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NUCLEAR REGULATORY COMMISSION

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SOURCE TERM APPLICABILITY PANEL

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THURSDAY

FEBRUARY 21, 2002

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ROCKVILLE, MARYLAND

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The Panel met in Conference Room 4-B-6, One White Flint North, Rockville, Maryland, at 8:30 a.m., Brent Boyack, moderator.

PRESENT:

BRENT BOYACK
BERNARD CLEMENT
JIM GIESEKE
TOM KRESS
DAVID LEAVER
DANA POWERS
JASON SCHAPEROW

A-G-E-N-D-A

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

8:40 a.m.

MR. BOYACK: Let's begin the meeting this morning. While I'm booting up, Steve Lavie has, as he gently said, has talked to some of his staff, cohorts and they have declined to come and talk to us but he has all the information you could ever want.

Why don't you go ahead and download that for us.

MR. LAVIE: What I heard from the people I talked to at Racka Systems is that the situation isn't as quite as easy as it has been characterized. GE is getting the power increases by largely moving higher enrichment fuel into periphery areas around the core in an effort to flatten flux.

However, in plants that have adequate margin of critical power ratio, they are not having to do that. They can raise the overall power of the core without approaching the critical heat flux. It isn't as easy as -- it isn't just all across the board flattening the flux. Some plants are getting the same shape. Some are getting certain degrees of flux flattening. It varies from reactor to reactor.

Now, one of the points that was pointed out to me, however, is because of the need to get flat

flux in the periphery of the core is fuel assemblies are not staying in the core more than two cycles at the present time so these assemblies will never get high burnup. They're not going to be there long enough. I don't know how that factors into our discussions here.

The individual I talked to, unfortunately these people don't get involved with the severe acts of aggression calculations. Jason is the best person to get along with those folks. They questioned how the decay heat is calculated in these codes. If decay heat was calculated more of an average method, there may be no change.

Similarly, the parallel, I think, here is on the core inventory. The origin code calculates an average flux across the core. It's based on the total power averaged across the core. We then apply peaking factors for those analysis where we're not taking all of the assemblies. In LOCA, however, we're just taking the average.

My initial concern I expressed is that I'm not sure that the flat flux -- the flux flattening will have a significant impact. It was also pointed out that when the source term work was done, they picked representative reactors and that our fleet of

reactors varies from the small units such as Oyster creek to large units such as Quad Cities.

There is more than a 20 percent power difference. There's a factor of four difference in some of the sizes of some of these reactors. Yet, we only have one source term. So the people who did the 1465 must have done something to massage that difference on the core degradation based on power. I know it doesn't answer the question and it probably throws more questions into the mix. That's what I was able to find out.

MR. KRESS: That's the only thing that affects is the timing. It doesn't affect very much the fractional releases most of the time.

MR. LAVIE: I think the decay heat is -- you know, if you track back, at least to the fission products, it's largely going to be due to diffusion pellet as it mounts. That is directly related to temperature. If the total Q in the core -- you know, we string it out flat and the Q has gone up but I'm not sure that those extra pieces of the periphery that we've now brought in will add that much to diffusion.

MR. KRESS: The source term in 1465 is real and almost totally by the oxidation in burnup. Decay heat doesn't enter into it. It enters in the

house as long as it takes you to get there. That may be different when you get to the high burnup and MOX because we're talking about releasing earlier and lower temperature. In that case, you start releasing significant quantities due to decay heat so you have a qualitatively different view of the source term.

MR. LAVIE: Because of the current core designs, these assemblies will not be in the core more than two cycles. If you look at the older fuel management schemes in the three region cores, we are removing the stuff burned out further and further on the periphery. I think this approach is putting fresh stuff on the periphery.

MR. KRESS: I think they will start doing those and being innovative with burnable poisons. It's expensive to take that fuel in and out like that.

MR. LAVIE: The problem with the burnable poisons is that they start to have problems with some of the factors that lead into their double limits.

MR. KRESS: But it's real expensive for them to change out fuel often. Anything they can do to stretch that out, I think they will do it.

MR. LAVIE: The GE fuel bundles now are unique. They are designed specifically for those positions.

MR. KRESS: That gets real costly.

MR. LAVIE: They already have some burnable poisons in them. We have extra water channels in the interest of trying to flatten the flux and get higher burn.

MR. KRESS: That is one of the things we learned is reload to the core to recalculate all of them. There's no such thing as a standard core.

MR. LAVIE: Nuclear engineers in GE plants have much more to do than the ones at PWR plants because these are calculations that are done throughout the cycle. They change the rod withdrawal and exertion sequence depending on where they are.

MR. KRESS: So it's not simple, is it?

MR. LAVIE: It's not a simple issue.

MR. BOYACK: All right. Thank you, Steve. You've taken just enough time for me to get back online because I certainly wasn't there. I'm still struggling here.

MR. SCHAPEROW: Are we ready to change the agenda yet?

MR. BOYACK: No, I'm just trying to survive here for a moment. We're ready to go now.

Okay. Let's talk a little bit about what we plan to do for the rest of the meeting. We'll go

until approximately quarter to 12:00 today working on the remaining four MOX groups. At that time we'll take a lunch break. There's a little uncertainty.

They are trying to go ahead and keep it so we don't have to pack up things and move out of the room here. The reason we have an hour and a half lunch currently scheduled is that, as I mentioned yesterday, there's a group that's coming in for an hour and a half, Toast Masters.

Evidently there is a room available upstairs and Martin is trying to get the Toast Masters switched upstairs which would leave us free us which means we can go to 12:00 and come back at 1:00. We just have to wait and see on that.

Now, what we're going to do after lunch when we come back at 1:00 we're going to go ahead and try to summarize and come to some points about what we learned out of the process here. You may have had better words for it and you're welcome to go ahead and give those.

The idea is to reflect upon what we've done, what we've learned, to go ahead and get those points listed and made available. They would be cleaned up for language because, as you know, we do things online here quickly. Then they would be made

available to Moshen as part of conclusions in the final report.

We'll finish the meeting at about quarter to 3:00. At least one of us will finish the meeting at quarter to 3:00. That's what I have to do to be able to make the flight and get through BWI security lines. Our ACRS colleagues can stay here and wrap up whatever else because they don't leave until late tomorrow.

Any questions or comments about how we plan to proceed? Okay. Let's go ahead now. Before we begin actually turning back to the release fractions for these last four groups, there was some discussion yesterday, preliminary discussion that says we've got the easy step done and now it's time to move on to the harder elements.

I wonder if -- it would be well for the record to have a little bit of that captured as to some of the challenges that we face and how we might go about dealing with them.

I know there were comments. Bernard had some and Tom had some. Would you be willing to just comment?

MR. KRESS: My point was we were able to deal with these earlier ones because they are

essentially all released from a MOX fuel. Then when we get down to the barium, strontium, noble metal cerium groups, then you've got fractional releases at some level.

The only data we have to pin that down as to relative amounts compared to the other things is the data we've seen from France. We don't have enough of that available to us to make a judgement in my mind. It would be almost wholly a guess.

Most of these things we are guided by knowledge and knowing something about it and having good data to guide our judgement. We just don't have that for these barium, strontium, noble metals and cerium for MOX fuel.

I frankly would be at a loss to figure out how to quantify those at the moment without more access to the data on those, data such as in RT-7 where they re-irradiated the fuel and got some online transient measurements of the releases of some of those lower altered material.

My problem is I don't have any -- there are no models appropriate for MOX that I know of. There are no -- there's not enough I've seen to guide my judgement so I have no real basis for establishing numbers. What I would like to do is say to be

determined in those areas and put that off until a later meeting beyond which time if these negotiations with prince people work out and we get to look at their whole range of data.

I think that's enough data that I can guide my judgement and intuition and even make a printable primitive model. I could then come to some believable judgement about what those levels might be. That was my thoughts on it. I just don't have the basis for making any numbers in those columns there.

MR. BOYACK: Jim.

MR. GIESEKE: I agree. What we've done so far has been driven by other source events rather than -- I don't know what to call it. Release chemistry perhaps the way to describe it. It's more like the accident progression that you surmise that we even defined yesterday that's been driving all the releases to this point.

Now we're down to the point where the nuances in fuel chemistry and such can have an overriding effect. As Tom had pointed out, those things show up when we have data and you can understand then when you have some data.

I think all we have if you look at what's been handed out to us, what we have are lot

qualitative statements like the barium is low volatility release. We could put that up there but that's not a number.

We can get qualitative statements derived from what we have been given in our handout but there are no numbers there, I think, to give us much to go on. It would be most useful to have a look at some data so we can draw analogies and extrapolate or what needs to be done.

MR. BOYACK: So are you in sort of the same position as Tom that at this stage -- I think your words were difficult, impossible, unprofessional, or whatever to --

MR. KRESS: All of the above.

MR. BOYACK: -- to actually come up with numbers or are you somewhere else?

MR. GIESEKE: Yes. Pick one of those words. I don't know. If you force people to pick numbers, I suppose people could pick numbers but they won't have no sense at all and ascribe no credibility to them which may lead people astray.

MR. BOYACK: I'm going to ask Bernard a question. If at some future time the French data becomes available, the data that you currently have, do you think that would be sufficient for a panel such

as this after a review of that data to move forward in these areas?

MR. CLEMENT: I think having this data things could be done. Data are not enough. You need them in an analysis of the letter and not take the chance and measure the value and say that's the same for the reactor. You need to go further.

I must say that our analysis process is not yet finished. If today we had to put numbers, I cannot use those same numbers for two reasons. First of all, the agreement for releasing the data is not yet done. Then we are at the stage where the analysis is not finished.

The only thing I could do is to put flags saying this we will have to look at with more attention on some elements. That is the only thing I could do today.

MR. BOYACK: Well, that was quite the information I was seeking.

Dana.

MR. POWERS: I certainly agree that the kinetics that influences the release, the gases, iodine, and cesium, is the kinetics of damage progression. We don't know exactly how that happens in MOX fuel. The ground rules kind of dictated those

releases.

Now to get the rest of these fission products kinetics actually counts and we have the data to draw straight lines. We do have some qualitative indications to suggest that the fuel is somewhat more oxidizing than low enriched uranium. You kind of suspect that.

MR. LEAVER: Higher oxygen potential?

MR. POWERS: Higher oxygen potential. So you say, okay, that suggest that noble metal releases go up a little bit and the barium and strontium come down a little bit and the cerium and lanthanum are about the same. The ruthenium moves around on you. You can certainly put numbers in that reflect that. This one's a little higher. Okay. You put in a number and say it's a little higher.

MR. KRESS: I think the barium is a lot higher compared to the 02 type level. It bothers me to say a little higher.

MR. CLEMENT: For barium I would say independently on the MOX issue you still have data from PHEBUS and data from VERCORS which are not consistent. We have some hypotheses about that, about interactions with zirconium, with elements coming from stainless steel that have not yet been confirmed.

There is still a question about barium for thermochemistry and things like that that could defer in MOX fuel and you already have the question for future fuel.

MR. KRESS: Antimony, for example. It goes all over the place. I don't understand it.

MR. POWERS: Fortunately the inventories on the antimony are so low.

MR. KRESS: It was just an example.

MR. BOYACK: David.

MR. LEAVER: I think most of these points have been made but I'll just kind of summarize it. For the volatiles even though there were some conflicting experimental results, it was, I think, in my mind hard to ignore RT-2, even though we couldn't explain why we didn't see that result for HT-1 versus RT-7 so there was a data point.

In addition to the volatiles with an LEU core assemblies were releasing most of the inventory from the fuel. It was possible to make reasonable estimates for the MOX. Even if we're wrong, we're not very wrong.

For the low volatiles there's two problems. One is we don't have -- we essentially have no data even though we may suspect that there is some.

I would be surprised if the differences were significant between LEU and MOX but we don't know that.

There certainly is this issue of at least possibly higher oxygen potential. Just how significant that turns out to be I don't know. Bernard made an excellent point about the fuel microstructure being different and that as an impact. So no data.

Secondly, the LEU releases are very low fractions so there is room if one believes the different phenomenon could have an impact. It could have a significant change in the release. For both those reasons I think we are not -- we really don't have a basis at this point in this meeting to even make judgements on this.

MR. GIESEKE: I think the differences might be significant in some cases. Right now if you forced me to put a number, say, on early in-vessel barium, I would look at the order of magnitude that we have there.

MR. KRESS: Yes, order of magnitude.

MR. GIESEKE: That's about where I'd guess it but there's no firm basis for that. That, I think, is significant. I may be wrong.

MR. KRESS: And just as a comment on that, I would probably do each one up an order of magnitude in spite of the fact that some go one way and some go the other way. That's not good enough.

MR. BOYACK: As I listen to sort of the comments here, let me just sort of tell you what I have heard in the pooling. There are perhaps three of you, Dave, Tom, and Bernard that say it's just too early to do anything by way of numbers.

MR. KRESS: Other than upping the order of magnitude.

MR. BOYACK: Well, that was my second one. I basically heard Dana say that he would establish a trend line at least. I guess I've heard a little bit of that, but the nature of the trend line sounded quite different to me, where Dana has said, "Well, I think I have a sense of which way to go." The others I just heard, well, it was almost an uncertainty statement. "Since I'm uncertain, I'm going to give it a larger number."

MR. KRESS: There is certainly a lot to be said about Dana's trend lines but I've been surprised too often in the past when the data comes in and goes the opposite direction. I would err on the conservative side by upping the amount, even though I

know some of them probably are not going up at all like the cerium. Some may go down maybe. I would just err on -- what I would do is err on the conservative side and up them all by another magnitude.

MR. BOYACK: So it's this sort of going around the table and hearing the view that suggest to me that trying to create numbers creates more problems than solutions for these groups at the present time.

MR. LEAVER: That would be my view. It does create more problems because I think people would have to probably increase these release fractions significantly and across the board probably it's not right. Then what do you do with it? It's really not something you can use.

MR. KRESS: I think upping the barium and strontium an order of magnitude, which is what I would do, has significant consequences in terms of source terms, particularly for the strontium.

MR. LEAVER: Twenty percent release is trying enough.

MR. KRESS: That's a lot and that has lots of consequences.

MR. LEAVER: You keep talking about barium and strontium. You did that yesterday and that's

because you're looking at the release rate for the cesium and RT-2. Why are you focused on sparing the strontium so much?

MR. KRESS: I'm looking at the data and the slides that Bernard made.

MR. LEAVER: You mean the less than 15 percent statement? What data are you talking about?

MR. KRESS: The other one is the RT-7 data. He had some qualitative statements that I used.

MR. LEAVER: Okay. Right.

MR. KRESS: Then I'm also factoring in what I know about relative volatilities and how those might behave relative to the alkali metals. For example, I have some metal volatilities in my mind. If I up the alkali metals a certain amount and up the barium and strontium a certain amount relative to them because I have this model in my mind and it goes up an order of magnitude in my mind.

I mean, as a guess it probably doesn't go up maybe five times or three times. What I'm saying is what I would have to do is make it an order of magnitude to be conservative because I don't don't how to refine it to that level.

MR. LEAVER: I guess that's the problem.

MR. KRESS: The other thing, just for the record, in all these source terms we have arbitrarily constrained ourself to make these things add up to one because of release fraction type things. I think that's a real mistake.

We shouldn't have done that because it really limits your ability to define a design basis source term that is applicable over a whole range of accident conditions. I, for one, would rather not constrain myself to having to say is that up to one. I would double count lots of places.

For example, the late in-vessel, I would base it on those sequences that have a lot of deposition and I would get a big number. Early in-vessel I would base it on those sequences that don't have a lot of deposition so I would be double counting.

I think we made a mistake in not doing it that way but that's the way the old 1465 was done and we're being consistent with it. People always question, "How can you do that? It adds up to more than one."

MR. CLEMENT: With what we did in France, we based on source term evaluation on sequences with low deposits so we've got a set of values for that.

Now we are processed for treating the revaporization processes that could be important in other sequences as a sensitivity for delayed source. It's another approach.

MR. KRESS: That's the right way to do it.

MR. CLEMENT: That's another approach.

MR. POWERS: I guess one of the questions I would ask goes like this. Suppose you don't put numbers in there but people are going to have to do some analysis. They are going to have to put some numbers in. Somebody has a gun at their head so let's get the analysis done and get it submitted. Is it less misleading to give them nothing to guide that by or something to guide it by?

MR. KRESS: My proposal is to put it in later at a later meeting after we get another data to guide our judgement.

MR. POWERS: I guess what I would ask is at your later meeting is starting with a light table better than starting with a table that had some preliminary thoughts in it.

MR. GIESEKE: We talked about the time requirements and it was like a year and a half before anybody wanted to use these numbers. That's not true?

MR. LEAVER: Well, Steve Nesbitt said they

were going to submit the schedule to submit the license amendment at the end of 2003 which would mean they probably ought to be doing calculations in the spring of 2003 which is roughly a year from now.

My sense was, and, Steve, correct, me if I'm wrong, this group if it were to go along the lines that Tom said which is try to get some data. I don't know what that process is but can we have six months or something like that to assemble this French data that we would be able to provide some numbers. I certainly think Dana's point is a good point. I mean, in some respects a blank table is maybe --

MR. KRESS: What Dana is saying is we could go around and get these. For example, when you got to me I would up the order of magnitude and Dana wouldn't. He would be changing some of them up and changing some of them down and holding some of them steady based on his chemistry model.

I don't know what the rest of you guys would do. We could put numbers in there and maybe put an asterisk for those explaining that these are subject to review and change or something. That would be one option.

MR. BOYACK: Richard Lee commented yesterday, I believe. He made a statement about

process, I believe, that led me to believe that the NRC was close.

MR. POWERS: They've been close for five years.

MR. BOYACK: You would take exception with close then?

MR. KRESS: The trouble I see with Dana's concept, one trouble is that too often I've seen when you put them down on the table, that's it. It never gets changed and people disappear with it and we have no more meetings and they never get changed. That's a danger inherent in that.

MR. BOYACK: When I first approached this activity, since I don't approach it much from an expert rather than just a facilitating approach, it was interesting to hear the initial dialogue regarding the PWR source term. There was some question in the minds of the people I talked to about whether the panel would be willing to go ahead and put numbers down and you've done that.

Then in a quick fashion we went through and did the BWR. Now we come to this area where there is less data for the MOX fuel and you went through a good portion of the table. When you came to the point that we are now, these last four groups, and there was

some discussion about that, it gave me pause for thought that the panel has been willing to do it to this point because you felt good enough about doing it that you could proceed. When we came to this point, you have a different perspective.

I wanted to make sure we dealt with that perspective because I tend to hold the view that once you get these numbers down, they are very hard to change because if nothing else, if somebody goes ahead and finds a way through the process with those numbers, then even if they've got better numbers, until they get further down the path, they say, "Okay, we got through the process once. Let's just do it again even if it causes some pain."

Eventually if there's enough data around and there's cost benefits to it, then they will go through the process once again.

MR. LEAVER: Would it be -- somehow if we could make this happen. I'm not sure how but maybe we could. It's a pretty significant enterprise that we all got together here for three meetings and it's costing certain organizations money to do that. I presume that people are going to pay some attention to what we say.

I think one of the things we need to say

loud and clear is let's get this data. Maybe people have been sort of trying to do that for several years and it hasn't happened. Well, damn it, it's really important because we're sitting here and we're being asked to make judgements. We're saying we really can't do that unless we have this data. What is the most effective way we could give that message to the right people? Maybe the right person is sitting right here.

MR. SCHAPEROW: That is one of the reasons I wanted to have a little discussion later about what are some of the themes -- overriding themes from the meeting. This is one that comes out loud and clear. Mark's data is very important.

MR. LEAVER: And Dana's point is a good point. I mean, we've been hearing apparently that this data is going to come for years and years and it hasn't come. In fact, it's going to take a lot longer to get it. Our plan here isn't going to work because we're not going to have anymore information than we have now.

MR. BOYACK: There are two sides to a negotiation from the release of data. Those who have and those who have to provide something to receive it. We all have individual perspectives but my feeling is

since so much has been invested in this activity today, a blank table will have more incentive to go out and negotiate seriously than a numbered table.

MR. LEAVER: I think that's right.

MR. SCHAPEROW: Would the panel be amenable to providing a range for each of these values?

MR. NESBITT: Can I make a comment here? It's something that I don't know that everybody has thought about but I certainly have. First of all, I'm very sensitive to the concept of if you put numbers down, they take on a life of their own and it gets very difficult to modify them later, even in the presence of better information. I've seen it time and time again. I see no reason why that trend is going to change.

The second point is everything that has gone on in this meeting is going to be used in a licensing proceeding against us. There's a formal transcript. The intervenors are going to pour over this transcript -- he just came in the door -- and we will be fighting this in front of the Atomic Safety Licensing Board.

I have heard that members of the panel sort of bring different perspectives into this concept

of giving some ratings now even though there's a general feeling that there's an insufficient amount of data to do that. One concept was sort of consistent with what I would say NURGEG 1465 says which is we're not trying to be conservative here. We're trying to be representative and this is what I think is going to happen.

Another concept is I just don't know so I up everything significantly and that will have an impact. I'm not saying either one of those concepts is right or wrong but when you go in to this kind of enterprise with different fundamental perspectives on that and how you're going to do it, you almost guarantee that the results are going to be misleading and used inappropriately. That's my speech.

MR. POWERS: Would that I had a dollar for everything that I've done that's been misused or used inappropriately.

MR. BOYACK: So, anyway, unless the NRC otherwise directs, I'm not inclined to push for numbers.

MR. SCHAPEROW: I guess I didn't really get any response to that question. Maybe I did. Maybe a range of numbers.

MR. KRESS: I think it's just as hard to

do a range as it is to give a range of numbers.

MR. LEAVER: I don't think that solves any problems.

MR. POWERS: Let Tom give you his numbers and I'll give you mine.

MR. KRESS: That's all right. That's going to be arranged.

MR. SCHAPEROW: We don't want the cerium numbers. That doesn't do anything for us.

MR. KRESS: Believe me, I'm not saying we're going to be overly conservative when I up these things.

MR. POWERS: You don't even want them to add up to one.

MR. KRESS: That's right. I don't want them to add up to one. If I had to say right now for the barium and strontium, I would put .1 which is likely it will remain to.

MR. POWERS: That's not such a bad number.

MR. KRESS: Yeah.

MR. POWERS: .1 could be released from the fuel pretty easily actually.

MR. KRESS: That's what we're basing it on and Dana might do the same thing. For the nobel metals, I don't think they matter that much so I just

up the order of magnitude on general principles. Dana would probably disagree with that even though they -- I don't think when you get down to the noble metals, cerium, and lanthanides, up in those numbers there an order of magnitude makes any difference.

MR. SCHAPEROW: How about the ex-vessel releases? I've heard discussion before about those are typically driven by niobium burst.

MR. KRESS: The ex-vessel releases I would probably keep the same.

MR. SCHAPEROW: Oh, then we can put it a little more on the table then. I hear we're mostly focusing on the in-vessel right now.

MR. LEAVER: The gap and early in-vessel is what NRC and the licensee need to do, radioactive design basis accident calculation. If you can't do that, you really haven't solved the problem.

MR. LAVIE: Does it focus -- the ability to just position the ex-vessel even for a qualitative statement may be useful in focusing the negotiations or additional research on the areas where we need the data worse as opposed to just a blank page.

MR. LEAVER: You need the data during in-vessel core damage progression. That's what you need.

MR. BOYACK: Were you here yesterday,

Steve? We had given the panel members an action item to try an individual letter on research needs which will be included in the report.

MR. LAVIE: One thing I wanted to throw in. Steve has pointed out his time schedule. It needs to be noted that the agency is already working on the environmental impact statement for the fuel fabrication facility. Part of that environmental impact statement has to address the indirect effects which includes the use of fuel in the reactor.

Bob Martin, who isn't here, has taken a stab at writing that section. It's logic qualitative and as time goes on I'm sure we're going to get pushed in the direction of making those harder numbers. It isn't just 2008 we're looking at.

MR. LYMAN: May I make a statement? I think I got the gist of this. Duke is requesting that you not put any numerical values down?

MR. BOYACK: Absolutely not. That's not even close.

MR. SCHAPEROW: No. This panel is operating independently of Duke.

MR. BOYACK: Just read the transcript when it comes out. The net of this is that we started out based upon some discussion late last night and this

morning with a concern by individual panel members that there was insufficient data for these last four groups for them to go ahead and create numbers. Duke had absolutely no statement until you came in and there's been now approximately 45 minutes of discussion on the transcript about this issue.

MR. LYMAN: However, they are crucial. I did an analysis of the sensitivity of the consequences in a severe accident to the actinide release fractions from MOX fuel and there is a substantial effect. The fact is if you have a large range because the uncertainties, that is something that you state so the public is aware.

MR. LEAVER: That has been stated. We've been talking about that the last 45 minutes and the end of the day yesterday.

MR. BOYACK: The real key here, Jason, is at this point I don't make any decisions other than to try and draw from the panel a perspective that would tell us where to go. Now, with respect to PWRs, BWRs, and the first increment of MOX we've been able to go forward to the point where we put either eclectic or individual numbers down.

My hearing of what's been said thus far is that it's difficult. The numbers that would be put

down don't have an underlying database that would lead people to put down numbers that had some sense of value to them because they really did capture underlying data.

It's just that I hear them saying the data is partially out there, has to be analyzed, but if it's analyzed and used, then it could lead us to a place where they would probably be willing to put numbers down even though that would still be a partial database.

MR. SCHAPEROW: I guess I'm still struggling with the value of tackling a gap in ex-vessels. We might be able to make some progress in those areas.

MR. LEAVER: I understand the point you're making. The point I'm making --

MR. SCHAPEROW: I kind of agree with Steve which is if we could really narrowly focus what the research need is which is --

MR. LEAVER: The gap is already tackled.

MR. SCHAPEROW: I think there's no reason to believe that there's going to be a gap, at least for these groups.

MR. LEAVER: Okay. If each panel member says that, that's fine. I want to get that on the

record that they say to really focus the research.

MR. POWERS: I think yesterday we pretty much agreed, at least two of us agreed formally that if we do a LOCA test with MOX we ought to measure the gap release.

MR. KRESS: That ought to be one of the research items. I don't think I'm yet prepared to zero all those columns for the gap.

MR. POWERS: You really want to put something in for the last four?

MR. KRESS: No. I really don't. I would like to have some data first.

MR. POWERS: What kind of data are you going to get?

MR. KRESS: We're not going to get any data.

MR. POWERS: Go ahead and multiply those by 10, Tom, and that will make you happy.

MR. KRESS: You're making me feel really good.

MR. POWERS: You have the order of magnitude, he feels happy and we're happy.

MR. BOYACK: Let me ask the question this way. Did I hear, at least, that either on gap release or some of the later things it is possible to put

numbers down but it's the early in-vessel that's the big problem?

MR. KRESS: Let me talk about the gap release. Dana has articulated this concept of the islands of plutonium and some of them are located very near the surface of the fuel where the gap is, or places that could be considered part of the gap. When there's fission you get recoil. You might kick either some of the fission products into that gap and it's not a disfussion process anymore.

It doesn't matter that these are low volatiles. Some fraction of them are close enough to the surface to be kicked into the gap. Maybe they're not very mobile even and may not be releasible but what you've got is these noble gases and the fuel gas that can sweep things out that are in there. To my mind, because this concept that you may have the ability to get some of that in there, I am disinclined to say it is absolutely zero.

I know it's a small level and you could probably but some number on it based on the distribution of PuO in the fuel. With the probability of a recoil in that particular direction you could probably come up with a number. I haven't done that exercise but I don't thin it's zero.

MR. POWERS: It's the same problem with the entrainment modeling. You can agonize over how uncertain you are on it, but you are uncertain on a real, real tiny number.

MR. KRESS: That's right.

MR. POWERS: Being two orders of magnitude uncertain on a 10 to the minus 10 release fraction really doesn't bother me very much. It's like these zeros that exist up here. Yeah, I know there's some variant in there.

In fact, Dick Lorentz when he measured -- when he did those experiments in Oak Ridge he actually reported -- I don't think he reported barium but he reported a couple of others.

MR. KRESS: The question one asks is if he put a number in there, does it make any difference?

MR. POWERS: It seems to me -- I mean, there's no twist of fate in chemistry that I can see that results in those numbers being --

MR. KRESS: And the real thing that I would think there is not much chance of actinides getting in there.

MR. POWERS: There's an inventory. I guarantee you when you flow down the rock you carry some atoms out but it's damn few atoms because the

temperature is so low at the time of the gap release.

MR. KRESS: I guess we would be willing to put zeros in the gap.

MR. SCHAPEROW: Does anyone not want to put zeros in the gap?

MR. BOYACK: I'm quite willing to go through the four tables and deal with what we can.

MR. KRESS: I'm willing to put zeros in the gap and I'm willing to keep the ex-vessel just about like it is because I don't think the difference between the MOX and the regular fuel makes any difference in excess.

MR. BOYACK: That information would focus the attention.

MR. KRESS: Late in this I'm almost willing to keep it the same because I don't think there's enough of those things to play it out. The one that bothers me that I don't really know how to do is the early in-vessel.

MR. BOYACK: Might the ex-vessel change because the inventory in the melt changes?

MR. KRESS: Of course it has an effect on it.

MR. BOYACK: I mean, if you're going to talk order of magnitude on some of those --

MR. KRESS: But going from .02 to .01 doesn't make any difference.

MR. GIESEKE: It shouldn't change the fraction, should it?

MR. POWERS: Fraction of the total so it does screw around with the numbers a little bit. Again, when you are releasing one or two percent, or even 10 percent, it's not a big deal.

MR. KRESS: So basically I've come down to the only one I'm really having a lot of difficulty with is there an in-vessel.

MR. POWERS: I actually have some heartburn about the ex-vessel simply because I put the can of residual fuel. It comes in but there's only one element that -- one group of elements that bothered me there and that's the noble metals. Everything else is about the same.

MR. BOYACK: Let's see where we go with this. Panel members are obviously allowed to say they don't feel they can agree with that number. Let's see. Okay. We're going to go one by one through these and we'll start again with Dave.

MR. LEAVER: I have nothing I can add to this.

MR. BOYACK: No entry. How's that?

MR. LEAVER: Yep.

MR. NESBITT: Excuse me just a second. Didn't you say a minute ago that you thought it would be zero?

MR. LEAVER: Are you only doing the gap?

MR. BOYACK: That's all I'm going to do. Well, what we're going to do is go down each one. We'll do it in turn.

MR. LEAVER: Are we talking ourselves into coming up with numbers here?

MR. BOYACK: No. Well, I don't think so. What I heard was people say -- Moshen, you didn't speak but I heard several people say -- well, two, right? -- that they would be willing to put down a number for gap release.

I heard Tom then say on early in-vessel he wouldn't put down a number. I heard him talk also bout the ex-vessel and the late in-vessel. At this point I'm just going to take a moment and see what people are willing to do. I have no problem with you saying no entry.

MR. POWERS: He knows where we are. He's just worrying about the price.

MR. BOYACK: I'm not sure what to do with that so I won't do anything.

Jim, your thoughts?

MR. GIESEKE: No entry.

MR. BOYACK: Okay.

MR. GIESEKE: All capital letters.

MR. BOYACK: Dana.

MR. POWERS: Ten to the minus fifth.

MR. BOYACK: Really?

MR. POWERS: Zero.

MR. BOYACK: You wish to say any words with that?

MR. POWERS: Gap release occurs when the temperature is very low. These things are not very volatile. There is some inventory. There is a gap in these things but it's not going to vaporize at this temperature. It pales in comparison to the curies I'm getting from the noble gases, the halogens, and the alkali metals. I'm dead serious about 10 to the minus fifth. I actually think that's the number but why even bother to force somebody to put that into a spreadsheet.

MR. BOYACK: Tom?

MR. KRESS: Ten to the minus five. Zero.

MR. BOYACK: Any difference on the reason?

MR. KRESS: No. Exactly the same reason Dana had. Exactly.

MR. CLEMENT: No value but keep what I was saying that these flags for differences for what was expressed before and here is no flag.

MR. BOYACK: No flag. Okay. Early in-vessel. David, can I just assume you're no entry on all of these?

MR. LEAVER: No entry.

MR. BOYACK: I'll speak for Jim. Dana?

MR. POWERS: .01.

MR. BOYACK: Okay. Let's spin out a little bit of rationale.

MR. POWERS: I think fuel is inherently oxidizing. If I'm going to jack up the noble metals releases in response to that oxidizing character, I better suppress the barium releases, an equal response to that oxidizing character.

I can be dead wrong on this because if the M-5 clad is not heavily oxidized on the inner surface and aggressively attacks the fuel, it produces a substantially reducing environment and the barium separates out as a distinct phase and it can be extensively released.

All that is saying is that I need information on how M-5 clad creates core degradation. It's independent of the MOX. It's M-5 clad. So is

that okay? Here's a number. Put a big star beside it and say "stay tuned" because I recognize I could be wrong, drastically.

MR. BOYACK: So you're saying your confidence is not high.

MR. POWERS: No.

MR. KRESS: .1. Confidence is not very high based on some observations, qualitative results of the French data, and some observations made at Oak Ridge in the past, and the fusion product release model for MOX that I have in mind.

MR. LEAVER: Tom, I want to ask about this again. One thing you said about this is you have in your mind a correlation between the volatility of cesium, for which we do have at least one data point, and strontium and barium and that is something you are thinking about.

The other thing you said is the French data. I don't believe there is any French data that suggest higher barium or strontium release. I don't understand what you mean by that. I'm saying there's nothing to suggest higher than LEU.

MR. KRESS: I don't recall if I ever saw the data. It might have been at the last meeting.

MR. LEAVER: There is a statement on RT-7

that says barium behaves as semi-volatile.

MR. KRESS: It's semi-volatile. That's like .1.

MR. LEAVER: All right. Here I am trying to say what Bernard's slides mean. Maybe you should ask Bernard what they mean.

MR. KRESS: I have confidence in his numbers. I'm interpreting things.

MR. LEAVER: If you just listen for a minute to what Bernard has to say.

MR. CLEMENT: Two different flags for barium. The flag is not specific to MOX, okay? I could have said that before for high burnup fuel because we have data from annealing tests and they are giving high release of barium, and data from PHEBUS giving low release of barium. We think this is linked to interactions between barium and materials such as zirconium oxide or transition metals. This is still to be confirmed.

MR. KRESS: When you have conflicting data you err on the high side.

MR. CLEMENT: So this is not specific to MOX.

MR. LEAVER: That was my only point. The same observation was made for high burnup fuel and we

ended up at .02.

MR. KRESS: My feeling is MOX is a lot like high burnup fuel in the way it behaves.

MR. LEAVER: Okay. But we did end up with .02 for high burnup fuel.

MR. CLEMENT: For barium for the source term maybe it's not so important. It could become important if you want to know where is the decay heat that has some effect. For strontium effects and so on you put high or low values.

MR. KRESS: I think it's significant for strontium.

MR. CLEMENT: For strontium I would say the flag is that we have not yet analyzed in sufficient details the results. This is just for technical point. You know that strontium is not measurable by gamma spectrometry so we have to analyze the results of measurements.

MR. POWERS: And both barium and strontium releases from the fuel substantially affect the late in-vessel release, especially the strontium. It's very nice because it involves heat and deposit as opposed to the gamma which is spread all over God's half acre.

There's another thing. You form a very

stable urinate with the barium and strontium species. It goes down on surfaces and just sits there and collects. In itself it doesn't do anything but, boy, it does things to everything around it. What we succeed in doing is saying there's an order of magnitude of uncertainty here.

MR. BOYACK: Okay. Ex-vessel. Dana.

MR. POWERS: .1.

MR. BOYACK: Comments?

MR. POWERS: It's driven by the zirconium presence in the first initial transience. Zirconium reduces everything down to barium metal and barium metal vaporizes. It's really interesting because if you do an experiment where you just take quarter break, you know, U02, pour in a little barium, poor in a little concrete and heat it up, you would get zip barium.

You get none whatsoever because it will form barium silicate that has a really low volatility. Do the same experiment with some zirconium metal and you'll have all the barium in your lap. It's interesting. Makes life fun.

MR. CLEMENT: For strontium 10 percent of the amount present in release strontium. Ten percent of the amount present in release strontium. For

barium 2 percent of the amount present in release strontium. That's basically the same fractions as for LEU fuel.

MR. POWERS: I think that's the right answer. MOX has completely disappeared at this point.

MR. BOYACK: Okay. Late in-vessel.

MR. POWERS: Zero.

MR. BOYACK: Tom.

MR. KRESS: I've got quite a bit coming out of ex-vessel. I would put .05 as just a number in there.

MR. POWERS: It's entirely possible because the variant urinate with hexavalent uranium is actually a pretty volatile compound so if we get enough flow over the piping system and you put some stuff down, you can move things around. It's kind of interesting. Strontium urinate with nexavalent uranium are practically not volatile at all. The barium is just polling radii.

MR. GIESEKE: Don't leave yet. I'm not done this last table yet.

MR. BOYACK: Now you have something to say. Let me go back.

MR. GIESEKE: That's what I said. I said don't leave yet.

MR. BOYACK: And I was gone, right?

MR. GIESEKE: I have to admit that yesterday before I left here I went through and put numbers on all these so I've made my statement today. I'll give you yesterday numbers. I'll just read them off from the top down.

MR. BOYACK: Before you do this, it will be on the transcript.

MR. GIESEKE: That's fine. You can put them on here. I think the important thing is that all the statements have been made about the lack of confidence or the lack of data to support some of this.

Okay. From the top down what I had was 0, .1, .1, and zero which it turns out was pretty close.

MR. KRESS: You did a good job.

MR. POWERS: Tom thinks you got those numbers just about right.

MR. GIESEKE: Is that right?

MR. POWERS: And you're within my uncertainty range.

MR. GIESEKE: I can't be the only one without a number.

MR. POWERS: Yes, you can.

MR. KRESS: That may indicate you're the

smartest one we've got.

MR. POWERS: You can just say all less than one.

MR. KHATIB-RAHBAR: Jim, did you say zero for ex-vessel?

MR. GIESEKE: Yeah. I could qualify that.

MR. KHATIB-RAHBAR: What did you say?

MR. GIESEKE: Late in-vessel I had a zero yesterday. Ex-vessel is .1.

MR. BOYACK: Let me just announce we stay here. That means we've got much more flexibility. We don't have to pick up things and pack them off in the corner.

MR. KRESS: I was looking forward to moving.

MR. BOYACK: You can go upstairs one floor.

MR. KRESS: We're overlapping then until they let us out at quarter to 12:00.

MR. BOYACK: We'll excuse you. Or, as another statement, we'll call your bluff. I have another idea. Why don't we stay here and you can give a toast here.

MR. KRESS: Yeah, but I'm not a master.

MR. BOYACK: We'll accept amateurs.

MR. KRESS: Toast amateurs.

MR. BOYACK: Very good. I am ready to go on to the next thing. Okay. Jim.

MR. GIESEKE: I'm going to go no entry on all of these. I've run out of information.

MR. BOYACK: Dana.

MR. POWERS: Okay. Zero for the gap release. Now here for the high burnup fuel we chose to distinguish between molybdenum, technicium versus rhodium ruthenium.

MR. BOYACK: You want to distinguish them again?

MR. POWERS: I think that's probably a valuable exercise to do.

MR. BOYACK: What was the first group?

MR. POWERS: Moly and technicium.

MR. BOYACK: TC.

MR. POWERS: And I put -- I have my release fractions in the containment and 0.1 for those. And for the ruthenium rhodium group I put that at 0.08.

MR. BOYACK: Okay. You want to have any comments?

MR. POWERS: The uranium matrix surrounding the particles where fission products are

because they end up there by recoil and then matrix is substantially oxidizing and creates the volatile forms of these.

MR. BOYACK: Uranium matrix surrounding particle for fission products creates what did you say?

MR. POWERS: Substantially oxidizing fuel creates the volatile forms of these radionuclide.

MR. BOYACK: What I'll do is make it a comment to go with the transcript where I really can pick it up. This flags it. Okay. Let's see. We started down so we might as well have you go through the rest of them.

MR. POWERS: .01.

MR. BOYACK: Same thing here?

MR. POWERS: Same groupings but the number is the same for both of them.

MR. BOYACK: Okay, .01.

MR. POWERS: 01.

MR. BOYACK: And late in-vessel.

MR. POWERS: Here for the moly .1. For the ruthenium rhodium .01.

MR. KRESS: Does that imply the release of the fuel as being .2?

MR. POWERS: At least .2.

MR. KRESS: Okay.

MR. POWERS: Because that's what I was going to use.

MR. KRESS: That's zero for me. I've always considered moly and technicium as semivolatiles because most of the time they get released under oxidizing conditions. I would have put about a .1 in there also.

MR. BOYACK: Are you going to want the same two?

MR. KRESS: It doesn't matter. We'll put .1 for both of them. I love things all together. I like the .1 on the -- .01 on the ex-vessel because I've got no reason to --

MR. BOYACK: For both of them?

MR. KRESS: Yes. I'll lump them together, too. There I like the -- there's where I would make a differentiation but I would use the .1 for moly and technicium.

MR. BOYACK: And?

MR. KRESS: I have no basis for the other one, the .01. I would have guessed that the ruthenium oxide would be entirely volatile and not subject to that much deposition and would have gone out early anyway so the low number I like. I would keep it at

.01.

MR. BOYACK: Any comments? Bernard?

MR. CLEMENT: Okay. No flag. Flag refers to what I expressed in the numbers I've given for high burnup fuel, not to the numbers of 1465.

MR. BOYACK: I'm sorry. I didn't quite get that.

MR. CLEMENT: When I say no flag -- when I say a flag, that means there could be a change according to the numbers I have given for high burnup fuel.

MR. BOYACK: Okay.

MR. CLEMENT: There's a flag according to that and not to 1465. It should be fully clear.

MR. BOYACK: Okay. Got it.

MR. CLEMENT: No flag means if I had to put a number, I probably would put the same as I had given for the high burnup fuel. Flag means portion maybe left to look at. I keep the separation between the two groups. For moly technicium I will put a flag and for rhodium no flag. That's preliminary for within rhodium.

MR. LEAVER: Why no flag?

MR. CLEMENT: Because of the preliminary analysis as compared to the numbers I have given for

high burnup fuel and the time being without preliminary analysis I don't see a big reason to make a change. That's a long sentence. This is for the total release. It will not separate. As I did in the past for this group, I will not separate between in-vessel, ex-vessel, and late in-vessel.

MR. BOYACK: Okay. So this is --

MR. CLEMENT: For early in-vessel that was for total release. Just separated for barium and strontium because release from MCCI are significant. We think it's not the case for the others.

MR. BOYACK: So I've got total release for the last one.

Dave.

MR. LEAVER: No entry.

MR. BOYACK: Okay. Dana.

MR. POWERS: Zero. Here would be the distinction between the cerium neptunium group and the plutonium group. Here I think that is probably crucial to do it here for MOX fuel. The cerium neptunium I take it as .01. For the plutonium .001.

MR. BOYACK: Okay.

MR. POWERS: That's okay. You got it right. You read my mind. The last one you can make zeros.

MR. BOYACK: Me and Bill Gates.

MR. POWERS: That's an Apple you're working on.

MR. BOYACK: Bill is ever present.

MR. POWERS: Omni present.

MR. BOYACK: I'll just add that to my lexicon of misused words.

MR. KRESS: I'm going to cop out on you on this one and say no entry. I think it is important, though, to separate out the plutonium group there from the cerium neptunium but I don't have any entry for that.

MR. POWERS: You might want to put a notation on there that I come up with this lower plutonium release only because we're working on a low-pressure accident sequence. If we were in a high-pressure accident sequence, that plutonium would come right back up because I have total faith in the experimental investigations going on at Los Alamos laboratory.

MR. BOYACK: Why the high pressure?

MR. POWERS: You get high pressure steam in here and you'll start creating kerkorian vapor phase hydroxides.

MR. SCHAPEROW: Don't forget about the

vapor phase hydroxides by the high pressure steam.

MR. KRESS: Unparalleled in the area of accelerator physics.

MR. BOYACK: What was the comment about steam?

MR. SCHAPEROW: High pressure steam reacting to form hydroxides.

MR. POWERS: Actually, all weapon systems I've ever worked on have been livable weapon systems. I'm only been up at Los Alamos twice in my life.

MR. BOYACK: Let's see. We're on ex-vessel.

MR. KRESS: I am sure there are observations having to do with MCCI when the early zirc is present.

MR. BOYACK: When you take it and shake them up and bounce them on concrete, it's pretty hard to get less than 1 percent release.

MR. KRESS: I think that is probably my impression of what you get from mechanical release.

MR. BOYACK: Late in-vessel?

MR. KRESS: Yes. It's mechanical release. Those look like good numbers to me.

MR. BOYACK: For late in-vessel that's okay?

MR. KRESS: Late in-vessel I have no idea. No answer at all.

MR. BOYACK: Bernard.

MR. CLEMENT: No flag here.

MR. BOYACK: Okay. No flag.

MR. CLEMENT: Then I keep separating cerium and neptunium from plutonium. I put two flags, one for cerium and one for plutonium. This is for total release.

MR. BOYACK: Okay. Dave.

MR. LEAVER: No entry.

MR. POWERS: This is been an inverse Jean Paul Sartre. Instead of no accent, there's no entry.

MR. BOYACK: Jim.

MR. GIESEKE: No entry. Post-modernism existentialism. If you're a French existentialist, you've got a depressed world view.

MR. BOYACK: Okay. You gentlemen realize what this is? The last group. How fitting that Tom starts.

MR. KRESS: I put zero in there. I would for the ex-vessel put in the same thing I had for the cerium group so I will put in ex-vessel. Same thing as I had for the cerium group. There's some doubt about whether the lanthanides are above the release.

There are some volatile species you can get so I'm just going to put no entry. I think the lanthanides will be a little higher than the other release but not very much higher.

MR. BOYACK: Dave.

MR. LEAVER: No entry.

MR. BOYACK: Jim.

MR. GIESEKE: No entry again.

MR. BOYACK: Dana.

MR. POWERS: 0.005.

MR. BOYACK: Any comments?

MR. POWERS: Yeah. It appears that these lanthanides exhibit less volatility than the cerium group. I'm just trying to reflect back. I can be dead wrong on this.

One of my nearest and dearest friends is LaO vapor species because when we were inspecting VANESSA the people at Argonne told me that I had no basis for hypothesizing such an obscure species so motivated to show what horse's asses they would be, I went back and did a complete literate search on LaO and I found the first report of the particular species. It's your volatile lanthanide. It was in 1924 so I accused Argonne of having a library that hadn't been updated since 1924.

MR. BOYACK: Okay. Now we move on --

MR. LEAVER: Hold it one second. Just so I understand, the .005 you're saying is less volatility. Did you mean for that number -- relative to LEU that's a factor of 25 higher than the LEU estimate. Is that what you intended?

MR. POWERS: Yeah, because those numbers are horseshit.

MR. LEAVER: I'm not sure I understand what you mean.

MR. POWERS: The lanthanide numbers that they put in there were generated by pressure from an analysis that was done by some people and they were misinterpreted in some experiments.

MR. BOYACK: Ex-vessel.

MR. POWERS: .01 and zero.

MR. CLEMENT: No flag. Then I keep separating lanthanum, europium, niobium and bromine from the others.

MR. BOYACK: Help me on the symbols on this.

MR. CLEMENT: Na, Eu, Nb, Br.

MR. BOYACK: Thank you.

MR. CLEMENT: From the others. I put flag only on europium.

MR. BOYACK: Okay.

MR. CLEMENT: That's for total release. We already assumed in our numbers the higher release so it is not inconsistent with your point.

MR. POWERS: Every time I try to calculate europium releases I end up with high vapor pressure. I could never understand exactly why.

MR. BOYACK: Okay. Well, that finishes the tables. What I would like to do is take a 15-minute break and come back at 10:25. By then we'll figure out how to proceed.

(Whereupon, at 10:11 a.m. off the record until 10:37 a.m.)

MR. BOYACK: Let's go ahead and talk a little bit first about what we want to do for a moment. First thing I'm going to leave in a few moments and Jason is going to take over.

MR. KRESS: Chaos.

MR. BOYACK: In case it turns out that the Mac approach to life is just too much for him, I have transparencies and whatever so you can work with those. Otherwise, he can go ahead and type here.

The thought here was that we would spend the remainder of the meeting, maybe another eight or 10 hours, talking about conclusions and insights.

Jason called them overriding themes but it did seem a little stilted to put that in there.

You can do this sort of preform. We don't have to be on any structure. If there are some general comments regarding gap early in-vessel, ex-vessel, fine. Or if they are specific to the particular fuel type, then that's fine and we'll go ahead and list them, too.

This thing will go on for a number of pages depending on how much. The thought here is that if we can go ahead and get some of these conclusions and insight, it will help us to focus the report better.

With that it becomes an open meeting. I'm going to leave at 5 to 11:00. I guess you'll break at noon for lunch. Come back at 1:00 and then we'll finish up.

MR. GIESEKE: And you won't be back at all?

MR. BOYACK: No. I'll be back at 1:00.

MR. GIESEKE: At 1:00.

MR. BOYACK: I'll be there to eat lunch with you.

MR. GIESEKE: Oh, that's good. What are we going to have?

MR. BOYACK: I have no idea what's on the menu here.

Okay. Anyway, if somebody would just start.

MR. POWERS: The point I would make about the gap release and connection with high burnup fuel and with respect to MOX is if we do LOCA tests for high burnup fuel or MOX, we really ought to measure the fission product release and nail down the gap as best we can. We'll lose what gets vented just in reformulating the fuel, but you'll get some of that material.

MR. BOYACK: Okay.

MR. KRESS: It's difficult to do those tests. They are different than the tests you see that VERCORS present. You're going to get a gap. You take the fuel out of the reactor or irradiate it or something and you can't bandage the clad.

MR. POWERS: In any kind of test you are going to reformulate the fuel and you lose what you do on the venting but I think you get insight into what the gap release is.

MR. LEAVER: Is there a plan to do LOCA tests? You guys have discussed that?

MR. POWERS: There's a French proposal to

follow into the PHEBUS program to do what is called PHEBUS LOCA. Bernard can probably tell you. Since he's designing the program he can probably tell you in great detail about it.

MR. LEAVER: Is this sirgraphsen test or a true LOCA?

MR. POWERS: LOCA.

MR. LEAVER: ECCS type or some kind of reflood.

MR. CLEMENT: LOCA includes reflooding with ECCS. Not describing, not addressing every issue for ECCS.

MR. BOYACK: What was the comment about bundles with contrast to a few pellets? Is a bundle test at all possible?

MR. POWERS: Yeah. These PHEBUS tests, I think they use 15 rods.

MR. CLEMENT: For the time being we think about two different configurations. There is one with nine rods, one with 25 rods. In the interest of performing bundle tests there are true interest but not for source term. The first one is to have a good gradual radio-temperature distribution. The second one to see what is the effect of that reflooding. That's not the source term, Masseur.

MR. KRESS: The other comment is I think there is need to confirm that M-5 pad behaves from the standpoint of failure and mishap when the gap release occurs to confirm that it behaves pretty much like zirlo or zircaloy.

MR. SCHAPEROW: For the start of the gap release phase?

MR. KRESS: Yeah. It may protect the quantities because if it's more ductile, for example, it may swell and give a path for release of the stuff that's different from the others. It could affect both quantity and the timing. It's just a confirmation.

MR. GIESEKE: We talked a lot about the duration of the gap release that might be different.

MR. BOYACK: But that was between what?

MR. GIESEKE: I think we need to take a look at the M-5 cladding and more than just the first opening of it, but also what we call the progression of melting in it, if that differs, if it looks different, because we've made pretty much an assumption.

MR. KRESS: That doesn't have anything to do with the gap. That's for something else.

MR. GIESEKE: The endpoint of the gap or

the beginning of the early in-vessel, I guess. We talked about matching these curves with different slopes. That was kind of a fuzzy concept as I recall. We talked a long time about that.

MR. BOYACK: What else?

MR. KRESS: For the early in-vessel I certainly think it's very, very important to inquire and analyze and understand. To me that is priority No. 1.

MR. BOYACK: This is the VERCORS data?

MR. KRESS: Mostly VERCORS.

MR. LEAVER: FPT-2 would be nice.

MR. POWERS: It's not especially high burnup fuel.

MR. LEAVER: FPT-2?

MR. POWERS: Yes. It's interesting.

MR. LEAVER: I guess good fission product release data.

MR. CLEMENT: Not specific high burnup.

MR. LEAVER: Yeah, that's true. Just that a test has been done 18 months ago and it would be nice to see it.

MR. POWERS: You've got a ways to wait on that one. It's four years after the report comes out that you get to find out about it.

MR. KRESS: And also on particularly the early in-vessel, I think it's very important to point out that data alone is not sufficient. You have to translate that into a model and relate it to a whole core accident. I think that's an insight that is very important.

MR. BOYACK: So we'll translate data into model and then --

MR. KRESS: Relate it to a whole core accident.

MR. POWERS: It seems to me we need a test, I mean, that determines the releases of the volatile elements for the early in-vessel is the corporate core-melting progression. We need a test in which we are exposing fuel that has restructured at the boundary. That means something with burnups on the order of 60 to 75 gigawatt day per ton fuel. Absolutely has to be over 55. Preferably 60 to 75.

MR. KRESS: I would like to see 70 at least.

MR. POWERS: You said you have a substantial restructure regime and see what it's looking like. I don't know how that restructure regime is going to interact with the clad. That could be extremely different.

It seems to me in the ex-vessel category that the general feeling is that we have a fairly good, at least qualitative understanding of the melt-concrete release and it's not likely to benefit from a whole lot more work. You could probably refine the codes a little bit and get them up to the current state of understanding.

The real issue is once you failed the vessel, how does that fuel that remains in the core, what does it do? Modeling perspective or experimental

MR. KRESS: I agree.

MR. SCHAPEROW: Some of these, actually the one above it, maybe should go under high burnup. You've got this under general, Brent?

MR. BOYACK: Yeah. You'll have to tell me where to do that. I realized that some of that was doing that and I was going to come back and ask about that. That's a good point. It's easier to do now. Do both of these go down under --

MR. KRESS: I think those are both general.

MR. SCHAPEROW: One more.

MR. BOYACK: This one?

MR. SCHAPEROW: Yeah.

MR. KRESS: That's high burnup.

MR. BOYACK: That was here, right?

MR. KRESS: I think Dana might talk about needing the information on the effects of erbium.

MR. BOYACK: If you don't mind indicating which area we're talking about here, okay?

MR. GIESEKE: We're talking a lot about looking at the French MOX results. There should be some kind of cross-check made to the US weapon's grade fuel.

I don't know what that cross-reference is, whether it's just sections to see if it looks the same after some sort of -- maybe it could be qualitative just to see that there's nothing really different. I don't know. Is that going to be directly applicable?

MR. KRESS: Confirmed French data is going to behave just like where the stuff is going into Catawba somehow.

MR. CLEMENT: Once you've got that it's quite easy to prepare how are the fuels, what are the microstructures and so on and so on. The VERCORS fuel is extracted from watts coming from a French reactor. As far as I've understood, the MOX -- to the one that was used in the French reactors. The check would be easy.

MR. GIESEKE: I think it's important to

take a look and see.

MR. KRESS: I don't know exactly what all is in the French data but I think it's important that it have a couple of experiments like RT-7 where you've re-irradiated the fuel and run it through more than one kind of temperature transient and back out of that. You re-irradiated enough that you can get some of the mid-volatile or low-volatile releases in order to have a full spectrum of source term in more than just one kind of temperature transient. Maybe two. At least two.

MR. BOYACK: You had, I think, through the course of the meeting some general statements about the quality or characteristics of the data you needed for model development.

MR. KRESS: What I need is re-irradiated fuel so I can get the whole spectrum of fission products. I needed to run through temperature transients that are ramped and given rate and then maybe held constant for a little while.

I would like to see tests done with a faster ramp than we normally do because a lot of the fission product release is driven by the high heat upgrade of the oxidation. We never really get data there. What we do is ramp up and hold and ramp up and

hold but we never get a fast ramp.

I would like to see some tests with a fast ramp because you get a different kind of release characteristic with fast grams. I don't know how to do those because it's hard to do with the invection heating. You can do it but it gets difficult.

What I would like to see is faster test with the fastest ramp I can get with the kind of test you have to do. That's why I think it's also important to look at the PHEBUS data because you do run into the fast ramps there and reconcile the PHEBUS with the out-of-pile test.

MR. BOYACK: Let me stop here for one second. If you need to save, you can use Control S. Yeah, just under file. That looks very much the same. Use good English. Don't give them an opening to make fun because they will get after you really easily. They're a tough group. Anybody else here know how to use a Mac? I do have the new Dell online.

MR. LEAVER: We want to make a point on the early in-vessel that we should be breaking up the radionuclide groups where appropriate primarily to reflect where the elements in one of the existing groups has a significantly different release fraction.

Also, we may want to take into account

elements that have a significant biological effect. Maybe that would be another reason to pull one out of a group, one or two or more.

MR. CLEMENT: Maybe we could resize and identify several groups to do that.

MR. LEAVER: Yeah, we identified where --

MR. CLEMENT: Cerium group, nobel metal, lanthanides.

MR. LEAVER: Yeah, those three.

MR. TINKLER: The reason for having an assorted number of groups at one time was to --

MR. LEAVER: Something like that.

MR. KRESS: That's right. You can stick all of them in there you want to.

MR. TINKLER: We could probably have many more.

MR. LEAVER: We've got 65 or 70 isotopes. I don't remember the exact number but in our code we could have instructions for all of them and it wouldn't substantially change because you have those conversion factors and the half-lives that are different for all those, you have to take that into account.

I agree with you, Charlie. I think it's probably mainly historical that we did it that way

because of computing limitations that really aren't a big thing now. We also don't, of course, want to break up the elements that imply that we know more than we know. I mean, I don't want to over do it. We made some suggestions in here and I think you really should do that.

MR. TINKLER: I also want to make a point about early in-vessel. This is something that we're wrestling with now. This distinction of early in-vessel on the basis of vessel failure, we know more about source term than we did 15 years ago. We also know a little more about melt progression and interaction on the lower head and vessel failure times.

I would generalize to say that our convictions 15 years ago was not vessel failure. Generally those calculations accelerated vessel failure, notwithstanding the debates on industrial pension. Even ignoring the argument about vessel retention, I think it's true that the general state of knowledge would suggest that we believe the vessel will hold together longer.

Now if you tie that to a larger in-vessel release simply because we're holding the vessel together longer, we need to think about what is the

significance of early in-vessel from a regulatory point of view because, in fact, the revised source term has been adopted for the early gap and early in-vessel failures.

We need to think about how then we want to apply this because if we stretch out the in-vessel phase, is that really our intent to simply increase the source term of the early in-vessel release? Should we apply the same portion of the source term for regulatory applications?

With boilers particularly some of these predictions of vessel integrity are going to stretch out and even for the PWRs. I don't know what you think about the location or whether or not it's a gradual process or a large coherent relocation.

MR. KRESS: A little bit of insight on that, Charlie, is that whenever you undergo a core damage accident and start releasing fission products and then things melt and move down to the bottom head and end up interacting with the bottom head to eventually fail it.

My own view of that is you release the fissions products while the fuel is still up there in place. Once it moves down to the bottom head, you cut off fission products. I don't care how long it takes

that to melt through the vessel. Early in-vessel release has already happened irrespective. I cut it off at the time the relay takes the core down there. That's when I cut off the early in-vessel.

You all can take a note of half an hour or so to melt through the vessel. I don't care. I've already released my early in-vessel. I separate the failure of the vessel from the timing of the early in-vessel release in my mind.

MR. TINKLER: So in your mind when you do this you're actually --

MR. KRESS: Calling relocation.

MR. TINKLER: It does have some implications because what burned up on late in-vessel, some of that in terms of defining in-vessel failure may start the rope line early in-vessel.

MR. KRESS: Yeah. Another point I made earlier which I think is an insight that doesn't fit in here is we can constrain ourselves to thinking that early in-vessel, late in-vessel gap and ex-vessel kind of add up to one including wherever you have residual.

I think that's an artificial constraint that we shouldn't have done because it limits your thinking on how to apply design basis accidents to a whole range of scenarios. I think limiting yourself

to make those things add up to one is a mistake in the design basis space.

For example, the late in-vessel release I would have those relate to those scenarios that deposit a lot. Early in-vessel release I would have those relate mostly to scenarios that don't deposit much. I would be double counting.

I think it was a mistake not to view the design basis in the first place. The 1465 did it that way and to be consistent with 1465 we kind of artificially restricted ourself to making those things add up to one. If I were going to redo our file list in the future, I wouldn't do a design basis space that way.

MR. TINKLER: Some have larger in-vessel releases and sometimes larger late in-vessel.

MR. KRESS: I would reflect that in my design basis space and not just say this is a coherent accident of some kind. I think the Fritz people kind of do something like that with theirs.

MR. TINKLER: Dominant sequences rather than --

MR. KRESS: That would be another concept is dominant sequence, but there's more than one.

MR. TINKLER: It emphasize that part of

the release.

MR. KRESS: But there is more than one dominant sequence. You could use several. That makes sense.

MR. SCHAPEROW: I'd like to ask the panel to reflect a little bit upon the -- we've talked about updating or reassessing the revised source term in light of high burnup fuel. It seems to me that the changes that were made when we did that were largely predicated on better data and not necessarily on high burnup. I was wondering if the panel would like to draw any insights in that regard. That's an area of conclusions that would be very useful for us on a regulatory basis in the regulatory area.

MR. KRESS: The original 1465 you say you changed for just LEU.

MR. SCHAPEROW: That's right. How much of that would have been from better data and how much of that is from high burnup.

MR. KRESS: That's a good point.

MR. POWERS: I don't think we have any release data for high burnup fuel.

MR. KRESS: Pardon?

MR. POWERS: Do we have any release data from high burnup fuel?

MR. KRESS: Only from the French. That's the only data I know of. Plus the stuff from the Japanese.

MR. LEAVER: I guess knowing ahead of time what we were getting into we could have prepared a table of updated LEU for nominal burnups and then said, "Okay, here's the delta." Probably that delta would have been generally quite small. There didn't seem to be any real smoking guns on burnup.

MR. KRESS: I don't know if we ever need to --

MR. LEAVER: The implication of the high burnup table is those changes are from high burnup. Frankly, I think generally it's not. It's relocating.

MR. KRESS: That's what Jason was saying. With the MOX fuel, we were generally thinking that's moderate burnup, like 40,000 gigawatt. The question is should we ever have to worry about the combination of MOX fuel and high burnup?

MR. LEAVER: You've been saying right now that they might want to do that. It gives you flexibility in your fuel management.

MR. KRESS: I don't know if there are synergisms there or not and if the effects of one would be exactly the same as the other and you don't

double them. I don't know what the combination does.

MR. LEAVER: I'm not sure there would be any synergism, though, because you're talking about assemblies. One assembly is an LEU high burnup and the other is MOX with nominal burnup.

MR. KRESS: I don't know if you have the combination of MOX and high burnup, if you have to worry about it or if it would be different than just MOX or just high burnup.

MR. LEAVER: I'm not sure why it would be different. You mean if you were to come up with a source term?

MR. KRESS: Yes. Having MOX tends to make it easier to release. Having the high burnup tends to make it easier to release. Does the combination double that or do nothing to you? I don't have any data on that. I don't know if it's important or not.

MR. NESBITT: First of all, I think it's pretty clear, at least initially for the first several years our use of MOX would be constrained in terms of burnup as we discussed. We're not going to start radiating significant amounts of MOX until 2008 at the earliest. I think this is a real long-term issue.

The second thing I would mention is that the French and Germans are looking at higher

irradiation burnup levels for MOX fuel even as we speak. My anticipation would be that before we were to go there, it's very likely the Europeans would have done the same.

I can't predict the future but I'm just saying this is my best guess for 2002. I don't think it's worth this panel's effort in trying to figure out that issue at this time. I think that is something that ought to be deferred until later down the road.

MR. CLEMENT: I could add about that. The French activity has the intention to bring MOX to the same burnup level as $U0_2$. I don't have the schedule in mind but they are working on that. I don't know how it will end but this is their intention.

MR. SCHAPEROW: A longer-term issue is high burnup MOX. Does the panel agree with this? Maybe?

MR. POWERS: If you take the conscriptorial theory of the Government strategy to use MOX in these reactors, it has nothing to do with plutonium disposal. It's only setting up the first step and going to reprocessing. Then, yeah, you've got a long-term MOX issue. Quite frankly, that makes a lot more sense to me than saying this is the answer for getting rid of excess plutonium.

MR. KRESS: I agree.

MR. SCHAPEROW: There is one thing I'm kind of confused about is the issue of oxidation, oxidizing versus reducing environments and effects. Tellurium was the big one that really saw a big effect. I was wondering if the panel would like to draw some insights in that area for the NRC.

MR. POWERS: Great big capital letters. I DON'T UNDERSTAND TELLURIUM. The inherent difficulty is that --

MR. SCHAPEROW: In-vessel or in-vessel and ex-vessel?

MR. POWERS: Ex-vessel, I understand. In-vessel is where I don't understand. It's in the fuel. The problem is that you are blind to it most of the time. There's only a narrow window of time following an irradiated experiment where you can see the tellurium. In other words, you just go blind to it.

You can't find out where it is. Then it decays away on you and then you really can't find out where it is. Clearly the powers hypothesis -- if you take the Oak Ridge hypothesis that the tellurium interacts with clad, it must interact with the tin in the clad.

Now you go to M-5. What implications does

that have? If you take the powers hypothesis that is interacting with the conclusions and the fuel, what implications does MOX have on that? When I did 1150 my distribution was a 45 degree angle line and I just threw up my hands at tellurium.

And tellurium itself is just no problem at all. It's decay to iodine that's the problem. If tellurium would just arrange not to turn into iodine, we wouldn't -- well, if they would just ingest their spectrum so they didn't get the 131 or 132 tellurium, then we would be okay.

MR. KRESS: To test our fission products.

MR. POWERS: They just put some filters in there. We've got stable chlorine.

MR. KRESS: It's my theory that the reactors would be a lot safer if they wouldn't produce fission products. Pretty soon Jim Gieseke is going to raise his hand and say, "Being an expert on aerosol behavior, I fail to see why we don't include the release of the nonradioactive materials in our source term."

MR. POWERS: We really need to provide guidance on that one because that became an issue in the AP600. Quite frankly, you and I came up with wonderfully defended hypotheses. What it was is just

that they were different.

MR. LEAVER: We do release nonradioactive aerosols in our calculations but we've clearly stated the basis for that. I mean, obviously one can debate just what the right relevant amounts of fission products was enough. We've looked at all the experiments and come up with what we think are reasonable estimates and they are somewhat different for PWRs versus BWRs. It's an important number because aerosol concentration is the sum of both the fission product and nonfission product aerosol. Generally the more nonfission product aerosol you get, the faster everything is going to be removed.

MR. POWERS: Except for equipment qualifications.

MR. CLEMENT: The problem is not only the amount of unirradiated material that is released but the timing of release because of possible chemical reaction with fission products and different transport and depositions. Timing, I would say, is more difficult to calculate than the raw amount.

If your degradation calculation does not relocate the control of raw material exactly at the right level, you will not have the right evaporation and the evaporation rate will be very sensitive to the

relocation.

MR. POWERS: Get rid of the silver Indian control rods because we can't calculate them. They are expensive and hard to deal with and whatnot and boeing carbide worked just fine.

MR. LAVIE: There was an issue that Tom brought up yesterday and I followed up on this morning, to what extent that core fuel management strategies have on the source term. It probably occurs to the gap and early in-vessel phase.

MR. SCHAPEROW: Would anyone like to reflect on that? Anyone think it might be a significant effect?

MR. LEAVER: I don't see a basis for saying that's a significant effect. I guess I don't understand that.

MR. TINKLER: Tom thought it was significant. The argument was that the in-vessel phase was in part or the magnitude of the release was produced by the fact that the outer fuel assemblies are colder, substantially so. The core reacts more coherently and, therefore, more of the fuel releases fission products during that early in-vessel phase.

MR. LEAVER: Right. That was my understanding of his argument. It's just hard for me

to imagine that we can make a coherent core damage event.

MR. TINKLER: Or, at least, more so.

MR. LEAVER: Yeah. We've also based on in-vessel release numbers, to some extent anyway, on the concept of roughly 70 percent of the core having undergone a thermal assault which means you've got some portion that opens and some portion in the form of debris with clad having failed and pieces of fuel relocated. Even if you had more coherency, I'm not sure how that model would change.

MR. TINKLER: My point was that you have more coherency cladding.

MR. LEAVER: Right. It doesn't matter whether it's more coherent or not.

MR. TINKLER: At some point it doesn't matter.

MR. LEAVER: And relocation, I think, someone said a little while ago, I think, that a good view is that stops the race. You're going to form a crust and you're going to cool the material pretty fast.

MR. SCHAPEROW: Jim, we're discussing something that you were interested in and I kind of stuck your initials on something here. The effect of

field management strategies on source term.

MR. GIESEKE: We also talked about related to fuel management is the loading of the MOX in the outer regions of the core.

MR. LEAVER: I think we abandoned that.

MR. GIESEKE: We abandoned that in our considerations but it does have an effect on fuel management I would think on release numbers if you're going to deal with that.

MR. LAVIE: I think mutual wrongs is what we've done reasonably bounding to handle all the fuel management schemes. For instance, there used to be this belief that you have these three regions. Yet, we find that Westinghouse is currently wanting to flatten the flux and is now putting thrice burned decedents in the core. Is what was done in 1465 and what we've done in these meetings here reasonably bounding to address those type of effects?

MR. SCHAPEROW: This job is a lot tougher than it looks. I'm in a lot of trouble on this one. We're covering a lot of issues.

MR. LAVIE: Why don't we do high burnup first.

MR. SCHAPEROW: Let's just do regular. Let's do normal LEU. Does anybody believe that there

is an effect of field management on the source term tables we've produced? A significant effect.

MR. POWERS: An issue that I've always been concerned about is how we get the high burnup because there do seem to be two strategies. Whether that has any bearing on the source term or not, it's really on how you select fuel for doing the experiments.

If you do a rapid burnup versus a long slow burnup you'll have a mix of the two where it burns rapidly at first and then you move out to a lower burn rate. I will hasten to comment that I think it is very much a second order effect.

MR. KRESS: My thinking on the burnup thing is that normally when you talk about a burnup, they're talking about an average burnup of the core and the effect of burnup of fission product releases, not linear.

If you take a piece of fuel that has this added burnup and ready to release a fission product and apply that to the whole core and say this is the source term you're going to get on a whole core basis is not a correct procedure. What you need to do is have fission product releases a function of that and then apply it across the core distribution.

That was my point about how you do it, burnup and then fuel management because the effect of burnup of fission product release is not linear and it's not appropriate in the average burnup. That's basically my point.

MR. SCHAPEROW: Burnup dependence is seen in a certain range, you think? There was suggestions you didn't see it early on.

MR. KRESS: My own feeling is from burnups from 20,000 up to 45,000 there's probably not much difference because it's the same kind of release rate. Then you've got 45 up to 60 and you go up through some sort of transition where you get enhanced release. I think when you get up above a certain level, it probably levels off again.

There's not a linear relationship in burnup to fission product release. Somehow you need that whole curve of the effect of burnup on fission product and then look at the core and say what sort of distributions and burnups do I have and how do I factor this dependence of burnup into my fission product release on a whole core basis.

MR. LEAVER: Haven't we made some injuring judgements here in coming up with the high burnup LEU where we have more or less integrated these effects

into our judgements that you're talking about?

MR. KRESS: Yeah, but we've tended to say high burnup has one release and LEU has another and factor in the radio and average --

MR. LEAVER: The numbers that we came up with, let me try to say what I think they are. This is important. For a core in which the licensee has a license amendment. Is that what you need to get high burnup fuel? It has a license amendment for high burnup fuel.

We didn't come up with a course term for only the high burnup fuel. We came up with a source term for that core. We assumed that roughly a third of it would be high burnup fuel. You're saying you're not comfortable with that?

MR. KRESS: I don't even know what you mean by high burnup. My guess was that we thought a third of the core was a 70 gigawatt days per ton and two-thirds of it were at 40. Well, that's not true.

What you need is a distribution of burnups across the thing. If you want to get an average burnup to give you the same release that the distribution gives you, you have to do an integral over the whole --

MR. LEAVER: Tom, we've never done that.

MR. KRESS: I know we've never done that but I think that is the appropriate way to do it.

MR. POWERS: Aren't you gilding the lily a little bit?

MR. KRESS: Of course I am. Especially when you're talking design basis space.

MR. GIESEKE: I think the impact would be in the noise.

MR. POWERS: Yeah, I think I talked with Jim that we dallied around in the second decimal place and I don't believe in second decimal places.

MR. KRESS: I think if I view one-third of the core as having 70 gouged days and the rest of the core having 40, I'll get a source term that is higher than reality. That's all right.

MR. LEAVER: That's probably true but not by much and the effect wasn't that much anyway.

MR. POWERS: Before you say not by much, what happens is people go through and they do this analysis for some plant like an AP600 and they say, "Ah, 25 rem at the site boundary and, look, we met that. It's 23.67."

MR. LAVIE: But how would your analysis, Tom, change if there were only two cycles. No longer three regions but two regions. Try to capture in this

bullet here, "Effect of higher burnup assemblies dampened by fuel in core with burnup less than 45 gigawatt days per ton."

MR. POWERS: No. We have integrated those things in our high burnup tables.

MR. LEAVER: Yeah, I guess you're right. Since the numbers -- we put up numbers that we believe may be the first digit but they get used in analysis which they attach a significant third digit. The fact that you're conservative and you say that's fine. If they are less than 25 rem by one one-hundredth of a rem, that's good enough because we --

MR. POWERS: It's a question of how these things are used.

MR. LEAVER: We know how they're used. We see it all the time. People come in and they are really parsing the number out to a lot more significant digits than the input numbers where we're good to but it can never be worse than that. Then you're okay.

MR. POWERS: A regulatory system that really hasn't come to grips with realistic analysis, that's all.

MR. LEAVER: Yeah, that's true, although I think the source term is a step in the right

direction but just not as big a step as maybe we could have taken or eventually will take.

MR. POWERS: The alternate source term is information that has come in for the last 30 years that said that iodine wasn't the beginning and end of everything. In fact, as far as the total dose of things like that, it's not a huge effect. The amount of radioactive is actually higher.

MR. LEAVER: You're measuring dose differently now. You're doing a total effective dose instead of just a whole body. That makes a difference.

MR. POWERS: The total number of curies that you put in the containment is higher now than the GID source.

MR. LEAVER: That's right. You're also recognizing more phenomenon in terms of deposition and trying to do that more. In some cases it complicates the analysis but that's the way nature is so it's good to take those effects into account.

MR. SCHAPEROW: There was one thing that I thought I heard somebody say earlier about the BWRs went through the meteor tails fairly quickly as I recall and they stayed about the same as the old BWR. The changes weren't like huge from the original 1465.

I guess I would like to capture that insight.

MR. POWERS: First place, the BWR is not taking things to as high a burnup. Second of all, everything gets dominated by the fact that you have two times as much zirconium metal.

MR. SCHAPEROW: The atmosphere or the environment inside the core is more reducing.

MR. POWERS: That little steam molecule that shows up there gets molested severely. The answer is if the ABWR becomes the wave of the future by stalk and white chain, right?

MR. SCHAPEROW: There was one thing. Tellurium? Charlie, maybe you can help.

MR. TINKLER: Okay. Actually, this is a good intro. We increase the tellurium releases for PWRs more than BWRs because we thought they were more likely to see an accident. Do we really think that the timing of the tellurium release is inconsistent with those other volatiles in the early in-vessel phase. Timing makes a lot of difference in regulatory application of the source term in the early in-vessel phase.

MR. POWERS: All you get is 1.3 hours or 1.5 hours. You don't have any time resolution.

MR. KRESS: We do the same thing with mid-

volatiles. They come out at the end of that but we spread them out.

MR. TINKLER: What I'm wondering is in the PHEBUS should we make a little more distinction between the early in-vessel and what I'll call late in-vessel. I don't mean late in-vessel in terms of post vessel failure.

When I looked at the PHEBUS, that's a normal lodge to the active cladding. It looked like you were oxidizing about 85 percent. That's 65 percent of the total. If it's 85 percent, we think it's coming from that active cladding.

That's about as high up on the distribution oxidation as we can ever imagine calculating for a reactor. I don't have any doubts that the upper part of the bundle is given up pretty quickly. Over the lower parts of the bundle, you would think that would be stretched out.

I don't want to go over old ground to address this. I'm still wondering whether or not some of that tellurium release doesn't happen a little later as opposed to initial iodine.

MR. LEAVER: It lags cesium. We have a plot from Bernard's presentation at the last meeting.

MR. CLEMENT: We have probably even

oxidized more zircaloy. The onset of release for tellurium is delayed as compared to iodine and cesium and this is significant. Once tellurium starts to release, you have a rather fast release.

Even at the end, it becomes a little bit higher than iodine and cesium. That means at the end of this phase probably you get the same thing. You should be delighted that this doesn't mean that at the end of this early in-vessel phase could not reach the same kind of amounts.

MR. KRESS: That is extremely consistent with what we have found.

MR. NOURBAKHSH: But you still require the oxidation. I think in the late phase you do not have the steam to oxidize. I think the late release will probably occur but it will occur ex-vessel.

MR. CLEMENT: In a big core you will have different timing release of tellurium.

MR. KRESS: Most of the severe accident sequences you're only in reducing conditions maybe 5 percent of the time when you have a lot of hydrogen.

MR. POWERS: You're always in reducing conditions once you've gone through the escalation because above that burning point --

MR. KRESS: Yeah, but that part of the

release is fission products is my point. Below that you're going into the oxidizing phase and reducing the -- you know, the fission product releases see oxidizing conditions most of the time in accident sequence.

MR. POWERS: Most releases oxidize conditions most of the time.

MR. KRESS: There are some exceptions to that.

MR. TINKLER: My understanding is they have some excess during most of that.

MR. KRESS: You can certainly run tests.

MR. TINKLER: I know we've only got so many numbers.

MR. CLEMENT: At the outlet the oxidation of cladding is where 50 percent steam hydrogen and more fraction. Downstream the oxidation falls so it was a fair excess of steam.

MR. LEAVER: FPT-1 was, I think, an important data point for at least some of you on coming up with the tellurium estimate. That was a pretty big increase that some of you made in tellurium. I think the plot that Bernard showed tellurium actually overtook.

The release fraction actually overtook

cesium at about 90 percent release or something like that. It lagged most of the way. I mean, the lagging, I think, is an interesting point.

Another point which Charlie made, which I guess I hadn't realized, is you were oxidizing one hell of a large fraction of the zirc which is kind of hard to imagine that you could do that in a whole core accident which probably had something to do with the high tellurium release, all of which is, I guess, a round about way of saying why I guess I was, and still am, reluctant to do that big of an increase to tellurium.

MR. CLEMENT: You have to make the transposition of such data if you choose a curve value shown for FPT-2. You could imagine then for one region of the core so that it would undergo oxidation. Then it will continue after it is oxidized to heat up and then you will get this curve of tellurium.

MR. LEAVER: It's a level effect.

MR. POWERS: Suppose in a real accident you don't oxidize maybe 35 or 40 percent of the clad. What happens to the unoxidized clad? Well, it melts and runs down so that fuel now is back being exposed to steam as though its clad had been fairly well oxidized.

MR. KRESS: You can't divorce fission product release from the melt behavior. That's why I kept saying those tests on fission you have to translate those into a model.

MR. TINKLER: I don't know. Maybe the fact that the early release catches up with the end of the stage. I just wanted to get the panel's views.

MR. SCHAPEROW: I think this is very good to put in the summary. This is a good job. Oh, boy. I wrote a lot here. Anybody want to volunteer to give me a hand on this?

MR. LEAVER: I don't think you can say that. That was true of the FPT-1. Is that what you meant?

MR. SCHAPEROW: I'm not sure. I was doing a lot of typing when you folks were talking.

MR. LEAVER: The FPT-1, what Bernard said was by the end it actually catches up and it is essentially all released.

MR. SCHAPEROW: Tellurium released from fuel cesium. Once tellurium starts to release having fast tellurium release. You want to change that second sentence?

MR. KRESS: I would restrict FPT-1. That's very consistent with what we saw.

MR. CLEMENT: The graph I showed was FPT-2. It was for FPT-1 but --

MR. LEAVER: FPT-2 you said there was actually absorption that took place which you couldn't quite explain.

MR. CLEMENT: For FPT-2 I'm speaking about the release from the fuel zone.

MR. LEAVER: Okay.

MR. CLEMENT: Then you've got the position. If you look at the release from the fuel zone --

MR. LEAVER: FPT-2 is released part of the time.

MR. CLEMENT: Yes, part of the time. Should we say zirconium or zircoloy? This has been seen with zircoloy cladding.

MR. KRESS: I would say zircoloy.

MR. SCHAPEROW: Do we want to try to answer the question does timing make a difference in the tellurium release for the in-vessel phase?

MR. POWERS: The answer is, yeah, timing makes a difference because it changes your deposition fractions. That's how you account for the fact that you're getting a burst of releases at the end just changing the amount of deposits in the reactor cooling

system. If you want resolution below the one and a half hour level, you've got to have more time frames.

MR. SCHAPEROW: How is that? I'm running out of steam. This is a good place to quit. People can think in the back of their heads over lunch break of what other issues might be important to identify. It was kind of good that we got started on this.

I propose we break until 1:00 and we'll reassemble at 1:00. If anybody needs an escort, I'll meet everybody downstairs at 1:00 in this building.

(Whereupon, at 11:49 p.m. off the record for lunch to reconvene at 1:00 p.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

1:06 p.m.

MR. BOYACK: I've had a tiring lunch and I thought maybe Jason could just go ahead and continue on.

MR. KRESS: He was doing so good.

MR. POWERS: He gained a renewed respect for your talents.

MR. BOYACK: That isn't what he told me. He told me, "This is the first time I've had any respect for your talents."

MR. SCHAPEROW: There's only so many things I can do at the same time.

MR. LEAVER: He also told us he now knows why you get the big bucks.

MR. BOYACK: Typist is what I call it.

MR. POWERS: What he doesn't realize is that Brent doesn't get the big bucks. Brent's supports services get the big bucks. Brent gets the little bucks.

MR. BOYACK: So just let me sort of get quickly updated here. Jason said briefly that he and Charlie had been asking a few questions and you had been responding. Obviously you've been listing those in the files here. At least, there's more than when

I started.

Let me just take a quick look here. You'll get dizzy following me but it looked like --

MR. POWERS: Can you read that fast?

MR. BOYACK: I'm looking for the blanks. It looks like we haven't done anything with respect to BWR.

MR. SCHAPEROW: I didn't put them in the right spots. I was just typing. We'll have to move them around later.

MR. BOYACK: It seems to me the area that we have not touched on here that we have discussed in the rest of the meeting is the need for experimental data on the revaporization source terms.

MR. KRESS: Absolutely.

MR. BOYACK: And that's associated with everything or just --

MR. POWERS: It's the same question and it's appropriate for everybody. It's really telling the PHEBUS bunch to get on the stick and finish up their work.

MR. SCHAPEROW: When you say revaporization, you refer to a couple different areas. You're referring to revolatilization. You're referring to resuspension.

MR. POWERS: Really revolatilization. Resuspension is an area that probably isn't that important but we probably have an adequate modeling for it.

MR. SCHAPEROW: The two areas in the revolatilization that one is referred to as self heating and the other one is more of a chemical affect.

MR. CLEMENT: There are also all the chemical effects including actions for substrates and so on.

MR. SCHAPEROW: Chemical reactions and self heating.

MR. POWERS: A revolatilization species really your first concern are the cesiums and the iodines. The iodines is really basically a conversion of metal iodides and something that's volatile. Cesium is understanding what the hell a surface species is.

MR. BOYACK: Cesium was surface species?

MR. POWERS: Yeah.

MR. BOYACK: And the iodides were?

MR. POWERS: It's really oxidizing metal iodides to create something volatile.

MR. BOYACK: Okay.

MR. POWERS: Now, if we get the molys and the rutheniums up there, moly I think we kind of understand because it will be present as either molytrioxide or the cesium ruthenate. ruthenium is more of a mystery because it appears to reduce when the deposits get to the metal. You have to reoxidize it to get it back on.

MR. KRESS: I am convinced. How come there are no leates? You've got to have ruth, you've got to have lea, right?

MR. POWERS: That's exactly right.

MR. BOYACK: Okay.

MR. POWERS: Of course, one would be interested in what to learn in the revaporization. Since we can't figure out what tellurium is doing on release, why the hell should we worry about revaporizing. It would be nice to know.

MR. KRESS: I'm convinced you can't do these little lab experiments. You have to do them in something like PHEBUS where you've got everything interacting.

MR. POWERS: That's what they've got. They've got a plan that they expose to God's own mixture.

MR. CLEMENT: Samples or some measurements

still have to be made. Then after when you try to make a transposition through models, you have to make some hypothesis about what are the reactions involved and what are the components involved. Sometimes it could be needed to make additional experiments to be sure. That is the process in order to finish having validation model.

MR. POWERS: It's tricky work to do because they expose these planchettes and then they ship them off to laboratories. Things are changing all the time. They've got a pretty good program of mixtures of both transportation experiments and Wagmere vaporization experiments.

It's going to give us the insights I think we need. It's like all such test data. Once we've got it, all you have is what happens on that planchette. To apply it to the reactor accident you've got to have some way to transpose it into the reactor.

I'll comment that that is probably the area where things like the MELCOR and, for that matter, MAAP are probably weakest in their treatment of chemical phenomenon.

MR. LEAVER: Revaporization being the weakest?

MR. POWERS: MELCOR locked itself in to a pretty restrictive model. And MAAP, I haven't stayed up to date on MAAP but at least the MAAP structure originally just didn't devote much attention to it even though some of the better calculations on long-term revaporization have been done, it's not chemically sophisticated. Marty Plies did Peach Bottom and heavy vaporization going on for 51 hours

MR. BOYACK: Okay. Other observations? Have you literally covered things? I mean, is there anything to be added that is specific that you haven't already got up above related to BWR?

MR. POWERS: Nobody understands BWR.

MR. KRESS: Except Steve Hodge.

MR. BOYACK: So the answer is not really.

MR. LEAVER: Brent, you know, one thing you might want to do is maybe indicate some priority on some of these things. I guess I'm a little concerned that buried in here is the observation about the need to get the French data and to get it fast and along with other things which if they don't happen probably won't matter. We want to make sure that in our desire to be complete here that we don't miss the few essential things.

MR. BOYACK: Okay.

MR. LEAVER: I would be satisfied to indicate which items are of highest priority.

MR. POWERS: I would list the top three or four as separate items.

MR. LEAVER: I like that idea.

MR. POWERS: It seems to me it's easier to do that.

MR. LEAVER: I don't think we should try to do priority ranking for each one but maybe pull out the ones that are the really import low-hanging through here.

MR. BOYACK: Let's try something. We've got to start over. What else would you have on the list?

MR. POWERS: I think the acquisition of the MELCOR data certainly pulls the category of high-priority low-hanging. If I were going to take and define the things that I think will have the biggest impact on thinking, the next thing I would say would be degradation of high burnup fuel and degradation of MOX fuel.

MR. BOYACK: Are they listed here?

MR. GIESEKE: We talked about degradation. I know that was very early today.

MR. BOYACK: I don't know who was typing

but they obviously didn't use the word degradation unless it's a matter of -- well, how about if we do it this way. What is the word?

MR. POWERS: I would say core degradation experiments with high burnup fuel and MOX fuel. Those are not things you're going to do next week in your spare time. You can't claim them to be low-hanging fruit by any means but they have the potential of changing their perceptions on what goes on in these tests as much as anything.

MR. LEAVER: The way to realize this point you're making would be, for example, to have a future PHEBUS test be with high burnup fuel and with MOX. That's what you're thinking?

MR. POWERS: Yeah.

MR. LEAVER: This is a good way to give a message to the PHEBUS plan people, I guess. I mean, if anybody reads what we end up with.

MR. POWERS: Top and moly up there would be my highest priority except I'm not sure enough doesn't already exist from the VERCORS. If enough doesn't already exist, then more tests.

MR. CLEMENT: The kind of test you would like just to have.

MR. SCHAPEROW: When you talk about the

core degradation experiments with high burnup fuel, that leads me to think also about overall use of code like MELCOR or MAAP. The thermohydrolics and treatment has been improved dramatically over the last 10 years.

We've only run a few calculations since then. I'll put it out for the panel's consideration. At least in the US we have limited experience with using this improved thermohydraulic for severe accident analysis.

When we did the work on 1150 with Jim Gieseke and these people had this much reports of STCP calculation. We did 50 sequences. I think at least a partial reassessment of a lot of sequences would be -- I'm not a panel member. I just want to put that out for consideration.

MR. POWERS: It would be very useful but I think the first step is to exercise -- start exercising those curves against the PHEBUS experiments that you already have.

MR. KRESS: I interpret it to be both the PHEBUS experiment and to do something like that with CORA with high burnup and MOX fuel. Remember the CORA test?

MR. POWERS: Yeah, but the trouble with

CORA is I don't think you go to enough damage.

MR. KRESS: They were able to -- they stuck a heater rod up through the middle and that's going to be tough.

MR. CLEMENT: Are you speaking about the CORA in Germany?

MR. KRESS: Yeah.

MR. CLEMENT: There's a problem now, CORA scrunch so you can make good things for low level of degradation but not for high level of degradation because they are not so high to use true so they are zirconia pellets. As soon as your important detractions, it's no more available. It's a pity.

MR. POWERS: And similarly you have got to understand why you get differences between the VERCORS and the in-pile test. We do see differences in the release fractions.

MR. KRESS: I bet they have something to do with something other than fuel.

MR. BOYACK: Now, to sort out all this priority, at the present time I've only marked two. One is require to analyze the French VERCORS high priority data and Bernard is doing that so we can take that one off the list.

MR. LEAVER: Don't we mean MOX data there?

MR. KRESS: Iwould say definitely the MOX. I would also like to have high burnup.

MR. LEAVER: Absolutely. But the MOX data is what really hung us up for the low volatiles on MOX.

MR. KRESS: Yeah, but even high burnup is a problem because we were extrapolating to levels that we didn't have data for. I would put both of them in that category.

MR. SCHAPEROW: Are there any high burnup tests?

MR. KRESS: Yeah.

MR. LEAVER: How high did you go, Bernard?

MR. GIESEKE: See third line down from the bottom, Brent? It's the same sequence pertaining to MOX fuel.

MR. KRESS: That one had to do with the fact that the utilities may be using a different specification of MOX than was in the test.

MR. LEAVER: There was one VERCORS test that was a 60.

MR. GIESEKE: Come down a little further to the third line from the bottom.

MR. LEAVER: VERCORS 6. That's the only one. It's under the heading of MOX. It's getting

confusing now because --

MR. BOYACK: All right.

MR. LEAVER: RT-5 is high burnup. What bout RT-4, 3, 7 and HT-3? You can't tell. So it looks like 60 might be as bad as we've got. You might not see the effects.

MR. POWERS: 55 is kind of where you start seeing restructuring.

MR. KRESS: I'd like to see it at 60 though I would rather it be higher.

MR. SCHAPEROW: Do you want to write that burnup level next to that, 60?

MR. LEAVER: The four cycle. Do you know what that is, Bernard? Is that 60 or is it higher than 60?

MR. CLEMENT: I am not sure. Let me have a look.

MR. KRESS: Brent, the very top of the thing, if the French data does not have enough transients, I kind of lump those first two bullets together. They are related to each other. If the French data doesn't have enough of that in it, I say there's need for more data, more tests in that area. To me that would be a high priority. I just don't know how much already exist.

MR. LEAVER: What would you be looking for there that you didn't have in the existing core test?

MR. KRESS: Let's take RT-7. This is one of these kind of tests. That is precisely what we need. It's got one ramp rate and one whole and another ramp rate. It does have a lot of the mid-volatile release rates. I need to see the data for those as a function of time.

Then I need to take that and take some other kind of tips from the ramp to show that when one uses a relative volatility scale that different temperature ramps and different thermal transients can be correlated with each other on a whole core basis. I need a couple of tests, at least more than one. I need a couple of tests to do that with.

They need to be different thermal transients in order to test the hypothesis that you can take the data for two or three fission products that are along the volatility scale and extrapolate into all the fission products because you don't have data for real low volatiles.

MR. LEAVER: Remember this curve that Bernard showed for RT-1 and RT-2 where you had the ramp and then about an hour and a half hold and then another ramp.

MR. KRESS: But they're not really irradiated fuel.

MR. LEAVER: Yeah, they are.

MR. KRESS: Are they?

MR. LEAVER: Because this is actually four tests. It's RT-1, RT-2, RT-4. Oh, three tests. Three tests, yeah.

MR. KRESS: Those are all the same temperature transients. That may be enough. I'm not sure. We don't have any high ramp-rate data.

MR. CLEMENT: A point to be taken into account for future MOX tests is more than RT-2 or RT-7.

MR. KRESS: Yes.

MR. CLEMENT: That's a point to be taken into account in defining future tests.

MR. KRESS: I can certainly use all those there and they may be sufficient to some extent right now. You only get cesium with that and I've got enough cesium data. What I want are some of these other low-volatility and that's the re-irradiated part.

MR. LEAVER: The reason you don't get iodine here is just it's just too short half-life.

MR. KRESS: Iodine is a problem even with

re-irradiated because you have to for a chemical reason.

MR. CLEMENT: When this has been designed, there are tests that have been designed, some with irradiated addition and some without because with irradiated addition it's much more expensive. So for the tests without irradiation what was expected is to get a normal amount of release of low volatile in case you don't get the kinetics. You get the overall integrated release along the transients.

MR. KRESS: I really need the kinetics for this, the mid-volatiles anyway. I don't care about the kinetics for the real low-volatiles too much.

MR. LEAVER: Are you talking about strontium?

MR. KRESS: Strontium would be nice or metedula. Somewhere in that range.

MR. LEAVER: Barium is easier than strontium.

MR. KRESS: Barium would be nice.

MR. LEAVER: Bernard, what is OSIRIS mean as opposed to SILOE? Under re-irradiation you have a much of nos.

MR. CLEMENT: Oh, the name of the reactor.

MR. LEAVER: Okay. And then SILOE is a

reactor?

MR. CLEMENT: Yes, because first VERCORS irradiated in SILOE. That was convenient because the reactor was in the same place and VERCORS when SILOE was shut down and was not so convenient because they are not in the same places. Transport problem and so on.

MR. BOYACK: Okay. So basically we've got --

MR. KRESS: Actually, that other bullet right underneath there kind of belongs to that same one.

MR. BOYACK: I was taking several points but let's do it this way. Now we've got basically a trail that talks about acquiring and analyzing French VERCORS data. We've talked about the nature and type of the data. If it proves to not address all the issues, then it would be more data. Then there was this exercise of the codes. Those are the three things that we talked about thus far.

MR. KRESS: You'll notice that these fall into a couple of categories. One of them is insights and the other category is needs. We're kind of doing what we were supposed to do with our letters.

MR. BOYACK: Yeah. You still need to do

that.

MR. KRESS: We still need to do that? Darn. I didn't know I was that transparent.

MR. POWERS: Just make a copy of this and send it back to them.

MR. KRESS: I didn't know I was that transparent.

MR. BOYACK: The real key is that you'll think it through, you'll organize it, and I think it will be better. It may be brief and you don't have to do it red time if that makes it easier.

MR. KRESS: The blue I think definitely.

MR. BOYACK: Anything else by way of priority statements? You know what you talked about but otherwise you're just running through a bunch of things. What about the issue of LOCA tests?

MR. KRESS: I don't think we added Jason's comment that what could be useful to redo some of the thermal hydraulic core melt behavior with codes just to see if you've got new insights to it.

MR. BOYACK: I wasn't going to list it until somebody made the statement to the panel because he would refuse to accept it.

MR. KRESS: It depends on how extensive the problem. It will take a few sequences.

MR. POWERS: I would apply the codes, see that you were getting roughly the same behavior as in the PHEBUS test before I undertook the accident sequence calculations. They really haven't been very aggressive in either pre or post-test calculations of the PHEBUS programs. They are doing some now but --

MR. KRESS: The problem with PHEBUS in that respect is that they let things go down and in a real core I think you do this. There's a little difference in time and interpreting.

MR. POWERS: One of the issues that you worry about for doing high burnup fuel is you get fuel cooling and the whole degradation modeling was wrong. They are all candling models. If you do foaming instead, everything gets boxed up. We have not been very aggressive in developing capabilities in the degradation models. That's one of the biggest reasons to want to do the high burnup fuel degradation is to see if --

MR. KRESS: I see changes in the market. I think you're right. I also think -- no, I guess not. I was thinking M-5 may have a different --

MR. POWERS: I think I would investigate M-5, especially with the MOX fuel just to see because of lower propensity to oxidize on the inside if you've

got a more aggressive attack.

MR. BOYACK: Anything else that falls in the high priority area? What I've been doing is I've been adding more code related --

MR. LEAVER: What have you got now on high priority, just the two?

MR. BOYACK: So there's coordinated relation experience with high burnup fuel and MOX and acquire and analyze data. One was high burnup and the other one is MOX. Then we have a minority of one. One person that is basically staying here and I didn't hear anybody else say anything. That was on an "if" basis.

MR. KRESS: On the priority list this is looking at the gap inventory. We have one down there on assembly.

MR. BOYACK: What was the statement, Tom?

MR. KRESS: To do experiments to reevaluate the gap inventory for high burnup and MOX fuel. I don't know where we have that but it's in there somewhere.

MR. BOYACK: Right here?

MR. KRESS: I put that it's a priority but I but low on the priority list. We were just saying what's a priority but we weren't giving relative

priorities to it.

MR. BOYACK: You would have that in your top five?

MR. KRESS: It would be one of the top five.

MR. BOYACK: What I could do is send you this PDF file and you could take a look through it. When you write your letters you could pick up your top five.

MR. KRESS: That's a good idea. My comment about the core degradation including M-5 is a bit wrapped up in that bottom bullet. I have a feeling if it really is more ductile and you really have higher things, the ductile may affect the meltdown in core degradation behavior. It may create blockages easier. It may not candle in the same way.

We have core degradation as a high priority. I include that as a part of that core degradation. In your red up there I would add within five in the red part. The red part you can see there, I would add in addition to high burnup and MOX I would add within five clad on there. That takes care of my --

MR. BOYACK: Dave, you suggested priorities and then you were silent.

MR. LEAVER: That's fine. I --

MR. BOYACK: These are all right, there ones here?

MR. LEAVER: Well, I mean, if something is a high priority and one person is trying it, it's hard to think that we shouldn't reflect that. I was just concerned that if we make everything a high priority, then we lose the discrimination. If we make too many things high priority, we lose discrimination.

I think some of the things that we've indicated as high priority are not as high priority as maybe one or two things that are really crucial. I mean, getting the French data is absolutely crucial. Somehow we need to communicate that in whatever it is we do here.

MR. BOYACK: So, again, one of the ways of doing that is for in your letter just break it up if the three categories say high, medium, or low or however you want.

MR. LEAVER: Each person can do that in his letter but I was thinking more about this final report that Moshen is going to write however that's going to be put together.

MR. BOYACK: I think that's what we would rely on heavily on the letters because that represents

your written, thoughtful, well considered, mature. How long do I need to go on? I'm running out of words.

MR. KHATIB-RAHBAR: If you could please send the letters in the final format so we can cut and paste.

MR. BOYACK: The thought that occurred to me, you get to choose to cut and paste whatever, but you're talking about talking information out and putting it in other courses of the report. Yes. What I will do when I get back to work on Tuesday is I'll go ahead and send you -- I guess I can send you PDF files of everything we've generated.

Now, some of you like Word files, too? I'll send both in two different e-mails. One will have the PDF files attached and the same files attached on a second e-mail in Word.

On this letter regarding research needs, I would like to at least have an agreement with you on timing. I'm willing to give up yesterday but I wonder if we could keep it as close as possible out from yesterday. What do you need to do this?

MR. POWERS: Next week.

MR. KRESS: Next week.

MR. CLEMENT: I need to go through

discussion and then go through approval. I think it is very fast. I have to discuss it with other people.

MR. BOYACK: Would two weeks be a reasonable target? I understand it may take a little longer to go through the bureaucracy. You might have to deal with your colleagues.

MR. KRESS: Jim Gieseke will give you his before he leaves.

MR. GIESEKE: The Bureaucracy is pretty small. For you too, isn't it, Tom? Don't you have to run it past your wife?

MR. KRESS: No.

MR. KHATIB-RAHBAR: That's the week of March 3rd or 4th?

MR. BOYACK: 8th is the Friday. That's March 8th, a Friday. I guess what I'll try to do, Moshen, by the 6th I'll have you my chapter.

MR. KHATIB-RAHBAR: I'll be out of the country by the 7th.

MR. POWERS: You're always out of the country. What difference does it make?

MR. SCHAPEROW: From the NRC's perspective we're hoping to spend some time on this in March. We've got some internal deadlines for a draft report by the end of March. It sounds like we'll be able to

make it happen which is good from my perspective.

MR. KHATIB-RAHBAR: The letters are only part of the deal.

MR. SCHAPEROW: Again, it's only a draft report.

MR. BOYACK: I felt that the biggest hole have been the chapters that I generate.

MR. LEAVER: Jason, what are you thinking about the French MOX data and if we are able to accelerate that process and get that data, say, in a few months which my guess is if this group were to be sitting here today, would that data -- we would have numbers for the level of confidence more like what we were talking about for high burnup as opposed to rather low level of confidence.

I'm just trying to understand suppose that data does come in a few months. What are you going to do? I mean, would you ask this panel to come back together and discuss that data or would you just use it yourself?

MR. SCHAPEROW: I guess that's an option.

MR. LEAVER: You don't know? Okay. We can talk about that.

MR. POWERS: The problem with data it makes it much harder to prepare MOX.

MR. LEAVER: To do what?

MR. KRESS: Without the data we can do really good MOX. It's like more than two data points.

MR. POWERS: If I get more than two, I'm in real trouble.

MR. KRESS: You can draw a straight line with two data points.

MR. BOYACK: Let me return to the report. I'll get my things to Moshen. The letters will be there approximately the 8th. He has some processing to do and then we need to get the report, the draft report back to the panel. We'll have to turn it around relatively rapidly. It may be a challenge.

MR. SCHAPEROW: I think it's not clear that it has to be much more than a really rough draft by the end of the month. That would probably satisfy our needs.

MR. BOYACK: You can always hold up the draft but the real key is the panel, I think, has to be able to review the information. I'm agreed that this is getting close to a final product before the NRC maybe starts using for any other purpose. Otherwise, that's the horse before the cart.

MR. SCHAPEROW: Also the further we get away from the meetings people's memory fades as to

what we discussed and some of the details. There are a lot of advantages to doing it right away.

MR. BOYACK: I'll have mine in on the 6th. Moshen will have everything else done on the 7th. You'll need a week, right? The real question is if you get it like the 15th or so, will you be able to turn it around in a week?

MR. SCHAPEROW: I think so.

MR. KRESS: I'm very dubious about March 15th. It's my Roman background, I guess.

MR. POWERS: Now, do you see yourself as Caesar or as Brutus?

MR. KRESS: Brutus.

MR. POWERS: So who are you going to stab?

MR. KRESS: I'm the stabbee. I mean the stabber, not the stabbee.

MR. BOYACK: You made the same mistake in the lunch line yesterday.

MR. KRESS: Oh, yeah. I did. I got my things mixed up that I ordered. I do that pretty often.

MR. POWERS: So the ides of March shouldn't bother you at all if you're the stabber instead of the stabbee. It's on the stabbee that suffers.

MR. KRESS: It puts a lot of pressure on me to figure out who the stabbee is.

MR. BOYACK: What I'm hoping is if we can get the draft report, it may have a few holes in it but the draft report by the 15th. Then we can give you like eight working days to review it.

MR. LEAVER: I thought the NRC was going to put a draft report together by the end of March. You're talking a draft of a draft?

MR. BOYACK: It should be a good draft. If the panel has read it -- well, the draft they're going to put together is what most of us get. That's their draft. That's the NRC's draft. If we come back and you come back with your comments by the 27th of March, that gives us a little bit of time to try and resolve the comments and make a draft that they can say, "Well, we did it." Then it will come back out again to the panel.

MR. LEAVER: Okay. Right.

MR. BOYACK: All of this will be written down in an e-mail but I just wanted you to be aware of it.

MR. LEAVER: One option I would hope that the NRC would consider is capture this as best we can in a draft but if it looks like we can get the French

data in some reasonable period of time, that we wait before we publish a final report to reflect that. I offer that as something to consider.

MR. SCHAPEROW: Sounds optimistic to me.

MR. LEAVER: Well, maybe.

MR. SCHAPEROW: Given the history of this issue.

MR. LEAVER: Well, you have a big hole there.

MR. GIESEKE: It's not a whole. He has numbers in it now. They'll use whatever numbers they have.

MR. LEAVER: Those numbers, we really didn't have a very good basis for those numbers. The purpose of this exercise was to deal with burnup and MOX and there's half the MOX problem that we really couldn't deal with very well.

MR. GIESEKE: Dana gave them numbers.

MR. POWERS: They've got numbers that are good to three decimal places.

MR. LEAVER: Three decimal points?

MR. POWERS: Oh, yes.

MR. LEAVER: Including the one to the left of the decimal?

MR. POWERS: It's just the one to the left

of the decimal.

MR. BOYACK: I haven't put dates on those others. Okay. Will that do it? Any other comments, questions, issues?

MR. SCHAPEROW: What is today? That gives you about a week and half to get your stuff to ERI?

MR. BOYACK: Sure. Gotta do it.

MR. SCHAPEROW: It's going to take a while actually. No need to get it done any sooner anyway.

MR. BOYACK: This is about all we can do.

MR. GIESEKE: What do you intend to do for those tables that have the distribution of values?

MR. BOYACK: They will appear as individual values entered but unnamed.

MR. SCHAPEROW: That's for A, B, C, and D.

MR. BOYACK: I don't have any plan to do that. Just the individual values. Do you have any other suggestions?

MR. POWERS: Especially on the MOX what you want to do is wish 1, wish 2, expert 1, expert 2.

MR. NESBITT: Why won't you put the names next to the radius?

MR. POWERS: Because that might motivate people to discount some of it.

MR. BOYACK: This is just my view. If

somebody else has a view and the panel wants their names, I'll be glad to put their names.

MR. SCHAPEROW: That's what we've done. We didn't write it down. It was never necessary.

MR. KRESS: How useful is it?

MR. GIESEKE: How useful is stuff where there's three no entry plus one number? June take the one number and use it, I suppose. That's what will be done.

MR. BOYACK: That would be inappropriate. What I will do in the text is I'll pick up comments. There was a fairly long discussion in the morning about these data and the lack of data. Dana when he gave his, this could go in an entirely different way. So I guess I shouldn't be making decisions on this.

I mean, the panel owns the opinions, not me. What happened towards the end is each individual elected based upon what they perceived, whatever set of perceptions they had either to say, "I'm not prepared to give a number," or, "I am with the qualifications in the listing." Now, the use of the stuff, of course, is not something we can control.

MR. LEAVER: Not something what?

MR. BOYACK: We can control.

MR. LEAVER: I think the NRC is the one to

decide how it's to be used.

MR. BOYACK: Well, for their use.

MR. LEAVER: We can maybe make suggestions how we think it should be used but ultimately that's their decision.

MR. KRESS: If I were NRC I would pick the highest number. Isn't that what they always do?

MR. LEAVER: They don't always do that.

MR. KRESS: They don't?

MR. LEAVER: No.

MR. KRESS: They could use the minimum entropy average but don't ask me what that is. I have no idea.

MR. BOYACK: Is there anything else before we adjourn?

Charlie?

MR. TINKLER: No. I think the committee probably got the kinds of deep facts that we were seeking. We can't reach a conclusion on certain things because of lack of data. That's understandable.

I guess my initial reaction to this is that the committee has done quite well considering the available information. It has involved a fair amount of integration. I would expect that we are a long

ways from adopting regulatory values.

This thing goes out and is published in draft and everybody in the world who has an interest in source terms comments on it. That's a long process.

I'm not adverse to the idea of reconvening the group at some later time to present some additional information. That's still possible. To the extent the committee has provided recommendations for high burnup, that's very useful information for the NRC. There's still some more to be done. That's the state of affairs. That's the way I view it.

My initial reaction is I can't imagine that if there's a preponderance of committee members that feel there's no basis for offering a number for MOX we would say we feel bold enough to substitute a value. Maybe but I have trouble imagining that one, to tell you the truth.

MR. BOYACK: All right. Any other comments from the panel members? Actually, it's been fun working with you. I like working with a small group. I've tried big groups and they are hard. Small groups are fine where six experts are really good and it seems to me you really tried to go ahead and meet the objective state of the panel. You've

done a good job so thank you very much and we stand adjourned.

(Whereupon, at 2:01 p.m. the meeting was adjourned.)