay, 62, but there was movement to make it less.

DR. POWERS:

DR. KRESS: Yes, the Rule Fuel Program has

a target of 75 giawatt days, and my looking at it says that if you constrain yourself to stay around 5 percent enrichment, that the fuel itself is capable of going up almost a hundred.

Now, whether your clad will stand that or not is a different question. But is it entirely possible to run fuel up to certainly 90 or maybe a hundred.

Nobody really wants to do that, because the energy game is -- I mean, you are really getting down to the low rate activity part of the fuel here, but you can certainly envision people going to 75.

Now, these mental integrations put a constraint on the use of this, and it is important to point out to people that you really are saying a third or a half, depending on what you choose to do.

And then you start putting other constraints on the thing, because there are two ways to get the high burn up. One is fast burn up and one is slow burn up. And fast burn up means you have hot center lines.

And that will jack your gas release up big time on you. So now if you are integrating over that, which is not really a bad thing to do, because right now what happens is you ramp the fuel hard, and then

you move it out to a periphery, and then ramp it not so hard.

Unless you power uprate, in which you ramp everything hard. I mean, you start playing these gymnastics on this stuff.

DR. KRESS: That is another thing, and I think we ought to assume power uprates.

DR. POWERS: They most clearly affect the BWR modes, because --

MR. BOYACK: At the last meeting when we had this discussion, I think the thing that moved it to prior value, in addition to the French data, was that there was this citation for the activity insertion experiment out of JAERI.

And then we listed a need and associated with that, recognizing that it was a fast process, as opposed to the more slow process associated with the LOCA. So where do we go on this?

MR. CLEMENT: I suppose what we can get for high burn up fuel, but no so high. I mean, we have the experiments for the last five, for which we have devised five processes, and that was at 50 gigawatt base per ton for these design basis LOCA transients or something like that.

And for the higher burn up fuel, from the

-- can get the same thing or even more. So we know that high burn up fuel can raise more. So if we put five percent, many will put constraints on the fuel burn up, and maybe it can be more.

We know that with density, that this is increased with burn up, and also with follow-up obviously.

MR. LEAVER: That's right, it does increase with burn-up, and I guess I was -- the point that I wanted to make was that this is -- as I understand it, this gap release is not -- we are not saying that this applies to a fast -- a high energy deposition. This is a LOCA, and this is the first part of a LOCA.

MR. CLEMENT: Yes.

MR. LEAVER: And really from a practical standpoint, when you do calculations, the GAP release and the early in-vessel are just a number, and it almost doesn't matter how much is GAP and how much is early in-vessel.

MR. CLEMENT: But when you do the calculations, if you are -- the precise value of that GAP release, you have to calculate where are the gases, and what is the inventory, and so on and so on, unless it is being validated in experiments.

If you want to calibrate this seven person value, for instance, and that is not so easy.

MR. LEAVER: But in any event -- and this also is for the high burn up fuel, which is what, a third or a half of the core, and so I just don't understand how we can jump to a conclusion of 7 percent per GAP release.

It seems to me that it is 5 percent, which is the existing value, and which is probably high enough.

DR. POWERS: Well, you are focusing in on test, and if I look at the JAERI test, you aren't putting an energy ramp in, but they are saying that 20 to 25 percent of the fission gas is on the green boundaries, and can come out.

And so take a third of that, and then you end up with seven.

MR. LEAVER: That is an energy deposition rate that is more applicable to a rod injection reactivity actually.

DR. POWERS: Yes, and it's where this thing, the gas, is.

DR. KRESS: But I think you have to have some consistency question here, too. If the old source term decided that you are going to get a gas

release of 5 percent, we know that it ought to be bigger than that, but I don't know what their thought processes went through.

But high burn up is going to -- more burn up is going to give you more release, and there is a consistency question there.

MR. LEAVER: But that does not necessarily mean that you -- well, that 5 percent may have had enough margin in it that we think it is still reasonable for the high burn-up. I guess if we are going to have a number of 7 percent, it seems to me that we need to have a better justification for it, because that doesn't do it.

I mean, that says that 5 percent at 60 giawatt days per ton, and we know it is roughly a third of the core, and I don't get 7 percent from that.

So if we want to cite some other experimental data, I don't know

DR. KRESS: I see what you are calling a justification here.

MR. LEAVER: Yes.

MR. CLEMENT: Two points. You can ask for more experimental data, and that is the first point. The second one is how do you use the experiment for

full data when you have a few answers from, and what we are doing now, but it is still an ongoing process, is to use the mechanistic goal.

And the point that you calculate all of the history of the fuel during the radiation, just where are the fission products, and where are the gases, and how much should be released.

So it is not so easy because we need some validations. And we had view graphs presented by Jean Michel at the last meeting, which indicated that some calculations not performed by us, but by other people, could indicate up to 10 or 20 percent of gases available for release.

But we are now trying to go further with our own tools to see really what happens. So it is not so -- and I think that for consistency this is one point where the values should be higher than for lower burn up.

All the values for lower burn-up should be decreased.

MR. LAVIE: Our process to that point is really -- if the cooling was maintained, and that original then was really transient. If you can keep that temperature at the peak cladding temperature requirement, then --

MR. CLEMENT: And the other point is that it depends on what you use your source term, okay? If you want just to look at that as important, and if your accident goes further, then very soon you will be in early in-vessel, and then you can find it with these five percent or seven percent.

After one-quarter of an hour, it doesn't matter, okay? So that is the difference of what you want to do with this table.

DR. KRESS: Exactly. And I assume that we are specifying a gap release for the particular uses for that particular release, and most of those are very particular, like how fast do you have to close the isolation valves in the containment. That is one of the big players in that.

MR. BOYACK: Let me interrupt the conversation just for a moment, because this is a good point for us to address what we will have to address later. Let me assume for the moment that Dave prefers a lower value entered here.

MR. LEAVER: At least I would like to have a justification that says it is consistent, or is more consistent with our numbers. In that, I can always ask that if you are willing to go with a higher number, you can help me with the justification.

But if not, then this would be the first case where we have different views amongst the panel members, and in which case, we would have to figure out how to do that.

And so the suggestion from the last meeting was that we just make a note in the table, which will appear in the body of the report, that the summary table will appear in the conclusions, I guess.

But the justification will appear in Chapter 3 or 4, or whatever that is, and there the information would be that one or more, however many of the panel members took exception, and for this following reason, and that is the way that we would handle it.

So that is the only way actually I can see through on a timely basis to complete the discussions on PWR.

MR. SCHAPEROW: There is another alternative, is to not to put a value, but to put an asterisk and say different values from different members, and that can take care of the aggregation process later to think about how we can -- well, maybe that is a cop-out.

But we can put it off until later, and that is not -- we don't need this source term today.

MR. BOYACK: I am not a real fan of putting it off, but that is primarily from the standpoint of delivering a product to the NRC, and that is a non-product to me.

But as you say, it just puts it off, and what I sense here is that there is probably -- I guess we probably have not heard from Jim on whether or not he would be still be with this.

MR. GIESEKE: There are a couple of things that are kind of bothering me. One is have we focused on a burn-up level that we are talking about for the amount released, what, 75 percent, or six out of --

(Simultaneous conversations inaudible.)

MR. SCHAPEROW: For the highest --

MR. GIESEKE: For a third of the core?

MR. SCHAPEROW: That's right, or a half.

MR. GIESEKE: So we are settling on some numbers there that uses to help us think through the rest of this.

MR. SCHAPEROW: That's right.

MR. GIESEKE: Okay. So we are looking at what -- what we are really looking at is the old values basically for two-thirds of the core, and high burn-out for a third of the core. I remember doing those kinds of calculations last time as I was trying

to get some of the numbers on this.

But I just thought that I would put that on the table. That is kind of a hand calculation kind of procedure that you could go through.

MR. SCHAPEROW: Just to get a sort of a idea, yeah.

MR. GIESEKE: So what we are looking to is if -- well, that is another assumption, and I know that we have not done it throughout. We have not assumed that the 1465 numbers are correct necessarily, because more we have more than learned since those were put out there.

So we are not in the position of really taking the old numbers for two-thirds of the core, and adding a high burn up for a third of the core.

MR. BOYACK: By that you are saying you are not required to take the old ones.

DR. POWERS: That's right.

MR. GIESEKE: I am just trying to set the ground rules here so we are on a --

DR. KRESS: You have to remember that the old numbers for the GAP are very, very speculative, and dependent on the two tests that ORNL and a couple of the -- well, I don't know. I am not sure what the other is. But there is a very sparse database for

those two.

MR. GIESEKE: What this kind of says is that for the burn out to make a significant effect it has to be a major change because it is only a third of the core that is coming into play. Is that kind of what you hear?

MR. SCHAPEROW: Yes.

DR. KRESS: You will notice that there is no real spectacular changes in numbers.

MR. BOYACK: Would you please identify yourself?

MR. LEE: My name is Jay Lee, and I am from NRR. Why don't you include sort of the definitions of a high burn up in your report, and what you meant by high burn up?

DR. KRESS: Well, that is probably a good idea.

MR. SCHAPEROW: What we plan to do is incorporate the slides that were shown at the beginning of the last meeting, and we went through all of that. That was one of the points that we went through. But, thank you, that is a good point.

MR. LEE: But here is a comment that I have as far as GAP release. I went to -- for one type of design basis, and the other numbers are different

for design basis, and fuel handling accidents, for example, or for LOCAs.

MR. SCHAPEROW: These are intended for the LOCA.

MR. LEE: The current 1465 neutral state is if you maintain cooling of the fuel, than it is lower GAP --

MR. SCHAPEROW: We really don't have the resources to go after the fuel handling accidents that is needed. We had to make a decision early on. Steve Lavie was at the last meeting, and he was a proponent of trying to tackle that in some manner, but we aren't really able to.

So it is going to be a stretch to get through the LOCAs.

MR. LEE: I think that these are all for the LOCA.

MR. SCHAPEROW: Yes, sir.

MR. LEAVER: I think that is an important point. As I understand it, it is part of the set of ground rules that you were trying to get your arms around, Jim.

This is for the LOCA and we should not be thinking in terms of reactivity insertion type accident, like a rod ejection, because that really is

a different accident, and I think we can probably get a different number if we were to do that.

So I envision this is the first part of a LOCA, and it is important for the reason that you said, Tom, and also, for example, it is important for some plants on how quickly their sprays have to come on. That is another thing.

So there are certain things. Bernard is right. If it is part of a continuing accident, then it is not, but it does have some implication in terms of how fast things have to happen.

MR. BOYACK: Dave, your value that we would have for this would be?

MR. LEAVER: Well, I guess -- the real question that I am asking is maybe a little different than that. The real question is do you want to take exception to the value that is listed.

MR. BOYACK: Well, okay, let me back off for just a minute. At the last meeting, the panel came to a point where I entered the number of .07. Dave has some questions personally, but before I deal with that, are there any other panel members that want to have that number changed?

(No response.)

MR. BOYACK: Then I am going to take that

as a no. So now let me come back to Dave and ask if you want me to go ahead and put in a different value for you?

And once I do that then, probably post-meeting, I will come back and say I want to make sure that I get the justification right, and I would work with you to go ahead and get that done.

MR. LEAVER: I don't see any data or anything in the justification that would in my mind justify seven percent, given that this is a third of the core, and given that we have some data of a 5 percent release that is 60 gigawatts days per ton.

Even if it is 72, that number is higher, and it still averages out to be something less than 5 percent in my mind, given that --

MR. BOYACK: And your value is?

MR. LEAVER: Five percent.

MR. BOYACK: Okay. Now, does hat change then anything on early in-vessel or ex-vessel for you also?

MR. LEAVER: Well, to have the total be .68 certainly implies more than I think we know. So I would just in the interest of having round numbers make it .65 so that the total is .7.

MR. BOYACK: Okay. Fine. Thank you.

Now, what I will do is that I will put a comment here to document justifications. Okay.

DR. KRESS: One more comment. What we are doing is developing source terms that are supposed to reflect the difference between what we have got as the current levels of burn up, to a core that would have a higher level of burn up.

And somehow putting the same number in for the GAP release, and doesn't seem to reflect that difference. And the number that we put in -- and let's say it is going to be higher, but not much higher.

We don't know how much higher, but based on what meager data we have, we end up with .07. So if we put the same number in, to me it doesn't reflect the fact that we are talking about what changes to expect to the source term if you go to higher burn-up, or a different level of burn-up. So that is what bothers me about putting the same numbers in.

MR. GIESEKE: So I think if we would go to .05, we would have to justify the lowering of the 1465 to meet part of your justification then.

MR. LEAVER: That is probably right. I think there is margin in the .05. Just quickly, I might just put this up if I might. Actually, I wasn't

thinking about this, but it is relevant data.

This was data that -- I think it is primarily from CD core, and I am not sure if there is any Westinghouse rods in here or not. But you can see that there is definitely an upward trend as you get the higher burn-ups, and how you bound this data, and what slope you want to give to this line.

I guess that is a judgment call, but when you get out to 60 to 70,000, you would expect to see numbers up around 4 or 5 percent.

DR. POWERS: Let's be consistent and say released to the gap or a sealed gap.

MR. LEAVER: I'm sorry, but could you --

DR. POWERS: These are sealed rods.

MR. LEAVER: What?

DR. POWERS: These are sealed rods. This is a release from the fuel to the gap. This is not fuel undergoing an accident, but this is released from the fuel to the GAP.

So you can shift all those all up on your accident conditions. And just take the whole thing and shift it up.

DR. KRESS: Yes, the purpose of this was to get an internal pressure and load on the clad itself.

DR. POWERS: And measure thermo productivity.

DR. KRESS: And measure thermo productivity so that it is not equivalent to what you get released during an accident. You would have to shift --

MR. LEAVER: You are saying during an accident that the rod is heated up, and so you are going to accelerate --

DR. KRESS: The gap is in an accident, and it is what is coming from the cracked fuel, and what is coming from around the periphery of the grains and stuff, and so you do release what is in the GAP, but you also release some other stuff.

So I would view that as a lower bound on the release, but once again you have to factor in how many of the fuel rod clads failed to get to the release, too. So there is some things going both ways.

MR. BOYACK: Before we continue on, let me just ask Bernard if there was a reference that I could put into this VERCORS test, whether it is the 5 percent at 60,000 megawatts per ton, or --

MR. CLEMENT: I can say that the final value we have a high burn-up of fuel in our French

evaluation was coming from flash tests.

MR. BOYACK: Flash tests?

MR. CLEMENT: Yes. From the work --

MR. BOYACK: Well, all I said was French data for high burn-up indicates a value of about 5 percent for 60, and you said that was a flash test?

MR. CLEMENT: Yes, flash five test.

MR. BOYACK: That was the flash five test for that number?

MR. CLEMENT: Yes, and there were also some values coming from measurements of internal pressure, but these experiments are the property of the utilities. So it is a problem of the values.

MR. BOYACK: Okay. I have -- well, what I am going to do now is you have in front of you --

MR. CLEMENT: Because VERCORS testing, the rods have been already and so what you are measuring is the additional raise, and not the fuel GAP irregularities in the VERCORS tests.

And you see some gases going out through, and so you see the GAP inventory and the releases (inaudible) --

DR. KRESS: That is supposed to cut the fuel up and basically get rid of all that is in the GAP.

DR. POWERS: It seems to me that you are going to have to add in to this is what Tom is saying, is that based on what we know burn up does to fuel, it should have a higher gap release, and we know that the flow structure is more open, and there is more gas on the in-boundaries, and there is much gas that has had a chance to migrate.

That if the center-line operating temperatures are higher, it will push it out there, and if there is a focus on one test, and then I think you should also cite phenomenological evidence that says it should.

The phenomenological evidence that says when they have put impulses in the fuel and you get more gas release, and some of those pulses are of small variety, and you can probably define them, and the Japanese have done what, 58 tests, or something like that.

It seems to me that you need a more elaborate justification to satisfy data.

MR. BOYACK: Is it fair to state -- I sense two primary arguments going on here. I don't know whether we will be able to resolve them, but the first argument is an incremental argument.

It basically says if we take NUREG 1465 as

a base, and not accurate, but as a base, and look at the effects of high burn up, then there is some incremental increase in the GAP release, and it does not go any further than that.

There is another argument that says, well, there is some data, and it is JAERI data, that is a different accident set-up, and high releases. Then there is the third argument, which I think is Dave's, which is that, well, even if I accept the incremental, then I don't like he absolute value of 7 percent.

I am interpreting that he doesn't dispute the incremental argument, but it is the fact that we accept the NUREG 1465 as the base and the number seems to be too high --

MR. LEAVER: Well, the incremental argument is a good argument, and I would accept a number higher than 5 percent of the high burn up fuel. But it seems like we are applying that to the entire core and saying the entire core is high burn up fuel, and it's not.

MR. GIESEKE: If you go to this map, this little on-thirds, two-thirds, kind of map; and you say that two-thirds of it is at 5 percent, the one-third is a high burn-up and is at -- and you take Tom's argument that yours were lower bound, and you take at

75 gigawatts, and you extrapolate to that and say, okay, it is half again that much --

MR. BOYACK: Ten percent you mean?

MR. GIESEKE: Or 12 percent, or to 8, and you go through that calculation and you come up with 7 percent, just like .073 if you wanted to get a number, and I just didn't.

So it seems like 7 percent is probably not a bad number, and it is higher, and it does have that justification being higher than the other one, and the comfort feeling that we are going in the right direction.

DR. KRESS: Now, before we get too far along with this direction, in some of the other fission product groups, we did invoke Dave's arguments, and we didn't like the old numbers for some reason.

So we have invoked this, but not using it as a pure base line, but to adjust our thinking on what the old one was worth. So that does show up in some of these others.

MR. NOURBAKHSH: Now, the problem with putting this table together, Tom, at the end, this will confuse the issue.

DR. KRESS: It probably will.

MR. NOURBAKHSH: Many things appear here which in your mind you know why they are there. But when you look at this, it is (inaudible), because for one thing the total number, the old number of .95 for industrial release, came from the fact (inaudible).

The other halogens (inaudible) are the same on the old numbers, because the old numbers are too high.

DR. KRESS: That's right.

MR. NOURBAKHSH: But on the other hand, you would expect a slight increase (inaudible) for high velocity.

MR. BOYACK: All right. I am going to move this along.

MR. LEAVER: Sure. I think we have spent enough time on this, and this is not the most important issue that we will talk about today. But it is important that we have some logic that we can defend for what we are going, and I think that is the point.

MR. BOYACK: Absolutely. Let's go on to the halogens. Now, I am not going to bring up the test, because you have the text available to you. But basically for GAP release, early in-vessel, and ex-vessel, the numbers stayed at the same value as they

were in NUREG 1465.

However, the late in-vessel release went to .2. Was there an error in the old tables for the NUREG 1465, or did we actually change that one?

MR. GIESEKE: I think we changed it.

MR. BOYACK: Now, do we need to -- are there any points that we wish to revisit on this one?

(No response.)

MR. BOYACK: Lacking any response, I am going to go on to the alkali metals group, and none of the values changed in the alkali metals group relative to those values in 1465. Is there any need to revisit any of the justification or logic?

While you are thinking about that, just let me indicate also that the place we finally want to get this all reflective of the important points is in the document.

And so when we come up with the draft document, a more complete draft document following this meeting, then you will want to very carefully review that, and comment, and we will have to do more by mail, e-mail, and things like that, lacking other meetings.

But you will want to carefully review it, and make your comments. And it will have to kind of

go until we can get everybody to, quote, sign off. Alkali metals. Any comments?

(No response.)

MR. BOYACK: Okay. Well, that finishes up to the PWR, and let's go on to the BWR.

DR. KRESS: Are we going to go into those question mark numbers?

MR. BOYACK: I'm sorry. That is one pass for humor, Tom.

DR. KRESS: Sorry, I'm asleep.

MR. BOYACK: Okay. I was just thinking now that I had one in a row, and I thought we would go on. Now, the tellurium group. Now, Dave, you had a comment on the tellurium group, and so I am going to bring that one up.

MR. LEAVER: I did. I guess my feeling continues to be that the increase to 30 percent seems too high to me in light of -- well, I think FPT-1 is certainly very interesting, and it appears that tellurium roared out in that test.

But I also can't get out of my mind the fact that the TMI data, and the SFD 1-4, in which the tellurium release was considerably less, I would also note that FPT-2, there is or was some information in one of the French presentations at the last meeting on

FPT-2 that said that apparently the tellurium release was less lower than it was in FPT-1.

So I think there is some uncertainly there, and there is no question about that, and under certain conditions, you probably can get a release of significantly larger than the 5 percent than is in this 1465 right now. A number like 30 percent.

But what we are doing here I think is that in 1465, as I understand it, is that we are not necessarily trying to come up with a bounding number, but we are trying to come up with a number that is representative or typical, and those are the words in the NUREG 1465.

So all of that leads me to question the 30 percent number as being just simply higher than maybe we should be. I am talking now about early in-vessel.

MR. CLEMENT: Well, you are saying certain conditions where you can get an important raise, and you could also say to the contrary that there are certain conditions that we can get high retention.

MR. LEAVER: What is in FPT-2 that causes the high retention?

MR. CLEMENT: Okay. The release from fuel is nearly from 100 percent. And then there is tension in the piping of the circuit, and so 30 to 40 percent

retention, and so we don't have the exact figures.

MR. LEAVER: Deposition, or is it a chemical reaction?

MR. CLEMENT: It looks like chemisortion, because it is very different from other elements. So, we know that chemisortion is possible, and if we are talking about FPT-1, that is not the only one.

If you look at the HI/VI tests, in some tests you have nearly 100 percent that is (inaudible), and so you have a number of experiments (inaudible), and also a number of experiments where the react probably with metals, and I am not so sure that we are in a position to make a probablistic study of that.

So then it depends on what we want to come. I mean, in the French approach, in certain cases, we tend to go to the bounding value, and here it depends on what the NRC wants to do with these values. But we took a series of tests.

MR. LEAVER: What was it that caused the chemisorption in FPT-2?

MR. CLEMENT: I don't know. I don't know. We don't have the measurements.

DR. KRESS: It was the gases.

MR. LEAVER: And you didn't get that phenomenon in FPTC-1?

MR. CLEMENT: No.

DR. KRESS: It was probably already tied up with tin or something else.

MR. LEAVER: Right. Right.

DR. KRESS: And that is not very accurate.

MR. LEAVER: And Dana had the paper in which we were talking before the meeting about that, and getting that on the record.

MR. CLEMENT: And there is also some separated effect experiments on tellurium trapping that were made on the pipes, but were not oxidizing, and in the case, you get very, very high retention.

DR. KRESS: The old fridge test, and we ran some tests where we pre-oxidized the plant to a high level, and under those conditions tellurium got released at the same rates as the iodine.

The tests were where you had clad in there that was very a smart amount of oxidation, and the tellurium apparently got tied up in the clad, and didn't get released, and until you are almost a hundred percent oxidized by cladding.

So it depends on how you view the accident, and whether or not you preoxidize very much of that cladding, and whether the ride off of the steam zirlo cladding does a sufficient oxidation to

get rid of some of that tellurium.

I am like Bernard. Under some conditions, you are going to release like a hundred percent of it, and so if you use a bounding value, and then use a high value for it, then I think you have captured all of the particular accident systems that you can capture.

I think that was my reason for going to the high value.

MR. CLEMENT: And I think you could have the common thing that under other conditions the releases could be lower, and that could be stated in that.

DR. KRESS: But the question is what do you want to do with that piece of information, in terms of the design basis source term. I am looking for practical applications of these things, and to me the practical application is under some conditions you can get a lot of tellurium release, and it is pretty bad stuff. And maybe you ought to account for it.

MR. LEE: You have mentioned about retention, and are you talking about retention within the fuel or retention within the system?

MR. CLEMENT: There are several retentions. You have retention with the zirlo

cladding, and looks like that anyway, just because you observe the tellurium release which starts at the end of the oxidation of the cladding.

And so it looks like the retention of the cladding, and what levels are observed -- for instance, in FPT-2, is the retention on the piping afterwards, and this was seen also in other experiments, but not always.

MR. LEE: So in case of a LOCA, you have all the cladding melt, and so therefore it will all come out anyway, is that right? Whether you retain the tellurium within the cladding or not, since we are melting all together in that --

MR. CLEMENT: Well, once you start having tellurium defusing from the fuel on the outside generally in the same period, then you are oxidizing the cladding, and this could eventually lead to zirlo melting.

But at that time, zirlo melt will have some dissolution, and then go down, but not until it is released from the fuel, and there is still a large amount in the fuel at this level.

And then when this one comes out, you cannot find metal zirlo. You only find oxidized zirlo, and just because if the metal zirlo is liquid

and it went down --

DR. KRESS: I think once you melt the cladding, you don't get much release of tellurium that is carried with that melted clad. But you will get much more of a release from the fuel that cladding left.

And what tellurium is left if that fuel is going to come out.

MR. LEE: Yes, but we will use this particular source with the LOCA primarily, and so we have got (inaudible) of clad melting, and there is 30 percent of what exactly (inaudible) --

MR. CLEMENT: You might imagine conditions where you are able to oxidize the majority of the cladding, having only a few molten zirlo. It depends on the accidents now you are getting.

You might have a large amount of liquid zirlo, or a small amount of liquid zirlo, depending on the transient. So you have an inspection of accidents now.

DR. KRESS: How about the accidents and reducing conditions, versus oxidizing?

MR. LEE: Yes. So (inaudible) -- we were talking about the noble gas pressure, whether it is 5 percent or 7 percent, and that amount doesn't make

much difference at all.

But in this case, this will make some difference. Do you see that data on --

MR. LEAVER: On the tellurium release?

MR. LEE: Yes.

MR. LEAVER: It makes some difference, yes. It is not a huge effect, but it definitely makes a difference

MR. GIESEKE: Can I ask a question, and maybe it is a later question. But what about the chemisorbed tellurium? Is that released with air ingression would you imagine? Would anybody know?

DR. POWERS: Well, what I know is that if you go through and just do the calculations, and you get air into the piping system, then that can happen and not happen, but you don't want to use the air twice.

The higher oxygen potentials, you do start turning any sort of telluride into TeO.

MR. GIESEKE: That's what I was wondering. That probably accounts for the 20 percent late in-vessel up here. I am jumping to the wrong column, and I apologize, but my mind went over there.

DR. POWERS: Well, that was the rationale that was used.

MR. GIESEKE: Okay.

DR. POWERS: And as long as I have got the floor, I might as well offer my two cents worth. I am on record in saying that I am totally uncertain about tellurium behavior under accident conditions, and did that in 1988, and nothing has changed.

My understanding of this great deal is that I am not a believer in the tellurium interacts with the clad hypothesis. I am a believe that the tellurium interacts with something until conditions get very oxidizing.

And I have somewhat persuaded myself that something is actually metals within the fuel, and those metals concentrations, those metal nodules go way up as we go to higher burn-up.

But what happens is that as you go to burn-up, you eventually overwhelm the ability of molybdenum to buffer the fuel oxidizing potential. And so a modest amount of oxidation of the fuel in these higher burn up cases will lead to tellurium release.

And if you look at tellurium inherent volatility, it is as high as iodine. I mean, it ought to come off like gangbusters. So the net effect of what I believe is the low burn-up fuel and modest

burn-up fuel, I predict essentially the same thing as the guy who says interaction with cladding. I just have it in a different position.

At the high burn-up, I end up with very high releases on tellurium, and the next question you have to ask is does it chemically interact with the surfaces, and a little bit along the transport path.

And that little bit depends on what you think the surfaces are. Now, the ground rules in our discussion last time were to look at a large break system. So we had relatively low transport path distances.

Nevertheless, when I look at those surfaces and PWR, I find that they are all heavily oxidized with a crud, which is primarily magnetite crud, and tellurium typically will not interact with magnetite to form iron tellurites.

It will go after any iron nodules that are buried down in there. We have to be very careful about PHEBUS tests, because they have used an inconel piping system, and so they have first of all very thin oxides, and a lot of nickel available.

When we have tried to do the calculations of the VICTORIA to get tellurium from 10 telluride or silver telluride, to subsequently react with the

surface for nickel telluride, it just does not do it.

And we just are not able to suck that out. I say, well, that is an interesting result, and I wonder if it is true. And you look at how they model the nickel telluride, it is very simplistic.

The model is a line compound, when in fact we know that it has a broad range of nickel chemical potentials that you can have, and they are just not taking that into account.

So it results in me being very uncertain the results of nickel, except to say that it is pretty clear to me that by any physics that I understand that if you go to high burn-up, the release fraction of tellurium goes up.

And when I look at especially the ground rules that are focused on large break LOCA, at low pressure there just is no chance to get the kind of near quantitative retention that we had in the original 1465 source term.

I also know that a lot of the retention -- the original version of 1465, it was sent out for review, and actually had a higher release of tellurium to the containment, and there was among the reviewers was a strong belief that retention was higher, the chemical retention.

And that was driven by some experiments that were done with pure tellurium vapor, and we just don't have tellurium vapor in the PWR case. Does that change if we go from silver cerium control rods to boron carbide control rods, and there seems to be a genuine drive to do that in the more modern PWRs? Probably.

MR. BOYACK: All right. Dave, where we are here is that I have gone ahead and made the same sort of comment here, and you will notice that we already had the type of process that we will follow invoked here, which is we will flag the number, and indicate the majority opinion, and one or more, or however many panel members take exception, we will go ahead and do that.

And what the more applicable value is, and to remind me to do that, I just put in green here so I -- I am color coded so that I can keep track of what this means, and I just put what that value was.

Is it still a range of 10 to 15 percent, or is there one of the values or the other, or have you changed your mind?

MR. LEAVER: Oh, no. I would say that reflects where I am.

MR. BOYACK: Okay.

MR. BOYACK: Any other comments?

MR. LEAVER: Let me just ask a question. Dana made a point, and that he is fairly certain of with regard to tellurium release is that for higher burn-up that it is higher.

Is that part of what is the thinking of other people on this release, and why you are thinking of the number of .3?

MR. BOYACK: Well, if it is, that is only a third of the core.

DR. KRESS: My thinking was a combination of that, and the fact that I think they set it too low in the original 1465. The reason that I thought it was too low was that many of the action frequencies, you get a lot of tellurium calculated to be released in fission product release model that doesn't tie it up with cladding.

You just (inaudible) and you get a lot of release, and so that was part of my thinking of it.

MR. BOYACK: Let's see. The barium strontium, the numbers did not change. Is there any need to go through those?

(No response.)

Okay. That moves us into the last three groups, and we will bring each of those up. We did

have a lot of discussion, but we were nearing the end of the meeting, and in the process of that we got to the discussion of the points listed.

But we did not come to the point of settling on values for noble metals, cerium group, and lanthanides. Now the e-mail from Dave Leaver said -- refers to noble metals, cerium group, and lanthanides.

These are undecided, and what we said in the summary table is that they will need to be revised to reflect the discussions of February 19th, and we have several slides addressing these three groups which I would like to present at the appropriate time.

So what I would like to do -- and you also?

MR. CLEMENT: I also have some slides.

MR. BOYACK: So why don't we take these one at a time. So instead of presenting all your slides, we will go first to noble metals, and we will have the slides presented by Bernard, and then we will have an additional discussion, and then see if we can wrap that up.

So the first one is noble metals. Dave, do you want to go ahead and present yours at this time?

MR. LEAVER: Yes. I have got these

results all on the same set of slides, and so we can put it back up more than once if need be.

MR. BOYACK: And you will also notice that in these discussions -- I think we were going pretty fast, and there really wasn't anything listed for needs, and if we do have needs, I would like to get those down also.

MR. LEAVER: Just to give a perspective, and I guess this is the poor man's version of Jason's nice poster board there that he did. The 1465 source term is based at least from a substantial core meltdown, whatever that is.

But I think we all kind of have a rough idea what that means.

DR. POWERS: The regulations requires a substantially challenging --

MR. LEAVER: Right. And in fact I believe the word substantial is even in the Code of Federal Regulations.

DR. POWERS: Yes.

MR. LEAVER: So the point is that this is not an accident in which you pop some clad. It is much more than that, and it is the GAP of early in-vessel release that is used in the radiological design basis portion of the application of this.

And 1465 says in a couple of places that the release fractions are intended to be representative or typical, rather than conservative, or bounding.

Again, I think the beauty is in the eye of the beholder, but this concept I think was in fact used, for example, in looking at this spectrum of accident sequence that was calculated by Brookhaven when they took release fractions that bounded perhaps 70 percent of the accident sequence, or something like that.

Finally, there is a statement in 1465 that release fractions are not intended to envelope all potential reaction sequences. So this is stated in 1465, and important to keep in mind, unless we think that is not the right approach, and then maybe we should suggest that they change it, but that is what is in there now.

Now, the French approach as I understand it is the GAP release that is used for the design basis for licensing French plants, and there is something called a reference source term which is GAP in-vessel and ex-vessel of these, and it is used as a basis for pre-planning actions on emergency planning.

And this reference source term is based on

enveloping severe accident sequences.

MR. CLEMENT: If you are looking for the exact wording in the reports, it is deterministic (inaudible) envelop scenarios, and it is not probablistic at that time, and the conservatism is in the scenarios.

And then you have some deterministic approach, because if you put everything on conservative --

MR. LEAVER: Okay. So just trying to bear this in mind, I made this table and we can look at the first four lines, which are the noble metal group, and then this is the cerium group, and then the last one are the lanthanides.

So just looking at the noble metals, what I did is I listed data which was easily accessible and understandable, and quantitative. I didn't list all of the VERCORS test data, but I did list HT-1, where you had releases at different points in the test.

For example, this column here is a release from the fuel. So for ruthenium, it was 8 percent, and for moly it was about .5. And then the release from the fermium you can see is significantly less ruthenium, and about the same for moly, and then from the thermal gradient tube even less.

And then finally from the loop even less. And then I listed the PHEBUS FPT-1 release to containment, and TMI release to containment, and SFD 1-4 release from the fuel.

So for the noble metal group -- and I have an existing 1465 here and then I guess in the interest of trying to come up with some numbers where we had nothing but question marks on -- or at least a spectrum of releases from individuals or from the panel at the last meeting, I got some numbers here which I am suggesting that we consider.

The numbers in bold if you will are different than they are in the existing 1465, and if you can see that. And just by way of observation on the noble metals, the moly in --

DR. KRESS: Are you suggesting that we write that group up, and --

MR. LEAVER: I am. I am. The moly in -- and we talked about that, although we seemed to in some cases think we should break up certain groups, and in others we were keeping the groups together, and basing the number for all of the elements in the group on a single element, which didn't make sense to me.

I think we absolutely should break them up. Otherwise, we are just masking important effects,

and we are doing the wrong thing, at least for some of the elements. Moly and technetium increased --

DR. KRESS: If you were to take the group and write the release fraction by the biological effectiveness --

MR. LEAVER: I think my next slide --

DR. KRESS: Okay. I'll wait until then.

MR. CLEMENT: I think it goes to --

MR. LEAVER: I don't think we need to do that, because it is not that hard if you run these codes, and it is not that hard to have -- I mean, you have all of the dose conversion factors in the libraries of these --

DR. KRESS: Well, the thing that is hard to come by is the fission product release model that you are going to use for each one of those.

MR. LEAVER: But if you have a release fraction, it is trivial to reprogram these codes to do these calculations to consider --

DR. KRESS: But you are reaching an assumption that the release fraction is the same for each one of those individually.

MR. LEAVER: When you group them you are, yes. And I think in some cases that it is pretty clear that these groupings don't make sense. In the

case of the noble metals, moly and technetium, the release increased by a factor of 80 and almost a hundred, compared to the existing 1465.

And that is based on the HT 1 and FPT-1. The ruthenium release was about the same as the existing 1465 if you look at VERCORS, HT 1, either thermal gradient tube or furnace release.

And if you look at TMI FST and if you look at VERCORS 1 to 6 in a qualitative sense, the observation that the low volatile deposit is very close to the fuel. And then the rhodium was increased by about a factor of four, and that is based on a comparison with the ruthenium release and VERCORS 3, 4, and 5.

And so with these observations, I guess what I am suggesting is the ruthenium remain the same, which is about .002, which is pretty consistent with the VERCORS release from the furnace, and the thermal gradient tube, and not substantially the same as PHEBUS, FPT-1, and I assumed FPT-1 was a factor of two higher.

And TMI was a factor of two higher, and PHEBUS, and that we increase moly and technetium, and I think we discussed this at the last meeting, and we have a somewhat less of an increase, and that we break

those up. Does anyone want a copy of that?

MR. GIESEKE: Yeah, I couldn't see that very well, and tell me again what your groupings were? You ended up with three groups? I can't see it and so I can't see what you are pointing at.

MR. LEAVER: I guess there would be three groups, although I am not necessarily proposing that we could group moly and technetium, but essentially we have really four elements with four release fractions.

It just happens that these two are pretty close, and we didn't even have a technetium measurement with HT 1, but we did in PHEBUS, and it was pretty close to moly. Can you see it?

MR. GIESEKE: Almost.

MR. LEAVER: I do have a hard copy of this

MR. BOYACK: Is that the entirety of the slides that you have there?

MR. LEAVER: Yes.

MR. LEE: Dave, can I ask a question?

MR. LEAVER: Yes.

MR. LEE: The way you group these, are these based on their similarities and behavior, and not only are you concentrating on the release fractions, but how their behavior is in the category?

MR. LEAVER: Yes. I think that in some of

the groups that is certainly appropriate, but I think -- for example, for these, I don't see any -- when you do the calculations, there is really no reason to group those.

You don't -- the only parameters that are relevant here for doing those calculations that we do for design basis accidents are the release fraction and the measure of biological effect, which is the dose conversion factor.

MR. LEE: We deal in individual isotopes, and even though it is grouped together --

MR. LEAVER: Right.

MR. LEE: -- we actually pick each number.

MR. LEAVER: That's right. You have a different biological effect from each of these, and when you get into iodine, then you have some different chemical forms, and that's right.

But here I don't see any advantage to grouping it. It doesn't make it -- the only thing it does is make it a little easier, and you only have to put one number in for all these elements for all the release fractions, and now you have to put in four.

But I think what we are trying to do with the alternate source term is be -- I would not say realistic, but be more realistic than what we were

based on the TMI or whatever it is., and the Reg Guide 1314.

And I think that this is a step in the right direction. It recognizes that we have really very strong experimental evidence that something is going on with moly and with technetium that isn't going on with other elements in this group.

DR. KRESS: Where is the rhodium numer?

MR. LEAVER: Well, it is not on this chart, Tom, but if I looked at VERCORS --

(Simultaneous conversations, inaudible.)

MR. LEAVER: Yes, here it is. If you look at VERCORS 1 to 6.

MR. CLEMENT: If you look at --

MR. LEAVER: If you look at ruthenium, and then you at rhodium, these are releases from the fuel, from a fuel pellet. Generally --

DR. KRESS: Are those the release fractions?

MR. LEAVER: These are release fractions. The rhodium is --

DR. KRESS: What is that number there?

MR. LEAVER: Right here?

MR. CLEMENT: It is released from the fuel.

MR. LEAVER: Released from the fuel, from this fuel pellet.

DR. KRESS: And if it is a release fraction where is the decimal point on that?

MR. LEAVER: There is no decimal point. This is 7 and 7 and 7.

DR. KRESS: It is not fractions percent?

MR. LEAVER: It is percent, yes. Sorry.

DR. KRESS: I was confused, because your title says fraction.

MR. LEAVER: I'm sorry. It is percent. So, I just observed that the rhodium release is up approximately a factor of four greater than the ruthenium release. So I just used that on this other chart for ruthenium.

And so what I am saying is based on the HT 1 release from the furnace, and from the thermal gradient, to this, and what we have in 1465, is about right.

DR. KRESS: Well, now you have to ask yourself what was the temperatures transient in HT 1 that was imposed upon this fuel, compared to what temperature transients that you may be dealing with in the reactor, because the temperature transient will affect the fission product release.

MR. LEAVER: Right.

DR. KRESS: And I wasn't sure what HT 1 was.

MR. LEAVER: It was hot.

MR. CLEMENT: HT 1 was -- it went to very high temperature.

MR. LEAVER: It was releasing at the higher temperatures for quite a while?

MR. CLEMENT: Yes.

DR. KRESS: You see, you have to ask yourself whether that is representative or not.

MR. LEAVER: Absolutely. If it was held at high release for I think a couple of hours. It is nice to go up to the prototypic ramp, but I don't know how long you should hold it up there to be representative.

You get a lot of good data out of it, in terms of release and being able to correlate your release fraction, but then you have to apply it to a real accident, and I don't think you just take the data and say that's what it is going to be for a real accident.

What I would do is I would extract a fission product release --

MR. LEAVER: Rate?

DR. KRESS: -- rate out of this, and put it on a linius (phonetic) form, and go back to a real set of accidents, and reapply it. And I am not sure if you would get the same thing.

But in terms of relative amounts, like rhodium relative to ruthenium, you might expect that relationship to hold. So if the ruthenium comes out to be .025, when you are taking the ratios, that might not be a bad idea.

MR. BOYACK: Well, when you are pushing it on HT 1, Bernard is really the right one to answer this question.

MR. GIESEKE: Well, then what we ought to do is have him show the view graphs that he has, and maybe we can continue to discuss it.

DR. POWERS: The one point that it is permeated on is the idea that you will get the right release fraction by going into a temperature scenario, and I think that is just wrong. I think what we are finding, and one of the most seminal results I think you get out of the PHEBUS results is every time you move that fuel around, and as it relocates and what not, we get bursts of aerosol generation.

What it is saying is that motions -- motions in the gas phase, and motions in the condensed

phase, are at least as important as temperature in determining the release fractions, and consequently I think when you look at temperature data, even if you scrumptiously follow the temperature ramp, you have to recognize that there are lower bounds on the release fractions.

Because if that fuel moves on you, or if the gas flow rates go up on you, that you will get higher release fractions, even though you have not changed the temperature at all.

DR. KRESS: That's a good point. We have no way of knowing when you factor that in.

DR. POWERS: You have to do the mental integration, and that means that when you look at furnace data that you have to recognize that if it is held for long times at temperatures, you have got to probably shorten that down, but then you have got to kick it back up, because the reason that it doesn't say at long temperatures in the core is because it relocates.

DR. KRESS: And that's what I do with my model here, I don't know how to handle this relocation, and so I arbitrarily hold it at a melt temperature a little longer than I would, and how I arrive at that little longer is sort of pulled out of

the air, but it is to help max some data where I did see relocation. It is to get a factor into it.

And I do that in my mind, but all I do is hold it at the temperature any longer than a real melt, a melt equator, and a situation in which to do it.

MR. BOYACK: Probably Bernard will talk about this, but this is --

DR. KRESS: Oh, this is HT 1?

MR. BOYACK: -- HT 1, and this is a temperature profile, and the time at different temperatures.

DR. KRESS: And the other question that I would have is what was the burn-up level on HT 1.

MR. BOYACK: That's available, too. I don't remember, but --

MR. CLEMENT: Burn-up level was 49.4.

MR. SCHAPEROW: There was one other idea that I have heard mentioned, but I have not or am not sure that I have heard it discussed a little bit, but we actually talked about it when we were doing the work on spent fuel pools a little bit, is that when you do get a relocation, you then have a geometry where you can't get flow through it, and you can't get things coming out of it is dull, and like up in the

bed, it is in a pile, or across the pile.

And so there is that possibility in addition for releases at that point from the geometry of it.

MR. BOYACK: Bernard, can you go ahead and show us the information that you have?

MR. CLEMENT: Yes. If you look at the biological effects box, and so for biological effects we fact we started from this table, and maybe it is too small. I don't know. But in fact (inaudible) is in the report, and in fact is reflected in the NUREG report (inaudible), and so you have several doses.

This is the table, and (inaudible) and has all of the elements, okay? And here you have the short term dose and the long term normalized lung dose. He is normalized (inaudible), and short term here. And the long term that is normalized is here.

So you see values of .1, .1, .7, 0.06, 0.02, 0.06, and so the biological effects are less than (inaudible) and cerium. If you look at ruthenium, you will find 0.3, 0.3, 3.0, and so the same as iodine for normalized lung dose, and 1.0 for normalized proper (inaudible) doses. That means the same as cerium.

So it seems like it is much more important

than the other biological collected. Rhodium is small; .21, .21, .28, .24. So that is maybe another reason for changing the grouping, because if we have a moly and technetium that are highly volatile, but much less biological effects than lanthanum, but is less volatile, and that is probably another reason to separate these in different groups.

So concerning the release, we get molybdenum, which is really volatile, and the release is (inaudible) to oxidizing conditions, to up to more than 90 percent in some LOCA experiments under oxidizing conditions. And such conditions looks like (inaudible).

And what is significant is there is a high retention above the fuels, but also in the fuel region (inaudible) move to the ruthenium, and it is probably (inaudible).

And rhodium releases are being measured as being weaker than ruthenium, because of VERCORS 4 and 5. So maybe (inaudible) recommendations, because this is what we use for our source term, and so I don't think unless we introduce the same thing that --

DR. KRESS: Suppose I -- well, let's take moly versus ruthenium. Now, the moly release is higher.

MR. CLEMENT: Yes.

DR. KRESS: And the inventory of moly is lower, compared to ruthenium. So the number of kilograms in a pour.

MR. CLEMENT: Well, for the radioactive isotopes?

DR. KRESS: Yes. And what I would say is if I take the release fraction, times the inventory, times the dose effect, I might get about the same thing as ruthenium.

So if I just let it have the same -- just call it ruthenium, and let it have the same release fraction, and the same inventory as ruthenium, then I may come up with the same number, and I think that was part of the thinking. But I don't know. You have to do the calculations.

MR. LEAVER: But in applying these things, it is not hard to get in the core inventory data, and we have codes now that can generate --

DR. KRESS: All I am saying is that you can get everything but the release co-efficients.

MR. LEAVER: That is the hard part.

DR. KRESS: That's the thing. And that's why you may tend to lump. You say that lanthanum has similar chemical characteristics as ruthenium, and

that is where you might expect release coefficients to be similar.

But since that is the thing that we can't get to, pull ruthenium with it, and we will factor in the fact that it has got a different inventory, and different dose calculations. So I think that is an argument to keep it lumped in with.

MR. LEAVER: That is probably the same thing that led to these groups. There is what, 7 or 8 groups, and they have been those traditional groups of --

DR. KRESS: At the time that we did them, we didn't have a lot of this release rate stuff. Now that we are getting more data, it may make more sense to separate them out.

MR. LEAVER: Exactly. Right.

DR. KRESS: But if you have enough data here to have separate release values.

MR. LEAVER: For certain elements, it is absolutely clear that something is going on that is leading to much, much higher release fractions, or lower, or whatever.

DR. KRESS: And if we have that data, and can translate it into a fission product release model that you can apply on a hope for basis, and if it is

a real accident, it may make sense to separate them out.

But at the time, we didn't have it yet, and we couldn't, and that's one reason why we grouped them.

MR. BOYACK: At the last meeting the question was asked of the NRC as to whether or not they would entertain more groups, and the answer was yes.

MR. SCHAPEROW: Sure. There is no magic in these groups the way they are grouped, and the impression that I got from what I had read is that these elements were believed to be chemical (inaudible), and so we grouped them.

(Simultaneous conversation.)

DR. KRESS: Once again I hesitate to take data from a specific test and say translate that directly into a hope release fraction for accidents for design basis, because I think you need to translate that data into a (inaudible) fission product release model, and then apply it on the same sort of design basis.

And I am not sure that you can do a one-to-one translation otherwise.

MR. LEAVER: I think that's right. I

guess what I was doing in the slide that I presented was looking for data that would suggest that what is in NUREG 1465 may be wrong, and there is a certain standard there.

And you don't want to just say, well, gee, I have got one result and it is a little bit different. So that means that NUREG 1465 is wrong, and let's change it.

But if there is a substantial difference, even without trying to apply to this data to some sort of empirical model as you say, then I think we know enough to say let's try to make 1465 a little more representative of what we understand today.

DR. POWERS: If I could touch on the groupings. The groupings that are used here are basically the MELCOR groupings, and I did those. And in fact the study that we put up there with respective to that is a study that I commissioned to help do those.

And a good question was posed are what are the important fission products, and the ground rules were thou shalt not create more differential equations than MELCOR's process is willing to handle.

And so there is come some computational difficulty in changing these things around when you go

to systems level codes. People don't like having lots of differential equations in these codes. And the way that they were set up was that -- in fact, MELCOR has 13 groups, but five of them are materials that don't have radioactivity compliments to them.

So they are primarily to affect the ursel (phonetic) physics, and what we did was go through and make arguments based on chemical similarity, and those are predicated by the statement that the reason we have different elements is the chemistry of these things are not similar.

And in fact I can make a fairly persuasive argument that grouping bromine and iodine is the most fatuous thing that on could possibly ever do, because they never behave the same on anything.

But nevertheless, if you are constrained by differential equations, you have that. What we did was say, okay, this chemistry is similar, and if one looks from a long ways away and doesn't ask quantitative questions.

And then we said we will use the dose effectiveness to define what are the representative elements of these groups, and then MELCOR -- the chemistry, for instance, in molybdenum is all dictated by the chemistry of ruthenium.

And similarly the chemistry of the actinide groups are all dictated by cerium. And the reasons for that are because of dose effectiveness arguments, and whether we knew the chemistry, for instance.

We are a little shy on neptunium chemistry, and so we chose to use cerium because we know more about it. So the complaint I would have about the groupings that we tend to forget was the representative element of the group is.

So when you look at molybdenum release, you get all excited. Well, we knew dam well that molybdenum behaved differently than ruthenium did, but ruthenium is the representative element.

Those get you in trouble. Again, one of the important things coming out of the PHEBUS program is that it appears there is some synergism between the release of molybdenum and cesium.

And that cesium-molybdenate may be the predominate form of cesium in the vapor phase in the piping system. So when you are doing systems level coding, and you want to reflect the chemistry of cesium, you have got to have molybdenum to react with it if you are going to do a good job on it.

And that is a problem, and we constrain it

to behave like ruthenium. The other thing to bear in mind is that PHEBUS is showing a lot more movement of ruthenium than we had ever anticipated before.

And quite frankly I don't fully understand it, and one of the things that we may not understand that has burned us a couple of times is that there is a cesium-ruthenate that is fairly volatile, and fairly active.

And again if high burn-up fuel takes you to the point that you no longer have the oxygen potential buffering of the molybdenum oxide, the you can get that cesium ruthenate.

DR. KRESS: So you would favor a finer ruthenate?

DR. POWERS: Tom, I want to say that there are 106 groupings, and I had great big guys who like to do coding threatening my life when I said 13.

MR. GIESEKE: Well, we have two proposals here so far. One says Dave wanted to group the first two, and then two singles, and there we have the same proposal; a double and two singles from two different independent --

MR. LEAVER: I think with regard to Dana's point that it is absolutely right if you are tracking these individually. But just from an application

standpoint for design basis calculations, this really -- whether you have 20 or 30 release fractions, or seven, it makes no difference to the computation time.

DR. POWERS: Well, that's because all you are doing is taking the numbers and running them through a spread sheet. If you ask the systems guys, they will yell at you a little bit, and they get visibly irked when you start playing with the code structure.

DR. KRESS: I think it is basically wrong to take the ratio of fission product release from the test and apply the ratio across the board.

MR. CLEMENT: These are the same values that we saw last time, and so I don't think it is the point of (inaudible).

MR. BOYACK: Let's give a moment to this and see whether there is a possibility of dealing with it. I am always mindful of the time, but it seems to me that the priorities are BWR and MOX. Those are the first two things.

The BWR I think is nice to have. Are you willing to cut into your MOX time?

MR. SCHAPEROW: No, I am not willing to cut into either of the other two. I would rather wrap this up. The others need attention, and we do need to

-- this is the last meeting.

DR. KRESS: Say to move on.

MR. SCHAPEROW: These meetings are a little costly, and very worth while, but costly. Typically about 50K a meeting.

MR. LEAVER: This whole thing of noble metals and the cerium group, and the lanthanum group, is fundamental to whatever we are going to do for BWR and MOX. And if you can't solve it for what we are doing here, high BWR high burn-up, I guarantee you that we are not going to figure it out for BWR and MOX.

MR. BOYACK: Actually, my approach was just to ask for a raise so that the meeting would cost more.

MR. SCHAPEROW: I think we are going to need time for the other two things.

MR. BOYACK: That's fine, but let me just ask, because I am not sure that this will take all that long. I would like to at least try it once. First off, would those be the three groupings, or is it something else?

I just took this off the last slide, and so I didn't enter anything earlier.

DR. POWERS: You are really down into

splitting hairs when you use split out ruthenium and rhodium, and then you have to worry about palladium as well.

MR. BOYACK: So would you group these two?

MR. CLEMENT: Most important is to --

DR. POWERS: You need to group the whole thing.

MR. CLEMENT: -- separate molybdenum and technetium from ruthenium.

DR. POWERS: You had better put palladium in there as well.

MR. BOYACK: What is the symbol for that?

DR. POWERS: Pd.

MR. BOYACK: Okay. Now, this is where I want to find out whether it goes quicker or so slow that we just have to give up on it, and come back to a single value, or toss up our hands, I guess.

The GAP release. You see the values that we are showing up above for the NUREG 1465 values, but would it still be zero for these?

DR. POWERS: My belief is that the GAP release on all of these is semi-volatile in a species, including cesium and iodine, and these would be the GAP, but based on the vapor pressure and the gas available, and it just does not change very much.

MR. BOYACK: Let's see, the thing that worked out pretty well last time is that as I quickly went through or around the table of the panel members in order, and just ask for the values.

And so why I don't do that now, and just change the order that I went in. So, Dave, early in-vessel for molybdenum and technetium. I am going to see if we have a doable thing here or not.

MR. LEAVER: I would go with a number that was -- oh, for early in-vessel?

MR. BOYACK: Yes.

MR. LEAVER: For moly and technetium?

MR. BOYACK: Yes.

MR. LEAVER: I would say .2.

MR. BOYACK: And for --

MR. LEAVER: These are not percents. These are fractions, right?

MR. BOYACK: Yes.

MR. LEAVER: All right. So, .2.

MR. BOYACK: Which is 20 percent, right?

MR. LEAVER: Right.

MR. BOYACK: And for ruthenium and the group?

MR. LEAVER: I would use the same number as in the existing 1465, .0025.

MR. BOYACK: Jim.

MR. LEAVER: But I don't see why we shouldn't pull rhodium out because I think there is a basis for saying it is a larger number. But I guess if we are compromising and trying to minimize the number of separate groups, then we could leave it in there.

There is no data for palladium by the way. I didn't see any data from any source on that.

MR. BOYACK: Jim, your thoughts?

MR. GIESEKE: Well, I could probably live with the .2 or something in that --

MR. BOYACK: Or something like that, right?

MR. GIESEKE: Yes.

MR. BOYACK: I will just do that.

MR. GIESEKE: It had said 2 to 5 percent, and that is 10 times that, but I see some reason to --

MR. BOYACK: I am going to let you come back at the end here and we will just go through around the table and do you have any comments on the second group?

MR. GIESEKE: These are so variable.

MR. BOYACK: You have to realize that I am pressured by time.

MR. GIESEKE: I think that .0025 might be a little bit too low, and I don't know where to go -- I might like to go a little bit above that, but I don't know how far. Not significant, but maybe double it or something. That's not a big deal I don't think.

MR. BOYACK: Dana.

DR. POWERS: I think you have to recognize two things. One is that you need to recognize that we are still dealing with a large break in the -- is it a large break?

MR. SCHAPEROW: I don't think it needs to be large. Just medium would be all right. Something that produces low pressure.

DR. POWERS: Low pressure with high flows. So what gets out of the fuel gets predominantly out of the system, and maybe get reduction to DF-2 along the way.

And then I am supposed to integrate in three low burn-up -- I mean, two parts low burn-up and one part high burn-up. So I have to dream up a release fraction, and it seems to me also that just knowing a little bit about how the VERCORS experiments that give you the high release fractions were done, and they tend to give you a higher release fractions because of the single particle things.

It seems to me that we are looking at release fractions of molybdenum, and because it is insoluble in the fuel, that maybe you are running around 20 or 30 per -- 40 percent, let's say, and upper bound from the fuel that is high burn-up, and much lower for the lower burn-up fuel.

And you divide it by three, and you come up with maybe a .15, and you divide it again by two.

MR. BOYACK: .075?

DR. POWERS: No, so it is .15. I have already built in my --

MR. BOYACK: And the second group?

DR. POWERS: In the second group, this really gets to be a mystery, because we don't fully understand what is going on, but I think I would go along with Jim. I would at least double it, and maybe even go to one percent.

MR. GIESEKE: As I think about it, I would like to go a little higher than that.

DR. POWERS: We would go up to one percent at least to reflect it.

MR. GIESEKE: I would feel better with that.

DR. POWERS: Yeah, I would go along with him on that.

MR. BOYACK: Okay. Tom.

DR. KRESS: Well, if we are going to separate out the moly, I think it is like an order of magnitude higher than the barium and the strontium releases.

And since we had .02 for those, I would go along with the .02 on that. Well, the .025 is probably about right, but I think I would stick with that.

MR. BOYACK: Bernard.

MR. CLEMENT: I think we would stick with these values, because last time these values were uses for GAP releases, and then we did not make any distinction between the three last or the -- well, the early in-vessel, and the vessel release, and 90 percent from molybdenum-cesium has been measured and VERCORS also.

And with such releases, we get .7 containment. But you can -- well, for a total.

MR. BOYACK: And is that for a full core burn-up? I mean, would it be a third of that, or is this --

MR. CLEMENT: Just some early in-vessel and ex-vessel of .7.

MR. BOYACK: Oh, I see.

MR. CLEMENT: For molybdenum and technetium.

MR. BOYACK: Okay. So this is like --

MR. CLEMENT: And for ruthenium and rhodium, we should take the values quoted for ruthenium here, .02, because with rhodium, there is a more important release of rhodium, but the biological effect of rhodium is very small. So it is better to take the .02.

MR. BOYACK: And that includes the ex-vessel then?

MR. CLEMENT: Yes. The next vessel, back to you, Jim. Sorry, Jim; Dave, David Leaver. What we are going to do is after we finish this table, we are going to take a break.

Then I am going to talk to Jason a little bit about how to proceed on this, and particularly since we have got such a range of values.

MR. LEAVER: Bernard, is your number of 70 percent to containment, this is an accident?

MR. CLEMENT: This is a hot leg break, large break.

MR. LEAVER: This is an ex-vessel accident, and the core comes out of the vessel? You are not being that specific, or --

MR. CLEMENT: No, it should come out or not.

MR. LEAVER: But, I mean, you said that it included ex-vessel up there. So I am assuming that in terms of --

MR. CLEMENT: Well, when we discussed prior in the meeting what you put in ex-vessel or in-vessel, that's a complicated matter, because if you just look at the phenomena that are purely related to what happens ex-vessel.

So that's why I don't make any distinction between these two.

MR. LEAVER: Well, I understand, but I would say the ex-vessel is the same and make it .2, and that is a real kind of pulling a number out of the air. But NUREG 1465 did have the same fraction for in-vessel and ex-vessel.

So I guess that is probably not a bad number, because of a representative or typical kind of a thing.

MR. BOYACK: And for the ruthenium, Dave?

MR. LEAVER: The same. Make it the same as the early in-vessel, .0025.

MR. BOYACK: Okay. While I have got you here, late in-vessel?

MR. LEAVER: Well, the ruthenium would be higher. The late in-vessel, you are going to get much higher oxidation potential because you are going to have air.

So that is going to increase the ruthenium release.

MR. BOYACK: Well, that's maybe not fair, and so I am going to let you think about that, and we are going to go back to ex-vessel.

MR. GIESEKE: You didn't log in that .2 for me in-vessel, early in-vessel.

MR. BOYACK: That was just a double asterisk, Jim, or a double slash.

MR. GIESEKE: Put another double slash here, and then down for -- I think it should be something higher than the .01 that I had before, but it is really fuzzy where to go with that. Let's double it and say .02.

MR. BOYACK: Okay. Dana.

DR. POWERS: Well, the molybdenum, the predominant way of releasing things ex-vessel is melt concrete interaction, and we have always doped our melts with molybdenum. So I have actually watched as molybdenum gets released from that, and it is not very extensive.

It has always amazed me how little it was released, even though we get it every opportunity to be released. So to my mind, the release fractions on molybdenum are on the order of -- oh, I will say 2 percent, 0.02.

Now, the one area where our modeling of melt concrete interactions in the release fractions there gave distinct under estimates, compared to the experiments with ruthenium.

And that was a singular puzzlement to me why we would miss so badly, and we missed big time. We would calculate release fractions 10 to the minus 6 during the melt concrete interactions.

And in the tests, they would measure on the order of 1 to 2 percent release fractions. I believe I attributed it to the alkali metal ruthenates coming off, but I have never gone back and modified the code to see if that would work.

So I am going to go along with Jim's two percent release there. It is one area that I would just definitely not trust the codes, because we just missed big time on that.

MR. BOYACK: Okay. Tom.

DR. KRESS: Well, I agree with Dana. What we are talking about is -- or at least for melt

concrete interactions, and I don't know how to deal with that other than look at a model like VANESSA, and/or with the data from the tests.

And as best as I remember, for the moly, VANESSA told you one thing, and the tests told you something else. I think if you made the calculations, you would get something like a .2 using VANESSA, and with the tests, you get an order of magnitude lower, like .02.

I am more inclined to go with .02, because I think there must be something wrong with the calculations.

MR. BOYACK: Okay. And ruthenium?

DR. KRESS: For ruthenium, whether you are bubbling up to the metals, and get the CO2 and the H2O oxidized first, and then it hits the ruthenium, I don't think it goes through the melt first, and then hits the metals.

I am not sure of this, but I would keep the ruthenium at a fairly low release, and I guess .02 would be a pretty good number.

MR. BOYACK: And Bernard, I've got yours already. Okay. Let's go to the late in-vessel now, and then take a break. Again, is it easier by taking a longer time, Dave?

MR. LEAVER: What's that?

MR. BOYACK: I gave you a little more time and I was wondering if it got any easier now for the late in-vessel.

MR. LEAVER: Well, not a lot easier. I think we need to reflect the volatility of the oxidized ruthenium and which you would tend to get, or more likely to get this late in-vessel. So I guess I would put a higher number for the ruthenium.

DR. POWERS: Like zero?

MR. LEAVER: Well, definitely higher than zero, but higher than what we have for -- say for early in-vessel.

DR. KRESS: Are you comparing late in-vessel with air-ingression?

MR. LEAVER: Yes.

DR. KRESS: I have been equating it with the revaporization from the --

MR. LEAVER: Well, I guess it is both.

DR. KRESS: Well, I am assuming that the air-ingression frequency number is low enough that I don't need to factor it in.

MR. LEAVER: What do you mean?

DR. KRESS: Well, I don't think it happens very often because you have to have two holes.

MR. LEAVER: Yes, you do. You do have two holes. I mean, we have some kind of a break as an initiating event. I mean, we can debate the size of it, but it is inches, and a six inch break medium LOCA. So you have a whole up there.

And then to get a lanthanum vessel release, I think that means that part of the cores come out of the vessel, and you have a hole, and so you have an entire containment full of air, and other things, and I think it is going to go up into the core.

And I think you are going to get some chemical forms of --

DR. KRESS: So you are going to factor into your thinking a fraction of the core that is still in the vessel?

MR. LEAVER: Right.

DR. KRESS: And then use that fraction, plus some different release model?

MR. LEAVER: You're right. Some portion of the core that perhaps is still intact, and there is some debris, and you are not cooling it very much.

I mean, you may have some steam cooling, and so you are probably going to have some additional melting going on and relocating, but most or probably

more than half of the core has done what it is going to do. But there is probably some portion of the core that is still --

DR. KRESS: Yeah, I have been thinking mentally when I make my calculations that the substantial core melt probably means about 50 percent.

MR. LEAVER: Right. That's probably right. So there is probably 50 or maybe 30 percent, or 40 percent of the core that could be affected by basically some new phenomena with two holes. But you are right about your revaporization. That's true as well.

DR. KRESS: Well, in my mind, I was going to give it zero revaporization. I figured that it is already gone, and it didn't deposit in the first place.

MR. LEAVER: Well, it could continue to heat up.

DR. KRESS: Yes, but I would just as soon none of it got -- it is a hot leg break, and low pressure, and the stuff shot right out.

MR. LEAVER: Yeah, I think for ruthenium that it seems to me that we ought to reflect the potential for the oxidized form of ruthenium, which is much more volatile.

So maybe a number like .1 for ruthenium, and then for molybdenum and technetium, I guess probably zero.

MR. BOYACK: Jim.

MR. GIESEKE: I agree with zero, as that is probably close enough. I don't know if that is going to be 10 percent that we are looking at. If we look at the other numbers that I have put across there, I can't see that late in-vessel is going to be greater than early in-vessel in my mind.

It would be spurred on by air ingression, and so I am going to go with .1, or I mean .01, again recognizing that there will be some, but not a whole lot.

MR. BOYACK: Dana.

DR. POWERS: I will communicate a distinction that I make in my mind. I agree with Tom that for a large break LOCA, about half the core is involved in the in-vessel portion of the release, and the remaining half that is left there to be exposed to the air, but I believe that takes place fairly quickly.

And I have been putting that into the ex-vessel period, that two hour period that we allow for ex-vessel release, because it happens very quickly;

whereas, the late in-vessel is spread over 10 hours.

I don't think it takes 10 hours to pull the remainder of the fuel out. So the late in-vessel release to my mind is predominated by a revaporization of the suspension.

And in that regard, what you get in the late in-vessel depends on how much you released from the fuel, all the fuel, and that is both the fuel during or involved in the in-vessel, and fuel that I have involved in the ex-vessel, that goes on the piping system, and then subsequently comes off, over a retracted period of time.

So my belief is that a substantial amount of the moly actually deposits and gets released and deposits on the piping system when it has a chance.

Now, there is not much chance in a large break LOCA and so that fraction deposition is small. As a consequence, I come up with a 5 percent release of moly from the late in-vessel, and a .01 for the ruthenium.

In other words, I took the total release to be about twice what you got on the in-vessel, and that went on the piping system. And then I just subsequently pulled it right back off as soon as I exposed it to the air.

MR. BOYACK: Tom.

DR. KRESS: I think it is almost exactly like Dana has described, and I am very persuaded by the use of the same numbers for both of them.

MR. BOYACK: Does the ex-vessel take you all the way?

DR. POWERS: Interestingly -- I mean, what has always interested me about the releases is the long term -- when we do melt concrete interactions and when you run them for long periods of time, and I mean multiple hours of time.

The one thing that continues to come off, no matter what the temperature is, and you can even let the thing solidify, is tellurium. It always comes off, and it comes off over the entire period of time.

It is the one thing that I am very confident that we can calculate the release fraction well, ex-vessel, and I can't calculate it at all in-vessel.

DR. KRESS: It is one if it is a fraction of the core in ex-vessel.

DR. POWERS: Yes, but it is spread out over --

MR. BOYACK: Why don't we take 10 minutes.

MR. SCHAPEROW: Can we make that 15.

MR. BOYACK: Not if you are leaving.

MR. SCHAPEROW: Why?

MR. BOYACK: We need to talk. Well, we will leave it at 15.

(Whereupon, the panel meeting recessed at 11:02 a.m., and resumed at 11:29 a.m.)

MR. BOYACK: Okay. We are ready to continue now.

DR. KRESS: Do you need a gavel?

MR. BOYACK: Actually, usually what I start doing is saying something like Jim Gieseke is now holding up the meeting.

MR. GIESEKE: Again.

MR. BOYACK: Again.

DR. POWERS: You think he will feel guilty about that, and he will feel a sense of pride.

MR. BOYACK: I stopped Jim, but I didn't stop you.

DR. POWERS: And equally well I do feel a sense of pride. Then I can say those guys at Las Alamos, they can't get anything done, Jason.

MR. BOYACK: Okay. Charlie Tinkler is here, and I am just going to give him a quick reprise so he can take a look at the table, and see what we have done.

We have divided the noble metals up into molybdenum, technetium, ruthenium, and rhodium. And the order here is that we started with -- well, you can see the order. David Leaver, Jim Gieseke, Dana Powers, Tom Kress, and Bernard.

A double slash means that it is just the same value that is repeated. So this gives you an idea of the spread of values. I have mentioned that the way that the panel has operated on this is that before they have given their values, they generally have given a pretty good summary of what it is that they were thinking that led to the values.

And so in the transcript, we will have that information prior to the numbers. So what we are going to do now is the following. We have two more groups.

MR. TINKLER: Brent, can I just say something real quick?

MR. BOYACK: Please.

MR. TINKLER: Because of the work that we had under way, I was actually in a position where I had to kind of provide a source term not too long ago, and I wanted to reflect the workings of this group to bring to bear our best understanding of this.

And when I went through our group as we

define them in 1465, and I came to the noble metals, to reflect on your past deliberations, I didn't separate out the group.

Now, I am not suggesting that you shouldn't as you have, but what I did is that when I looked at this, I considered the views that you expressed previously, and this group as a group, more or less had specified a higher noble metal release, on the order of about 2 percent to the containment.

I discounted the molybdenum because of its lesser radiological significance, but we would have no problems with breaking up the group that way, but just to give you an idea of when forced to look at what the group has discussed, I ended up with a release fraction of about 2 percent to the containment valve, and it was kind of bimodal.

There were people that were in the two percent, and there were people that were arguing for lesser than that. And the same sort of thing, the same sort of discussion produced a bimodal distribution in the cerium group and the lanthanides as well.

And in those two groups though, when I look at what this committee discussed for the cerium and lanthanides, I ended up with the smaller of these

fractions, the .1 percent for both the cerium and lanthanides.

I don't say that to influence you, because I know that it won't anyways, but that's what it was in past deliberations. So I think either way is workable, if you either split it, or not. And I am not sure that some of those radio nucleates make that much difference, even with an increase in the fraction. It didn't seem to.

MR. LEAVER: Well, yeah, some it won't, and some it will. I think it is fair to say that we are trying to call it the best that we can, as opposed to saying this is important, and this isn't, and so we will do that here, and something else here.

MR. TINKLER: And actually when I was forced to (inaudible), I dramatically increased the tellurium release. It seemed as if the consensus view of this group was that the tellurium release, based on PHEBUS data, was more attuned to the cesium and iodine releases.

So for the purposes of my use, I assumed that the tellurium release was roughly equivalent to an average of the cesium and iodine use. And I was doing a scenario with specific calculations. So based on my scenario and specific calculation, I just

believed it was midway between the cesium and iodine.

MR. BOYACK: Let me just ask one more thing to wrap up this work. What we will do is we won't go any further on this particular table, except just to ask is it really indeed just a generic statement and any more data?

Is there something specific about this grouping that there are needs that we have? Any comments on needs on either in-vessel; is that early in-vessel or ex-vessel?

DR. POWERS: What I think we need for the noble metals are the data that Tom can invert and put into his model for calculations, which means that he needs release at various fractions at various times, and at a few different temperatures.

DR. KRESS: That's right.

DR. POWERS: So he needs data that has 2 or 3 plateaus, or two or three different experiments at different plateaus.

DR. KRESS: I can use the one test as long as it has got plateaus.

DR. POWERS: And by distinguishing the two, he needs data for both molybdenum and ruthenium.

DR. KRESS: Yes, exactly.

DR. POWERS: What I think he really needs

is palladium, but he doesn't agree with me on that. You are going to learn to love palladium when we go to carbide coded fuels.

MR. BOYACK: So you need to distinguish between -- was that ruthenium and what?

DR. POWERS: Molybdenum.

DR. KRESS: And these transient data I need, and sometimes it is hard to get that. I need the temperature transient, and then you superimpose all of that to the release fraction transient as a function of burn-up.

MR. BOYACK: Would the same statement apply to the ex-vessel?

DR. KRESS: The ex-vessel is --

MR. BOYACK: Any comments then about the data needed for ex-vessel?

DR. POWERS: Well, in ex-vessel, we actually look the releases, and actually predict them, and we actually have datasets for all of them.

MR. BOYACK: Because you really don't need it.

DR. KRESS: And I think you could mine what is existing.

DR. POWERS: What people really need to do is to go back and correct the code to handle the

enhanced ruthenium release, is what really needs to happen.

MR. BOYACK: Okay. Thank you. Now we go on to the cerium group. Jim, you are going to be on the hot spot here.

MR. LEAVER: Could I put this back up just to give a suggestion here.

MR. BOYACK: Go ahead.

MR. LEAVER: The cerium group is -- I have got three elements here; cerium, plutonium, and neptunium. That's a complete list based on the elements that are in this group in 1465, unless the noble metals, where I had left out palladium. But there was no experimental data on that.

So again this is just a VERCORS HT 1, and there is a fuel release, which for cerium is .05, and then neptunium is .07, and then from the furnace it is zero, and of course zero for the internal radius, and zero for the loop.

PHEBUS, there was no cerium measurement, and there is a plutonium measurement, which is three zeros and a two, and then neptunium is around 1 percent.

There is a cerium measurement from TMI, and SFD 1-4, and it is pretty low, and plutonium is

even lower. So on the basis of the relatively high release of neptunium from FPT-1, close to one percent, what I have suggested is that we separate that out, and make it one percent.

And for cerium and plutonium, on the basis of the furnace and the thermal gradient tube releases, which are essentially zero for HT 1 and the relatively low releases that were measured for TMI and SFD, that we retain the 5E minus 4 release fraction from the existing 1465 for cerium and plutonium

MR. BOYACK: Bernard.

MR. CLEMENT: Yes. Quickly, if I have come to the same conclusions under consideration. Again, for the biological aspects on the same table, on the next view graph, where we can see that they are all important in terms of biological aspects, depending on the (inaudible).

For instance, cerium that is compared lung dose, and cerium as compared to latent cancers, and neptunium is also important. It is important if you have a good treatment of all these three elements.

Then if you look at release, and if you look at HEVA, HI/VI, and VIS, also. So cerium is released and is enhanced in reducing positions, and that is continued to be brought forth in HT1 and SDI-5

assigned to a certain percent, and (inaudible) -- and so just by making a mixing of all of these experiments, this is what you get.

So a high release for neptunium and cerium, and then for plutonium, you don't have exactly the same values as they did, and so you have 2 percent for neptunium and cerium, and .2 percent for plutonium in the containment.

So the values are the same as we are given during the last meeting, but there is some more explanations while we arrived at this values.

MR. SCHAPEROW: Is plutonium in more quantity than uranium? Uranium. And I remember at one point in the FPT-4 that we were really worried about getting a lot of uranium out, like one percent, and there is a big number, and that was an issue for the filters.

And one percent doesn't sound like a big number for something so heavy like that.

MR. CLEMENT: There are two things. They are what you have taken for reevaluation, where you have a very release of uranium, but also this one, but with a very high retention in the upper plenum of the reactor vessel.

Then when you look at the VERCORS

experiments, the uranium is different than neptunium and the plutonium. The plutonium is small, and less for the neptunium, and you can look at the results, and if you look at the new reevaluation, you can devise the calculations with much higher values, but we did not observe that.

DR. POWERS: I mentioned last time that in a famous laboratory in Northern New Mexico, where they do great science, and the one in the middle of New Mexico only does engineering.

And they are a bunch of handbook engineers. All the great scientists are all up in Northern New Mexico. He is buying me lunch after this.

MR. BOYACK: I'm glad that you finally have come to realize that.

DR. POWERS: Well, I have always realized it. They had been looking at doing some interesting work on looking at plutonium release during vitrification of nuclear waste, and things like that.

And what they had speculated about was that plutonium could form vapor phase hydroxide, and in the course of doing the work, first of all, they did succeed in identifying the trioxide of plutonium and it is the counterpart of UO3 that is responsible

for so much of the uranium vaporization.

And people for a long time had speculated that PuO3 probably didn't exist, and they did find it, although it is not as stable as the uranium trioxide.

And consequently because there was slower stability, you would expect plutonium releases to be somewhat lower than uranium releases under these oxidizing conditions.

And somewhat higher under reducing conditions, and I think that is what you observed. Similarly, they identified an oxihydroxide vapor species, which enhances the volatility of plutonium substantially under the vitrification scenarios.

And I think it is probably more important if we were looking at a pressurized sequence. Here we are looking at an unpressurized sequence, and so I think it is the vapor phase hydroxide that is not so important.

But what they are finding, I think, on the vaporization, the stability of the trihydroxides is completely consistent with what Bernard was reporting on the relative volatilities.

And then suggest if we look, we would probably find a neptunium trihydroxide. But as far as I know, no one has ever found that.

MR. BOYACK: What I would like to do -- well, first, are there any other comments before I go around the panel and select numbers?

MR. GIESEKE: Yes. I think you have had two different suggestions to break this down.

MR. BOYACK: What are they?

MR. GIESEKE: Well, the first was to combine them differently wasn't it? They were going to put plutonium separately. Look at his last slide.

MR. CLEMENT: To group neptunium and cerium, and separate plutonium.

MR. GIESEKE: Yes.

MR. BOYACK: Well, if somebody will tell me how to group them, then I will do that.

MR. GIESEKE: Did you suggest this grouping?

MR. BOYACK: Maybe I just wrote it down wrong.

MR. LEAVER: I suggested that.

MR. BOYACK: That's right.

MR. GIESEKE: And what I am saying is that the French had a different suggestion on the grouping. You had better deal with the groupings before we deal with the numbers.

MR. BOYACK: Surely.

MR. GIESEKE: So, J.G. is the first name that I have up there, and so do you want to tell me?

MR. GIESEKE: How to group them?

MR. BOYACK: Yes.

MR. GIESEKE: Well, I don't know, but it seems to me like the data would suggest that they be grouped differently than what you have there. I think, even looking at Dave's data, and I don't know if you want to put the --

MR. BOYACK: Dave, any comments?

MR. LEAVER: Well, there was no plutonium measurement in the VERCORS HT, and the cerium release, when you looked at the release from the furnace or from the thermal gradient tube, was quite low.

And then if you look at where you do have measurements for plutonium, where you have one in FTP-1, and you have some in an TMI measurement, and SFD-4 measurement, that was also quite low.

So I just did not see a basis for changing the cerium or the plutonium release fraction that is in 1465. So that is why I left them as a group, because it is both as if they are the same number.

For the neptunium, there is a measurement, an FTP-1, that suggests that the very low release fraction is in the existing 1465, and is really not

right. So I suggest that we pull that out, and that's why I did that. That is the main difference, I think.

MR. CLEMENT: There is always some difficulties when you pick up some experimental results, and where you don't measure the same thing. And sometimes you have got the release from fuel, and sometimes you have got it somewhere after it has escaped from the fuel, and for all these lower elements, you have some retention.

MR. BOYACK: You do.

MR. CLEMENT: Yes, you do, and so that is what we have made for making our evaluation, that it was made without complicated calculations. It is just to come back from a release from fuel, and apply roughly the same retention for all these elements.

And, for instance, for cerium, I have got at least a VERCORS 4 experiment, and the HT 1 was 5 percent of cerium release; and for the HI experiments, it was a little maturation with 10 percent.

But it looks like it is more close to neptunium than from plutonium, and the complete analysis of all of the experimental results. You should just look at what is arriving at the thermal gradient tube level, and there is already some retention from before.

And you apply your factor, and in that case, you apply the same retention and factor for all these elements, which is 80 percent retention. And in some cases, when you look at what is in the VERCORS thermal gradient tube, you have already this retention.

And to look at the release from the fuel, you don't have this retention to take care of that.

MR. LEAVER: I guess I felt that the release from the furnace or from the thermal gradient tube was a more appropriate measure to use as a release to containment, because you are going to have that kind of phenomena in a damaged core.

MR. CLEMENT: And in some cases, in the tables for experiments, as in VERCORS, in some cases you have a measurement that is from the fuel that is accurate, and in some other cases the measurements are more accurate.

So you have to mix all of that all together, and come out to the correct values.

MR. BOYACK: Tom any comments on which way to group these things, or just to leave it as a single group?

DR. KRESS: Well, the data that I am familiar with, the release of neptunium is about 5 or

20 times that of the cerium release rate, and the molybdenum release is more like five times.

So the question is whether that is close enough in terms of biological effectiveness and inventory to tellurium to group them together, and I'm not sure whether I can do that or not.

But if we are going to have different groupings, I would separate the neptunium out by itself, and I would have cerium, plutonium, and neptunium as three separate groups, because their release rates are considerably different.

The plutonium in my experience -- and I have very very little with plutonium, is a lot like cerium, from the amount of fraction release. But I have very little experience with it.

MR. BOYACK: Well, if you came to that scientific lab in Northern New Mexico, maybe you can get more of that information downloaded. That was totally wasted information. Ignore that.

DR. KRESS: Dana doesn't ignore that.

MR. BOYACK: Did you have any comments? Are these the three groups, and if so, we can get the numbers quickly, or what?

DR. POWERS: I'd make that as one group.

MR. GIESEKE: I think if we can't decide

how to break it up, you might as well leave it in one bunch.

DR. KRESS: The release rates are low and they are all within a factor of about five of each other. So in my experience --

MR. LEAVER: I don't think they are within a factor of five. I the neptunium is -- well, it is a factor of --

DR. KRESS: At most, it is factor of five.

MR. LEAVER: Well, it depends on -- I mean, if you look at -- you have that PHEBUS measurement for neptunium that is pretty close to one percent. In my mind, it is kind of hard to ignore that.

Whereas, for cerium and plutonium, if you don't take the release right from the fuel, which is what Bernard is saying, and if you look at, say, release from the furnace, or from a thermal gradient tube, for HT 1 here is cerium.

And I am assuming that this is a very low number, because you had a five percent release from the fuel, and --

MR. CLEMENT: Either number is above -- either low release or above detection limits.

DR. KRESS: What I would like to look at

is the total release from the fuel.

MR. LEAVER: But I don't think it is appropriate to take the release from the fuel and say that is what we are going to put in the containment for these low volatile elements per 1465.

MR. GIESEKE: I don't think we are saying that.

MR. CLEMENT: No. What we do is we don't put release from fuel and put it into the containment. We take release from fuel, and we don't have a measurement of the deposit.

We say we are such a regular retention, and that is 80 percent.

MR. LEAVER: But one could argue that the 80 percent is too low for these elements.

MR. CLEMENT: It could be too low. That's right. It could be too low.

DR. POWERS: You are going to have a hard time arguing that (inaudible) --

MR. LEAVER: You mean retention of the RCS? That's not what I am talking about. I am talking about retention in the vicinity of the core.

DR. POWERS: Even there, you are going to have a hard time making --

DR. KRESS: But these are low volatile

materials especially, and you are just not going to get much retention.

MR. LEAVER: Well, I think you have seen that. You have seen that in VERCORS, and even in VERCORS 1 through 6, you had retention very close to the fuel, or at least that is what your slide said. If you didn't have that data, and you just have that release from the fuel.

MR. CLEMENT: You have ways of affecting values, and in fact (inaudible) cerium is around 10 percent, and you if you would just come back to this experiment results, you should have the tendency to put only five percent for cerium, and with two percent factors and to ensure that it is about one percent containment.

Because when you look at the experiment results, you get less for cerium than for neptunium. Probably they wanted to make some kind of grouping with the same values.

But when you look at these results and from when we analyze it, the cerium (inaudible), and it is not so easy to --

MR. BOYACK: Okay. One group, two groups, five groups.

MR. CLEMENT: More than one.

MR. BOYACK: Okay.

MR. LEAVER: Well, in FPT-1, you had -- you see, here is my reality. We have to move on. You had a factor of 60 difference in the release of neptunium to plutonium, and neptunium was 60 times greater release fraction for neptunium versus plutonium in FPT-1. How can we ignore that?

MR. CLEMENT: Yeah. In FPT-1, most of the FPT-1 was conducted in the oxidizing conditions.

MR. LEAVER: And we don't have the FPT-2 results.

MR. CLEMENT: Yes, and in that the high part of the transients was under oxidizing conditions that you may take into account sequences where you are, and the reducing conditions where tellurium is released, and for that reason, you have to increase the tellurium release.

You cannot take the value measured from the PHEBUS. You have to increase it.

DR. KRESS: In my model, I have got significant release data for cerium put into it, but I don't have much release data for plutonium, although it is generally what I have that indicates it is less than cerium.

So I just lump the two of them together,

knowing that that would be a bit of a conservatism for the plutonium. So when I group mine, I group the cerium and the plutonium together as one group.

And with the neptunium, I separate that out, because it has the higher release than either of those two in my sets of data that I keep looking at.

So I lump them together and mainly because I didn't have the data, the transient data for plutonium that I could use to put in my model.

MR. BOYACK: And you still don't have that.

DR. KRESS: No, I still don't have it. I know that it is generally less than the cerium. That's about all of it.

MR. BOYACK: Is it essential that these things be -- is there any end usage of these source terms that in a sense provides or places high value on breaking them into parts? End-usage?

MR. LEAVER: Well, if there is a difference in the release fractions, then I think we should break them apart because otherwise you are going to skew your total dose one way or the other.

DR. KRESS: The other thing is that it depends on what plutonium that you are talking about. There came be huge amounts of plutonium ina pool,

compared to the inventory of cerium.

MR. CLEMENT: Yes.

DR. KRESS: And so you have to be kind of careful talking about its release fraction.

MR. LEAVER: Plutonium?

DR. KRESS: Yes, because when you measure that by an inventory, you can get some really big lumps.

DR. POWERS: Well, with a high burn-up fuels, your plutonium inventory is actually pretty good. I mean, you have got several thousands of kilograms in a high burn-up core.

MR. BOYACK: Right.

DR. KRESS: He is talking about things like cerium, and there is a lot of it in there.

DR. POWERS: That may be the biggest reason to split them apart; is that the inventory accumulates so badly than plutonium. Of course, it accumulates for plutonium, too.

DR. KRESS: But when you split them apart like that, then you need to know something more about the release fraction than I know, and the only way I know to get that --

DR. POWERS: For plutonium?

DR. KRESS: Yes. And the only way I know

to get that is to have a test on it.

MR. LEAVER: We have three measurements.

DR. KRESS: Yeah, and most of that would help if I had the thermal transients, and if I knew that the tests were.

MR. LEAVER: We have three measurements for plutonium; FPT-1, PMI and SFD 1-4.

DR. KRESS: Well, I don't count PMI. That is an experiment and is not well characterized. But you can't just --

DR. KRESS: But once again, I hesitate to say that and say that is my release fraction. I would like to convert it into a model and then go through the full core, and where I can count in things like height that the core is melting, and height of that, and that sort of stuff.

So I hate to go to a release fraction a test directly --

MR. LEAVER: Yes, and I would agree with you on that. I guess it is just --

MR. BOYACK: Well, I am going to try it this way. Jim.

MR. GIESEKE: Okay. You want me to go?

MR. BOYACK: Yes.

MR. GIESEKE: Do you want me to lump them

all together in numbers across the different release the different release times, which are zero, one-and-a-half, and zero. How do you like that? I am lumping them all zero, one, and a half-zero, from the GAP --

DR. POWERS: I assume you are talking percent and not release fractions.

MR. GIESEKE: That's right. Okay. Are we going to talk fractions?

MR. BOYACK: Well, if you have got them, I will take them, but otherwise, if this is all the place that you want to go on that --

MR. GIESEKE: Well, I am just throwing that out there for everybody to shoot at.

MR. BOYACK: All right. If you broke them up, what would you do?

MR. GIESEKE: Well, I have seen some conflicting information. I think I would go with -- well, I would do that for cerium. From left across, I was going zero, one, or --

MR. BOYACK: Well, I didn't do anything here, because --

MR. GIESEKE: All right. Well, zero, .01, .005 for the next one.

MR. BOYACK: .005?

MR. GIESEKE: Yes.

MR. BOYACK: Now, do you want to go down further and break it up?

MR. GIESEKE: Well, if I needed to break them up --

MR. BOYACK: You don't have to. I am not forcing you. I am just saying --

MR. GIESEKE: I am not going to do that right now. I am still struggling with --

MR. BOYACK: Okay. Dana, which way -- you wanted to leave them in --

DR. POWERS: Well, I buy into Tom's argument that we ought to just split the plutonium out just from an inventory issue, and from the fact that in a lot of countries there is a great deal of public interest in plutonium as an entity in itself, nd probably misplaced, but you know how people get agitated about plutonium a little bit.

MR. LEAVER: Yeah, Woody Allen makes jokes in his movies about it.

DR. POWERS: Well, I have never seen a Woody Allen movie in my life, and I am going to hold it against you for even knowing about these things. I think that Bernard's rationale for pulling the plutonium out as separate entity seems to have a good technical foundation.

And since I am going to use his numbers, I am going to use his split. And as I recall, Bernard, your argument was for two percent in-vessel release for the neptunium and cerium group, and .2 for the plutonium group, and those seem to be pretty rational to me.

MR. BOYACK: That was -- well, the cerium one more time?

DR. POWERS: .02 and the plutonium is .002, and the neptunium is .02. Now, one of the reasons for not getting too educated about high release fractions from the neptunium is when I tried to calculate the thermodynamics of it, I could never get -- I mean, it seems to me that cerium is the more volatile of the group.

I can never get it to come up high, though I will admit that our neptunium thermal-chemical data is not what I would call the best that I have ever seen.

That I did have to do the grouping when we put together the VANESSA code, and so we looked explicitly at neptunium, plutonium, cerium, and came away content to treat cerium as representative of those groups.

I will admit that the plutonium definitely

was a little bit less volatile, and when I come to the ex-vessel releases, I think the existing tables are a tab low on those things, because I think I can get that much release just from the mechanical release fraction, with bubbles coming up through the mount.

And so I will go with one percent across the board on all three. And I don't believe we have any late in-vessel releases.

MR. BOYACK: All right. Tom.

DR. KRESS: I agree with the reasoning for separating the outer three based on inventory and radiological consequences. My release fraction is based on a whole range of types of data rather than just a couple of experiments, and converting that into models and factoring the core heat-up rates and factoring into that a half of the core only gets melted, and things of that nature.

And I get numbers that are considerably lower. For the cerium, I like the original number of about .002. For the plutonium, it is a little less than that, .0001, just because it is a little les volatile.

For the neptunium, I like about .001 or a little higher. I agree completely with Dana that the ex-vessel is probably driven by the bubble breaking

through to the surface model. I think that is where the .005 came from in the first place.

I would stick with Dana's number though, the .01. I am not sure what the real number is with the bubble bursting. But the .01 is about right to me for all three of them. They are the same.

And I agree that there is no late in-vessel release.

MR. BOYACK: Okay. Bernard.

MR. CLEMENT: For the total releases?

MR. BOYACK: Yes.

MR. CLEMENT: .02 for neptunium and cerium.

MR. BOYACK: And that is total release.

MR. CLEMENT: And .002 for plutonium.

MR. BOYACK: If I do this right. All right. And that comes back to you, Dave, now. Did I do something wrong? Oh, I haven't got one, right? And the last one was?

MR. LEAVER: He has neptunium.

MR. BOYACK: So, .002.

MR. LEAVER: No.

MR. BOYACK: I knew that and I just wanted to see if you guys were awake. Okay. Dave.

MR. LEAVER: Can I just ask Bernard a

question. For the noble metal, you had a number for early in-vessel, which you then parenthetically said included ex-vessel?

MR. BOYACK: The total.

MR. LEAVER: Well, that is what your chart says. I don't know if we -- is that not the case here?

MR. BOYACK: That is total release.

MR. CLEMENT: That is total, yes.

MR. LEAVER: Okay. But what about this?

MR. BOYACK: It is the same. I just put TR on there.

MR. LEAVER: Oh, TR.

MR. CLEMENT: Yes, total release.

MR. LEAVER: So your 2 percent number is total release?

MR. CLEMENT: Yes.

MR. LEAVER: All right. I would say for cerium that I would use the .005 number. I see no basis for changing it. The same for plutonium. And for neptunium, I would use 1 percent. And for ex-vessel, I would use the .005.

DR. KRESS: And the reason that I used .002 is because the .005 data is to be consistent with the noble metals. I was saying that the cerium

release is like a factor of 10, and less than the nobel metals.

And if you look at what we put in for the noble metals, I come to .002, but the five is just as good for me.

MR. LEAVER: I understand. I guess I was just saying that I don't feel like I know enough to differentiate is all I am saying. So I would just leave it the same as was in the existing 1465.

MR. BOYACK: Is that for all three species, .005, or just cerium?

MR. LEAVER: Yes, for all three.

DR. KRESS: And that to the extent that .005 is based on bubble bursting in the first place.

(Discussion off the record.)

MR. BOYACK: Okay. We will come back at 1:00, and we will go through that last group, lanthanides, and then we will move on to BWR.

(Whereupon, at 12:14 p.m., a luncheon recess was taken.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(1:09 p.m.)

MR. BOYACK: The last group to work through for PWR release in containment are those associated with lanthanides. It looks like from the information that I have listed before that it was early in-vessel.

But I am going to retrace my steps just for a moment here, because I don't think I asked whether there was any comments about early in-vessel. For early in-vessel, I have an improved understanding of something is needed.

I don't know whether that was because at the time I thought that there was something and that was a standard phrase, or whether there was something that I couldn't take off the tape.

DR. KRESS: For lanthanides?

MR. BOYACK: This is for the cerium group. And so the question is are there any needs that I should list for early in-vessel release or ex-vessel release for the cerium group.

DR. POWERS: What is down there for the cerium release is the reduction to the monovalent oxides. So you get CEO, or PUO, or MPO as the dominant vapor species.

And so what you need to have an improved understanding of is how is the oxygen potential and the high burn-up fuel behaving. Are you indeed getting the saturation, and the exhaustion, and the ability to buffer the oxygen potential, or are you maintaining it at some nominal and further reduced value.

DR. KRESS: I think sort of a general comment about most of these is the need for knowing the oxygen potential in the fuel and how it might be affected by things like erbium that we were talking about, and BWRs.

DR. POWERS: Well, certainly it is true for an understanding of how these metal inclusions are doing from the noble metals.

DR. KRESS: And I guess there are oxygen-potential changes from burn-up, too.

DR. POWERS: It could be going up, but it wouldn't go up very much as long as you can have the MO2 equilibrium going on. But if you exhaust that, and you turn all your molybdenum metal into molybdenum dioxide, then it starts going up really dramatically with burn off.

DR. KRESS: That is your buffer in there.

DR. POWERS: That is the buffering that

you have. And from my money, it looks like somewhat over 60 gigawatt days per ton, like 65 gigawatts days per ton, and you start to lose that buffer capacity.

So I think you get some fairly dramatic behavior, and that is what I would look for experimentally, is doing tests on either side of that to get the co-efficients to go in your model.

DR. KRESS: Once again, it boils down to give me fuel that has this level of burn-up, at a couple of levels, one below and one above, and give me some fission product release data from it, like the VERCORS type tests, so that I can either factor it into a model, or look at it and see what to do with it. But I think it just needs the data.

MR. BOYACK: Okay. Anything different on anything that we added on ex-vessel?

DR. POWERS: With ex-vessel, the cerium has always been included in every melt concrete experiment that has been done in this country, and cerium has been included in it.

So I get the feeling that we have whatever data we are going to get. I mean, I guess the French are planning some more melt concrete tests; is that true?

MR. CLEMENT: I don't know.

DR. POWERS: I have heard rumors to that effect. I have heard rumors to that effect, and they may go back and reexamine some of those release fractions.

DR. KRESS: I guess the question that I had is should you think about over line two and water so much?

DR. POWERS: Well, not for this, because this is to give you the release to the containment, and then the licensee can put in whatever retention he wants.

And if he floods the sump, then that is one mitigating process that he can have, but what I can tell you if is you put a little water on top of this, and boy, it just knocks the source term galley west.

I mean, even so much as a foot of water, which ordinarily you wouldn't think is very much, but it is just enormously effective.

DR. KRESS: If you had asked me beforehand before you had any data, I would have said no way. That was a surprise to me.

MR. BOYACK: All right. I am going to move us on into the lanthanides, and Dave, did you have any comments on that? I know that it is on your

table at least.

DR. POWERS: You might want to know that for the cerium group that we are pretty much voting as a body that thee is no late in-vessel release. That means that we are saying that nothing revaporizes off the structure late in-vessel.

And I don't know of any experiments that have been done to look at revaporization from hot, but certainly not melting structures like you would have in either an air or reducing environment.

And it should be nice to have an experimental conformation in this confidence that we can neglect the late in-vessel release for these elements that we have been neglecting.

MR. BOYACK: Well, the words that we had before, an improved understanding of the volatilities of the species and the cerium, is that not specific enough?

DR. POWERS: I think I would call that revaporization specifically. My world view on this is that I know cerium vaporization chemistry about as well as I am ever going to know it.

I am probably knowing plutonium vaporization chemistry because of the exemplary and unparalleled excellence of work at Los Alamos pretty

well, but neptunium is -- we are still have pretty pathetic vaporization data on the neptunium.

MR. BOYACK: All right. Okay. Now we are ready to go to the lanthanides.

MR. LEAVER: For this lanthanides list here, there are eight elements, and there is a lot for which we don't have measurements, but we did get an 8 percent release from the fuel for HT 1, and a .001 for the furnace, and especially nothing from the thermal gradient tube.

And then there were some measurements for niobium, and they were somewhat higher, and I guess I have a blank here, meaning that there was no measurement.

And then we have some pretty low measurements here for a couple of elements from FPT-1, and then there was some measurements of curium and americium from the SFD 1-4 test that were low.

So overall my suggestion from this is that there is really no -- other than possibly niobium, there is no strong basis to change what we have in 1465, which is three zeros and a two for a release to containment.

Niobium I am suggesting we increase that by a factor of 10, although it would be nice to keep

a nice grouping here. But that was what I came up with. So the niobium release to be increased by a factor of 10 based on the HT 1 and a loop and thermal gradient tube release.

The lanthanum and zirconium and all of these are the same as 1465 on the basis of thermal gradient tube release from HT 1. And also there was a VERCORS 1 to 6 observation of no significant release was measured for non-volatiles.

That was a qualitative observation. And then curium and americium is the same as 1465 based on SFD 1-4 and again the VERCORS 1 to 6 observation of no significant release for non-volatiles.

MR. BOYACK: Okay. Bernard, did you have any comments in that area?

MR. CLEMENT: Yes. The same remarks, and taking the remarks for cesium from this table, just to show that it is worthwhile to look at it carefully. So cesium was 1.1 to 1.6 (inaudible) in the short term, compared to eight in the long term compared to cesium, which is less important.

If you look at the same tables for all the other elements, in that table, we go from the low impaction the lung dose rate and the (inaudible) compared to the total latent cancers (inaudible).

DR. POWERS: You might note the curium results at the bottom. They are substantial, especially when you realize that curium doesn't have much of an inventory.

MR. CLEMENT: Okay. With the same methodology, begins to come the high release from VERCORS HT 1, and also from our other experiments, and I believe we 0.2 to 2 percent containment, and the same pending for lanthanum, and maybe that is more in the (inaudible).

And for the others (inaudible) that is an .002 release (inaudible), and maybe to take into account the release of lanthanum, because it is not negligible from a biological point of view.

And as you can see here, we have taken 10 percent release from fuel, and that should be (inaudible) to 9 percent, and then you have taken into account the 80 percent retention. This is a figure for the 2 percent to the containment.

MR. BOYACK: Okay. So the first thing we have to decide is whether and how far to divide this group up. I have got Dana on the list for the initial conversation anyway, and so why don't you go forth on at least on whether or not to divide this up into subspecies.

DR. POWERS: The predominant characteristic of the lanthanides of course is the chemistry, and it is all very, very similar. There is a periodic -- a slight periodic effect as you fill up the F elements.

Lanthanides, the progenitor is really not one of them. It doesn't have any elements in the oxides, but the chemistry is so similar that it takes fairly sophisticated chemistry to separate the elements.

And consequently it seems to me that variations that we see in the measurements probably reflect the measurements more than anything else, because chemistry is so similar in that group of elements.

Now, placing americium and curium in that group is remarkable, but I don't think it poses any great difficulty. Consequently, I would keep them grouped, and I would take the GAP release fraction to be zero, and the in-vessel release to be one percent, and the ex-vessel release to be one percent, and the revaporization to be zero.

MR. BOYACK: You moved so quickly that you caught me by surprise.

DR. POWERS: I was trying to do that. I

will point out that a prestigious laboratory in mid-to-somewhat-Northern Ohio, a mass spectroscopist, did look at the release from a radiated fuel.

My recollection was that he came up with some fairly high volatilities for curium. It's inventory is so low that he didn't know what to make of it, but it did cause pause when you find that data, because it was behaving quite differently than what you would expect.

MR. BOYACK: And what were the values for that again?

DR. POWERS: You have got zero on the first one, 0.01, and then the next category, 0.01, and zero. And again I would say that I have never seen any attempts to measure revaporization of lanthanides off structures that are hot.

The predominant way you release the lanthanides is as the LaO, or its equivalent vapor species, that is suppressed in oxidizing environments, and enhanced in more reducing environments.

It would be useful to have some confirmation of that decision to say that there is no revaporization.

I might also go on to mention that the reason that you get lanthanum release ex-vessel is

that it occurs almost always when there is zirconium metal present in the core melt that comes out. So you get these experiments where people formulate oxide melts, and put them on concrete.

They don't have any zirconium metal initially present, and they don't see any release. And they say, well, the lanthanum release is predicted by codes like VANESSA are all wrong.

Well, VANESSA predicts nearly all that is released when you have very reducing conditions of the gas coming up, and that's only when you have zirconium metal present.

MR. BOYACK: Okay. So Dave answered my question about whether to break up the groups by giving me his values for a single group and you can't break up groups, and so we will just do that if we need to. But, Tom, you are up now.

DR. KRESS: I don't think I can differentiate between groups, and I would keep a single group. I would ask Dana what the basis is for early in-vessel results, because I was going to have a considerably different number. So I want to hear what Dana's basis was for the .01.

DR. POWERS: I am influenced heavily by the VERCORS results. They come up with a little

higher release fraction, basically 10 percent, from the fuel. And then they have some deposition. Maybe I am not willing to go that high.

I have some deposition, and I think about half of what gets released in the fuel gets deposited, and they take 80 percent. I think that just reflects the general uncertainty.

DR. KRESS: I have very little data base to the .01 for the lanthanides. We didn't really separate those out at the Oak Ridge tests, and so I don't have much information on the lanthanides.

What I do have comes from old tests -- TREAT, SASHA -- and those old tests which may in general tends to over estimate the releases. But I don't know about lanthanides.

If I use that data for the lanthanides, as opposed to the VERCORS, and get a release co-efficient and factor it into my model, I get a number like .0005. So that is not factoring in the VERCORS data, but the old data, which I thought was conservative.

Now, on the ex-vessel release, I would stick with the .01 because I still think that it comes out early when there is still metal there, and a bubble burst, and so I think that is a relatively good number for that. And late in-vessel, I would go with

zero there.

MR. BOYACK: Okay. And Bernard.

MR. CLEMENT: Okay. So the values, I have given actually for lanthanum and niobium, and zirconium --

MR. BOYACK: So I should break these apart now?

MR. CLEMENT: Yes.

MR. BOYACK: Okay. So give me the groups.

MR. CLEMENT: Yes, the groups. The first one is lanthanum, niobium, palladium.

MR. BOYACK: What was that third one?

MR. CLEMENT: PR.

MR. BOYACK: PR. Okay. And sodium wasn't it?

MR. CLEMENT: And the second one is --

DR. POWERS: No, the release fraction is very high.

MR. BOYACK: Sorry.

MR. CLEMENT: Zirconium, erbium, and niobium (inaudible). So for the first group, 0.02.

MR. BOYACK: Now this is for which release?

MR. CLEMENT: Well, the first groups, the ones with lanthanum.

MR. BOYACK: But a GAP release, or --

MR. CLEMENT: No, no, GAP release zero, and then overall release is 0.02.

MR. BOYACK: Okay.

MR. CLEMENT: And for the others, the overall release is 0.002.

MR. BOYACK: I need all the clues that I can for later. All right. Very good. Now, Dave.

MR. LEAVER: I would say based on the PHEBUS measurements and there is a couple of measurements for SFD 1-4, and the VERCORS HT 1 as you take into account the deposition in the furnace and the thermal gradient tube.

MR. BOYACK: Well, since I have got it individually, if you want me to put -- what was it? You had niobium in one group, and everything else in the other?

MR. LEAVER: Yes, that's what I had, but I --

DR. POWERS: I don't want to interrupt your, but you are going to confuse yourself, because you have got zirconium in two groups.

MR. LEAVER: Well, let's see, that's because I never did change this one. What he is showing me is that I have zirconium here, and

zirconium here, and I have a wrong one in the two places.

MR. BOYACK: How do you get nine elements?

DR. POWERS: Should we not put zirconium in --

MR. LEAVER: Let's see. LA, EU, CR, NB, and ZRY, NB, AM, and CLO, and I was just taking it off the list.

MR. BOYACK: All right. Yes. Right. I can do that if everybody agrees.

DR. POWERS: I wonder if it is tetravalent, or fission products, or any other --

DR. KRESS: Yes, but I want to know what the multiplier is in (inaudible.)

DR. POWERS: A bunch. Zirconium is right on the peak of the yield curve.

MR. BOYACK: Are you still pondering, Dave?

MR. LEAVER: No. I guess at this point I will give you a separate number for niobium, a .002, although I wouldn't object too strenuously if we lumped it in together with everything else.

And everything else, I don't see a basis for changing what is in the existing 1465. So that would be .0002.

DR. KRESS: You get a lot of high burn-up. That's why I didn't use .0002.

MR. LEAVER: I don't disagree with your overall number, but --

DR. KRESS: Well, I just did it to reflect the fact that there ought to be some burn-up.

MR. LEAVER: Although the burn-up effect is probably much less on these refractories.

DR. KRESS: Well, I factored it in one-third of the core. I mean, one-third of the core at high burn-up and the rest at the old burn up levels, I just made it a little higher to reflect that thinking.

MR. BOYACK: So when you do this, Dave, the .0002, early in-vessel, what happens to ex-vessel?

MR. GIESEKE: You need one more zero there.

MR. LEAVER: No, that's right, for niobium, but you want to put an .0002 for the others.

MR. BOYACK: The GAP release is all right, right?

MR. LEAVER: Yes.

MR. BOYACK: Now what for ex-vessel?

MR. LEAVER: Well, we are not done with early in-vessel.

MR. BOYACK: Oh, I see. What you are saying is that all the rest?

MR. LEAVER: Yes, that's it. And ex-vessel I would leave the same as existing 1465. I think there is logic that it is conflict, but if you think that there is a high burn-up effect, I don't think it would be 2-1/2, but I couldn't argue that the .0005 is wrong and .0002 is right.

They are so close that it is the same. Jim, what are you going to do?

MR. GIESEKE: I think I will make it easy for you, I hope. I looked at the calculations that I had done before, using the Tom Kress correlation, and I weight that sum by the French data.

So I sort of have compromised between those, I guess, and I can't distinguish between all the ups and the downs, and the nuances. I might be included to go with some French kind of up and down kind of relationships between those two groups.

But I think just for now am going to go with one group, .0002, and then maintain this .0005 over the rest.

MR. BOYACK: Okay. All right. Needs. Dana, you started to go through that a little bit, and I heard you say data on lanthanides is lacking, but I

didn't get too much further than that in what I was trying to put down. But any other comments that individuals have as to that.

DR. KRESS: I went back to the Oak Ridge HI and VI tests, and root fission didn't get any numbers for that, and so I would like to see some of the VERCORS data.

MR. BOYACK: And again this data would be used for processing through the models?

DR. KRESS: Yes, per model.

MR. BOYACK: Okay. It is my understanding that we are now done with PWRs.

DR. POWERS: I would still like to seriously consider moving zirconium to the list of cerium groups.

MR. BOYACK: Well, okay. The discussion was been so far, and so let's say, Tom, you were next on the rotating list, and so why don't you respond and talk about this idea of zirconium so that we have a proposal, I guess.

DR. POWERS: To move the zirconium to the cerium group, and it is a tetravalent species, four eyed (phonetic) structure, just like all the others in that group.

DR. KRESS: Well, I think I would support

that for a number of reasons. One of them is that I think it has about the same release rate, and lumping it in with the cerium is probably okay. I think that's where I would put it, with the cerium..

DR. POWERS: I would put it with the plutonium, because it has zip volatility.

MR. BOYACK: It is what now?

DR. POWERS: Zip volatility. A lot of it doesn't vaporize. In fact, that's how we track the differences between aerosols formed by mechanical processes, and those formed by vaporization, and we put zirconium in the melt.

DR. KRESS: Actually, you are right, Dana. Of all of the fission products that we have got, and even the structural materials, I have zirconium as the lowest volatility. And you are right. So I would lump it in with the lowest volatility.

DR. POWERS: Well, when we make melt, and we go throwing them around and want to tell the difference between aerosols form by vaporization and notice form by mechanical, we look for those with zirconium in them, and with zirconium in them, we say it is a mechanical formed aerosol, and you just can't vaporize it.

DR. KRESS: Of all of the things that we

have talked about here, that is probably the one with the lowest volatility.

DR. POWERS: And they do the same thing in the PHEBUS tests, and when they want to know where fuel is, they just track the zirconium.

DR. KRESS: I would put it in plutonium for that reason.

MR. BOYACK: Since I am now looking at cerium, I realize that I confused myself here. Jim Gieseke said on cerium that he would leave it in a single group. Dana Powers -- was it Dana who in the second position broke it apart?

You see, what I am looking at here is that I really don't have everybody recorded that I can tell, and the question is did I stop?

DR. POWERS: I wouldn't --

MR. BOYACK: What happened is -- well, that I think was my main problem was my other file, and maybe you made more than one file.

MR. LEAVER: I think -- well, it is in the trash.

MR. BOYACK: Well, it looks like I will have to go back to the transcript, because --

DR. POWERS: Well, I wouldn't really worry about it, because I am not uncomfortable with breaking

it apart.

MR. BOYACK: Well, what bothered me was the fact that I had not --

DR. KRESS: I remember Dana's comment was that because of the inventory issues that he would tend to break them apart, and particularly plutonium, and neptunium, which have inventory issues, and that it would probably be worthwhile to look at them separately. I remember what Dana said.

MR. BOYACK: But you should know that you don't need to break these apart necessarily in defining release fractions in order to consider them separately from the standpoint of a biological effect in inventories. You have to do that now.

DR. POWERS: Yes, but if you break them apart separately, you call attention to the people that are doing the modeling.

DR. KRESS: You call attention to them, but from a calculation standpoint, for inventory and dose conversion factors, those are broken apart anyway.

MR. BOYACK: I apologize for causing a problem here, but the real key here is that we need to deal with the zirconium issue, and when I got looking here, I couldn't quite realize what I had done. But

I did something to the files, and so I will just pick it out of the transcript.

MR. LEAVER: Did it get erased by mistake?

MR. BOYACK: Well, supposedly I have been renaming these files up here for the meeting, revised for the meeting, and somewhere along the way I was into two files, the old file and the new file. But the old file doesn't have it either.

So the real key here is to just come back to the zirconium issue, and I have heard two people weigh in on it, and Bernard, do you have any thoughts on it, or does it matter to you?

MR. CLEMENT: No. I have no objection for zirconium in the other group, provided that it stages a very small release, something like .2 percent.

MR. BOYACK: Okay. Dave.

MR. LEAVER: I have no objection.

MR. BOYACK: I heard it said it would be with plutonium.

MR. GIESEKE: It is interesting that you would do that. I think the numbers that are shown there, if you look at it from cerium down, it drops in order of magnitude, and then comes back up again in order of magnitude, right? I think the French went the other way.

MR. LEAVER: Well, he doesn't have all the numbers in there.

MR. GIESEKE: I know, but you are putting it in with maybe a contingent issue is all I am saying. He is putting it into a contingent box on your table. Whereas, it might be clear by itself.

I mean, it is not going to have the same problems as plutonium, where you have data going one way, and opinions going the other way, or whatever it is.

It may be clear cut that if you leave it by itself, or group it somewhere other than with the plutonium is all I am saying. If you had the other numbers, and we didn't lump the other numbers, I think that you would see that they went -- that instead from the cerium down, instead of going high-low-high, they go high in the middle and lower on the ends as I recall. Is that making any sense to you at all?

MR. BOYACK: Well, I understand that you are concerned about lumping the plutonium, and maybe when we get the table reconstituted, it may not give the trend that you wanted.

So I just apologize for losing that. I don't have any idea where those numbers went. But evidently, I think what it is, I probably shut a file

down without saving it or something.

MR. LEAVER: Do you want to take five minutes to recreate it?

MR. BOYACK: Well, if we can do that. If people can do that, that would surely save a lot of potential difficulties. And I think I have Jim's right here. And then, Dana, I can't remember what you said. I think you kept them together, Dana, didn't you?

DR. POWERS: Well, I wanted -- I thought Bernard made or Tom made a good point about the inventory effect, and it is clear that lots and lots of people focus on plutonium a lot, and they want that number pretty cleanly, and trying to explain to them why that number that you have is not really the number, because you really would be tracking cerium chemistry, and I know for a fact is a chore, because that is what we do in the VANESSA code, because plutonium is represented by cerium.

And trying to explain that they will probably overestimate and that plutonium is a chore. You sound like a blathering idiot is what you sound like. And so separating out plutonium just did not give me any -- I mean, I see advantages to doing that, and I believe it is true that the plutonium

vaporization is just less than cerium.

MR. BOYACK: Well, the reduced values that you gave me --

DR. POWERS: Those are fine.

MR. BOYACK: Okay. Tom.

DR. POWERS: Well, actually, you have everything in in-vessel release, and I think I actually divided it among the two. So, it would be .01 and .01, .01, .002, .002, and .01 and .01.

DR. KRESS: Well, that was quick.

MR. BOYACK: Okay. Give me cerium first, early in-vessel.

DR. POWERS: One percent.

MR. BOYACK: Okay. Then this one, .001?

DR. POWERS: A tenth of a percent.

MR. BOYACK: Ten percent?

DR. POWERS: A tenth.

MR. BOYACK: And I now have the --

DR. POWERS: I could really screw you up.

MR. BOYACK: It wouldn't take much.

DR. POWERS: I didn't tell you about (inaudible) effects, and it drastically changes neptunium.

MR. BOYACK: Okay. Tom, are you going to be up here on the same value, or --

DR. KRESS: I am going to be different, because I am going to use the three. Now, for the cerium, I have -- my numbers are .0002. No, one more zero.

MR. BOYACK: Let's put it in the right place.

DR. KRESS: And for the plutonium, I have actually less than the cerium, and that is .0001.

MR. BOYACK: I've got it now.

DR. KRESS: And for the neptunium, it is the higher volatility of .0001, and in the excess, I have got .01 for all of it, and I am not sure what you have got for Dana's in there for plutonium, but I had .01 there for it, too.

MR. BOYACK: And he has got .001.

DR. KRESS: Well, I don't know.

MR. BOYACK: So you are telling him that you don't know why.

DR. KRESS: Right. I think you copied it wrong.

DR. POWERS: I think he is right. I think that should be (inaudible).

DR. KRESS: Yes.

MR. BOYACK: Okay. So I really didn't copy it wrong, but he is changing it, right?

DR. POWERS: Yes. Tom is always correcting me.

DR. KRESS: So you might notice that I have an order of magnitude less plutonium coming out than previously.

MR. BOYACK: Right.

DR. POWERS: Small amounts.

DR. KRESS: Yes.

MR. BOYACK: Bernard.

MR. CLEMENT: So I had total release of 0.02 for neptunium and cerium, and 0.002 for plutonium.

MR. BOYACK: And what was plutonium again?

MR. CLEMENT: 0.002.

MR. BOYACK: And that is total release?

MR. CLEMENT: Yes.

DR. KRESS: That's not far from our total releases, because the .01 we have, and I think that is part of what he is calling a total release.

MR. BOYACK: Dave.

MR. LEAVER: For cerium, .0005, and plutonium, .0005, and for neptunium, .01.

DR. KRESS: I think you need another zero in the plutonium.

MR. LEAVER: Right, three zeros.

MR. BOYACK: And that was the same for cerium?

MR. LEAVER: Right.

MR. BOYACK: Okay. And over here on the ex-vessel.

MR. LEAVER: The same, .0005, for all of them.

DR. KRESS: Burn up shouldn't effect ex-vessel.

DR. POWERS: Well, it depends on what you are counting as ex-vessel. If you just have no concrete interactions doing the ex-vessel, then you are right. I mean, the inventory affects it a little bit, but it is hard to get too excited about it.

MR. LEAVER: Well, everybody else used the same value all the way down anyway on that column.

MR. BOYACK: On which one, the first one?

MR. GIESEKE: The last one.

MR. BOYACK: So you are saying that I can go what, .005, right?

MR. GIESEKE: Yes.

(Pause.)

MR. BOYACK: Okay. Now, zirconium. Now, you can look at the values and see whether that causes any difficulty. Now, you have the totality of the

table.

MR. GIESEKE: It is going to create a problem for me now, instead of trying to get an average across all of those, I am going to drop a number on the plutonium and zirconium.

MR. BOYACK: Okay. Now this is associated with this up here isn't it? No, it is down here. Okay. Now, the question was zirconium, and let's try to finish this up and be done. The question you raised, Jim, was would zirconium have a different behavior than plutonium.

MR. GIESEKE: Yes, and I will go down -- if we are splitting them up like this, I want to drop the plutonium and zirconium number, rather than trying to pick a number that averages out down through all three of them.

MR. BOYACK: Like that?

MR. GIESEKE: Yeah.

MR. BOYACK: So did everybody get their comments in? Is it all right to have zirconium with plutonium then with these values?

(No response.)

MR. GIESEKE: Did you save that by any chance?

MR. BOYACK: Yes, I did. What me to see

if I can recover it? There you go. How embarrassing. Now, what I would like to do is just for a moment go ahead and list the factors that we should consider when we deal with the BWR area with respect to source term applicability.

I had a little bit of a discussion at noon asking Tom about this, because I had heard this morning that it sounded like there was such a long and significant list that it was going to be difficult to go ahead and do anything but BWRs, and he said no.

The real issue I think was the one that Dana raised about the erbium, and that is a future possibility, and so I would like to list it on the list here, but remove it from the consideration as far as this source term applicability, except that we make note of the possibility.

And then any other factors that you think are appropriate. We may want to go ahead and provide some sort of importance.

DR. POWERS: Well, number one on the list is how much higher is the zirconium inventory.

DR. KRESS: And number two on my list is that I think we ought to consider BWRs as now having a flatter power profile, and therefore thinking about what fractions of the core enters into this thing

might ought to be changed by then. So instead of 50 percent, maybe we ought to use 70 percent or something.

MR. BOYACK: Okay.

DR. POWERS: No silver-inium-cadmium control rods.

MR. BOYACK: Anything else?

DR. KRESS: The other thing that may affect the source term for BWRs is they have a lower power density, and more water, and therefore they tend to heat up slower, and slower heat up actually, believe it or not, increases the fractional release for changes in the duration of those changes.

DR. POWERS: You mean makes it of a longer duration?

DR. KRESS: It makes it of a longer duration and increases the fraction.

MR. BOYACK: Was the corollary of no silver-inium-cadmium control rods, did that have anything to do with the --

DR. KRESS: It is usually B4C.

MR. BOYACK: But they have control rods, and so what are they replaced with?

DR. KRESS: I'm sorry, but I think B4C is in PWRs.

DR. POWERS: A lot of the PWRs are going to a boron control rod either from borcivitive (phonetic) glass or the B4C itself. Not many in this country now have it, but nearly all the vendors for testing, they use a boron carbide control blade in the G.E. designs.

And what it affects is that it affects what you think tellurium is doing.

DR. KRESS: The control blades in the BWRs have a different timing for failures than the control rod, and so the question of whether they mix with the fission rods or not --

DR. POWERS: What we saw in the DF4 test was that pretty much burn up, the steel melted and dissolved boron carbide, and out it came out of the core region.

DR. KRESS: Before the fuel started.

DR. POWERS: Before you really got the fuel going real seriously. Now, that is different than the scenarios that you would have for PWRs with boron carbide control blades or control rods, because the boilers have a lot of steel in those blades, and so they can totally dissolve all the boron carbide.

Whereas, if you use just tubes, you don't have have enough steel to dissolve everything.

MR. LEAVER: In the PHEBUS, are they giving any thought to doing a BWR?

MR. CLEMENT: Over many lengthy discussions.

MR. LEAVER: Yes, I have heard that there were lengthy discussions.

MR. CLEMENT: And boron carbide, and the main influence as Dana stated is the amount of steel, and it also depends on the volume of the water reactor, and the design.

If you look at the Swedish design, you have an amount of steel that is varied width, and so full of (inaudible) tests, and so the amount of steel that is a little bit greater than pressurized. But that is low enough not to be so low that boron carbide (inaudible). And if you look at the DF4 experiment, everything that is dissolved in that, is all down in the core regions.

DR. POWERS: I just think the thinking was that first of all, the interest in PWRs, you want the excess boron carbide, but if you are going to get new phenomenology, you have got to have a new boron carbide, because it is hard getting steel to resolving the boron carbide. It is like a test with no control blade at all.

It all flows and goes down, and mixes, and that is not much interest. And there is no guarantee that what was observed in one DF test is going to be true of an entire boiler core. It is a test, and that's about all you can say about it.

DR. KRESS: Another thing about boilers that may be important is that there is a more open core, and instead of things melting, and falling down, and plugging up the core, it may just go straight on through.

So your impressions of what might happen after melt may be different.

DR. POWERS: In modern boiler control rods -- I mean, modern boiler fuel rods look an awful lot like PWR rods.

DR. KRESS: Yeah, they are about the same size.

DR. POWERS: Yes, about the same size, and about the same clad thickness, and everything else.

MR. LEAVER: The fuel rods themselves?

DR. POWERS: Yes.

DR. KRESS: Yes.

MR. BOYACK: Now, what was the material that you mentioned, erbium or something that you mentioned? The poisons, or --

DR. KRESS: Yeah, poisons.

MR. BOYACK: What was the issue that you mentioned about erbium?

DR. POWERS: Well, I just don't know what erbium does to the fuel. My suspicion is that they can go with lower concentrations of erbium than they do with gadolinium. And we also have suburbium.

MR. BOYACK: Now, let's see. That is 2 out of 6.

DR. POWERS: And now an M on the end.

MR. BOYACK: And that is 3 out of 6. They go at lower concentrations, and I suspect that this fuel is looking more like pure UO2 than with these two with the higher burn-up PWR fuel.

I bet it is just different, because they are cutting the concentration of poison down by almost a factor of 10.

MR. BOYACK: Now, I guess we are going to as you go ahead and do your considerations, do we consider all of these? We have been asked. We won't do this, because that is a future thing, but much higher zirconium and flatter profile, and different control rods. All these are factors that come into play.

MR. LEAVER: And one other one is that the

fact that you have these steam separators and dryers in the upper plenum, which I think it is generally felt that you may bet get more deposition.

DR. KRESS: In fact, I think that was the main difference between the BWR and PWR source terms. I don't know, but maybe you ought to say why they were different in 1465.

MR. NOURBAKHSH: (Inaudible.)

DR. POWERS: The separators and dryers have big passages in them, and they are not real super effective at retention. The esteemed laboratory in Northern Ohio did some experiments on a full-scale of devices.

MR. GIESEKE: As a matter of fact they did.

DR. KRESS: Was that (inaudible)

DR. POWERS: No. I said esteemed laboratory.

MR. GIESEKE: Well, the DFs weren't real great, maybe two or something like that.

DR. KRESS: I think with better profiling that you could certainly factor in our thinking, because what we are talking about is a profile that gives you a 20 percent more power than the normal profile that has been used, and that means --

DR. POWERS: It better be 20 percent more forma.

DR. KRESS: That's exactly what I was trying to equate. Is it going to be 20 percent more, and as I said before, use 70 percent instead of 50 in our thinking?

MR. GIESEKE: What is the difference in performance level between (inaudible) --

DR. KRESS: They are all restricted to the 65 or --

DR. POWERS: 62 giawatts right now, and I think the boilers are all running a little lot relative to that pressure.

MR. BOYACK: Was that 20 percent higher core power, is that what I heard?

DR. KRESS: Yeah.

DR. POWERS: You let these crazy guys at the NRC and ACRS keep approving these things.

MR. BOYACK: So are some of these thoughts --

DR. KRESS: What they have been doing is using these same release fractions and just increasing the inventory. And what we are telling them now is, hey, you ought to use a different release fraction, too, which would be factored into their power uprate

requests.

MR. BOYACK: So what I am going to do now, just to see if it works, and we will just start with Tom, and I would like to have some sense of what is perceived to be the higher impact items on this list.

Because there may be some useful dialogue that comes from this, and so they could all be high, and they could all be low. I don't know. But relative to PWRs.

DR. KRESS: My feeling on that is a flatter core.

MR. BOYACK: What was that?

DR. KRESS: A flatter core.

MR. BOYACK: So this one is where you would put the high, right?

DR. KRESS: Compared to the PWR, which has the same burn up rates.

MR. BOYACK: And is there anything else that would fall into that category on the highest, or the others of less importance?

DR. KRESS: I think the rest of them are less important, but I certainly don't know about molybdenum today.

MR. BOYACK: Well, we are not going to deal with that today.

DR. KRESS: But as far as those others, I think that would be my feeling.

MR. BOYACK: Okay. Bernard.

MR. CLEMENT: Really, I should not make a classification about that, we don't have such extensive boiling water reactors.

MR. BOYACK: All right. Dave -- well, you can get by with that. That's all right. That's a good one. Dave, which one of those, or 2 or 3? I mean, which ones are the high important factors relative to the PWR as far as source terms in your opinion?

MR. LEAVER: It was my impression, or it is my impression, that while the separators and dryers may not result in a large DF, that they are going to result in a larger DF than whatever it is that we assume for a PWR, because you have a huge surface, and they are designed to take out particles.

That is, condensed steam, and so I think there is an effect there, and it may not be a large effect, but it is something.

MR. BOYACK: What about relative to the cladding profile?

MR. LEAVER: I don't know. I would have to think about that.

MR. BOYACK: So if you want higher or moderate down here under impact?

MR. LEAVER: I would say moderate.

MR. BOYACK: Okay. Jim.

MR. GIESEKE: Well, I think the higher is more powerful, but it would have to be significant if it was like 1.4 times the amount of material involved. I also think the first and third ones are both going to affect that.

Of course, Dana discounts the -- he thinks it is the tellurium and zirconium relationship is something else. But traditionally the first and third would affect the tellurium behavior.

MR. BOYACK: So you think down to a moderate level or are you up high still?

MR. GIESEKE: The third one might be pretty significant.

MR. BOYACK: What is the third one?

MR. GIESEKE: It is the boron carbide. I don't know. Let's say the tellurium is transported with having reacted with things in the air, and a lot of what would be in the air I would think would be different in this case.

Well, I will put medium on one and three then, I suppose.

DR. KRESS: A lot of that which increases zirconium is in the channel boxes.

MR. GIESEKE: Yeah, I know, but that (inaudible).

MR. BOYACK: The reason that I am having this discussion as we start is to have you try to get you to hear each other's arguments about what is more important about this as we start. I think it is worthwhile to just get a sense of that before we start trying to assign values. So that is what is going on.

MR. LEAVER: Can I ask a question? We are going backwards here, but this item on the lower power density, it is about maybe a little more than half, about half, about 55 percent maybe of a PWR.

DR. KRESS: They are going to increase that little bit, with an increased power uprates, but you're right. It is about half.

MR. LEAVER: I would say maybe 60 percent, and I remember that from AOWR days, and so it is probably not much different for our plants, which will tend to have a slower heat up, and then prolong the event.

But if we are talking about an event that is a recovered accident in-vessel, and so we say this is nominally like an hour-and-a-half to two hour

event, and 30 minutes or so to heat up, and then you have this GAP release, and this fuel release, and then you stop.

Wouldn't that tend to balance at least to some extent the fact that you have a flatter power profile, and therefore more of the core would be involved.

I guess I am asking you because you raised the point about the effect of the flatter profile involving more of the core.

DR. KRESS: Well, if we are talking about PWRs, 50 percent of the core melting, that is almost not a terminated accident. It is almost going all the way.

MR. BOYACK: Fifty percent?

DR. KRESS: Yes.

MR. BOYACK: But in TMI, you had in round numbers 50 percent, and maybe you had less melt than that. I guess you had about what, 20 or 25 percent of the core was molten? But you released half of the iodine. So in my book in round numbers, that is half of the core.

DR. KRESS: My feeling is that it has a flatter profile and 70 percent of the core taking it hard is not a terminated accident.

MR. BOYACK: No, I don't think so. We do have to -- I mean, I don't think this accident is an accident in which we let things keep going, and we just stop considering the release at 2 hours. I think this is a recovered accident.

DR. KRESS: That's why we even both with ex-vessel.

MR. BOYACK: Well, I guess that NRR has decided to not consider that for design basis event. And I think that there is good reasons for that.

DR. KRESS: Spread out over a long period of time usually.

MR. LEAVER: Yes, and with the improvements in accident management procedures, there is certainly -- it is not unreasonable to expect that the operators would figure out something, some way to get water in there.

DR. KRESS: I could never have considered the design basis accident source terms to be a terminated accident, in the sense that you turn water back on, and start it.

It is terminated by the fact that the core melts and falls into the bottom head, and then quits releasing because you don't release much from a molten pool fuel. That is what terminates it in my mind.

And the fact that you melt from the middle of the core out means that there is some left in there, and you only get about half of the core to participate with.

So there is no terminated accident in my mind. It is one that is just the way the accident progresses. And that is generally what you get.

MR. LEAVER: I am just wondering how could you get 70 percent of the core molten if you are saying this is a roughly two hour event, and you are relocating molten material as it will relocate downwards, and much of it down into the bottom of the head, but not fail the vessel?

I mean, I don't think it is possible to get that much of the core molten without failing the vessel, and I don't think you can do it that fast.

DR. KRESS: I think you do fail in this and you get an ex-vessel release because of it.

MR. BOYACK: One of the things that I am curious about is the source term was not overly totally mechanistic to start with, right?

MR. LEAVER: Right.

MR. BOYACK: And it is guided by regulation, and it says you have to have a substantial.

MR. LEAVER: Right.

MR. BOYACK: So I am trying to understand where you are talking the dialogue here. What I hear you say is that you are wondering about compensating effects here.

On the one hand, the flatter power profile, which may relate to a lower core melt, and you are wondering about the lower power density and the slowing down of the transient, and how that all plays together.

MR. LEAVER: Yeah, and I was wondering how long do we consider this accident to be, because the longer you wait, the more that things will happen.

MR. BOYACK: But eventually we will come back here, right? And we go to these tables?

MR. LEAVER: Yes.

MR. BOYACK: And the first thing we start out with is duration. And so what we are asking here is to list and provide some list of the things that might influence the duration or the releases. But I wanted to get some initial discussion which is going on.

MR. GIESEKE: Presumably, all of the things that we have been talking about have already been accounted for and the differences between Table

3-1-2, and 3-1-3.

DR. KRESS: Not the flatter profile?

MR. GIESEKE: Not the flatter profile? Okay. So you are saying that you would like to change the original tables here somewhat, the 1465?

DR. KRESS: The original tables would apply to the original BWRs. What I am saying now is that essentially every BWR is going to go a 20 percent higher power, all of them.

And now our thinking ought to change, because the BWRs that are out there now are different than the BWRs that were there when we had this source term.

MR. GIESEKE: Okay. So you are saying that you want to go back and change these two tables, or change --

DR. KRESS: I would change that one now.

MR. GIESEKE: And without considering a high burn up?

DR. KRESS: Yes.

MR. GIESEKE: Yes.

DR. KRESS: Because the BWRs have changed.

MR. GIESEKE: That's what I am trying to figure out, because there are two issues that we are talking about. This is not a burn up issue that we

are talking about yet, and I guess it is the way to get to the bottom line, is to see what effects these have on the table, and then see if there is any effect of high burn up on the table if we are going to take them one at a time perhaps.

DR. KRESS: That is not a burn up issue.

MR. TINKLER: I would like to say something about that, because this issue really starts addressing a lot of other matters, such as core melt progression, and tie into the boiler versus PWRs, and I guess we would be pressed to say that we can capture all those differences and still retain fidelity to the original in-vessel.

I am not sure how you are going to balance or how you are going to prolong the duration of the early in-vessel, or transfer this. There are other issues that are just merely the operation of an AES system in a boiler, and making it more (inaudible) environment, and all those kinds of things that we have.

And frankly we don't have as much core melt progression data on boilers to suggest how that melt pool forms in the in-core region, and whether or not -- you mentioned it reducing the lowerhead molten in the core region.

DR. KRESS: You might --

MR. TINKLER: Right. I mean, most of our thinking is influenced by the behavior of TMI, but the other point is that how much of the core can actually be molten before you think you are going to have a relocation of the lower head and fail the lower head.

Do you really think you can sustain more than 50 percent of the core in a molten state before you get a relocation of the lower head (inaudible). And the other issue of the boiler is that you have got a different kind of lower head.

If you look at the BWR core melt progression, it might suggest a much longer early in-vessel base than some of those same kinds of analysis. I would suggest that the committee not try to sharpen that pencil.

DR. KRESS: It's because we need a lot of calculations and information that we really don't have. I think you may be right there, Charlie.

MR. GIESEKE: And just try to focus on the effects of burn out and frankly until there is an equivalent BWR, I don't know where you are going to go on this.

DR. KRESS: If we could figure or factor in the effects of burn up, it would be almost the same

factor as what we threw in for the BWRs if we were just looking at burn out.

MR. LAVIE: It depends on the second one. I am not exactly sure this is a major issue, but remember that each of these plants, even though they have a 20 percent higher power, still have to demonstrate that they don't exceed 2200 degrees fahrenheit. So if they can demonstrate that, then that --

DR. KRESS: That's only if you have got an ECCS that comes on.

MR. LAVIE: But if this is a design basis source term, and --

DR. KRESS: It is not for design basis source terms.

MR. TINKLER: To get a substantial core melt, the ECCS has to stay off for some period of time. And there is something to this --

MR. LAVIE: But the point that I am trying to make is regardless of where ECCS starts, that if the core is capable of staying at 2200 degrees with the increased power level, it will not have a 20 percent high heat up rate if the ECCS doesn't work.

DR. KRESS: You can't stop there. It just keeps on going if you don't have the ECCS.

MR. LEAVER: But Steve's point is the fact that you are going to hire -- I mean, one thing for sure, with the higher power, you are going to have a higher inventory of certain isotopes, and that is going to affect the dose for the same release fraction.

DR. KRESS: Decay heat levels, too, but not as much as you think.

MR. BOYACK: So basically what Charlie has said is -- and it turned out that in these listing of issues that we have, the only one that was new, quote, new, was the one regarding the flatter power profile and the raised power levels.

That was the only one that was new, and so the point that was --

MR. LEAVER: Well, the power profile existed before high burn ups.

MR. BOYACK: What about the higher power?

MR. LEAVER: The higher power didn't.

DR. KRESS: The only thing that is different from the original 1465 is that.

MR. BOYACK: So there were two things here. One of them was the flatter profile relative to the BWR?

DR. KRESS: Yes.

MR. LEAVER: That existed before, and that is not new.

DR. KRESS: No, it wasn't in the 1465.

MR. LEAVER: What?

DR. KRESS: It wasn't in the 1465.

MR. LEAVER: Why not?

DR. KRESS: Because they only had power uprates of about 5 percent at that time, and none had gone to 20 percent when 1465 was put together.

MR. LAVIE: But we have used this source term for a fleet of plants whose power level varies from plant to plant by at least 20 percent. So that is not a change.

The existing source term was being applied to plants that carried that power level.

DR. KRESS: That is a change, because that power level was raising the amplitude of this co-signed distribution up and down, and not flat. That is different, and so it is a change.

But it is not related to burn up. There is a slight relation to burn up. It means that a fraction of the core goes to the higher burn up than it had before. So there is a slight relationship to burn up, and it is not wholly a burn up issue.

MR. BOYACK: The point to be made is one

that was made by one of the panel members, and that is that when we go ahead and look at a table like this, these factors had already been accounted for evidently, and not we were just looking at the burn out issue.

And this apparently was the only issue that might have been a factor, which was that in addition to that, you were running 20 percent higher on the core power or 15.

DR. KRESS: It could be a burn up in the sense that we have built into our thinking that when you talk about higher burn up, you are talking about one-third of the core.

That may be a different number in BWRs. I don't know what it is, but it may be different.

MR. BOYACK: So the real key of the discussion here is just to decide whether or not to do anything with this. So, Charlie has basically said --

MR. TINKLER: I am saying with (inaudible) that we see a lot of uncertainties (inaudible) core breach, and that (inaudible) will make you more able to form a large (inaudible), and more severe, and thermal gradient tube, if you could, but there is still other -- that is a small part of the uncertainty in this core melt (inaudible).

And I just think it is too much to try to factor it in quantitatively.

DR. KRESS: Well, it wouldn't be tough though if we are talking about -- if we just knew a simple thing, and that was what fraction of the core should we consider to be at high burn up.

And is that number different for these BWRs than PWRs, and that is a number that we ought to be able to come by real easy, and we could factor that in our thinking real easy. That is what we have been using, is one-third.

DR. POWERS: When we have asked about that, they keep talking about one-third.

DR. KRESS: For BWRs also?

DR. POWERS: Yes.

DR. KRESS: Then in that case, they would not be related to burn up. That is another issue.

MR. NOURBAKHSH: Another thing to consider is the impact on the ram plate (phonetic) temperature. I mean, we found that the zirconium oxidation drives the ram plate (inaudible), and therefore the 20 percent increase in power level is one small contribution than zirconium oxidation, because --

DR. KRESS: Well, I would agree with that 100 percent. But what we have been thinking is that

that zirconium oxidation run away starts in the middle of the core, and it works its way out, and then quits, because you have a channel for the water to go through, and you have got better heat transfer out there on the edges, and it just stops, and releases the peripheral fuel.

And my thinking was, well, we will say that is about 50 percent of the core that takes place in that, and now all we are saying is that it is still driven by that, but because you have channel boxes and BWRs, and because the profile is flatter, that the ones that are melting in the middle are probably going down at the same rate as the ones that are around them.

The channel boxes want to make them one-dimensional, and it all just depends on the power of that channel box.

DR. POWERS: So what you are saying is that at the point that you trigger off significant zirconium reactions, that temperature that you get is spread over a bigger part of the core.

DR. KRESS: It is spread over a bigger part of the core, and that is exactly right.

DR. POWERS: And the fact that you melt it down, yes, it is driven by --

DR. KRESS: Well, it may be different from BWRs because of a flatter profile, and the fact that they had channel boxes.

MR. NOURBAKHSH: It also doesn't make sense that with a open channel flow that drives more steam away from this region where there a molten pool than you would in a BWR. So this issue between BWR and PWR is very difficult to resolve.

DR. KRESS: It is difficult to resolve and I will admit that.

MR. TINKLER: There is more than one point of clarification. Dave raised a point about recoverable accidents. Part of that is because when utilities are trying to do portions of the calculations, part of the other boundaries for the release seems to be (inaudible), and there is an attempt to try to model this as a recoverable accident.

In some sense, when you are trying to predict an accompanying steam break, and --

MR. LEAVER: And keep the thermal hydraulics.

MR. TINKLER: Because all of that drives things like MSID leakage for boilers and things like that. So there is --

DR. KRESS: There is a factor.

MR. TINKLER: That is factored in, and that means that you end up stopping your core damage in anticipation of reaching a certain level. But I think the more important way is that the flatter profile still doesn't tell you necessarily that you can accumulate in the core region, the active core region, that much more melt before you have significant relocation in the lower half.

DR. KRESS: Well, the question is would that matter.

MR. BOYACK: What we are having now is a discussion, and now we are going to go ahead and work on the product here. And am I correct in assuming that you will just enjoy the meeting now for a couple of hours?

MR. CLEMENT: Yes.

MR. LEAVER: What if France decides for its next generation plant to build a BWR?

MR. CLEMENT: I don't know.

DR. POWERS: That means that the Germans will be marching through Paris. That's a fact.

MR. BOYACK: So, with that, what I have tried to do is on the quick create just do things that you don't want to. So what I would like to do -- and

I guess I should ask, as I didn't give Dana a chance to weigh in with his -- with anything else, but have we had enough discussion to just go forward now?

(No response.)

MR. BOYACK: Well, with that, we have four individuals, and so what I am going to do -- and we will have to move fairly quickly, but I like what you have done in the morning, which basically -- and we will change the order. We will continue to rotate.

I just put them on in the same way so that I would have a template. So we would start with Dave, and I would say with the GAP release, and early in-vessel, and ex-vessel, and late in-vessel release, and I would ask you to go ahead and give your rationale as you have done before.

And a brief rationale, and then the number, which I will try to write down correctly this time, and then we will go on to the next individual. But if you go through literally the whole thing on duration, and give us as much explanation as you want. Would that be all right?

MR. LEAVER: Sure. Where did we end up on this for the PWR? Did we increase this to .6 or something like that?

DR. KRESS: We decreased it.

MR. LEAVER: I think we decreased it to .4, right?

DR. KRESS: To .4, and then the other one we decreased a little bit. We increased it a little bit.

MR. BOYACK: Right. Okay. I think the same logic would apply in terms of a faster release. So I would say for the GAP, .4, and similarly the sum of the two, keeping them the same, I don't see any reason to change that notion, and so this would be 1.6. And then 3 hours and 10 hours. Jim.

If you want to give a dialogue, then we will let the record carry that through the transcript, and then the numbers.

MR. GIESEKE: Well, I see two conflicting things, or two things going on here. One is the burn up effect and the second is the profile, and the difference between the old 1465 numbers and the new thinking, and I am not sure exactly where they cancel each other out.

I have a feeling that with a flatter profile that you wait longer before things start, and then when they start, more is going to happen in a shorter time.

So I think if we are going to shorten it, you don't know how much to shorten it. I guess I am going to go .4 also, and sort of a whimpy way. I don't know any better number than that .16, and add 3 and 10.

But I think that is worth a -- if somebody knows better than those calculations, then maybe we could get a better number on what that impact of the flatter profile might be, but we don't have that data here to look at.

MR. BOYACK: When we were back here -- let me just try to capture this. We decided to leave this in, but you are just going to have to deal with it in your mental gyrations; is that basically the idea?

MR. GIESEKE: Yes.

MR. BOYACK: So we didn't discount it if I understood you correctly. Certainly that is what Jim was referring to.

MR. GIESEKE: Correct.

MR. BOYACK: Dana. Well, I think I would go right along with Dave, and I would shorten down the GAP release simply to reflect the effect, and at the expense of increasing the early in-vessel to the numbers looked good to me. Everything else I would leave just about the same.

MR. BOYACK: All right. Tom.

DR. KRESS: I'm thinking.

MR. BOYACK: Actually, that's encouraging.

DR. KRESS: It seems to me like when we talked about the GAP release, we talked about how long it took to heat the fuel up to the 1200 degree failure point of the clad, and then we talked about how long -- that is one fuel element, and how long that would spread over the whole core given the power distribution.

And I am not sure which of those numbers was the dominant one. If you were spreading over the core with the duration of the GAP release, then a flatter profile certainly shortens that considerably.

If it is the heat up to a temperature which felt at clad, then the flatter profile doesn't do anything for you, because it is roughly the same heat up rate for the hot fuel to carry.

So my feeling is that if it was the heat up rate that did it to you, and that is not going to change because of the flatter profile, but because you have a higher burn up, it will change about like we change the PWR.

So I would go down to the .4 also, just because that is what we did for the PWR. Now, here we

had some thinking about the fact that the higher burn up made you start releasing earlier and releasing at a little faster ram, and thee were some peculiarities about where you started the early release, and with respect to the GAP release.

And we didn't exactly -- I guess we kept the same total of 1.8 and 1.8, and so I guess I will do the same and keep the same total here. And that gets me to the 1.6.

So with all this meandering around, I came around to the same thoughts that you guys did.

MR. BOYACK: Okay. The last two.

DR. KRESS: Why the ex-vessel differed from the PWRs than it did the BWRs, is that because they use a different concrete?

DR. POWERS: It was because of the higher zirconium inventory.

DR. KRESS: Because of the higher zirconium inventory? It makes it last longer?

DR. POWERS: What they were thinking -- I mean, their thinking was that the release, the fission product release is predominantly while there is zirconium present, and we had calculations for plans in the source term code package development, where we were holding zirconium in contact with concrete for

many hours.

But on the average, it was roughly around three hours. And then once you depleted that, you had incorporated so much concrete in the oxide melt, and concrete, even when it is fairly basaltic, nature has enough siliceous material in it that it really was tying up the fission products pretty badly.

There were exceptions to that general thinking, but they thought not too big, and so predominantly your release is while you had plutonium metal present.

And it is just longer. I mean, in some of the cases, and in some of the transient cases, where we didn't have ADS operation, it had some heroic amounts of zirconium coming down.

And that was done without the Reverend Hodge scenarios, in which he would bring the zirconium down early, and it would do its thing, and then the fuel would get hot and come down.

But that was never taken into account. If you follow the Reverend Hodge scenarios, you come up with a very different looking source term, because what happens to you when you have zirconium metal present is you are not releasing any of the noble metal, and you are just stripping the ceriums and the

lanthanides out of there.

I mean, they are just ripping out of there. The bariums and the strontiums are coming out like they had no home. If you did the Hodge scenario, where you did the metals first, and then you did the oxides, you suppress the releases of the barium strontiums, and ceriums, and lanthanides, and you extenuate the release of the noble metal.

So it is a pretty much night and day situation. I mean, there is no middle ground between those two, and so it is your world view on how these things melt down.

I mean, Hodge makes his case based on the idea that the core comes down, and it freezes up, and then it reheats up. And the last melted things comes out first.

And he has done a pretty substantial effort to figure out which things melt out first, and he looks at a lot of things, and what not, and you really can't fault the general scenario.

And the idea to use these metals once they form will cut through the vessel head just like a knife through butter, and he is absolutely correct on that. It just does not slow down.

I mean, it will be at terminal velocity

coming through that steel. And the duration of delay that he hypothesized between melting the metals, and then melting the oxides, even though you have got a lot of heat source in the oxides.

In fact, Hodge puts too much of the heat source in the oxides. He really does not put enough in the metal. You know, it seems to me to make a lot of sense.

On the other hand, there is the map and world view in which everything is flogging up the channel boxes, and then it all comes down as a big lump.

And so you have lots of world views here to look at, and maybe this is some sort of a compromise, between those world views.

DR. KRESS: Well, given that discussion, I am going to go along with the 3 and the 10.

MR. BOYACK: Okay. Now I want to show you something. I would like to now scroll through the table, and give you a sense of what lies before us.

MR. LEAVER: He is trying to make a point here.

DR. POWERS: Just give each one, one set of elements, and they do it, and everybody buys theirs, and you are not allowed to contest them.

MR. BOYACK: There is other responsibility, because with this large number, we are not going to be able to spend a lot of time talking about it. So another possibility is that we print out the table and let you take it over the next two nights and return it.

And what you lose is that you get your values, and you don't get any of the dialogue. The way I perceive this is that if you are going into dialogue, I think this is probably the rest of the day.

And I don't think that is a bad day. I think it is probably a pretty good day, as far as what you get for it, because I think the dialogue is really quite important.

What this would say is that we have about a day-and-a-half to do the MOX, and I have no idea about what that is, and how that will work. One of the reasons that I would like to have either Charlie or Jason here, is that for me to outline the alternatives, and then get a little bit of help on what product or what outcome would be of the most use to the NRC.

MR. TINKLER: I think in (inaudible) in distinguishing between BWR and PWRs, you could use the

same kind of logic.

(Multiple Inaudible discussions.)

MR. BOYACK: Could you live comfortably with the idea of putting down the numbers without too much cross-debate, your individual numbers, because --

MR. GIESEKE: I think we have talked through all the issues related to the burn-up through to the fuel rods themselves, and they are the same fuel rods, or well, close enough here as before.

We have talked to the nuances there, and the differences here, and there is only the one, the power profile difference. And then they are just the same kinds of burn up issues that we have talked through as I see it.

I think the discussion would be pretty minimal, and another opportunity here might be just to take a look at these and see how willing people are to make a simple adjustment based on the burn up kind of issues that we discussed before to change the BWR numbers that are here.

I think it might be pretty quick to go through there, because we have hashed them all out, and I think they are all going to go in the same sort of direction.

I think we can go through them pretty

quickly, and on the other hand, we discussed all those issues, and so it would be less of a problem to go away and come back with numbers.

MR. BOYACK: It sounds to me like the thing that would work actually for the total, since we really do have an end period to finish this off, is to let people take this off, and spend some time on it tonight.

And if you have serious issues or questions, to come back and we will deal with them tomorrow, because we can go through one of these and give it a try, and Jim would be the first one to speak.

So it would be real easy and see how it plays out. So do you need to see what we did on the PWR?

MR. GIESEKE: Well, you have that there anyway don't you?

MR. BOYACK: Yes, for the early part. So let's give this a try just for a moment and see how it works. And where we go across noble gases.

MR. GIESEKE: I guess to deal with this, I would say the same issues exist here that existed before. We have a shorter time, but the end-point is the same, and I don't think the time is going to

affect the release.

So I would be inclined to take a look at what we have here, and adjust it in a similar fashion to the way that we adjusted the PWRs. So I would be inclined to go with like .07, and for all that matter, maybe .63 that we had before.

I would be inclined to take the same numbers. I don't -- you may want to play with the details, but I don't think it is going to be a lot different.

MR. BOYACK: Okay. So what I am doing is --

MR. GIESEKE: And then keep the ex-vessel and in-vessel the same.

MR. BOYACK: So for revised PWR applicability. Thank you. Okay. Dana.

DR. POWERS: .07, .93, zero and zero. Same-same, same-same.

MR. BOYACK: That helps me actually you will just say same-same in writing. Okay. Tom.

DR. KRESS: Dana, would you go and say yours again?

DR. POWERS: Mine are the seven, and it is the 7 percent release of noble gases in the GAP, and both or the rest of them come out in the in-vessel

phase, on the theory that you pop every single rod in that core, and it may not be 100 percent true, but I think it is close enough.

DR. KRESS: So you have a zero for the third column?

DR. POWERS: Right. Right.

DR. KRESS: And we have a .3 for the PWR.

MR. GIESEKE: We are getting all of these mixed up here.

DR. POWERS: I am doing the noble gases. I'm sorry.

DR. KRESS: Well, that's .3 for the PWR, and that is the red numbers.

MR. GIESEKE: We are getting mixed up I see already. I think I am, or at least with the numbers that we have here. I wanted to make an adjustment to the BWR table and go to the .07, and that would be .93.

MR. LEAVER: You had better put the number in rather than put an "S" there, because the "S" could mean one of a couple of things.

MR. BOYACK: Could you just give me the number then. Just say gap release, and give me the number.

MR. GIESEKE: Gap release, .07, .9300.

MR. BOYACK: You mean .63?

MR. GIESEKE: No, I am taking -- I am adjusting the --

DR. KRESS: The question is why did we adjust the PWRs to the .63 and the .3.

MR. GIESEKE: That is a good question.

DR. KRESS: Does anybody remember?

MR. NOURBAKHSH: Yes. It was 70 percent (inaudible) and in-vessel, because of the shorter duration, included that in the --

DR. KRESS: So, we want 5.95 or .70?

MR. NOURBAKHSH: Yes. And then in order to (inaudible) --

DR. POWERS: Well, I'm glad that has been corrected, and do you want my numbers now?

MR. BOYACK: Yes, because we are going to use numbers. Yes.

DR. POWERS: 0.07, 0.76.

MR. BOYACK: Well, that's why I wanted "S" as I can't type.

DR. POWERS: And 0.17., and zero.

DR. KRESS: Now, I like Dana's numbers now.

MR. GIESEKE: So now have you decided what is going where and in which slot?

DR. KRESS: Yes, now that Dana has fixed that middle one down there, and what he has done is factored in the larger fraction of the core, and so I kind of like --

MR. BOYACK: Do you accept those values, too?

DR. KRESS: Yeah, I like them.

MR. BOYACK: Okay. Dave Leaver.

MR. LEAVER: I guess --

DR. POWERS: Actually, for the core degradation part of it, and the relocation and anything after that.

MR. LEAVER: -- I was at .05 on the PWR, and until there is -- well, until I understand a better basis for the .07, I will stick with .05, and I will go with .65, because I just don't feel smart enough to quantify this increased power effect on the BWR, and whether that has any effect. And then .3 and then zero.

MR. BOYACK: Okay. I started with Jim the last time didn't I? Okay. Dana.

DR. POWERS: Okay. For the GAP release, I would say with the same number. I think --

(Discussion off record.)

DR. POWERS: Well, that's okay. Dave goes

along with me on everything. So, we never argue about anything. So, I will talk for him. I think it is .05. The in-vessel release fraction to the containment is .30.

MR. BOYACK: Is this like the French to the containment?

DR. POWERS: To the containment. The ex-vessel release is .15, and the late in-vessel is 0.12.

MR. BOYACK: Tom is going to ask you why now.

MR. GIESEKE: Are you sure that's not .11, Dana?

DR. POWERS: No, .2.

MR. BOYACK: Okay. Tom.

DR. KRESS: I have no reason to change the .05, and so I will go with the .05. I think in talking about a bigger fraction of the core releasing over a longer period, although we got a little bit longer time up there, and a bigger fraction of the core, and a longer period tells me that I am going to release more than I did in the PWR, and in the PWR, we had .35.

And I don't think it is going to be much more, but I am going to go to .4. And ex-vessel is what didn't get released, and the in-vessel was --

well, it is going to get released ex-vessel, and so I would take that about .65.

MR. LEAVER: And now you have more than one.

DR. KRESS: Well, I don't want more than one. Well, .6. So, what I need is how much of that was -- I am assuming gets platted out. So I still want the 40 percent, but the fraction of the late in-vessel in the PWR case for that was .2, and so that would have meant the total release in that in-vessel part was .55, and so if I am raising this to .4, my total release would have been -- if I add .2 to that, it would give me .6.

And there is a five on the end, and so it is .65, and that leaves me .35 to that one right there. And the other one would be .2.

MR. BOYACK: Down here?

DR. KRESS: Yes, and that should all add up to one.

MR. BOYACK: Okay. Dave.

MR. LEAVER: Hossein, if I could ask you a question. Could you just tell us -- I think I know the answer, but tell us what it was that -- what was the reason why the iodine release in the BWR was .25, and for the -- well, the early in-vessel was .2, or

whatever it was. It was .25, yeah.

So for the BWR, the early in-vessel iodine release was .2, and for the PWR it was .25.

MR. NOURBAKHSH: It had to do a little bit with the fraction and the retention in the BWR.

MR. LEAVER: And this was as a result of calculations on the average?

MR. NOURBAKHSH: Most of these numbers came mostly from the (inaudible), and so there were some differences between these values, and (inaudible).

MR. LEAVER: Well, we discussed at some length on the PWR about the high burn up effect, and we ended up with the same numbers. So I guess I am just not persuaded by the limited discussion that we have had on the BWR that in the absence of some careful calculations -- and maybe that is something that we ought to be doing.

But in the absence of that, then I would argue that the burn up effect, and the fact that we saw no change in the PWR, I am not going to change the BWR number.

So I would make it .25. And that is the same as the BWR number for the existing 1465, and then .3 and .1.

MR. BOYACK: Okay. Jim.

MR. GIESEKE: .05, and .35, and .2, and .1.

MR. BOYACK: Okay. Are we ready for a break? Let's take a break.

(Whereupon, at 3:06 p.m., the meeting was recessed at 3:06, and resumed at 3:21 p.m.)

MR. BOYACK: We are back, or at least the panel members, and there are the four of you that are doing BWR things. Let me expose you to one of the things that Charlie has had to say, which by way of concept sounds pretty good, and I would just like to pursue that.

If you go back to the original NUREG 1465, and you come down to this point, and then continue on down, there is absolutely no difference in the 1465 table between BWRs and PWRs from here on down. They were the same.

So, Charlie is making the point, and I would like to make it to you, is if there is any reason -- and we have gone through fairly detailed through these groups here, and some of them we finished up today.

We have partitioned them into some groups, and things like that, but the question is would we do

anything different with these groups for BWRs. In NUREG 1465, nothing different was done. Would we do anything different, and is there a rationale for doing anything different here.

DR. KRESS: Well, if I had any rationale, no. I am using the higher fraction of the core melt, but I would say for those down in that level, that the uncertainties in the numbers far and away outweigh that difference in fractions, and it probably would say to just wrap it up into the same change basically.

MR. BOYACK: Okay. Well, we will come down to this in a few moments, and what I wanted to do was give you a little lead time to think about that.

Now, Steve, you were going to go off and see if you could find somebody to -- well, did you want to try to articulate the flat core number four issue anymore? Will it be today or tomorrow?

MR. TINKLER: To introduce it on Friday afternoon.

MR. BOYACK: Yeah, you can go ahead for Friday afternoon and that will be fine. I won't be here.

MR. TINKLER: Me either.

MR. BOYACK: Steve was going to see if he could find somebody to articulate the condition that

the power uprates, in and of themselves, have resulted in flatter profiles. These cores have had flatter profiles in some time, but he power uprates have not substantially changed the profiles across the core.

DR. KRESS: That's news to me.

MR. TINKLER: I am not vouching for that. I am trying to find somebody who actually knows something about this. The point was that if that turned out to be accurate and defensible, then they wanted certain individuals to sit at the end of the table in a white shirt and no coat, and to just have that information.

DR. KRESS: I would love to have that, because we have been reviewing BWR power uprates for constant pressure power uprates, and what they do is they maintain the temperature channel the same so that they don't exceed the 2200 degree figure of merit, and design basis calculation, and so that means that the highest peak power is the same.

And I don't see that you can do anything else to flatten the profile, and either one direction or the other, and I guess you could flatten it across the length of the core.

MR. LAVIE: What is the difference that it makes, because even (inaudible) boiling water reactor,

they change their flux patterns throughout the cycle.

And in Westinghouse cores, the move rods and banks, and GE cores, they don't. At the nuclear sites, the (inaudible) cell has too much burn-up, he will pull a rod, and counterbalance that somewhere else in the core by withdrawing the water.

So throughout the cycles, they are changing their flux patterns, and maintaining a somewhat equal burn up. So what I am trying to raise is that I don't think the percentage difference on source terms due to power uprates is that much different than what they have already experienced by changing the flux around.

I think the effect we are looking for is going to be buried in the noise is what I am trying to raise. And not that the flux isn't flatter. You're right.

In order to maintain a constant steam pressure, and get an increased amount of steam flow, they have got to get more steam coming from the core, and without also changing the critical heat flux.

MR. TINKLER: So if you find somebody tomorrow, we will let them come in and talk a little bit more.

DR. KRESS: There are two things I would

like him to talk about, and they are how much flatter in the radial direction the profile is for uprates of 20 percent for BWRs, and if you go to extended power burn-ups, what fraction of the core is going to go to the higher burn-ups compared to PWRs. Those two numbers I think I need to factor into my thinking.

MR. LAVIE: The way that boilers tend to do this is that they mess with the recirc pump flow, and with the recirc pump flow, they move the axial flux around.

DR. KRESS: I am not adverse to saying that moving the axial flux doesn't affect the source term. In fact, I think it very well could. I don't know how actually it moves it, but --

MR. LAVIE: Well, plus the void coefficient, and it increases the flow of the recirc pump, and changes the void coefficient.

DR. KRESS: Which means that higher up you have less power and more now, which tells me that you are going to start setting up the steam zirc reaction at a lower level in the core than you did before.

I don't know how to factor it in, but I think it is going to include affecting things, even if it is the axial profile.

MR. LAVIE: Once you have had the trip,

the void coefficient is going to be relevant. The power moves to zero.

DR. KRESS: Yes, but the decay heat level --

MR. LAVIE: The decay heat isn't going to change (inaudible).

MR. BOYACK: These are the two questions that you asked and were stated up on the board here. Okay. Good enough. Let's do alkali metals then, and so who did I have stat last? Do you guys remember?

DR. POWERS: I'm ready to start.

MR. BOYACK: Okay.

DR. POWERS: Okay. 0.30 -- and this is the GAP release, and this is 0.05. And 0.30, and I think this next one if memory serves is the in-vessel phase or ex-vessel release, 0.10.

MR. BOYACK: I can turn it back.

DR. POWERS: And 0.22.

MR. BOYACK: Okay. Tom.

DR. KRESS: 0.05, and if you will give me a second so I can manipulate the numbers like I did before.

(Brief Pause.)

MR. BOYACK: If you are waiting for me, I will --

DR. KRESS: Well, I am inclined to make my numbers for that line exactly the same as the halogens.

MR. BOYACK: The same as what?

DR. KRESS: As the halogens.

MR. BOYACK: The same as you had for the halogens?

DR. KRESS: Yes.

MR. BOYACK: Okay. David Leaver.

MR. LEAVER: Okay. I don't -- on the basis of the fact that we did not increment the release fractions with the PWR, I see no basis for changing it for the BWR.

So I would rely on the 1465 calculations that gave these numbers. And so it would be .05, .2, .35, and .1.

MR. BOYACK: Did I get that right?

MR. LEAVER: Yes.

MR. BOYACK: Okay. Jim.

MR. GIESEKE: .05, .25, .3, .1.

MR. BOYACK: Okay. Now, we are to the point where we ask ourselves the question about since these groups were the same in 1465 for tellurium down to lanthanides, do any of the panel members perceive that there would be changes between the extended

applicability of PWR cables and the extended applicability of BWR cables for these parameters?

DR. POWERS: Yes.

MR. BOYACK: Okay. Tom, tellurium group.

DR. KRESS: Well, I want to hear data on it.

MR. BOYACK: Okay. We could do that. Now, let me ask another question. Are we going to want to create these subgroups as we did before, too?

MR. LEAVER: Probably so.

DR. KRESS: Yes. I thinking that they should be exactly the same for BWRs.

MR. LEAVER: Otherwise, you will run into the same problems of running into the same groupings as before.

MR. BOYACK: Okay. Dana, would you hold up on your thoughts.

DR. POWERS: Are you ready for the numbers?

MR. BOYACK: Well, I think the first thing he asked was what was going to happen in general, and a question about what.

DR. POWERS: Well, it goes something like this. I believe that the core degradation process within a BWR core is substantially reducing throughout

the process, because we have got a lot of zirc, and you have got to get rid of the channel boxes, and things like them throughout the incident.

I think that has the effect of suppressing the noble metal release, and I think the PHEBUS data are not reliable for the noble metal release in this case, because even in those tests where they attempted to have a period of reducing conditions, those reducing conditions were very brief.

And the noble metal release probably occurred when the test was at high temperature, and very oxidizing. I believe that things bet very high temperature in the BWR, but they do not get oxidizing.

On the other hand, that is going to accentuate the releases of the cerium and lanthanum groups. So the things that we were discussing in connection with those groups just are not applicable here. So I can't take the PWR extended applicability tables and just plug them in here.

MR. BOYACK: Are there any other comments on that particular view, because it seems to me that some of these things that I am thinking of, in terms of downstream of the document, and if I pull something like that out, I would like it to represent more -- I would like to know that it is not just one panel

member's view that the others either have held forth on the view or not.

DR. KRESS: I am not sure about the reducing or oxidizing conditions in the gas stream influences the release from the fuel, and I see how it might influence the chemical form that actually gets out of the fuel and it gets transported.

And maybe Dana could comment further on this, because I think it is more the oxygen potential within the fuel itself that determines the kind of speciation of fission products. And I can't see that reducing your oxidizing conditions in the gas stream is having a strong effect on that.

DR. POWERS: Well, I think you are clearly correct when we talk about the early in-vessel release from the fuel. But we subsequently have to discuss what goes on in the piping system to really gain numbers to the containment itself.

And there is some effect -- and it used to be a pretty profound effect, because you had just a lot more as zirconium wrapped around the fuel rods, and now the BWR fuels are beginning to look a lot like PWR fuel.

And you are probably right about the release from the fuel itself, and it is difficult to

see how you communicate. It does communicate, but it has to communicate through a clad, and the gas just does not communicate to that clad once the clad begins to disappear, and then things become more profound.

So I suspect -- I mean, what I am going to give you are numbers for tellurium that look much more like the older values of tellurium, because I think you do get caught up in the metals there, and that you don't get species.

And even if you get tin tellurium, Elwick and Willette did some experiments in which they looked at tin tellurate interacting with stainless steel, and they found out that it did. It's deposition velocity was a little lower, substantially lower than tellurium metal itself, but it did interact.

And it was driven primarily because the tin wanted to go into the grain boundaries on the stainless steel. So I think the net effect for tellurium is to look much like the original value, even if the release is substantially -- still looks like the original value.

Barium and strontium, again probably the release from the fuel prevails on fuel oxidizing until you get the clad interacting with the fuel itself, and then you get this mishmash coming down.

And then you are punching barium and strontium out. We have these incredibly low values here in the table for the cerium and lanthanum, which just are not bearing up, and for fairly oxidizing tests. I mean, relatively speaking, oxidizing tests. So I think we have to bring them up.

MR. BOYACK: Okay. So would it help if I had Dana give his values, and then continue on?

DR. KRESS: Yes.

MR. BOYACK: Okay. So we will break pattern here, and Dana, we are on the tellurium group.

DR. POWERS: The GAP release is .005, and again that was because we felt the high burn up, we were going to start getting a little inventory out into the GAP because of the high burn up that covers. So, .06, and .25, and .01.

MR. BOYACK: So, .06, .25, and .01?

DR. POWERS: Right.

DR. KRESS: And that .01 is -- you don't think very much of tellurium flattened out or is releasable once it gets flattened out?

DR. POWERS: Well, you could put a little bit of tellurium -- well, of the tellurium that is released, about half of it goes on to the piping system, and then when we subsequently expose it to

flow, some of it vaporizes off.

I really don't know how much vaporizes off, but again I think we need -- we very much need to get the results of the revaporization experiments from the PHEBUS program so that we can understand how things revaporize.

And we really never explored that issue. We have explored it in a computational sense, and verily, even at the relatively reducing atmospheres that you have in many of your boilers, and not strongly oxidizing anyway, but those that are inerted, still are inerted to the surfaces, and they are very oxidizing.

And of course the Mark IIIs are not inerted at all once you break the drywell/wetwell boundary. But we don't know how much gets released, and so all I am really reflecting in that one percent value is a sense that you will get some revaporization of the deposited material.

DR. KRESS: Okay. Am I next then?

MR. BOYACK: Yes, you might as well.

DR. KRESS: I like Dana's talk, and that power burn up will give you some tellurium in the GAP, and I will go with his .005. The .06 to me reflects an effective burn up on the old numbers that we had

for BWRs, and so I would go with .06 there.

And .25 is fine, and the .01, and so basically I agree with Dana.

MR. BOYACK: David.

MR. LEAVER: Yeah, .005, and we did use that non-zero number for the PWR, right?

DR. KRESS: Yes.

MR. LEAVER: I was waffling on the tellurium for the PWR, or otherwise I thought it was increased relative to what was in the original 1465 on the basis of recent test data, and the particular FPT-1. I thought the number .3 was too high.

And I guess a number more like .1 would have been what I would have used for a PWR, and perhaps the numbers lower for a BWR as Dana is suggesting because of a more reducing atmosphere above the core in the RCS.

But I don't -- I guess I can't -- it is hard for me to sit here and distinguish these effects without having the benefits of some calculations, and so I would say for the BWR that you use the same number, is what I would have used for PWR, which is .1.

MR. BOYACK: All right.

MR. LEAVER: And I would use .25 and .005

for the next two.

MR. BOYACK: Jim.

DR. POWERS: Let me comment that I personally do not view Dave's number as different than my number.

MR. LEAVER: I couldn't disagree with that. It is awfully hard to claim that you know -- well, I guess it is just a judgment call.

MR. BOYACK: And I wasn't disagreeing with you so much as to say that I just don't feel comfortable saying that there is a significant difference between the PWR and the BWR release in the absence of a lot more study, and in particular some calculations.

MR. NOURBAKHSH: The question of whether that 25 percent (inaudible) reduction or line of thinking that you had before with the (inaudible) vessel in the transient. How much of it. That tellurium ex-vessel right now is 25 percent of transient or --

DR. POWERS: Nearly all core concrete interaction.

MR. NOURBAKHSH: So this is different than PWR? PWR is (inaudible).

DR. POWERS: Yes, that's right.

MR. BOYACK: Okay. Jim.

MR. GIESEKE: .005, .05, .25, .01.

MR. BOYACK: Okay. We are going to move now to barium strontium with Tom.

DR. KRESS: I don't have any basis for really differentiating the barium strontium between PWRs and BWRs, and so I would go with the same numbers that we have had previously with strontium.

MR. BOYACK: So this is PWR (inaudible); is that right?

DR. KRESS: What did we have for BWRs before?

DR. POWERS: Two percent.

DR. KRESS: The same line? I think it was the same numbers down there.

DR. POWERS: Well, you had two percent in-vessel.

DR. KRESS: The same as the PWR.

DR. POWERS: Yes, the old ones were.

MR. BOYACK: We didn't change them when we went through this process of expanding applicability.

DR. KRESS: Those are PWRs, and the question was were the old BWRs the same as these two numbers. And if they are, I don't have any reason to change them since we didn't change them for the PWRs.

MR. SCHAPEROW: It is right after the word strontium, and it gives you the old release fractions of .02, .1.

DR. KRESS: No, I think those are the new PWRs.

MR. BOYACK: They are.

MR. SCHAPEROW: Oh, they are? I'm sorry.

DR. KRESS: My thinking was that on some of these others, the release fraction is higher up, and I factored in the fact that I still think the flatter core profile gives me a bigger fraction of the core melt.

And by rights then, I ought to factor that into these numbers here for all these here, and I ought to factor that into it, but like I said before, that factor is going to get lost in the uncertainties in these numbers.

And I am willing just to go with these numbers that we got from the PWR at that level, because I don't think that I can mentally factor in that level of difference, and that is like a small percentage difference compared to the order of the magnitudes in the uncertainties here.

So I am willing just to go with the same numbers for BWRs, because I don't think I can

differentiate.

MR. BOYACK: Okay. Dave.

MR. LEAVER: I would agree with what I think Tom said, which is if it is the same with what we decided for the PWR.

DR. KRESS: Yes.

MR. LEAVER: Okay. Right. I agree with that.

MR. BOYACK: I am just going to use this PWR EA for period extended applicability. Jim.

MR. GIESEKE: Same.

MR. BOYACK: Dana.

DR. POWERS: Well, when I sat down and did my calculations on these numbers, I actually came up with a little higher in-vessel release, but it is .024, as opposed to .02, and I hardly see a reason for changing that.

So I am the same across until we get to the late in-vessel release, and then I get to the late in-vessel release, and I think -- well, what I calculate is about one percent of the core inventory of barium, and about .1 percent for the strontium revaporizing off the surfaces over the 10 hour period.

So there is a substantial difference between barium and strontium on revaporization off the

surface, both of which are different than the zero. So I have integrated that together by saying it is .01 for the late in-vessel release.

MR. BOYACK: All right.

DR. POWERS: And again I will emphasize that it is strictly a theoretical construct on my part, and that needs to be substantiated by an experiment, and I am basing it on the volatility of barium urinae and strontium urinate.

And on the other hand, if it is barium zirconate, and strontium zirconate, the revaporization fraction is in the minuscule level.

MR. BOYACK: Okay. Let's move on. Now, the noble metals, that's where we started bifurcating isn't it?

MR. LEAVER: Yes.

MR. BOYACK: Bifurcating may not have been the correct word.

MR. LEAVER: Or trifurcating.

DR. KRESS: That sounds like --

DR. POWERS: And you might want to consider here whether you want to do the bifurcation, because one of the reasons for bifurcating originally was in contrast to the original PWR, we were seeing evidence of a little higher release fractions for some

of the noble metals and some distinctions between molybdenum and ruthenium in particular.

Here in this relatively reducing environment that we are going to have, I suspect that you are not going to see that relative volatility, and whether you want to change these noble metal release fractions substantially, if you don't, then you may not want to bifurcate here.

I know that I am not going to bifurcate.

MR. BOYACK: You are going to do what?

DR. POWERS: I am not going to bifurcate. I do not want to run across or run afoul of the laws of the home state, or of the former Vice President. What were the old numbers?

DR. KRESS: The numbers that we bifurcated for PWR.

MR. BOYACK: This is noble metals, right?

DR. KRESS: We had moly, and technetium as one set, and rubidium and rhodium, and ruthenium as another set. And the reason that we have different release fractions for those is because -- I am trying to figure out the oxidation relationship.

DR. POWERS: Well, what you have to understand is that we have the VERCORS data showing substantially high molybdenum releases, and we have

the PHEBUS results that show ruthenium moving around. All these tests are paraoxidizing in nature, and I just don't think you get oxidizing conditions in the BWR core degradation.

DR. KRESS: But that wouldn't affect the moly.

DR. POWERS: You keep the moly down in a metallic state, and even if you do it not in the fuel, but out in the environment, it deposits. The only way you can move moly around is to turn it into the hexavalent state.

DR. KRESS: The ruthenium releases up there look to me like they might already be for non-oxidizing conditions.

DR. POWERS: I think those releases reflect mechanical effects.

MR. CLEMENT: Just one point. You look at the VERCORS, that is for ruthenium, and also movements in the reducing conditions. I don't understand why.

DR. KRESS: Well, moly I think comes out producing (inaudible) in some of the data that I have seen, but my feeling is that the ruthenium numbers would come up with something other than oxidizing conditions already, like mechanical releases.

And the molys will come out at a higher

rate, whether it is oxidizing or reducing, based on the information that I have seen. So I would have kept those numbers as they are basically the same.

MR. BOYACK: We have had discussions now, and so I am going to turn it over to Dave Leaver since he is now first on the list. I just wanted you to have that chance to talk.

MR. LEAVER: Well, Bernard, you said something -- and I was just looking at VERCORS.

MR. CLEMENT: VERCORS 4 and 5, yes.

MR. LEAVER: VERCORS 4 and 5, yes. And four is reducing, and five is oxidizing, and then your ruthenium fraction release is the same.

MR. CLEMENT: Yes.

MR. LEAVER: So I don't know. These are all really good points that people are making, but yet I think even for the PWR, we probably should at least recognize that one could get reducing conditions for at least some portion of the accident while fission products are coming off.

So I am not sure, but yet in the absence of a lot more data, experimental data, and/or calculations, I just am uncomfortable proposing something substantially different for the BWRs than what I did for the PWR.

So I would propose the same type of bifurcation and the same release fractions.

MR. BOYACK: Okay. So here would be the PWR, and so I would go to the PWR table for David Leaver, and I would pick out the same values.

MR. LEAVER: Right. I would also just add the point that I have or I believe that 1465 was a very well documented study, and while it is certainly easy in hindsight to say we could have done this or could have done that, there is a well-documented basis for saying the PWR and BWR release fractions are the same once we get down below the halogens.

And that is within the uncertainties, and I find that to be somewhat persuasive here, in terms of the concept of making them the same.

MR. BOYACK: Okay. Jim.

MR. GIESEKE: Well, I find myself in about the same position as the others with respect to the PWR that I put down there.

MR. BOYACK: Okay. I want to remind myself that I am doing something general, and I am doing it on a name basis. Okay. Dana.

DR. POWERS: 0.0025; and 0.0025; and zero.

MR. BOYACK: Tom.

DR. KRESS: PWRSA.

MR. BOYACK: Okay. Good. The cerium group, and it turns out to be you, Jim.

MR. GIESEKE: I am going to go with the same logic again. I can't see any reason for changing beyond what we have talked about. So I will go back to PWRSA.

MR. BOYACK: Dana.

DR. POWERS: Your see, your definition of PWRSA is the extended applicability table?

MR. LEAVER: If you go back on a name basis and pick out the same value from the tables that we generated.

DR. POWERS: Okay. I believe I could probably live with PWRSA.

MR. BOYACK: No matter how it is pronounced. Tom.

DR. KRESS: PWRSA.

MR. BOYACK: You didn't get much time to ponder this one, Dave. It came back real quick.

MR. LEAVER: I am all right with that.

(Discussion off the record.)

MR. BOYACK: Okay. The lanthanides, and Jim, you were the last one. I really am getting --

MR. GIESEKE: I did the last one.

MR. BOYACK: Okay. So it is Dana.

MR. GIESEKE: But I will do the first if you like.

DR. POWERS: Zero. You can give it to Dave, but I bet he doesn't agree with me on the next one.

MR. BOYACK: And it is?

DR. POWERS: Zero, 0.01 and zero. Actually, 10 to the minus third, but I think that is close enough to zero.

MR. LEAVER: Brent, could you put up the PWR table for a second for lanthanides.

(Brief Pause.)

MR. LEAVER: I sit possible to print that just so we could look at that? I guess we don't have a printer here do we. Never mind. That's all right. Never mind.

MR. BOYACK: I am going to give him a 3-1/2 inch floppy this time that has all this stuff on it.

(Simultaneous discussion inaudible.)

MR. BOYACK: Okay. Where are we?

DR. KRESS: I think we are down to me. PWRSA

MR. LEAVER: PWRSA.

MR. BOYACK: Head nod.

MR. GIESEKE: We are getting lazy and not talking.

DR. KRESS: What table is this? Is this MOX?

(Simultaneous conversation inaudible.)

MR. NESBITT: Our intention is to ask for regulatory burn up limits of the maximum of 50,000 megawatt days per ton basis, and the average burn up we would anticipate would be about 43 or 44.

MR. LEAVER: Is that pretty much the same as your existing core?

MR. NESBITT: The existing core is a little bit higher than that, and is in the range of an average assembly burn up.

MR. LEAVER: Well, I stand corrected. They are the --

DR. KRESS: Well, we don't want to factor in high burn up and MOX at the same time.

MR. NESBITT: That's right.

DR. POWERS: Actually, I don't think they really want it. They take too high a burn up because they just create more plutonium than what they burn up and they do that; and once you get over above 20 or 25,000, you have got the isotopic mix addressed.

MR. LEAVER: Which is the whole point, I

guess.

DR. POWERS: Yes, and that is all that you are accomplishing. In fact, a lot of the Oak Ridge Calculations -- and I am sure that Ed Lyman will be glad to point out to us tomorrow that you end up making plutonium.

MR. BOYACK: Okay. What I did was that I tried to go through Steve Nesbitt's presentation last time, and see if we could get the characteristics of the MOX fuel down that we are going to use in common as a panel.

So feel free to correct anything that I have got up there, and I think that you had gone through this, Jason, and looked at little bit at this, too.

But as I was going through this, there was information on pellets, which were five percent with molybdenum, with approximately 95 percent completed uranium oxide.

The reactor grade plutonium information wasn't any U.S. It was all European that was listed in the guides, and it was 60 percent, Pu 239, and something greater than 20 percent Pu 240.

The cladding was M-5, and the field assembly was identical to small and enriched uranium,

except for the fuel pellets.

MR. NESBITT: On the plutonium, it is a little confusing, because that was the European -- what we call the plutonium isotopic vector, but that is not what was used.

MR. BOYACK: Would you tell me what we are proposing to use?

MR. NESBITT: Yes. It is about 93 percent of Pu 239, and about 6 percent Pu 240.

MR. LEAVER: It's what you are going to get, right?

MR. NESBITT: Right. We are going to use what we get. And that is it in round numbers.

MR. BOYACK: This may have been in the presentation.

MR. NESBITT: I don't think I put that in the presentation.

DR. POWERS: What do you think the americium is going to get --

MR. NESBITT: It is going to be polished shortly before the fuel is fabricated to remove impurities and that will improve the americiums, and the americiums would be the trace level.

There will be a little bit of decay in the (inaudible).

DR. POWERS: How much per gallon do you think they will get out?

MR. NESBITT: Supposedly down to PPB levels.

DR. POWERS: How much do you think their quality control is going to ensure that that is the case?

MR. NESBITT: To be quite honest, I don't think it matters whether you are PPB or PPM.

DR. POWERS: How about one percent?

MR. NESBITT: One percent would be a little high.

MR. BOYACK: So the fuel assemblies themselves were identical to LU?

MR. NESBITT: Yes.

MR. BOYACK: And the cycle length was about 18 months?

MR. NESBITT: Right.

MR. BOYACK: And then we had a typical, a maximum, and a limit on the burn up per day.

MR. NESBITT: The confusing part there is that typically it will be in the low 40s, and I would say probably more like 42 based on the way the cycles are, on an assembly basis.

MR. BOYACK: Okay.

MR. NESBITT: But you could get some on an assembly basis on about a 4 to 6. The absolute limit on 10 days is (inaudible).

MR. BOYACK: Okay. Thanks.

DR. POWERS: Well, one of the critical issues that will be discussed at nauseating length in connection with this fuel is the plutonium particle size distribution.

And DOE has come in and specified a size distribution that seems to allow some pretty big particles.

MR. LEAVER: Are you talking about the fabricated ones?

DR. POWERS: Yes.

MR. NESBITT: It is not really exactly widespread. DOE doesn't have anything to do with the specification on the part of the size, or any important fuel -- they are not a player in that game.

We are using essentially the Framatome specifications, and which is identical to what is being used for European reactor MOXs. We use the same specifications.

Patrick Multan presented some information last time that showed some of the actual particle size distributions from their fabricating experience, and

that is what we would expect to see since we are using the same manufacturing process, in terms of the same plant.

MR. CLEMENT: (Inaudible) two sizes.

MR. NESBITT: Right.

MR. BOYACK: Are there any other parameters that you would like to have captured?

MR. LEAVER: Is that 40 percent number right? 40 percent MOX fuel assemblies?

MR. NESBITT: That is based on our current core designs, and we would see that as a maximum. And what that means is that we load actually more than 40 percent MOX fuel assemblies in each batch.

And for a batch of, say, 80 assemblies, you might see 36 MOXs, 44 uranium assemblies for a given reload, but we are planning to run the MOX assemblies in two cycles, and we will uranium assemblies, some of them in three cycles.

MR. LEAVER: So are you really going to have 40 percent MOX assemblies throughout the core?

MR. NESBITT: In the core, yeah. In a given batch, it might be more than 40 percent MOX. I guess I would like to make a point here that was mentioned earlier, or comment on something that was mentioned earlier about a third of the fuel in the

core being hot burn up at one point in time.

The reality in our current core design scheme is that we discharge most of our fuel after two cycles, and people don't realize that typically, but for at least for our four Westinghouse pressurized water reactors, we use the discharge, and we load about 80 per batch.

We have got 193 in the core, and you do the math on that, and you only run 33 of them through the third cycle. So there is really less high burn up fuel in the core in any given point of time than you might think than just a single third, third, and third.

MR. LEAVER: And you are on an 18 month cycle?

MR. NESBITT: An 18 month cycle.

MR. LEAVER: And I don't have the data to prove it, but I would speculate that most of the similar plants in the country are running 18 month cycles and doing it pretty similar to the way that we are. If you go to other reactor types, like BWRs, I have no knowledge and won't speculate.

MR. BOYACK: The objective of this particular portion of the activities is just to get a common set of characteristics with regard to a MOX

core. Is there anything else that needs to be captured?

MR. CLEMENT: Yes, maybe one. The radiation follow-up also. If you will remember what you presented last time, and I don't remember if it was you --

MR. NESBITT: Well, I brought all the information that we presented last time, and so if I don't remember it, I can look it up.

MR. CLEMENT: If you will look at gas release, and with burn up, or something like that, and the dependence with the maximum (inaudible) power and (inaudible), and this could also be a piece of information.

MR. NESBITT: That was brought out, I think, in Patrick Multan's presentation.

MR. CLEMENT: Yes, he gave a presentation.

MR. NESBITT: And that showed the predicted models and data for fission gas release and there was a bunch in the burn up, and also a bunch in the power, and that was the key point that he tried to make, that the biggest driver for the MOX fission gas release is not that it is MOX.

It is the fact that because it is MOX that it has a different power history for higher burn up,

or excuse me, higher power than for higher burn up. I don't think you can capture that on just one line on your slide.

MR. BOYACK: Maybe not.

MR. LEAVER: But he is going to try.

MR. NESBITT: Although I will just complicate it a little bit and say that I think that is probably more applicable to the French core design approach, and their hybrid core management approach that they use than it would be for the one that we are proposing.

And that is a question that I will look into a little bit tonight and get some data back on that for you.

MR. CLEMENT: It depends on the core management, of course.

MR. NESBITT: Right. I think the fact that we are going to withdraw our MOX after two cycles, and we are really not going to run any of it in the exterior core positions, and the one core power position, means there won't be that much of a difference.

MR. LEAVER: Is that going to be part of your licensing basis, that you will only run the MOX assemblies for two cycles?

MR. NESBITT: Well, we are proposing the limit on maximum fuel pin burn up of 50,000, and that will almost effectively require that we only run it two cycles.

MR. LEAVER: Okay. So the limit is the 50,000?

MR. NESBITT: Yes, the limit is the 50,000.

MR. LEAVER: Is it fair to say while we are on the subject here -- I have a copy of Mr. Lamplan's (phonetic) presentation and there is a fission gas release as a function of burn up for U02 and for MOX, and this is based on EDF data, where I guess just surveying spent rods.

And it looks like if I am able to distinguish these dots, that you see up at around 50,000 that you are seeing for UO2 that the numbers may be as high as 2 or 2-1/2 percent.

And for MOX it is like a factor of two. Is that a fair characterization of that in your mind?

MR. NESBITT: Do you have a page number on that one?

MR. LEAVER: There is one, but I can't read it. It is about three-quarters of the way through, and -- well, I will show you. This is the

one.

MR. NESBITT: I think that is an accurate characterization, and if Patrick was here -- and he is more of the expert than I -- he would say that effect again goes back more to the power history and the higher powers at the burn ups of 40,000 or above, than it does for the fact of the MOX.

Now, Mr. Clement may have a comment that he may care to make.

MR. CLEMENT: It could be both.

MR. NESBITT: Yes.

MR. CLEMENT: Because you may have higher power, but also you have a much higher local (inaudible) -- at the same power.

MR. BOYACK: Okay. Anything else? MOX assemblies typically to be withdrawn after two cycles? NO.

MR. GIESEKE: Are you citing any differences between --

MR. CLEMENT: The last line is more difference. The first lines are characteristics of the MOX are to be used, and the last one is a more general statement from differences between MOX fuel and low enriched uranium fuel.

What would be an interesting

characteristic throughout, and not for our meetings now, is just to state that it depends on the core arrangement and the core history of each MOX.

MR. NESBITT: We have some information on the proposed fuel management and the fuel qualification plan, which I am trying to remember if this committee or group got a hold of that or not.

We provided it to the NRC, but I don't know if you all were distributed that document.

MR. CLEMENT: We were distributed one document about assessments, but it was not quite -- I am not so sure it was related to exactly what is foreseen in the reactors (inaudible).

MR. GIESEKE: Have we resolved all the questions relative to the oxidation -- surface oxidation on M5 relative to the other findings? There was information to be gathered wasn't there about the intersurface oxidation? I am trying to remember. Does anybody remember that?

DR. POWERS: The major controversy was at what level of oxidation do the zirconium-niobium alloys embrittle, and there was or has been a report from an investigator in Germany, I think, who was arguing that they embrittle at lower oxidation levels than the 17 percent that we allow for a LOCA accident.

And it was presented information that was not the case, and Ralph has a program where he is having to investigate it, and it is like a lot of these mechanical properties, that a little bit depends on the technique we use to measure it.

And the German investigator was using a technique that is thought to give lower embrittlement results than what most other people use, and the regulations specify exactly what you are supposed to use, and he was not doing that.

So now Ralph is looking at it, and my perception -- and without having heard any final results, is that the M-5 is actually better and not worse. Certainly it oxidizes less during normal operations.

DR. KRESS: And does it affect the GAP release?

MR. NESBITT: I believe the German data was based on some samples of cladding that were actually Russian if I am not mistaken.

DR. POWERS: It was a Zirconium-niobium alloy.

MR. NESBITT: Compositionally, they were the same, but in terms of how they were manufactured, and processed, it was significantly different, and

(inaudible).

DR. POWERS: I think even compositionally that they were best described as similar and not the same. And my perception is that Fran Adams come back with his data that will show that the ductility is actually better in the Niobium alloys than in the tin alloys.

And so I don't know if the issue is closed, but it just doesn't seem like -- there is no smoking gun that says we have got a problem. I think the problem, if it were to manifest, and there is really very little during the source term that is acceptance criteria for ECCS performance.

And it might cause some variation of exactly where you pop the clad, but I think that is not so crucial as whether the ECCS was acceptable or not.

DR. KRESS: I think you are right.

DR. POWERS: And like I said, my perception of this is that there is no problem there.

DR. KRESS: And a more ductile material may block the core worst than -- and depending on which it goes.

DR. POWERS: That could be, but the regulation has a pretty well described acceptance

criteria.

MR. BOYACK: Okay. Now I can return this back now. I just took your suggestion that I would go ahead and characterize this as being different rather an a MOX inherent characteristic.

MR. SCHAPEROW: The question is what kind of reactor is going to be used? I guess that was important for some of the reducing oxidizing issues. Is it all going to be PWRs?

MR. NESBITT: No, our plans are to use it in four loop Westinghouse designs, water reactors (inaudible).

DR. KRESS: That's because of the ice condensers, which are most suited for MOX fuel than the --

(Laughter.)

DR. POWERS: A more interesting issue is to see how these ice condenser plants stand up to 757s orders with gasoline.

(Discussion off the record.)

MR. LEAVER: Let me ask a question. The MOX assembly would seem that it would -- that you basically are going to have a higher power throughout the time that the assemblies are in the reactor, and from that assembly, versus a UO2 assembly; is that

right?

MR. NESBITT: No, that's not true.

MR. LEAVER: That's not true?

MR. NESBITT: No. And in fact, my anticipation is that due to some slightly greater uncertainties in power distribution predictions, and things like that, they will actually have a slightly lower peak limit, in terms of power, and FQU, and FWA, and things like that.

But we are going to use this in a manner -- we are going to control the power of the assembly, using things like burnable poisons, and core loading, et cetera.

So that the fuel management with MOX is very similar to our current field management with LEU fuel. So I guess what I am saying is --

MR. LEAVER: So you are saying that the peak power wouldn't be any higher?

MR. NESBITT: Right.

MR. LEAVER: But at the end -- and let me try and say this differently. If you are at the end of a cycle, isn't it true that the MOX assemblies will have a higher power than a UO2 assembly would that has been in there the same amount of time?

MR. NESBITT: And in the same location and

in the same burnable poison?

MR. LEAVER: Right.

MR. NESBITT: Another way to characterize that might be saying that in the overall field management strategy, we might see MOX carrying less of the power at the beginning of the cycle relative to LEU and more at the end of the cycle relative to LEU.

But all of the assemblies, of course, would be less power peaking limits.

MR. LEAVER: Right. Right.

MR. CLEMENT: That means that for short term, when you look at (inaudible), you have probably come to the end of the cycle, and at that time you have more power, and the more power you have at the beginning of an accident in a MOX fuel, then you (inaudible), and this is the difference.

MR. SCHAPEROW: At the beginning of the cycle, we have less.

MR. CLEMENT: Oh, the beginning of the cycle --

MR. LEAVER: You will have less plutonium at the end of the cycle than you did at the beginning?

MR. NESBITT: Yes, less in the MOX.

MR. LEAVER: And so you have less plutonium at the end, and now I am wondering --

MR. NESBITT: When we do dose calculations, we are always looking into --

MR. LEAVER: You always do, and that's right, and maybe that is still the right thing to do. But it is true that you have less plutonium in the MOX assemblies?

MR. NESBITT: Well, yes. We typically --

DR. KRESS: Plutonium doesn't drive any of the regulations that I am aware of.

MR. NESBITT: No, iodine tends to.

MR. SCHAPEROW: Not yet. I'm just kidding. Iodine is still way ahead.

DR. KRESS: Well, the release rate is to one percent, then --

MR. LEAVER: And are we smart enough to -- I don't think any of us are smart enough to sit here and do it, but if we did calculations -- for example, MELCOR. You are pretty familiar with MELCOR, right, Jason? You guys have done a lot of calculations. How do you model the core? How many nodes do you have for the core?

DR. KRESS: Not very many.

MR. SCHAPEROW: Not too many.

DR. KRESS: Actually, I think it is 3 or 4.

MR. SCHAPEROW: Not too much right now. We only have three radial rings, and even for the recirc it is only flow calculations. And maybe we have between five axial --

DR. KRESS: And MELCOR doesn't know anything about MOX fuel.

MR. LEAVER: Right. So you really couldn't calculate if there is an effect, but if there were an effect due to the fact that you have got at the end of cycling we worry about the source term if you have a higher power -- if your MOX assembly is a higher powered assembly, you are saying that we can't calculate that?

MR. SCHAPEROW: We model the corner rings, and if one of the rings can be identified as a (inaudible), but I don't know how they are laid out in the core.

MR. NESBITT: David, in the severe accident analysis, we tell the code what greater nucluoride inventories are, but I am not saying the code --

DR. KRESS: Do you calculate that with a more detailed model or scale?

MR. NESBITT: Right.

MR. SCHAPEROW: We assume radial symmetry,

and I would think MAP does, too.

MR. NESBITT: So if you can analyze that effect, for example, per a given accident beginning in the cycle, worst or better within the cycle, simply by loading those inventories in, and to the extent that you have confidence in the ability of the code to calculate the releases, then you can assess the impacts from that.

DR. KRESS: Right.

MR. SCHAPEROW: We may end as high as five radial rings on a 10 axcisa (phonetic), like this did with --

DR. KRESS: But you are never going to capture the details that we are talking about.

MR. NESBITT: I don't think so.

MR. BOYACK: Do you feel like you have enough specification the MOX to go ahead and actually begin proceeding with the work of the source term characterization? We call it extended applicability, but it has not been done before, and so it is a little different.

Do you have enough to go ahead by way of specification?

DR. KRESS: Yeah, and if there is anything else to affect it.

MR. GIESEKE: Did you bring all your release rate data in to discuss with us, Tom?

DR. KRESS: That is not a specification, but --

MR. SCHAPEROW: When you say typical, as typical end of life; is that what that is?

MR. NESBITT: After two cycles, yes.

MR. LEAVER: One other question, Steve. Is there a substantial difference in inventory? Well, let's just take iodine, which I know is important.

MR. NESBITT: Right.

MR. LEAVER: An iodine inventory in a end of cycle MOX assembly, versus end of cycle UO2 assembly?

MR. NESBITT: I think it depends on the isotopes.

MR. LEAVER: What I have from before for a 131 was a radio of about 1.02 (inaudible) and a 1.74.

MR. NESBITT: And you said that the iodine is only about 2 percent higher?

MR. LEAVER: 131, that's correct. Krypton is 68 percent, and --

MR. LEAVER: And ruthenium would be the most extreme, in terms of the difference between the

two assemblies?

MR. NESBITT: Xenium 135, and 1.6 in MOX (inaudible), and so those are the two that have a significance, and the rest are -- and in krypton, a much lower rester than having the .9 to 1.1 range.

MR. LEAVER: Thank you very much.

MR. NESBITT: Well, the information came from a calculation, but it is not a published record.

DR. POWERS: If I am not mistaken, there are whole suites of calculations done at Oak Ridge on burning MOX.

MR. NESBITT: Yes.

DR. KRESS: And there exists calculations on these inventory differences, and I have seen them, but I have forgotten what they were. But I think they are consistent with what he said, that only a few of the isotopes changes considerably, and I just don't remember the numbers. They have the numbers and they have been run.

DR. POWERS: I can't recall the fellow that was in charge of that program, but the same name shows up a lot on the publication.

DR. KRESS: Yeah.

MR. NESBITT: Trent Prim.

MR. GIESEKE: If the inventory is the

same, then we can identify the major differences with what two there are, and think about what that does to the chemistry, which may not be too big of a deal.

DR. KRESS: I don't know if the inventory is going to affect fission product release much.

MR. GIESEKE: What?

DR. KRESS: I don't think it is going to affect the release fractions of the inventory. We think it is what he MOX does to the tool characteristics that is going to take more.

MR. LEAVER: You mean in terms of --

DR. KRESS: Release fractions.

MR. LEAVER: Well, when you say what the MOX does to the fuel characteristics, which fuel characteristics are you talking about?

DR. KRESS: The plutonium and --

MR. LEAVER: Which is mainly going to be a gap --

DR. KRESS: Well, I am not so sure of that. The plutonium tends to separate itself from the uranium and hang out on the edges of the uranium grains, and from the standpoint of how much of the fission products are born in the plutonium, versus the regular plutonium, there may be a distribution difference of where the fission products reside in the

fuel.

And that distribution could have taken a release fraction, and that is one of the things that has been speculated to have affected the reactivity insertion accident, and just where the fission products reside in the fuel.

MR. LEAVER: And when you say where it resides, you are talking about within the fuel pellets, and residing let's say at the edge of the pellet, versus at the center?

DR. KRESS: Within the grains that make up the fuel pellet.

DR. POWERS: And in LEU, you have lots and lots of intragranular bubbles, and they are very, very tiny. And with this grain --

MR. LEAVER: Intragranular?

DR. POWERS: Within a grain, and what I am talking about is when you are burning the flute itself to get a corona about around it, and where those bubbles may no longer be behaving like gas bubbles.

And so you put power impulse into them, and they want to expand rather than just redissolve.

DR. KRESS: But I don't know if that affects the fission product release for LOCAs, as opposed to RIAs.

DR. POWERS: Well, I think what you give with the data on that is that you affect the release rates.

DR. KRESS: Well, I think with respect to the mobility of the stuff in the grains and stuff, and that goes into the rate, and maybe feeds the rates back into the overall integral amount, because you have got a fixed temperature profile, and so the rates will affect the total amount of release, which goes back to the fractional release.

And so I would have to see some data before I can figure out how much --

(Discussion off the record.)

MR. LEAVER: In the interests of trying to understand the impact of MOX, how much more plutonium, or less, is there -- probably more -- in a MOX assembly at the end of the cycle than in a UO2 assembly? Is it like a factor of two, or --

MR. NESBITT: If you look at a PWR fuel assembly, once you have burned it a couple of cycles, you are up towards about one percent plutonium; and with the MOX assembly, you start at an average across all the pellets of 4.4 percent say of plutonium, and by the time you discharge after a couple of cycles, you are down to around 3 percent plutonium.

So you are talking about burn up in the forties of a difference of about 3 to 1 or more than that --

MR. LEAVER: Would this affect whatever it turns out to be that you are talking about, possibly a higher rate due to the plutonium oxide? You see that I guess to some degree, even in uranium fuel at the end f the cycle just due to the presence of the plutonium.

DR. KRESS: I don't know how it would be affected unless I see some data. I can't make a model for it.

MR. BOYACK: Okay. Let's see. These are the characteristics, and we have a few differences. Let me just take a moment then and talk about how we might proceed tomorrow. In effect, this is the table that we have to fill in, right?

But what I think we ought to try to do is basically fill in the tables here. Let's see. We still have a GAP release here, and --

DR. KRESS: Are you going to add high burn up on the title?

MR. SCHAPEROW: Yes, please do. You might want to add PWR to that, too, because we did have some different issues.

MR. BOYACK: Let's see. PWR containment or PWR model --

MR. SCHAPEROW: PWR reactor. If the panel agrees, we need to distinguish for something that we do.

MR. BOYACK: Release fractions. Let's see. I think we are retaining this word aren't we?

(Laughter.)

MR. BOYACK: In effect, quite what happens in these panels is that we learn as we go, and the last meeting turned out to be very, very effective, and we have used it again, which is that we basically make these main comments, and just go and let everybody have their input regarding that.

And so that is what I would propose to do, and I will have these tables slightly updated tomorrow. But basically it will be what we would have Dave Leaver do, and he would go ahead and hold the fort on the GAP release.

And this happens to be the duration, and it is the duration part of it that we are dealing with right here. So if that is all right, that is how I intend to proceed.

We have a day-and-a-half to work this, and it seems to me that it is achievable, because you are

in such a groove now, and we may be able to go home at noon tomorrow after doing the MOX.

And I watch Dana, and when he is sitting there writing, it usually means that he is doing his calculations numbers, and so he is ready to go. Usually.

DR. POWERS: I'm just trying to understand how we are getting rid of plutonium from the inventories and what not.

MR. BOYACK: If you go from 4-1/2 to 3?

DR. POWERS: Yeah, and if you take 60 percent of the core and go from zero to one, and it is not obvious that we are getting rid of any plutonium there.

MR. LEAVER: The key is to normalize or the key is to change 95 percent to (inaudible) 239 to a mixture, which is 240, isn't that right?

DR. POWERS: Why don't we just grind it up and mix it then?

MR. BOYACK: One of the questions that occurred to me as we were talking was if you have this core that is now mixed with LU and MOX, and we are looking at the source term for this fuel, which has this combination of several things, are you to the point where you are able to sort of --

DR. POWERS: You know what, I am going to come down and say just over and over, and over again, is that I have some vague understanding, limited understanding, of some of the microstructural things that occur when you make MOX fuel, and I can relate that to some of the early parts of the fission product release.

And I can say qualitatively some things move up and some things move -- almost nothing moves down, but where data start. And it seems to me that the real chore tomorrow is going to be where fill out the tables fairly expeditiously as you indicated, but the real chore is to come down and say if you want to do these tables right, here are the kind of data that you need.

MR. LEAVER: Yes.

DR. POWERS: And I think that takes some thought.

MR. BOYACK: Tom.

DR. KRESS: I am going to be very uncomfortable when giving the numbers at all.

DR. POWERS: I don't know what the database that we have, but I will tell you that the database that we are deriving most of our fission product releases from has been a fairly unsatisfactory

database, and constructed very heuristically.

And there has been lots of work that has been done saying let's get some quick numbers here, and there was not this interaction between what the code really needs and the experimental program, which I think has become the norm now for these experiments.

It seems to me that we ought to spend some time and say do these experiments, and do them this way.

MR. BOYACK: Tom, you started to say something about being uncomfortable with any numbers at all?

DR. KRESS: I have an intuition that MOX fuels is going to give higher source terms, higher release practices. How much higher? I haven't the vaguest idea without some data. I just don't know how to quantify it.

MR. BOYACK: And, Bernard, are you fairly comfortable with working through to a MOX system?

MR. CLEMENT: I have two points. I think we should before giving values try to point out what are the differences, and where we have some indication where we don't as the first step for filling up tables with numbers.

And if we feel compatible with MOX, and I

would remind you that our source term that we used that made our reevaluation, that a few of these somewhat envelope, and you have seen that from, generally speaking, the values that you have given during this panel.

And they are very often higher than the values of the other members of the panel. So it is much easier to cope with the uncertainties that are still existing for most, and for which we need more data.

And so it depends on the use that you make from the numbers you put in the tables, and you accept that there is some conservatism.

MR. LEAVER: Bernard, at the last meeting, you talked about the RT test, RT1 and RT2.

MR. CLEMENT: HT1 and R27

MR. LEAVER: And then HT1 and R27, which were good comparisons for UO2 versus MOX, and they have similar burn ups and peak temperatures. And you said that you observed faster releases in one, and the other the opposite, but that there was no data. Is there data available for those?

MR. CLEMENT: I don't have the data, but there are some things that I could present to clarify what was said last time.

MR. LEAVER: We are very data starved, and so I think it would be useful. I mean, I don't know what we are going to decide to do here. We may not be able to fill this table out, but there were two VERCORS tests that were pretty good comparisons, because everything else was almost the same, except UO2 versus MOX.

MR. BOYACK: Okay. What I have done is I have started to create just the form of a table, which we can sit around and talk about, which is particular characteristics of what it is with low enriched uranium, and what it is with MOX, and if this would meet the data needs with the experimental amenities that would spring from that.

And I think you suggested that this would be a good first step to start. Is that the type of thing that you had in mind, that discussion? And then I would try to capture some of it on a table, and we would have the transcript in addition.

But it helps if I can get a little bit down and print a copy, and have it in front of people.

MR. CLEMENT: Basically, the same information from last time, that maybe with MOX the use of more explanations. So I will say that David that the RT1 and HT1 (inaudible), and this one is

comparable to the RT1 test from which you drew, and this one comparable to the HT1 from which you drew.

Okay. This one, RT2, was reducing conditions for RT2, and oxidizing conditions. The two fuels were quite similar, and you can see the results, and (inaudible), and do you put three pellets here, and heat them up with (inaudible), and you get everything in the filter here (inaudible).

And then you have the measurement on line on the fuel, and have you going out, and measurements on line on the filter, and you go get (inaudible).

There is also an important addition between the two tests, and that the first one after fuel was not (inaudible), and so you don't have the measurements of a short life fission product. You only have measurements of long life, and measurements (inaudible).

The other one was more greatly (inaudible), and we have much more measurements, both from the fuel and from the filter. Now, the two fuels are quite similar and occupying (inaudible), and then oxidizing the tests and reducing tests.

And in both cases, before coming to this rate here, the (inaudible) oxidizing conditions, there is a plateau in which cladding normally (inaudible).

So the point is and which complicates the situation, is that we know fuel degradation has a large impact on short term, and fuel degradation has been very different in (inaudible).

And so in the RT7 test, in oxidizing conditions, fuel relocation has been observed at nearly 3000/xelvin (phonetic). In fact, there is a very good measurement of fuel relocation by having the gamma (inaudible) measuring just the top of the pellets and looking at the volatile fission pellets (inaudible).

And that is a good indication that it will relocate, and that has always been confirmed afterwards by additional examination. So the temperature was about 500 degrees lower, and that is the same for the (inaudible).

MR. LEAVER: You maybe were saying you get eutectic is what you were saying?

MR. CLEMENT: That is one explanation. And the people in charge of the experiments will try to make confirmation tests on these relocation (inaudible) temperature is very important. And if you look at the final state of degradation, they are quite the same.

These are reviews coming from the

(inaudible), which is the bottom, and this is the top, and (inaudible), and the upper is really eroded here (inaudible).

And if you look at the uranium distributions, and you do all the tests, they would give you an indication of fuels that we have here, and fuels (inaudible). So here there is a little bit more degradation, but you have to remember that 500 degrees (inaudible).

So that is the first thing, and this was for conversion of the (inaudible), and as I said before, for the RT1 test, we are less better than for the RT7, because the (inaudible), but this is an interesting comparison between RT1, RT2, and for another test, RT4 (inaudible).

And further the temperature evolution for the RT1 test (inaudible), and this is 2700K, and this is for the RT2. Forget the spikes here. And the indication earlier in the MOX fuel at that point, and in the MOX fuel at that point.

You can forget the spike here that would be for the measurement. But what is quite interesting is if you look at the cerium 137 measured on-line, you would have roughly the same temperatures as here, and in the MOX fuel.

DR. KRESS: Is that one at the top?

MR. CLEMENT: What?

DR. KRESS: Is that number one at the top?

MR. CLEMENT: Number One at the top?

DR. KRESS: I mean the release fraction values.

MR. CLEMENT: For the release fraction value, I am not so sure that I can get to that value (inaudible). You can imagine.

DR. KRESS: So, RT1 was for regular UO2 fuel?

MR. CLEMENT: Yes, and RT2 for MOX fuel. The burn ups are not very different. This RT4 was released from (inaudible) and quite similar (inaudible). This is for comparisons between RT2 and RT1, and for a test with a few indications of temperature that it is low.

MR. LEAVER: Hold on one second. RT1 and RT2 is a valid comparison of MOX and UO2, right; RT2 being MOX and the RT1 being UO2, and those are both oxidizing.

MR. CLEMENT: Yes.

MR. LEAVER: And so I guess your conclusion is that for the MOX that it is occurring faster?

MR. CLEMENT: Oh,yes.

MR. LEAVER: What is the time scale?

MR. CLEMENT: The time scale is an hour, 4:00 p.m. to 5:00 p.m., and 6:00 p.m. This is the plateau, for instance. The temperature plateau has been devoted to cladding oxidation. So rather low temperatures.

MR. LEAVER: If you can't tell us the numbers on the release scale, the Y axis, but can you tell us is it logarithmic?

MR. CLEMENT: No, it is linear.

MR. LEAVER: It's linear? Okay.

MR. CLEMENT: But if you look for instance at the span of time for (inaudible) for the MOX fuel is a better release fraction than for the UO2 fuel release fraction.

And that is at the end of the transient when the fuel is relocated, you have the same (inaudible) release fraction. The difference in the (inaudible) truly liquified and relocated (inaudible).

Okay. For RT2, there is not much so and not many conclusions. So at the beginning all the fuel is at a lower temperature, and volatility fission techniques is accelerated, and the difference is nt a general difference for the MOX fuel, and from the

results of one single test.

And with the discussions that we have had with europium and cerium, it seems that most of europian and cerium stayed in the fuel (inaudible) and values that are given on the French and European (inaudible).

For there is more information for RT7, and that is because it was radiated, and so this one is not permitted, and this one is (inaudible), but this is roughly 3,000 gallons, and (inaudible). And this is gas release from xenon to measure on-line and the gas capacity on the (inaudible).

So this is krypton 85, and this is at 1500 temperature, this plateau, and the purple line is from Krypton 85, and the green line is xenon, and the same values roughly for 133 and 137 (inaudible).

And so first that this has already been observed, and as soon as we start to have the release activities, and then the flowing temperature plateau to lower (inaudible), and the spike, and this area --

DR. KRESS: That is completely different behavior than you see with regular fuel.

MR. CLEMENT: And with such kind of measurements for high burn up fuel, and because of the transient and things are ready to go outside.

DR. POWERS: I think that (inaudible)

DR. KRESS: They tended to always -- oh, this is the release rate.

MR. CLEMENT: This is the release rate.

DR. KRESS: Oh, I'm sorry. I'm sorry. I was thinking about the curves and has the integrated release. Yeah, we definitely saw it.

MR. CLEMENT: That is the release rate.

DR. POWERS: You had explained it to me once, but I can't remember all our explanation. But it was totally reasonable.

MR. CLEMENT: Well, if you want, here you see for cryptium a larger piece than for (inaudible). And what was present initially in the GAP is not there, and has already been released. That's because these are open.

This small pike of cryptium is interesting, because it (inaudible) at the temperature plateau at 1200 (inaudible), and so you can see here that with the high burn up and that with long life fission products has more (inaudible) than the short life (inaudible).

And then to point out some differences, and this is an interesting one. Okay. In that case, molybdenum was not released in the air, and had a very

low release, and the comparable release as with ruthenium.

And what is interesting is that this is the lower part of the crucible, and this is the upper part, and this is the initial repartition of ruthenium, and molybdenum, and this is the repartition afterwards.

And it seems that would have some application, and that is important to know where are the fission products at the beginning, and how will they react and have different behavior. So, on RT7, zirconium and neodymium are retained.

There is a low, but significant, release of these elements -- niobium, rhodium --

DR. KRESS: When you say less than 15 percent, is it the fact that that didn't say less than 10 percent means that it is somewhere about or in between those two?

MR. CLEMENT: The release is the same for all. This was a comment, and I don't know whether it is right or not. But the same comment was made before that we have reached a very high temperature, and (inaudible), and compared to the RT2 test, and then the MOX test (inaudible). We think that molybdenum is found and associated with ruthenium (inaudible).

Barium is semi-volatile, and these are (inaudible). And total release of gas (inaudible).

DR. KRESS: And that was (inaudible)?

MR. CLEMENT: So that is the same as was for the last time, and (inaudible) earlier, and almost (inaudible). And this is for the contrary, and this is not well written. In fact, in RT7, the release is not earlier. But the release of UO2 is later, but it is not earlier.

MR. LEAVER: I didn't understand that.

MR. CLEMENT: In the RT2 test, the release of volatile MOX is earlier than not --

MR. LEAVER: Right, the MOX is earlier than the UO2.

MR. CLEMENT: When the fuel is relocated, and this is what I said before. When it is liquified, it goes down and has the same amount of (inaudible).

MR. LEAVER: Right.

MR. CLEMENT: In RT7 the release of volatile UO2, this is not earlier.

MR. LEAVER: It is not earlier?

MR. CLEMENT: Right. But the release is almost total in both cases, but this sentence is not very good.

MR. LEAVER: When you say it is not

earlier, you are saying -- first you said contrary, meaning -- well, it's not really contrary, but it just is not earlier.

MR. CLEMENT: It is not earlier.

MR. LEAVER: Okay.

MR. CLEMENT: The sentence is bad. So you can imagine that we still need to work on the (inaudible).

MR. BOYACK: Okay. The question was whether a copy could be made of those for use tomorrow. Okay. Is there any other comments or questions before we adjourn?

MR. LEAVER: Thank you very much.

(Simultaneous conversation inaudible).

MR. BOYACK: So I take it that that is it, and we convene again tomorrow at 8:15, and be escorted up. We will begin on the table that Bernard suggested and that I had pulled up here a moment ago, wherever I put it, which was basically to list the characteristics, and talk about the LEU behavior, the MOX behavior, and see if there is anything that characterizes the current state of the knowledge.

MR. LEAVER: We should probably have Charlie Tinkler and Jason in the morning first thing, and decide whether we want to -- I am concerned, I

guess, like Tom that if we fill out this table that we are going to maybe misrepresent what we know here, which is not much.

And we might be better off talking about what we know and what we don't, and maybe helping to come up with a plan, because the NRC is going to have to be able to come up with a source term for this in order to license the plant.

But I don't think necessarily this is the time to do it, when we really don't have any information.

MR. BOYACK: I do know what they intended to do. Now, whether or not -- well, I think they have to rely on the panel, and the panel basically has said that we are not in a position to do that.

MR. LEAVER: Well, maybe not at this time.

MR. BOYACK: And this information would be used in what we are talking about, and in any case, we will mention that tomorrow. I don't know whether Charlie was planning on being here. Jason obviously was. And so we can make that point to him early.

And then we could start on the table and see if Charlie can come to deal with this.

MR. LEAVER: Sure.

DR. KRESS: Are we going to get copies of

those slides to take home with us tonight so we can think about them and see how they --

MR. BOYACK: Which slides?

DR. KRESS: The ones that Clement just presented.

MR. BOYACK: If we can get copies now.

DR. KRESS: I would like to take them home and think about them, because that is all the data that I have seen, and all I have got to base my opinion on.

MR. BOYACK: Okay.

(Discussion off the record.)

(Whereupon, at 5:19 p.m., the meeting was concluded.)