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NUCLEAR REGULATORY COMMISSION

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SOURCE TERM APPLICABILITY PANEL

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WEDNESDAY

DECEMBER 12, 2001

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ROCKVILLE, MARYLAND

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The Panel met at the Nuclear Regulatory Commission, Two White Flint North, T2B1, 11S45 Rockville Pike, at 8:30 a.m., Brent Boyack facilitating.

PANEL MEMBERS PRESENT:

DANA POWERS, ACRS

THOMAS KRESS, ACRS

BRENT BOYACK, Facilitator

DAVID LEAVER, EPRI

JAMES GIESEKE, Consultant

BERNARD CLEMENT, CEA France

JEAN-MICHEL EVRARD, CEA France

<u>ALSO PRESENT</u>:

JASON SCHAPEROW

STEVE LaVIE

HOSSEIN NOURBAKHSH

* indicates unintelligible word(s) due to accent.

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

(8:34 a.m.)

MR. BOYACK: We have the panel here, so we might as well begin. A full day of activities today requires thinking, concentration, attention, skill, background, experience.

Let me just quickly review what we are going to do using the fantastic diagram here. We will be projecting up on the screen here momentarily, I guess, information. We have a number of tables to fill in, both by way of numerical values, if we are able to, and also by way of rationale.

So, as we go through, I proposed yesterday that for each numerical entry in the table that I go ahead and ask if, in the case of the source terms, if the given source terms are applicable. We also have durations that we have to handle. That is the one exception. If the answer is yes, then we will go ahead and record the value and also the reason why.

Anyway, if the answer is not yet, as we discussed yesterday, then we will go ahead and talk about whether we are able to specify a new value. If yes, we will record the rationale for that. If no, then we will talk about why, and we will get that down, and also at that point try to go ahead and get

some information down about what might be done in the way of research.

So probably what we are going to do now is take our first break.

(Laughter.)

You know, I tested this even before you came in (referring to electronic equipment).

(Whereupon, the foregoing matter went off the record at 8:37 a.m. and went back on the record at 8:39 a.m.)

MR. BOYACK: I am going to have to work on this a little bit, but I am going to ask Jason to help me.

For just a moment, what I would like to do is the following: Let's go ahead, the first thing we had to do is work on the duration of the phases: gap release, early in-vessel, ex-vessel, and late in-vessel for PWR releases in containment.

So, if Jason will just lead you through questions about the timing on those four areas, I will see if I can recover the capability here (referring to electronic equipment). I will just unplug everything and start from scratch, because that worked this morning when I came in.

MR. SCHAPEROW: All right, let's start

with the duration of the gap release. The duration of the gap release, as we talked about, is, at least as I suggested and NUREG 1465 says, it is the duration from basically the beginning of the accident up through the point of significant release of fission products from the pellet.

Right here it says we've got half-an-hour. I guess I would like to see if anybody would like to suggest that that value is sensible or if anybody disagrees with that value.

MR. POWERS: It seems to me that you have the choice of either collapsing that value down or increasing the release fractions of the halogens and the alkaline metals. That depends a lot on how you are going to use the gap release. When it was originally conceived, it was, as I said, a "no, never mind." Now it has assumed a larger significance.

It seems to me that I would tend to say, let's make sure the gap release is that release that will happen in a fuel-handling accident, because that is where it gets used.

MR. LEAVER: But isn't there a fundamental difference between the release that you get in a fuel-handling accident and the first half-an-hour or so of a severe accident? I mean those are two different

things, if they're going on. The fuel-handling accident, the fuel is cold and what comes out is what was already there; whereas, in the severe accidents, it's that maybe and some.

MR. POWERS: Don't argue with your bet.

MR. LEAVER: So I'm wondering, in the case of, if we are talking about a severe accident here, which certainly that is one thing we are talking about for sure, do we even need to distinguish between this first half-hour and the next hour-and-a-half? What's the point? I mean, I can tell you from a calculational standpoint it doesn't make much difference. The main thing is how much comes out and over what period of time.

Now certainly you need to define a gap for the other non-LOCA accidents, like the main steamline break and fuel-handling accident and those kinds of things, but I am not sure that this gap release, thinking in terms of severe accidents, to me that is a different thing.

MR. POWERS: But that's what I said. Let's not think about severe accidents because, when we did, we gave that short shrift. Now we find that it does get used, and it gets used in things like fuel-handling. So why don't we tailor the gap release

to reflect what happens in a fuel-handling accident rather than tailoring it for a severe accident, where it is a "no, never mind"?

MR. SCHAPEROW: I guess I would like to point out that the original expert elicitation, or remind people - you folks did it -- that there was a never a question on gap release. The first question was, what's the release from the core before lower head failure? At least, basically, it is released from the fuel into the --

MR. LEAVER: Are you talking about 1150 now?

MR. SCHAPEROW: Yes, that is what I am looking at here. The distributions are something called F-core, which is a release from the fuel into the RCS. I think that maybe the best way to proceed or the path of least resistance may be just to try to repeat this kind of a thing.

I kind of agree with Dave that we could tackle the gap one outside of this meeting.

MR. CLEMENT: I understand from yesterday's discussion from the presentation and then the questions that this question of gap release, you say, for what we call in France * this accident up to * was maybe not so clear for high burnup fuel.

I heard also that experiments are planned in the future to get the best values for high burnup fuel.

I think it is not so important for accidents, but if you have to put a value that is corresponding to this gap release more accurate than this one are probably not so bad. I mean, we know that experimental programs will be done. Maybe it is best to answer the questions by then. If people who are in charge of these problems, okay, that's not our severe accident system, need more accuracy, then we have to design more accurate experiments other than the typical ones. So I don't know.

But I understand, too, from yesterday that there are still incentives for high burnup, but I do agree that this is not so important at the end of the day on the final release for our severe accident system. I mean, it is actually now what you want to do with that.

MR. SCHAPEROW: Go ahead, Hossein.

MR. NOURBAKHSH: Just one comment: These values are core averaged. When we are talking about high burnoff, basically, all we need -- previously, high was conceived like 40; now we want to extend it to just like 50, roughly. If high burnup is 75, the

core average would be like 50.

If you look at, if you don't mind, I will show you these two, which are basically some calculations in 6703, based on FRAPCON. FRAPCON only gives you the fission factor. This is high burnoff. This is as a function of *. This is core average release, if you average it over the core.

So what we are using right now in 1465 is 5 percent, and the authors of this NUREG are actually saying these values are still based on ANSI standards. I mean one of them is 200 percent higher, 11.4. So these are relatively *.

So these are what we use, and they are not being used for fuel-handling. For fuel-handling, it would not be based on their peak rod. These are the type of values we are looking.

If you plot the same thing based on high burnup, this is what we have. This is now peak rod release based on the burnoff. So these values that fuel-handling regulations is struggling with, they want it to extend to 75, and they don't know most of the data on here.

So fuel-handling actually does not use 1465, let's say, those fractions. They are looking at high burnoff individual rods.

MR. KRESS: They are assuming peak rod because that is the one that is going to have the accidents.

MR. NOURBAKHSH: Peak rods, exactly, and we are assuming that the peak rod is going to fail. When you are averaging these, these values are only 1/10th of the core maximum has that high values. So you are not going to provide that much conservatism if we assume the same 5 percent that we have before on the core average.

The only thing is that we don't have data that will extend this. The question is whether these values could be extrapolated to 75. Right here, these values should be extended to -- basically, this is up to 40. Now we can move to 75 peak rod temperature.

We don't have data on more than 62 on this. These are calculations, by the way, of the FRAPCON, but they are usually higher than fission gas --

MR. KRESS: Is this the gap inventory that is shown there?

MR. NOURBAKHSH: Yes, what is in the gap.

MR. KRESS: That's in the gap?

MR. NOURBAKHSH: We are assuming it is released.

MR. KRESS: And you have that? That is a calculation using FRAPCON?

MR. NOURBAKHSH: FRAPCON, exactly, except that FRAPCON, from what I understand, reading that, will only have fission model.

MR. KRESS: Sure.

MR. NOURBAKHSH: So in order to get iodine and cesium, they did their ANSI. I mean it is ANS-540, and then they used that for scaling, applied that to scaling with this.

MR. KRESS: That's reasonable.

MR. NOURBAKHSH: Then the argument is whether the diffusion of iodine and cesium is much higher than 5.4, whether that should be -- if we assume that the fission is similar to normal gases, this would come down.

MR. KRESS: So the only thing you are correcting is the birth rate of the decay then?

MR. NOURBAKHSH: Exactly.

MR. KRESS: I would have been tempted to look at a distribution of burnups. You say one-third is a high burnout, and one-third is somewhere else?

MR. NOURBAKHSH: It is 20/40/75, if I remember correctly.

MR. KRESS: Okay. I would add up the

total inventory of all those.

MR. LEAVER: Do you mean like a burnup average, a mass average or a --

MR. KRESS: I would give it a mass average inventory.

MR. LEAVER: Yes.

MR. KRESS: That would give me my release facts.

MR. NOURBAKHSH: I think that is what they are.

MR. LEAVER: I think Hossein is saying he thinks it is roughly --

MR. KRESS: That is what I would use.

MR. NOURBAKHSH: What they did, the FRAPCON gives you the inventory, too.

MR. KRESS: Okay. So this is average over the whole core?

MR. NOURBAKHSH: Average over the core.

MR. KRESS: Okay, that is what I would use then for the fractional time; for the time duration, that is something else. I would have guessed that would have been from the time the first fuel fails to the time the last one fails.

MR. NOURBAKHSH: And that is something I mentioned about that last time we spoke of the

calculation, I believe 10 years ago. When I look at look at France, I mentioned 10 seconds, but I really meant 10 minutes. I went back and checked it. It is between 6 -- depends on the sequence. It is a variable. But the whole time, for the large break LOCA-type accident, you really don't have a temperature homogenization by natural circulation basically. So the old calculations still may be valid. I mean the new modeling is not going to impact that much between maximum and the minimal.

For an AG sequence, which is typical of a terminated large break LOCA accident, it was not 27 minutes.

MR. KRESS: So the half-an-hour is not that far off?

MR. NOURBAKHSH: Unless we are asking whether in that period of time the burnup is going to impact that.

MR. KRESS: Well, you know, it doesn't matter. The only thing it is going to add is the amount of heat, and I don't think it is going to add that much.

MR. NOURBAKHSH: It is not that much.

MR. KRESS: So it is the half-an-hour is probably still pretty valid. It looks like, in my

mind, right now, that Ralph Meyer was probably right; there is basically no change in the gap release.

MR. NOURBAKHSH: As for that application.

MR. POWERS: I absolutely do not accept that. I don't even accept this argument.

MR. KRESS: Okay.

MR. POWERS: Either you are going to say here is the effect of burnup and then allow people to use it as they will for the particular core they have or you have to specify one of these tables for every core that exists, because you're going to go on averaging, some magic averaging, over the burnups. It is going to change for every core.

MR. LEAVER: But if you put new fuel in every two years, and you leave the fuel in for, what, three, maybe four cycles -- I am not sure what the fuel management scheme is -- you know roughly, if the peak burnup is 75,000, that the average is certainly not 75,000. It's probably something more like, what, 50? I mean, maybe it's 55. Maybe it's 45. I don't know, but is it really so different from one core to another that we can't --

MR. POWERS: He says that he has to average. There's some averaging scheme. Why not let him do it, as opposed to Tom doing it, for the core

that he has?

MR. LEAVER: Yes, we were going to, I guess you guys were going to try to get some information on average burnups in the next day or so? We discussed that yesterday. I don't know if anything's --

MR. SCHAPEROW: Yes, I guess I would like to take a fallback position on what I had in my slides, which is the NRC will take a look at the gap release fraction for high burnup fuel within the next year, based on the Holden tests. I think this is a point of contention.

Maybe we can come back to it in a little while. I would like to move up to the in-vessel. I think that may be a bit more trackable.

MR. POWERS: Can you agree on a definition between the two?

MR. SCHAPEROW: I think the definition -- I would like to repeat, I would like to read one sentence out of 1465. Maybe the definition no longer applies.

"The gap activity phase ends" -- oh, this is on page 8. Let me pass this out. This may be used for reference today. I have plenty, if there's not enough copies.

If you'd like, on page 8, in the first full paragraph, the paragraph on page 8 that starts, "The gap activity release phase," if you go down a few sentences, it says, "the gap activity phase ends when the fuel pellet bulk temperature has been raised sufficiently that significant amounts of fission parts can no longer be maintained in the fuel."

MR. POWERS: What does that mean? That's useful.

(Laughter.)

MR. SCHAPEROW: That's the definition.

MR. LEAVER: Does that nail it down for you?

(Laughter.)

MR. LEAVER: The problem with this concept of gap activity is it is mixing apples and oranges. It is sort of implying that this definition of gap activity is the right thing to do for fuel-handling accidents, but it is also saying it is part of a severe accident, and they are really two different things.

I am not sure that the concept of a duration for this portion, early portion, of the release, as part of a severe accident really even is necessary.

MR. SCHAPEROW: Well, actually, it goes on further to say that, "A review of STT Source Term Code Package results indicates that significant fission product releases from the fuel will commence no earlier than 30 minutes for PWR's." That is a very significant volume of work that was done by Battelle. It is six volumes or so.

MR. BOYACK: I would like to offer at least the following comments:

The first of them is we are engaged here in extending the applicability of NUREG 1465, and 1465 shows a gap release. I don't understand the underpinnings of it, but at least it is there. It says that there is a gap release. The definition that was just read on page 8 seems to me, if not to be precise, at least it's workable for a panel like this, if they choose to say that significant is approximately "X" percent of the releasable inventory, and then you use your expertise to say approximately when that occurs.

So I offer that not out of a great understanding, but as a possibility that, one, we ought to hold NUREG 1465 approach. Now if we want to defer it, as the NRC suggests, Jason, that's fine with me, and you go on to the next one. But it does appear

to me that we ought not to spend too much time trying to redefine all of the 1465 things. The objective was not to go ahead and essentially go back pre-1465 and redo it.

MR. KRESS: A problem with that is this half-an-hour because you calculate how long it takes the fuel to get up to the temperature of the first burst, and then you calculate how long it takes the fuel to get up and light off the zirc oxidation.

MR. LEAVER: Right, the zirc oxidation.

MR. KRESS: That is the half-hour.

MR. LEAVER: Right.

MR. KRESS: And that is where we come from. That is no longer applicable now because, in the case of the low burnup fuel, the fission product released doesn't even start until you lit off the oxidation. Now it is starting much lower because of the burnup effect. You are now releasing fission products during that heatup time. You no longer have this luxury of just saying, we'll go to the point where the oxidation ramp sets off. We just don't have that anymore.

There is no way to define this. Where is the temperature where significant fission products can't be held anymore? You can't define it.

MR. LEAVER: What you're saying is another reason perhaps for not trying to distinguish between this early period and then the latter portion of the early in-vessel release, but just consider it starting earlier and then figure out what the magnitude is for the high burnup fuel and have it be some kind of uniform release over that period.

MR. SCHAPEROW: For the high burnup, no distinguishable path --

MR. LEAVER: I think that is what I heard Tom say, and that kind of makes some sense.

MR. BOYACK: Now I've got to know how to spell "distinguishable".

MR. KRESS: Pretty good.

MR. BOYACK: All I am doing here is just testing the process a little bit here. Some of this stuff we will wipe out and others we will keep, when you get to a point of a decision about how to proceed.

It seems to me the first question you have to answer is, are you going to go ahead with the gap release phase?

MR. LEAVER: I am not sure that is the first question.

MR. KRESS: I still like this duration being given by the time between the first fuel failure

to the last one.

MR. LEAVER: Do you mean the first clad failure to the last clad failure?

MR. POWERS: That could be days.

MR. LEAVER: Yes, that could be forever. I mean, a lot of clad may never fail. At least some of it may not. You may have releases from the center of the core.

MR. KRESS: Every piece of fuel in that core gets up to the clad failure temperature very quickly, according to NUREG 1465. Plant failure temperature is what, 1200?

MR. POWERS: Yes.

MR. KRESS: It all gets up there pretty fast.

MR. POWERS: I don't think so. I think the stuff that is out around the core barrel has a hard time getting to that temperature.

MR. KRESS: Might as well forget about it then.

MR. POWERS: It still would be a substantial period of time to get for the second one. I mean it is not days. It is an hour.

MR. KRESS: Well, I have never seen the calculation.

MR. LEAVER: Or you want to say 80 percent of the clad maybe or something like that.

MR. POWERS: If you say 80 percent, then you are down to a half-hour.

MR. LEAVER: If you want to stick with this half-hour, how much, what fraction of the total inventory is noble gases that gets released in this half-hour?

MR. KRESS: That is something we don't know, the calculation, the FRAPCON calculations for the average. I don't know; it is probably pretty close to it.

MR. LEAVER: Right now 1465 says 5 percent. So, presumably, there was some margin in that. So what you are saying is that, because of the high burnup, if you look at the average, which we still don't know but we think it is in the neighborhood of 50,000, given the fact that there was some margin in the 5 percent, we probably wouldn't change the 5 percent.

MR. KRESS: That would be my guess.

MR. POWERS: That's his vote.

MR. LEAVER: What?

MR. POWERS: That's his vote.

MR. LEAVER: That's his vote.

MR. POWERS: When I look at the Japanese experiments on reactivity insertion, where they have tried to measure the amount of fission gas that gets released, I see numbers up around 20 percent. I acknowledge that those experiments involve putting a pulse of heat in, but it is not clear to me that that pulse of heat is all that different than the more tracked amount of heat that we are going to put in.

So I come up with much higher noble gas numbers. In fact, I would argue the gap release fraction would be more like 15 percent.

MR. KRESS: I suspect the reactivity insertion accidents doing damage to the bulk of the fuel --

MR. POWERS: I wouldn't argue that a bit, but I don't see how I can ignore them when I do reactivity insertion accidents with low burnup fuel and I get 5 percent --

MR. LEAVER: You get what?

MR. POWERS: Five percent noble gas release. Then I go to a high burnup and I get 20 percent. Gee, maybe I'll just scale those suckers.

MR. KRESS: That is a little tougher to explain, except it is probably easier to damage the fuel at a higher burn level.

MR. POWERS: I don't know whether it is easier to damage fuel; it is that there is more damage there and you are adding to it.

MR. LaVIE: Steve LaVie.

The reactivity insertion accident, you have got to recognize how that is used in regulatory space. In that particular accident we assume the gap activity from those pins that have exceeded the clad burst temperature, which is 180 cals per gram is burn, it's assumed, will have released its gap contents.

We then look to see whether or not any of the pellet reached the heating melt point, and then that is added. I believe the Japanese data is looking at the total. So in our regulatory space we have already taken care of that total, because if we do get to melt temperature, it is added to the gap release.

MR. POWERS: That doesn't have anything to do with the question. The question is --

MR. KRESS: Well, it gives you an explanation for why the extra might be there.

MR. LaVIE: Why the Japanese have 20 and we are only looking at 5 here. This is the only gap.

MR. KRESS: That may be an explanation for why that extra --

MR. POWERS: It's not an explanation of

the issue.

MR. KRESS: No, I guess it's not, but a fuel explanation.

MR. BOYACK: Let me just explain that a few moments ago my comment to Jason was that the NRC's going to have to decide what they want on this. The panel can discuss it for a period of time, but it has been my sort of position that there are certain parts of the problem that the NRC owns by way of specifying what they are after.

So Jason listened a little bit more to the argument and departed. It is my speculation --

MR. KRESS: Did he have a disgusted look on his face?

MR. BOYACK: -- no -- that he has gone upstairs to try to pull down Tinkler or somebody else to get a little bit of guidance on this, because there are some areas where I am just not sure whether we are going to be able to work through it by discussion, because in part it is the definition of the end use that is going on.

You notice the silence here. I had claimed you had maybe gone up to check with somebody, but --

MR. SCHAPEROW: No, I went to get these,

though.

MR. LEAVER: I guess I am interested, Dana, in what you said about reactivity insertion accidents. I don't know what data it is that shows 4 or 5 percent for low burnup and 20 percent for high burnup, but that is interesting. I mean, do you know more about that? If you would like to share it with us?

MR. POWERS: The problem is, when I bring it up, people say, oh, well, that's reactivity accidents and go off into never-never-land about reactivity accidents and that they are different from gap release.

MR. LEAVER: Well, they are different.

MR. POWERS: In that case my response is measure the thing.

Well, you can't measure it. There is no way to measure gap release. So you have got to derive either from code calculations or inferentially.

The problem is, I think, that the existing codes are just terribly deficient in how they treat gaps. I mean the ones that are routinely used. So any calculation that comes out, I just blow off.

MR. KRESS: And I think you are right that this is a kind of measure of what is available to get

out with the gap, even though it is a different kind of accident.

MR. POWERS: So, I say, okay, well, maybe I don't take the whole thing, because I don't think the NRC wants this source term to smack of being bounding for every conceivable situation. They want some kind of a mish-mash in there.

I mean, it is reasonably valued -- you know, it is a word I just hate, "reasonably," because nobody ever tells me what their reason is, but I think, like everybody else, I have kind of sense for it. It shows the noble gas has to go up.

They asked me about the more condensable species, and I said, look, the condensed species are released from the gap because they are vapors. So what you really need to know is what the temperatures are and the vapor volumes. Those didn't change. That vapor volume is dominated by the fill gas. So I will come back and say, well, the gap release fractions for cesium and iodine are the same as before.

MR. BOYACK: Let me ask a couple of questions. I am going to primarily ask the NRC.

I guess Jason actually went upstairs to garner a few more pieces of documentation. Do you want to tell us what that is about first?

MR. SCHAPEROW: Yes, I would. I went up and pulled the work done by Battelle with the Source Term Code Package. It is a series of six volumes. Jim's name is No. 2 on each volume, I think, here.

MR. KRESS: You've got all six volumes?

MR. SCHAPEROW: Oh, yes. I haven't read every one of them, but I have skimmed them. The idea of a start time for the events of release or an end time for the gap release of this certain -- you get it to this certain temperature and you start releasing significant amounts of fission products.

The sequences in here, I am just looking at the PWR. The first two sequences I come across are seal LOCAs, reactor coolant pump seal LOCAs. That implies it takes a while to empty out the RCS to start heating the fuel up.

I think the assumption that some of us are going under is that maybe a 2-inch break or maybe 5-inch break, a bigger break where the thing empties out pretty quickly, and then you start getting heat up and melting, heat up and degradation.

So I am almost wondering if we should be tackling the timing issue because, again, as I look at the expert elicitation we did back in the late eighties, I don't see the timing issue in here either

really. That is another thing that was done kind of later when we were putting together NUREG 1465. I don't know if you folks want to tackle that.

MR. LEAVER: The timing issue?

MR. SCHAPEROW: Yes. What was elicited here was the release fractions, the release fractions and the deposition amounts in the RCS.

MR. LEAVER: I think we've got to say something about the duration of the release. It is fundamental --

MR. SCHAPEROW: All right.

MR. LEAVER: -- to the design basis for the systems that respond to the accident.

MR. SCHAPEROW: I think what we need to do is we need to list what sequences we are talking about, what is being considered. Because I think if you go to the large-break LOCA, you will get a very fast timing and the gap release will be indistinguishable from the --

MR. LEAVER: Well, we are talking about low-pressure sequences --

MR. SCHAPEROW: We are talking about other sequences --

MR. LEAVER: -- but we are also bearing in mind other sequences that may take longer. So we

don't miss something that is important. We talked about that yesterday. I don't think that has changed.

MR. SCHAPEROW: I am just wondering if it will be useful to list the sequences that we are considering.

MR. BOYACK: We can certainly do that easy enough just by an overhead transparency.

MR. SCHAPEROW: Right.

MR. KRESS: NUREG 1150 doesn't go to the sequence.

MR. SCHAPEROW: Pardon?

MR. KRESS: NUREG 1150 doesn't go to the sequence.

MR. SCHAPEROW: No, it says high oxidation and low oxidation.

MR. KRESS: It is just high pressure, low pressure, high oxidation, low oxidation.

MR. SCHAPEROW: That is true, but what David is suggesting is that we should be tackling the timing issue.

MR. LEAVER: Here's a list of sequences.

MR. SCHAPEROW: Okay, that what was considered for the in-vessel duration.

MR. LEAVER: Right.

MR. SCHAPEROW: All right. All right.

MR. LEAVER: It is not a bad list.

MR. SCHAPEROW: All right, why don't we go ahead and put this up. I think we will need, at least for me I will need to spell it out, what these things are, because it is written in ERA lingo.

MR. LEAVER: I think I know what most of them are.

MR. SCHAPEROW: Oh, you do?

MR. LEAVER: Roughly. I mean, you can tell small breaks, large breaks, the VC points.

MR. SCHAPEROW: They are in these two volumes.

MR. BOYACK: Why don't you just refer to page 8 again of the document? I think that is where you are.

MR. SCHAPEROW: Yes, page 8. Page 8 has got a list of the sequences. They are in these two volumes.

MR. NOURBAKHSH: Some of them, we had the actual audit books to check these numbers.

MR. SCHAPEROW: Do you have a blank slide and a pen? I will go ahead and write them up.

Here they are. In the front of the chapter here is all the sequences. Shall I do them all or just the low-pressure ones?

MR. LEAVER: It wouldn't take long to do them all.

MR. SCHAPEROW: All right. Shall we take a five-minute break while I do this?

MR. LEAVER: Actually, people have this list, don't they? I'm not sure you have to write it, do you?

MR. SCHAPEROW: Well, what they are.

MR. GIESEKE: Is this list going to be different than the 1465 list?

MR. BOYACK: No, it's the same list. I think what he is saying is, what are those accident sequences? The VSIC quench was an interfacing systems LOCA. That is what he was after.

MR. SCHAPEROW: Maybe you can help me with this, Hossein.

MR. BOYACK: Hossein probably knows what they are off the top of his head.

MR. SCHAPEROW: Yes, I don't even see a correspondence between the VMI report and the page 8 table here.

Why don't you help me put this list together here?

MR. NOURBAKHSH: What do you want it to do here?

MR. SCHAPEROW: I want to do each of the -- these are the ones you considered?

MR. NOURBAKHSH: They are same as that.

MR. SCHAPEROW: The same as that, yes. All right, let's do the first one. CML, I guess we all know what that is. That is the --

MR. LEAVER: Brent, have you got one of your earlier PERC reports here, the ones that look -- somebody sent me -- I didn't bring them.

MR. BOYACK: Oh, I sent you the --

MR. LEAVER: Two of them. They were for --

MR. BOYACK: I sent you PDF files that described --

MR. LEAVER: No, I had hard copies.

MR. BOYACK: Oh, you are after the full program. No, I didn't bring those with me.

MR. LEAVER: Nobody has them here?

MR. BOYACK: Well, the NRC has them because I sent them to Jason.

MR. BOYACK: These are for high burnup fuels, is that correct? These activity insertion accidents?

MR. LEAVER: I think it was activity insertion accidents.

MR. BOYACK: And PWR.

MR. LEAVER: Yes, and PWR.

MR. SCHAPEROW: All right, Richard pointed out that on page 6 of the 1465, these are the PWR ones. It looks like the biggest LOCA is a 3-inch LOCA for O'Conee. For what it's worth, I think it is the time that that takes to drain the vessel and heat up the fuel that's going to make a difference here.

MR. KRESS: So somebody took all these numbers and ended up with 1.3 hours. What did they do, add them up and average them?

MR. SCHAPEROW: Actually, for the in-vessel, there's a listing of the time. It's a lower head failure on page 8.

MR. BOYACK: Table 3.4?

MR. SCHAPEROW: Yes, Table 3.4.

MR. LEAVER: I see the sentence down at the -- I don't know if these 1465's are all consistent in the way their format is, but in the lower, the very bottom of the left column, it says, "representative times for the duration of the in-vessel release phases have been selected to be 1.3 hours or 1.5 hours per PWR and BWR plants, respectively, as recommended by Reference 17."

I don't know what -- oh, Reference 17 is

probably 5437.

MR. SCHAPEROW: That's Hossein's work.

MR. LEAVER: 5437? Yes. So they are called representative. I don't know if -- that probably doesn't --

MR. KRESS: Well, he probably took those sequences and weighted them by frequency and got the frequency weighted average.

MR. SCHAPEROW: It looks like some sort of an average because 90 minutes is right in the middle of all the stuff that's there.

MR. LEAVER: I mean, I can tell you from the standpoint of the impact on the dose, if this duration were shorter, conceivably you could have a system, mitigation system like a spray system, which is a short-duration system, and if this release, instead of being 1.5 hours was 45 minutes and you had a 45-minute spray system, that would work pretty well.

So I think there's something to be said for not making the release too short, even if we think that there are some sequences where it would be short, because it gives the possibility that the plant could come up with a system, or the designer could come up with a system for mitigating this accident that would work fine for a shorter release, but not so fine for

a longer release.

MR. KRESS: Well, my feeling is that for this high burnup fuel, you don't release them at all, the nobles and volatiles during the adiabatic heatup, which would release none of the release tanks during that period, at least very little, but that's with high burnup fuel.

MR. LEAVER: Which is some fractional portion of the core.

MR. KRESS: Which is only a portion of the core.

MR. LEAVER: Right.

MR. KRESS: So you want to start releasing from the high burnup fuels during the adiabatic heatup. That's the start. This is going to come up on some sort of a ramp, and I don't know where you're going to say the start is because there is no definitive demarcation for it.

Then, as you continue in the adiabatic heatup, you are going to start releasing the lower burnup fuel. Then when you get in and drive off the oxidation kinetics, you are going to start releasing from the regular burnup fuel that is in there.

So what you have done by including high burnup is you have stretched out the timing over a

longer period actually. You will end up releasing basically 100 percent, I think, at the end point.

MR. KRESS: A hundred percent what?

MR. SCHAPEROW: From the high burnup fuel.

MR. KRESS: From the high burnup. I don't know how much, what fraction you release from the lower burnup, but that is a calculation I haven't made yet. Normally, it releases about the same fraction to the low burnup fuel that you did previously. So you have to weigh that fraction in the 100 percent by how much of this is in the core. That is why I needed this distribution.

MR. LEAVER: Yes, that's right.

MR. KRESS: So what we are going to do is, because some fraction of the fuel is releasing more than it did before, we are going to up the source term; it is going to start earlier, and it is going to stretch it out in time, is the view I have of what is going to happen.

MR. LEAVER: It is going to stretch it out in time, because it is going to start, the high release, maybe start a little sooner?

MR. KRESS: Yes.

MR. LEAVER: That's what you mean, stretching it backwards?

MR. KRESS: Yes, backwards. Start it earlier.

MR. LEAVER: Right.

MR. KRESS: And you will end up releasing from the lower burnup fuel about the same time you move forward because it is driven by the zirc oxidation phase. That is the way I would view this thing.

I'm going through answering the questions in my mind --

MR. LEAVER: Yes, I think you've kind of given a qualitative explanation.

MR. KRESS: Qualitative. Now how to put numbers on it --

MR. LEAVER: Right, right, but that's what we have to do.

MR. KRESS: The first number, I don't have it yet, is where it starts, because the start is significantly released from the high burnup fuels when it starts. This doesn't come up and suddenly start. It is slowly rising up, because of the kind of heat that is coming out. I don't know where you make the demarcation on how you decide to do that, but that is the first thing I think we have decide: How do you do that?

One way to do it is arbitrarily like we do in structural mechanics, is you plot the release of high-burning fuel versus time, and it does come up on an angle, but it kind of gets a little straight edge. You draw that straight point down to where it meets the timeline and just say that's the start, and forget about this other one. That is the way they do in structural mechanics.

MR. LEAVER: When you actually calculate doses, controlled doses, offset doses, that effect that you just described would be pretty small.

MR. KRESS: Pretty small.

MR. LEAVER: So you could assume a uniform release, if you wanted to, and it wouldn't really --

MR. KRESS: Normally, the source term does take the total time and reduce it on a straight-line basis --

MR. LEAVER: Right.

MR. KRESS: -- and that would be a way of doing that.

MR. LEAVER: Yes. But what burnup would you say this -- maybe it is a gradual sort of thing and there's no good answer to this, but what burnup would you say you would see this accelerated effect that you described?

MR. KRESS: Oh, somewhere around 60 or 70.

MR. LEAVER: But if we are trying to figure out what -- if we knew the distribution burnup over the core, then at what point would you start to say, well, above burnup of "X", I would see this effect? Say 60?

MR. KRESS: Once again, that is a continuous function.

MR. LEAVER: Yes, it is a continuous function, but I'm not sure we are smart enough to --

MR. KRESS: To factor that into it.

MR. LEAVER: Yes, yes.

MR. GIESEKE: A while ago, Tom, you were talking about the onset of release. Did you attribute that to the failures of rods at the higher burnup levels?

MR. KRESS: That was for the gap release.

MR. GIESEKE: The gap release for the old case.

MR. KRESS: Yes, I just assumed the gap release failed with rods, and that is generally said to occur at a roughly given temperature.

MR. BOYACK: You are back far enough that we are missing the pickup (speaking of the microphone).

MR. KRESS: The gap release I think is assumed to happen when you fail the clad, and that's a stress calculation.

MR. GIESEKE: So what we are saying, then, is regardless of the burnup level, that the initiation of release is the same time?

MR. KRESS: Just for the gap.

MR. GIESEKE: Yes, just for the gap, but it's --

MR. KRESS: But then you've got real problems with the in-vessel release.

MR. GIESEKE: Okay.

MR. KRESS: Because it used to have a line. It didn't start releasing until you lit off the zircaloy oxidation.

MR. GIESEKE: Yes.

MR. KRESS: And you no longer have that line, is my problem.

MR. GIESEKE: That's right, you no longer have that line. But if you are talking just -- that is, presumably, the end of the gap release anyway, right, when that lights off?

MR. KRESS: That is the way they define the end of the gap release, I think.

MR. GIESEKE: Yes. Okay.

Now if we follow the same kind of logic and we just look at the failures, they are going to occur probably over the same time period, but because of the burnup level, they are going to be different amounts, is that true?

MR. KRESS: For the gap. That was what my earlier speculation was. Dana didn't particularly like it, but --

MR. GIESEKE: So you questioned the timing? What is the reason for the timing difference then?

MR. KRESS: I have already left the gap release and said I'm going to leave it for Jason, and I am focusing on the in-vessel. I forgot about the timing of the gap altogether, and I am just focusing on the in-vessel release: What does it start and what is its duration?

MR. LEAVER: If we could somehow generically take into account this portion of the core that has a burnup that is higher than what we have traditionally been thinking about, what went into 1465, then we could perhaps come up with some increase in the magnitude of release. It is now 40 percent. So, say, now we think it is something a bit higher than that. Actually, that 40 percent is released into

the plate.

In this document I know there may be some disagreement on the details on this. I'm sure there will be, but I am reasonably certain that the thought that went into this was that, for a 40 percent release to containment, that that means --

MR. KRESS: Ninety percent release.

MR. LEAVER: Yes, or 80. I don't know, 75, 80, 90 percent, something like that, from the fuel, and then some retention in the RCS. It was kind of a mish-mash of sequences in terms of whatever that term is called for retention in the RCS.

So, anyway, if we knew what fraction of the core was sort of pushing the limit on where we thought this increased burnup effect would occur, we could estimate increase on this 40 percent, say, to 44 percent, or whatever the heck it is. Then, if we were able to do that, I think we could assume a uniform release from 30 seconds, or whatever the onset is, over the duration of two hours or 1.8 hours, or whatever the total is.

I think that would define what was needed to do the calculations of the release transport and the dose calculations, and whether you had some different release rate in the first 30 minutes than

you did in the next hour and a half would make no difference at all to the calculation. I don't think we have to worry about that, even though they do distinguish that in 1465 now.

What I am hearing you say, which makes sense, is that you think that with high burnup fuel you are less able to distinguish those two periods because there is a higher release hurdle earlier.

It seems to me I am sort of with you, Tom; the more fundamental question is: I'm even past the duration. What's the effect on the total release obtained from high burnoff fuel? What do we want to say that is?

MR. KRESS: The question there always comes down to how much you want to assume gets plated out.

MR. LEAVER: Right. Right. Well, also the distribution of burnup in the core.

MR. KRESS: And whether the burnup affects that plate. It possibly could because it may change the total aerosol concentration that is released, and thus, enhancing its plate-out. So it may be that higher burnup fuel does plate it out more, but, by the same token, you are starting a release over a longer period of time, so the concentration may not go up

that much because you are spreading it out over a longer period of time. So I don't really know what happens in the plate-out. I would almost be tempted to use what was done in the previous one for the plate-out. I don't think we have places for selecting --

MR. LEAVER: Yes, I am not sure we know exactly what was assumed because it was kind of a mish-mash, but I seem to have, as I said, in my head there is roughly this factor of two. I don't remember whether that was discussed at meetings or whether that is actually in the -- I don't think it is in 1465.

MR. KRESS: No.

MR. GIESEKE: I think we are trying to confuse ourselves here a little bit because we are trying to define the regions like the gap release and the in-vessel release in terms of the amount that is released, and really --

MR. LEAVER: The fraction of the core --

MR. GIESEKE: Yes. And shouldn't that be defined independently by thermal hydraulics somehow rather than in terms of the amounts released? I mean, Tom is concerned because the gap release is higher now, which clouds the boundary between the gap and the in-vessel release, and we have talked about the

amounts of release and mitigation or deposition in the primary system affecting the amounts. I think, can't we define all these durations strictly in terms of thermal hydraulics or significant events like fraction of the core, rods failed, something like that?

MR. KRESS: You can do it, except thermal hydraulics doesn't help you on deciding when the in-vessel release starts and it doesn't help you in telling you how much deposition you are going to get from the primary system, because both of those things are more than just thermal hydraulics.

MR. GIESEKE: Yes, but if you can't define the thermal hydraulics for the basis, you could never define the rest of it.

MR. BOYACK: Let me ask a couple of questions. I've now listened through one full meeting and a second, and the one thing that is becoming evermore clear to me is that the source term is not precisely logic, single-logic-driven. So there is one of the key factors right off, was that there wasn't a single transient which you could support by a single set of calculations, tie it to a set, predefined or a defined set of non-logical behaviors, sort out the predicted release, no matter how good or how bad the methods were, and say, that's it.

So, as we talk today, what I sense is this sort of struggling because now nobody is very, very comfortable with that. They may not have been comfortable with it some time ago. But that is the reality.

I ask myself the question on something like gap release, do we need to really worry so much about tagging it to a bunch of non-logical events or can you conceivably take the approach that says, okay, what we'll do is we will take half-an-hour, we'll leave it there, and we just won't change it, and during that time we will recognize that there is going to be, since it is not changed, not tagged now to the onset of the zircaloy oxidation, but we do have some insight phenomenologically now that there is going to be larger releases quicker. Just leave the time the way it was, and then embed that new physical insight or these physical insights about high burnup into your source terms. So there is one possible approach.

Now the second possible approach, which I am not sure would be any better, and that is that, instead of starting at the front end, we go to the back end. I've heard some discussion about this. Where there's some discussion and you say, well, we kind of know the transient of high burnup fuel and we

think there is going to be higher release fractions by the time we get to the end, and then we can try to march forward.

I don't think we --

MR. LEAVER: You mean march backwards?

MR. BOYACK: March backwards, I'm sorry.

MR. LEAVER: Right.

MR. BOYACK: We could march backwards through the problem, but with the idea that we can build upon successes at the end and come forward.

Now Jason's got both a curious look and --

MR. SCHAPEROW: I think I understand the second one. That's when we go right at the iodine in-vessel release magnitude.

MR. LEAVER: Right, exactly.

MR. SCHAPEROW: I think that would be most fruitful.

MR. LEAVER: Me, too.

MR. SCHAPEROW: I think that is where we have maybe our most experience and are most comfortable in that area.

MR. LEAVER: And I think it is the most significant number that we are going to decide.

MR. SCHAPEROW: Yes, and the in-vessel release magnitude --

MR. BOYACK: Well, you are talking about in-vessel.

MR. SCHAPEROW: -- and then work back from there.

MR. BOYACK: Okay. So I was actually going to the far end --

MR. SCHAPEROW: No.

MR. BOYACK: -- but you're saying, no, that's no problem. To me, it does not matter. If there is one place, any place, that we could start and work backward, that would be fine.

But my other thought, listening to all this, was that it would be possible just to set the gap release time the same as it was, and then just embed the phenomenological knowledge about high burnup and drop it into the source term. I'm not worried about the timing because it really is going to all -- whether it is a half-an-hour gap release and 1.3 hours on early in-vessel, containment-wise it doesn't seem to matter.

MR. SCHAPEROW: Yes, I tend to agree with Dave Leaver that, given our need, given our application of this for the design basis LOCA analysis, that we don't really need to distinguish between the gap and the early in-vessel. We should

just lump it together for now.

MR. GIESEKE: I don't have any trouble with Brent's suggestion of holding it to the half-an-hour if we can make a statement or an understanding that burnup does not affect the things that affect that decision, the previous decision. What determined it before was the takeoff of the zircaloy reaction basically, and the onset of that was fuel rod failures. So that defines it in terms other than the amount released.

What was stated in the report was that the two regions are defined by the amount released, which I think is a mistake to define it in terms of the amount released, rather than tying it to phenomenological effects in the core, which in fact lead to the changes in release. Does that make any sense at all?

I can see if we can that the progression of the accident is the same, independent of burnup, or what happens in the core when the rods fail, when this zircaloy reaction takes off, which are the controlling factors -- if those are independent of burnup, then we should be able to retain our definitions, or at least some understanding of the two kinds of releases. The amounts are going to change maybe several times, you

know, up to four times, what Dana was saying, from 5 to 20 percent, you know, but that's laid over the same kind of a timescale, which is defined by other events and not by the release. Does that make any sense?

MR. KRESS: No.

MR. GIESEKE: Oh, okay.

MR. KRESS: If you think about what is going on, there is some time for blowdown and there is time for blowoff and residual water until you start uncovering the core. If you start uncovering the core, the tops of it start heating up, and so the core -- this is behind down here -- the core will start heating up at some rate, which is mostly adiabatic heatup but a little cooler. It is about half the adiabatic. This is something like 1 K per second.

You come up here until it gets up to a temperature, so that is generally thought where the zircaloy fires off. This ramp goes up very fast. This is something like 40 K per second. It really changes quite markedly.

Now if you superimpose on that low burnup fuel, like 30,000 or 40,000, what you get is you get somewhere right about here you get a little fission product release action in here. You get a little zap. That's the gap. Then you don't get anything until you

get way out here. Then all at once it starts coming up just a little bit. It comes up and this thing sort of hits a temperature and just lots of little things here. It just comes up here, and once this thing happens, it kind of cools. So that is your general fission product released for low burnup fuel.

Now what happens when you take -- which there is some still in the coal, even in high burnup. Now let's take a real high burnup. You come up. You probably hit this thing about at the same time because it doesn't hurt this heatup ramp much because it's not that much decay, heat change. So you're going to hit this at about the same. There's probably going to be a little more because burnup gives you a little more in there, and it may come just a little earlier because the stress is higher, but it is not going to change this much. It is going to do this, and I'm going to superimpose that curve.

Somewhere around about here, and I am not sure exactly where in this adiabatic heatup, this thing is moved down to right here, and it is starting to do this.

MR. GIESEKE: Move down to where the --

MR. KRESS: Pardon? This is overlapping this thing. I am not sure what this temperature is

because that takes a real fine calculation. But this will go on up higher because it is superimposed on this ramp, and these two got time for this high burnup fuel. So it will go up. This may be 50 to 70 percent of the volatiles. This is like 100 percent of the volatiles here, because it had more time --

MR. GIESEKE: Why does the release take off before the --

MR. KRESS: Burnup has an effect on when the release, how easy it is to release. This is burnup like 70,000.

MR. LEAVER: Structural effect --

MR. KRESS: Structural effect on release.

MR. GIESEKE: Okay, so you're saying that --

MR. KRESS: So this curve still happened, or some fraction of the fuel, this curve will happen with a high burnup fuel. Now how you look at that, you can get a total release. If you know this curve for each fraction of the fuel, you can get a total release. It is weighted by the amount in it.

Where do you decide it starts? Well, I would say you draw this line here down and say it starts right here, if you know this curve. That would be my idea of a start, and that is the end of the gap.

Now this ramp right here is no quiet ride because what you want to do is -- this is not the total fraction; this is the fraction of the fraction. So you really get the total by some weighted average of this, and the total may be right down here, and it ends up, that total ends up arriving better. So you start it here, and you draw this line instead. So you've got the total; you've got a ramp rate; you've got a start time, and you've got a gap and a start. That is the way I would do it if I had all this information.

MR. GIESEKE: Well, you are doing what I was asking them to do, which is defining the release regions in terms of phenomenological effects rather than defining them in terms of released amounts.

MR. KRESS: Right.

MR. GIESEKE: The way we have done it before, we have defined the regions in terms of release amounts rather than in phenomenological --

MR. KRESS: I feel strongly that that's what is going on. Now the question is: How do we quantify it? Because previously the source term people had in front of them umpteen dozen calculations. It told them all this timing, and the guy went through it and said, okay, you made good

judgments, and said, I'll take this and this and this. He had a lot of information to do it.

We do not have the equivalent of the information for the high burnup fuel. We just don't have it. We have a real problem in doing something other than what I said earlier: It starts earlier. It goes on a different route that is stretched out in time. We can say those things, but to put numbers on them is going to be difficult because somebody has to do some calculations.

MR. SCHAPEROW: Would you mind if I just marked, made one mark on this?

MR. KRESS: Well, use a different color of pen.

MR. SCHAPEROW: Oh, okay. Good idea.

(Laughter.)

The point that I was trying to make earlier was that the scenarios that we are considering is a seal leak or even a two-way break --

MR. KRESS: Yes, it starts way back. Actually, it starts way back there.

MR. SCHAPEROW: Well, nothing will happen for a long time.

MR. KRESS: That is exactly right.

MR. SCHAPEROW: You won't have any release

for a long time, and the temperature is also going to be low

MR. KRESS: Yes, you're exactly right.

MR. SCHAPEROW: So I think a lot of this gap release timing is due to this assumption that the start of the gap release is for a large break LOCA, and the start of the early in-vessel is for a 2-inch LOCA.

MR. KRESS: Yes.

MR. SCHAPEROW: I think that half-an-hour is the difference between a large break LOCA, gap release start time, and a 2-inch LOCA start time, gap start time. I think we are getting thermal hydraulic benefits where we are getting that half-hour.

MR. KRESS: You could take the gap release off and everything else still holds that I ad up there.

MR. SCHAPEROW: Yes.

MR. KRESS: And you could deal with the gap --

MR. BOYACK: If you sort of were considering, covered by this representative scenario, five, six, or seven of these that are listed on page 8, now this is the timing for early in-vessel, but, in effect, let's see, at 1.3, 60, 70 minutes, there were

some that were larger and some that were smaller as far as timing, right?

MR. SCHAPEROW: I guess I'm hearing that the -- oh, I'm sorry.

MR. BOYACK: I just don't think it matters too much -- you are trying to be representative -- as long as it will be appropriately used in downstream applications. So the gap release also appears to me to be in the same park; that is, if you use representative sequences, there's a bunch of them. Some of them are very short, like a large break LOCA; some of them extend out a little while, like your much smaller break LOCAs. One could continue on and use roughly the same timing, because it is just representative.

MR. SCHAPEROW: I guess I'm hearing, I think I am hearing from Tom and Dave that the sum of the gap in early in-vessel release phases should still be about 1.8 hours, and also from Jim. I think I am hearing that.

MR. BOYACK: 1.8 or 1.3.

MR. SCHAPEROW: The sum, the sum of the gap in the early in-vessel release phases should still be about 1.8 hours.

MR. BOYACK: But isn't that early in-

vessel, that's actually the duration of the phase then? You add them up to come to the --

MR. SCHAPEROW: That's correct.

MR. LEAVER: When you do the calculation, you add them up and you get the total.

MR. BOYACK: So the actual elapsed time for the end of early in-vessel as shown on the 1465 table is 1.8 hours?

MR. SCHAPEROW: That is correct.

MR. POWERS: That assumes that burnup does not effect degradation?

MR. SCHAPEROW: That's correct.

MR. POWERS: And you have no evidence that that is true?

MR. BOYACK: Now a while ago, Dana, you were filling out tables. I wonder if you literally have them all filled out for us now. So would you like to comment? Not yet?

MR. KRESS: The question is, my figures and values are driven by that double, triple line heatup curve, and Dana has said that's what you get for low-burning fuel, and the question is whether burnup will affect that double-wide curve, which it very well could.

MR. GIESEKE: Affect it in what sense?

Make it move, shift left or right, or --

MR. KRESS: I don't know. If you had a lot of foaming, for example, it would hold fuel up --

MR. LEAVER: And it could either go left or right or up or down. What would the effect be? You've shifted it to the left due to burning.

MR. KRESS: No. No, all I'm talking about now --

MR. GIESEKE: Yes, he's shifted way to the left. He's shifted --

MR. KRESS: That curve is not really affected by burnup. I am just using the old curve. That's what you get from low-burning fuel.

MR. LEAVER: Right.

MR. KRESS: I don't know what happens with high-burnup fuel.

MR. LEAVER: But I thought your double-line curve was intended to be sort of a representation of what happens for higher burnup?

MR. KRESS: No, that's what you get for low burnup. I don't know what you get for high burnup because I have never seen the tests or even the calculation. Dana surmises there might be changes due to things like foaming and the changes in thermal conductivity, and changes the candling behavior and

the fuel -- you know, it doesn't change the decay heat enough or it doesn't, in my mind, change the zircaloy steam reaction enough to really affect those first two lines very much, but --

MR. CLEMENT: We have seen that in very few experiments on these small pieces of fuel that it is able to relocate earlier at lower temperatures.

MR. KRESS: Yes, that's the kind of thing; this thing may cut off earlier.

MR. CLEMENT: But this is another picture for the whole core that is much more complicated. Because if you have a foaming, it might affect one region.

MR. KRESS: Right.

MR. CLEMENT: You could have other fissions in other regions.

MR. KRESS: And that is what Dana is speculating, that kind of stuff. They have seen it relocate at earlier temperatures. So you may cut that thing off sooner, but it may still be in there releasing fuel, fission products, anyway. So I don't know what happens to that curve, that fuel.

MR. CLEMENT: It might affect also the overall behavior of the degradation in this whole thing because, as Dana said yesterday, once it becomes

comes a molten pool, whether it comes from the fuel or another process --

MR. KRESS: Then it doesn't matter.

MR. CLEMENT: -- it doesn't matter, but what you don't know is what is the amount of fuel you will get in your molten pool. It is different also than the experiment or the accident. What is the amount? I think that is a general problem. I assume it is not so much things, but you can imagine that fuel burnup will affect this degradation, the degradation of the fuel in the in-vessel release. Giving numbers, it would be 1.3, 1.4, 1 --

MR. KRESS: That's going to be a real problem.

MR. CLEMENT: I don't think you can do that.

MR. KRESS: Yes, that is going to be a real problem.

MR. LEAVER: Yes, that is very difficult. That is for sure. But we do know that it's not -- if we make that duration shorter, then we have the possibility that we're being non-conservative, which really bothers me. I mean, I can tell you that that's not a good idea.

Now if you make it longer, well, that

possibly might also be non-conservative. I don't know that we have a basis for knowing whether it is shorter or longer. I would almost rather leave the same.

MR. BOYACK: Let me ask the question this way: You talked about the risk of making it shorter I think because --

MR. LEAVER: No, it is just that it could be non-conservative.

MR. BOYACK: Well, but you are thinking in terms of the previous case of, I'll call it, the normal burnup fuel and high burnup, and you say, if I take a given transient scenario, then there is going to be a release earlier. So we maybe accept that.

But if we also think in terms, then, that we are doing with this "representative" scenario, which has a multiple scenarios in it, some of which run longer, and then it doesn't seem to me that there is any basis for either shortening or lengthening your time. You just leave it the same.

MR. KRESS: What the old source term did was basically most of the fuel maybe could be described in this code, and there were several sequences where this varies. So there are sequence variations.

So what they did was --

MR. BOYACK: Again --

MR. KRESS: They said this was the start of in-vessel release, and then they sort took two or three of these curves and do a line up, and the total amount was about the same; it was just spread out. So they took a line up to the total amount. That is the 1.3 hours. From here to here is the 1.3 hours.

MR. NOURBAKHSH: That is vessel failure.

MR. KRESS: What?

MR. NOURBAKHSH: 1.3 is vessel failure.

MR. KRESS: Well, this is when vessel failures --

MR. NOURBAKHSH: No, that's much earlier than vessel failure, when we release some of the water --

MR. KRESS: But you know it took about what, a half-an-hour, for the vessel to fail up to this point. So it is like this distance here plus a little bit of time added for vessel failure. Now you're going to have to make it this length of time right here, plus vessel failure. So I think there is a difference in timing.

MR. BOYACK: Okay, the panel needs a break. Come back at 10 after.

(Whereupon, the foregoing matter went off

the record at 9:55 a.m. and went back on the record at 10: 16 a.m.)

MR. BOYACK: Let's reconvene.

During the break I talked with my NRC colleagues a little bit about how to proceed, and the key idea here is to use this day to move through marking up the table. I don't have it shown right now.

A lot of the discussion this morning has revolved around the difficulties of the timing of gap release, which is somewhat affected then by the high burnup fuel. So the answer that was given to me and the proposal to you is that we deal with the duration or the period from the start of the transient scenario to the early in-vessel period; that is, we just deal with that as a single item. So if we had done the same thing on NUREG 1465, it would be a duration of 1.8 hours.

Then we deal with the releases, release fractions, at the end of that period of time. Now if you want to change 1.8 hours to 1.8972, that would be okay, but I would like to keep that discussion fairly brief, and basically, to the end, is there any reason to believe that the vessel failure would be any different if you had high burnup fuel, the timing of

vessel failure? That is, is there a reason for changing it from 1.8 hours to 1.-something hours?

So, first off, is there any concern, issues, or discussion about the concept of now going to early in-vessel, skipping all the releases associated with gap release, that is, dealing specifically with early in-vessel and the fractional releases associated with the timing through periods one and two early in-vessel? Any comments?

MR. POWERS: Yes. I think you are ignoring how the thing gets used.

MR. BOYACK: Go ahead and just continue discussion. Let me ask you this: Do you see another way through this?

MR. POWERS: What I would tend to do is to say, okay, let's follow your logic individually and then by integration collectively. That is, you had a logic thing, applicable or not applicable --

MR. BOYACK: Yes.

MR. POWERS: -- and if it is applicable, change the numbers or not change the numbers. Do that. If in the integration you find that people are unwilling or unable to make decisions based on the gap release, in-vessel release dissector, then you can say, okay, lump them together, and then can you make

your decisions?

MR. BOYACK: Are you talking about just bypassing the gap release timing issue?

MR. POWERS: No, I don't talk about it explicitly.

MR. BOYACK: You would go to that first?

MR. POWERS: Uh-hum.

MR. BOYACK: Okay. So let's take a moment and see if we can proceed, if that is all right. We will give it one try and see if it works.

So, basically, the statement is, let's do a logic diagram and ask the question first: Is the 0.5 hours for gap release timing applicable? And I might just ask the people to go around the table and give their answer. Now would you like me to start over here with Tom first?

MR. KRESS: Sure.

MR. BOYACK: Oh, Tom, the question is: We are following the logic diagram upon the screen. Is the 0.5 hours applicable for high burnup fuel for gap release time?

MR. KRESS: The basis on which it was chosen is not applicable.

MR. BOYACK: Jim Gieseke?

MR. GIESEKE: Same statement.

MR. POWERS: Is?

MR. GIESEKE: Not applicable because the end-point is defined as the onset of significant release, and we know that is moved up when you have high burnup fuel.

MR. KRESS: Yes, that is my basis.

MR. GIESEKE: Yes, that is the same as --

MR. BOYACK: I don't want to get this wrong. Okay, it says: Not applicable because end-point defined as release of significant fission products. That now occurs earlier in high burn fuels. Is that the statement?

MR. CLEMENT: The same statement: The end-point failure with high burnup fuels.

MR. BOYACK: So really what I am hearing, Tom, you are saying that you would go with Dana's -- that was the underlying basis?

MR. GIESEKE: I agree, that is what I was basically saying.

MR. EVRARD: The same statement because there is an overlapping between the gap release and the release of fission product.

MR. BOYACK: Would you like to disagree?

MR. LEAVER: No, no, I think that is right.

MR. BOYACK: Okay, so the first thing I am going to do is I am just going to do this. So 0.5 hours not applicable. Okay?

All right, now the next thing is --

MR. GIESEKE: You need to put an "n" on "not."

MR. BOYACK: "Not"? All right. Okay, are we able to specify a new value?

MR. KRESS: I would add some calculations.

MR. BOYACK: Okay. So the question I ask is that this 0.5 hours was not a single point on a single transient, but it was representative point on a bunch of transients. So are you still not able to specify a new value?

MR. KRESS: Now you can I don't have those transients.

MR. BOYACK: All right.

MR. KRESS: And they are not the same for this high burnup fuel as they used to be.

MR. BOYACK: Jim?

MR. GIESEKE: No. A similar statement. It is not defined from what we know.

MR. BOYACK: Dana?

MR. POWERS: Whereas I think Tom and Jim are precisely correct, I think we can specify a new

value, and I think it is remarkable in that you haven't got a whole lot of room here. So my tendency is to say, yes, I only want to reflect that it is earlier, but it can't be too much earlier. So I have a tendency to put down .4 hours, simply to reflect that things are earlier. In fact, I would cut it in half or 90 percent, but it is going to cut it some. So to reflect some, I would say .4 hours.

Tom and Jim are correct; the right way to do it is to do a calculation. Unfortunately, I think that is a very difficult calculation to do right now. In the end you need a good set of VERCORS or VERCORS-like data or something like that to calibrate your model and decide exactly what it is, but the fact is it is going to be a relatively small change because there is just not much you can do to it.

MR. BOYACK: Okay. Bernard?

MR. CLEMENT: Okay, there are some data from the LOCA experiment with high burnup, but we have not yet finished to analyze this data or derive it sufficiently to evaluate it. However, this should be the way to define this duration.

MR. BOYACK: Let me just clarify. So you say there is some data from VERCORS experience still under analysis, but then did you tie to Dana's

statement or --

MR. CLEMENT: It depends the degree that you are seeing in it there.

MR. GIESEKE: Depending on how much uncertainty you are willing to live with in your selection of a number, this is 4 but maybe not .4. I don't know how accurately --

MR. BOYACK: Is there anything that I should add from this data for VERCORS experiments still under analysis that would clarify what that does or are you just reflecting uncertainty about it?

MR. CLEMENT: I cannot give any figure about uncertainty.

MR. BOYACK: Well, personal uncertainty about whether it would go earlier, later.

MR. CLEMENT: I cannot give a number today.

MR. BOYACK: All right.

MR. KRESS: The adiabatic heatup period probably lasts about a half-an-hour to an hour. You can see it in some of these calculations. I don't know if it is in these things. An adiabatic period lasts half-an-hour to and hour.

If we think this early release starts during the adiabatic heatup periods, it is more likely

to be near the hot end of it than the cold end of it. So Dana's .4 hours is probably about right. You can make some guesses. If your half-an-hour was right before and you are starting earlier, it is not going to start much earlier. So I think Dana has made a real good guess there on .4 without having any real calculations. It is certainly not going to be much slower than that.

MR. POWERS: It is difficult to imagine cutting it in half.

MR. KRESS: Yes.

MR. POWERS: But, on the other hand, you want to reflect that it is earlier.

MR. KRESS: Yes, and that is a pretty good guess, I think.

MR. POWERS: Okay. Jean-Michel?

MR. EVRARD: When you pass it down to the root core and when the gap release is at the beginning of the severe accident, I think it is somewhat difficult to define a limit between gap release and pellet release. So I will say it is difficult.

MR. BOYACK: So am I to conclude from your statement that you would prefer to see this, remove the boundary between gap release and early in-vessel --

MR. EVRARD: Yes.

MR. BOYACK: -- and just deal with the whole duration?

MR. EVRARD: In France we distinguish between the design basis LOCA, so it is clear. In seal accidents we make no clear distinction between a gap release and what is for sure because it is a continuous process.

MR. BOYACK: My brain just went.

MR. LEAVER: It's an "l." That will probably be the only thing that is right about what I am going to say.

Now I think it seems to me it is earlier, but considering the fact that it is only a fraction of the core that is in this higher burnup range, and I don't know whether we are talking about a continuous change or whether there is something, some kind of step function things going on in there, it is still only a fraction of the core. So that in itself would suggest that, while the release comes out faster, it is not a great deal faster because most of the core isn't any different than what was evaluated in NUREG 1465. So I would say that I think it would be appropriate to reflect the fact that it is earlier.

MR. KRESS: Did I hear somebody say it

might be a third of the core, the high burnup?

MR. LEAVER: I have heard a third, a fourth. It depends on what we say by what is high burnup, but I have heard a number of people saying that the average burnup -- oh, I can't remember.

MR. KRESS: But, anyway, if it is a third of the core, it is pretty good fraction.

MR. LEAVER: Yes, but it is still, that is still a fraction.

MR. KRESS: Yes, but it is not like 10 percent.

MR. LEAVER: Well, a third of the core wouldn't be 75,000, but a third of the core might have higher burnup than what we have traditionally been thinking.

I also think that it is not going to the matter to the way that licensees apply this whether we distinguish between a gap duration and an in-vessel release duration. So agonizing over the right number, whether it is .4 or .45 or .35, is really not going to have very much effect at all.

MR. BOYACK: Will not be a large impact on loss, let's see, if the --

MR. LEAVER: I would say it even more strongly. It is not that it is not only a large

impact, it is a small, possibly negligible impact on the way that this is applied.

MR. BOYACK: That is it? Okay. So the question is, what we do on this, but, basically, what I hear from the U.S. participants, who are probably going to make a stronger statement, is that -- I shouldn't even be summarizing this. But, basically, I heard a couple of people that said, okay, maybe this 0.4 hours would be fine. I heard I guess David say -- he didn't say a value, but he says as long as it is --

MR. LEAVER: If the group felt strongly that they wanted to have a value for this and retain this distinction between gap release and early in-vessel release, I think .4 is not an unreasonable number. I keep using the word "reasonable" and "unreasonable," and I agree with Dana that it is maddening, but I don't know what else to say.

(Laughter.)

I don't know what else to say.

MR. POWERS: You can't find a better word to go with it.

MR. LEAVER: Yes.

MR. BOYACK: All right.

MR. LEAVER: Yes, it is hard to argue with somebody who is reasonable, right?

(Laughter.)

MR. POWERS: With the exception of Tom, everybody's kind of reasonable.

(Laughter.)

MR. BOYACK: Okay, so let me ask the question this way: If I put 0.4 as the timing for the gap release, is there anybody that will walk out of the meeting?

MR. POWERS: Is that an option?

(Laughter.)

MR. LEAVER: It's tempting.

MR. BOYACK: Okay, let's see, so we had -- I am just going to highlight that. If I were to clean up the language, not here, but to use these as the key elements of the statement, which is that basically the half-hour is not applicable because the point defined as release of significant fission products and now that occurs earlier with high burnup fuel. Possible to specify new value, basically, with the logic that it could be earlier, but not too much earlier, and that 0.4 hours seems reasonable.

I would probably move some of this logic that Dave offered, because of the earlier fraction of the core. Therefore, most of the core would still be represented by the NUREG 1465 value.

MR. POWERS: Well, it seems to me I would also reflect what Jim and Tom say, because I think they are literally correct: that what you need to do is be able to do a valuable calculation. In order to do that calculation, you really need data from experiments like VERCORS to calibrate that model for the high burnup fuel.

What you are doing in setting that .4 is to reflect the qualitative sense that, instead of extensive release, it is now earlier. That all you are doing right now. To get something really good, you need to do what they say, which was do some calculations with a code that has been calibrated against responsible data, which I think VERCORS probably provides the kind of data that Tom certainly needs for his view to do that calculation in a way he would feel good about it.

MR. BOYACK: Is there an "s" on the end or just "VERCOR"?

MR. SCHAPEROW: I think there is an "s."

MR. POWERS: The French a long time ago decided that "s'es" were there for decorative purposes.

(Laughter.)

MR. BOYACK: Okay, "See needs," the needs

being that new calculation of the code must be validated with applicable data such as is being generated in the VERCORS's experiment.

Okay, I think we can all go home now.

(Laughter.)

MR. POWERS: My tables look more extensive than that. Do all numbers flow from that?

MR. BOYACK: Let me ask, I'm curious about how long this might take, but what about the early in-vessel phase, the duration now, that if you want to think it through to duration, we can subtract off 0.4, but is this something we can rapidly do or do we just need to go back and work on the fractions now?

MR. LEAVER: Well, this worked pretty well. Do you want to try it?

MR. BOYACK: Yes, let's give it a try. Like many of these, it is finding the right technique, right, after the discussion? The discussion was necessary first.

So now I am going to try to just do this.

MR. POWERS: I'm not even going to try to guess what he is doing.

(Laughter.)

MR. KRESS: This time we will go around the table, I suspect?

MR. BOYACK: Yes, would you do that, sort of an alternative approach, and then we can start right at the head of the table on the third approach, and then we could go both ways two at a time.

(Laughter.)

So now we are on the early in-vessel, having experienced rather marvelous success on the gap release. We get to start with Dave Leaver.

MR. LEAVER: Okay. Well, you recognize, of course, you are changing a living strategy by the order in which you do these.

(Laughter.)

MR. BOYACK: I was just going to write that down.

MR. LEAVER: I think, again, that the impact of the high burnup on any of these parameters -- in this case, the duration of the early in-vessel release -- you have to recognize that it is only a portion of the core. That is one factor.

So I would say, if you would think of it in terms of a third, you are probably not too far off on that, whether it is a third or a fourth, or whatever it is.

I also think that this number of 1.3 hours was intended to be a representative number -- I think

that is the word that is used in 1465 -- over a spectrum of accident sequence types. So I don't see a strong basis for changing -- I am not saying that 1465 is right necessarily, but if we use the concept of trying to do a delta, I don't see any basis for, strong basis for changing the 1.3 hours.

MR. BOYACK: I am going to write that down in just a minute, but it did occur to me that what I at least should have done is made sure we all agree with the definition of the end of the phase.

MR. GIESEKE: Yes, I was going to interrupt, but I decided not to.

MR. BOYACK: So if you go to page 8, on the righthand column, the first full paragraph beginning, "During the early in-vessel release phase," go down halfway, there's a statement: "This release phase ends when the bottom head of the reactor pressure vessel fails, allowing molten core debris to fall ont the concrete below the reactor pressure vessel."

MR. LEAVER: Yes, I would have a comment on that. I think that 1465, that may have been the thought process that people went through when they came up with the numbers. In my mind, I think more in terms of a recovered accident, because that is what is

used for the design basis.

So you've got to have some kind of injection, recovery of ECCS and injection. So that, to me, is more kind of what defines the end-point for early in-vessel release as opposed to the bottom head failing in the vessel.

MR. BOYACK: Okay, what I am going to do is just now try to catch -- would you summarize what your thought was on the timing? You were saying you see no reason to change it from, I guess a duration of 1.3 or the total timing, 1.7, now, but you will have to make that a clear point again. I thought you heard you say you see no reason --

MR. LEAVER: Well, I would say no reason to change the total of 1.8. So if we change the gap duration to .4, then I would just say, because in my mind the thing that really ends this release is not vessel failure; it is a recovery which is taking place somewhere around this time of 1.7, 1.8 hours, or 1.6 hours, however you want to define it. I don't think high burnup has anything to do with that. That is an operator action and a system kind of function. So I guess I don't see a strong basis for changing the 1.8 hours.

MR. BOYACK: Okay. Jean-Michel?

MR. EVRARD: If the end of the release is defined as a bottom rupture or fracturing of the system, I think the high burnup fuel in a fraction of the core would have a very low impact.

MR. CLEMENT: So high burnup may have an impact on the fuel movements before.

MR. BOYACK: Yes.

MR. CLEMENT: I mean on the lower pressure. So different scenarios of the relation. However, I think that 1.3 hours is a good amount of magnitude, given the different possible scenarios. One of the possible impacts is from failing, as already mentioned.

MR. BOYACK: I would just ask you, after I have concluded, you may want to take a look at what I have put up for your words, and if they turn out to be wrong, you can enhance them or correct them for me.

Dana?

MR. POWERS: Well, I think my view is almost exactly what Bernard said, that I remain concerned that degradation may change with the high burnup fuel, but that can be deduced really only by experimental investigations that change the modeling we have, because that is a very integral sort of undertaking.

So my tendency is to say that the existing analyses probably are not applicable, but in the face of that I would keep the 1.8 hours as the value, the sum, in other words, in the in-vessel and ex-vessel, or in-vessel and gap release, with the strong codicil that we really do need to do kind of bundled-type degradation experiments to see if things change any significant way in the degradation of these high burnup fuels.

I am very sympathetic to Dave's point that what really gets at interest here is intervention steps.

MR. LEAVER: Is what?

MR. POWERS: Intervention steps. It is something that we have never really investigated a lot of. So quenching-type experiments may get very interesting as well, and there are source term consequences, but we aren't dealing with that. That may be a mistake.

MR. LEAVER: Bundle experiments, would you consider PHEBUS to be a bundle experiment?

MR. POWERS: Oh, yes. Yes. Yes, I think that the utility of CORAR-type experiments for these kinds of investigations are pretty low because you are really looking at types of degradation that go beyond

what CORAR can do. Yes, you don't get as big a bundle as you would probably ultimately like, but that is just the practicalities of the world, and PHEBUS is a decent enough experiment. An experienced team that knows how to do the experiments --

MR. LEAVER: Is it decided that PHEBUS will do a high burnup experiment?

MR. CLEMENT: What we envisage for the future program, we have the high burnup issue, yes.

MR. LEAVER: But that's not -- funding hasn't been collected? What about a quenching experiment? Wouldn't that be a good idea?

MR. CLEMENT: Quench experiments will also be done. Quench experiments --

MR. LEAVER: That's also on the radar screen for the future? Okay.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: I also recognize the need for experimental information, and in view of the range of accidents that we are talking about and the uncertainties in calculations, I think we should stick with the -- the 1.8 is a reasonable estimate.

MR. LEAVER: There's that word again.

MR. GIESEKE: I wouldn't say it's unreasonable.

(Laughter.)

MR. POWERS: Ergo a not unreasonable suggestion,.

(Laughter.)

MR. BOYACK: Tom is ponying up to the microphone to make a statement.

MR. KRESS: Getting around to me next, and I plan on being unreasonable.

MR. BOYACK: Good.

MR. KRESS: The 1.3 duration, which is shown up here, should have a half-an-hour added to it, to make that duration 1.8.

MR. LEAVER: Why is that?

MR. KRESS: It is because in-vessel release starts about a half-an-hour earlier with the high burnup fuel than it did with the old fuel. The old fuel still starts at the same time and ends at the same time. So you have to add a half-an-hour to that 1.3.

Now then you go back and add the .4 to that 1.8, to get a total of 2.-something.

MR. BOYACK: You caught me playing with the computer. So I got caught.

MR. KRESS: I am dealing first with the 1.3.

MR. BOYACK: Okay, so first --

MR. KRESS: And that should be 1.8, in my opinion. That is not the same 1.8 the other people are using.

MR. BOYACK: So it is the duration of the phase itself?

MR. KRESS: Yes.

MR. BOYACK: Should be 1.8 hours. And the reason?

MR. KRESS: The reason is that the release of significant amounts of fission products starts about a half-an-hour earlier with the high burnup fuel than it did with the regular fuel.

MR. POWERS: But up there we said that only --

MR. GIESEKE: But before the gap release, you're saying?

MR. KRESS: After the gap release.

MR. POWERS: But we only moved the gap release up a tenth of an hour.

MR. GIESEKE: We might let you add a tenth of an hour, but you can't add half-an-hour because it starts before the accident starts.

MR. LEAVER: I thought we took that into account when we speeded up the gap release.

MR. KRESS: I don't care what the gap release does. I am saying that the early in-vessel starts a half-an-hour earlier than it did before, and it ends at the same time it did before. So there is a half-an-hour missing in there somewhere.

MR. POWERS: Where is the half-an-hour coming from? Where is that? I don't know where the half-hour came from.

MR. KRESS: I calculated it.

MR. POWERS: Oh, oh.

MR. KRESS: It is the duration of what I all the adiabatic heatup, which is not truly adiabatic, but I call it that anyway.

MR. LEAVER: Is that the zirc oxidation phase? Is that what you mean?

MR. KRESS: No, no. It is prior to the zirc oxidation. It runs about an hour --

MR. LEAVER: A degree per --

MR. KRESS: -- at a degree per second. At a degree per second, it runs about a half-an-hour. At a half-a-degree per second, which is not -- 1 degree is adiabatic. Half a degree is cooler. It runs about an hour. That is where I got my half-an-hour to an hour.

I am saying that the release of the high

burnup fuel starts at about the middle of that, which is about a half-an-hour. I am using the one hour because it is not truly adiabatic. So that is where I get the half-an-hour from. It starts about in the middle of that.

MR. LEAVER: But if 1465 says that the 1.8 hour ends when the bottom head of the pressure vessel fails, why would that be delayed by half-an-hour because of adiabatic cooling?

MR. KRESS: It's not.

MR. LEAVER: It's not?

MR. KRESS: You didn't understand what I said. If you take the old 1.3, what that is is the start, basically, it is the start of the zircox heatup, the runaway zirc, to the time of in-vessel melt. The reason it is that is because that is when fission product release started with the old fuel. I am saying it starts --

MR. LEAVER: That is where it really took off.

MR. KRESS: Yes, and I am saying now it is starting a half-an-hour earlier.

MR. LEAVER: So you are saying it starts, instead of starting -- it starts at 30 seconds?

MR. KRESS: It still ends at the same time

and starts a half-an-hour earlier.

MR. GIESEKE: So it starts before the gap release starts?

MR. KRESS: I don't know. I am forgetting, I am just going to ignore what you guys did with the gap release. I am dealing with the in-vessels.

MR. BOYACK: Let me ask the question another way. Assuming this representative event starts at time zero, how long does it take for the vessel to fail in this representative vessel, the collection of things that you are using? Just combine the two periods, the start of initiating then to vessel failure.

MR. KRESS: It takes about the same amount of time that we had before.

MR. BOYACK: Right. Okay.

MR. KRESS: But the in-vessel release part of the effort occupies a bigger component of that amount of time.

MR. BOYACK: Oh.

MR. KRESS: If we don't reflect that properly, we are going to miss something.

MR. BOYACK: Well, I am tracking your arithmetic here, and so prior on NUREG 1465 that

period of time was 1.8 hours.

MR. KRESS: 1.8 hours.

MR. BOYACK: And are you saying that that total time is still roughly the same?

MR. KRESS: I'm saying it is still roughly the same, but the duration, the 1.3, is bigger. The duration of that phase occupies a bigger part of it.

MR. BOYACK: So if that is the case, then what you are saying is up here on this other, that .4 -- I mean, you've got fixed end-points. Now you are just deciding how to divide up the two pieces.

MR. KRESS: Uh-hum.

MR. BOYACK: So what I hear you saying is that you are looking for the earlier phase to be very, very short.

MR. KRESS: Or the two phases overlap, is what I'm saying.

MR. BOYACK: Oh, okay.

MR. KRESS: You're talking about durations up there and totals, and they don't necessarily add up to the same amount if they overlap. That is why I am saying I'm just ignoring what you guys did about the gap release, and I am looking at the in-vessel part.

MR. BOYACK: So --

MR. KRESS: The total amount of time from

the start of the accident to when you melt through the vessel is the same.

MR. BOYACK: Okay.

MR. SCHAPEROW: So you are okay with 1.8 hours?

MR. KRESS: A fraction of that is occupied by in-vessel release has to increase by about a half-an-hour over the 1.3.

MR. POWERS: Tom, let me ask you just one question. It is kind of distressing, but I think I actually understand you.

(Laughter.)

MR. KRESS: Wonderful.

MR. POWERS: What you are saying is that, if I had my magical microscope and I could look at this spore rod by rod as it moves through temperature transient, that, indeed, we get kind of a collective rupture and start a gap release in a bunch of rods, kind at the same time. But a couple of those rods almost immediately were screaming up in temperature.

MR. KRESS: Ah, ha, you've got what I'm talking about.

MR. POWERS: They, very shortly after they have ruptured, go into the start of what we call in-vessel release.

MR. KRESS: Uh-huh.

MR. POWERS: There are others that are lagging and engage in gap release, and they languidly creep up to the temperature at which they start getting -- but there is a substantial overlap and there is a complete distinction that has been drawn between the gap release duration and the in-vessel release that ignores the heterogeneity of core behavior and should never have been made to begin with?

MR. KRESS: I wish I had said it like you did, Dana. That is exactly my thought process, exactly.

MR. POWERS: I think you are in fact technically absolutely correct. There is always the problem with our codes. When we did the original March code, we saw the core was very uniform in behavior, and the evolution since that time has been make its behavior more and more heterogeneous, especially for these low-pressure accidents. When you stay pressurized, there is a tendency to make them homogenized, but for these low-pressure accidents there's a great amount of heterogeneity, both axially and radially.

The distinction, it really comes from

saying, okay, we are going to work with the large break LOCA. Had they said work with a panoply of accidents, then you might be more comfortable with the distinction, but because they have said a large break LOCA, now you can't use -- you are saying there is so much overlap that summation is just not good.

MR. KRESS: That says it very well.

MR. POWERS: I don't think I disagree with you on that view, and I understand where you are coming from there.

MR. KRESS: That was a very nice clarification. Thank you.

MR. BOYACK: Okay, basically, what I have said is that total time is about the same, or 1.8 hours; a fraction of that occupied in-vessel release is about 1.8 hours. This implies that the gap release and early in-vessel release periods overlap.

MR. LEAVER: It implies more than that. It implies that the early in-vessel release begins at time zero.

MR. KRESS: Yes, and I didn't really mean to say that.

MR. LEAVER: I think that's a little bit too strong.

MR. KRESS: I really didn't mean to say

that. I think the total is maybe not 1.8 hours. It is more like 1.7. The fraction is like 1.7, 1.6 maybe.

MR. LEAVER: Yes.

MR. KRESS: You know, I'm guessing. I don't have the calculations.

MR. LEAVER: I understand, but if we are going to --

MR. KRESS: But you have to be consistent in terms of it doesn't start at zero time. That's for sure.

MR. LEAVER: Okay, let me ask you this question, because this is another way of trying to find that number: Recognizing that we have a spectrum of sequences that we have to kind of bear in mind here, even though low pressures may be the main one, and recognizing about this distribution of burnup in the core, at what time, 10 minutes, 5 minutes, 15 minutes, whatever, would you say it would be reasonable to specify that the early in-vessel release starts? I know it is not zero.

MR. KRESS: I know.

MR. LEAVER: And you don't like 30 minutes.

MR. KRESS: I think it is about a half-an-

hour.

MR. LEAVER: You think it starts at half-an-hour?

MR. KRESS: Well, I think --

MR. LEAVER: No, that would --

MR. KRESS: It depends on the sequence. From the time the melt, the water, is starting to uncover the core until the time you get this release, I think is about a half-an-hour, if that helps you.

MR. LEAVER: It might be sooner.

DR. GRIFFITH: I think it is about a half-an-hour. Well, it depends on the heat, of course, but if the heatup rate of the top parts of the core are about a half a degree centigrade percent, then I think it is going to take an half-an-hour to get up to the temperature where I think these high burnup fuels start releasing.

MR. LEAVER: Start releasing, yes.

MR. KRESS: But if that heatup rate is a little faster, it is going to take less time.

MR. LEAVER: Okay, but if the answer is half-an-hour, that would say that -- and you believe the 1.8 hours is still a reasonable number for vessel, that would say that you are back to 1.3 hours for the duration of the in-vessel release, but you have

started out by saying you think it is longer than that.

MR. KRESS: Well --

MR. LEAVER: Do you see what I am saying? I mean, maybe it is in between. That's what I thought you were going to say, is, instead of a half-an-hour and 1.3 hours, you would like to see it maybe have the in-vessel release start at, instead of 30 minutes, have it start at 15 minutes or 20 minutes, and then instead of 1.3, you've got like 1.5 or 1.6 hours for that duration.

MR. BOYACK: What I propose is the following: As we get to the end of the day, what I will do is I will create PDF files of all this information. I will work with Jason. I think what I will do is I will leave somebody here to work through the next meeting time, and Jason and I will go upstairs and we will kick out these files and get print copies, and then overnight you can spend a little time reviewing and thinking about this and correcting them, if you wish, and return them to me. Is that okay?

MR. KRESS: Yes.

MR. BOYACK: Good.

MR. LEAVER: I am still --

MR. KRESS: I think the problem is in the 1.8 that we talked about, the start of the thing until the time you got through the vessel.

MR. LEAVER: You think it may be longer?

MR. KRESS: I think it's definitely longer.

MR. LEAVER: But if that is true --

MR. KRESS: But that sort of relation of things, and so when we are trying to fix these numbers that I have here with that, it gets to be sometimes the total doesn't seem to add up right, and you have to face that.

MR. LEAVER: So one way to address your comment, and Dana kind of just summarized it beautifully --

MR. KRESS: Is extend that 1.8.

MR. LEAVER: -- is extend the 1.8, and then you are looking at maybe, say, the same 30 minutes or .4 hours, whatever that is, and the instead of 1.3, maybe it's 1.6 or 1.7, which makes the 1.8 go to, say, 2 or 2.1.

MR. KRESS: Something like that probably.

MR. LEAVER: Yes. Okay, now I see what you ar saying. That would work. I think that would work.

MR. BOYACK: Okay, now I may need a little help on this because -- just see if there is some overall agreement. What I have done right now and highlighted in red is just to indicate that I have left it 1.8 total hours. I haven't increased the time to a larger total period.

MR. KRESS: In addition, you have integrated bundle experience with high burnup fuel. That needs to be translated into models and whole core. See, integrated bundle experiments are not whole cores. It needs to be translated into models like VERCORS and whole core calculations made.

MR. BOYACK: Okay, I guess what I heard was a suggestion which said that this 1.4 hours might be increased.

MR. LEAVER: Yes, I think what Tom is saying is he doesn't necessarily believe that the 1.8 hours for vessel failure -- and I must say that is awfully fast, but we also have a spectrum of sequences here, and so we are just trying to come up with something that is representative, but it would, in my mind, not be at all inappropriate to suggest that the 1.8 seems fast to us for the original 1465. So we want to push that out a little bit. That gives us a way to address Tom's comment of the fact that he

believes that this release duration is longer, because you've got the high burnup fuel on a low burnup fuel, and it just takes longer for this to occur.

MR. POWERS: Looking at some of the Source Term Code Package calculations for large break LOCAs, I believe there is one in there where for the period of time from core code rate to vessel failure, I understand there is like zero time in the rule of thumb in the Source Term Code Package calculations. I mean it is a very brief time. It is only 38 minutes.

MR. LEAVER: From the time of relocation?

MR. KRESS: Yes, but it takes about -- I don't know when they decide this accident starts. I takes about sometimes half-an-hour to get to the top of the core.

MR. POWERS: Oh, yes, it takes a long time to get to the top of the core.

MR. KRESS: There is a blowdown on top of that which takes maybe 20 minutes for some accidents. It depends on the size. So I don't know when they are assuming the start of this thing is. That is why I worried about this 1.3 hours or 1.8 hours, because I don't know when they are starting that accident.

MR. POWERS: I think these things are

basically started at the top of the core. I'm not positive of that.

MR. KRESS: Yes, I am not either.

MR. POWERS: But, traditionally, that is where the --

MR. LEAVER: Are you talking about the onset of gap release? When does that start?

MR. POWERS: Yes.

MR. LEAVER: I think that could start --

MR. KRESS: When is zero time?

MR. LEAVER: No, no, see, the way they do those calculations now is -- well, no, I'm sorry.

MR. POWERS: There is nothing new with those calculations. It has to do with the code that is set up.

MR. LEAVER: Yes, that's right. Yes, but you have a small LOCA. You could be six hours into the accident before you get down to below the core centerline.

MR. POWERS: All those things are true, but this is a large break LOCA. So before you jump and say, well, our time period is short, my recollection is there is at least one large break LOCA; I think it is an AB sequence, but I could be wrong about that. It is an extraordinary short period

of time.

There was a lot of controversy when the calculation actually came out. I can't remember what the resolution was. I think the resolution was we are not going to worry about it because it is a low-frequency accident. But it's a blitzing fast thing.

MR. SCHAPEROW: To interject, actually, to reiterate, there is a table, Table 3, which lists the duration of each of the sequences considered in the in-vessel release phase. The 3-inch LOCA, which is done for O'Conee, with no ESF, was 84 minutes.

MR. LEAVER: But there is one -- oh, okay. But here's a series sequence, S3B, that is 36 minutes. Those are really simple bottom head failure models, though.

MR. POWERS: Oh, yes, the bottom head failure models were very simple.

MR. LEAVER: I know that the models in Melcorn map now are quite a bit better than what was used.

MR. POWERS: They are still fast.

MR. LEAVER: I don't think they are as fast as this though.

MR. POWERS: Well, I think that was, and Jim can probably speak better than I can, but my

recollection is the way we were treating the lower head was really very simple. I mean, it was one step better than March, which was fuel sees head; head fails.

(Laughter.)

MR. BOYACK: Let me ask the question this way: So far, I have heard Tom make a strong positive statement for a longer period for early in-vessel. I heard Dave make a, "Well, maybe that would be okay." Jim, keep it where it is or change it?

MR. GIESEKE: I could see it creep up a little. You know, these numbers are not very precise in any case. You're looking at how many different sequences, and you are making seat-of-the-pants guesses as averages anyway, and you are not even doing it a numeric average. You are doing a, well, I don't know, flight-of-fancy average on these. So if it is 1.4, 1.5, 1.6, those are all the same numbers as far as I am concerned.

MR. BOYACK: Okay. Dana?

MR. POWERS: My view is that the end-points of these times are dictated by the core degradation process. I'm not here as an expert in core degradation process except to raise the possibility that that direction is a bit different

with high burnup fuel than with the fuels that we thought about in the past, which, quite frankly, were heavily biased toward 24 gig, 20-gigawatt-day fuel --

MR. LEAVER: Average.

MR. POWERS: -- and peaked at like 34 or 35.

MR. LEAVER: And homogeneous.

MR. POWERS: And relatively homogeneous core.

So I am unwilling to move those things around. I say that that is one of our needs, that we really need to investigate experimentally the degradation of cores with high burnup fuels, with an eye to seeing if we need to introduce a qualitatively different core degradation model than the one, the basically candling model that we've got now.

MR. LEAVER: Similar comment to the one you made on the .4?

MR. POWERS: Yes.

MR. LEAVER: The need for an integral experiment.

MR. POWERS: Yes, I mean we just can't ignore these things because we've had this hint. It first showed up in a PBF3 experiment. It showed up again in a Sandia pilot experiment. It showed up in

one of the FLHT experiments. It showed up now in some of the PHEBUS experiments. Some sort of fuel foaming goes on. It has always been a hint.

When you think about fuel foaming, the things that are moving us toward more fuel foaming are higher-efficient product gas goods. That is what high burnup fuel is.

If we see the kinds of foam that we saw in the Sandia experiment, it provides a transitory period in which core degradation is completely different than the candling model, and you get a period of very high core temperatures and then sudden collapses, things like that, actually moving back toward more of a March-type model. March may have been right all along.

MR. BOYACK: Do our French colleagues have any comment about the difference between 1.4 hours and 1.6 hours?

MR. CLEMENT: Okay, two comments. First of all, the main difference is about fuel degradation processes. If we have a collapse that's earlier or not, I don't know exactly.

The other point is about the 1.3, 1.4 hour, about sequences. I mean, some sequences we have used for our source term reassessment, but we have

gotten even those that are shorter in AB sequences than the 1.3 or 1.8. There is one for which we have 1.3 hour because there is a break in the lower head failure, for instance, but with others it was much longer times. So, as I said before, the order of magnitude is not bad.

MR. LEAVER: It's not bad.

MR. BOYACK: Other comments? So the real question here to me is whether I leave it at 1.4 or whether I change it to 1.6. I just need help, like three or four statements.

MR. CLEMENT: Yes. If we change it from 1.4 to 1.6, I think you probably would be aware that there would be a problem, because we lower as well the concentrate. We believe we lower it for this. Okay? We think that using hydronic fuel goes in the other direction that we raised earlier. We can get fast, higher release rates. So I would not favor shortening this period of in-vessel release, because this could give wrong indications to people who will use it.

MR. SCHAPEROW: You have the wrong person there (referring to what Mr. Boyack is typing).

MR. BOYACK: Oh, I was putting words in your mouth.

(Laughter.)

There we go; you've been cut-and-pasted.

Okay, if we are going to increase the period, it would imply lower fission product release rates; that seems to be contrary to the expectation for high burnup fuel.

MR. POWERS: Let me say that I wholeheartedly agree with the "do no harm" philosophy. Don't mislead people with your changes in qualitatively incorrect corrections. Do no harm is the first step in making changes.

MR. BOYACK: All right, I will figure out how to handle this. Leave as is. So I come back up and so 1.4 is what I sort of hear now as the outcome. All right?

Now at this point it seems to me there is a break between these later times and these earlier times, and what you want us to do is to get the numbers for the earlier time as far as releases. Is that correct, or should I just go on and try to deal with the ex-vessel and late in-vessel durations?

MR. SCHAPEROW: I think we should stick with in-vessel right now.

MR. BOYACK: Yes, okay.

MR. SCHAPEROW: If Dana feels differently, that's fine. You guys are making much better progress

without me.

(Laughter.)

MR. POWERS: Well, I think here's what I will offer this time: Most people say, "ex-vessel, skip it, leave it alone." But I think Bernard may have some interesting comments to make about the late in-vessel release that I would like to hear what he has to say on that.

MR. BOYACK: Well, to me, other than just moving along, which we are doing now, it is not too much concern to me. We won't be dealing with BWR's tomorrow. We'll never make it.

MR. POWERS: On the BWR's, let me make this comment: that Ralph made a pretty important point, I think, in his presentation that said that BWR fuel is evolving toward looking more and more like PWR fuel with like channel boxes.

I suspect that the channel boxes dictate the timing for the degradation, and the fuel rods dictate the release fractions. So I bet you the timing on the BWR's looks about the same as it does now, and the release fractions that we derive for the PWR's begin to look a lot more like the same for BWR's.

MR. BOYACK: So we might be able to move

quickly through the BWR here, you're thinking? At least we would have the information on the BWR's that we could take a look at that.

Well, look, let's take a minute then and see if Dana's statements about ex-vessel hold true. We said we would start at the center of the table. So Dana?

MR. POWERS: I think the people that developed the ex-vessel releases that has dominated by the period of high zirconium, I don't see that changing very much with the high burnup fuels. I don't see any reason to change either the timing or the release fractions then.

MR. KRESS: Now the release fractions are the fractions of the full inventory. If we are getting more out in-vessel -- the release fractions are the fractions of the original core inventory, and if we are getting more out in-vessel, we will get less out --

MR. POWERS: You've got to keep the sums correct.

MR. KRESS: You've got to keep the sums correct.

MR. POWERS: Yes.

MR. KRESS: I think Dana is exactly right

on the zirconium. So I do see a need to change the timing, change the release fractions, but not the timing.

MR. BOYACK: Yes, just stop with timing?

MR. KRESS: Yes.

MR. SCHAPEROW: Yes, no "oxidation" after "zirconium".

MR. BOYACK: Pardon me?

MR. SCHAPEROW: After the word "zirconium," add oxidation.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: I agree with Dana.

MR. BOYACK: Tom?

MR. KRESS: I agree now with what you have.

MR. BOYACK: Okay. Bernard?

MR. CLEMENT: Yes, in our first-time evaluation it was the same thing, so high zirconium goes into action. So in our evaluation we had less inventory in the ex-vessel. So, in fact, less fission, so the duration was shorter. But I am not a specialist about that.

Jean-Michel?

MR. EVRARD: If you consider relative value against the produced value, I see no reason to

change the numbers. No change. In fact, we have a rather small value.

MR. BOYACK: Bernard, what I heard you say is that in the French study you may have a shorter --

MR. CLEMENT: For the main release, not for the duration of this phase as concerns degradation, but to say what this should be released, it would be released at the beginning of this phase. That's it.

MR. BOYACK: Oh, okay.

MR. POWERS: I think the reason that the ex-vessel was two hours long is they said, well, it may be that you get a big plump come down and then you get stuff dripping down over some period of time. That's what actually caused them to extend that. Otherwise, it is a huge worry, and then it dies off very quickly.

MR. CLEMENT: Yes, that was the point, yes.

MR. BOYACK: Dave?

MR. LEAVER: I would retain it.

MR. BOYACK: And do you guys promise that from now on every one of these will be the same pace, right?

MR. GIESEKE: Just like this.

(Laughter.)

MR. POWERS: I thought we were going to move a little faster now.

MR. KRESS: Now we will get to the late in-vessel.

MR. POWERS: I thought the plan was to go faster as we go through this.

MR. BOYACK: That's right. I'm sorry, I misspoke.

MR. POWERS: What you need to do is say, okay, no lunch break until we get through this much, and see if we are smart enough to delegate pieces to each one and say, "Whatever he says, I go along."

(Laughter.)

MR. KRESS: We would speed it up by a factor of five.

MR. POWERS: Yes, right.

MR. BOYACK: I am using end-points because that works for me. So, Tom, we are back to you. Move up, yes, right there.

MR. LEAVER: Is this the duration of late in-vessel?

MR. BOYACK: Yes, the duration of late in-vessel.

MR. KRESS: I think it has probably

shortened because I think there is more fission product volatiles out there to create a faster heatup rate wherever it is deposited. When it starts coming out a little sooner and gets out a little faster -- it is a qualitative statement. I don't know how to pin down any difference. What have we got there, 10 hours? That is such a long time anyway that I suspect it is not going to change it much, because the difference between the quantity of fission products that are plated out is probably not going to be much. So I would stick with the 10 hours. If that is what they calculated before, I would stick with it now.

MR. SCHAPEROW: The idea is that this is a self-heating basically, that fission products, it heats up the layer of fission products?

MR. KRESS: That's my view of it, yes, but I don't think there is much difference in how much is there and how much the heating is, and how much heating is contributed by the steam and hydrogen.

MR. BOYACK: If somebody can help me -- the period is shorter because there is more fission product volatilization --

MR. KRESS: It is shorter, but not significantly shorter is the point.

MR. BOYACK: Jim?

MR. KRESS: We have probably increased the semi-volatiles component, and I don't know if they have any effect on this or not, probably not.

MR. SCHAPEROW: The only three things that we show as revitalizing are --

MR. KRESS: Iodine, cesium, and tellurium.

MR. SCHAPEROW: Iodine, cesium, and tellurium.

MR. KRESS: Yes.

MR. SCHAPEROW: Tellurium is a very small number.

MR. KRESS: Yes.

MR. GIESEKE: With high burnup, that may change a little bit.

MR. KRESS: It may change.

MR. GIESEKE: But probably not a definable amount?

MR. LEAVER: The magnitude would change.

MR. GIESEKE: Yes, the magnitude of some of the other groups of fission products may change, but -- is it my turn to talk?

MR. BOYACK: Yes, it is, it's your turn to talk, and in short phrases, succinct, correct, and accurate.

(Laughter.)

MR. GIESEKE: You're saying that I usually don't do that, I gather.

MR. POWERS: No, he's says as opposed to the previous speaker.

(Laughter.)

MR. GIESEKE: I will say that, although the composition of the deposits may change slightly, it is not a definable change, and I don't see that we need to change the duration.

MR. BOYACK: Dana?

MR. POWERS: Get ready to type. Are you ready?

MR. LEAVER: He's going to be unreasonable.

(Laughter.)

MR. POWERS: Yes, I see the late in-vessel release as being composed of three constituents. One is the revaporization of deposited radionuclides, degradation of residual fuel within the core region, and air ingress.

MR. BOYACK: Air ingression?

MR. POWERS: Air ingress. The existing analyses really only addresses the first two of those. We don't know what the third would contribute as far as source term, and that needs to be experimentally

investigated because we imagine there to be a competition between the degradation process in air and the fission product in air, and we don't know how that competition will come out.

MR. KRESS: You are saying the original is wrong?

MR. POWERS: Yes.

MR. LEAVER: The 10 hours?

MR. POWERS: The 10 hours and everything about it.

MR. GIESEKE: the change is not the result of higher burnup.

MR. POWERS: But I've got to do a little introduction here.

MR. BOYACK: Just help me for one second. The competition between?

MR. POWERS: Degradation and fission product release.

MR. BOYACK: Okay. Thank you.

MR. POWERS: Okay. The duration of the late in-vessel release was dictated by the revaporization of fission products off, deposited fission products off the reactor coolant system. I think that is a best speculation, whether that is 10 hours or 9, because we don't understand that very

well.

So right now, in the absence of additional information, I am going to say stay with the 10 hours, but clearly this is an area where we need substantial further investigations. Some of that will undoubtedly come from the post-test analyses of PHEBUS deposits, deposits from the PHEBUS that peaked on that test.

I think that says what I have to say.

MR. BOYACK: Okay.

MR. POWERS: I am anxious to hear what Bernard has to say because I may want to amend and say, "Me, too."

(Laughter.)

MR. CLEMENT: Okay, I had some trouble with this phase before, but then I made it very clear, because I think we should not only consider the long-term revaporization, but also other processes. In the other processes, for instance, air ingress, you may have, for a given period of time within these 10 hours, higher fission. That should be stressed because consequences would be different when it comes into the containment.

MR. BOYACK: Jean-Michel.

MR. EVRARD: No further statement at this point.

MR. BOYACK: Okay.

MR. LEAVER: I know of no basis for changing the 10 hours, even though it is probably wrong, and I think the air ingress is absolutely crucial to understanding this late in-vessel. We don't know much about that.

MR. POWERS: We need an experiment because --

MR. LEAVER: Yes.

MR. POWERS: We simply cannot right now -- I don't see any way to analyze my way out of the accident, out of the dilemma. I mean, I can do an analysis and get a result, but the result is as good as my analysis, and I don't know how good that is until I do an experiment.

MR. KRESS: You get a hint from the high oxidation tests on fission product release, you get a hint that that is going to make a difference. It may make more --

MR. LEAVER: It's going to be even more.

MR. POWERS: When I tried to use MELCOR, I could get any result I wanted by just dictating how fast I got sloping to the curve. Yes, I could get anywhere from absolute catastrophe, a catastrophe made for movies, to nothing, because it sloped together so

fast, and without this prescient -- when it is in a molten pool, there isn't any mass transport capabilities.

MR. LEAVER: There's no free surface.

MR. POWERS: As my assumptions went, my results went.

MR. KRESS: And the calculations previously done assume vapor pressures above the particular temperature for species deposited. That is almost surely to be wrong. There is no activity coefficients for the intermixed species, no chemical reactions for the surfaces.

MR. POWERS: That's right, and everything that comes out of the PHEBUS experience says these deposits are far more complicated than we thought. There's some on there that they can't chip off with a hammer, and there's some of them that blow off when they remove parts from the system. Trust me, we never thought of that diversity in the codes.

MR. BOYACK: All right, let's continue on and finish the rest of the PWR entries before lunch.

(Laughter.)

MR. KRESS: It's time for lunch now.

MR. BOYACK: Let's see, okay, that's fine.

Okay, what I have done here is, that is

the first one in the table. Isn't that noble gases?

So the values that are listed here at the top are the NUREG 1465 values. Let's go ahead and go through the phases.

David, would you like to --

MR. LEAVER: Okay, what are we estimating here?

MR. BOYACK: Okay. We are looking at the gap release. So what we are going to do is, for noble gases, the gap release first; the value for NUREG 1465 was 5 percent, about .05. So if we go to our logic, we ask, the first question is: Is the given source term of .05 for a gap release of noble gases still applicable? Yes or not yes, is the answer.

So you might think through that logic yourself, each of you, as we go on. I do recognize that during the course of the dialog there may be points made that allow us to change our mind. That's fine. We just have to go through the process. This seems to work.

MR. KRESS: For some reason.

MR. BOYACK: It evokes comments.

MR. LEAVER: I can take a crack at this, and I'm sure that I may well want to revise it when the points are made.

We have shortened the interval of this because we recognize that things happen faster for the high burnup fuel. I also think that there is some data, although it is kind of running together in my mind. That is why I was interested in getting those slides that we talked about yesterday that you showed.

But it seems like the gap release is a bit higher for the high burnup fuel, but the high burnup fuel is only a fraction of the core. So it would not be unreasonable to maybe nudge the .05 up a little bit, but not too much because we've already shortened the interval, and the high burnup fuel is roughly a third or a fourth of the core. So maybe a 20 percent increase from .05 to .06 or something like that would at least qualitatively get the idea.

MR. BOYACK: Jean-Michel, would you have any comments?

MR. EVRARD: Yes, as I mentioned yesterday, EDF performed measurements on MOX for different burnup, and this measurement shows that above about 60-gigawatt-days-per-ton, you can reach a value of 5 percent of noble gases in the gap now. And the burn --

MR. LEAVER: Above 60?

MR. EVRARD: Yes, about. About. The

other point is that there was an in-pile experiment in FLASH with high burnup fuel, and I think it was 50-gigawatt-days-per-ton, which showed about 5 percent, but it only 50-gigawatt-days-per-ton. So the explanation was that the part of the gases would have released in the experiment. So I would say that maybe go over a 50-gigawatt-days-per-ton, and it is possible to have, I believe, not just 5 percent. But, as you mentioned, the high burnup fuel is only a part of the core. So there are all sorts of things to take into account.

MR. BOYACK: I'm not sure I have yet got to the point where I can summarize this. I heard you say that French data for high burnup fuel indicates value of about 5 percent?

MR. LEAVER: Is that the experimental data? Is that what you said?

MR. BOYACK: You referred to EDF data.

MR. EVRARD: No, that's two parts. The first part is measurement by EDF on --

MR. KRESS: On spent fuel.

MR. EVRARD: Yes. They show that you can reach 5 percent at about 60-gigawatt-days-per-ton. So this is the amount of fission product of noble gases in the gap.

MR. BOYACK: All right.

MR. EVRARD: And the second part is that there was an in-pile experiment with fuel, high burnup fuel with 50-gigawatt-days-per-ton which showed the release of both of the previous values is about 5 percent.

MR. BOYACK: Higher than 5 percent gas is what I heard. Again, you can certainly correct any of these that I may have misinterpreted.

MR. EVRARD: Okay.

MR. SCHAPEROW: Is it prior to this 5 percent or at 5?

MR. BOYACK: That is what I am not quite sure.

MR. EVRARD: So what is it, it was 5 percent?

MR. SCHAPEROW: At 5 percent.

MR. BOYACK: At 5 percent, but then I thought I heard a qualification.

MR. LEAVER: Well, I think his opinion is that that's higher, the 5 percent is higher than what you would get at, say, 30 or 40. Is that what you said?

MR. EVRARD: Yes, because if you consider the measurement of EDF in the reactor, you would have

less than 1 percent at 50. It would be better.

MR. BOYACK: Okay. All right, Bernard?

MR. CLEMENT: Really nothing to add to what Jean-Michel said, except that people if want -- this is not, I mean, for just to show some kind of statistical work on a LOCA experiment, and what is the released during the transient up to 1200. If you need more accurate data, you have to make fission using the LOCA experiments. I mean, not for this Source Term Panel, it's not so important, but for people like Ralph Meyer or other people working in the same field, you need more data. I think you have to plan LOCA experiments to make fission product measurements.

MR. BOYACK: Dana?

MR. POWERS: First of all, I think you ought to add that comment.

MR. BOYACK: Well, help me with it then.

MR. POWERS: Need LOCA experiments, need fission product release, fission gas release.

MR. LEAVER: For high burnup fuel.

MR. POWERS: For high burnup fuel, yes. Now they've got a few out-of-pile experiments, and they're thinking about some in-pilot experiments.

Okay, let's see, I would put a plot up here.

MR. BOYACK: Just hold for a moment.

MR. KRESS: Don't mark on the screen.

MR. BOYACK: Oh, this one?

MR. POWERS: I've got to get back to my microphone.

What I had put up here is fission gas releases observed in reactivity insertion accidents in Japan, and the points are I have indicated what the average burnup in the fuel was in gigawatt days per ton. I mean, I understand what they are doing is that they are putting an energy pulse into this fuel, and in some cases the fuel is coming apart, what-not. They do them in a capsule so they capture all the fission gas release. So you get some indication of what an energy pulse does, and you see what amounts to kind of a step behavior.

Somewhere between 42 and 50 gigawatt days per ton you get a lot more fission product release, and that is pretty consistent with the existence of a high burnup structure, where you have created a lot of fission products adjacent to the perimeter of the fuel that are available for release.

What you would really like to see is, is there a grouping of the results, not just at 50, but as you approach 75 gigawatt days per ton? And we just

don't have those experiments.

Now this leads me to believe that there are more fission products available for ready release when you stress the fuel in some way. Here this is a pulse thermal. Does a mild thermal? Well, I think it probably does.

So it leads me to believe that there is a substantially higher fission gas release possible from the high burnup fuel. Dave correctly points out, well, that's only a fraction of the total fuel in the core. So if I wanted to do a core averaging, I've got to reduce that down.

So I propose to increase the noble gas release associated with the gap period from the existing 5 percent to 10 percent, with the recognition, I think, in total agreement with Bernard, that one needs to look and see if in this LOCA experiments the formal transient that you get in a LOCA is that sufficient to get that near surface gas out.

MR. LEAVER: These are RIAs?

MR. POWERS: Yes, these are RIAs, but they're putting --

MR. LEAVER: We notice the transient is more severe than --

MR. POWERS: Yes, it is a real energy pulse you are putting into the fuel, far more energy that you're getting in in this period between fuel rupture and lightoff of the zirconium. That is not a whole lot of energy. On the other hand, it is a lot of time, and time and energy count here. So I don't know exactly how it goes, but I want reflect that things went up and then just say, measure it.

You can get that measurement because, when you reconstitute the fuel for one of these LOCA experiments, yes, you lose the gap inventory, but it is really this near-surface material that you are interested in, whether that adds to the inventory or not.

MR. CLEMENT: Couldn't you then control the gases?

MR. POWERS: Yes, well, that will give you the amount that is actually in the gap. What I want to know is in this .4 hour period do we get a lot more out.

MR. CLEMENT: Yes.

MR. POWERS: and I raise to say, yes, I think you will. I'm taking it to 10 percent. Am I going to get down to brass tacks with Dave over his 6 percent to reflect that? No, I'm not going to argue

with him. I mean, we're both doing the same thing. We're trying to reflect, yes, there is more gas in a more releasable form near the perimeter, and it might come into the gap release, I think.

MR. LEAVER: I can't give you numbers, but in talking to people at Westinghouse, for example, that worry about these non-LOCA-type events such as main steamline break, steam generator tube rupture, locked pump, it is my understanding that the energy deposition, the rate of energy deposition is significantly less than you would see for an RIA. But at the same time, certainly there is some energy deposition, and without really having an experiment or some good benchmark models, we don't know how much of this increase that you see here, which is really rather kind of a step change --

MR. POWERS: Yes.

MR. LEAVER: You don't know how much of that would apply, but I would say it would be probably appropriate to say that you would expect to see this kind of a step change for these more mild transients.

MR. POWERS: Yes, I think what you have to understand is a lot of this energy actually goes fairly deep in the fuel in these things, and it is really the surface that counts here. That is why I am

a little suspect of the out-of-pile experiments, where the heating is coming from the outside instead of from the inside out in this phase. I think they will give us an upper band on the amount of gas at least during the gap phase, in addition to that that is actually in the gap, the inventory.

MR. LEAVER: Right.

MR. POWERS: In-pile experiments might be better here.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: Listening to all the comments and the discussion, my sense is that these reactivity insertion experiments probably overestimate the effects, but they do indicate that there might be a significant effect. As you see, we are still not up to the kinds of burnup levels that are of interest to us, which is at least what, a third or fourth of the core.

If you would say, well, maybe it would double the rate for that one-third region that is high burnup to, I don't know whether we are talking 70 or thereabouts, gigawatt data per ton, you might take the overall core average up maybe beyond the .6, or the 6 percent, up closer to the 10 percent value that Dana is talking about. Somewhere in that range I would

support, probably towards the upper end of that range, maybe not quite as high as 10 percent, but in there.

MR. BOYACK: Okay, I'm basically saying that you don't thing the reactivity and insertions events are fully representative. Would support some increase in gap release above 5 percent, but 10 percent is at the high end of the range that might be supported, and we haven't decided where it was in here.

MR. GIESEKE: Yes, that's good.

MR. LEAVER: Just so I understand, Dana, on your increase from 5 to 10 percent, is that in the high burnup fuel or would you say core-wide?

MR. POWERS: That's core-wide.

MR. LEAVER: So what you're really saying, then, is?

MR. POWERS: The high burnup fuel is doing a lot.

MR. LEAVER: Is doing more like 20, 25 percent?

MR. POWERS: Yes, yes, that's what I'm saying.

MR. SCHAPEROW: So he is saying it is dominated by that.

MR. LEAVER: And you think 20 or 25

percent for this transient which is LOCA?

MR. SCHAPEROW: So you're saying like 30 percent of the noble gases --

MR. LEAVER: Would be released --

MR. SCHAPEROW: -- from high burnup fuel?

MR. LEAVER: -- from high burnup fuel?

MR. POWERS: Around 20, 25 percent, somewhere around in there I think is a pretty good guess. Because, remember, that as we go to high burnup-ness, coefficients are taking place in the perimeter, because the plutonium we have generated is what fissioning. It has a huge a fission cross section relative to uranium.

MR. BOYACK: Okay, Tom?

MR. KRESS: I must say in this case I am in agreement with Dana that these experiments do indicate that there is more possible releasable material in the gap, and I see no reason not to think it couldn't be released in the less energetic experiments. So I would support the 10 percent actually.

MR. LEAVER: What is the conversion of joules per gram to calories per gram?

MR. POWERS: You divide by four.

MR. LEAVER: So that data at 200 joules

per gram is of the order of 50 calories per gram?

MR. POWERS: Yes.

MR. KRESS: Pretty darn low, right?

MR. LEAVER: And the energy deposition that Steve LaVie was talking about that's in the --

MR. KRESS: Two hundred eighty.

MR. LEAVER: So that would be out around 800 or so.

MR. POWERS: The regulation would build upon trace-radiated fuel basically.

MR. KRESS: Yes, almost fresh fuel.

MR. POWERS: And what we are learning is that high burnup fuel, you just have more damage in the fuel; it behaves funnier.

What these data suggest to me is that, indeed, restructuring that perimeter does interesting things to you in the release of the gas. Now I hasten to emphasize this is just the fission gas. I don't think that there is a similar effect with respect to cesium and iodine, and we will get to that number.

MR. BOYACK: Yes. Okay, what we've got here is a range, and we had David speak initially but roughly 6 percent he said he could support, 10 on the other end with a couple of participants. I heard Jim saying, well, I'm certainly a little bit more in the

middle; I could maybe stretch to 10 percent. Our French colleagues basically I think they would see higher burnoff, but didn't really say, higher releases but not any specific value.

So the question is: What do I put up here for the release?

MR. LEAVER: Okay, that's a good question. Let me say something since I was the first one and some things have come up.

MR. BOYACK: Absolutely.

MR. LEAVER: I would just give an opinion; that is that, to go from 5 percent to 10 percent on the basis of this RIA data -- did I say that right, RIA --

MR. BOYACK: Yes.

MR. LEAVER: -- just strikes me as too extreme.

I am looking at the FLASH data which was just a 5 percent release. That's fission gas; krypton, was that? Jean-Michel, that was a krypton measurement, right?

MR. EVRARD: Yes.

MR. LEAVER: Yes. I guess it just seems to me to be too extreme. Doubling that number means you really are up to 25 to 30 percent for fission gas

release for the gap release from this LOCA. I just don't believe it is that high.

MR. BOYACK: So you would certainly support more data? You would be one who would say, "See needs."

MR. LEAVER: Yes, I think certainly there is a need. In all these things there is certainly a need for more data pretty much. It is hard to argue with that.

MR. BOYACK: But, specifically, the relationship, the difference between the RIA approach to fuel testing and the more representative France?

MR. LEAVER: Right, right.

MR. SCHAPEROW: Can you put a range in there for now and go on, say 5 to 10?

MR. LEAVER: Yes, what's the process for resolving like where there's differences?

MR. BOYACK: I was just going to ask that, and I didn't know the answer. But, of course, the one thing that somebody like myself often tries is they say, oh, since Jim was on that end, would anybody feel, could they live with 7.5 percent or would that still be of great concern to you? If it is, then I think we would just put in a range.

MR. GIESEKE: You're asking me if I --

MR. BOYACK: I am asking all of you, 7.5 percent?

MR. GIESEKE: I did like a real quick estimate. If a third of the core is at 15 and the rest of the core is at 5, you get about 8. So that was why I said 5 to 10, 10 being a little high. That was my judgment to assume. You know, maybe the 15 for that third is too high or too low. You play with that number a little bit, but somewhere in that range I think is where we should put it, maybe mid-range. I don't know.

MR. BOYACK: Dana, your thoughts? I think the options here are a range or a value that we sort of live with, but probably lower than 10 percent.

MR. POWERS: Since the rationales weren't included here -- I mean, there's an explanation. We see a range here where you have a great deal of uncertainty about this. We have some physics we understand, some physics we don't understand. One guy guesses kind of low at 6 percent, and that is because, in no small part, I suspect, because he believes that the real gap release for low burnup fuel is more like 1 percent. I think everybody recognizes that 5 percent is a little bit high. I mean, there was some bounding nature, and nobody argued over it because it

wasn't at the time worth arguing.

So, you know, that rationale is written out. The rationale for 10 percent is written out. The rationale for 8 percent is written out, and you're selecting 8 percent, yes, I'm going to say yes because in the end you want a number here, because it doesn't do any good to have a range. Then everybody argues about what value to use within the range.

MR. GIESEKE: Take the top or the bottom.

MR. POWERS: Yes. So I guess I'm comfortable with the averaging kind of approach.

MR. KRESS: I don't like "075" because I don't think we know that many significant figures. I would prefer --

MR. POWERS: Jim said 8.

MR. KRESS: I would prefer the 8. If you look at Dana's --

MR. BOYACK: Seven?

MR. KRESS: No, 8.

If you look at Dana's chart, there's a hint of a trend versus the energetics from the fifties, and if you extrapolate that back to roughly 100 to zero energetics, you really might say that the percent of the high burnup fuel release might be like Dana, like Jim said, like 15. If you use that with

the one-third and recognize that the rest of the fuel, the 5 represents an average of 1 to something for the burnup that was in there, and do the math, you get about 8. So I would support a number around like 8.

MR. BOYACK: Let me ask a report question. There are several approaches to doing this. There seems to me some value in possibly retaining some identity to the comments if you wish. I'm not adverse to going in and just summarizing and basically make a statement that there were some who felt that the value was smaller and some larger, and we ended up with this medium value. It was important to have a single value.

MR. LEAVER: how do you decide that your number of .08 is the number? Is that like a majority vote?

MR. BOYACK: Yes, and I'm still struggling --

MR. LEAVER: I mean, you could do it that way.

MR. BOYACK: Well, we could do that, yes.

MR. LEAVER: Or you could present the information and let the NRC worry about how to figure it out. I mean, you're in the range of 5 percent to 10 percent, so it's not a huge range.

MR. GIESEKE: We're drawing numbers out of a hat.

MR. POWERS: But you would end with 7 and 8.

MR. BOYACK: Well, for instance, each one of you comes and you represent something, either an individual consultant or a constituency in some regard, which I am sensitive to. So when we were doing the high burnup fuels, we had a very large panel, and we just voted, and we left the span of votes.

MR. LEAVER: You had like 20 or 25 people.

MR. BOYACK: Yes.

MR. LEAVER: It was terrible.

(Laughter.)

MR. BOYACK: As opposed to this? Oh, absolutely. This is ideal, a small group.

Now, generally, in the past what has been possible to do is to sort of reach a general idea that, okay, this is okay with a smaller group. But, on the other hand, I'm not sure that we've had the same constituencies represented where this impacts, real impacts.

So I'm struggling here a little bit myself. Each one of these is new. It has its own

circumstances and its own dynamic.

So I'm not quite sure which way to go. Obviously, what we've got here is a situation where three people over here say, yes, I could live with .8, 8 percent. You're saying 6. That was why I was using 7.5, not significant figures, but it was halfway in between.

MR. LEAVER: But Tom is right. We don't know it to 2. We probably don't even know it to 1, but we're going to give it a good try.

(Laughter.)

MR. CLEMENT: Maybe you could add something. There is some filtering or edging between different burnups because from the few data we have, we don't have precise values, but we are certain as for RIA there's an acceleration of release, not at all a linear process.

MR. BOYACK: Yes.

MR. CLEMENT: So that's why it's difficult to have a value. Personally, I have no difficulties with a range because when we really go up to the in-vessel release, all the rest of the * should be released during this phase. I mean that. For me, putting a range is reflecting * and saying we need data.

MR. LEAVER: Yes, although this isn't necessarily our specific problem, but the buck stops at NRR, and they need to decide, to have a number which the licensees use, unless they tell the licensees to figure it out themselves and do the calculations, but there is no licensee who is going to be able to do that. It just takes too much money and time, and that ain't going to happen.

MR. BOYACK: Is it fair to say that if we had 8 percent shown as I have, that that would not be of great concern to you?

MR. EVRARD: I think we are convinced that it's higher than 5 percent, but the precise value, we have no way --

MR. BOYACK: I guess, yes, you don't know the precise value. Okay.

MR. GIESEKE: How about 7?

MR. CLEMENT: We should put 8 percent; we have no problem with that.

MR. BOYACK: Could you live with 7 then?

MR. LaVIE: I will play devil's advocate here. I by no means an expert, but I'm picking up threads here that I want to follow up and see whether or not facts are being considered.

Yesterday we had the discussion that the

gap release phase consisted of activity that had been deposited in the gap during normal operation, plus some that was released from the pellet due to the increased heating.

Now I don't know where that division lies, but the stuff that has been deposited during normal operations is at much lower energy deposition rates. The diffusion is going to be lower.

So that when we look at the reactivity insertion data, the reactivity insertion data includes the release of the gap, but it also includes release from the pellet at much higher energies.

Now how does this directly apply to our low-pressure LOCA? I'm thinking the 5 percent or the 6 percent may be good, but I'm extremely uncomfortable with the 10. It seems to me that we've gone way out the far end. Because, as Dave has pointed out, not all the fuel is at high burnup, but all the fuel is not at the same radial peaking factor for the buildup of the activity that is in the gap to be released. I haven't heard that mentioned yet. Maybe it is a "no, never mind." Maybe somebody can address that.

The other thing I was thinking here, looking at this, is that fission gas release here, is this just the gases that are of significance to

radiological releases? Does this include the stable gases? If it includes the stable gases, I don't think we should be considering it.

MR. LEAVER: Do you mean like krypton 85 by stable gases?

MR. LaVIE: No, there's some that are even more stable than krypton 85.

MR. SCHAPEROW: Actually, that's a point that I had forgotten. I believe the dominant dose contributor for the kryptons is a two-hour half-life, krypton 89.

MR. LEAVER: Krypton 88.

MR. SCHAPEROW: Krypton 88, excuse me. So that will knock this down some. I don't know --

MR. POWERS: Not on a percentage basis.

MR. SCHAPEROW: That's true.

MR. POWERS: It is similarly stable or unstable. I mean, it is just a percentage. I can look up what they actually measured. I suspect they're measuring the gamma signal or something, probably 85.

What it does not include is actually the inventory in the rod because these were reconstituted rods; the inventory of the gap, that was released when they reformulated the rod to do the experiment.

MR. KRESS: Yes, because they lost that.

MR. POWERS: Yes. So you're really looking at what is released out of the fuel itself. But that's okay. I mean, it is how you draw that curve back to the kinds of energy in there and then how do things change as you go from 50 to 75, all of which are enormously uncertain.

I think that leads to agree very much with what we said based on the EDF; it's higher. We don't know how very much. I'm comfortable, again, with the Sulinan view of, okay, we'll take the middle ground. It reflects higher. It doesn't really matter because whatever we don't get here, we're going to get in the next one. That's how I'm going to do the early in-vessel, is that I've got to get the sums correct.

MR. GIESEKE: You subtract that from 100, 95.

MR. BOYACK: Okay, Sulinan tries 7. Could you live with the 7?

We're going to go on, and then there will be some debate on the document. That's when comments ought to be addressed. Maybe I will have to pull you all together again.

MR. LEAVER: Yes, maybe the thing to do is to go on and talk about the fuel releases and then

maybe come back.

MR. BOYACK: Maybe after we go through a bunch of this, because, again, we will print out this information, give it to you tonight, take a look, and I think it is fair game to come back and spend a little time in the morning, as suggested by Jason, revisiting areas of concern.

Okay, the next thing we have to do is eat.

MR. LEAVER: Good idea.

MR. BOYACK: Did you need 15 or 20 minutes?

(Laughter.)

MR. POWERS: An hour and 20 would be just fine.

(Whereupon, the foregoing matter went off the record for lunch at 12:08 p.m.and went back on the record at 1:02 p.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(1:02 p.m.)

MR. BOYACK: Now that we've got everything done but the last two entries on -- I couldn't remember what it was -- on the last page --

MR. SCHAPEROW: Okay.

MR. BOYACK: Okay, the panel is here. We're ready to go.

Let's continue on now. We are in noble gases. We have handled the gap release. We are now going to move to early in-vessel, and the 1465 version was that by this time you were up to 95 percent releases in noble gases.

Where did I start? I ought to start with somebody new. How about Jim, all right?

MR. GIESEKE: Ninety-five percent.

MR. POWERS: So we have 102 percent of release, huh?

MR. SCHAPEROW: That's what we should do, 102.

MR. BOYACK: That's right. That's what it was. So is it 100 percent released by this time? See, it was 5 and 95. The total of those was one.

MR. KRESS: I don't know where we got 95.

MR. POWERS: Don't agree with 95?

MR. LEAVER: Where did it come from? If it came from the fact that they just made it a total of 100 --

MR. KRESS: No, no, where did the 5 percent go?

MR. BOYACK: It was over here.

MR. KRESS: Oh, okay.

MR. BOYACK: Gap release was 5 percent.

MR. KRESS: Okay.

MR. BOYACK: The early in-vessel was 95.

MR. GIESEKE: So if you make that 95, then you've got 102 percent. See that.

MR. KRESS: They just assume all the fuel failed and there was no residual fuel that kept its noble gases in it?

MR. BOYACK: So back to Jim. Are you saying --

MR. GIESEKE: I don't know that we have any data to show significant radiation.

MR. KRESS: It doesn't make any sense.

MR. BOYACK: Is the given source term applicable, meaning by the end of the early in-vessel phase --

MR. GIESEKE: Yes.

MR. KRESS: Yes.

MR. BOYACK: Okay.

MR. KRESS: So we just make the math add up right.

MR. GIESEKE: If you want to leave it 102, that's okay, too. That will add a little spice to somebody's life.

MR. BOYACK: Okay, I'll change that in a moment here. Tom?

MR. KRESS: I'll agree.

MR. BOYACK: Dana?

MR. POWERS: You are going to force me to be the outlier here, aren't you?

MR. KRESS: You're like me; you don't really think release on that --

MR. POWERS: The problem is this: that we see now that only a fraction of the core is going to be involved at this stage in the accident. This is a large -- I am much more comfortable going along with the summation if we are looking at the range of accidents. But when you tell me I have to look at this large break LOCA specifically, then I want to look at what fraction. By my back-of-the-envelope digestion of what they have, when applied to the large break LOCA, I come up with about 65 to 70 percent of the core being involved at this stage of the accident.

I am willing to concede that most of the core has gone through gap release. I am not willing to concede that most of the core goes through the early in-vessel release. So at this point my predilection would be to come along and say, okay, the early in-vessel release is about 62 percent for the noble gases. What it takes to get to 100, I stick the remainder out into the late in-vessel release.

MR. BOYACK: Did you say 62 percent?

MR. POWERS: Yes.

MR. GIESEKE: Not 63 now.

MR. POWERS: That's right.

MR. BOYACK: I thought you did.

MR. POWERS: That's right. Now I'm exact. I am just making the numbers come out. I say it is about 70 percent of the core, but I have released some from the gap release, and so it's about 62 percent here.

MR. BOYACK: I hate to write this down.

(Laughter.)

MR. KRESS: Let's make it a round number, 70 percent.

MR. POWERS: What he was saying was not worth hearing.

(Laughter.)

He was just picking on me.

MR. KRESS: Put a little note: Kress picks on Powers.

(Laughter.)

MR. LEAVER: And then follow that with another note: So what's new?

(Laughter.)

MR. BOYACK: Bernard?

MR. CLEMENT: Okay. On the French side, we considered 100 percent at the end of the early in-vessel release. I said to Jean-Michel yesterday we should not be below.

MR. POWERS: Yes, it is going to be tough to get above that. It will take a very severe criticality event to get it above that.

MR. EVRARD: No additional comments.

MR. BOYACK: Okay, David?

MR. LEAVER: I thought clearly, in my mind anyway, that 100 percent is conservative, but the number could be 85, 90 percent in the extreme. So I think it is not unreasonable to round it up to 100.

MR. SCHAPEROW: Dana, I think that's what was done when we did NUREG 1465. Looking at the expert elicitation, each expert gave distributions and they were all below 100, other than the most

conservative point.

MR. KRESS: So you're going to take 562 and divide it by 6?

MR. BOYACK: So we have a range from 62 to what now for the total released by the end of this phase, from 62 to what would be 100 percent, right?

MR. POWERS: Well, it would be 93, up there, right.

MR. BOYACK: Yes, I have to subtract 7 from 62.

MR. POWERS: No, 62 is the number; .62 is the number to put up there properly.

MR. BOYACK: Oh, okay.

MR. POWERS: If you want to reflect most of the members of the panel, you put down 3.

MR. BOYACK: Right.

MR. POWERS: Now they're wrong, but I can't help that.

(Laughter.)

MR. LEAVER: But it would be the first time.

MR. GIESEKE: That what, we're wrong?

MR. LEAVER: Uh-hum.

MR. GIESEKE: I'm not admitting to ever having been wrong.

MR. LEAVER: Oh, you're not admitting. I'm sorry.

(Laughter.)

MR. KRESS: Dana was wrong once, I remember distinctively.

MR. LEAVER: Really? When he said you were wrong?

MR. KRESS: No, it was that time he thought he was wrong, but later changed his mind and decided he was right.

(Laughter.)

MR. POWERS: I was wrong twice that day. I was wrong and then I found out I was wrong about that.

(Laughter.)

MR. BOYACK: You know, the reason that David Leaver did not get through the entire two days of transcripts the last time is because of this, this nonsense.

(Laughter.)

MR. POWERS: This is the part that helped him, entertained him, right?

MR. KRESS: It kept him awake.

MR. BOYACK: There were one or two times that I laughed when I read it.

Okay, so, basically, unless Dana really somehow --

MR. POWERS: Has an epiphany.

MR. BOYACK: Well, you have an opinion; I know that.

MR. LEAVER: No, he said an "epiphany."

MR. BOYACK: An epiphany? Oh.

(Laughter.

Well, I'm sorry, I didn't hear it so well. It must be the projector.

Okay, well, with that in mind, I am going to put the 93 percent because I will do it unless somebody tells me otherwise.

MR. POWERS: Is this where I get to stomp up and walk out of the room?

MR. KRESS: Yes.

(Laughter.)

MR. BOYACK: Don't give him that option.

MR. SCHAPEROW: I would leave it the way it was --

MR. BOYACK: Because, eventually, the reason I went to this is because now I can always see the heading that goes with the information, whereas otherwise it just gets cluttered. So it's simplicity for one of us.

Okay, ex-vessel, now we're zero and zero, aren't we?

MR. LEAVER: Yes, we're done.

MR. POWERS: No, it is 31 percent out at the late in-vessel.

MR. BOYACK: Okay, so --

MR. GIESEKE: And then average that, divide it by 6 and get an average.

(Laughter.)

MR. BOYACK: So what I can put down here -- no. So you had 62 and instead of 69 -- you had 31 percent.

MR. POWERS: That's what I said, 31 percent in late in-vessel release. Because, if you recall, timing-wise they overlap with the ex-vessel release.

MR. BOYACK: So this is one way, Dave, of sort of dealing with this.

MR. LEAVER: Sure.

MR. BOYACK: This is the only reason Dana did this, was just to illustrate a way that we could have different opinions.

MR. POWERS: Yes, Brent asked me to do this beforehand.

MR. LEAVER: You guys cooked this up at

lunch?

MR. POWERS: Yes, yes.

MR. KRESS: Actually, truth be known, I agree with Dana. I just thought it was probably a "no, never mind."

MR. LEAVER: Oh, I think he's more right than we are.

MR. KRESS: Yes. If we want to really have a vote --

MR. LEAVER: But, I mean, if we're going to change the .93, we would be, I guess, saying 1465 was wrong or too conservative.

MR. KRESS: Or that it didn't make any difference probably.

MR. LEAVER: Well, it would make some difference for the design basis accident.

MR. KRESS: I don't know. Equivalent qualification or anything --

MR. LEAVER: Probably not much difference for EQ because, if it doesn't come out early in-vessel, it will come out in the later phases, all of which is 12, 16 hours. The EQ is like a 100-day --

MR. KRESS: It might impact on meeting 10 CFR 100 doses at the --

MR. LEAVER: Yes, it would impact the

design basis, doses, yes.

MR. KRESS: But that is governed mostly by iodine, isn't it?

MR. LEAVER: It is, but you would see, one of the things that turns out is, if your mitigation systems are effective or if you do a careful job of evaluating aerosol deposition, for example, about all you're left with, at least for the 30-day doses, control room and the LPZ, is noble gases and organic iodine.

MR. KRESS: Well, maybe we ought to do it the way we really think. I really think about 30 percent is late in-vessel and the rest of it is gap release and early in-vessel. Dana, you said what was your number?

MR. POWERS: I have between 65 and 70 percent of the core involved in this phase, this early phase of the accident here.

MR. KRESS: It's at least 30 percent roughly.

MR. POWERS: Roughly 30 percent. It comes in, the late in-vessel release --

MR. KRESS: Yes, I would definitely support that. It is what I believe.

MR. LEAVER: Interestingly, these

calculations were the basis for 1465. These were in NUREG CR 5747. You have mean values of noble gas release for BWR's - or for PWR's of 80 percent for this sequence.

MR. KRESS: The mean?

MR. LEAVER: Yes, they do a variety of sequences, but you can imagine a sequence in which you kind of keep the core intact as long as possible and it would toast things and you can get higher --

MR. KRESS: Get higher releases.

MR. LEAVER: -- releases. So their number is a little higher than what Dana is suggesting, but not a lot. I mean, yours is about 7 percent full. So they're saying 80 percent.

MR. BOYACK: Okay, I have now heard two people with the Dana Powers approach. So, Jim, do you want to stay where you are or do you want to respond to the argument?

MR. GIESEKE: Well, I think it is a good argument. I can back off a 100 percent. I think we all agree that not all the core is going to be involved to that extent at that point in time. I don't now if we have any good sense of what that number should be. It's a big number, but it's not 100 percent, maybe 80 percent.

MR. LEAVER: Well, we had a calculation, for what it's worth.

MR. GIESEKE: Yes, that 80 percent.

MR. LEAVER: Yes, 80 percent.

MR. GIESEKE: And we have this here.

MR. BOYACK: David, where do you end up on this?

MR. LEAVER: I guess I would ask a question of the panel or possibly the NRC. Are we -- and I think maybe I know the answer -- but the delta approach would say we would just leave this at a total of 100 percent because it can't get any worse than that.

Then the other approach is to say, well, where we don't like what 1465, the original 1465 did, we're going to suggest a different number, and that's kind of what we are doing here.

MR. KRESS: It has nothing to do with burnup.

MR. GIESEKE: Yes, it has nothing to do with burnup.

MR. LEAVER: It has nothing to do with burnup.

MR. GIESEKE: It has nothing to do with burnup, but the implication is that what we're doing

is the result of burnup.

MR. LEAVER: But I haven't heard anybody say that we shouldn't try to be more correct, if we think there is something wrong with 1465. Up to this point, I haven't heard anybody say that. So I would say probably that's -- I would go along with a number more like 70 or 80 percent.

We're kind of stepping outside of our objective, I guess.

MR. BOYACK: I don't think we are.

MR. SCHAPEROW: No, I don't think so.

MR. LEAVER: You don't think so?

MR. SCHAPEROW: I think just about anything we've learned in the last 10 years is game as long as it is applicable.

MR. KRESS: We were supposed to ask, is the given source term apt, and in this case we would say no. Can we specify a new value? Yes. So we haven't followed that chart up there.

MR. BOYACK: Our French colleagues basically told us what they had done. You can see the way this is trending, which would be that we would put 62 percent.

MR. GIESEKE: I think 62.5 percent.

MR. BOYACK: No, not 62.5.

What I really wanted to ask was, because I understand what you've done, but is there any strong disagreement or facts you would like to bring to bear on this change?

MR. CLEMENT: What we've done does not make any difference from a late in-vessel release and early in-vessel release. So at the end we've got the 100 percent.

MR. BOYACK: Okay, right.

MR. CLEMENT: That was what was originally --

MR. BOYACK: Right, that would be in agreement with 1465, yes.

MR. KRESS: Why not 63? Put the 63 there, so it makes the other one 30.

MR. CLEMENT: I have a question for Dana. When you say 65 to 70 percent of the core is involved, but there is some release from the part of the core that is not involved.

MR. POWERS: Yes.

MR. CLEMENT: So it's not right just to --

MR. POWERS: Yes, the two numbers are not quite consistent with each other.

MR. CLEMENT: Yes.

MR. POWERS: I would say it would be 60 to

70 percent, and that will make it consistent.

MR. CLEMENT: Yes.

MR. POWERS: Yes, you're right. You're right.

MR. BOYACK: Is there something I ought to be doing up here?

MR. POWERS: Yes, change it to 60 to 70 percent. Bernard's caught me on a consistency problem. That will get it back to --

MR. BOYACK: Okay, now just I need some statement regarding the ex-vessel release phase. Let's see, this was now 63, and this was 30, but I'm just going to put both of these on the same sheet.

But what was the statement about ex-vessel and there being no releases there?

MR. KRESS: We just don't think there is any down there.

MR. POWERS: If there is any noble gas in that fuel when it's molten, dumped out on the floor, and hits either water or concrete, it is gone so quickly, it's indistinguishable for having released it in the fuel.

MR. LEAVER: Yes, it probably wouldn't be much.

MR. POWERS: No, it is not going to be

much.

MR. SCHAPEROW: How about no remaining noble gas.

MR. POWERS: In molten fuel that is expelled from a reactor coolant system.

MR. LEAVER: I mean, you might have some chunks of unmelted fuel.

MR. POWERS: Yes, you could, but not much. When you blow steam or concrete, composition gases through it, it spreads it out so quickly.

MR. EVRARD: The only problem I have, if you increase the late in-vessel release, I mean, it's not too long, but this will be done quite early in the late in-vessel release.

MR. POWERS: Yes, right.

MR. EVRARD: The situation we have, we have put the situation for revaporization. So if we put the logic on here, we have to be consistent in saying it will be released at the beginning of the late in-vessel release.

MR. LEAVER: That's a good point.

MR. POWERS: You could probably do that with a footnote or something.

MR. LEAVER: It's a good point.

MR. CLEMENT: It's the continuity of the

early in-vessel, early impact. So it continues.

MR. POWERS: Right.

MR. BOYACK: Is that what I heard, that basically this remaining release would occur very early in this phase?

MR. POWERS: I would be heavily biased toward the early phase. You might actually get the last of the 30 percent out at the end of the late in-vessel, but it would be a bunch at the beginning and then kind of tail-off. So the limit of this resolution, it is in the early part of it.

I think just the way Bernard said it is correct.

MR. BOYACK: Is that what I wrote?

MR. POWERS: Yes, I think you got it.

MR. BOYACK: All right.

MR. KRESS: The late in-vessel was 10 hours?

MR. BOYACK: Yes.

MR. KRESS: I agree, it's not going to take 10 hours.

MR. LEAVER: But it might for the last fuel rod

MR. POWERS: The last fuel rod in the most peripheral location up or next to a downed cooler or

something like that, that one might actually release it at the end of the 10 years, but by far most of it is going to be up in the front end of it.

MR. BOYACK: Okay, Dave, do you have any additional comments?

MR. LEAVER: the only comment I would make is I would just offer this to think about: That number .63 sounds like we know what it is, and of course we don't.

(Laughter.)

And we have a calculation that was done as part of and in support of the original 1465 that has a -- well, there's several different numbers, but .8 is kind of, I guess, looks like the mean number. I am assuming this .8 is the total of what we say, the .07. Then if we made that number .73, we would have a number that is, in fact, the same as what they used as the basis for 1465, even though they then increased it to 100 percent. We are not sure why they did that.

So we might be on a little bit more defensible ground if we said, well, we see no reason to use a number different than what was in the NUREG CR 5747. You use a lower number.

MR. KRESS: They used MELCOR to get those numbers, and it notoriously overestimates the noble

gases.

MR. LEAVER: I don't now if that's right. Did it use MELCOR for these numbers, the NUREG 5747? Where do these numbers come from?

MR. NOURBAKHSH: They are all expert elicitation.

MR. KRESS: Not MELCOR, but CORSOR.

MR. LEAVER: Expert elicitation.

MR. KRESS: They got the original numbers, though, and guided the experts out of CORSOR, and CORSOR notoriously overestimates noble gases.

MR. BOYACK: So would you help me with what comment you would like me to enter here? Sometimes the conversation gets long enough that I don't know quite what to write.

MR. SCHAPEROW: I think it is probably worth noting, just in case we ever come back and revisit this, that NUREG CR 5747, which was referenced in 1465, is the basis for a lot of these numbers. Now I am told that the numbers from 5747 were from the expert elicitation. I didn't realize that.

But it would give you a number, total noble gas release for the gap plus early in-vessel of .8. So we're suggesting a number that's about 10 percent less than that. I guess maybe one basis would

be what Tom is saying, that we believe that the expert elicitation was at least in part based on CORSOR, which people believe has overestimated the noble gas release.

MR. SCHAPEROW: Yes, I think it is 83 and 80.

MR. LEAVER: Yes, I guess there were two, a high zirc case and a low zirc case. TMI is about 55 percent, just for perspective.

MR. POWERS: But that's a high pressure sequence.

MR. LEAVER: Yes, I think they had some high pressure numbers in here. I'm not sure I can figure out which ones are which, but, yes, that's right.

What was the RCS, down to about 600 pounds TMI?

MR. POWERS: It got down to the point that the accumulators were starting to dump, but they were still closing --

MR. LEAVER: Yes, so it's probably about 600 or 700 pounds?

MR. POWERS: That's 625 to 650, something around in there.

MR. LEAVER: Yes.

MR. POWERS: It was only there for a little bit of time because they kept bumping the main coolant pumps and things like.

MR. BOYACK: Any comments, Jim? Any different comments? You're allowed to say, "okay" to the above.

MR. GIESEKE: Yes, it's okay. I think we're pretty consistent throughout.

MR. BOYACK: Essentially, what we have done here is we have said, okay, we'll leave the values the same, but we have noted that you could make a consistency argument for NUREG 5747. Is that NUREG CR?

MR. LEAVER: Yes, it's a CR, right.

MR. BOYACK: 5747, which would go to 80 percent through the early in-vessel phase, in which case this would be 20 in the late in-vessel, but we haven't changed the numbers.

MR. GIESEKE: It's good.

MR. BOYACK: Okay.

MR. LEAVER: The other thing we might just note is in 1962 they issued TID 14844, which became the basis for licensing every single operating plant except those that have since gone to the alternate source term, and it was 100 percent. It doesn't make

it right, but that's what was done.

MR. BOYACK: II was going to say, where did that file go?

MR. POWERS: I think Boyack has been doing his homework. Look at this. He's got all of these files all set up. He's earning his money, Jason.

MR. SCHAPEROW: He was on vacation last week while I did this, up in Utah or something.

(Laughter.)

MR. POWERS: You know, these guys from Los Alamos, they really don't have lives. They live up there on the top of the mesa.

MR. KRESS: Nothing to do up there.

(Laughter.)

MR. BOYACK: Are they related?

MR. SCHAPEROW: With respect to releases from the core they are, but now we've got deposition coming in.

MR. BOYACK: If they're not related, I would rather not do it because then you run out of space here on the screen.

MR. SCHAPEROW: I was suggesting that we add a line right above "halogens" for the new noble gas numbers, because that is reflective more of how much of the core is given up there. They're volatile

fission products. This is it on the additional element of the deposition. I don't know if that would be of any use to the panel.

MR. POWERS: Well, I actually did it the other way around. I said, if I'm going to get 35 percent of the halogens out, how much of the core must have deposited, given that it is a high burning sequence, and I said, well, you aren't going to deposit more than about half in a large break sequence. So I said, well, about 65, 70 percent, something like that, and then I was corrected to 60 to 70 percent. I wasn't going to argue over that.

MR. LEAVER: Well, you're right, we've got these tables, which actually are not in 1465, but are in this NUREG CR 5747. It assumed a retention of not quite a factor of two, I guess a transmission of .55, which would be a retention of .45, and it had a release of iodine of about .7. So you take .7 times .55 and you get about .4. That's where the number came from.

MR. POWERS: I mean, I just worked it the other way around --

MR. LEAVER: Yes, sure.

MR. POWERS: -- to come up with how much core was creating.

MR. LEAVER: Right.

MR. POWERS: I did it in my head, so I'm not quite exact with those numbers.

MR. BOYACK: I was actually looking for the slide that had the PWR releases from NUREG 1465. I just would have marked on the revised values and put it up on the screen.

MR. LEAVER: Well, here, take this copy and I'll show you where it is. The table, you mean?

MR. BOYACK: Well, actually, no, I'm after the transparency. It's gone someplace and I don't know where it is. It disappeared.

Okay, so with that, the reason you're on the panel is you all have fantastic minds and memories. So let's go on --

MR. POWERS: We all have CRS, as Tom Kress will assure you.

MR. KRESS: That's right, CRS.

MR. BOYACK: Okay, gap release.

MR. KRESS: He isn't going to ask. He knows better than to ask.

(Laughter.)

MR. BOYACK: Okay, what is "CRS"?

MR. POWERS: Can't remember stuff.

(Laughter.)

MR. LEAVER: I've got that disease, too.

MR. POWERS: I vulgarized it from the transcript.

MR. BOYACK: Okay, we're on halogens, gap release. Tom, it's time for you to go back to work here.

MR. KRESS: We're on the gap release part of halogens?

MR. BOYACK: Sure, unless you want to just give me all four numbers.

MR. KRESS: I was going to give you all four numbers.

MR. BOYACK: I'll just write down "TK" on each one then.

MR. KRESS: I think the gap release is the same. I think the early in-vessel release would be about 70 percent, if you didn't count deposition. And if you counted deposition, it is going to be about 40 percent. So, let's see, that gives me 75.

Ex-vessel, I think the late in-vessel is probably -- I don't have any reason to change it. I want to keep it and the remainder is excess.

MR. BOYACK: Okay, so that says 5, and this was 75, 85; this is 15 percent.

MR. KRESS: Now the early in-vessel comes

out to be 40.

MR. BOYACK: Oh, I'm sorry.

MR. KRESS: Yes.

MR. POWERS: I mean, you're revaporizing it, that 30 percent that you deposited, I mean some fraction of it. So you've got to make a revaporization argument here.

MR. KRESS: Yes, but -- no, no, I think I would put the full 70.

MR. BOYACK: Was that 50? I think it was 75.

Shall we let somebody else weigh-in on this? You can do just one, if you want, Jim, but otherwise if you want to look at it in total, that's fine, too.

MR. LEAVER: Can I just ask Tom a question? Is this a release from the fuel or into container?

MR. KRESS: The 70 percent I meant to be released from the fuel.

MR. LEAVER: Yes, that's what I thought.

MR. KRESS: I'm trying to figure out what -- I meant that to come out of the fuel --

MR. LEAVER: Right.

MR. KRESS: -- in addition to the gap. I

have no reason to believe the amount that gets deposited in the RCS is any different than it was before. I'm trying to figure out -- they've got a late in-vessel of .1, which means they think 40 percent or they think only 1 percent got trapped and all got released.

MR. POWERS: I assumed that they assumed that, the way they went, they said about half of whatever was released in-vessel got trapped in the piping system.

MR. KRESS: Forever?

MR. LEAVER: No, no, not forever.

MR. POWERS: It just got trapped there, and about a third of that subsequently revaporized, to give them the late in-vessel release.

MR. KRESS: Okay, so they started out with the total release of the fuel of .45?

MR. POWERS: No, the total release from the fuel, the fuel itself, I suspect they were looking at a number, if Dave is correct, it's 80 percent.

MR. KRESS: What's not in that total up there was left on the surfaces.

MR. POWERS: That's right.

MR. KRESS: Okay. I don't have any reason to change those fractions except I think that the

total it gets out of the fuel is about 70, and they had about half of it, do you think? So I would still say it still becomes 35 because that's about half of 70.

MR. SCHAPEROW: I did make a copy of the deposition that was done on 1150, if anybody's interested. It's for each group on one page. I'll pass it around.

MR. KRESS: So I didn't change anything, even though I thought I was going to.

MR. BOYACK: So, in effect, when we came down the logic diagram, you said, is the given source term applicable, and you just said yes.

MR. KRESS: Yes.

MR. BOYACK: And so you told me yes.

MR. KRESS: I thoroughly agree we get more iodine released from the higher burnup fuel. This tells you you would get more released. Mentally, it tells me that I get some fraction of higher release from the fuel than you would have previously, because you've got higher burnup fuel in there, and that tends to give you more release.

Except I thoroughly believe that they overestimated the iodine release in the 1465. So if I correct the 1465 for the overestimate and then add

the increment that I get from the high burnup fuel, it turns out to be the same, and that's why I was surprised, that I got the same thing. I thought I was going to adjust it, but I'm ending up at the same place that they were.

The reason that I think they overestimated it is once again because CORSOR overestimates the release.

MR. POWERS: I think that's where I am, I guess. I think; therefore, I am, right?

(Laughter.)

MR. BOYACK: Okay, now was that just the gap release phase that you just described or was that --

MR. KRESS: No, that was the early in-vessel part. It was the early in-vessel part that I would say CORSOR overestimates. I correct for it and then add in a correction for the high burnup; I end up at the same place.

MR. BOYACK: Okay, and the gap release, you just feel like it is the same?

MR. KRESS: There is a burnup correction in ANS 5.4 which contains a little higher, but I think the .05 is already kind of a higher estimate than it should have been. So I'm going to leave it where it

is. I don't have any reason to change it.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: My sense was that the gap release should ease up a little higher, to a little higher number. I don't know how far to take it. It's not a lot. It's just a nominal increase perhaps, .6. No, that's a little bit -- that's not enough to worry about maybe. That's within the uncertainty. Perhaps we could just leave it the same.

I followed as much the same logic as Tom on the others. I wanted to bump the releases. Particularly the in-vessel release was above a good 1465 number, I mean NUREG 1465 number, but if the sense is that those are too high, and I don't know how much too high because with CORSOR it is kind of hard to judge. So it gets to be maybe to the best of our knowledge a wash, like Tom is calling it. I could go along with that. I could accept that.

MR. BOYACK: So your logic was basically --

MR. GIESEKE: Yes, basically, the same as Tom's. I wanted to raise them, but if they're already overestimates, then you have to make that correction back down. Raise them for the effect of burnup, but then back them down to the previous overestimation.

MR. BOYACK: What about the logic, either Tom or yourself, for ex-vessel, which has stayed the same also? I guess by the time you get to the last one there's no -- it's a big system, but --

MR. POWERS: No, that's not the case now.

MR. KRESS: The ex-vessel just says whatever didn't get released from the fuel in-vessel is going to get released ex-vessel.

MR. BOYACK: Oh.

MR. KRESS: It's the late in-vessel that's questionable.

MR. GIESEKE: That's right.

MR. KRESS: Because it's a resuspension revaporization thing, and all I did is say I don't know how to correct it for what they did before, although it very well could be different.

I think that is the same.

MR. BOYACK: Pardon me?

MR. KRESS: I think that one is the same. I don't think that one changed. It's the late in-vessel that I'm saying.

MR. BOYACK: Oh, that's why you're keeping me straight here.

MR. KRESS: I have no reason to change it because I don't know how to change it. It very well

could be different.

You can zap the ex-vessel part of that. You can zap the ex-vessel part of that sentence.

MR. BOYACK: Okay. All right, Jim, anything else you want to add on either of these?

MR. GIESEKE: Ex-vessel, I would hold the same -- oh, you're down on late in-vessel?

MR. BOYACK: Well, no.

MR. GIESEKE: I would take that as the same logic as Tom; whatever was left there goes out.

The other one --

MR. BOYACK: Well, I thought that related to the late in-vessel. I don't think I had logic for ex-vessel, but I --

MR. KRESS: The ex-vessel is, whatever didn't get released in-vessel will get released ex-vessel. That's the logic there.

MR. GIESEKE: That's right.

MR. KRESS: The logic for the late in-vessel is different.

MR. GIESEKE: Yes.

MR. BOYACK: Now is this correct?

MR. KRESS: No, that's wrong. It is a mixture of the two.

MR. BOYACK: So help me get it right.

MR. KRESS: What it is is I have no basis to change it, although I think it could be different.

MR. BOYACK: Okay.

MR. KRESS: Yes, that's the one.

MR. GIESEKE: No, no, no, no, I'm not done with --

MR. BOYACK: Ah, good.

MR. GIESEKE: We're just getting down -- the logic basically for 1465 is that roughly, what we're saying, a third of what was deposited is released. I think it is higher than that because I believe the models that are used in that don't take into account the discontinuities or the irregularities in the depositions or the deposits. It would tend to assume that they were more uniform across surfaces, where, in fact, they are not going to be that uniform, which I think will lead to higher releases, but I don't know how much.

Again, I could take up. Instead of saying a third, I could say a half, which would raise that number maybe not enough to bother with, but it could take it up to maybe late in-vessel to be 17 percent. Pick a number.

MR. SCHAPEROW: And this is based on the idea that they are self-heating, the fission products?

Because Dana mentioned a couple of different mechanisms, and it might help us in the end to see which mechanisms are referred to. It's a little confusing to me.

MR. BOYACK: So it's self-heating --

MR. SCHAPEROW: Self-heating of fission deposits, deposited fission products.

MR. GIESEKE: I think that's underestimated because the deposits are not uniform.

MR. SCHAPEROW: Oh, that's right, because the deposits are not uniform.

MR. BOYACK: So let's see --

MR. GIESEKE: I think we are going to have hotspots, is what I'm saying, more than calculated.

MR. BOYACK: Dana?

MR. POWERS: I believe that we can leave the gap release at 5 percent because it is basically a vaporization phenomena into a gas that is dominated by the fill gas. Since the temperatures and timing are about the same, and the fill gas is about the same, it is going to be about the same.

MR. BOYACK: I didn't get all that.

MR. POWERS: Vaporization phenomenon into the fill gas which didn't change.

MR. BOYACK: Good, thanks.

MR. POWERS: I believe that the roughly 60 to 70 percent of the core degradation is a strong function of the core degradation, and I'm not willing to change that. I acknowledge that we need to experimentally investigate core degradation with high burnup fuel, which might be different. But at this point I am not going to change and I am going to stick with the roughly 50 percent deposition in the piping system, so the number doesn't change.

I want to emphasize that, to do this right, you've got to understand how high burnup fuel degrades.

MR. BOYACK: And that's experiment?

MR. POWERS: Yes. You need to understand, experimentally investigate it.

MR. KRESS: It is a tough experiment to do because I think you have to take it to its burnup in situ.

MR. POWERS: I'm not that big of a bug on that. I'll take it any way --

MR. KRESS: You have to be very careful with transporting it, moving it.

MR. POWERS: I think you have to --

MR. KRESS: I don't think you can go pick up old fuel that has been sitting around 20 years that

has that burnup and bring it over and do the test.

MR. POWERS: I think you've got some challenges ahead of you in extrapolating from the test to the reactor accident, but I'll take anything I can get right now.

Okay, in the ex-vessel release we have dumped the degradation of fuel that didn't get it in the early in-vessel phase, and so I am going to leave that alone because that is eventually how the fuel goes.

MR. BOYACK: Does it conserve the fuel degradation and early in-vessel phase? Is that what you said?

MR. POWERS: No, it's --

MR. BOYACK: No change because?

MR. POWERS: Because that's just the degradation of the fuel that was left over. It's a mass balance thing.

MR. BOYACK: Okay.

MR. POWERS: Okay, the late in-vessel, I'm going to go along with Jim, but I'm going to elaborate on the reasons. He believes in hotspots and what-not. I also think that, with respect to the halogens, they're sensitive to the oxygen pressure. So if you have air circulating through this piping system, we're

going to jack up the releases.

On the iodine, I note that if we look carefully at some of the recent PHEBUS results that we had seen evidence of revaporization occurring after they shut off the source, even though they are only circulating steam at that point, so I think air will have a more profound effect.

I had independently arrived at a number of like 20 percent of the iodine inventory. I don't know that that's conceptually different than Jim's 17 percent.

MR. KRESS: I would prefer to use 20.

MR. GIESEKE: I like 20, too. That's a round number, you know.

MR. POWERS: And I would again say that I anticipate that in the post-test analyses that are going on as part of the PHEBUS program, we are very likely to get some more information on whether air will cause revaporization or not.

Better said that way than the way I said it.

MR. BOYACK: You know, I could shift, instead of putting you first, we could put Jean. Okay, all right.

MR. CLEMENT: Before coming to numbers, I

would like to say some words about how we've done this same job. First of all, for the sequences that are considered, there are large break LOCA, hot leg break, okay? Given what we have seen on the different experiments, in the case of a hot leg break, we consider that we could neglect deposits of iodine in the RCS. So that's the first point.

Then, in fact, in our numbers we have put 100 percent release at the break for all the phases. That means we have considered that we have zero release from ex-vessel. We think it is released during the gap release or from a short release.

So I would say we do not like the distinction that's spoken before, for noble gases, for the part of the fuel that is 60 to 70 percent involved in the degradation in the late in-vessel release, and the remainder that are in the -- the remainder is in the late in-vessel release, and it is 60 to 70 percent involved is in the early in-vessel release. So, thus, we are not to make the difference, and we should like to be consistent with what was done for another guesses.

So concerning gap releases, I think we are a little bit lower than the 5 percent.

MR. EVRARD: That's not including --

MR. CLEMENT: Not including the high burnup. So for high burnup, it could be increased, but lower than noble gases. Maybe you can keep the 5 percent. So 5 percent. Zero for ex-vessel. In-vessel, okay, well, 95 percent is split between the early in-vessel and early phase of late in-vessel, okay, because that's a degradation.

MR. POWERS: The way you can get that early phase of the in-vessel is actually to put it in the ex-vessel because that is only a two-hour period that overlaps with the late in-vessel. So if you want to jam it together, if two hours is good enough for you, put it actually in the ex-vessel. It's not a clearing ex-vessel. Its timing is such that it's overlapping.

MR. CLEMENT: Yes, it is overlapping.

MR. GIESEKE: Is that misleading, though, to the reader of this document?

MR. POWERS: Well, he just has to recognize it.

MR. BOYACK: Well, the reader probably won't recognize it.

MR. GIESEKE: I think either way, whether you put it in the late in-vessel and say it is early or whether you put it in an ex-vessel and say where

it's coming from, either way, comment has to be made --

MR. POWERS: Either way, yes.

MR. GIESEKE: -- to make it clear to the reader.

MR. POWERS: My 25 percent from the ex-vessel is the same as whatever percentage he gives us here, and he's right, what we did for the gap releases, we put it in the late in-vessel and said it was biased toward the front end. It's the same story here. I put it in the ex-vessel because it all put it in a two-hour period.

MR. CLEMENT: Yes, okay.

MR. POWERS: I mean, that's one way to do it. But somebody has to understand that that's what you are doing.

MR. EVRARD: Of course, it could be late in-vessel.

MR. POWERS: It is actually a late in-vessel release, but I want it on the front end of that.

MR. EVRARD: Oh, okay.

MR. POWERS: So I put it in the ex-vessel because those two actually overlap each other in time. I mean, the way you do it is, while late in-vessel is

occurring, you also have ex-vessel occurring for two hours.

MR. CLEMENT: But late in-vessel during the time period of the ex-vessel?

MR. POWERS: Ex-vessel, that's right.

MR. CLEMENT: Okay.

MR. POWERS: Yes.

MR. BOYACK: Do I need to just put a qualifying statement down here under late in-vessel then for you? Because what I basically heard you say is that this is --

MR. POWERS: Subsequently, in my late in-vessel release, that's really the revaporization off the piping system, and it does occur over a 10-hour period, and involves interactions and all kinds of things. It is just a way of getting the timing approximately correct on this.

MR. BOYACK: All right, we're early in-vessel on your comments, and I am a little confused, but --

MR. CLEMENT: In our approach we have about 5 percent for the gap, and now we have 95 percent to share in between early in-vessel and the early phase of the late in-vessel. Okay, so this work we have not done during our reassessments because we

have merged these two. So I don't know.

MR. BOYACK: Now let me get this right.

MR. CLEMENT: So maybe I don't want to give a definitive number. Maybe it could be 17, the early in-vessel, and 25 in the early phase of the late ex-vessel, or something like that.

MR. GIESEKE: This late in-vessel source, however, is not previously-deposited material. That's zero.

MR. CLEMENT: Well, no, a negligible amount of deposited material.

MR. GIESEKE: Yes. So his late in-vessel that the others of us are attributing to revaporization from surfaces, he is saying is zero because he is taking no account of deposit of the early in-vessel material. So when you fill in early in-vessel up there, you have to note that it is so much released from the fuel with zero deposition or zero retention during that phase.

MR. BOYACK: Okay, I just need a little wording in this. So I start out saying 95 percent remaining for these phases, and there's three phases. It's divided between -- and then I heard talk about two phases, but there's three remaining. So I got confused.

MR. CLEMENT: So with ex-vessel, zero, because consider that everything is released from fuel.

MR. BOYACK: Oh, okay.

MR. CLEMENT: Okay, so you put ex-vessel as zero.

MR. BOYACK: Got it.

MR. CLEMENT: Then the 95 percent, you have to share in between the early in-vessel and the last phase, the beginning of the last phase.

MR. BOYACK: Okay, so now I've got it. That was confusing to me for a moment.

MR. CLEMENT: But we do not make this distinction because for us it was rather artificial because it is a continuous process.

MR. LEAVER: Sure.

MR. CLEMENT: So it is just to enter within your tables. It is difficult to enter our numbers within your tables because --

MR. BOYACK: Right.

MR. CLEMENT: -- of the distinctions, some are the same, different phases.

MR. SCHAPEROW: You don't feel that there will be deposition of iodine inside the reactor coolant system?

MR. CLEMENT: If we consider a hot leg break, all that we see as experimental evidence is, I mean, vapor for iodine with maybe a few depositions. As we want to be getting the same difficult term, "reasonably", we could have been one or two persons, but it doesn't matter, the one or two persons.

MR. EVRARD: I think the main part of the deposit will accumulate *.

MR. LEAVER: It depends on the sequence.

MR. CLEMENT: Yes, but the most important deposits occur where the vapor is condensing, while it is still vapor and there is not any suction --

MR. LEAVER: But it won't be vapor up in the upper --

MR. CLEMENT: As I said before, we have made the hypothesis in order to able to consider a hot leg break and iodine emitted as a vapor into the containers. That's why we do not consider any deposits.

MR. LEAVER: Because you wanted iodine emitted as a vapor into containers?

MR. CLEMENT: Yes.

MR. LEAVER: I'm not sure how that could happen though.

MR. CLEMENT: Again, it depends on what

use you are making of the source term evaluations.

MR. LEAVER: Yes, okay.

MR. CLEMENT: For our own purposes, we thought we had to go up to this degree of conservatism, but, again, it depends on what you want to do with your source term assessment.

MR. POWERS: That's absolutely true.

MR. LEAVER: Yes, that's true.

MR. BOYACK: Go ahead, Jean.

MR. EVRARD: Generally speaking, we take values which are a lot of the release fraction itself in VERCORS experience to make up for it.

MR. BOYACK: For this entry, shall I just put a slash and your name, too, if that's all right?

MR. GIESEKE: The best you can do there is that 95 minus "X" and down in the late in-vessel put "X."

MR. SCHAPEROW: You are suggesting that the original value, that 50 percent deposition -- see the set of tables there in front of you?

MR. CLEMENT: It's zero.

MR. SCHAPEROW: Instead of 50 percent where there is iodine, it has a mean value suggesting maybe closer to zero deposition.

MR. LEAVER: I think he is saying that

they --

MR. CLEMENT: No.

MR. LEAVER: I don't think he said that.

MR. SCHAPEROW: Then you said, even if we were more realistic, we would still get only a few percent deposition?

MR. CLEMENT: I mean, it depends on the sequences you are considering. If you are considering not only hot leg breaks, you will get deposition.

MR. SCHAPEROW: Okay, and we have got a whole bunch of different sequences.

MR. LEAVER: Clearly, a large break, double-guillotine break at the hot leg nozzle --

MR. POWERS: You aren't going to get quat.

MR. LEAVER: -- you aren't going to get much deposition.

But I personally think that some kind of risk-informed approach to this is the right thing to do. So I would not think that you should just consider that particular type of sequence.

MR. BOYACK: Okay, we are on to you, Dave.

MR. LEAVER: Okay. I think the 5 percent, based on the discussion we had on noble gases, I could argue the 5 percent is high, but then we have the effect of the additional burnup. So I would say leave

it the same. I wouldn't have any basis for changing it.

The iodine will tend to condense as it moves away from the core, and so there would be some deposition even of what comes from the gap. So it seems to me it would be, potentially could be significantly less than the noble gas, but I think that is okay to leave it at 5 percent.

I think in the 35 percent, there are some competing effects here of Tom's point about the CORSOR tending to overestimate all released, but then we have some portion of the core in high burnup. So I would say, in my mind, those two tend to balance one another. I wouldn't have a basis for changing that number.

I think on the ex-vessel, it is hard for me to imagine that you are going to get 25 percent of the iodine down there, but I would tend to think you would get more, a higher release from the late in-vessel, for one thing, because of the ingress of air, but I don't think it matters too much on the ex-vessel. So I guess I would leave that the same. Presumably, these plants are smart enough to figure out how to get water in there and at least scrub some of that.

But I would increase the late in-vessel. I think the 20 percent number would be a reasonable estimate. So the total would be a little higher than the total within the original 1465.

MR. BOYACK: So under the context of what was being done in the United States, is this what I heard: Everything else just stays the same?

MR. GIESEKE: I think we need to address the French position, though, because I don't know what Dave's position was on that, since it got lost in the numbers, but are you assuming, then, 50 percent deposition --

MR. LEAVER: Yes, I'm looking at what --

MR. GIESEKE: In-vessel and then as --

MR. LEAVER: From these expert elicitations and what they used. If they extracted that and assembled it in NUREG CR 5747, which is the basis for 1465, and we have all these arguments about: Where is the break and how big is it, and hot leg versus cold leg, and all that sort of thing. But we really are, I think, trying to get something that is representative of the spectrum of sequences. So I think the .5 is not unreasonable.

So I guess to make a long story short, yes, .5 of the release fraction from the fuel, which

would is estimated to be about 70 percent, which we think may be an overestimate for 1465, but there may be some increase of that due to the effects of the high burnup fuel, and I would say that those things balance.

But now as I look at this, I am wondering how in the heck can you get 25 percent of the iodine down on the floor of the container? How can that happen?

MR. POWERS: What you are really looking at is the degradation of fuel late in the accident sequence.

MR. LEAVER: Where is that fuel?

MR. POWERS: In the core.

MR. LEAVER: So it's not ex-vessel then?

MR. POWERS: It is not really ex-vessel, but you want it to occur for a fairly short period of time.

MR. LEAVER: It is really late -- so you demand that you put it in the two-hour --

MR. POWERS: What you say is that it melts, falls down, and the release occurs ex-vessel.

MR. LEAVER: So you're just putting it down there because you want to release it sooner?

MR. POWERS: Yes.

MR. LEAVER: Yes, okay.

MR. POWERS: It's timing. It's to get the timing.

MR. LEAVER: Yes.

MR. POWERS: It is not iodine left in the melt that transcends all of this.

MR. LEAVER: Right.

MR. SCHAPEROW: You need to identify that.

MR. BOYACK: Yes.

MR. LEAVER: I think if you do do an overhaul of 1465, probably the thing to do is put that 25 percent out in the late in-vessel and say that some of it gets released sooner. Because the idea that you get that much iodine down on the concrete is --

MR. SCHAPEROW: That is why I wanted to keep the noble gas on the same page there. I think we did it the other way for noble gases. For noble gases, we specifically said that there will be noble gas release late in-vessel. But in this case we are putting it in the ex-vessel column.

MR. POWERS: We probably ought to go back and switch that around just to get the timing correct.

MR. GIESEKE: Well, we put a comment in the previous one that said it occurs early in the late --

MR. LEAVER: Yes, we did.

MR. GIESEKE: -- in-vessel.

MR. LEAVER: And it's the same thing.

MR. SCHAPEROW: That feels better to me, just from the point of view of where is the material.

MR. LEAVER: In the interest of not misleading people that use this, maybe we should say we think that ex-vessel iodine really isn't ex-vessel; it's early in the late vessel, or late in-vessel. Sorry. Early in the late in-vessel.

MR. GIESEKE: I was doing the same thing. I was getting all those words mixed up.

MR. LEAVER: Mixed up.

MR. BOYACK: I would be very happy if somebody would give me some words. We're on the halogens, right? Ex-vessel, and you want to qualify this. This is showing up as 25 percent right here, but you want to qualify this because it really isn't halogens on the floor?

MR. SCHAPEROW: Go up the previous numbers. You had the noble gases. It's the same release we put in the last column.

MR. BOYACK: So that's this statement here for continuity of the early in-vessel release, the late in-vessel release would occur in -- so you want

to do something consistently or what?

MR. POWERS: Yes.

MR. GIESEKE: We are talking about the same mechanism basically, that the material comes down, but it is an in-vessel sort of release, release in-vessel or from in-vessel materials after the failure of the vessel. It may occur ex-vessel if they fall through, and they probably do. Unfortunately, some will be in the vessel as well as outside the vessel.

MR. BOYACK: This is an area where I am having trouble by trying to do the actual physical work and then to keep track of the arguments. I have lost it on this one. I just don't know where to go.

MR. SCHAPEROW: Well, the fuel that is releasing the fission products is situated inside of the reactor vessel.

MR. BOYACK: Yes, what I am really looking for is for you to tell me what to do on the document, or someone tell me.

MR. POWERS: You have to remember that, unlike gap release and early in-vessel release, ex-vessel release and late in-vessel release, the way they are used, they overlap in time.

MR. BOYACK: Okay.

MR. POWERS: Okay? What we would like to do is to bias these releases that are occurring from residual fuel early in that 10-hour period.

MR. BOYACK: Yes.

MR. POWERS: One way to do it is to put it in the ex-vessel, which is only two hours.

MR. BOYACK: Okay.

MR. POWERS: It looks to me like that's what people were doing when they came up with the original thing, because even at TMI, a fully pressurized accident, you really only have about 10 percent of the cesium left in the fuel melted; we're down to zip with the iodine.

So I think we are putting it in the ex-vessel to get it forward to the time. Because if you put it over the entire late in-vessel, then you are putting it up over a long period of time, and there is a tendency for it to remain and be even more of a containment for a long period of time.

So, to be realistic, I think they put it in the ex-vessel. That suggests that we do the same thing here for the noble gases. It is not so terribly important where you stick the noble gases, except if you don't want to do -- you don't want to mislead people about the timing on it. So you move that .3

over to the ex-vessel, even though it is occurring in the vessel; it is not in the melt concrete interactions or anything like that.

MR. NOURBAKHSH: You could say that this should not be exposed to the air for overlaying water.

MR. POWERS: That's right. That's right, yes. It doesn't have any--

MR. NOURBAKHSH: Somehow that clarification should be made in a footnote.

MR. KRESS: Yes, that's a good point.

MR. SCHAPEROW: Looking back at the original expert elicitation, there are no curves for iodine and cesium for core/concrete interactions. There's no curves for releases. I think there might be a statement upfront. Wait a minute.

MR. POWERS: When we did the first experiments on melt/concrete interactions, we actually put cesium-iodine into the melt. We poured it on the concrete, and said, oh, well, we'll just monitor how fast it comes out.

MR. KRESS: And then you couldn't?

MR. POWERS: Well, it came out faster than the sampling time. The very first sample was on at the time the melt hit the concrete. The second sample came on 10 seconds later after it hit. The first one

had cesium and iodine in it; the second one had nothing. It blew it out --

MR. KRESS: That's interesting.

MR. POWERS: It just doesn't stay in the melt when you sparge it through with gases.

MR. SCHAPEROW: It is hard to tell where they got the originals numbers. They are not in Load 50. Maybe Hossein knows. Where did the iodine and the cesium release fractions in 1465 come from?

MR. BOYACK: Hossein, if you're going to answer that, come to the mike.

MR. SCHAPEROW: Here's the expert elicitation, and it doesn't show any iodine-cesium numbers. Maybe you can recall?

MR. NOURBAKHSH: That's exactly the same. Whatever retained melt, when it comes go core construction, everything been released. So that is basically whatever comes from these numbers, they are not for iodine and cesium, but it means that they are 100 percent.

MR. SCHAPEROW: Okay, thank you.

MR. BOYACK: Okay, two questions. The first of them is: Do any of these release fraction values for halogen change or are they still okay? The way that we had them down was the only one that we

changed was the 0.2 on the late in-vessel.

Now there is talk about a footnote or another explanation. So what I am asking is, is that something we just enter as comments by one of the people or do you want it just down below the table here as a separate footnote.

MR. POWERS: I would put it down here at the bottom of the table. In the footnote I would say is that cesium and iodine releases ex-vessel are not mitigated by an overlying water pool.

MR. KRESS: That's a little cryptic. Somebody will say, why not? The answer to why not is because it doesn't go through any water.

MR. POWERS: Okay, and the reason is that the release is actually occurring in the vessel and going transit to the ex-vessel. So it never sees the water.

MR. KRESS: Yes.

MR. SCHAPEROW: I have a request that we fix the noble gas table to be the same as this table as far as which phase --

MR. KRESS: I think that would help. I think it was just a little confusing to me otherwise.

MR. SCHAPEROW: Thank you, right.

MR. KRESS: Thank you.

MR. BOYACK: Now is this one okay now? David, I guess we didn't give you a chance to get through anything.

So let me go back and try to correct -- first, I'm going to get rid of that one. Okay, noble gases. Now you're talking about changing numbers or just text? You're talking about putting some of this over here?

MR. SCHAPEROW: Did we?

MR. BOYACK: No.

MR. SCHAPEROW: Oh, we never did. Oh, my goodness. I thought you had one where you had a late in-vessel release for noble gases.

MR. BOYACK: No.

MR. SCHAPEROW: Oh, we never did?

MR. BOYACK: No. Oh, wait a minute. Wait a minute. Wait a minute. Sorry. Let me fast forward.

MR. LEAVER: What was that first one?

MR. BOYACK: I was just taking them one phase at a time. This is the gap release comment. Then you go down to the next one, here's the early in-vessel information.

MR. LEAVER: Oh, okay.

MR. GIESEKE: It's misleading because your

heading up --

MR. BOYACK: Well, I was just changing the red numbers as we went along.

MR. GIESEKE: Well, if you know what you're doing, then that's fine.

MR. BOYACK: Hey, listen, I am perfectly competent and I know what I'm doing.

MR. GIESEKE: I know you are. That's because you're from Los Alamos, right?

MR. BOYACK: Okay, now --

MR. KRESS: The idea was to put the 30 into the ex-vessel.

MR. BOYACK: Is that right?

MR. KRESS: I think.

MR. POWERS: I think certainly Bernard's comment that they're biased forward in time, and I believe that as well. That rescues us out of this problem.

MR. KRESS: Yes, you need a footnote.

MR. SCHAPEROW: It's really a mess with a table. Yes, that's the ex-vessel and late in-vessel table there. The table covers both. That covers both the last two phases.

MR. BOYACK: Well, it doesn't right now.

MR. POWERS: Yes, it does.

MR. SCHAPEROW: Yes, see, there is an ex-vessel --

MR. BOYACK: It's got the numbers; it doesn't have the justification.

MR. SCHAPEROW: It doesn't? Oh.

MR. BOYACK: The justification is right here.

MR. SCHAPEROW: That's early in-vessel. You've only got three tables.

MR. BOYACK: By golly, I do, don't I?

MR. POWERS: Jason is on top of this.

MR. BOYACK: Because there's the other. All right. I am so tricky with my file here, I tricked myself. Okay, I'm with you. Now what do I do with the number?

MR. KRESS: Put 30 in the next estimate. Then write a footnote.

MR. BOYACK: Which is that the noble gas releases?

MR. POWERS: Aren't mitigated by an overlying water pool because they can't be.

(Laughter.)

MR. LEAVER: Because it's not possible.

MR. POWERS: Actually, when I did the analysis for the ACRR reactor, the safety analysis,

the release there is predominantly a krypton release. There is such a huge pool of water and it's so deep that it goes into the solution deep and then actually it stays in the solution and releases very, very slowly. So much water and such a small amount of krypton gas --

MR. LEAVER: There's a small amount of solubility.

MR. POWERS: It's a small solubility, but it is a huge volume of water. It actually goes into solution, and the net result of it is that you can't go in and recover from the accident. This slow release of dissolved krypton keeps the radiation load high enough that people can't go in and turn off pumps or anything like that. It's actually a pain in the ass. If it would just go through the water pool and come out, then it's clean in the atmosphere and you could come in. So if you have enough water, you can mitigate krypton release, but it's a lot of water.

MR. KRESS: The note that you want here is the release is actually occurring in-vessel, but after the time when the bottom head fails. So it is counted as an ex-vessel release.

MR. POWERS: Yes, it's actually occurring in-vessel.

MR. KRESS: Yes.

MR. GIESEKE: There you go.

MR. SCHAPEROW: That would be a nice comment to add on the other table, too.

MR. BOYACK: Okay, where is this --

MR. KRESS: In-vessel.

MR. BOYACK: So?

MR. KRESS: So it's counted as an ex-vessel release.

MR. BOYACK: All right.

MR. SCHAPEROW: Could you copy that for the other one, too, the other table?

MR. BOYACK: You were asking if I would get tired on a day like today.

MR. LEAVER: You're starting to?

MR. BOYACK: The eyes are starting to feel it. Absolutely.

Now it is the halogen releases?

MR. KRESS: Yes.

MR. GIESEKE: It is in-vessel and during transit.

MR. BOYACK: Yes.

MR. GIESEKE: This is your last sentence.

MR. BOYACK: So this last sentence, discard it?

MR. POWERS: You want to keep that.

MR. GIESEKE: I think you want to keep that instead of the first one, but just add the last one. What is it? "Therefore, it's after the time that the bottom head failed, and so it's counted as an ex-vessel release."

MR. KRESS: Yes, you want to get rid of that middle sentence. They are if they are released ex-vessel.

MR. GIESEKE: You're right, they are. Well, they're not.

MR. KRESS: That's confusing.

MR. BOYACK: Okay, I guess now let's see, this is halogen. So these numbers are okay then. The only thing that changes is this late in-vessel?

MR. SCHAPEROW: Did you want to add a comment also on Bernard's statement that no deposition due to consideration of an AB sequence or deposition is sequence-dependent?

MR. BOYACK: Where do I do that?

MR. SCHAPEROW: Right now all there is is a statement saying 95 percent released to containment. I wondered if you wanted to qualify at least that.

MR. BOYACK: This is right?

MR. SCHAPEROW: Yes, right there, maybe at

the end. Are you going to qualify that?

MR. LEAVER: Explain why they are assuming that.

MR. SCHAPEROW: Yes, just to say that deposition is sequence-dependent. Is that okay to say that?

MR. LEAVER: They wanted to wanted to wait for a release --

MR. SCHAPEROW: Oh, okay, worse case or a conservative --

MR. CLEMENT: Reasonably --

MR. LEAVER: Extremely unlikely.

MR. CLEMENT: But, I mean, reasonably it could be different from one to another. When you want to be sure that your emergency plans outside of the plant will be okay, you need some conservatism in writing. Okay?

MR. BOYACK: Conservative approach to assure --

MR. SCHAPEROW: Well, it's conservative because they chose an AB sequence. They chose a hot leg break, a large hot leg break.

MR. POWERS: A large break, hot leg break was assumed.

MR. BOYACK: And there's no deposition in

such an event.

MR. SCHAPEROW: Yes.

MR. BOYACK: Oh, I like that.

MR. SCHAPEROW: I don't want to put words in your mouth. Right now we've got -- I'm just seeing if one more thing might help the explanation. Somebody looking at it later might now know why does it say 95 percent.

MR. KRESS: We will forget.

MR. SCHAPEROW: And other experts say --

MR. POWERS: Reminds me of a panic-stricken phone call I got from an NRC staff member, who's not present, so I can talk behind his back. They had one of the Commissioners, who is no longer a Commissioner, so I can talk about him, had been in Europe and they were talking about the source term there. They were, of course, talking about the source term to the environment. RES had come in and talked about the source term, and of course they were talking about the source term to the containment. The Commissioner says, "Oh, my God, RES has gone off the deep end. They have this huge source term.

MR. LEAVER: It's a hundred times greater than what --

MR. POWERS: Oh, thousands of times

bigger. This was in Sweden. They were talking about 1 percent release, and it made all kinds of accusations of irresponsibility on the part of the NRC staff, and they wanted me to conduct and write a report for this guy on the different source terms used around the world for these things. There was this massive phone-calling going on just to straighten out one element of confusion, but it couldn't be straightened out with this guy by the RES staff. He was convinced that they were irresponsible, duplicitous, overly conservative individuals, and it was simply a matter of nomenclature.

(Laughter.)

MR. SCHAPEROW: Do you want to take a break now? I can go ahead and print those out.

MR. BOYACK: I was actually going to try to go to 3:00.

MR. GIESEKE: Rest your eyes, Brent. Don't kill yourself.

MR. LEAVER: Yes, let's take a break.

MR. GIESEKE: If you want to take a break for a while and rest and we'll take another break, and we'll work a little later.

MR. BOYACK: How about 10 minutes then?

MR. GIESEKE: No, we'll work later, if

necessary.

(Whereupon, the foregoing matter went off the record at 2:33 p.m. and went back on the record at 2:45 p.m.)

MR. BOYACK: The next fission product on the list is alkaline metals.

MR. GIESEKE: Dana said he wants to be first on all of these.

(Laughter.)

MR. BOYACK: This may be right, but I have sort of forgotten where we were last time.

MR. GIESEKE: I think I was first last time.

MR. BOYACK: So why don't we -- I've allowed our French colleagues to not go first unless they have a desire to do so. Would you like to weigh-in and offer your opinion on alkaline metals?

MR. CLEMENT: Okay. So for gap release, I think we should have the same values as for iodine. There is no reason to have differences.

Then go to the other points. For cesium we consider that there will be some deposits, okay? So we consider --

MR. BOYACK: Now are we down in the early in-vessel area here or is it just that I'm still

continuing on with -- no, gap release you said.

MR. CLEMENT: Gap release, that's okay.

MR. BOYACK: Okay.

MR. CLEMENT: Then we have the same -- okay, how to express it? We consider that we will have 65 percent release both in the early in-vessel and in the early phase of the ex-vessel, as I said before.

MR. BOYACK: Total?

MR. CLEMENT: Total.

MR. KRESS: No deposits?

MR. CLEMENT: Yes, deposits. That's at the break, through the containment. We consider, in fact, an overall release from the fuel of 95 percent.

MR. KRESS: Okay.

MR. CLEMENT: And overall, that is not --

MR. BOYACK: Did I get that right?

MR. CLEMENT: Okay. So I will summarize. So phase 1 --

MR. BOYACK: Yes?

MR. CLEMENT: -- that was gap release, 5 percent.

MR. BOYACK: Yes.

MR. CLEMENT: Okay? Then for the phase 2 and 4, okay, that is early in-vessel --

MR. BOYACK: Okay, so the --

MR. CLEMENT: That is early in-vessel and early phase of late in-vessel, okay? You have a 90 percent release from fuel, that there are some deposits. You consider some deposits, and there is 65 percent release at the break.

MR. BOYACK: Okay.

MR. CLEMENT: We did not consider an ex-vessel release. In fact, there is some cesium left in the fuel because we only release 95 percent from the fuel. So we consider that there is some cesium left in the fuel, but not all will be released, so we did not consider it.

MR. BOYACK: Okay, so --

MR. CLEMENT: Then I am considering our figure from the two in-vessel phases, except the gap releases. This should be shared in the same way then for other volatiles.

MR. BOYACK: So did I read this right, that you retained about 30 percent of the cesium in the fuel or --

MR. CLEMENT: No.

MR. BOYACK: Then I've got my numbers wrong somehow.

MR. CLEMENT: Ninety-five release from the

fuel, including gap release.

MR. BOYACK: So, okay, this figure is 95. Pardon me.

MR. LEAVER: Ninety.

MR. CLEMENT: Ninety.

MR. BOYACK: I'm sorry, I misunderstood.

MR. CLEMENT: Okay. >From this 90, 65 percent goes to the containment from this 90.

MR. BOYACK: That will be 90 percent, 60 --

MR. CLEMENT: Five

MR. BOYACK: Sixty-five percent is released to containment.

MR. CLEMENT: Yes.

MR. SCHAPEROW: Twenty-five is deposited. Is that okay? After "deposit," did you write in "RCS"?

MR. BOYACK: And this represents both of you?

MR. CLEMENT: It is, in fact, what is in our own estimates.

MR. BOYACK: Okay, we'll stay over on the other side of the table, David, if it's all right with you and ask for your comments. David Leaver?

MR. LEAVER: Well, the cesium, at least if

you believe the tendency in the expert elicitations from 1150, you had maybe a 10 to 15 percent less of a release from the fuel for cesium. I'm not sure that's right, but I had in my mind that the iodine and cesium releases would tend to be about the same. So I guess if one accepts these expert elicitations, and there's also information in here that suggests that there's slightly greater retention of the cesium in the RCS, that tends to justify the -- that is the basis for the 25 percent versus 30 percent number. Is it 25 percent?

MR. POWERS: Twenty-five percent early in-vessel release.

MR. LEAVER: It is a difference of 40 percent versus 25 percent?

MR. POWERS: Thirty-five versus 25.

MR. LEAVER: Thirty-five versus 25?

MR. POWERS: Yes.

MR. LEAVER: Yes, for the PWR, right. Okay.

I guess it would seem to me the logic that we apply to the iodine release in turns of the effect of burnup would tend to be the same here, which is maybe for the higher burnup fuel, you might see slightly higher releases, but at least for the iodine,

and I would think the same logic would apply with the -- they may have been overestimated, the cesium may have been overestimated in the first place. So that would suggest that the .05 and the .25 not change.

I've always struggled a little bit with the idea that the cesium would be that much less than the iodine, say .35 versus .25. I'll just check one thing.

MR. BOYACK: While he is doing that, which one is the real problematic group? Because I wanted to start it out with Dave on that one.

MR. LEAVER: Oh, you mean the contentious group?

MR. BOYACK: Yes, of these fission products.

MR. LEAVER: Barium, certainly barium, strontium and ruthenium might be. Interestingly, at TMI, I know -- I was just trying to recreate --

MR. CLEMENT: Sorry, but for barium, strontium, and ruthenium, we would have some differentials because we don't have the same classification that you have for release in groups.

MR. BOYACK: Actually, what happened was I was trying to be funny and it wasn't.

(Laughter.)

That's because people think it may be true.

MR. LEAVER: At TMI both iodine and cesium, release fraction was 55 percent from the fuel. In the PBF, the one I have here, which is 1/4, the cesium release was twice as much, almost as twice as much as the iodine release. So I, for one, have never been comfortable particularly with the cesium being less than the iodine, but the iodine is pretty high. So I don't know if it would make sense to increase the cesium up to the iodine. So I guess I would be, at least at this point, satisfied to leave it the same.

MR. BOYACK: Okay, and on the early in-vessel now, of course, what our French colleagues have done is sort of combined these phases. But, by necessity, I think we have to separate them.

So early in-vessel?

MR. KRESS: That is the one he just said was the same.

MR. LEAVER: Yes, the gap in early in-vessel.

MR. BOYACK: Oh, it was early. See, you've gone ahead of me, and I hadn't realized it.

MR. LEAVER: I was just kind of treating a few together.

MR. BOYACK: So you're saying hold it at 25 percent?

MR. LEAVER: Yes, although I would certainly be willing to enter into the discussion. I would like to understand better why it makes sense for the cesium to be less than the iodine, given the fact that probably among the two best integral experiments we have, TMI and one of the PBF's, had it just the opposite.

I would also say it is hard to imagine 35 percent of the cesium being in the debris that comes out of the vessel. So that same logic that we used on the iodine would tend to apply here. Well, that's another story, but whatever it is, there's not going to be very much cesium left in what's on the floor. And if anything, cesium would tend to be more volatile than iodine. So I would think that the late in-vessel would make sense. Since we increased that for iodine, we would expect to get the same kind of increase for cesium.

MR. BOYACK: Okay, so can you help me a little bit with what these values might be? So for the early in-vessel, it clearly is less than what we were seeing for iodine, but what would you suggest a value of?

MR. LEAVER: Well, let me ask a question before I do that. Why is this number less than the iodine number?

MR. POWERS: Because we can identify a lot of vapor pressure cesium compounds that form in the system, and especially if you have boric acid.

MR. LEAVER: Cesium borate, you mean?

MR. POWERS: Forms cesium borate and things like that with relatively low vapor pressures. So they found that both, there was the potential of retaining more of the fuel as cesium uranate, and the potential for depositing more in the reactor coolant system as things like cesium borate or zircanate, things like that. Though people were aware of experimental evidence on cesium release, I think most people conceded iodine tends on the whole to be released more than the cesium.

MR. LEAVER: Even though two pretty good --

MR. POWERS: Yes, they also knew what the uncertainties in those numbers were.

MR. LEAVER: Yes, yes, but it is interesting that both of them fly in the face of that.

MR. POWERS: Yes. What we also know is finding all the iodine, the mass balances on these

experiments on iodine is not very good. At least on the PBF experiments it was never very good. Finding all the iodine is a chore. So I think they just underestimated it.

MR. LEAVER: So the denominator may be wrong for the iodine.

MR. POWERS: Yes. And let's see, I did think, based on TMI, that there would be some retention and the fuel in ex-vessel. Most of that 35 percent is, in fact, the 25 percent of the residual fuel that didn't melt during the in-vessel phase melting out late.

MR. LEAVER: The same as for --

MR. POWERS: As for the iodine.

MR. LEAVER: -- as for iodine, right. Yes, that I understand.

Okay, so the point you're making is cesium we know it is wrong to assume that it is all hydroxide. Maybe even none of it is hydroxide. It's these other forms which tend to have reduced vapor pressures, and that can explain -- it is not as volatile as it would be if it were hydroxide.

MR. POWERS: Yes, I think that people didn't know exactly. All they knew is they could identify all these things when they had more vapor.

We had done some calculations explicitly with various forms of cesium borate and showed that we could just drop the vapor pressure down to zip, depending which of the borates formed, and bend it on the boric acid/the cesium ratio that you had.

MR. LEAVER: It is very stable stuff?

MR. POWERS: What happens is just the cesium activity drops off so low that there's no vapor pressure, is what happens. We find these polyborates that have one cesium atom in every 100 boron atoms. There's no activity.

So it is largely based on just identification rather than explicit calculation. Whether it is really true or not, of course, not bad. Not bad.

MR. LEAVER: Well, if you want a number, I guess I will do the number of 25 percent and maybe listen to what everybody says and be allowed to reconsider that at the end perhaps.

MR. BOYACK: But that was a good question because it did solicit the information.

MR. LEAVER: Well, I think Dana has a very good point about the cesium compounds. I think 10 years ago everybody said it's hydroxide, and I think we know now that that's not that simple. That would

explain why people think the number is less, although I tend to believe the experimental results than what people calculate on these things, but, of course, there is uncertainty in experiments, too. These measurements are hard to make.

MR. BOYACK: Okay, so let's move on to the ex-vessel phase.

MR. LEAVER: For me you mean?

MR. BOYACK: Yes.

MR. LEAVER: I don't believe the 35 percent, but I guess what it is is it's a way of saying that this release from what's left in the fuel that is in the vessel goes out sooner, the same as for iodine. So I think that structure you had with the footnote would apply, and the reason it is 35 percent instead of 25 is because I guess you've got an additional amount of cesium left up in there, which if you heat that fuel, if that fuel continues to heat up long enough, it's all going to go.

MR. SCHAPEROW: You are on halogens, right?

MR. LEAVER: Yes, you call it halogens, right.

MR. SCHAPEROW: I think you included that in the ex-vessel column, though, for halogens.

MR. BOYACK: Pardon me? Oh, okay, yes.

MR. SCHAPEROW: The actual number.

MR. BOYACK: I see. Okay, so were we leaving this at 35 percent then on the ex-vessel?

MR. LEAVER: Well, it's the footnoted ex-vessel, which is that it is really released early in the late in-vessel phase.

MR. BOYACK: Okay, and that leaves us late in-vessel. So maybe this comment that I entered in is no longer valid because it was prior to your discussion with Dana.

MR. LEAVER: What does that say?

MR. BOYACK: It says nothing because I don't think it made sense. I had gone down quickly and made some others.

So let me just ask you, what about the late in-vessel phase, your thoughts?

MR. LEAVER: I guess I would leave it the same.

MR. BOYACK: So what this is saying, 60, 65, 75, this is saying what, 25 percent is retained, the cesium is retained, 30, 65, 75, right?

MR. SCHAPEROW: In the RCS.

MR. BOYACK: Uh-huh.

MR. LEAVER: A third of it.

MR. BOYACK: Yes. So you're okay with using --

MR. LEAVER: Yes, I think that is kind of consistent with, if we believe the cesium really is somewhat less volatile form, then that is probably not a bad number.

MR. SCHAPEROW: There was also a chemisorption model that we put in our codes over the last few years, some Sandia experiments where this chemisorbed on the interior surfaces of the RCS. Dana can probably help me with that.

MR. POWERS: For a long time they have modeled cesium in terms of a condensation and an irreversible chemisorption.

MR. LEAVER: As a hydroxide?

MR. POWERS: They don't call it a species because what they observed in the experiments was that they would find these tightly-bound cesium species on the surface associated with the tramp elements, like phosphorous-added steel, alumina that came from God knows where, probably the water supply, and things like that. Of course, it was forming the cesium luminates and cesium phosphates and things like that. It was forming these mixed oxides through a relatively non-volatile.

So they set up -- so from back in the days of trapped milk, they set up an irreversible chemisorption term in the deposition models, as well as a more physical absorption due to cesium hydroxide.

As you go on in time in the analysis, what happens is the physically-absorbed material vaporizes and some fraction of it deposits irreversibly. So you get a slow conversion into this irreversible kind of thing.

Similarly, in TMI we found cesium bound to the lead screws as cesium silicate, which is the silicate impurity in the lead screw steel. So it slowly converts itself into a --

MR. LEAVER: It's very reactive stuff.

MR. POWERS: It's very reactive stuff.

MR. BOYACK: Okay, Dana, how about you begin with gap release for alkaline metals?

MR. POWERS: I'm going to be enormously controversial here and just say I want to leave the numbers alone. My reasons closely parallel Dave's thinking on this.

The one I have question about is whether we are overestimating the vaporization release, but I am attentive to Jim's comment that we probably tend, we could naturally tend to disperse the material so we

have a low heat source, don't get these hotspots, which will perhaps counterweigh the tendency to form less volatile species.

So the net effect is that understanding the revaporization of cesium is a critical need. We need experiments in that area. I'm not prepared to argue for substantial changes to the numbers. I think revaporization becomes much less likely as you get air intrusion because you tend to form iron oxides and chromium oxides that will form cesium chromates and cesium ferrates, which will be even more type of binding material.

So the upshot is I can't find a basis to change the numbers.

MR. BOYACK: Okay, but you did have a statement of need, did you not?

MR. POWERS: Right.

MR. BOYACK: And that was?

MR. POWERS: We need experiments on revaporization of cesium.

MR. SCHAPEROW: In the last in-vessel and the early in-vessel or just late in-vessel?

MR. POWERS: It is really the late in-vessel.

MR. CLEMENT: I think Dana is right

because we have some similar experiments and complex reactions of cesium with sulfates, not only with chemisorption, but even when only deposited by vapor or condensation, then it reacts with the sulfates and forms different species, many bundled, some being able sometimes to revaporize at rather low temperatures. So it is a very complex pattern.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: I am torn between going through some hand-waving with leaving everything the same. Because I went through a logic system and I don't come up with anything that's very different from what is there, thinking about all the things.

MR. BOYACK: But sometimes just the comments to the rest of the panel are useful.

MR. GIESEKE: Well, I was going to say, as far as release from fuel, I don't know, that could range pretty widely, 30 to 100 percent, maybe half that retained, or thereabouts, in transit. A few percent, 5 percent gap release, if you want to put that, that's okay. Maybe 30 to up close to 100 percent or 90 percent release with continuation. So I would guess like 30 percent for the early in-vessel, but that -- what's the number that's there? It is not that much different.

MR. POWERS: Twenty-five.

MR. GIESEKE: Yes.

MR. BOYACK: So you are saying, if you --

MR. GIESEKE: I went through some numbers and I came up with these, but they are not that different. So it is hardly worth quibbling about.

MR. BOYACK: All right. So if you're going through this the first time, you may have put 30 percent.

MR. GIESEKE: Yes.

MR. BOYACK: Given sort of the fact of the uncertainty, you can live with the 25 percent?

MR. GIESEKE: Yes.

MR. BOYACK: Okay. Like I say, any of these, later on you will be able to strike or modify any of these statements.

MR. GIESEKE: What's this one attaining?

MR. POWERS: Thirty-five percent.

MR. GIESEKE: Thirty-five. I guess a little lower.

MR. BOYACK: So I had this one wrong.

MR. GIESEKE: Yes, retain the 25.

MR. BOYACK: Actually, I --

MR. GIESEKE: The 35 I mean.

MR. BOYACK: Yes, yes.

MR. GIESEKE: Actually, those should be about the same also, for the same reasons that Dana mentioned, the two conflicting reasons.

MR. BOYACK: I didn't really enter that in this table. I might be able to just enter it one place, those two conflicting reasons. One of them was air ingress, was it?

MR. GIESEKE: Air ingress and hotspots.

MR. BOYACK: Okay.

MR. POWERS: Well, I would call it compound formation and hotspots.

MR. GIESEKE: Yes.

MR. POWERS: Because it's complex structures and things like that that we are seeing. I mean, this has been known for a long time: that cesium just loves to react with stuff. Unfortunately, those reactions are things that we don't have in the thermo-chemical databases.

MR. BOYACK: And that gets that.

MR. SCHAPEROW: The second one is not air ingress. It's --

MR. BOYACK: Hotspots.

MR. SCHAPEROW: Hotspots

MR. POWERS: Hotspots caused by localized deposition. What happens, like you say, you've got a

little discontinuity of the pipe; you get these recirculating structures, and all of the products go right back there, whereas the code tends to steer those out over the whole pipe.

MR. GIESEKE: These are the same.

MR. BOYACK: If I read this right, this brings us to -- drum roll --

MR. POWERS: You're going to be difficult, I can tell. You're not going to be difficult?

MR. KRESS: Not very difficult, a little difficult.

.05 is okay, for the same reasons that we have said. I am going to be a little contrary on the in-vessel. I'm going to go with Jim's 30. I think it's actually a real number, partly because of what Dave said. When I looked at the separate effects test, release rates for cesium, they are not that far below the iodine, as shown. So when I make a correction for that, I get it releasing something like 60 percent as opposed to 70 percent for iodine. But about half of that gets retained in the vessel. So that makes it 30 percent for me, and I'm going to go with it. I actually believe that's a good number.

I would keep the 35 for ex-vessel, and for the same reason everybody else is.

MR. LEAVER: This is not a burnup effect that you're talking about though?

MR. KRESS: No, there's a slight burnup effect.

MR. LEAVER: Slight?

MR. KRESS: There's not a lot.

MR. LEAVER: But if there was no burnup effect, you would still tend to --

MR. KRESS: I would still raise it, and I wouldn't go all the way to 30 if it wasn't for the burnup effect.

I thoroughly believe that cesium is retained much better than the one-third that we are using, but I think for a design basis source term, it's better to take care of the uncertainty by letting it get out. So I'm going to stick with the .1, although I think it is less than that.

MR. BOYACK: This is the revaporization?

MR. SCHAPEROW: Yes.

MR. BOYACK: There's this comment that I have entered for cesium, retained better --

MR. KRESS: I'm saying that 30 percent got retained and then 10 percent of that got released in my numbers. I'm saying I really believe that less than 10 percent got released; more of it got retained.

But I think it is better -- I am uncertain about that, and I think it is better to deal with that uncertainty by going ahead and releasing it in the late in-vessel. So I'm saying I'm going to stick with the 10 percent because I think it is a better design basis number to use, although I thoroughly believe more of it will be, when it gets locked onto those surfaces, it stays there, is what I believe.

MR. BOYACK: Okay. Now we face this challenge between sort of the French approach and the U.S. approach, but this will go mainly to the U.S. usage, so I will probably continue to divide these.

So the only issue, if I heard it right, was -- these are okay.

MR. LEAVER: There is not that much difference between necessarily what the French are saying and what we're saying.

MR. POWERS: The difference is between 60 and 65 percent release into containment, and then we have 10 percent later on.

MR. LEAVER: Yes.

MR. POWERS: Okay. So we are 5 percent below them at start and 5 percent above them at the end, and there is not a whole lot of difference there.

MR. LEAVER: I feel like they are kind of

validating a little what we're -- okay, it is all kind of hand-waving, but not too bad.

MR. BOYACK: My comment was breaking it up into tables, but, yes, that notwithstanding, I understand.

MR. LEAVER: Yes.

MR. KRESS: I wonder if my very unconvincing arguments convinced anybody to go with the 30 percent instead of 45?

MR. BOYACK: So, basically, we have a couple of people that have got to 30 percent.

MR. KRESS: Yes, Jim was wanting to go there, but he didn't quite do it.

MR. GIESEKE: I gave up.

MR. BOYACK: Is there --

MR. KRESS: I think it is inconsistent with the iodine mainly.

MR. SCHAPEROW: I think we have a larger alkaline metal ex-vessel release than for iodine, unless we change that.

MR. BOYACK: We could always change that.

MR. POWERS: If you take Tom's 30 percent there, I'll bet you you're going to take the extra 5 percent out of the ex-vessel release.

MR. KRESS: It adds up.

MR. SCHAPEROW: What was the iodine table, Brent?

MR. POWERS: I mean the sums are the same. The iodine table has 35 and 25 and this one has 25 and 35.

MR. SCHAPEROW: Noble gases.

MR. BOYACK: That's what we want, isn't it?

MR. SCHAPEROW: Iodine, halogens.

MR. BOYACK: Halogens? Oh, well, let me get the right one then. All right, there we go.

MR. SCHAPEROW: 05, .35, .25, .10.

MR. POWERS: Are we going to normalize?

MR. KRESS: If you look at that, the 35 is half of 70. The add the 5 for the gap, and it makes it 75. So that leaves you 25 ex-vessel. It is the same thing here. My 30 is half of 60. Sixty and five is 65; that leaves 35 in ex-vessel.

MR. GIESEKE: But there's a difference. The French are saying a total of 90 percent release.

MR. KRESS: I think it all gets eventually released.

MR. BOYACK: I haven't heard anybody say change that to 30. With enough numbers, then I'm going to do it, but I just wanted to know whether

anybody has a strong opinion.

MR. KRESS: You're not getting much of a hue and cry for 30 percent, are you?

MR. BOYACK: Well, no, and I shouldn't be the final arbiter of this.

MR. POWERS: are you going to take the extra 5 percent out of the ex-vessel hide?

MR. KRESS: No, it still adds up.

MR. LEAVER: It adds up to more than iodine if you don't take it out. Your total release would be greater, wouldn't it?

MR. KRESS: Yes, my release of the cesium from the fuel is 60 percent. That is what I calculated using my release model with the high burnup correction. Okay. If that 60 percent gets 30 percent or half of it gets deposited, I don't even need to worry about what gets deposited. The 30 I have is half of the 60. The rest of it gets deposited.

MR. LEAVER: Okay. So you've got 30.

MR. KRESS: So I have 60 plus 5 over here.

MR. LEAVER: Right, right, 65.

MR. KRESS: Sixty-five.

MR. LEAVER: Plus 35.

MR. KRESS: Plus 35. That's all of it.

MR. LEAVER: So you have no release for

late in-vessel?

MR. KRESS: Yes, the part that gets deposited, 30 percent. Thirty percent got deposited; 10 percent of that got re-released. So it adds up.

MR. POWERS: Okay, so you just want early in-vessel to go up to 30 percent?

MR. KRESS: No.

MR. LEAVER: Yes, he does.

MR. KRESS: Yes, yes, that's the only thing I want, is the early in-vessel to go up to 30 percent, right, and my numbers still add up.

Now this is where it doesn't add up, I don't think, unless they are assuming 60 percent release, but --

MR. POWERS: There's more deposition.

MR. KRESS: There's a lot of deposit.

MR. POWERS: There's more deposition. There's a combination. There is more retained in the fuel. I mean, at the time they were heavily biased by the 10 percent number for the melt in the bottom at TMI, plus in the analysis for cesium-iodine all I had was a physical process. They knew for the cesium, without the iodine, that they had an irreversible chemical deposition. Okay, that is what leads to that number there.

MR. KRESS: What leads to the extra deposition? I don't know what leads to the extra deposition of cesium compared to iodine. You would have thought they deposited about the same.

MR. POWERS: No, what they were saying was the cesium, at the time they were saying cesium-iodine deposits largely by condensation mechanisms. Cesium hydroxide deposits both by condensation and chemical reaction with the surface.

MR. KRESS: Yes, but the amount that goes there is condensation.

MR. LEAVER: It's what stays there.

MR. POWERS: No. No, because --

MR. KRESS: Condensation is a two-way street?

MR. POWERS: Well, the partial pressure at the interface, the chemical reaction is very, very low.

MR. LEAVER: Yes, that's right.

MR. POWERS: Whereas the partial pressure when you have condensation is actually quite high.

MR. LEAVER: You could have no condensation; if you have the chemisorption, you're going to have deposition.

MR. POWERS: Deposition, yes, and that is

what the code did for you, was it gave you deposition from the leading edge, whereas condensation only occurred way down the pipe. That's how it came about. Now whether they got their sums right and things like that, that is open to argument.

MR. KRESS: So I might be tempted to say --

MR. POWERS: I wouldn't have to say anything.

MR. KRESS: Yes, I think I will leave it at that.

MR. POWERS: Thirty, between 25 and 30 just doesn't get me too excited.

MR. LEAVER: Me, either. You couldn't argue that he is wrong with 30. That's for sure.

MR. POWERS: No. No, I've got no argument for him, but I simply have no argument that the 25 is wrong either.

MR. LEAVER: Right.

MR. POWERS: So, I mean, if he was going to froth at the mouth and blubber and bawl and things like that, I would give in to him. Since he's not going to do that, I am going to make him sweat it out.

(Laughter.)

MR. GIESEKE: One reason to raise the

number would be to be closer with the French guidance.

MR. POWERS: That would get you right on top of the French guidance.

MR. GIESEKE: Well, no, they're saying 70 percent penetration, 30 percent deposition, roughly. We're saying 50 percent deposition, 50 percent penetration. But this would move us up a little closer to them, which is the only consideration, is a consideration for raising it to 30 rather than the 25, looking at everything together, regardless of what I compute, doing a people averaging exercise.

MR. POWERS: Show us what we have done so far. Can you just flip through these now. Start from the top.

MR. LEAVER: It would be nice to have a transparency that you could mark up. I have forgotten already what we did on the duration.

MR. BOYACK: I'll tell you what --

MR. GIESEKE: I have, too. I don't even remember the duration.

MR. BOYACK: Is it possible to get a transparency made of this?

MR. SCHAPEROW: Sure.

MR. BOYACK: And then we can just mark on the changes and keep it up there.

MR. SCHAPEROW: Yes, I think we need that. I really do.

(Whereupon, the foregoing matter went off the record at 3:31 p.m. and went back on the record at 3:40 p.m.)

MR. BOYACK: The first thing I want to do, as I mentioned to you, Jason, during the break we took the time to look at the calendar and to schedule, assuming that there is no conflict with NRC schedules, the third and final meeting of the panel.

MR. SCHAPEROW: That's just fine with me.

MR. BOYACK: Okay. So, for the record, if you would consider that we will be holding the third and final panel meeting on February 26th, 27th, 28th. That's a Tuesday through Thursday. For your travel arrangements, if you would sort of hold to the idea that we would finish maybe two o'clock in the afternoon generally is about the time that we finish. We will need a little afternoon time. Occasionally, we get done early, but generally that is the approach.

Now, given that, why don't I flash these up and let you --

MR. SCHAPEROW: I'll mark them. Okay, go ahead and put the other thing up and I'll mark them.

MR. BOYACK: So we start out with noble

gases. Okay, let me come down here. Okay. The next one is 0.63.

MR. POWERS: Brent, if people would like to get coffee, the coffee place will close in the next few minutes.

MR. BOYACK: Is there somebody that would like to have coffee? Then Dana could take them down or one of the badged people could. Anybody that wants coffee?

Okay, it will take us a moment.

MR. SCHAPEROW: Are we going into the timing also?

MR. BOYACK: Yes, I can do that.

MR. SCHAPEROW: We'll do the whole thing here, see how we're making out.

MR. BOYACK: Let me get down to the bottom here and see. Okay.

The first one is 0.4, then 1.4, and the last two are staying the same.

Then we go to halogens. The only thing we changed on halogens was the late in-vessel, which went to 0.2.

Let's see, now we're down to alkali metals. I believe this is the one. Okay. So alkali metals, we left those all the same, didn't we?

MR. SCHAPEROW: That's what we were discussing, actually these two numbers, the early in-vessel and ex-vessel.

MR. BOYACK: Okay. So now is this an ongoing discussion?

MR. KRESS: I think we're going to leave it.

MR. SCHAPEROW: We leave it?

MR. KRESS: I couldn't convince anybody.

MR. BOYACK: So that moves us on to the tellurium group, right?

MR. SCHAPEROW: Just for clarification, I guess I'm kind of wondering why so much of an alkali metal release ex-vessel. I think that kind of does tie into the --

MR. LEAVER: It's not ex-vessel.

MR. KRESS: It's the same comment as the halogens do.

MR. SCHAPEROW: But it comes out later though. The idea is that cesium comes out a little bit later than iodine.

MR. KRESS: But not much.

MR. SCHAPEROW: But not much, all right. All right.

MR. BOYACK: Okay. So the next one on the

list would be the tellurium.

MR. SCHAPEROW: Would it be all right if I put like as asterisk on these two, saying they're released early in the late in-vessel period, or I don't know?

MR. BOYACK: Well, if you want --

MR. SCHAPEROW: Well, let's see how the next one comes out, I guess. All right.

MR. BOYACK: Now, Tom, I want you to start thinking because you are going to be first.

MR. KRESS: I'm ready to start, except I want Dana here, because I don't, for the life of me, know where they got those tellurium numbers. I would have them almost exactly the same as the iodine and even a little higher or close to the iodine.

MR. SCHAPEROW: There is also a comment in the final points that talks about tellurium release being reduced somewhat in response to comments. Shall I get those? Those are up there in my office.

MR. BOYACK: You notice we're in a mini-break here now while we wait for Dana to come back.

(Whereupon, the foregoing matter went off the record at 3:47 p.m. and went back on the record at 3:55 p.m.)

MR. BOYACK: Tom Kress said that he just

found it impossible to go ahead and launch on the tellurium until Dana got back.

MR. KRESS: Tellurium.

MR. GIESEKE: The big "T" group.

MR. KRESS: If one looks at the volatility of tellurium, it is even higher than that of iodine. It's close to it. If it were not for the fact that some people think, and the evidence is there, that it gets held up by the clad, it would get released just about like the iodine would.

Is it a fact that the clad or somewhere it gets tied up, either in the fuel or in the clad, until about 90 or 95 percent of the clad gets oxidized, and then it gets released as a burst, almost quantitatively.

So the question comes down to, how much of the clad will get fully or 95 percent oxidized in a given transient, so that that fraction of the tellurium gets released from that fraction they've got tied up, say, if it got tied in the clad?

I think the .05, which is 5 percent, is ridiculous. I think more than 5 percent of the core gets fully oxidized in most of the accidents. So I would raise that number to something, but I'm not sure what yet.

Even more ridiculous is the 25 percent ex-vessel because, if it didn't come out in-vessel, when it gets down there in the ex-vessel, it's going to come out during the zirc oxidation part of that. So I don't know where they got the 25 from.

I'm not too happy with the late in-vessel either because I think there are some questions of residual fuel that, when you get to the higher oxidation, it might very well come out there.

So those are my comments. I don't know exactly how to change the numbers, and I don't know where they got those numbers from. Any discussion about that?

MR. BOYACK: Hossein, do you have any background or comments you wish to make about the tellurium?

MR. NOURBAKHSH: In the draft document, based on again 5747, it was 15 percent. There was a comment, from what I see here, from the British -- I don't remember who -- that made the comments that this is too high. So they made it -- I mean, the NRC decided to lower that down.

MR. KRESS: I think what they are talking about is, what fraction of the plant gets oxidized to 95 percent? That fraction is going to release that

fraction in tellurium. I think it ought to be more like 15 percent or even 20 percent. I don't know where they get the 5. Then what about the 25 percent?

MR. LEAVER: So it was a British comment, but you don't know the substance of that comment?

MR. NOURBAKHSH: I know the comment; I have a copy of the comment, at least the part that I highlighted for NRC.

MR. KRESS: How did they come up with 25 percent for the ex-vessel? I don't recall at all that.

MR. NOURBAKHSH: It doesn't say that point. It says it is high because of the diffusion by cladding.

MR. LEAVER: They were talking about the in-vessel.

MR. NOURBAKHSH: In-vessel.

MR. LEAVER: Yes.

MR. NOURBAKHSH: In the final report it came down from 15 to 5.

MR. SCHAPEROW: And for the in-vessel release, one of the experts had a very low release of tellurium. It was expert B. He's got this curve here. He doesn't get --

MR. NOURBAKHSH: Yes, but if you look at

the onset of these, the tellurium is much higher --

MR. SCHAPEROW: Then in the ex-vessel the experts all suggested that tellurium would be released ex-vessel. That is one of the five curves we got from the expert elicitation. That is the first one.

MR. KRESS: Then what's that 25 percent up there for?

MR. SCHAPEROW: It should be the mean value from this curve.

MR. POWERS: Twenty-five percent of the core has slumped down into the ex-vessel, and it releases 100 percent of its inventory.

MR. KRESS: But there is only 25 percent of the core.

MR. LEAVER: No, there's got to be more because --

MR. POWERS: There's got to be more.

MR. LEAVER: I think it is more like 35 or 40 percent because the cesium release ex-vessel was 35 percent, unless these guys meant by that, that that's what was released from late in-vessel. I don't know.

MR. POWERS: One thing, I mean, what you know is a lot gets released, but tellurium has this propensity to react with any metal it sees along the flow pathway.

MR. LEAVER: I would offer --

MR. KRESS: So maybe that 5 percent, the difference is what gets plated out in-vessel? It gets latched onto things in-vessel, do you think?

MR. POWERS: I don't know. I know that they were really, at the time, these crazy guys at Oak Ridge were saying that you had to oxidize clad up to these fairly high numbers --

MR. KRESS: Ninety-five percent is what they said.

MR. POWERS: -- to get any kind of release. At the time there was a bias to argue that the codes were overpredicting the amount of clad that was oxidized.

MR. KRESS: So you put those things together --

MR. POWERS: They may have suppressed the release fraction and then they may have tied up a lot of it in the primary piping system, was their thinking.

MR. KRESS: All those things lower that one number.

MR. POWERS: Yes.

MR. BOYACK: In your testing it in France, have there been any insights into tellurium behavior?

MR. CLEMENT: Yes. We cite here that clearly the tellurium reaches very higher release fraction. That's true that the release stops at a given place. We are close to 90, 95 percent. But I am not so sure that we should apply this amount of oxidized cladding to calculate the amount of tellurium because when the zircaloy is not oxidized, what happens when you are speaking of that melt, there are some interactions. It slows down. It could be, I mean, oxidized after *, and so on. This doesn't mean that if you have some zircaloy, unoxidized uranium, that's getting oxidation one way, * tellurium.

I know that during the oxidation the tellurium or something *, and so on.

For instance, in PHEBUS we had reported metal interactions that probably the counts, at least our counts, underestimate, and this didn't appear until nearly 100 percent tellurium release, very high, or 85 percent.

So I think that, from our point of view, we should stick to very high release fraction from the fuel for tellurium.

MR. BOYACK: Right now they are only showing about 1 in 30 percent release.

MR. KRESS: Well, I don't know how much is

retained in the primary system. You can't tell from those numbers.

MR. BOYACK: Even if it was just these two figures, you're saying some fraction of that may be redeposited or --

MR. KRESS: Redeposited in the primary system.

MR. BOYACK: But even at that, that's the maximum. You could have that none of it reacted or redeposited.

MR. POWERS: Some of those numbers reflect some material being deposited in the primary piping system and being fairly irreversibly retained.

MR. BOYACK: So, basically, what I hear is you're not comfortable -- I presume this is all -- you are not comfortable with the current value of the NUREG 1465 numbers. So I guess at this point the thing would be to do to try to ask ourselves what might be the appropriate values and the physics to go along with that. And you're telling me, I guess, that it's not clear what the physics are.

MR. POWERS: The empirical observation is that as long as you keep that fuel so that the fuel environment itself is substantially reduced, there is clad around to suck up the steam, that you don't seem

to get a lot of tellurium release.

MR. BOYACK: Okay.

MR. POWERS: Now where that tellurium is I don't think has been conclusively demonstrated. We have certainly had the Oak Ridge experiments in which they certainly found the tellurium somewhere around the clad. I think a gamma standard or something, but you found it.

When we did the Sandia in-pile experiments, it is a very tricky measurement to do because you very quickly get blinded by the cesium signal, but we deliberately took the scan of the fuel right after the test in a window where we could see tellurium, and we had melted the clad and it had drained off the fuel, and the tellurium was right in the fuel. It was not in that clad. The molten clad pool down at the bottom was tellurium-free.

MR. KRESS: So the clad may just be preventing the oxygen from getting into the fuel.

MR. POWERS: The fuel. I mean that was the conclusion, but it still didn't tell you where the tellurium was. It had to be something, because if it is just tellurium, that sucker would be a vapor.

MR. KRESS: Oh, yes, at those temperatures, it just --

MR. POWERS: So we knew it had to be something, and I have always assumed, well, it must be reacting with the metal inclusions because they form incredibly stable tellurides, but I don't know that for a fact.

So then why does it un-react when it becomes oxidized? Well, it will do that. I mean, that is just the truth of those things.

So now we have the PHEBUS results in which they say they've got tellurium all over the place. They're hip-deep in tellurium in there.

MR. CLEMENT: We can give you more precise figures from our work on the PHEBUS tests; for the VERCORS 4, VERCORS 5, and VERCORS 6 tests, that's 100 percent, more than 98 percent, and 97 percent release of tellurium.

MR. KRESS: That's pretty close to the lot.

MR. POWERS: When we run the Victoria Code on these things, we tend to see the tellurium tied up and it's things like silver tellurides, stuff like that, like it's pre-reactive. In other words, it is already a telluride, so that when it sees the piping system, it doesn't want to turn into iron telluride or nickel telluride or something like that.

Just knowing the environment that Brian Bausher was operating in, he had just been doing experiments on tellurium reaction with nickel planchettes, and of course nickel sucks up tellurium like a sponge. I mean, it really goes after tellurium. So he may have, when he made his comment, been thinking of the nickel in the piping system.

But, I mean, I think for the PHEBUS it's pretty clear. They are running it up out of the fuel along several meters of 700 degrees Centigrade, in canal. I mean, that's a serious nickel.

MR. CLEMENT: But it's oxidized.

MR. POWERS: Well, the surface is oxidized.

MR. CLEMENT: The surface is oxidized. That's why there is no --

MR. POWERS: Well, even oxidized in canal sucks tellurium, and then it goes up through the steam generator tubes and out into the containment. So, I mean, it looks to me like understanding that we have very few actual tellurium measurements from in-pile tests, because it's a tricky measurement to make.

MR. KRESS: In fact, that's one of the few I know of. I don't know what they did in PBM.

MR. POWERS: PBM had a heck of a time with

it.

MR. GIESEKE: That's really dramatic there, Dana.

MR. CLEMENT: Well, there is uncertainty for the VERCORS, if I look at the PHEBUS, if I compare * that is retaining the fuel through other *. So we are 17 percent of tellurium retained in the fuel as compared to 16 percent for cesium and 13 percent for iodine.

MR. LEAVER: You said 17 percent?

MR. CLEMENT: Seventeen percent.

MR. LEAVER: One seven?

MR. CLEMENT: Yes, one seven, as compared to 16 for cesium and 13 for iodine. So it is really comparable and it corresponds to re-entering the lower part of the bundle that was not so hot. Then if you look at the retention in the hot zone that made the in canal, 700 meters long, it was 3 percent of the inventory.

MR. KRESS: Three percent?

MR. CLEMENT: Yes.

MR. KRESS: Okay.

MR. CLEMENT: Okay. So that is our results.

MR. KRESS: Those results argue for a lot

different numbers than we've got up there.

MR. BOYACK: David, why don't you talk to these numbers. Then we can put that up after he has shown that, and then maybe we can talk about it.

MR. LEAVER: This is just for the release of FPT-1 and FPT-1-14, which of course are radiated fuel, and FPT-0 is not radiated.

MR. CLEMENT: You are with respect to your first column --

MR. LEAVER: Tellurium release fraction.

MR. CLEMENT: Okay.

MR. LEAVER: It is 79 percent, 53 percent, 21 percent.

MR. CLEMENT: Seventeen percent.

MR. LEAVER: Seventy?

MR. CLEMENT: No, 17.

MR. LEAVER: Seven zero or 17?

MR. KRESS: It's the released.

MR. LEAVER: Released fractions?

MR. KRESS: This is the amount released.

MR. LEAVER: That's to the fuel?

MR. CLEMENT: It is .83.

MR. LEAVER: Oh, it's .83?

MR. CLEMENT: Yes, .83.

MR. LEAVER: All right, well, I had

something that said 53, but all right.

So it sort of correlated with the percent zirc oxidized, although we are getting a very high release with a little over half of the zirc oxidized. But even more significant I think is the time and temperature. It does seem to be strongly correlated to that.

I don't know -- the NRC, it was research that ultimately decided what to put in NUREG 1465, and they got, of course, the work that Hossein did and then they got input from other people, and that is where these comments from Brian Bausher came in.

But I suspect that TMI had something to do with where they ended up because that is a heck of an experiment. Even though we can argue that the accident wasn't typical, it's an accident on a reactor and that's what they got.

I think the time and temperature are significant. If you hold the fuel and the melt and the debris and you let it sit there and cook, then that would suggest that at some point it's going to go. Now how much of this sort of phenomena we want to push into that 1.4 hours, well, I guess that's the question.

MR. BOYACK: Okay, I wonder if we could go

ahead and see the French data one more time.

MR. CLEMENT: So that is PHEBUS FPT-1. Here this foam is what is retained in the fuel. So it is tellurium, 17 percent; cesium, 16 percent, iodine, 13 percent. So very similar.

Then we have what is deposited in the zone just above the fuel. So we have only 5 percent for iodine. That is not condensed in that zone. For cesium and tellurium, roughly 26 percent. That is because the vapors are condensing in that zone.

Then we have aerosol, and we are convinced that that our rates are rather small. So that's the overall value.

Now when you get kinetics --

MR. LEAVER: Can I take a look at it?

MR. CLEMENT: Yes, sorry.

MR. LEAVER: If I can just hold it?

MR. CLEMENT: So this is cesium released. This one is tellurium. Okay? What you can see here, this is the trapping value of zircaloy foam, non-oxidized zircaloy. Both go in the same direction. That's the degradation process *. This is why we are concerned.

So why don't we have sufficient regulation level? We think the release of tellurium would be

similar to that of other tellurides.

MR. BOYACK: So you're showing about 85 percent? I'm not sure which one --

MR. SCHAPEROW: One of the things to think about, I think, is after about an hour or so, they have some separation. We are talking, as Dave said, about a 1.4-hour heatup time. So even around 14,000, 15,000 seconds, one's a bit below the other, but it's not a huge difference.

MR. BOYACK: All right.

MR. LEAVER: This is released from the bundle?

MR. CLEMENT: This is released from the bundle, yes.

MR. BOYACK: So then you have to decide what was deposited, but the release is 85 percent, roughly.

MR. LEAVER: Bernard, how would you explain, in light of this and the intuition and insight that this gives you, how would you explain what happened at TMI with tellurium? Have you thought about that?

MR. CLEMENT: No.

MR. LEAVER: No?

MR. BOYACK: So what did they do to do the

TMI measurements? They took some --

MR. LEAVER: Well, they did a lot of things. I mean, they measured what was in the containment. They took the entire what was left of the core out and did a lot of examination of what was left. I mean, perhaps Dana could speak to the measurements they actually made, but their guys spent 10 or 15 years of their life doing those measurements. It was very, very carefully done, and there are a lot of papers written on it.

So I think maybe there is some uncertainty on the 2 percent measurement for TMI, but certainly not -- I would be surprised if it was too large of an uncertainty.

MR. SCHAPEROW: That's 2 percent of the entire fuel --

MR. LEAVER: Of the core inventory.

MR. SCHAPEROW: Released from the fuel?

MR. LEAVER: Probably that's okay for us to assume, that it was released from the fuel. I'm actually not sure if they say that's released to containment or released from fuel, but it doesn't really matter. If it is 2 percent for the fuel, then it is 1 percent to containment; if it is 4 percent to the fuel, then it is 2 percent to the containment.

But the idea is that they were looking at 2 percent.

And then SFD test, which admittedly was a lower zirc oxidation, but, nonetheless, they saw the same kind of thing from SFT-1-4.

MR. BOYACK: To this point in the tellurium discussion, what we have really done is we have gone back and reviewed the information. So having done that, now I would like to come back and go around the table and see what people have to say by phase. We have the four phases, and sometimes I get lost because I don't track when you change from phase to phase, but if you would tell me your thoughts for the four phases, we'll go around the table. Tom?

MR. KRESS: My thoughts are that --

MR. BOYACK: Starting with gap release.

MR. KRESS: Gap release, I don't care; it's zero.

MR. SCHAPEROW: You might save yourself some time and see if anybody thinks there is a significant gap release for tellurium.

MR. BOYACK: Well, I have to enter it anyway. Okay. Early in-vessel?

MR. KRESS: I'm inclined to put a lot of weight on the PHEBUS results and say that the tellurium releases in-vessel are very high. I don't

know how high yet, but this is significant because tellurium decays to iodine. It is a pretty serious isotope. If we have to deal with it in design basis space, we ought to know where it is coming out. There is a lot of it there, and I am inclined to say we had better think about those PHEBUS experiments and put a lot of weight on them.

MR. BOYACK: Is there a need here underlying this? It seems to me this is one --

MR. KRESS: There is a need to explain the difference between what PHEBUS gives and what you get in these little separate effects tests, where you heat up fuel in a furnace, and all of the tellurium gets trapped in the fuel and doesn't get released until you oxidize 95 percent of the clad. Why do you get that in these experiments and why does PHEBUS get quantitative release summarily? What's the difference?

MR. CLEMENT: At VERCORS we got a lot.

MR. KRESS: Oh, you got it in VERCORS.

MR. CLEMENT: Yes.

MR. KRESS: So there is an inconsistency between VERCORS and, say, the ORNL tests, and I don't know why the difference, but TMI I don't think that was a very good experiment. I don't think TMI was a

very good experiment.

MR. SCHAPEROW: ORNL tests, he said various ORNL.

MR. BOYACK: Okay.

MR. KRESS: So, primarily, the conception that you don't get tellurium release until you get 95 percent of the clad comes out of the ORNL tests. Here we've got two sets of tests, one of them in-pile and one of them separate, that contradicts that. We need to know why, but that sure puts a big question mark on the ORNL tests.

MR. POWERS: I mean, I think ORNL and SFD tests.

MR. KRESS: Oh, the SFD tests also did that.

MR. POWERS: Those guys -- you guys at ORNL started focusing on it because they weren't seeing the release in the SFD.

MR. KRESS: Why if something is that volatile, why didn't you see it? That's right.

MR. SCHAPEROW: SFD, Severe Fuel Damage.

MR. POWERS: Somehow in our in-pile tests that we did we didn't get the tellurium out. It was still in the fuel.

MR. LEAVER: The SFD tests?

MR. POWERS: No, the ones that were done in Sandia. There were some specialized tests done to look at the highly-reducing conditions, and they did a couple of tests. Tellurium did not move.

MR. KRESS: There's a need to reconcile these contradictory results, but in the meantime if we have to specific a source term for design basis, I am one of these people that falls down on the conservative side and says, let's use the PHEBUS results, because that gives you more problem in what you have to do.

MR. POWERS: Well, I tend to look at it a little this way: that we were really floundering around with the SFD tests. I mean, they were basically core degradation tests, and they monitored fission products. We really didn't know what we were doing, whereas PHEBUS tests were dedicated, directed from inception to look at fission product release and transport, and they had lots of help in ensuring that they did well-designed test.

MR. KRESS: And, similarly, the Oak Ridge test --

MR. POWERS: Is that how you describe it, "lots of help"?

(Laughter.)

MR. KRESS: Similarly, the Oak Ridge test had trouble finding tellurium because there wasn't much of it in that fuel going in and they didn't re-irradiate. So they really had problems with their tellurium analysis.

MR. POWERS: Well, that's another thing to bear in mind, is that the tellurium, the radioactive tellurium you are seeing in the PHEBUS test is short-life stuff. If it has been generated, it perhaps has not had the opportunity to go around and find something to tie itself up with.

MR. KRESS: And that is another possible explanation for the difference. The question is, what would a reactor do? Maybe that short-life stuff --

MR. POWERS: I mean, what I'm going to argue is that, as we move to high burnup fuel, we take the tin out of the clad. As we consume the metallic alloys that can react with things by inherent oxidation, that we're losing --

MR. KRESS: You're moving towards more tellurium.

MR. POWERS: So we are going to move much more toward the PHEBUS result.

MR. BOYACK: So, basically, what I said for Tom for the early in-vessel release of tellurium,

which decays to iodine, those PHEBUS tests were designed and directed to fission product management. There is justification for basing source term applicability on the PHEBUS results.

MR. KRESS: That's pretty much what I think.

MR. BOYACK: Okay. So if you were basing it on, for this early in-vessel phase, what do you use for the --

MR. KRESS: I would be tempted to make it just like the iodine phase that we had, except I would have to rethink a little bit about retention in the primary system, but I would also be guided over the PHEBUS tests, that if your primary system is oxidized, maybe the tellurium doesn't have that reading. So I would be tempted to make a lot like the iodine.

MR. POWERS: You know, there is another thing about the Severe Fuel Damage tests. When they blew the control rods, that material slumped promptly. Control rod material is highly reactive toward tellurium. If that sucked it out and it fell out of the hot zone very quickly, you would not get --

MR. KRESS: It sequesters it.

MR. POWERS: -- it sequesters; you wouldn't get any release; whereas, in PHEBUS it tends

to hang up.

MR. KRESS: That's interesting.

MR. POWERS: I think what you are going to see, I mean all of the analyses that I have seen on the primary piping system always show the tellurium tied up, silver-Indian tellurides and things like that, I mean you see lots of tellurides, like it's already reacted from that --

MR. LEAVER: Not just tin telluride, but there's --

MR. POWERS: In fact, I can't recall ever seeing any tin telluride. It's always Indian telluride or silver telluride, but I can't remember the calculations that well. There may be tin telluride, but I just don't remember.

MR. LEAVER: Well, that's what I remember, but there's a lot of Indian in the PWR's.

MR. KRESS: Yes, but they tend to go first, and I'm not sure if they're still around -- I don't know; it depends on how you calculate the core melt progression, but those things tend to go first and get dispersed. Maybe they are still there; maybe they aren't by the time the tellurium --

MR. POWERS: Well, they tend to be all over the place because you've got control rods going

off sequentially in time.

MR. LEAVER: There's just a lot of mass of Indian, a lot of moles --

MR. KRESS: Oh, yes.

MR. LEAVER: -- relative to the moles of tellurium.

MR. KRESS: Yes, a lot there.

MR. LEAVER: So, yes, some of it could go off. It is hard to imagine that there wouldn't be some of it around.

MR. POWERS: It depends on two things. You have this -- the control rods are going off as a function of time, and then if you model it to slump down out of a hot zone very quickly and quench on the lower support plane, you don't have very much. If you spray it, interact a little bit with the clad, hang it up in things, then you've got silver all the time.

MR. LEAVER: But, of course, things aren't homogeneous.

MR. POWERS: The answer is probably, yes, it does all those things.

MR. BOYACK: Okay, we've got you through two of the phases now, but these are hard phases. Do you want me to stick with and just take the first couple of phases and go around the table, and then

come back?

MR. KRESS: Yes, let me think about the other things.

MR. BOYACK: Okay, Jim, let's start with the gap release for tellurium. Your thoughts?

MR. GIESEKE: I'm go with Tom on that. He's saying -- what is it, zero? It's zero.

MR. BOYACK: Yes. You've heard the dialog on the early in-vessel releases. What are your --

MR. GIESEKE: It's really confusing is what it is

(Laughter.)

MR. BOYACK: Really confusing.

MR. GIESEKE: I'm inclined to think that there should be a lot of release, but I don't know what's going to happen to it. It wants to get tied up, and whether that happens in-vessel or in transit is not clear.

I was wondering if we know the computed retention was in the RCS for tellurium.

MR. LEAVER: Yes, I've got it here.

MR. GIESEKE: Do you have those?

MR. LEAVER: Yes. This is the transmission, not the retention.

MR. GIESEKE: Oh, okay, what was that?

MR. LEAVER: These are the numbers under the tellurium column for the different environment conditions.

MR. NOURBAKHSH: They are the 1150 elicitation.

MR. LEAVER: Yes, those are the 1150 elicitations.

MR. NOURBAKHSH: Those are quite high. If you look at chad melt-type calculation, you are getting -- you can bond it like 50 percent.

MR. GIESEKE: So the retention is similar to the iodine, cesium --

MR. NOURBAKHSH: No, the total release would be like 50.

MR. GIESEKE: You mean the retention fraction, the retained fraction?

MR. BOYACK: If you are going to talk, stand by the mike.

Jim, if you were forced to give a value, would you -- pardon me, encouraged to give a value -- do you have a sense of what release fraction you would use for the early in-vessel phase?

MR. GIESEKE: Well, I'm inclined to think it might be similar to the iodine.

MR. BOYACK: Okay, Dana?

MR. POWERS: Tellurium is one of the first ones where I think we started seeing really profound --

MR. BOYACK: Just a minute.

MR. GIESEKE: Okay, fraction released from the pool goes into containment. Those are fairly high, aren't they? There's not too much --

MR. BOYACK: Okay, thanks, Dana.

MR. POWERS: Tellurium is the element where I think we start seeing profound burnup effects potentially intruding, and especially as you move toward 75-gigawatt-day fuel, I think that you will have lost your oxygen potential buffering capacity. In that light, I would think that I would need to include some tellurium release in the gap release. I don't think it is large, but I will argue for something on the order of 1 percent. You could talk me into a half a percent without much trouble.

But I want to emphasize that this is an area where I think we need experimental measurements of fission product release during design basis accidents.

MR. BOYACK: So the need is -- part of the problem is I'm typing while you're talking, so I missed it. What was the latter part on need?

MR. POWERS: We need experiments with high burnup fuel under LOCA conditions and measuring fission product release, both gap release, which you only get kind of in the experiment, but you'll get something, and --

MR. KRESS: Is that PHEBUS LOCA?

MR. POWERS: Well, I mean, they may be able to do them in the single-rod experiments. I don't see how if they don't re-irradiate. You have to be careful of re-irradiation because then you're getting short-life stuff that hasn't had a chance to move around. So you may get an upper bound there, but you have to be careful, and I think we ought to be measuring that. I mean, why guess?

PHEBUS LOCA experiments are ones that haven't been done yet. They're proposed.

MR. BOYACK: Okay, so you're talking about a gap release of half to 1 percent and the need for experiments. Okay.

Is that what you have in the gap release phase and your other earlier comments --

MR. POWERS: I'm ready to move on to early in-vessel release.

MR. BOYACK: Okay.

MR. POWERS: I just can't ignore the

PHEBUS results. So my tendency is to say, gee, I believe I'll just take the average of the iodine and cesium or that fraction that gets released from the fuel in-vessel and transported through the reactor coolant system.

MR. BOYACK: Take the average of iodine and cesium that is released, transported, and used for -- is that what --

MR. POWERS: Yes, yes, and say it is going to be right in between those two someplace. I swallow the PHEBUS results, and I mean I've watched those tests from their inception. It's kind of hard for me to neglect them. They were well-designed. They seemed to yield a pretty incontrovertible result.

MR. BOYACK: What is that, roughly 30 percent now?

MR. POWERS: Yes. See, this is Tom's 30 percent that he has been arguing for all along. We're going to give it to him here. I'll just put it in a different box, Tom. We were going to get there.

(Laughter.)

MR. KRESS: I don't mind as long as we get it up there somehow.

MR. BOYACK: You mean any place you get 30 percent?

MR. KRESS: Yes.

(Laughter.)

MR. GIESEKE: Any 30 percent will do.

MR. KRESS: If you could do that on my IRA, I would appreciate it.

(Laughter.)

MR. BOYACK: We talked about reconciling the differences, but is there any need for additional experimental programs that are not already planned?

MR. POWERS: Again, I think that we should be doing experiments with high burnup fuel under both severe accident and low conditions.

MR. BOYACK: All right, Bernard? We're sort of dealing with the first two phases here, gap release --

MR. CLEMENT: Okay. So for the gap release, we don't think about what *. We did not consider any gap release, but I understand the arguments that maybe there could be some, but I don't know.

MR. BOYACK: Okay.

MR. CLEMENT: Concerning the other phases of in-vessel release, so without distinction between phases, as for other volatiles, okay, we arrive with the same figure of overall release; that is, for the

containment, 70 percent fuel deposition and 70 percent for the containment. They should be separated in the same way.

MR. BOYACK: Same way. Would you remind me what the breakdown of the 75 percent was?

MR. CLEMENT: Seventy. We did not make any breakdown.

MR. SCHAPEROW: The same as cesium is what he said.

MR. CLEMENT: Yes. We personally did not make any breakdown.

MR. BOYACK: No, I wasn't talking about the breakdown between the phases, but I was referring to what was deposited and what was containment.

MR. CLEMENT: Okay. It should be clear: 100 percent release from fuel, and 70 release to containment.

MR. BOYACK: Okay.

MR. CLEMENT: We say 100 instead of 95 because of the 5 percent we did not consider as released during the gap will be released anyway.

MR. BOYACK: So 100 percent is released from the fuel, and released to the containment was -- was that the 75 percent?

MR. CLEMENT: Seventy percent.

MR. BOYACK: Seventy percent; I thought I heard you say that. Okay.

Anything else?

MR. CLEMENT: No.

MR. BOYACK: Okay, Dave?

MR. LEAVER: I think that the FPT-1 data is new since 1465 and is significant. I would just observe that from the integral release curve that it tends to be on the order of maybe two-thirds to three-quarters of the cesium release right until the very end, when they both kind of jump up to 85 percent or so on that curve.

So I think, in my view, that the tellurium number should come up. This is primarily, I guess, a relooking at 1465-type observation as opposed to effect of higher burnup. I think I would make these comments irrespective of the higher burnup, and I think the FPT data is the main reason to make this some kind of increase.

But if you observe, first of all, that the tellurium release is on the order of two-thirds to three-quarters of the cesium on that integral release curve, and the second thing is while TMI and SFD are old, I don't think we should just ignore them completely.

Oh, I guess another observation would be the fact that the FPT was at temperature for maybe twice as long as what we're saying our in-vessel release duration is, roughly, or 1.7 or 1.8 times. So at least acknowledging that TMI and SFD results and the fact that we're looking at this early in-vessel release as a significantly shorter period than the FTP-1, but balance that with the fact that the release rate seems to be higher than what came out of 1465, perhaps something a little bit less than the cesium, I would say a number maybe like for tellurium released from the fuel, maybe 20 to 25 percent with some amount of retention.

I don't see a basis for deviating too much from the factor of two, which tends to be kind of a mish-mash-type number, considering a lot of different sequences conditions, which would lead me a number for tellurium of, say, into the containment of somewhere in the neighborhood of 10 to 15 percent.

MR. GIESEKE: Just as a point of information, the computed values for transmission of tellurium were roughly 80 percent, for better or worse.

MR. LEAVER: Oh, they were?

MR. GIESEKE: Yes.

MR. LEAVER: These are transmission is 15 to 20, maybe 30 percent, according to this. This is from the expert -- do you mean this table?

MR. GIESEKE: No, his computations. The computations.

MR. LEAVER: Which computations?

MR. NOURBAKHSH: The table you are looking at are NUREG 1150, the mean values. They have looked at the result of Source Term Code Package, but they tend to look at them like upper bound. When they quantified the distributions, it came lower.

MR. LEAVER: So, like all these numbers, there's a range, and, yes, these are median and means, but I guess a factor of two. I'm not smart enough, I guess, to say that this number should be different than a factor of two.

So, with a release from the fuel of 20 to 25 percent, based on the FPT result, but acknowledging that we are looking at duration on the order of 1 to 1.5 hours, and remembering the fact that there was this event called TMI and the SFD tests, I guess I would come in at 20 to 25 percent, with a factor of two reduction, giving me 10 to 15 percent.

MR. BOYACK: Okay, Dave, on the gap release, I think all your comments with respect to

early in-vessel and then subsequent to that --

MR. LEAVER: Yes, I would have no basis to change the gap release, though we might make it something other than zero. We could make it some really small number, but I don't think -- I wouldn't adjust it to a number like 1 percent or something, if we want to make it something other than zero. That would at least qualitatively acknowledge that maybe we think something would come out during the gap. I think it is hard to defend zero for the gap release; that's for sure.

MR. BOYACK: Okay. Now let's walk through a couple of these and see if we can't wrap these two.

The first of them is the gap release, which is somewhere between zero and 1.

MR. GIESEKE: I said a half is somewhere between zero and 1.

MR. BOYACK: I guess that's right.

MR. KRESS: Probabilities are between 0 and 1.

(Laughter.)

MR. BOYACK: Some of us are getting near the end of the day, I can tell.

(Laughter.)

Okay, the question, I think, is basically:

Do we want a non-zero value? Originally, we started out over here, and then there was some discussion about the possibility of perhaps having a non-zero value, and Dana would live with a half percent and David would live with a half percent. The question is, anybody else --

MR. GIESEKE: I'll live with a half percent.

MR. KRESS: Five percent is all right with me.

MR. BOYACK: Okay.

MR. POWERS: I think you need another zero in there.

(Laughter.)

MR. BOYACK: Okay, I'm sorry. Now we come to the early in-vessel, and there is a span here, if I've recorded right, between 25 and 35 percent, 20 to 35 percent.

MR. SCHAPEROW: Actually, Dave had about 10 percent.

MR. LEAVER: No, not 20.

MR. BOYACK: Oh, I guess what happened is, when I heard you, I interpreted you to be telling me something down here.

MR. LEAVER: Okay.

MR. BOYACK: So this is wrong.

MR. LEAVER: No, my thought was early in-vessel.

MR. BOYACK: So it is 10 to 15 percent, right?

MR. LEAVER: Right.

MR. BOYACK: You've got to watch the typer all the time.

(Laughter.)

Okay. So now what I would like to do is try to figure out how to deal with this. Actually, we had three of the panel members were up in the area of 30 to 35, and I guess the French don't break it down. Okay. But you get 100 percent release from fuel and eventually delivery to the containment of 70 percent, which is, again --

MR. CLEMENT: Not so far from the total.

MR. BOYACK: Okay. This thing is starting to get warm evidently because, if you can see, I am losing some of the lines here (referring to equipment).

So we have Dave who is down at a much lower value.

MR. GIESEKE: I would like to propose a higher value. I would like to look at Tom's

calculation for this sort of burnup. He gets 100 percent release predicted. If you take the computation of 80 percent of transmission, then you are looking at 80 percent to the containment as an upper bound. I think that is probably an overestimate, but I think that is consistent with what the French are finding in their experiments. I think it is a little high, but I would like to see the number higher.

MR. BOYACK: Higher than 35?

MR. POWERS: Understand that you're working in this phase with only 60 to 70 percent of the core melting down. So if you take --

MR. GIESEKE: Okay, you can take that times 60 percent, if you want.

MR. POWERS: So if you take 70 percent of the core and 80 percent transmission, that would give you a 56 percent release.

MR. GIESEKE: Yes.

MR. KRESS: So perhaps we can play with that.

MR. POWERS: That's what he means by -- he says 20 percent of that being plated out.

MR. GIESEKE: Yes.

MR. POWERS: You're going to pasted by the

transcriber.

MR. GIESEKE: I'm just saying I think to go down to 10-15 percent is lower than what I would say. My 60 percent Dana is saying is probably on the high end. So I think it is closer to maybe a half; 40, 50 percent would be a better number than the low end, especially since it is supported by the experimental results from the French.

MR. BOYACK: So is that number different than what you've got here?

MR. GIESEKE: It needs to be modified for some fraction of the core, for the fraction of the core involved, say 70 percent of the core involved in this in that time period.

MR. BOYACK: Yes, what I'm asking, Jim, is I'm having a hard time tracking whether this number is the same as you offered initially or changed.

MR. GIESEKE: I didn't give you a number initially.

MR. BOYACK: Well, sure you did. See, it's right here.

(Laughter.)

MR. GIESEKE: That was from the fuel.

MR. BOYACK: Oh, this is a release fraction from the fuel, right?

MR. GIESEKE: Right. But it could be higher than that even, if you want to use Tom's calculation. He calculates 100 percent, but being a little skeptical, I'm giving 90 percent or something like that. So you take .9 times .7 times .8 and what do you get? Fifty percent. Now that still may be high. You know, it seems like a big number to me, but it tends to be more supported by the French data is all I'm saying. There is some logic behind those numbers.

I'll start talking down from that number now, if you want.

MR. BOYACK: And I'm putting 50, is that right? See, I'm not quite sure whether I'm missing something.

MR. GIESEKE: Yes, take out "from the fuel" and just say 50 percent.

MR. KRESS: That's the number that goes in the box.

MR. BOYACK: Yes, that's the number I was after. Okay, so now we're between 10 and 50 percent. You heard some of the dialog. Does any of this change your mind or are you still back where you were?

MR. LEAVER: No, I guess not. I think if we believe cesium to be 25 percent in-vessel, and if

we think we're talking about a release duration on the order of 1.4 hours, and then the reason, at least one of the reasons for that release duration is we are saying this is a recovered accident. That is what we say in design basis space, and that is the reason why we don't consider the ex-vessel and the late in-vessel release.

So with that release duration and the fact that we felt that the right number for cesium was 25 percent, and if you look at the French data, the tellurium is lagging the cesium release such that you get roughly three-quarters or so of the cesium release, although at the end, as I said, it catches up, but that's out of like 6500 seconds or so, I don't see how we can justify a larger number for tellurium unless you believe tellurium isn't retained in the RCS.

If you look at the French results, it shows about the same retention. Now I'm not personally -- I don't feel like I am smart enough to say that the retention would be different.

MR. GIESEKE: The French shows -- what did they say -- 70 percent?

MR. LEAVER: Well, I was just looking at --

MR. GIESEKE: It's a 30 percent retention.

MR. LEAVER: Yes, I was just looking at the -- if you look at the cesium, this is for FPT-1, the cesium line and the tellurium line, the percent in the upper plenum and the percent deposit in the hot leg, and what-not, look roughly the same.

So I would say, even if you were to say three-quarters of our cesium number, we would be close to 15 percent. I mean, maybe it is a little more. Also, I just don't feel that we should totally throw out what happened with TMI and SFD. I think we want to consider a range of conditions and sequence types and that sort of thing. So that is kind of what we are doing, and those two data points, in my mind, tend to kind of make me want to tamp the number down a little bit or at least accept the kind of logic that I just went through that would give us a number of, if not 15 percent, then between 15 and 20.

MR. BOYACK: Okay, I don't know where to go with this one, but are there any other comments? As you heard the dialog back and forth, is there anything that changes anybody's mind?

(No response.)

Okay, so this is a case where we get a large range.

MR. POWERS: It's a large range, but understand, even if we took the bottom of that range and made some radical change in the way we look at the source term, I mean even if you were to take Dave's number which is at the bottom of the range at 10 to 15 percent, it is three times what you've got now, but, more importantly, it is a big difference in the way of looking at things.

MR. SCHAPEROW: A big difference in what?

MR. POWERS: How you look at things.

MR. LEAVER: In terms of tellurium.

MR. POWERS: That's right, in terms of tellurium.

MR. LEAVER: Are acknowledging, I think, that there is that difference. Yes, that's right.

MR. POWERS: We've got some really good data points now.

MR. LEAVER: There is some significant data that we didn't have a couple of years ago.

MR. POWERS: A guy can come along and make an argument and say, okay, you've got a core that's like a third high burnup fuel and two-thirds lower burnup fuel, and if you said, well, lower burnup fuel behaves just like TMI, releases squat, and higher burnup fuel behaves a whole lot like PHEBUS and

releases a bunch, you come up with these kinds of numbers, I think.

MR. LEAVER: Wait a minute. Are you saying that the FPT-1 was high burnup fuel?

MR. POWERS: Well, it was not, but it was up around -- I think FPT-1 was 20 --

MR. CLEMENT: Twenty-four.

MR. POWERS: Twenty-four?

MR. CLEMENT: Twenty-four.

MR. POWERS: Gigawatt-days per ton.

MR. LEAVER: Twenty-four.

MR. POWERS: Whereas, TMI is 8.

MR. LEAVER: oh, core average was 8?

MR. POWERS: Yes.

MR. KRESS: That is why TMI shouldn't be extrapolated to these kinds of numbers. The burnup is just too low.

MR. LEAVER: Well, yes, I don't think we are extrapolating it. I am just saying I can't bring myself just to completely ignore it. So it tends to want to at least impact a little bit on my thinking.

But I agree with what you said, Dana. I think that we are acknowledging a different way of looking at tellurium than we did five years ago when we did 1465, or eight or nine years ago when we did

the thinking for 1465.

MR. SCHAPEROW: Twelve years ago.

(Laughter.)

MR. POWERS: It was a long time ago. As I said, for the NUREG 1150 elicitations, I said, for tellurium I'm totally uncertain, drew a diagonal line --

MR. KRESS: Just straight across.

MR. POWERS: I'm totally uncertain. Today I think I would end up drawing maybe not a diagonal line, but it would still be a vast uncertainty associated with this.

MR. SCHAPEROW: I see your diagonal line right there. There it is.

MR. POWERS: Yes, that's me.

(Laughter.)

MR. SCHAPEROW: Somebody thought it was very low. This is a very low fraction.

MR. POWERS: Well, you have to understand at the time that they weren't seeing anything in the SFD tests. So they asked Oak Ridge to take a look particularly at it, and Oak Ridge couldn't find it. Then they came up with this clad thing. They wrote a paper on it. I mean it's in Nuclear Technology or something like that.

At the same time we were going in an era from a period in the March code where we said every bit of the clad gets oxidized during the in-vessel core degradation to saying, no, wait, lots of this escapes oxidation in the core and comes ex-vessel. So those combinations of things led at least one guy to say, oh, it's very low release inside.

But, understand, what you don't get inside, you bring it ex-vessel and my melt concrete gets it for you. I mean, if there is one fission product that I think we know well experimentally on melt concrete interaction and calculate well on melt concrete interaction, that is tellurium.

MR. KRESS: That is why I never understood that 25 percent.

MR. POWERS: Well, I think that is 25 -- well, I don't know why. I guess I don't understand it either.

MR. BOYACK: Let me ask a question at the extremes of the values. What is the user impact of a number now that goes into the source term for high burnup fuel of 15, even though it is reflecting knowledge not on high burnup fuel, but just on tellurium? What is the impact on the 15 percent value and what is the impact on a 50 percent value? Is

there any significant real-large impact on the end-user?

MR. SCHAPEROW: Well, as Tom pointed out, one of the tellurium isotopes decays to Iodine-132. Generally, right not it is pretty small.

MR. LEAVER: What's the half-life of I-132? The half-life of Iodine-132, like about 90 minutes?

MR. SCHAPEROW: If you multiply that --

MR. POWERS: No, it's a few hours.

MR. LEAVER: A few hours? Five, six hours?

MR. POWERS: I think it is longer than that, but I can't remember exactly.

MR. BOYACK: The reason I am asking is if relatively insensitive to one end or the other, then maybe you can just pick one of the values and say, okay, it's not a matter that is really highly sensitive on the impact, but if it has large impact, then there is large uncertainty.

MR. GIESEKE: Let me reconsider my number. I think I overestimated because I didn't consider the distribution of high burnup in the fuel.

MR. BOYACK: Yes.

MR. GIESEKE: If I go through that

estimation of the fractions of the fuel for high burnup compared with lower burnup, I can get my estimated number from the 50 down to 30 percent.

MR. KRESS: So he is right online with me. in saying it is about the same as the iodine.

MR. GIESEKE: Yes, I forgot about that when I made my first calculation. Of course, that puts it to be not consistent with the French line, or less consistent.

MR. POWERS: Well, we will get back up to the French when we do the other two sections. We will catch up with them.

(Laughter.)

We're just slow; that's all.

(Laughter.)

MR. KRESS: What's the half-life of it?

MR. POWERS: 2.3 hours. But, effectively, Iodine-132 has an 88 half-life.

MR. LEAVER: Well, Tellurium-132 has a 78-hour half-life.

MR. POWERS: Seventy-eight? All right.

MR. LEAVER: That defines the half-life for iodine.

MR. POWERS: Tellurium-132 in an accident acts like it has a 78-hour half-life.

MR. BOYACK: I am not sure we know the answer to the question you are asking, which is --

MR. KRESS: I thought I would just try that question.

MR. LEAVER: It is too bad we didn't come to this at two o'clock because I probably could have made a couple of phone calls and had a couple of quick calculations done where we could probably tell you pretty darn good what the impact that would be. It is not trivial.

MR. BOYACK: Well, I don't even know it's a problem for us.

MR. LEAVER: The 50 percent number I think I could be a potential disaster.

MR. BOYACK: Well, the 50 percent just went away to 30.

MR. SCHAPEROW: Imagine that.

MR. LEAVER: A factor of three. If we went from 5 percent to 15 percent --

MR. BOYACK: See, this isn't my call. So the question is: How do we deal with this?

MR. SCHAPEROW: I think we should sleep on it because it's five o'clock.

MR. BOYACK: Well, yes.

(Laughter.)

MR. POWERS: Yes, I don't mind sleeping on it, and I emphasize what I said earlier, that even if we take Dave's number and put it in there, you're telegraphing then that you really need to do some serious study in this area because you just made a huge change in this thing. It calls attention to it. Maybe we need to sleep on it and just go on and do the others so that we catch up with the French, you know. Those guys are always ahead of us.

MR. LEAVER: They were ahead of us when they built standardized plants.

MR. POWERS: Yes, that's the thing. Ever since they went to standardized plants, they've been ahead of us.

(Laughter.)

MR. KRESS: Now we are trying to --

MR. POWERS: Well, we gave up. We just imported them and had them make the MOX fuel for us.

(Laughter.)

We will never catch up on MOX. We should buy theirs; it's easier.

MR. KRESS: We should have listened to them, shouldn't we?

MR. POWERS: We would, but they were speaking French all the time.

MR. KRESS: Yes.

(Laughter.)

MR. LEAVER: And then they dropped the "s," and that really confused us.

MR. POWERS: Yes. They said, "l'reactors" and "reactors" and we thought they said "la reactor," and thought they were only building one, you know.

(Laughter.)

MR. KRESS: But they kind of screwed up on the liquid metal. We'll forgive them for that, right?

MR. POWERS: Well, they built one, as opposed to us.

(Laughter.)

It is just our arguments that had holes in it.

(Laughter.)

MR. SCHAPEROW: If you can go upstairs for like five minutes, we can get this stuff printed out, I think. I can plug you into a printer up there, if you would like.

MR. POWERS: Hey, that would be kind of neat.

MR. SCHAPEROW: We'll be back in about five or ten minutes, hopefully, with the recording of today here.

(Whereupon, the foregoing matter went off the record at 5:09 p.m. to reconvene the following day, Thursday, December 13, 2001.)