ML020770189

# Official Transcript of Proceedings

# NUCLEAR REGULATORY COMMISSION

Title: Source Term Applicability Panel

Docket Number: (not applicable)

Location: Rockville, Maryland

Date: Wednesday, February 20, 2002

Work Order No.: NRC-245 Pages 276-527

**NEAL R. GROSS AND CO., INC.**
**Court Reporters and Transcribers**
**1323 Rhode Island Avenue, N.W.**
**Washington, D.C. 20005**
**(202) 234-4433**

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

+ + + + +

SOURCE TERM APPLICABILITY PANEL

+ + + + +

WEDNESDAY

FEBRUARY 20, 2002

+ + + + +

ROCKVILLE, MARYLAND

+ + + + +

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

C-O-N-T-E-N-T-S

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

(8:46 a.m.)

MR. BOYACK: All right, let's go ahead and begin then.

What I would like to do is just review for a moment what we are going to do today. There's also been a few questions that have been asked about procedure, and I think those would be worthwhile covering.

Let me first deal with the matter of the documentation that will be produced as the product of this activity. Most of the people are aware that ERI is pulling together a document that has pieces coming from various individuals. I've just given the tables; most of this, compiled some front-back parts. Then after this meeting I believe I will have the major piece of work to do, which is to get the tables and the associated chapter or chapters that talks about that information.

After we do that, then the document will be compiled and it will be sent to the panel members for review and comment. You should feel entirely free to review that, comment on it as you feel appropriate.

We will have to do our document updates and revisions by email, .pdf files, Word files, et

cetera, whatever you can work with, so that you can work with all the document and provide your comments. Those in general will be incorporated, and then they will have to go back, and what we'll do probably is any changes made could be put in color. So that when you get the file back, you can look and see what has been changed since you last saw it.

MR. LEAVER: Just regular text editing would highlight that.

MR. BOYACK: Well, there is that possibility of doing that, but I find that they're awfully hard to read sometimes.

MR. LEAVER: Oh, okay.

MR. BOYACK: Because they have --

MR. LEAVER: So you've done this before?

MR. BOYACK: I guess they have a mechanism where you can just have the changes shown, and that would work, mostly just to do that. The real key is that the changes will be highlighted in a way that you can easily discern them.

Now we'll work on the schedule and talk a little bit about that tomorrow.

The second question that came up was a little bit about whether we are going to refine the BWR tables specifically, and that portion of the BWR

tables in which we have multiple entries, one for each individual. What I have tried to do is explain what I perceive to be the NRC policy or priorities. I will go ahead and say this now. So if it doesn't agree with what Jason feels, then he will go ahead and correct me.

We have had a very ambitious set of objectives for this meeting. That is, the PWR source term applicability, the BWR source term applicability, and MOX source terms, if you will. We have had three meetings to accomplish this. Of course, the first meeting was very much of a startup meeting. We made good progress in the second meeting. We have made good progress in this meeting.

The NRC has basically told me that what they want is the panel's input on all three areas. So we haven't eliminated anything. We have worked on the PWR; we've worked on the BWR; we'll be working on MOX today and tomorrow. But we do have a definite time limit, and that time limit is three o'clock tomorrow afternoon, when we all turn into pumpkins.

So what will happen is that we will go through the MOX today and as much as we need tomorrow. If there is any time left, then we can come back and talk about these, the BWR and PWR -- we have

individual values -- to see if a little more discussion brings you to single values as a panel. So time will be the determining factor on whether or not we come back and look at PWR and BWR anymore.

The real key is to get to the end, to get the panel's input on PWR and BWR. What we have done thus far in each of those areas is satisfactory to the NRC, as I understand it.

Now the question is, well, who will go ahead and process these multiple inputs to come up with source terms, say, for the BWR, and the answer to that is the NRC staff will do that. We will not do that as a panel. We will not do that as authors of the report, unless we are able to come back and come to these single items.

MR. NOURBAKHSH: So the report will have what regarding tables?

MR. BOYACK: It will have, essentially, the tables regenerated in the form that we've generated them. My guess is that we will not name the individuals, but we will show their values.

MR. GIESEKE: So then I presume that the NRC will take that -- I assume they want to do another one of these sorts of reports --

MR. BOYACK: I don't know the answer to

that.

MR. GIESEKE: -- tables with single values in them?

MR. SCHAPEROW: I think what we are going to need to do is look at the needs area, because a lot of the numbers are based on best judgment, and there's an idea that they may even go back and do some code calculations and maybe even a few more experiments, particularly in the MOX area.

The two ideas that we have been discussing at great length is ideas of improved, better data, more recent data, better data, and the second idea being the effect of burnup. We are going to try to think this through on our conclusions in both areas.

It seems to me that the major effect we are seeing is that of the more recent data and the better data, the more recent data being better instrumented and better analysis. So that's one issue.

The other issue is the burnup issue. It seems to be much less of an issue as far as changing the numbers. We will need to sort through that after our meeting. I don't think we are going to have time to do that in this meeting

MR. LEAVER: There is data for burnup, and

there really isn't for the MOX. That is the problem. You might not see much of a difference for MOX if you had the data.

MR. SCHAPEROW: We will have to wrestle with that after the meeting. We certainly aren't going to be able to resolve that today or tomorrow. I guess there is a far outside chance we may be able to call another meeting in a few more months. I doubt it, though. I don't know. That's in the back of my mind as a possibility, if people are available and all.

But I think we really just need to get through MOX as best we can, and the NRC is going to try to draw conclusions and provide directions on programming. It would be nice to be able to publish another document like that, but I'm not sure we're quite there yet. I don't know if that's disappointing or not.

The thing is again this issue of what we call back-fed, or whatever you've got, and now we've got some higher numbers in certain areas, not as a result of higher burnup, but as a result of improved insights from recent experiments. But we have to wrestle with that issue a little bit.

I don't know if you have anything to add,

Jay, in that regard.

MR. BOYACK: Let me return then to the activities for today. We are going to be working on the MOX area today, MOX source term.

The procedure is that we are going to first -- well, second -- bring up the table that we talked about yesterday. That's the table which we will go ahead and try to list the differences, the characteristics that have differences between the MOX and the LEU. If we see any research needs, we can identify them at the time, but it is not absolutely necessary. We will go through that table.

Now once we get that information down, which sort of serves as a foundation, common viewpoint, then we will go see if we are able to do the source term tables. Now prior to that, we have two other pieces of information, very brief.

One of them was Steve Nesbitt wanted to just make a few points about MOX. Steve, you are willing to arrange for somebody to come in and talk about power --

MR. NESBITT: We said we would do that on the last document.

MR. BOYACK: So that will be tomorrow afternoon?

MR. NESBITT: Right.

MR. BOYACK: Now by that time, we will have very little time to go back and do anything with the tables, if it's in effect. Now the primary factor there was that you heard us talking about the fact that the additional power would lead to additional releases, and that may have been affecting one or two of the people's input regarding source terms. So it would help if we could have that before the time in time to react.

Now before I turn the time over to Steve Nesbitt for just a moment, is there anything else that anybody wants to bring forward to the panel before we continue on?

(No response.)

Okay, Steve, you have a few comments?

MR. NESBITT: Yes. First, there was a couple of follow-up items from yesterday. There was a question about the kind of power history that the MOX fuel assemblies would see, we think, in our folders, and we operate on them.

There is some information on that, although it doesn't present it side by side with the LEU fuel, but the power histories are generally similar. There is some information about that in the

Fuel Qualification Plan, particularly I think it's Figure 8-2.

That kind of leads me into a bigger thing that I wanted to talk about. I had been under the impression that you, as a panel, had been provided the Duke, COGEMA, Stone & Webster Fuel Qualification Plan for mixed oxide fuel for review as a part of this activity. Based on the discussion yesterday and last night, I guess now I understand you didn't get that. Maybe everybody didn't get it or all that kind of thing.

There's a fair amount of information in there in terms of our overall approach for getting regulatory approval for use of MOX fuel here in the United States that I didn't bring out in the discussions back in December because I thought it was kind of inherent there in material that you may not have seen. I guess I want to cover a couple of things there. I promise I'll be brief.

But the fundamental basis for our application, upcoming application, to get approval to use mixed oxide fuel in the United States is that mixed oxide fuel is very similar to uranium fuel, not identical, and some of those aspects of differences we brought out in December, and you're well aware of.

One of those differences that bears on what you're doing being the fact that obviously the plutonium is in very small, dispersed, plutonium-rich particles throughout the fuel rather than being completely homogeneous. But recognizing that there are some differences there, fundamentally, it's ceramic oxide fuel with similar characteristics, predominantly uranium.

When it comes to source term, I am going to tell you something that I think everybody in this room knows, but I'm going to tell you anyway. Pardon me if I'm preaching a little bit.

Here in the United States we employ a fundamentally conservative approach to using source terms for the analysis of design basis actions. We use a source term from a core melt event for accidents that don't give you core melt. That is consistent with the conservative deterministic philosophy that we used to license nuclear power plants, and it served us very well.

As Dave Leaver pointed out to me a couple of minutes ago, it is water under the bridge, and we're certainly not proposing to change it. Nevertheless, it is a major conservatism that we need to keep in mind, I think, as we move forward.

Alternative source term, we at Duke Power view as predominantly a good thing. As I have discussed, we are planning to move forward with application of alternate source term for our plants in the near-term.

It offers -- and the words are quoted from NUREG 1465 -- "a more realistic portrayal of the amount of fission products present in containment from a postulated severe accident."

NUREG 1465 also says, "Release fractions are intended to be representative or typical rather than conservative or bounding values. The release fractions are not intended to include all potential severe accident sequences, nor to represent any single sequence." I think everybody knows that, but I think it's worth a reminder every once in a while.

So what are you guys going to do today and tomorrow on MOX fuel? Well, the way I see it, the fundamental question before your panel is: Is the NUREG 1465 alternate source term reasonably representative of plants that are operating with some fraction of the core being mixed oxide fuel, in light of the inherent uncertainty in a representative source term that's derived from a combination of experiments and calculations that model complicated, interrelated

phenomena in the thermal hydraulic and chemical and mechanical area, and given that there is a fundamental conservatism that's inherent in how we apply these source terms in regulatory space that we shouldn't lose sight of?

So, with that being said, I don't presume to answer the question for you. You all are going to answer the question.

MR. KRESS: I don't understand your fundamental conservatism. The reason I don't understand it is because primarily I'm interested in preserving a level of risk that's acceptable. I do that by this somewhat stylistic approaches and DBAs, and then couple them with some sort of switch term, to design a system that's robust against all accidents.

I don't know that putting in a source term like we put in, or coupling in that manner, is conservative or not. In fact, it very well may not be, if I'm trying to preserve a level of risk that's acceptable from the standpoint of really looking at the risk. I can't make that connection between the design basis phase and this phase. I don't know that we're conservative at all.

MR. SCHAPEROW: I would like to further suggest that this is what the agency uses to

demonstrate their protection of the public against a severe accident without core cooling. This is it. We also do have evacuation plans, but I tend to agree with Tom in this regard.

MR. NESBITT: And I'm not challenging that. I think I'm agreeing with it.

MR. SCHAPEROW: Oh, okay.

MR. NESBITT: But what I'm saying is that that's not all we use it for. I mean, we use it to determine, for example, whether the results of a loss of coolant accident are acceptable or not from a dose perspective. You might argue that, well, we're not really just looking at loss of coolant accidents. We're really looking at anything that might happen.

MR. SCHAPEROW: That's right. This is a long-term loss of coolant accident that we're looking at.

MR. NESBITT: Yes, but we look at it in both contexts, and in risk base I agree with everything you said, Tom. We do think it's important, and we have a lot more risk insights now into how our plants operate than we did when the original licensing basis was constructed back in the sixties and seventies. We have probablistic risk assessments and safety goals, and all that kind of stuff, that give us

more information about how we stack up in that area.

Again, I'm not trying to answer a question for you all. That's what your panel is going to do. I'm throwing out, I guess, the way that I see the question, which is that, given what the alternate source term is used for, is what we've got appropriate for application to MOX fuel? If not, are there adjustments that can be made? And if that's not the case, is there additional work that can be done to fill the gaps?

I wasn't here at the first meeting, and maybe that's what the NRC told you, or maybe they told you something else, but I guess I'm throwing it out because I wanted to have an understanding, if possible, that that is what you all are doing or maybe you all are doing something else. Maybe that's a question for the NRC more than it is for you all.

MR. KRESS: I think what we're doing is actually trying to carbon copy what was done in the past with regular fuel, in the sense that we've developed the design basis source term that somehow comes out of information about how a core melts and how these fission products get into a containment, and what those quantities might be, representative of a range of accidents. Then we are going to take those

and apply them in the design basis phase.

Nobody has proven to me that that's the best way to regulate. It turns out that it's pretty good because, if you go back and look at all the plants that resulted, the design that resulted from this, they've been pretty safe from a risk standpoint.

So we are taking a leap of faith, but that's probably a good way to do it, and, in fact, may be a conservative way from the standpoint of, is there a design that's realmly something like the risk acceptance criteria?

So what I think we're doing is we're just going back and repeating that. The only difference is MOX may have a different set of fission product releases over a range of accidents. We're just going to repeat the same process. We didn't know if it was going to work the first time, and we don't know if it is going to work this time, but it might. The proof of the pudding is going back and doing a complete risk analysis to show that you didn't really achieve it.

See, the problem is what I envisioned going on is -- we'll take Chapter 15, "Range of DBA Accidents." We'll use a new source term, if somebody comes up with it, and it shows me all of the triggers of merit that you have to meet.

MR. NESBITT: Hopefully.

MR. KRESS: Hopefully.

MR. NESBITT: Or not.

MR. KRESS: If you're not, you'll have to change something in the design of the plant --

MR. NESBITT: Right.

MR. KRESS: -- like the leak tightness of the fuel or the containment or something, or you may have to do something, back to sprays, or whatever. But it looks to me like the source term is not a stone plate in meeting those Chapter 15 figures of merit. The possible exception is the leak tightness or the containment.

MR. NESBITT: And the controller.

MR. KRESS: And the controller. Those are the drivers.

So what we're going to do here, coming up with a source term, assuming it is going to be higher, it can give you some grief because you're going to have to show that the end leakage of the petroleum is maybe different than you thought or the leakage is the same. I think that's about the only -- it's not going to do much to your equipment qualifications or much to your isolation. It's not going to do anything to ECCS.

So what's going to happen is you're clearly changing the risk significantly if you have a much higher source term. You can really change the risk significantly, although affecting the design basis phase is relatively insignificant. So I have a real problem with going in on the design basis phase only. I think you're going to have to come forth with here's the design basis phase; we need all these things, plus, here's our risk analysis to show that we did get out --

MR. NESBITT: And that's exactly what I wanted to add, and that's laid out in our Fuel Qual Plan. In addition to addressing the design basis accidents, it is our intent to perform full level 3 PRAs for --

MR. KRESS: Well, that's what I was leading to. You will need --

MR. NESBITT: For a side-by-side comparison of the risk involved.

MR. KRESS: Yes. Whatever we use in the way of thinking and models and data to develop our design basis source term, you will need those models in thinking, in doing your risk analysis. That's where I think a lot of this is going to be most useful, because I think you can meet these Chapter 15,

no matter what we come up with, because we're not going to do that much to it. But when you go to do your risk, you can't just use the map; you can't use it. It doesn't have the right fission product release for what you've got in it. I think that's where what we're doing is going to be useful to you. It's going to tell you what you have to do to do your risk analysis better for this.

This it the perspective that I thought I'd throw in. So I see the thinking and the models and the data we're using here, it's probably going to be more useful to you than the actual source term we come up with.

MR. MARTIN: I'm Bob Martin. I'm the Project Manager for the Nuclear Reactor Regulation here in Rockville, focal point for communications regarding MOX. Several of other NRR members are with us today: Steve Lavie and Jay Lee.

The report referred to earlier, the Fuel Qualification Report, is one that Duke has submitted to us. Our most recent revision of it is April 2001. They submitted it for information to the NRC staff. It has been useful as an information reference for us since then. I have a few copies in my office, which I'll share with you today. I'll send them to

reproduction and I'll have copies for you tomorrow.

I have a recent summary of minutes, summary of a meeting we had here regarding research activities; also, information that's on the public record. I'll make copies of that and provide it to you before you leave.

MR. SCHAPEROW: I appreciate that. I'm pretty certain that I mailed it at least to the panel members. It was a while ago. It was months ago I sent it to the panel members, that's true. I can go check. I think I have a pile still in my office. I can go check on the break.

MR. BOYACK: All right, Steve, was that your comments or do you have any others?

MR. NESBITT: Yes, that was basically it. A specific question came up yesterday about the power profiles for the LTAs and the MOX fuel assemblies. There is a figure in that report -- I think it's Figure 8-2 -- that shows some additional information on that. I don't think it's any earth-shattering fact that's going to change any conclusions of the panel.

MR. BOYACK: All right. Now if I recall from yesterday, what we said we would do first is that we would go through a few moments where we talked about various characteristics that we thought were

important, go ahead and indicate the LEU behavior, the MOX behavior. If I understood that right, this is the type of thing that we might do: MOX assemblies in the core, zero in LEU and 40 percent in the MOX case; the plutonium in the two cycles, 1 percent in LEU, 3 percent in MOX; cladding with zircaloy, M5. This was just my attempt to take a few things and start to list them.

So that's what I would like to do now, is to have you identify the characteristics that you think are worthwhile taking into account, and then we will go ahead and just distinguish between the LEUs and the MOX.

MR. CLEMENT: I think the most important difference between the two fuels is the microstructure of MOX as compared to LEU. Because in MOX you have two phases, too many phases for the fuel. You have the plutonium-rich agglomerates which are roughly of the size, say, of 16 micrometers, and inside are uranium-rich matrix.

That means that, in fact, nearly all the fission will concentrate in the plutonium-rich agglomerates, and this is where fission products will be created. As a consequence, if we speak of local burnup, local burnup in the plutonium-rich

agglomerates will be very high for the same average burnup on the pellet. Maybe it's difficult to get an answer.

MR. KRESS: And not only that, your distance between fission products is very small --

MR. CLEMENT: Yes.

MR. KRESS: -- and we have a chance for them to interact with each other where they didn't have in the --

MR. CLEMENT: And a high concentration of fission products.

MR. KRESS: Yes, exactly.

MR. CLEMENT: There is a very high concentration of fission products in the plutonium-rich agglomerates. This will impact on where are the fission products because you know there are several different phases. So this will impact on where are fission products -- I mean whether they are dissolved in the matrix, whether they are in metallic precipitates, whether they are in the gray phases. I don't know if you've got the same phases with plutonium or whether they are in the grain boundaries.

So, as a summary, you could say impact on the repartition of fission products in different phases. So, generally speaking, different repartition

of fission products in the different phases.

MR. BOYACK: Did I capture it -- well, go ahead.

MR. CLEMENT: This is also influenced by, I will say, plutonium in thermochemistry. That's not exactly the same as uranium thermochemistry. So when, for instance, we have to calculate the repartition of fission products in the different phases and in the solvent test matrix and grain boundaries and gray phase, in metallic precipitates, we have to take into account all the thermodynamic equivalents, including specific plutonium thermodynamic properties.

MR. KRESS: Which depends on the local concentrations.

MR. CLEMENT: The local concentration.

MR. KRESS: Yes, I agree with you. That's the main difference; that is going to affect things.

MR. CLEMENT: So specific plutonium thermochemistry, I'd say.

MR. BOYACK: Is that MO or MAL?

MR. KRESS: MO.

MR. CLEMENT: I don't know.

MR. KRESS: One word.

MR. CLEMENT: It should be also the influence of the surrounding matrix to look at the

gases. If you look at the gases, you will very quickly have bubble indication, bubble correlations, bubble issues, and so on. Then when they come out the boundaries, you could have resolution of bubbles in the matrix also.

MR. BOYACK: If you want me to summarize that one, you're going to have to help me.

(Laughter.)

MR. CLEMENT: How could I do that?

MR. KRESS: The fact that you have a high concentration of fission products locally means you've got a lot of xenon and krypton there. It actually makes little bubbles easier than it would be if it was distributed. So you can make the bubbles easier, and they're local, and they're not stable or they might move. They can move down temperature gradients and thermal gradients, and as they move, they're encountering a different chemical environment, and they can go back into solution or not, or whatever happens to them. I don't know what happens to them.

MR. CLEMENT: All these differences in structures could impact on the fuel degradation processes. That, in turn, could impact on the fission product careers.

MR. BOYACK: So what is --

MR. CLEMENT: So fuel degradation processes might be different, could be different, and fuel degradation processes impact on fission trajectories.

MR. BOYACK: So is there anything that you understand currently about the difference between LEU and plutonium that I could put?

MR. CLEMENT: Well, make the comparison with differences between high-burnup fuel and moderate-burnup fuel. In high-burnup fuel it is much more easy to have -- for instance, you have liquid zircaloy, having access to fuel for dissolution into actions, and so on, and less impacts on fuel degradation and less, also, impacts on fission products release kinetics.

Here for MOX I don't know, but I'm just saying that, as soon as your structure is different, degradation process is linked to interactions with other materials, may be different.

MR. BOYACK: Okay.

MR. CLEMENT: And fission product release kinetics may be affected.

MR. BOYACK: And may affect which kind of kinetics?

MR. CLEMENT: May affect the fission

product release kinetics.

MR. BOYACK: Okay.

MR. POWERS: Let me ask a question and reflect my own ignorance maybe. Suppose we have a 16-micron particle of plutonium in a sea of uranium, and we have a fission event there. The recoil will push the fission product maybe about 4 microns? So it comes to rest not in the UO2, but in the uranial lattice by far and away most of the time.

With 16 microns, if you figure anything on the outer 4 microns, it can push at least half of its fission products out into the lattice. That turns out to be seven-eighths of plutonia can push half of its fission products out into the lattice.

MR. CLEMENT: Yes, I agree with you there. I've seen some calculations of the zero state for reactivity in accidents where they calculated a significant amount of gases that are uranial by recoil processes. That has to be taken into account.

MR. POWERS: And it means that the uranial lattice adjacent to the inclusion is positively bombarded by high-energy, high-mass particles. So it surely must be structured so it doesn't look anything like the lattice, the bulk lattice?

MR. KRESS: I think that's wrapped up in

one. I think you're right.

MR. BOYACK: What I've listed so far are microstructure, thermochemistry, fission product concentration, fuel degradation processes, and is there anything else you want to summarize out of these last points that Dana made in a way that I can get them down?

MR. KRESS: I think the effects of these things on the microstructure and restructuring the microstructure -- I'm not sure how to say it, but what Dana is saying is you're changing that microstructure in a different way by the fissioning process that you would in a regular LEU fuel.

MR. CLEMENT: Changes in the uranial lattice.

MR. KRESS: Yes.

MR. SCHAPEROW: It seems like a lot of the area wouldn't be affected, though, because it is a very concentrated effect. There are probably large -- it seems like there might be large -- not large swaths, but there are areas between the inclusions where you would have it completely intact, where you don't have any fissioning at all.

MR. KRESS: Something's got to give. You kind of have a structure there that's a little bit

rigid.

MR. POWERS: What I was wondering is, if, in fact, you push all or some significant fraction of the fission products, maybe it's half, some significant fraction into the host lattices, then in those lattices there is no fission intake in place to any great extent in that host lattice, but the fission products are starting to move their grain boundaries much like they would in any -- so you develop an interconnected microstructure from a little island of highly-disrupted areas. Does that mean anything other than you get the interconnected porosity leading to the gap quicker than you would in straightforward urania fuel?

MR. KRESS: I think the evidence is in the porous tests, which shows that you take a MOX fuel element and run it through the temperature of transient, and you get more and earlier release. So it's something about MOX fuel is reflecting this in basically more easily-released fission product.

MR. LEAVER: But, Tom, there was another VERCORS test of fuel, too, that had the same result.

MR. CLEMENT: That's right, but if you look at the data that we have for gas-releasing operation, this is also an indication that it tends to

be higher in MOX fuel than for low-enriched uranium fuel for the same burnup. So this is an indication that the repartition of fission products is not the same main operation. So it is not the same before starting of an accident.

MR. LEAVER: You're talking about RT 1 versus RT 2?

MR. CLEMENT: No, I'm talking about what people just do by puncturing the gases after degradation and measuring the amount of other gases.

MR. LEAVER: Right, right, the EDF data, yes.

MR. CLEMENT: They are different for a different burnup level. That means that the repartition of fission products, this is only for gases, but --

MR. LEAVER: Right.

MR. CLEMENT: -- but, generally speaking, it is different. This will affect the subsequent fission products released. That's why a big difference. Maybe it's not always pessimistic, but if it turns out that trapping in metallic agglomerates is more efficient, this would be less pessimistic. That's very complicated.

If you'll remember during our last

meeting, Dana has explained to us in his presentation several things about what happens in microstructures when you have vacancies for oxygen, on/off, and so on; explained the possible wall of barium buffer for oxygen potential, and so on, and if you change a repartition of the various things and the concentrations, and so on, you will change that. So we cannot just say that the fission products are the same. It will be affected anyway.

MR. KRESS: We've got experimental evidence that MOX fuel releases more and earlier. Certainly I don't want to argue experimental evidence. These are really believable reasons as to why the MOX fuel may behave different in a fission product release standpoint. All we're doing is explaining the data.

MR. LEAVER: Yes, we certainly have that evidence in the RT 1 versus RT 2, but then you didn't see it in the HT 1 versus RT 7. All I'm saying is that I just think we need to understand why you're seeing it in one test and not the other before we take this too far.

MR. KRESS: I don't understand why you say that. What are you looking at that tells you this?

MR. LEAVER: I'm looking at a slide that was presented by Bernard yesterday.

MR. KRESS: Which slide are you talking about?

MR. LEAVER: I'm looking at, it's the last slide, the third bullet.

MR. CLEMENT: Yes, that slide I mentioned yesterday the sentence is not correct. The contrary effect on RT 7 compared to RT 1, what is collected is release of volatile FP. Volatile FP in RT 7 is not earlier than in HT 1, as a conclusion.

MR. BOYACK: So if you take the second bullet and insert "not," "is not earlier" --

MR. CLEMENT: No, no, the third bullet. The third bullet.

MR. LEAVER: The second bullet is okay.

MR. BOYACK: Oh.

MR. LEAVER: What he's saying in the third bullet is it's not the opposite; it's the fact that you didn't observe this earlier release.

MR. KRESS: And that's where the "not" is.

MR. CLEMENT: Somewhere it should be the release of volatile FP in RT 7 is not earlier than in HT 1.

MR. KRESS: I don't think that tells me a lot. It tells that it depends on the temperature of transient you're going to. You could release in both

of them at about the same time, if you had the right temperature, and one of them may get released faster than the other. So you might get released more in a given timeframe. That's not implied in that statement.

MR. BOYACK: Let me ask Bernard, what I put up there for fission product release is your conclusion from the French test; that is, large amounts of fission products released earlier if it's plutonium?

MR. CLEMENT: Released earlier in RT 2 tests. A large amount of volatile fission products. You should have volatile fission products. Okay.

MR. KRESS: Fission product release tends to be a continuous thing. When you say, when does it start releasing, it's hard to say when it started. This is a continuous thing.

MR. CLEMENT: But at a given time in this transient, you find more --

MR. KRESS: One is higher than the other during a given time in the transient. I think that's a general statement.

MR. LEAVER: Certainly the RT 2 versus RT 1 suggests that, and there is the EDF data that was in the slides that Steve Kollie presented at the last

meeting on the fission gas being factor two or three greater in the gap.

I think Bernard has said that the French are still trying to explain what happened with RT 2 versus RT 1 and the fact that it apparently didn't happen in RT 7 versus HT 1, which is kind of interesting.

MR. KRESS: Look at this curve here, in the asterisked line, which compares RT 1 and RT 2, and look at the fission product cesium released for RT 2 and the one for RT 1. You can clearly look at that and say, oh, yes, the RT 2 started earlier and released more, but that's because down at this level of RT 1 you're probably releasing, but you're just not picking it up within the uncertainty of your ability to measure. It's releasing. So you can't really say one started earlier than the other. It's just if you compare the curves all alone there, one is higher than the other. And I think you can say that for RT 7, too.

MR. LEAVER: Well, there's not a curve for RT 7 and HT 1. All we have a statement.

MR. KRESS: I'm reading between the lines.

MR. LEAVER: Yes.

MR. KRESS: We had an RT 7 curve in the

previous handout last time that showed the iodine and the cesium release, and clearly for that time of transient you're getting more and earlier release than you would have --

MR. LEAVER: I was just going on the basis of the statement on this slide, which says that it's the same.

MR. KRESS: Yes.

MR. CLEMENT: Maybe it would be better to say higher release rates than "it starts earlier." So higher release rates.

MR. KRESS: Higher release rates would be a better way to say that. I think that's pretty clear and related, and these earlier things that Bernard talked about are relatively good explanations for why. It would be hard to convert those things into some sort of model, but it helps your thinking on why this might be.

MR. POWERS: Suppose that we accept the stipulation that the release rates of fission products are higher in the case of MOX.

MR. BOYACK: Volatile fission products?

MR. POWERS: Volatile fission products. But let's just concentrate on the volatiles, cesium and iodine, things that release completely,

essentially, during the core degradation process. So you can release it faster, but you can't release more than 100 percent.

MR. KRESS: Yes, that's exactly right.

MR. POWERS: But you want a source term that reflects that it's faster. Does that say that, instead of having the in-vessel release portion of the source term to be one period with a constant release rate, one ought to have two periods?

MR. KRESS: You're representing the transient as a matter of two. You might do all right with just shortening the overall transient time and still having the uranial --

MR. POWERS: As a first approximation, the input that you need to melt MOX is about the same as what you need to melt low-enrichment uranium. I can't imagine it's wildly different. The melting point may be a little bit different.

You go through this, so that that overall time period has to be about the same. I mean, I can't say how --

MR. KRESS: To the end of the accident when the stuff falls down.

MR. POWERS: Yes, penetrates the vessel or something like that.

MR. KRESS: That time period is about the same.

MR. POWERS: So you are kind of fixed on that.

MR. KRESS: Yes.

MR. POWERS: But if you want to have a higher rate, you're also kind of fixed on releasing 100 percent. You can't release more than 100 percent. So it looks to me like if we want to reflect higher rates of release, we have to do something about breaking up the interval.

MR. KRESS: Part of this is you may release 100 percent from 40 percent of the core, but not 100 percent from the rest of the core.

MR. SCHAPEROW: Are you suggesting that the fission product release may be the main thing that's affected by going to MOX and not fuel relocation nor head failure timing, and things like that? This is the one big effect that, if any effect is affected --

MR. POWERS: My order of approximation it would be that, that the entropy is roughly the same. That does not address the issues of reactivity events, but if I'm talking about just a conventional LOCA analysis or transient analysis, I mean the first

order, the amount of heat that I've got to get into things is about the same. The amount of clad I've got is about the same. The steam reaction, the boildown, is going to be about the same.

So, I mean, I can't even see wild differences in the core degradation process. There may be differences that develop when the clad interacts with the fuel because you've got little islands that are incapable of holding a lot of interstitial oxygen, but you've got a fission process that's generating interstitial oxygen, so they're pushing the oxygen out into the UO2 lattice. They haven't dropped down the melting points on you, and make a zirconium attack on the fuel a little bit more aggressive.

But in the heatup from the point at which the clad balloons and ruptures to the point you get to wild temperature escalation where the clad goes molten, it's kind of hard to say why.

MR. BOYACK: Any other similarities or dissimilarities you want to talk about? I was checking with Steve here before the meeting started. In his handout of last meeting, if you looked at the core layout, the interior 36 units right around the central core are all LEU, and then from there on you

have your 40 percent, whatever constitutes the totality of the 40 percent of MOX, then out just a little bit from the center periphery. So I just wanted to note that, since we talk about core progression and melt appears to be coming from the center outwards.

MR. NESBITT: And, Brent, let me throw out one other thing. I don't consider this a major factor, but I think it is something that ought to be remembered. In the time period before melt sequence, the decay heat from the MOX fuel is lower than the decay heat from the uranium fuel. It's not a big deal. It's just a 10 percent. I think we are talking about differences that are in general on the level of nuances.

MR. BOYACK: So your statement was a few percent less?

MR. NESBITT: Yes. I have presented a graph I think that gave a little more detail.

MR. KRESS: The decay heat matter may be more important for the MOX than it is the LEU because they're going to be about the same. If I look at some of the data, you think about how a core heats up and goes into its oxidation transient, then a lot of the release, the MOX fuel, is going to be during the decay

heatup time, whereas in the low-enriched LEU fuel you generally get very little. Unless it's very high burnup, you get very little release during that period. Release really comes about during the severe oxidation transient.

So there's a qualitative difference in the release timing because you're releasing earlier, and it's coming out during the decay heat whereas --

MR. LEAVER: Don't you think that oxidation is started, though? If you look at this curve, the temperature --

MR. KRESS: Which curve are you talking about?

MR. LEAVER: This one that shows the delay of RT 1 versus RT 2 --

MR. CLEMENT: Oxidation is not typical of a severe accident test, that kind of test. The oxidation takes place during the one-hour plateau at 1500 degrees.

MR. LEAVER: The temperature scale is cut off on mine.

MR. CLEMENT: This is oxidation takes place at low temperature during this one-hour plateau, and at the end you have got the tallying that this is fully oxidized. So this oxidation phase is not rather

typical of a real accident. Just recall people wanted to measure fission released during this heatup with an oxidized carrier. That's where that kind of trend is.

MR. KRESS: His ramp rates going up to that are typical of the decay heat ramp rate. You can see during that first ramp rate he's starting to release significant amounts of cesium and iodine with VI 2 during that first ramp-up rate. You wouldn't get that with the LEU fuel. It would wait and start releasing somewhere --

MR. LEAVER: Except you are getting it in RT 4, and that's --

MR. CLEMENT: No, but not before -- there's a difference in the fuels.

MR. LEAVER: Well, it had zirc oxide in it, though, right?

MR. CLEMENT: There's some uranial fragments that have oxidized as shards.

MR. KRESS: I don't think that that particular transient, an LEU fuel of a moderate burnup of about 30,000, you would start releasing that cesium until you got at least halfway into that flat part. Somewhere in there you'd start releasing it.

MR. CLEMENT: This releases is more --

MR. KRESS: Yes.

MR. CLEMENT: Whether it is high in MOX, it's much more --

MR. KRESS: So there was a qualitative difference in the release as it's related to the core heatup transient.

MR. LEAVER: Well, it looks like the mixed oxide, the volatile release will occur at lower temperatures.

MR. KRESS: Well, it will start faster at lower temperatures.

MR. LEAVER: Yes.

MR. CLEMENT: What we'll see on this test , RT 2 as compared to RT 1. So, as I mentioned before, RT 7 is different. At the time being you don't know why. What we have listed at the beginning is just articulation of all the different effects that may affect the fission product release, and they are different. This explains this difference and this explains also why RT 7 is different from RT 2.

MR. LEAVER: It would be nice to have that explanation of why you don't see this effect in RT 7.

MR. KRESS: I still think you see a faster release rate in RT 7 than you would in LEU fuel. I still think you see that. I'm mining my memory from the slide that we saw last time.

MR. LEAVER: I've got it here.

MR. KRESS: Oh, you've got the RT 7 slide from last time?

MR. LEAVER: Yes, it's in this package, if you want it.

MR. KRESS: I didn't bring it.

MR. LEAVER: I'm not sure which slide you're talking about, but this is package.

MR. KRESS: I didn't bring the package with me.

MR. LEAVER: I don't know about two time intervals because I think in a kind of a stylized release such as we're doing here, whether we want to try to get that complicated, but there is certainly some evidence, at least if you look at RT 2, that we could argue that the interval should be shorter than what's in 1465 now for the volatiles.

If you say by 1.3 hours, or whatever it is, for PWR, UO2 fuel may be half that or two-thirds of that for MOX fuel, but, of course, MOX fuel is only 40 percent of the core. So take that into account.

MR. BOYACK: Tom, do you want to speak into the mike in a sense that all the people can hear?

MR. KRESS: The slide I have doesn't have the scales on it. So we have to kind of think what

the scale might have been.

Given this temperature transient here and this release rate of cesium and iodine, I think if I had posed the same temperature transient on LEU fuel, the release would have actually had the same characteristics in the sense that it would come out later and lower.

MR. LEAVER: You're saying that maybe the temperature profile of the two tests would explain why they're the same?

MR. KRESS: Yes, because there's a big difference on release.

MR. LEAVER: Yes.

MR. KRESS: That's exactly right. It could explain a lot of it.

MR. LEAVER: What?

MR. KRESS: It could explain a lot of it.

MR. LEAVER: It could. We just don't have it. We don't have it.

MR. KRESS: Yes. We have the profile; we just don't have the scale for it. I can probably guess what the scale is, but I'd be guessing.

MR. POWERS: Suppose we had all kinds of data, every bit of data you would want to have from MOX that was generated using reactor grade plutonium.

Would that help you on understanding what was grade plutonium?

MR. KRESS: Yes, I love data. If I had reactor grade plutonium and the kind of data that Clement is producing with his temperature transients and release rates, if I had it for at least two elements separated far enough apart in their release rates, I could make a correlation for the MOX that I could translate into a whole core.

MR. POWERS: What I'm asking you is, suppose I got you this data, everything you asked, but I got it for reactor grade plutonium. Now I ask you to calculate the behavior of fuel made with weapons grade plutonium.

MR. KRESS: Oh, reactor grade meaning end-of-cycle EO2 matrix plutonium?

MR. POWERS: Fuel plutonia with an initial substantial amount of 240 isotope versus only about 6 percent of --

MR. LEAVER: Oh, okay. So you're saying MOX fuel but reactor grade, okay.

MR. KRESS: I don't know, I don't think I have a way to translate that into the weapons grade. I think it would be different.

MR. POWERS: So you're saying you didn't

really do it on MOX made with reactor grade plutonium?

MR. KRESS: It's better than nothing, but I would rather have particular needs, which I presume is weapons grade. It's better than not having any. If I didn't have any, I would take it and presume it was representative, but I wouldn't be able to stand up in court and back that up.

MR. NESBITT: Could I add I think a piece of information that might bear on that? The plan for production of weapons grade MOX fuels to adjust the master mix of the blend such that the amount of fissible plutonium in the plutonium-rich particles is equivalent to the reactor grade MOX that is used in France and that the data which you are seeing is from, I know that doesn't address everything that you could possibly raise, but the intent there is to make the power profile within the microstructure be as similar as possible to the reactor grade MOX experience.

MR. SCHAPEROW: One of the outcomes of the meeting is hopefully to identify not just the need for more experiments, but how much they're needed; confirmatory or pre- are essential before we're able to say, hey, let's go ahead and license. It's a question of the degree of need. That's it. We would like to assess that.

MR. KRESS: It would be nice to take one of their pieces of fuel using their MOX and replicate one of these tests to see that there's no difference in behavior.

MR. BOYACK: This seems to me to be one area where I think I would like to give an action item to the panel members, and that is on the research needs, where you can sit down and focus individually for a time on research needs. You would focus on data that you could then process into code. We could send that by way of a letter, because I think that would be really a little more structured than what we do here, and I think it would be very helpful.

MR. KRESS: I think significant data already exists in this program that the French have, and if we could somehow purchase it, it would go a long way. I think a replicate experiment using the real fuel they're going to use with one of these tests to show that there's not much difference would give you a lot of confidence in at least that these can be extracted to their tradition.

I think probably looking at the matrix of tests they have, they've probably got enough data that I can do a lot with, if we could somehow purchase it from them.

MR. LEAVER: Tom, let me follow up on that point. Bernard, though you obviously can't share data that is proprietary to some agency in France, in your opinion does a substantial amount of quantitative information exist on some MOX tests in France?

MR. CLEMENT: For MOX behavior in severe accidents that you call meltdown, I think that most of the information is in France. Other people did not perform experiments.

The other source of data is what happens in reactor accidents where you have an international corporation, for instance, where people look at also what happens to MOX, what is the MOX structure, what will be the impacts, and so on.

MR. KRESS: We are focusing just on LOCAs now, and there is need for reactor and source, too.

MR. CLEMENT: Yes, that's right.

MR. LEAVER: Jason, I'm surprised that the NRC hasn't been poking around at least if the French have a lot of data. Hasn't somebody from here been at least involved in that or --

MR. SCHAPEROW: I'm sure Lee has. He's not here right now, so I really can't speak for him. He's been our contact and working with the French.

MR. LEAVER: That's got to be the most

cost-effective way to get some data compared to running our own tests.

MR. SCHAPEROW: Well, actually Tom did point out one advantage of doing your own test, which is we can use weapons grade levels.

MR. LEAVER: Yes, but even there, I mean, maybe the purpose of such tests would be to confirm what the French have done. We haven't really used the French data.

MR. CLEMENT: Data you will get for the studies performed for reactivity-initiated accidents, that you an use for severe accidents, mainly concern the initial repartition of gases in the fuel, where they are looking at or they focus on gases, but not on fission products, and they are looking at, where are the gases before the transient? This can be a bit of usable source. What happens during the transient is not apt to give an initial repartition and study the initial repartition of gases. You can use that in the zero state form. So your accident rises, but only for gases, not the detail of all the fission products.

MR. BOYACK: Okay, so what I wrote, using Tom Kress's name, of course, liberally, is that you believe that a good deal of data exists from French tests, and if acquired by the NRC, could be used for

correlation and development, and could be used for appropriate code tools.

MR. KRESS: That's exactly my position.

MR. LYMAN: Can I just raise something I raised at the last meeting, I think, for the record? I don't think it's clear that the specs for the microstructure ECSs as selected in the existing Fuel Qualification Plan in a sense -- you know, it's a process that's used in France. We need to clarify --

MR. KRESS: Yes, that's why I wanted this, using actually their spec --

MR. POWERS: They told us that the review of the MOX fuel fabrication facility, they said that the spec was different.

MR. NESBITT: With respect to the plutonium-rich particle size and distribution and things like that, is it the same?

MR. POWERS: They claim -- all I know is that --

MR. NESBITT: I can't speak to that meeting because I wasn't there.

MR. POWERS: Somehow somebody's going to tell the truth on this or I'm going to get really irritated.

MR. LYMAN: Here is the spec. The spec is

the means concerning which particle distribution shall be less than 13 microns. I think, from what I've seen, it's smaller --

MR. NESBITT: That's a different issue. That's the difference between actual manufactured parameters versus the spec. Of course, you don't manufacture the fuel exactly on the spec. The spec specifies an upper limit for these parameters. You can't manufacture anything that way. It's just not physically possible from an engineering perspective to do what you're talking about.

MR. BOYACK: I am going to interrupt for a moment here. What I need to understand from the panel, whether this is germane to what we are trying to do. I understand it is an issue, but I'm not sure it is an issue for the panel as we deal with trying to --

MR. KRESS: It is an issue if you want to take the French data and say it's applicable to theirs.

MR. BOYACK: Okay. Why don't you continue then? That's fine. I just wanted to make sure that it was applicable to what we were doing.

MR. LYMAN: Let me just finish. Here's a spec that says that 95 percent of the plutonium

required will also have an effective diameter of less than 100 microns. That's where the fuel qualification comes in.

But, according to a report from the IPSN, 2 percent of the clusters for the U.S. fuel may have a mean size higher than 100 microns --

MR. BOYACK: You're going to have to keep your voice up because I can't understand you. You said what?

MR. LYMAN: That 2 percent of the clusters have a mean size higher than 100 microns for just France, while the DCS factor would be 5 percent, at least no more than 5 percent the number greater than microns. So that's one difference in the spec.

MR. NESBITT: I have no idea what document you are referring to, but the spec's the same.

MR. LEAVER: Yes, it sounds like the IPSN you're referring to was some observation about the as-manufactured fuel. That's not a spec; that's for sure.

MR. BOYACK: Okay, I would like to continue on now. All right, let's return -- any more difference --

MR. LYMAN: I'm sorry, let me just clarify. The IPSN, this is a record by Shumanz. It

said a maximum of 2 percent of the clusters may have a mean size higher than 100 microns, according to the fabrication specifications.

MR. NESBITT: I have no idea what the IPSN document you're talking about is or anything. I can assure you that, as far as particle size is concerned, we're using the same specification as the current one using --

MR. LYMAN: Well, can you get confirmation of that?

MR. NESBITT: We have.

MR. LYMAN: Then why is there a difference?

MR. BOYACK: Okay, so are there any other characteristics?

(No response.)

Let me tell you, of course, what we'll do is actually finish this statement and we'll take a break. Then we'll come back and start filling in the source term tables.

So is there anything else, any other characteristics that you wanted to have in mind regarding differences between LEU's behavior and MOX behavior that would potentially affect the source code?

MR. POWERS: It seems to me I guess there are two things that weigh heavily in my mind. Every attempt that we have undertaken to predict fission product release adopting a first-principles approach has floundered. And it flounders on the challenge that you do one experiment, you get one result; you do another experiment, you get a different result. In order to do that, people bring up more parameters than Carter's got pills, and new phenomenon to explain each of these things.

Then in the face of that difficulty, we have gone to a far more empirical approach, which is take some data of variable quality, put it on a plot, dream up some straight line you can run through it, and run through and explain why things deviate from the straight line, sometimes by a lot. Convince yourself that the straight line is the one you want. Derive some parameters from it and say that's what it is.

Now the process is probably not that horrible when you look at fission products that are released early in the transient. That would be the cesium and iodine. I don't think we run into any problems on that.

The process probably has some real

questions if you go to the more refractory materials. As I have repeatedly said, I simply don't understand tellurium.

But, despite that history of being unable to use first principles to predict fission product release, I keep coming back to this. It seems to me I need to understand better how the oxygen potential varies as I come from the bulk urania approaching one of these inclusions, and whether I get to the point that, because plutonia has less ability to sustain an excess of oxygen, I am saturating out my ability to buffer that oxygen potential with moly.

It becomes a concern because, when you look at what are the inventory differences likely to be, they are all fairly small except for the one element that we really don't understand very well, and that's the ruthenium behavior.

MR. KRESS: I don't know how you measure this, but it looks to me like it would be minimal to fill out the calculations, this part of it, if you have a good solid state chemistry code.

MR. POWERS: Well, it seems to me that, whenever you try to do solid state calculations, I've always ended up one parameter short. It doesn't matter how much physics I put in. I always end up one

parameter short.

We've had some luck doing it when we've made mass spectropic measurements of fission product release. That certainly helped. The problem we've always run into in doing that is that what we observe by putting fuel in a high vacuum, heating it up, any of the fission products coming off seems to bear no resemblance whatsoever to what we get when we put fuel into a reactor and heat it up and measure what's coming off of it.

It's very frustrating. I don't want to overemphasize getting first principles, stated in first principles approach, because I've never seen it work yet. We keep edging toward more and more closely to first principles, but we still have a model that basically relies on root diffusion. We just don't have the diffusion proficients to put into it.

MR. KRESS: That's exactly right. We can extract some out of this data. That's what I would do with the data. I would extract the diffusion proficients out of it or as many of the elements as we have transients for.

MR. BOYACK: Anything else?

(No response.)

Let's take 15 minutes. Let's come back at

20 after 10:00, and at that time we will go ahead and start on the source term tables. We'll begin with duration.

(Whereupon, the foregoing matter went off the record at 10:05 a.m. and went back on the record at 10:28 a.m.)

MR. BOYACK: I have in my hand four copies of the updated MOX Fuel Qualification Plan dated April 16, 2001. I would like the panel members to have access to this during our discussions.

What I would like you to do is forgo lunch and read this during the lunch hour. I don't need to read it, so I will be going to lunch.

Tomorrow we'll talk about action items, but let me go ahead and formalize one action item that I mentioned a few moments ago. That is, we would like the panel members to send a letter to Moshen Khatib-Rahbar with your input regarding not data needs. I guess we call it data needs. Research needs, yes, is a better word. This is specific to MOX.

The current thoughts are that what we would do then is that we would create an appendix in the report and we would include those letters. So be aware that they would have public distribution and be in the document.

MR. LEAVER: Is this a letter from all -- signed, one single letter, coordinated?

MR. BOYACK: No, individual.

MR. LEAVER: Individual.

MR. BOYACK: I've handed out this table. We found out that neither of us knows how to get the NRC machines to print landscape, no matter how many times we tried. So the first column is cut off a little bit, but I think you can go ahead and figure out what it was we were covering or trying to cover there.

That, of course, is a table that just gives you a little bit of the characteristics. Really the terms are all showing up in the MOX behavior, and the contrast comes from the statement.

So now what we want to do is see what we can do with these tables. If the pattern follows yesterday, I did update the tables so we have the named individuals here, and of course I'll attribute whatever received to the wrong individual after I get past the first person, the first entry. But you guys kept me straight yesterday; I'm hoping that you'll keep me straight today.

If we come up with needs in a particular area, then we can do that. Otherwise, the letters

will be the primary vehicle for doing that.

Now when I presumed this -- as soon as I say the first person, Dave Leaver, what do you think about the duration for the various phases, gap release, early vessel, ex-vessel, late vessel, then we'll maybe want to have a few more pieces of discussion. But if not, we'll continue on.

I have listed up at the top here the NUREG 1465 times. That's where the table came from. We may have to struggle through this first one, but let's go ahead and give it a try and see what you have to say.

MR. LEAVER: Well, I guess one general thing I have to say is I'm struggling with how to characterize not only my own estimates, whatever they may turn out to be here, but for the whole group how to characterize these estimates in the sense that, if we are not careful about how we communicate this and how we present it, how we characterize it, that one could pick up a document that's produced at some future point and misinterpret what it's saying. Because I really don't believe that we have a basis for estimating these numbers.

Having said that, we can certainly make estimates, and in the process of making these estimates we can discuss what we know and what we

don't know in a manner similar to what we've done this morning, and that's a very valuable process. But I think that needs to be captured, the fact that maybe more important than the numbers themselves are the caveats and the statements of what we know and what we don't know.

So, having said that, I have no problem with talking about numbers or directions that numbers could go, but I would maybe, Brent, urge you as the facilitator to make sure that in the end that this is -- I think, for example, these estimates are different in terms of our certainty, or lack thereof, compared to what we did for burnup. Now we struggled with burnup, but here we really don't have a lot of data.

So you just need to be careful you don't have a table with a bunch of numbers and somebody picks this up and says, "Oh, here's the answer." Because I don't believe that in the end that that's what we're going to do here.

MR. BOYACK: Okay, let me ask a few more questions about how to handle this. Let me tell you what I intended to do. Then it may be that with your input then I do something more or change direction.

My thought is this: that I would go through the discussion portions of each of the

transcripts, and as I read those, I'm going to flag those, cut them out and paste them into a separate document. That will become the raw material for me to create the text for the chapter that contains these tables in their fullness. So, as you express reservations, I will capture reservations, and I will make those very clear in the text. Now you'll have a chance to review that.

What we would also have to do is in the concluding chapter, which Moshen is putting together, then we would have to pull out a few of these key points and punch them home in the concluding section, too, of the report. So that was how I envisioned handling this process-wise.

After the draft had been prepared, you would, of course, have the chance to review and say there's some things you didn't capture here that I want captured, and then we would, of course, capture this on the review stage.

There were a couple of comments. Moshen and Jason?

MR. KHATIB-RAHBAR: Yes, one important thing I think for the record is the importance of the ECCONO's report, not my report or Glenn's report. So, therefore, you folks have to stand behind it.

MR. SCHAPEROW: Yes, it's going to receive, hopefully -- we're going to send it out for your close scrutiny after we get it put together.

MR. BOYACK: My guess, that will be two assignments. It has to be because you have to see what comments other people put in.

MR. SCHAPEROW: So we'll make sure everybody's views are correctly represented in the report. But the particular issue at the table, I think Dave's got a good point on the people not taking this thing and running too far with the table itself.

I would like to suggest that one improvement, there would be a big footnote or a big thing either at the end of the title or right at the very bottom of the footnote saying" This table, the numbers are uncertain because of `X', because of the lack of data. So somebody doesn't walk away with the table and try to start implementing it all over the place.

MR. BOYACK: We can certainly do that. So did I understand you? What we did is reach -- we'll should put a footnote at the bottom of the table that says, "This table is no darn good."?

MR. SCHAPEROW: I would say it's based on what, the presentations on lost data from IPSN which

lacks a Y axis or something. I don't know. I mean, whatever the --

MR. BOYACK: Dave has given a very clear statement of it, and everybody here understands it. That is, we had this discussion on the way over to the other building. That is that, when you're dealing with opinion, informed, expert opinion, the quality of that opinion is highest when it's informed by a good, solid database, which the members understand relatively the same way.

What we're lacking here is that database. It's only a partial one. It's a sparser set of data than exists for the LEU. So in a relative sense it's less, and people know just less about MOX in a subject that's already difficult. Is that a fair statement?

MR. SCHAPEROW: That's true.

MR. BOYACK: Yes. So that's what I heard him say, and what I think the thing we have to do is capture that in the text.

Now the next statement is this question of somehow making sure that it's captured in the table. I made a pretty blunt statement, but I did that for a point. I wanted to force the issue of, how do you -- you can qualify it in the text. You can put a page-long qualifying statement in the text in the table.

How do you handle that? I mean at some point you basically say it's no darn good.

MR. SCHAPEROW: You don't have to go that far, but another statement a little less strong may serve the purpose. It's for consideration if somebody can put some words --

MR. BOYACK: We do that in the text. Now I just don't know quite what to do on the note with the table, but maybe somebody will be able to suggest some wording for me.

MR. LAVIE: "Data preliminary; requires confirmation."

MR. LEAVER: Something to that effect. We may be able to come up with a way to say that that we're comfortable with after we talk through this.

Let me ask a slightly different question before we get into numbers. I assume there's some -- this is, I guess, partly addressed to Steve and maybe to Jason, or whoever -- I assume there is some schedule driver here where the licensee, Duke, DNS, whatever the acronym is, DCS --

MR. NESBITT: It will be us for the use of MOX. We will be the licensee.

MR. LEAVER: Okay, so it will be a license amendment, and part of that license amendment is going

to have to be a calculation of a radiological design basis axle, for which you're going to need release fractions. If things go reasonably well -- obviously, you have to use your crystal ball here with all these other issues that we're talking about that could affect the schedule on this -- when would you like, when do you need to know this? When does the NRC need to know? When do you need to know?

MR. NESBITT: Our schedule is to submit a license amendment request for use of large quantities of mixed oxide fuel at the end of 2003. To clarify that, the request would go to the NRC at the end of 2003. The fuel use would not begin until 2008 at the earliest.

MR. LEAVER: So if you're going to submit a license amendment at the end of 2003, you would need to be doing the calculation, say --

MR. NESBITT: In 2003.

MR. LEAVER: -- in early 2003, something like that, 2003?

MR. NESBITT: Uh-hum.

MR. LEAVER: Okay. So I guess it's fair to say we have of the order of 12 to 18 months where presumably there could be some work done to try to supplement this sparse database and come up with

something that people feel is acceptable for this calculation.

Probably what we do here, I mean if we do come up with a table, that table has numbers in it, it would be characterized as, in some sense, as preliminary or provisional or to be confirmed, and probably it would be necessarily then conservative because generally, if you don't have data, you want to try to err on the conservative side. I think that's what the French have done, is they tended to choose enveloping sequences and round numbers up, such that they feel that what they have is adequate because they do have plants operating with mixed oxide fuel, and obviously somebody over there must feel that their licensing basis is acceptable.

So, all right, having said all that --

MR. BOYACK: Just to help to wordsmith a statement here, which I've now put up a strawman: Panel member inputs are based upon partial and preliminary data regarding MOX characteristics and behavior available to the panel at the time the source term input was prepared.

MR. LEAVER: That's close enough. Yes, that's close enough.

MR. BOYACK: All right.

MR. LEAVER: So I think on the gap release it seems to me that the release is going to occur sooner than LEU.

MR. POWERS: When you say the release is occurring sooner, do you mean that the clad breaks more easily?

MR. GIESEKE: Yes, that's what that means, I think.

MR. LEAVER: I think it means the clad breaks sooner and that there is more fission gas either in the gap or near the edge of the pellet that can be released quicker than LEU.

MR. POWERS: And then, typically, the M5 clad is less extensively oxidized in the normal operation.

MR. GIESEKE: Someone made the comment that the M5 clad was more, a little ductile and might --

MR. KRESS: The internal pressure is dominated by the fuel gas and not the fission gas. So you don't have much pressure difference. The M5's likely to be more ductile.

MR. LEAVER: Are you thinking that the clad may not rupture as quickly?

MR. POWERS: What about its melting

temperature?

MR. GIESEKE: About the same.

MR. LEAVER: Why wouldn't the gas pressure be greater in this, in mixed oxide?

MR. POWERS: It doesn't matter, but they charge the rod with 100 -- they put 100 atmospheres of helium in there, so you can release an awful lot of fission gas and not change that very much.

MR. LYMAN: There are still unanswered questions about the embrittlement of --

MR. POWERS: What I know is that there has been a claim by a German investigator looking at a Russian-Niobium cladding, that it embrittles at 7 percent oxygen instead of 17 percent oxygen. People manufacturing the M5 say, well, it may well be for the clad he's looking at, but it isn't so for our clad.

I know that the NRC has got M5 on its list of things to do, but I don't know what they've done.

MR. NESBITT: If I can interject, the NRC has approved M5 clad in several plants. We have it operating.

MR. POWERS: Yes, but all that approval has been based on a regulatory decision that needs confirmation. That means, in other words, that they can come back and say, "Whoops!"

MR. NESBITT: You can say that about anything.

MR. POWERS: Yes, yes.

MR. NESBITT: But I don't think there's a probable issue out there. Well, I'll let NRR, if they want to chime in --

MR. POWERS: So far as I know, the biggest part of the differences in results comes from a different measurement technique than the rule specifies for measurement technique. So they really can't --

MR. BOYACK: So what I hear is competing effects. More fission gas, successful release relates to potential for cladding that is less essential to failure.

MR. LEAVER: Let me ask on the cladding, is the use of M5, which we've I guess discussed yesterday, people tend to think is maybe a little more ductile than earlier generations of clad, is that a licensing basis requirement or will it be, Steve?

MR. KRESS: It can be. Right now they use the percent oxidation as a substitute for that.

MR. POWERS: The regulation is written for Zirlo. So if you're going to use M5, you've got to come in with an exemption request. So we'll come in,

and people will expressly look at it. We will presumably get around to changing that regulation one of these days, so you don't have to do that, but you have to come in and say, "I'm going to use M5."

MR. LEAVER: Do you mean the codified regulation specifies Zirlo --

MR. NESBITT: Zirlo is a trade name of Westinghouse. We will apply at the same time, if the regulations haven't been changed by that time, at the same time that we submit a license amendment request for using zircaloy, we will submit an exemption request similar to what's been submitted and granted for other plants for use of the M5 clad.

MR. KRESS: The figure of merit for the regulations that has to do with the productivity is the percent of oxidation. That's why it's in there. That percentage is strictly applicable only to Zirlo.

MR. LEAVER: And that's this 17 percent?

MR. KRESS: Yes, that's why that number is there.

MR. LEAVER: So your license amendment actually, presumably, then, you had to get a license amendment to use M5 and you said it's in a --

MR. NESBITT: No, we had an exemption. We submitted an exemption request that was approved by

the NRC.

MR. LEAVER: And so you, as part of the license amendment or a previous exemption, will do the same for --

MR. NESBITT: Yes, it's pretty standard now in the industry to submit and get these things approved.

MR. LEAVER: On the gap release, if we have this rather large LOCA, when does the first rod pop?

MR. KRESS: Normally it's when the hot rod gets up to 1200.

MR. LEAVER: Right, and that's pretty damned fast with no ECCS. ECCS doesn't come up.

MR. KRESS: It starts from the original temperature of 600 --

MR. LEAVER: Right.

MR. KRESS: -- and .1 degree per second, I think is the decay heat, and see how long it takes you to get up to 1200. I didn't do the calculation, but you can do it. Six hundred degrees at .1 degree per second is 6,000 seconds, and 3600 seconds to an hour. Wow. There's something wrong with that. Well, I don't think you start from 600. You start at the maximum plant temperature; 600 is the coolant

temperature. So you've got to adjust that. I don't know what the starting temperature is.

MR. LEAVER: Yes, I've seen this calculation for a BWR but I haven't seen it for a PWR. It's like maybe 20 minutes or something.

MR. KRESS: Actually, .1 is probably wrong. It's more like one degree per second. It's probably more like one degree per second anyway. So I was off by a factor of ten.

MR. LEAVER: Or maybe a half degree per second.

MR. KRESS: Yes.

MR. SCHAPEROW: I guess I would like to kind of remind the panel what I recall NUREG 1465 as being the determinant of the gap release timing. We did a guillotine grade calculation. We did several of them, and we came up with timings of about 10 to 30 seconds to the time of first fuel rod rupture, but then the end of this gap release here, that half an hour, is meant to represent the time at which large quantities of fission products start coming out of the fuel, and that was not based on a large break LOCA. That was based on like a 2-inch LOCA or a station blackout.

MR. GIESEKE: You got up and drew pictures

with your pen.

MR. CLEMENT: Generally, you're right. I remember our discussions for this duration for high-burnup fuel. We have shortened it just in order that the next phase, late in-vessel release starts earlier. This was the rationale for shortening it. It was not because of gap release by itself. It was so that the early vessel release starts earlier. This is what you have done for high-burnup fuel.

MR. LEAVER: That's not what this table says though.

MR. CLEMENT: The end-point of the gap release phase is defined as a raise of significant fraction products and this process is accelerated with high-burnup fuel. This is what is written in the table.

MR. LEAVER: But the next sentence says, "The shortened time reflects the quality of understanding the fuel has restructured, putting more gas near the periphery and accelerating the release kinetics of volatile fission products."

MR. CLEMENT: But that is the following phase; early in-vessel release will be accelerated. So that's why you have shortened the duration of the gap release phase.

MR. SCHAPEROW: The acceleration refers to the earlier in-vessel phase, which is starting a lot earlier. Significant quantities of fission products come out earlier. That's what that's saying.

MR. GIESEKE: Then they're saying, with the higher burnup, this is moved up here, which has the effect of shortening this.

MR. KRESS: I think what we are doing is taking the little bitty part, the fission product release, versus time. We've got a little bump, and then you go out, and then it starts coming in. We're taking that little bump and making a straight line out of it, and we're intersecting with this other and trying to get the same quantity in there over a timeframe that I don't know what the timeframe actually means, but because of what we're doing.

Now we're saying, instead of the little bump and the little gap and something, you get a little bump that intersects this gap. We're now trying to figure out how to make that look like a straight line.

MR. GIESEKE: We're moving this one back and forth, moving this intersection point to shorten it mainly.

MR. KRESS: And I think Bernard is right,

that's how we did do some of that.

MR. BOYACK: I'm going to relieve Dave in making a number, a figure, just for a minute. I'm going to ask to guess. I can see on this one we need the discussion. So, Jim, your comments on the gap release arrangement, and then I'm going to come back and ask for a number.

MR. GIESEKE: I think we've talked molten. They had to do it, the difference in the cladding, the fact that the gap release time is not defined or is defined by the rate of release during the early in-vessel, which moves it back. We're talking about the intersections of the curves. What other issues are there?

MR. BOYACK: What was that gap release time?

MR. GIESEKE: If you read the definition of gap release, what we say is postulate that it's two curves. Here's one curve. Here's your gap release as you fail the rod. Then the ones that have failed take off like this. So we're saying here's the gap release time.

What we're saying in this case is that the early in-vessel release rate is increased, so we're going to put this line up higher. That means that

this time is shorter from here to here. I think that's what the French were talking about. It's basically reducing the gap release time by raising the rate in the early in-vessel. Does that make any sense?

MR. BOYACK: Yes, I think when you think about it. I think also you have a higher fraction of fission products either in the gap or at the edge of the pellet in the MOX field.

MR. LEAVER: That's the next question, yes, when we get to the percentage of it.

MR. BOYACK: Oh, okay, you're saying that's a refraction issue not a duration issue.

MR. LEAVER: Yes, not a duration issue.

MR. BOYACK: Okay, I hear you.

MR. LEAVER: Right.

MR. BOYACK: Any other comments? Jim? Yours were on the definition of gap. Anymore? You don't have to come up with the number yet.

MR. GIESEKE: I have a question to put out here. I presume -- what did we say, we have 40 percent? What's our ratio of the --

MR. BOYACK: Forty percent of oxygen.

MR. GIESEKE: That's primarily - well, now we're tying it into where you put these in the core,

but that's not in the center region.

MR. BOYACK: There's 36 fuel centers in the center that are LEU --

MR. GIESEKE: Yes.

MR. BOYACK: -- and then it's distributed.

MR. GIESEKE: Yes, and it's outside of that. So the onset of gap release would come at the same time probably because that would occur in the middle, and there's no MOX fuel there anyway.

MR. LEAVER: It's a start, but we're after the duration period.

MR. GIESEKE: We're after the duration, and that's defined by the early in-vessel, I think.

MR. LEAVER: Well, the early in-vessel is certainly going to come out fast, I mean if you believe the RT 7.

MR. GIESEKE: Yes.

MR. LEAVER: Or RT 2. RT 2.

MR. GIESEKE: Yes.

MR. LEAVER: Not RT 7.

MR. BOYACK: Anything further?

MR. LEAVER: That's enough. Go on.

MR. BOYACK: I am just going to move this along here.

MR. POWERS: I see no reason for any

dramatic change in the start of gap release. That's usually not part of the source term. They usually specify that externally, but it's specified in 1465.

Now the issue of whether you shorten it down or not is one of whether you think that the fission product release is more rapid, starts at a lower temperature and is more rapid in the case of MOX fuel or not. I think we have a test that suggests it is and a test that suggests it isn't, but prudency would say, yes, let's shorten down the duration of gap release a little bit, just to reflect that it's possible that we have a shorter duration there and we get into fission product release more rapidly, and ask people to go confirm that. But that's one of the areas you need experimental data.

Then the next question is, well, how much more to shorten it down? Of course, I haven't got a clue how to do that, but we didn't have a clue on that for high-burnup fuel either. So we suggested let's do a little bit. It can't be too much. So we suggested dropping it down to .4 hours and let it go at that. So it doesn't sound to me like that's a bad prescription for the process at all.

MR. BOYACK: Sounds reasonable.

MR. KRESS: You've basically said we

didn't have much more data for the LEU, for shortening the LEU period? Is that what I heard you say?

MR. POWERS: Well, what we had in the case of the LEU was a physical understanding and substantial data that says, yes, the release starts earlier here, and we needed to shorten it down. Here we have equivocal information, debatable information, to suggest it all starts earlier and be faster. But just to be prudent fellows, maybe we ought to reflect that, shorten it down a little bit.

I mean what you're shortening down is you're saying that nothing has changed really about the gap release. It's just at the point where you're started getting bigger releases due to the fission out of the fuel starts earlier; that's all. Since that marks the end of the gap release, that means ipso facto the gap release is shortened.

Now you're going to be very careful. You ought to reduce what you call the gap into the tori by the amount of shortening, which is a 20 percent shortening. Somehow that strikes me within the uncertainty range that we have here. In fact, I will later argue that I think the existing gap inventories that we've got for MOX are conservative for MOX. I mean there's enough margin there that there's no point

in changing that.

Even if it does feed the gap more, and I think the evidence from Halden is that, indeed, MOX fuel during normal operations is feeding the gap more; there's a little higher inventory in it, but it's still within that 5 percent range. So I don't see any reason to get too excited about changing that gap release fraction.

MR. BOYACK: Tom?

MR. KRESS: I give the .5 as the time when the first clad, when the first failed fuel fails. That may be a wrong view, but I see no reason, just because it's MOX in there, to change the .5 at all. I would start -- that's the duration. The gap release starts at zero, zero time, because we're just looking at the duration of that.

What happens is you fail with the first fuel, and then you fail with the next one, and probably moving radially outward failing more and more clad as you go along, until you fail most of them, and then it starts dropping off, and you get this bump. The duration of that bump is probably equivalent to -- I don't know how fast that spreads across the core. So I think that's why we went to some artificial plant, and that was, how long does it take to start

getting significant release from the in-vessel plants? Because I don't think we have this core melt progression data to tell you the real number.

My question is now, when do I start getting significant release from the in-vessel phase? In the first place, I don't know what the word "significant" means. I don't exactly know when it starts because I have a feeling that the middle of the core where the LEU is is heating up first compared to where the MOX is. So the initial release you're getting from the early in-vessel is probably coming from the ordinary LEU fuel that goes through the same kind of heatup rate and transient that it had in the regular core.

So I think that's when you're first going to start getting the significant, unless the MOX, which is just heating up slower and is faster release, wins the race compared to this. And I don't have any idea without having good models and heatup rates as distributed across the core, given that's the function of the power distribution across the core. My guess is I don't have enough information to change either one, .5 or the 1.3.

MR. BOYACK: As we began this discussion, I should have reminded us of what the definitions are,

and Steve Nesbitt has reminded me that I ought to remind you of that. But let's just take a look at what the definition is, because it ought to be the same definition. That I think is true. So this is out of NUREG 1465.

We know when the gap activity phase begins, and that's when the fuel cladding failure begins. There's no discussion about whether that's later because of the M5 cladding, but that's not a matter because we're going with the duration here.

This phase involves the release of radioactivity that has been collected in the gap between the fuel pellet and the cladding. The process releases to the containment of 2 percent of the total inventory of the more volatile nuclides, particularly noble gases, iodine, and cesium.

Now the gap activity phase ends when -- so you've got failed fuel, and now it ends, the fuel failed cladding, when the fuel pellet bulk temperature has been raised sufficiently that significant amounts of fission products can no longer be retained in the fuel. So I guess I read that as now you've got the temperature where you start to migrate and move the fission gases out into the coolant through the ruptured clad. When we finally get to that point,

that's what you were pointing out, that occurs.

So I guess the basic question is, is there something different, sufficiently different, about a MOX core that you begin to get significant amounts of fuel -- pardon me -- fission products coming out of the fuel. We talked about the fact that at the center of the core you've got LEU. So I don't know how far out that progresses.

MR. KRESS: It's a race. It depends on which one wins the race. The stuff in the middle is going to come out at sort of the same timing that the molten core did. The stuff with the MOX is going to come out according to its temperature transient and the earlier release. So it's a race because you've got cosine power distribution and they're heating up at different rates. So you have a different thermal transient for the different parts of the core.

My guess is, looking at some sort of data like this, my guess is that the MOX at its cosine power distribution probably wins that race. So you start getting significant release earlier. But this is a speculation on my part, and I don't have enough information on all these things to change these numbers.

MR. CLEMENT: I have a question. Should

we concentrate on the exact loading that will be made in the future years within this core with MOX fuel or should what we put in our tables reflect the differentials between MOX fuel and low-enriched uranium fuel, generally speaking? That's a question.

MR. GIESEKE: Very good question.

MR. KRESS: What if the next application doesn't load it the same way? It could be loaded in the center.

MR. CLEMENT: I could imagine that in several years one could change the core refinements.

MR. KRESS: Given that comment, I would change 1.3 -- I mean the .5 and make it shorter. I don't know how much shorter to make it. Just like Dana, I have no idea, but .4 may be a reasonable guess.

MR. SCHAPEROW: I think we should try to limit the scope of this. I don't propose that we prepare a MOX table for our core with all MOX in it. Even this particular application is years away. I don't know that we need to do that.

I think we're going to capture the logic and the ideas, even considering the MOX setup the way it is and the Duke Power proposal. I am a little nervous -- that's very ambitious, and I appreciate

people's intentions. I just am not sure that's --

MR. NESBITT: I think when I presented the loading pattern information earlier, last meeting, I think I made it clear, but I'll reiterate this point now. These are the designs that we have analyzed now about how we would load, how we currently think we're going to load MOX fuel in 2008, or whatever. It's not carved in stone. I don't think it's going to change, quite honestly, based on our overall fuel management scheme and things like that and our overall approach, but I'm not prepared to sit here and swear on a stack of Bibles.

MR. SCHAPEROW: How about the 40 percent?

MR. NESBITT: Well, I think that's pretty much -- we haven't submitted an application yet. I don't think that -- again, I think that we're probably not going to go over 40 percent, for a number of reasons. But is it wise to speculate on that and paint ourselves in a corner two or three years before we submit an application on that point? I don't think so. I'm trying to give you all a picture of our best guess, our best guesstimate of how we're going to plan to use this fuel, based on our state of knowledge at this point in time.

MR. GIESEKE: What's the current practice

other places regarding that 40 percent?

MR. NESBITT: What is that now?

MR. GIESEKE: What is the current practice, like in France or other places, regarding this 40 percent? Do they find that to be --

MR. NESBITT: Different countries use different amounts of plutonium in the core based on various reasons. For example, in Belgium they use something on the order of 10 to 15 percent because that's how much plutonium they have available that they need to get rid of that, and that's why they're using it. In France they've chosen to go with 30 percent. In some of the German cores they're at like 38 percent.

Our value of 40 percent is based on, first of all, a desire to get as much plutonium disposed of as quickly as possible, consistent with the overall goals of the program, and our desire to keep the plant characteristics and operation reasonably consistent with what they are right now with uranium fuel. Because the more MOX you put into the core, the more the characteristics of the core change, and that drives the characteristics of the plant.

That's another thing, when we do all of the licensing basis, safety analyses, we might find

that -- I'm throwing out a speculation here -- we might find that for some reason we can only get a 35 percent. Maybe we find out we can get a 45 percent or something like that, but, based on the work that we've done now, which is pretty good, I think, that's the ball park.

MR. GIESEKE: But it sounds like, when you consider other places, like the other countries, and look at our goals that we need here in this country, the 40 percent is probably a good rule of thumb for a long time out into the future, I mean to guide us. It's probably not going to make any difference whether it's 50 or 20 or 30.

MR. BOYACK: Let me clarify what Jason was saying. My understanding of what Bernard said was that we just distribute the 40 percent uniformly across the core, not that you have a fully MOX core. In your statement, I believe you were talking about a full MOX core.

MR. NESBITT: Let me say one other thing on that. How far do you want to take it? A full MOX core is taking it all the way.

MR. CLEMENT: I was thinking about this matter. So I was wondering whether we have to reflect the differentials between MOX fuel and any fuel in

general and not go into the detail. You can have this MOX fuel that we put in the periphery, let it flow into the center, and so on, given the uncertainties that we have.

MR. KRESS: Brent, do you have this curve, if you could put it back? We had it a while ago.

MR. BOYACK: Yes.

MR. KRESS: Could we put that back on the screen a second?

MR. BOYACK: Yes.

MR. KRESS: This is the RT 1 and RT 2. Looking at it, it bothers me considerably with respect to this gap release. This is ordinary LEU fuel and this is cesium release. This is the MOX cesium release. If we're talking about a significant quantity of fission products, and we'll just use cesium as ours, because the iodine comes out about the same, and so does the krypton. Let's just draw a line and say that's significant. You could draw it anywhere, but just say, the difference between significant in this line and this ramp heatup rate is like the heatup of the core. The difference between this significance and that significance is like a half an hour. You're talking about a half-an-hour difference. You say, well, we have a half an hour for

the --

MR. GIESEKE: I think that is a very good point, yes.

MR. KRESS: That bothers me. I don't know what to do with it. But it's the only piece of data I've got.

MR. GIESEKE: This is not across the entire -- this is 40 percent.

MR. KRESS: I know, but I'm just reflecting the MOX and the other fuel. The 40 percent doesn't matter because, if you get that from 40 percent of the fuel, it's a significant matter. That's why I say you could draw it any way you wanted to.

MR. CLEMENT: With this transient, whereas you would get cladding ruptures, these represent ruptures at that time. That means that once you get the cladding rupture, at that time you already have significant adjustment. This is less than 30 minutes. Because you have to take care of it here. The cladding opens at the beginning of the experiment.

MR. KRESS: The claddings are ruptured in both those experiments.

MR. BOYACK: So what I hear Bernard saying is that with this MOX fuel, that if you have a

significant release at the time you have sufficient fuel failing, then it will come out. I mean, it will come out as soon as it's spilled. The question is, how long until you have sufficient failures to --

MR. CLEMENT: I agree with what Dana said, that we need to have more data. What you could do today is to reflect the fact that release will start earlier.

MR. LEAVER: It could start earlier. I mean, someone read from 1465, and it was jogging my memory. It's stylized actions, but it is true, I believe, that the release, the gap release, is assumed to begin when the rods are popped. So there is no delay there.

So one could argue, if you look at this -- I mean, this would probably be too conservative, but you could say that the significant release starts basically at time zero.

MR. KRESS: What I think Bernard is saying is true. If you follow that temperature around until you get to about 1200, then the first fuel pops, and now you haven't released the fission products up to there, even though this line says there's fission product release, because that's already for failed fuel.

So what has happened is the fission products are redistributing themselves inside the fuel during that heatup ramp to some extent, but they are not being released. Now you pop the fuel, and you've still got to diffuse these -- you get rid of the noble gases and the stuff in the gap, and then, actually, as the fuel is starting to diffuse now, the other stuff, there's some time, as it's still heating up, there's some time before you get to this part. This curve will look different in the lattice. It will start after the pop. It will come up here somewhere. So there will be a time before you reach a significant amount on this curve, and this would have been offset over this way. So both of these curves will be offset in a real accident over in this direction.

I think one could compare that distance right there, though, as a change.

MR. LEAVER: Isn't that about a half an hour?

MR. KRESS: It's about 15 minutes or so. These are half-hour marks here. I'm thinking more like half an hour. So we didn't drop it down to .25, but maybe we should have.

MR. GIESEKE: Then you get into the progression of the mount regularly.

MR. KRESS: Yes, there's that issue, too.

MR. GIESEKE: There's that issue that comes into play. That may be slower than the -- so maybe it's better not to go all the way down to .25, but to pick a place in between. So you're arguing for .3 instead of .4?

MR. KRESS: Yes, I think it's shortened more than we said.

MR. BOYACK: So where did you end up, Tom?

MR. KRESS: .3, which, you know, I hesitate to throw that up there, though, because that could start giving people trouble. A .3 duration of gap release can cause significant problems to have to deal with it. If it's not allowed, I hate to --

MR. SCHAPEROW: One of the ways I think about this, and maybe this isn't quite right, but the beginning of the gap release, the beginning of that half-hour period is the clad failure for a large grade LOCA. The end of the gap release, the end of the .5 hours, is clad failure for a small break LOCA, which is basically the time -- not long after that you start getting significant releases from the pellet.

I think that gap release timing does, in fact, reflect the thermal hydraulics issue. How long does it take to heat up for a very typical or

relatively fast severe accident scenario?

I'm appealing to Jim in this regard. He's had a lot of experience in this area on the heatup for these different scenarios: small break LOCA, station blackout, and --

MR. SCHAPEROW: It's a pretty short period of time.

MR. LEAVER: Thirty minutes.

MR. BOYACK: Let me come back to Dave now, and see if you are ready to give me a figure.

MR. LEAVER: Is that what we want to do here?

MR. BOYACK: I thought so.

MR. LEAVER: I mean, first of all, we had one test which suggests quite a bit earlier release from the fuel, maybe a .2, .3 kind of number, but that's one test. We have another test that doesn't. We also have the fact that the fuel, the MOX fuel, I guess would tend to be toward the outside of the core, which means it probably wouldn't see the same temperature transient as the LEU fuel, which is more toward the center of the core.

We also have maybe at most half the core that's mixed oxide. We also have recognized that this 30-minute gap duration is for a very, very unlikely

accident, large pipe breaks.

MR. BOYACK: As an individual, you're allowed to say, "No opinion."

MR. LEAVER: I'm trying to understand what we're going to do with this number, I guess. What are we doing here?

MR. KRESS: We are going to fix the thing about closure time.

MR. LEAVER: You mean make it slam close faster?

MR. KRESS: Yes, which is a bad thing--

MR. LEAVER: I think that's a bad idea.

MR. NESBITT: That's what we use the 10 seconds for, though.

MR. KRESS: Oh, we use the 10 seconds.

MR. NESBITT: Yes, that's right. This is for the dose calculation.

MR. KRESS: This is just a dose calculation. We aren't going to do anything with it.

MR. LEAVER: It's okay to talk about these things because I think that some of the things we talk about are necessary to understand when we make estimates of release fractions, but really this number doesn't have a huge effect unless we make zero; then it might. But it has to do, I think, with questions

like, how quickly do you have to actuate drawdown systems, systems that drawdown the back in secondary containment, and how quickly would you have to actuate sprays in the primary containment? Those sorts of questions I think are relevant, but as long as it's tens of minutes, a couple of tens of minutes, then it probably isn't going to have much effect, if any, on plant design.

But we could change the .5 to .4. That's not going to matter, even .3, but if you get much lower than that, then I think you could have an impact. Frankly, I don't see all these -- there's some competing effects here, and if there's a change, I guess my judgment at this point, which probably isn't worth a whole hell of a lot because there's nothing like some good, solid data; in the absence of data, one is very uncertain, but I would say, if we're going to change this .5 number, in my view, it wouldn't be much of a change. So I guess I kind of end up where Dana was at the beginning, which is, we could say .4, just to acknowledge that maybe things happen a little faster.

MR. BOYACK: Jim?

MR. GIESEKE: I like the .4 number. I hate to go over to .2 or .3 based just on this one

curve, but I think there's an indication it should go down from the .5. I think .4 is enough to reflect the direction of change.

MR. BOYACK: Okay, Dana, you've already given your value. Any other comments?

MR. POWERS: No.

MR. BOYACK: Okay, Tom?

MR. KRESS: I still like my .3, as a reflection of the differences between MOX and the LEU.

MR. BOYACK: So you're basically looking at the VERCORS RT 2 test?

MR. KRESS: I'm mentally integrating those with the raw heatup to a failure of a clad and a subsequent other heatup to start releasing fission products. I don't know how to make that mental integration, but it does look to me like the difference I see on there between those two tests would be reasonable representation of the differences in this mental integration.

I shortened it some, but not as much as we have up there. So I still like the .3.

MR. BOYACK: That's the RT 1 and RT 2 tests?

Okay, and, Bernard?

MR. CLEMENT: I'd say .4. There's

uncertainties, obviously. As you've written, you can start based on VERCORS experiments, but also I would say there is some insights from gas release measurements that tend to say that the release will start earlier.

MR. POWERS: I just did kind of a crude little calculation over here. I said, suppose I've got these little nodules of plutonium-enriched materials and around them are halo points. How many of them do I have up to next to the fuel cladded gap? Is there a direct release pathway? There's no incubation at all. It turns out a bunch.

MR. KRESS: You used a distribution function?

MR. POWERS: The linear distance is occupied by a halo around the perimeter. So you're feeding fission products in there pretty fast.

MR. KRESS: As soon as you fail the clad.

MR. POWERS: Yes, you're getting a little bit as soon as you fail the clad. It is not a huge amount, but it gets your attention.

MR. KRESS: That tends to tell me that the duration distance might be even shorter.

MR. POWERS: Yes, it may depend a little bit on how you see the progressive in the loading

thing, but you can make it shorter. Myself, I think you've got a much bigger challenge trying to get these types of tables in this kind of approach to the source term reflect higher release rates; that is, getting material into the containment earlier, for the engineered safety features to deal with for the early in-vessel release. I think that's much more challenging mental gymnastics that you're going to have to do there.

MR. KRESS: Yes, that's a big assignment.

MR. POWERS: What you're going to come down and see this next phase, what gets really released during this next phase really is cesium and iodine. Everything else, who cares?

MR. KRESS: And maybe a little bit of tellurium.

MR. POWERS: The only reason tellurium has any dose effectiveness at all is it decays to iodine.

MR. LEAVER: No, there's some - but, yes, it's actually even cesium doesn't contribute much relative to iodine. Iodine is really the thing that gives you the dose.

MR. POWERS: The cesium only works -- I mean the 137 only works on a long-term basis.

MR. LEAVER: Yes, long-term. For example,

a two-hour dose, that's all iodine.

MR. BOYACK: Let me ask, these gas release measurements, do they have a name of a series of tests?

MR. POWERS: I would just look at the Halden data.

MR. BOYACK: Doesn't EDF have some data, too, on that?

MR. POWERS: Yes, I mean there are a variety of reports that have come out of Halden, and they're continuing to generate more. They're running MOX and they're loading up the gap a little bit. As I say, the significance to attach for a boiling water reactor operated in Norway by a Finnish crew or a PWR located in the southern part of the United States, a little lost on me, but it gives you some information.

MR. BOYACK: Okay, let's move on into early in-vessel, and just remind you -- see, I'm a quick learner, Steve -- that the definition of the phase, we have the start of this phase defined by the end of the previous phase.

So what we say is, "During the early in-vessel release phase, fuel as well as other structural materials in the core reach sufficiently high temperatures that reactor geometry is no longer

maintained, and fuel and other materials melt and relocate to the bottom of the reactor pressure vessel. During this phase, significant quantities of volatile nuclides in the core inventory, as well as small fractions of the less volatile nuclides, are estimated to be released into containment. The release phase ends when the bottom head of the reactor pressure vessel fails, allowing molten core debris to fall into the concrete molten reactor pressure vessel."

Now we've somewhat compromised that in some of the other discussions as we moved stuff back and forth, I guess, but that's the latter two. So on this one I think it's clear.

Okay, guess who's first?

MR. GIESEKE: The issue, since it is defined from the end of this to melt-through, I don't have any reason to change the melt-through from that endpoint. Since we start a little earlier, we just add a little bit of time, the tenth of an hour, to the early in-vessel duration time to account for the earlier start, which is basically what we did with the high-burnup issue.

MR. BOYACK: So you're actually offering me a number of 1.3?

MR. GIESEKE: 1.4 I believe is a tenth

added onto 1.3.

MR. BOYACK: Okay, Dana?

MR. POWERS: I agree 100 percent with Jim. I see no evidence of a profound change in the core degradation scenario. Perhaps it will, and if I was going to highlight an area that I would be interested in seeing how things go, it is experimentally, this is one of the areas that I would like to see experiments done with some of these, because I can imagine all the troubles that you had getting relocation correctly modeled in the code, that if we're changing fuel chemistry and oxygen potentials and clad fuel interactions with this MOX fuel, there's a potential here.

But in the absence of having some real data on that, maybe there's some insights from the VERCORS experiments because they did seem to melt things out in interesting fashions, but that was explained, that they were still looking into that, and they pre-oxidize on clads. So it's a little too early for me to get excited about that.

The core degradation scenario is going to about the same. If you stipulated that, you would come out where Jim is coming out. We know that the effect of that is that I am going to have a lower

release rate of cesium and iodine into the containment, when, in fact, if anything, the data say it's the same release rates or higher release rates.

So now I'm caught on a -- I have a dilemma here. I mean the formalism is set up for one type of fuel, and then I try to apply it to a different phenological area. How do I properly reflect what's going on here?

The only thing I can come up with on that is that, okay, the timing, you have to be very careful with the timing on this. You've simply got to accept Jim's argument that it is now 1.4 long.

What I don't think I have to accept is that the release rate is constant during that interval. So I can say, why can't I have a triangular or some other kind of release rate during that period? I've got to do something to reflect what I think I know, and I can't do it living with the existent formulas on it.

MR. SCHAPEROW: Are you talking about release rate from fuel or release rate from the reactor? It seems that there might be a couple of things to kind of smooth things out on the release of the systems, such as steam flows and --

MR. POWERS: Not if you specify a large

break LOCA as our groundrules. There ain't no guide at all.

MR. LEAVER: There's no transport. I mean it's very, very short, a minute or two.

MR. SCHAPEROW: That's considered here, including station blackout.

MR. POWERS: Your groundrule at the start of this was a large break LOCA.

MR. SCHAPEROW: Or a low-pressure sequence.

MR. POWERS: A low-pressure sequence.

MR. LEAVER: You're right. Certainly the high-pressure sequences, there is a delay, but I think we probably should be looking at the low-pressure kind of open system.

MR. SCHAPEROW: The movement throughout the core, the time it takes to propagate throughout the core, maybe that's --

MR. POWERS: I mean, that's another view of the thing.

MR. SCHAPEROW: But that's not enough to spread it out maybe over two hours, but --

MR. POWERS: That's another view, is to say that this release rate is determined by the propagation of damage, has nothing to do with what's

going on in the fuel itself. It's really the movement of burn front, and that's just not going to change very much. So, yes, I will stipulate for you that that's an alternate view.

MR. BOYACK: I have two questions for you, Dana. The effect will be, this is no evidence of profound change. This effect will be a lower release rate of cesium and iodine into containment because?

MR. POWERS: Take the same release fraction and divide it by a larger time. That's the problem I'm getting into here. I'm very attracted to what Jason says, which is, no, no, no, release rate is just driven by how the damage propagates through the core, and you're not really seeing anything due to the fuel at all.

MR. SCHAPEROW: Again, I don't have a lot of experience there. Jim may have more.

MR. BOYACK: Then the second one was that this conclusion is not consistent with data. Just tell me what data.

MR. POWERS: Well, the data that I'm trying to reflect is the VERCORS data that we have seen with all the equivocations that Dave Leaver puts in. You've got one that goes one way and one goes the other. So it's a work-in-progress there.

Maybe it would help me puzzle this out. Is, in fact, the release rate we're reflecting in this early in-vessel, the release from the fuel that we see or is it damage propagation rate? Jason may be right, it may be damage propagation, in which case leave it the same, you know.

MR. SCHAPEROW: Although I've seen actual no core calculations or any test results that show, it only takes about 20 minutes to go from one end of the fuel rod to the other --

MR. POWERS: It's not one end to the other.

MR. SCHAPEROW: All right.

MR. POWERS: It's this way that counts. It's always this way that counts. I mean it's always this way that counts.

MR. BOYACK: Is the 1.4 something I can retain or do you want me to change it?

MR. POWERS: No, no, I mean, I think you're boxed into that because I just don't think we're in any position to say that the core degradation is wildly different. It's not that big of a change.

My only concern is the relocation story, just because, Bernard can tell you, we have fought like crazy to get the codes to predict properly where

fuel relocates, and it is intimately tied to the phase diagram and how much of the fuel is oxidized and how much of it is interacting with the zirconium. In the end, what the code guys do, they throw up their hands, the thermodynamicists, and they put in a temperature, and they adjust that temperature until they can match data.

Well, what that says is you've got to have the data because we need to do a MOX degradation test in order to confirm this is a very subtle change to things or this is a big change. Unfortunately, what we're seeing is subtle effects in the experiments.

He can tell you, he can explain to you the differences they observed between FPT-1 and FPT-3. All it is is changing a little bit of the steam flows and all the ramping. I mean, you get big things on this relocation business, and relocation is what marks the end of this.

MR. BOYACK: Tom?

MR. KRESS: Would you just back up and remind me what our old number was for the duration? That was for LEU fuel without burnup?

MR. BOYACK: That's 1465.

MR. KRESS: That's 1465? I'm not going to add the .1 to it because I can't see any reason why

the duration of the gap has anything to do with how long it takes the core to melt down and go to the bottom. So I'm going to keep the 1.3. I have no reason to change it. I think the core melt process is about the same.

MR. POWERS: If you don't add to it, Tom, you're shortening it because it's the sum of those two.

MR. GIESEKE: You're saying that the total of the first two is invariant?

MR. POWERS: Yes.

MR. GIESEKE: Well, then you've got to change the second one. You change the first one, and you've got to add.

MR. KRESS: I am not saying the sum of the two are invariant.

MR. GIESEKE: Well, you just said that.

MR. KRESS: I'm saying the 1.3 is invariant. I can do whatever I want to with the gap. The 1.3 is how long it takes it to heat up and melt down. It had nothing to do with the gap. It is independent of the gap.

MR. POWERS: It is .5 plus 1.3, is the time it takes to heat up and melt down. You have to add the two.

MR. GIESEKE: Yes, it's additive. That's the way the table is built. Do you want to change the table?

By definition -- he read the definition to you, right?

MR. KRESS: I want reality. I think it takes a certain amount of time to heat up and melt the core. It doesn't have anything to do with gap duration.

MR. BOYACK: All we're saying is you've used up four-tenths of an hour -- pardon me -- three-tenths of an hour in your first thought here.

MR. POWERS: In the original table it says it takes 1.8 hours to heat the core up and penetrate the vessel.

MR. KRESS: Okay, I see what you're saying. I'll make it 1.4, too, because I want the endpoint to come out the same.

MR. POWERS: Then you have to go to 1.5.

MR. KRESS: I would do the 1.5.

MR. BOYACK: You were the one that had the shorter period of time for the first interval. So they're saying, if you go to 1.8 and hold that constant, then this has got to be 1.5. That's all they're telling you.

MR. KRESS: Okay, make it 1.5 to be consistent.

As far as if we end up with a total release fraction, which is going to come out of MOX, some of it, and some of it is going to come out of LEU, and they're going to come out at different rates and possibly different total amounts because the release of the MOX fuel is going to go higher than the LEU, and then you're going to factor in the 40 percent. I think once you end up doing that and getting a total amount, that probably it's just as legitimate to draw a straight line for that as it was to draw the straight line in the first place. It's just a different straight line. It goes to a different level.

MR. BOYACK: Bernard?

MR. CLEMENT: 1.4, for the same reasons as for high-burnup fuel, as recalled by Dana.

MR. BOYACK: Okay.

MR. CLEMENT: And, also, the same remark as Dana, we need more data about degradation experiments with also measurements over the associated short-term, because putting back fission products release kinetics, I agree with you it would probably not be constant during this time in general.

MR. BOYACK: Is this a correct statement of the needs or a reasonably close one?

MR. CLEMENT: Yes, if I come back to high-burnup fuel, we said bundle experiments, bundle degradation tests, because you want to look at degradation. You cannot do that in a VERCORS test with three curves.

MR. POWERS: What about the PHEBUS test?

MR. SCHAPEROW: You can't do that. If you want to understand degradation, you've got to have a few rods.

MR. POWERS: A few rods?

MR. SCHAPEROW: Yes, it would be nice to have 100, but we'll take 21, if that's all we can get.

MR. BOYACK: Okay, Dave?

MR. KRESS: I think a single straight line is sufficient.

MR. BOYACK: Pardon me? Go ahead and just state it for me.

MR. KRESS: Constant release rate is a sufficient representation of this phase.

MR. POWERS: You're putting the model out of business. You're putting your model out of business here.

MR. KRESS: We'll have to apply the model,

though, to get the input.

MR. SCHAPEROW: Do you want to say it's because of the mix of MOX? It's because of the mix of MOX and LEU fuel in the core.

MR. POWERS: From the MOX, but it's not from the LEU, I don't think.

MR. KRESS: It was not 100 percent before from the LEU.

MR. BOYACK: Okay, Dave?

MR. LEAVER: I would like to I guess reflect the fact that at least in the RT 2 the volatiles come out faster or at a lower temperature. So I guess I don't want to increase the 1.3 hours. I'm not sure if this result is, in fact -- we can generalize, and there's certainly some question about that because of the other test, and we really don't have a good explanation for that, but I'm not even sure we can generalize to low volatiles.

We have a single duration for all fission products, but, even with that, this is very, RT 2 is a very interesting result, and it's hard to ignore. So I guess I'm not fixated on keeping the total interval the same. It would seem to me, at least in the case of the volatiles, that the release could occur faster. It's probably not constant and probably

what we're looking at is maybe the interval is the same, but it's a nonlinear shape. But that's way too complicated to even try to figure out. So I would just say I will stay with 1.3 hours, which makes the overall interval slightly less than for the LEU.

MR. SCHAPEROW: That suggests core degradation is faster, heatup, and everything --

MR. LEAVER: No, I'm saying that the volatile releases occur at lower temperatures.

MR. GIESEKE: No, the definition for this is until it knocks through the bottom head. You want to change the definition?

MR. LEAVER: No, I don't want to change that. I don't want to change that. I want to reflect the fact that between the time when this starts, which we've sort of generally said may be a little sooner than 30 minutes, that the volatiles, at least based on this one test, and certainly we need some confirmation, but the volatile fission products appear to come out faster.

MR. GIESEKE: No one denies that. I agree with you 100 percent, but you're saying that it melts through faster.

MR. LEAVER: No, I'm not saying that.

MR. GIESEKE: Then you have to change the

definition. The definition is until it melts through.

MR. LEAVER: Then let me pull out 1465 and we'll see.

MR. GIESEKE: He just read it to you.

MR. BOYACK: Yes, I just read it to you, and that is, "This release phase ends when the bottom head of the reactor pressure vessel fails."

MR. LEAVER: All right, but we also, I think, generally, at least some people have said, and I believe that the release rate is not constant. When we apply this to calculations, we have no basis for assuming anything other than a constant release rate. So if you're going to assume a constant release rate and you want to reflect the fact that it really isn't constant, and that they do come out earlier, then --

MR. GIESEKE: I am going to say that when I talk about the release rate.

MR. LEAVER: What are you going to say? It's not constant?

MR. GIESEKE: I'm going to say it's not constant. It comes out faster. It comes out not linearly.

MR. LEAVER: So you're going to have some kind of nonlinear release rate?

MR. GIESEKE: Yes, when we get to that.

MR. LEAVER: Well, how are you going to decide on that?

MR. GIESEKE: Well, I don't know. I'll just say it is. I can't define it, but when I get to putting the number in there for how much is released, I am going to say it doesn't come out uniformly over the time period. I think that's the place to take care of this issue, unless you want to break this time, I mean this period, into two parts or something like that.

MR. POWERS: That is one way to do it, but if one believes that this rate of release is really driven by the propagation of core damage, rather than release from the fuel, then there's no reason to do that.

MR. GIESEKE: Yes, that's true.

MR. BOYACK: The point is, David, that if you want to stay with this, there's no problem. It's just that I'm going to call out that it's --

MR. LEAVER: Yes, it is a little difficult to do this and not talk about release fractions at the same time. Let's go on and talk about that, and then we can revisit this as necessary. That's fine.

MR. BOYACK: All right. One more, and then lunch.

Dana, ex-vessel?

MR. POWERS: The ex-vessel release is really composed of two parts. One is the melt-concrete interaction and the continued degradation of that fraction of the fuel that didn't degrade up to the point of vessel failure.

The melt-concrete portion of it is determined largely by how much zirconium metal is present in the core debris. Consequently, all those things, nothing seems to have changed. I just don't see any change here. So I just can't justify changing from the original value. That's two hours.

MR. BOYACK: Could you just go through the two parts again, compose the two parts?

MR. POWERS: It is the degradation and expulsion of that portion of the core that did not degrade during the in-vessel release. That's roughly half the core. My own number, it's half the core. The rest of it is due to the melt-concrete interaction.

Since I didn't believe the core degradation changed in the vessel by any profound amount, and I certainly don't believe that the melt-concrete interaction changes by any profound amount, I can't change the time.

Now if what we find is that there is some substantially different interaction of clads with MOX fuel than what we have seen with low-enrichment uranium fuel, which I will agree is entirely possible, then those numbers could change. Fortunately, I think they all go shorter. So I think we're reasonably conservative with these numbers.

MR. KRESS: Shorter is worse, isn't it?

MR. POWERS: Not by the time you've gotten out to this point. What you're really playing with is the long-term release at this point. The worse two hours are now over. You're working on the tail at this point. So how long does your tail take?

Now the other thing is that you have to remember melt-concrete interactions puts up formidable amounts of nonradioactive aerosols. So it has an inherently limiting effect on the short-term. I mean, you get big particles at this point. So it kind of marks the end of real high concentrations in the containment atmosphere at this point.

That's right; you know, it's kind of a mixed bag on whether you want to cover up with water because, if you put up all this concrete stuff, it really sweeps out the aerosol big particles for you.

MR. KRESS: That's sprays, isn't it?

MR. POWERS: Sprays are real nice. I like sprays.

MR. BOYACK: Okay, Tom?

MR. KRESS: The same. I buy every bit of what Dana said. That's just my view exactly.

MR. BOYACK: Did you ever hear that before or was that a unique statement, "I buy his view exactly."?

MR. POWERS: Tom and I tend to be the structuralists on the Committee, and what we disagree with is over some of the details of philosophy because he thinks more about these things, as they make my head fuzzy.

(Laughter.)

But I would say that we tend to vote alike far more often than we vote disparately.

MR. BOYACK: Do you have "possums"?

MR. POWERS: I didn't hear you.

MR. BOYACK: Do you have "possums"?

MR. POWERS: No, I almost never have a possum. But when I do have to eat armadillo, I do put molasses on it.

(Laughter.)

MR. BOYACK: Okay, Bernard?

MR. CLEMENT: I would say two. I would

say no reason to change now. Parameters that could impact would be the amount and composition of melt that goes through the reactor lower head.

Here we come to the point of degradation experiments. If degradation experiments show large differences, not minor but large differences, core calculations will be needed, and at that time maybe duration could be changed.

MR. POWERS: You can envision things that would change radically. Because the M5 doesn't oxidize very rapidly, the interoxide could be quite thin in a fairly aggressive attack, get a large homogenization, and slumping much quicker, so that you get things on the lower head path.

On the other hand, it could go completely the other way and melt off the clad, it slumps down, and you're left with a lot of fuel.

MR. CLEMENT: That's what I call a large difference.

(Laughter.)

MR. BOYACK: Okay. All right, Dave?

MR. LEAVER: I agree with what Bernard said.

MR. BOYACK: Jim?

MR. GIESEKE: The same.

MR. BOYACK: The same as what?

MR. GIESEKE: We'll make it, "See DL." How's that?

(Laughter.)

MR. BOYACK: Now you guys were too quick on that one, which gives me a chance to finish the last one.

MR. POWERS: Already he proves to be an unreliable person.

(Laughter.)

MR. BOYACK: Well, that's true. But what this will do is shorten the time to get through the last one.

MR. KRESS: Now you've made him mad and he's going to be obstreperous and hard to get along with.

(Laughter.)

Talking about the suspension and revaporization, those things are suspending in the primary system, which I think we talked about height of what got released.

MR. GIESEKE: Talking about what?

MR. KRESS: Height of what got released. That's the way I read it.

MR. BOYACK: Let me interrupt just for a

minute. I am being fed material up here, which helps.

Back to NUREG 1465, it really is the right thing to do. It is just to talk about the definition.

"The late in-vessel release phase commences at vessel breach, proceeds simultaneously with the occurrence of the ex-vessel phase. However, the duration is not the same for both phases. During this release phase, some of the volatile nuclides deposited within the reactor coolant system earlier during the core degradation and melting may revolatize and be released into the containment."

MR. KRESS: That is what I said?

MR. BOYACK: Yes.

MR. KRESS: Now in the previous case, we had about 25 percent of what was released that was available to do this. Now we're going to, I'm going to increase the release fraction, so that I'm going to have more deposited and more to revaporize. Therefore, it is going to heat up faster, I think it will heat up faster. At least the contribution from what's on the wall is going to be more. The combination of steel heat coming from other places, but I think this is maybe driven mostly by the decay heat that's on the walls. I think that's about it.

So my duration is going to be shorter for

this because it's going to be more there and it's going to heat faster. How much shorter is it going to be?

MR. SCHAPEROW: More fission products deposited.

MR. KRESS: Yes.

MR. BOYACK: More deposited?

MR. KRESS: Yes. What was the duration before?

MR. BOYACK: Ten hours.

MR. KRESS: Ten hours? For the main in-vessels, 10 hours? I'm going to back off on what I just said. I didn't realize it was 10 hours. I thought it would be like three hours. This change in the amount deposited, it's 10 hours. I don't understand that 10 hours.

MR. LEAVER: Hossein, where did that 10 hours come from?

MR. SCHAPEROW: It says right in there it has to do with the amount revaporized.

MR. NOURBAKHSH: We made a couple of calculations. One, SPCP calculations, but for extended hours, to look basically at how much, because only the revaporization of the release from in-vessel -- we have only one or two calculations, and Gei Wu

did some adjustments, but he used that.

MR. KRESS: RCS piping over a 10-hour period with insulation on it?

MR. POWERS: I did some calculations, and what we found is the -- I mean, this was really to demonstrate that revaporization was a possibility. Initially, we put cesium hydroxide down on the surface and looked at its heatup and revaporization. It came off quite quickly. I would say over the course of 45 minutes to an hour. It was very quick.

Somebody said, "But I don't really believe in cesium hydroxide." I said, "Suppose we make it cesium monoborate," and you'll see why I picked monoborate. They came off pretty quantitatively over the course of three hours.

And I said, okay, suppose it's cesium pentaborate. So I'm knocking down the cesium partial pressures here with each of these steps, and it came out over the course of about 10 hours to the extent of about 25 percent on the material.

That range, we said, well, we have no idea what the surface deposit is going to be.

MR. KRESS: You're assuming that the whole pipe heats up over this time and it's insulated, and there's none of this heat lost?

MR. POWERS: No, we had heat loss roughly, I think we were losing something on the order of 2 megawatts. I mean we had a lot of heat loss.

MR. KRESS: Cesium pentaborate, significant vapor pressure?

MR. POWERS: Well, significant vapor pressure? We were getting substantial vaporization, which I take as partial pressure is 10 to the minus 6 atmospheres. I think we were certainly hitting it by the time we got to 800, 900 degrees centigrade. Now we never melted the pipe. We did melt some internals. Upper internals were melting out on us, but those weren't doing it. All that happened was that stuff was going on down and depositing back on the pipes. There was a natural convection calculation.

MR. KRESS: And this is where the 10 hours came from?

MR. POWERS: Yes, what we came up with was, I mean our conclusion was, what revaporizes depends crucially on what you assume to be the chemical form. We don't know. We argued that this thing will fight like crazy to go to the least volatile form it possibly can, and because it was a PWR, we had the borate available to react with, so we did.

We had the Elrich experiments that suggested it could very well form a silicate. We had the British experiments that said it could equally well react to form a chromate. All those things had repercussions somewhere within the range we were looking at. So we said, gee, it could go anywhere between, we said, three and ten hours.

Then Marty Plies took on the MAP code, and he said, I want to get into this game, too. He looked at Peach Bottom. What he was really looking at there was the heatup of the piping system in the drywell due to the core debris down below. He really wasn't doing a natural convection calculation.

He had the revaporization going on for 50 hours. So you can get any number you want to. I suspect the authors of 1465 looked at all this and said, "Ah, 10 hours."

MR. KHATIB-RAHBAR: If you look at the MELCOR calculations, if you look at the MELCOR results, Dana, you can run the code as long as you run to get revaporization. There's no end in sight.

MR. POWERS: That's right. I mean, it starts and it goes forever.

MR. SCHAPEROW: In NUREG 1465 it doesn't talk about a stopping point. It says, "the time at

which 80 percent of the revaporization happened." There are some words to that effect.

MR. BOYACK: What it says here is, Reference 17, "After review of the source term uncertainty methodology used in NUREG 1150 estimates the late in-vessel release phase to have been a duration of 10 hours." What I detect is it was just sort of pick a time.

MR. POWERS: In other words, a number that gives you a long-term tail on the source term to reflect what we thought was legitimate physics. The problem is, one of the biggest the NRC bought into the PHEBUS experiments, is to have a reasonably prototypic test as far as composition, to have a reasonable idea of what the chemical composition was in the piping system. Because I can turn the revaporization off completely. You let me pick the fission product chemical form, and I can turn it to zero or I can have it all come off in a half an hour, as long as you give me that flexibility.

MR. KRESS: Given all that, I'm going to keep 10 out.

(Laughter.)

I can't change that.

MR. GIESEKE: There's not strong enough

variation to change it.

MR. POWERS: Victoria has been run out for fairly limited periods of time, and Victoria finds many interesting things that we just never pursue, for the FPT-1 test in setting up, or FPT-0 test, for setting up the instrumentation, where we came back and recommended instruments be saved at the end of the test because we were calculating the cesium iodide was decomposing on the surface and you were getting iodine gas coming off or gaseous iodine coming off, after they had shut down the experiment, but still had flow from the system.

This is after they've gone through the core degradation, and they turned the test off, but they keep the flow going through the system. What we saw was the iodides on the surface were getting torn up, and we were getting long-term revaporization.

MR. BOYACK: I'm going to move on now because Dave's hungry, not to say me.

MR. CLEMENT: Ten. No reason to change associated with MOX fuel.

MR. BOYACK: Dave?

MR. LEAVER: I would agree with Bernard's statement.

MR. GIESEKE: "See BC."

MR. LEAVER: Revaporization calculations, why wouldn't you want to -- I mean even iodine I guess there can be different forms, but certainly you expect to be cesium iodide, that would be and that is most of the heat, I mean a substantial fraction of it. So that would be -- you're right, absolutely, the chemical form has a huge impact, but that would be a good one to do.

MR. POWERS: For 1150 we did cesium iodide, tin telluride, nickel telluride, ruthenium dioxide, cesium molybdenate, cesium urinate, and the answer is they revaporize as they see fit. You can actually get a chromatic graphic effect. It just kind of migrates down the piping system. It depends on the size of leach you have. I means, like I say, give me flexibility on the fission product chemical form and I can get you any result you want. You let me know what result you want; I'll get it for you.

MR. BOYACK: I would like the result on the late in-vessel.

MR. POWERS: Obviously, I'm going to stick with 10 hours. I'm going to have you put a caveat in. Dave is right that, if releases are qualitatively the same, in-vessel releases are qualitatively the same, the dominant source of heat really is the iodide. If,

however, we see substantial amounts of release of either the ruthenium or the barium and strontium in-vessel, they can accelerate the revaporization release. But right now I don't see strong bases for doing that. I can see bases for enhancing the molybdenum release in-vessel; it just doesn't carry enough heat to do anything. They've got to be different than --

MR. LEAVER: They are already 2 percent.

MR. POWERS: They've got to get up around 20 percent.

MR. LEAVER: For MOX?

MR. POWERS: You get up to those levels and then you start pulling with the heat a little more.

MR. LEAVER: What basis do you have for saying for MOX fuel strontium barium is 20 percent?

MR. GIESEKE: He says it has to get to 20 before it will affect his heating.

MR. LEAVER: Oh, okay.

MR. KRESS: No, no, I'm saying you're going to release that --

MR. LEAVER: You're saying that? Why are you saying that?

MR. KRESS: Why? Integrating a fission

product release model that puts barium and strontium in relation to the cesium, and I'm releasing all the cesium. I'm releasing all the cesium from the MOX. And the question is, how much of the barium strontium am I releasing? I've got a mental model of how related to cesium, and then it gets up to about 15, 20 percent, my mental model.

MR. POWERS: The reason you get interested in things like strontium, and whatnot, is because it's beta. When you put so much of that heat actually in the deposit --

MR. BOYACK: Those of you who would like to go to lunch now can do so, but we're going to be coming back at one o'clock. Those of you who would like to stay and talk may do so.

(Whereupon, the foregoing matter went off the record for lunch at 12:17 p.m. and went back on the record at 1:11 p.m.)

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

(1:11 p.m.)

MR. BOYACK: When we left, I had finally worked out through Tom the opportunity to go ahead and lead, if you would. We're on noble gases.

MR. CLEMENT: Okay, noble gases. So I'm wondering for gap release whether --

MR. BOYACK: I was just going through not putting numbers.

MR. CLEMENT: Two pieces of information. The first one is on the French side what is assessed for MOX fuel for design basis large break LOCA, as first presented last time by Jean Schiliva. In that case, it's 0.05 for MOX up to 37 EON base per ton. So 37 is not so far from what we are dealing with today.

So we could either keep the 0.05 like that or increase a little bit to reflect differentials with boron-rich uranium fuel with the same burnup. So I'm not so sure about can we come in with 0.05 or 0.07, as we did for high burnup.

MR. BOYACK: I think as we start these discussions it's fair to let other people go ahead and offer their comments.

MR. SCHAPEROW: Would there be any value in putting up the PWR high-burnup table that you had

before?

MR. CLEMENT: It was 0.0 --

MR. SCHAPEROW: It just seems like we're going to -- we seem to be heading down that road.

MR. BOYACK: Did you say from the high-burnup?

MR. SCHAPEROW: I've heard a lot of statements about how this may have quite similar behavior and it may burn up fuel. We have these rates. There may be a little earlier degradation in some cases. I don't know. Maybe I'm drawing too big a conclusion from that.

MR. POWERS: I think at our last meeting we got presented some information on the gap inventories. Similarly, there's literature on this. It seems to me that, yes, it's pretty clear that the gap inventories can be a little higher, but they're all within the 5 percent level. I don't see any reason to -- I mean, remember these guys are keeping their average burnup in the 40s. That's about where that table came from. We were thinking 38 at the time we put that table together; 38, 40, I mean that's all kind of the same number. You're not going past the transition where you get a change in structure. You may be feeding it a little more, but you're still --

I mean, there's supposed to be margins within that 5 percent, and I think there is.

MR. SCHAPEROW: I take back my comment then. I just saw we were starting to move toward down the road of a high-burnup fuel behaves like a MOX fuel --

MR. POWERS: I think you've got more arguments when you get to the in-vessel release, but for the gap --

MR. CLEMENT: 0.05 for gap releases and 0.95 for early in-vessel.

MR. BOYACK: Ninety-five, right, and then 00, I can take it from there.

MR. NOURBAKHSH: That .95 indication is 100 percent.

MR. CLEMENT: Okay. When we expressed our position from the French point of view, we always give the total release for that. So that means in-vessel and early in-vessel and late vessel, the .95. We don't make the distinction.

MR. BOYACK: Okay, let's see, so, Dave, I guess what I did there, there was a rationale that was given, but I was sitting back and listening in awe.

MR. LEAVER: Bernard's .95 is a total release. So to be consistent with what you were doing

yesterday, you will want to make that notation.

MR. BOYACK: Thank you. Okay, Dave?

MR. LEAVER: Yes, .05. I think that there is some data. Patrick Lampah presented at the last meeting showing higher fission gas inventory, but there is some margin in the 5 percent. So I think Dana's point is a good point. I think any change that I could make would be sort of the same as the high-burnup. I have trouble with the .07. So while one could make arguments that the .05 should be increased, I think that there's enough margin.

Do you want to do the early in-vessel, too?

MR. BOYACK: As long as we've got started, sure.

MR. LEAVER: Yes, on that one, I feel I need to -- since the release seems to start sooner, and I know there's this question about, is it a core degradation-driven release or a fuel release, and I need to think about that point, but I'm not sure that you could argue that the core degradation is faster, but maybe it's a little bit faster.

But I guess initially my thought is that, if you take the 40 percent release, total of 40 percent, I guess it's 35 percent for early in-vessel,

roughly, where that came from was a release of perhaps up to 70 percent of the iodine from the fuel and retention of about half of it, which I think is an okay way to think about it.

I think in the case of MOX probably that --

MR. NOURBAKHSH: This is noble gases.

MR. LEAVER: What's that?

MR. NOURBAKHSH: This is noble gases.

MR. LEAVER: Oh, I was thinking iodine. I'm sorry. Okay. Yes, all right, I was thinking iodine.

MR. BOYACK: I probably should have backed off. Why don't we just finish the gap release and then take these discussions one at a time?

Jim?

MR. GIESEKE: I can go with the .05 since that's the same as -- a little bit higher number, which it might be; .05 is a good number.

MR. BOYACK: And just for the heck of it, Dana, could you quickly rattle off your rationale again?

MR. POWERS: Well, the rationale is based on the database we've seen, limitations to the burnup that are to be imposed, and the fact there's probably

margin in the 5 percent level. I haven't seen data suggesting that I will be much above 5 percent on the gap inventory.

MR. BOYACK: Tom?

MR. KRESS: .06. I don't know how much margin is sufficient, and if we had a margin of 4, maybe we'll have more inventory in there. Maybe we ought to increase it to reflect that we think there's more inventory in the gap for MOX fuel, and I arbitrarily just made it a little bit, just to indicate that there's more in there. The .06 is no different than .05. It's just an indicator.

MR. BOYACK: Anybody have any statement about needs at this point?

MR. CLEMENT: When we have discussed about this point for high-burnup fuel, we have stated that for future LOCA experiments that will be performed in various spots it would be worthwhile to have in many cases a measurement of release for gases and also for other fission products. If there are LOCA experiments with MOX fuel, I think the same requirement is to be done.

MR. POWERS: Yes, it seems to me it's a logical thing to say. We are going to get some Halden data, and that will give us fission gas, but it didn't

give us anything else.

MR. BOYACK: Okay, let's go now to the early in-vessel phase. Dave?

MR. LEAVER: Why do we have this chart up over here, Brent?

MR. BOYACK: Which one do you want?

MR. LEAVER: Yes, that one, yes.

I think for the MOX fuel it seems we have this data point of an earlier volatile release. I guess if one were to just sort of simplistically say that roughly half the core is MOX, and we believe this number for LEU of .63, and we say, to be conservative, the MOX would release 100 percent of the nobles in this period, which is what we originally said in 1465 for LEU, which I think was conservative, but I think this approach here for the high-burnup fuel, the UO2 fuel, is a good concept. I don't know about the number.

Then I would say pick a number that is an average of the two, which would be, say, .78 or .80, for a total of 85 percent during this period of gap plus early in-vessel.

MR. NESBITT: Can I interject a question? Are you all looking at the core in its entirety when you do these MOX ratings or are you looking at the MOX

fuel --

MR. LEAVER: We should be looking at the core in its entirety, and assuming that maybe half or a little less than half of it is MOX.

MR. NESBITT: If that's what you all are doing, I think you need to make it clear because I was kind of assuming the other way around. I thought you were coming up with the source term for the MOX fuel in a partial MOX fuel core and you would rely on some sort of integration, depending on how much fuel is in there of each type.

MR. LEAVER: I would propose that we do this, think about this the way we did the high-burnup, which is it's for the total core, where we have a qualification here, which is that the kind of fractions you are talking about of the core are MOX, something in the range of 25 to 50 percent. So at the outside it would be 50 percent. Now if somebody wants to come in with a core of 100 percent MOX or 80 percent MOX, then maybe this wouldn't apply.

MR. CLEMENT: That means we have to make the arranging here. So which burnup should we consider for the LEU fuel? I'm sorry, but --

MR. BOYACK: No, that is an excellent question. I hadn't thought about that.

MR. SCHAPEROW: It is going to vary. It is vary over a number of years.

MR. LEAVER: What were you licensed to for your LEU fuel --

MR. NESBITT: What are we currently licensed?

MR. LEAVER: No, what would you be when your mixing in MOX? Still the same?

MR. NESBITT: Who knows?

MR. LEAVER: Huh?

MR. NESBITT: Who knows? I mean, I'm serious. I have no idea.

MR. KRESS: The 65 will probably still apply.

MR. SCHAPEROW: I would like to make a point that in the earlier assessment of release fractions for PWRs, built into that was an assumption that about 70 percent of the core would heat up to these high temperatures before lower head failure. So we have a smaller number there than we used to.

MR. LEAVER: No, we still have 70 percent.

MR. SCHAPEROW: Okay, but you just gave him .80.

MR. LEAVER: Yes.

MR. SCHAPEROW: So you're suggesting in a

MOX core a larger percentage of the core would be involved with heatup and relocations --

MR. LEAVER: No, what I'm trying to reflect is my concept, right or wrong, and for which there's data to the contrary, is that the volatile fission products come out faster in the MOX fuel, which means, if we're going to sit there and hold it for this period of 1.3 hours, or whatever that interval is, it's easier for me to visualize that you could get to essentially all the nobles released.

MR. GIESEKE: Only in a fuel that's failed and melted.

MR. LEAVER: Well, yes.

MR. GIESEKE: Which is -- I don't know what we said before --

MR. LEAVER: Right.

MR. GIESEKE: -- 60, 70 percent of the core. I think we used 70 percent of the core. That's the basis for our numbers. The first two total the 70 percent because 70 percent of the fuel was molten and 30 percent was still standing at the end of this time period. So we put 100 percent release --

MR. LEAVER: Still standing meaning what? Does that mean the clad is still intact or?

MR. GIESEKE: Yes, pretty much so, but

that's insignificant compared with the release you get if you melt it in any case. Because we say if the clad fails, you only get 70 percent without melting it. If we melt it, then you get basically 100 percent or the rest of it.

Go ahead. I'm sorry. It's not my turn to talk.

MR. BOYACK: If you want, I think we could go around. See, I have no objection --

MR. LEAVER: I am not saying 70 percent of the core is molten. I mean, TMI, you released about 55 percent of the noble gases, and you did not have 55 percent of the core molten. You don't have to melt fuel to release noble gases.

MR. GIESEKE: That's right, but you have to fuel the clad.

MR. LEAVER: Yes, you do. I think it's conservative to assume 100 percent, but who knows what the hell it is. But at this point I think, in the absence of data, this being provisional, preliminary, it's probably not a bad idea to be a little conservative.

MR. BOYACK: It's always difficult, a little more difficult, for the first person to have to go ahead and give a number, which is fine. I'm glad

to have it. But what I would like to do is move through, and then there's nothing, absolutely no wrong -- it's actually good if during the course of discussion other points are made and the person comes back and says, "Hey." Tom's done that once or twice. So let's do that.

So, Jim, your comments?

MR. GIESEKE: I am going to depending on --

MR. BOYACK: The way the wind is blowing?

MR. POWERS: He's a flexible person.

MR. KRESS: What do you do when you find out you're wrong?

(Laughter.)

MR. POWERS: Resist like a son-of-a-gun.

(Laughter.)

MR. BOYACK: Okay, Jim, you said -- now it's your turn.

MR. GIESEKE: I am going to say 65 percent, .65 here, which is kind of an estimate of how much of the fuel is going to be either melted down or the cladding failed to release, or we figured 7 percent of the claddings failed, something like that. So I figured 65 is a good estimate.

MR. BOYACK: Dana?

MR. POWERS: 0.65, for exactly the same reasons Jim was articulating there. I am taking about half the core as melting over this, actually melting and slumping, but there's another fraction of it that's suffered some substantial thermal insult, and that gets after the fission gases pretty good here. So .65 looks like a decent estimate to me.

What I believe is that this number is driven more by core damage than anything about the details of fuel microstructure and things like this.

MR. LEAVER: As opposed to an iodine or cesium?

MR. POWERS: Yes. It seems to me that it will allow me to integrate over 1.3 hours; a lot of the kinetics just aren't very important to me. As to whether you're melting it or not internally, I take half of it as melted, and a third of what remains has gone through some substantial thermal insult.

MR. BOYACK: Tom?

MR. KRESS: I am thinking.

MR. BOYACK: Okay, that's all right. We can wait.

MR. KRESS: It's .6523.

MR. BOYACK: Two extra decimal points, whoa.

(Laughter.)

MR. KRESS: You didn't type that in, did you? .65 is going to be all right with me, and it's for the same reasons. I was assuming about half the core was MOX and about half of that would undergo the high temperature melt, and that half the MOX would release all of its noble gas, and then you've got the other half of the core as LEU, and some bigger fraction of it, like most of it, is going to go through the high temperature melt. It's going to release similar to what we had in the other one.

When I put all this together, it came out close to 65. So I just said 65 is a good enough number.

Whatever Gieseke says is probably right, and he didn't even have to go through all of this machination. He just did this, and there it is.

MR. POWERS: That seems to be what he did for the source term code package stuff, wasn't it? He just made up new graphs?

MR. NESBITT: Pardon me for an interjection. It seems to me an implicit assumption that you're looking around a 50 percent MOX core in these numbers. I ask that you make that implicit assumption evident in the report.

MR. POWERS: My number does not depend on what fraction of the core is MOX.

MR. KRESS: Well, mine did because I assumed that MOX that underwent the full temperature transient released all of its fission products, noble gases. But I'd assume some of it didn't -- well, including the full transient, because some of the residual fuel is left in there. So that split was on account of an arbitrary number. I don't know how much is going to go through it and how much isn't.

MR. LEAVER: Are you assuming that all of the LEU fuel undergoes the transient, but only half of the MOX will? I mean, that's your rough concept?

MR. KRESS: It was more like 70 percent of the LEU.

MR. LEAVER: Seventy percent of the LEU?

MR. KRESS: Yes.

MR. LEAVER: And about half of the MOX?

MR. KRESS: Yes. And that 70 percent released -- you know, this number has implicit, the 63 has implicit, a fraction also.

MR. LEAVER: Right.

MR. KRESS: So I have to convert that.

MR. LEAVER: Right.

MR. KRESS: Then when I did all that, it

come out to about 65. It came out, and I don't remember what it was, but it was close enough to 65 to sound like it was a good number.

MR. LYMAN: Can I make the suggestion, as a member of the public, that you reconsider whether it should be based on a partial MOX core or not? Because I think for understanding what you're doing, if you want anyone who is reading this report to be able to understand it, I think you want to isolate the MOX portion, particularly the MOX. I mean because otherwise everyone is using almost a different core fraction in their minds. It's going to be very hard to obtain.

MR. KRESS: That's an interesting thought.

MR. BOYACK: It's a reasonable point.

MR. KRESS: You'd get entirely different numbers if you did that.

MR. NESBITT: I hate to agree with Ed, but in this case I think he's right.

MR. KRESS: Or we could say a constraint on these numbers is that 50 percent of the core is MOX. That could be a constraint on the numbers.

MR. GIESEKE: I think we established it's more likely to be 40 percent.

MR. KRESS: Forty is the same as 50.

MR. GIESEKE: Oh, I see, now you're saying it doesn't make any difference, which is what I would have said --

MR. KRESS: Between 40 and 50, it doesn't make any difference, but between 50 and 100 it probably makes some difference. That would work. We were just considering 100 percent MOX.

MR. BOYACK: The issue has been raised, and the point has been made, that from the standpoint of trying to discern the effect of MOX alone, if one considered a MOX core, totally a MOX core, and then went through this process, you would have a very clear insight, if you had a MOX core.

MR. KRESS: Then somebody could take their fractions and adjust these by the fractions they actually have; that would make some sense.

MR. NESBITT: You don't have to assume it's all MOX. You just have to assume that your release fractions are based on MOX.

MR. GIESEKE: Look at what we did without MOX. You get 70 percent through early in-vessel and no MOX at all. Here we're saying 70 percent through in-vessel with MOX. So it doesn't make any difference in this case, but this is noble gases, and it's good to have the conversation because down the road, as you

go down the list of groups here, you're going to get into situations where you care about it.

MR. BOYACK: So what happens is that, if you give a straight MOX composition, then somebody downstream has to do the integration.

MR. KRESS: What they're going to do downstream, I think, is take the old source term and take the MOX source term and add them together related by the fraction of MOX. That's what they're going to do. I don't know if it is the right thing to do, but it makes about as much sense as anything.

MR. BOYACK: Well, but it would be easier for us to work through as a panel a straight MOX, I think.

MR. KRESS: Yes, at least you're thrusted with straight MOX, rather than factoring in these percentages. It makes some sense to me to do it that way.

MR. GIESEKE: The problem is yesterday we weighted it according to percentage of high-burnup.

MR. KRESS: Now that's harder to do.

MR. GIESEKE: Because they're all going to be the same, and it's not an a priori variable, variable.

MR. KRESS: Yes, but you're right, we

could have assumed it was all high-burnup and let them factor in the fraction of the other two.

MR. BOYACK: Bernard, was this your initial suggestion sometime ago?

MR. CLEMENT: I was raising the question. It was a thought, yes.

(Laughter.)

MR. SCHAPEROW: I'm the one who suggested, what happens when we go all the way to 100 percent?

MR. BOYACK: Well, to me, it's the customer that's going to use it. So I can go either way. But I do sense some simplicity. Not only that, but it seems to me that you have a longer life, shelf life, for the information if it's MOX totally, because then as there are these changes downstream, you just adjust accordingly.

MR. KRESS: And people wouldn't have to make this integration or wouldn't have to figure out how we made this integration ourselves.

MR. CLEMENT: But this is different than from high-burnup because for high-burnup you cannot imagine having a whole core with high-burnup fuel. You cannot imagine that. The proportion of MOX may vary.

MR. SCHAPEROW: It would help to

streamline neutronics though.

MR. KRESS: I don't think so.

MR. LEAVER: Constrained by what --

MR. SCHAPEROW: The neutrons, the fissioning rates, the decay fraction, or whatever affects control --

MR. LEAVER: Do you mean in terms of what fraction of the assembly is going to be MOX?

MR. SCHAPEROW: -- controller reactor.

MR. KRESS: He's talking about the delayed neutrons and control. You can still control them. You could have a whole core of MOX if you wanted to.

MR. LYMAN: You are not doing a whole neutronics analysis.

MR. KRESS: No.

MR. LYMAN: All you're doing is a release. It's not like you're studying a whole MOX core and then have to do the whole regimen for a whole MOX core. It's this particular calculation.

MR. KRESS: So you wouldn't have to confuse that --

MR. LYMAN: You're not putting information about the spectrum anyway.

MR. SCHAPEROW: I would have to talk with somebody else in my office before we went that far.

That's a change to the original intent of the -- it's not a big change, but I can make some calls now if you want to take a break.

MR. KRESS: It might be worthwhile because it makes some sense.

MR. SCHAPEROW: This is kind of a big change for me to just say let's do it.

MR. LEAVER: We have a lot of questions in our minds about core damage progression in MOX, and I guess I take just a little bit that there's not much basis for taking comfort in doing these estimates because of lack of data, but I do take a little bit of comfort in the fact that realistically I expect this core to be mostly LEU. So even if there are some significant differences in some of these phenomena for a MOX core degradation, it's less than half the core, and so I figure, gee, I can't be too far off in sort of trying to imagine an LEU core with some MOX assemblies. Now if you say 100 percent MOX or 90 percent MOX core, I'm even more uncomfortable.

MR. NESBITT: You can have it both ways. You can say these are the release fractions we assume for the MOX fuel with the implicit assumption that MOX is 50 percent or less of the fuel in the core. You can have it both ways.

MR. LEAVER: That would be okay.

MR. BOYACK: I'm a happy camper. I can go any way. Dana, any thoughts?

MR. POWERS: I am going to be making such modest adjustments to the original PWR table that have more to do with the other features than the MOX-ness of the fuel. For instance, I will be adjusting the tellurium releases a little bit back toward what they originally were because of the M5 clad. The other release fractions I think are, the important ones -- that is, the cesium and iodine releases in-vessel -- are driven by the extent of core degradation so much that it is really fairly inconsequential to me whether you look at those things as the MOX fuel alone or the entirety of the fuel.

Now if that entirety of the fuel was very high-burnup fuel, that would be a problem, but I don't think that's the case. No matter how they run the reactor, at any one time the low-enrichment uranium fuel won't be a lot of it -- I mean you're only toying with 60 percent of the core, so maybe 20 percent of it could be very high-burnup. So it really doesn't influence me one way or another.

MR. BOYACK: So it doesn't really, in fact -- we've got now three options: continue as we

started; full MOX core, and the last one was just the MOX fuel assembly release fractions in one of these cores that has about 60 percent LEU.

MR. SCHAPEROW: How is that different from how we started?

MR. NESBITT: Because you don't have to smear your release fraction to account for this much of the fuel is uranium and this much is MOX, uranium is going to do this, and this much is going to be melted, and MOX is going to do this; this much is melted.

MR. KRESS: If you did that, your release fractions would be the same as if you assumed the whole core was MOX, I think.

MR. CLEMENT: It depends on how we would consider the values. If, from indications that we have that we can have higher release rates with MOX fuel, generally speaking, if we consider that, roughly speaking, for an intermediate burnup, MOX fuel behaves roughly like a high-burnup of LEU fuel. If you make the average, I think we should come to values that are not so different from the high-burnup fuel, given the uncertainties we have right now because of the lack of data.

MR. KRESS: The high-burnup fuel, I think

we would call one-third of the fuel high-burnup. The MOX we're calling it half the core. So you might get numbers that are a little higher for the MOX, if the effect of the MOX is about the same as the effect of high-burnup fuel.

MR. GIESEKE: I can't imagine that you're going to know anything close enough to know the difference between .3 and .5 for the MOX, except for Tom Kress, who makes it .6523.

MR. KRESS: If they're going up, I can go up.

MR. SCHAPEROW: Why don't we continue as we were doing, and I will go to see if I can get some further guidance as to whether we can switch over to just considering MOX and not worrying about the rest of the core. Okay?

MR. NESBITT: Make sure they understand that it's straightforward to transform it to the whole core if you've got the MOX numbers and the LEU numbers.

MR. SCHAPEROW: Well, maybe. I'll be back in a few minutes.

MR. GIESEKE: Who had the lead on the next one?

MR. KRESS: I think Dana's the one.

MR. GIESEKE: The next number is .3.

(Laughter.)

MR. KRESS: I think Dana's point suggests that he would go back and change the 1465 numbers for LEU fuel, and then if those were changed and we did a full MOX, and you let somebody agglomerate those by the percentages, then you would have a coherent system. But let Dana say, what he's going to do now is, rather than go back and fix the 1465, he's going to factor that into what he says this is. It's going to confuse the heck out of a lot of people because they're going to say, "Where in the heck did that come from?" As long as you've got enough of your rationale up there, I think it could be figured out. But I would rather, to be rational, say 1465 numbers ought to be changed to this, and, oh, by the way, if you had a full MOX core, it ought to be this, and you guys figure out how to put them together.

MR. BOYACK: Did Tom have that right? Is the 1465 numbers that you would say also needed to be adjusted?

MR. POWERS: Well, where the adjustment becomes dramatic is on the large release fractions because 1465 is essentially degrading 100 percent of the core in-vessel for noble gases, and then did

something else for the others. It just looks strange. Now I'm just correcting that strangeness.

The only place that it stands out, that it looks really peculiar, is, in fact, the noble gases. There's one other area where things will look a little peculiar, and that is I think we do have some hint that the noble metal releases for the MOX fuel are a little bit higher than what we've done for low-enriched uranium. That distinction that we choose to draw between moly and ruthenium for the high-burnup fuel we may want to preserve.

MR. BOYACK: Well, let's continue on with ex-vessel with the noble gas and give me whatever you want on that.

MR. POWERS: This is Dana. Still .3. Oh, I'm sorry, I must have been listening to my own self talk.

MR. KRESS: It doesn't add up to 100 percent.

MR. POWERS: It should. Do I have to do the 35?

MR. KRESS: Yes, you do.

MR. POWERS: So I've got 5 percent on the gap and I've got 65 on the in-vessel.

MR. KRESS: Oh, oh, oh, okay.

MR. POWERS: Only in Tennessee does it not add up to 100.

MR. KRESS: Well, we use 3.0 for 5.

MR. POWERS: Yes, see, that's the problem. You integrate it around the circle, and you've got 14 percent.

MR. KRESS: I have to figure out how to make mine add up to the same thing, and I've got a .06 up there.

MR. BOYACK: You surely do. I think you end up with .29.

MR. KRESS: That makes it .29? Oh, crap.

(Laughter.)

MR. BOYACK: How could you know such a thing?

MR. KRESS: No possible way. I'll make it .3 and say I'm done but don't know how to add

(Laughter.)

MR. BOYACK: Tom plans to be rigorous on his demanding 1.0.

MR. KRESS: In reality, people would use .3 there anyway. So we might as well put in .3.

MR. POWERS: They'll use your .29.

MR. KRESS: Well, you're right. I wouldn't. I'd use .3. I'd use .3, and they'd say,

"Hey, but you're adding up to more than one," and I'd say, "So what?"

MR. LEAVER: 0.15.

MR. BOYACK: We're compensating for the errors which you know exist.

MR. LEAVER: So we're down to zero, are we? Nothing left?

MR. BOYACK: Now on the round robin, Dana, you --

MR. POWERS: Do I get to do them all or do I have to do them one at a time?

MR. BOYACK: Whatever way you want to do it.

MR. POWERS: I want to do them all.

The gap release is 0.05, the same rationale as for the noble gases, and coupled with the fact that I fundamentally believe that that gap release of the condensible fission products is driven by the amount of gas you've got to flow out during the gap release. You can put all the inventory you want in there, and you ain't going to get it in the gap release if you can't vaporize it.

Now comes an interesting number. This one deserves some explanation: 0.325 Okay, what you notice right off the bat is that it's a smaller

release fraction than where you had the PWRs and you say, "Hold it, how can that possibly be?" I mean we have all this evidence that we get faster release, more intense release fractions, for these halogens coming out. We can get them for cesium, but I can zoom the halogens about the same. "How can you come up with a lower release fraction, you ding-a-ling?"

And it's real simple. I get these higher release fractions. I have higher concentrations of the piping system. I'm putting more on the piping system. So less is getting out. A smaller fraction is getting out during this phase. I'll get it later because I'll jack my late in-vessel release up in response to the higher heat rates that I've got on the piping system.

MR. BOYACK: So higher deposition.

MR. LEAVER: Just you are talking about higher concentration of aerosol?

MR. POWERS: That's right, the vapors in aerosol. I mean, if I'm going to release it faster, then I'm going to increase those concentrations in there, and especially for aerosol physics, it's going to go roughly to the square of the concentration, and I'm going to reflect that.

MR. GIESEKE: But I don't think there's

going to be more aerosol. I think that's all structure and stuff, is the aerosol, by tons.

MR. POWERS: Sure, and this fission product goes right on the surfaces of those, binds up to it and goes onto the surface.

MR. GIESEKE: But it's not going to deposit any faster than it ever did? It's not a concentration effect on aerosol deposit because aerosol concentration is the same.

MR. POWERS: Yes, but the partial pressure of vapor, it's interacting with those structural materials and things like that's higher.

MR. GIESEKE: Then more also has to go out with the gas phase, because the only way to get the partial pressure higher is to get it in the gas phase. It's not absorbed --

MR. POWERS: Initially, and then I'm going to go through the cooler section, and it's all going to condense out to aerosols, but those are going to go out. It's the initial deposition on the particles that's going to go up.

MR. GIESEKE: Okay.

MR. BOYACK: Ex-vessel?

MR. POWERS: Okay, let's see if I can find my ex-vessel number. Ex-vessel is 0.20, and late in-

vessel is 0.20.

MR. BOYACK: Okay, Tom?

MR. KRESS: I am going to be consistent and stick to my .06, but it's going to give me trouble later.

(Laughter.)

The same rationale I gave before.

MR. BOYACK: It's easier this time because now you don't have to --

MR. KRESS: Yes.

MR. BOYACK: You can say it's just on the surface sometimes.

MR. KRESS: Yes, that's right, I can.

MR. BOYACK: Okay, early in-vessel?

MR. KRESS: I'm going to go by Dana's argument about the higher fraction deposited makes up for the amount released. I think it's too confusing. I buy Jim's argument that most of those aerosols were already there. I don't know how to do the partitioning between vapor and gas phase just yet because they go through a temperature grading. So I'm going to stick with the amount that I think gets released, and I think I'm going to heavily weigh it to large break LOCAs, so I don't get a lot depositing all over, because the original had about 50 percent of it

deposited, I remember was built into the .25.

We're on the halogens. It's .35 in early in-vessel. What's implied to me, that about .7 is really the release fraction. I think that release fraction has to be higher because that was for LEU fuel and now we've got 50 percent of it as MOX, and I think you're going to release all of the iodine for the MOX that only goes to transient.

Once again, I'm stuck with how much of the MOX is residual fuel and how much is not. But I assume I'm talking about 50 percent core melt as a substantial core melt quantity, which is what we did at one time, or 70 percent I think was what Jason said we used. So if I use 70 percent for both the LEU and 70 percent for the MOX, and say the MOX is going to release all of its fuel, all of its iodine, then that's a .7, but half of that is going to deposit out if I be consistent with the core. That gives me .35. I have to add that to the .35, the .35 we had before, but only half of that because half of .35 is like .17. So .35 plus .17 gives me the number I want.

MR. BOYACK: .52?

MR. KRESS: I would make that .6, round it up. That's the number I want to go with.

MR. BOYACK: I wasn't quite able to get

the mathematical formula down.

MR. KRESS: Never mind. Just say that this factors in the fraction of the MOX that undergoes the transient as well as the fraction of the LEU, and that the MOX that undergoes the transient releases all of its iodine.

MR. BOYACK: Let's see, we were continuing down ex-vessel.

MR. KRESS: Ex-vessel, okay.

MR. BOYACK: You've got .34 left.

MR. KRESS: No, I've got that .34. There's something screwy in my math here. Why don't you go on to the next guy and let me do my math over again?

MR. BOYACK: All right. Bernard?

MR. CLEMENT: Okay, so I want to give the same values as before, .05 for gap release and then .95 for the remaining.

Now source term, in fact, you release all of the remaining in the early in-vessel, as we did in the faster to make the repartition move on, because you don't have the same hypothetical.

MR. BOYACK: Now is that a regulatory approach or do you believe that it's all released?

MR. CLEMENT: No.

MR. BOYACK: Two questions in there; one answer. Yes, you believe it's all released? Or it's a regulatory approach? Which is it?

MR. CLEMENT: No, it's not a regulatory approach.

MR. BOYACK: Yes, it's what you think physically happens?

MR. CLEMENT: It's a thing that physically -- I mean a large amount of iodine is released. We think that for the situation of a hot leg break, that is an envelope scenario for that. All of the iodine could be as vapor, so that we can make the hypothesis of a very small retention, so that we come out for reasonably enveloped situations for the mentioning of events dependence to 100 percent release.

MR. NOURBAKHSH: What time duration?

MR. CLEMENT: In our case for the time duration, it corresponds to the .95 during the early in-vessel phase. That's what's taken.

MR. NOURBAKHSH: Yes, but how much do you use for your duration --

MR. CLEMENT: That doesn't matter so much because you will see if you have to recommend your population or not, but this is not within a few hours of your actions. That's a much longer time. So we

consider a shorter duration, but it doesn't matter so much.

When we take into account this early release of iodine, then we apply the content and performance, and so on.

MR. BOYACK: Based on this?

MR. CLEMENT: Based on this.

MR. BOYACK: What time?

MR. CLEMENT: What time? I don't have the exact time, but it corresponds to the duration of the early in-vessel, and even shorter. Even shorter. Then you apply the measure of the performances of the containers.

MR. BOYACK: Okay, Dave?

MR. LEAVER: I think for the same reason I said on noble gases -- oh, gap, yes, .05.

On early in-vessel, again for the same reason as I said on noble gases, I think that for the MOX that there's a faster and higher release, but it's only a portion of the core. I guess, based on the calculations we've done, I would estimate this effect being slightly in the other direction from what Dana did. So that if the release is larger, while you will get a bit more deposition, it's not enough to make up for the larger release, and in fact you will get

larger release from the fuel and you will get larger release to containment.

So if you use numbers like half the core being LEU, half being MOX, in the case of the LEU we get melting and/or significant thermal damage to 70 percent of the core, which is where the .35 number came from, roughly. Then I would say that number may perhaps be more like .4, but there would be some modest increase in deposition, so I'll go with a number in between, .375.

MR. BOYACK: See what you started, Dana?

Well, that's all right. There was a rationale there. But for me, it's sort of staying awake and engaged, being kind of a fun guy for the rest of you and giving you somebody to laugh at, sort of.

Okay, let's go on to ex-vessel.

MR. LEAVER: Yes, I mean you could round up the .375 to .4, but I'd just as soon have it be .375 for the moment.

MR. BOYACK: Yes, that's fine. Ex-vessel?

MR. LEAVER: I have no basis for using a different number other than it shouldn't add up to more than one. Make it .2 then, and then make that .2. What's that add up to?

MR. BOYACK: 4, .775 -- is that .825? No, wait a minute. Yes, .825. I get .825 out of that.

MR. LEAVER: Yes, that's okay. You can actually make ex-vessel .25 because I wouldn't have any basis for changing the number that we came up with for the high-burnup fuel.

MR. BOYACK: Okay, Jim has now been working out his numbers, right?

MR. GIESEKE: Yes. .05. Down here, following through what we did before, if we have, oh, I don't know, we talked before for the noble gases of maybe 70 percent of the core being involved. I think that's what we did before when we did the .35, like Dave has said. So we're going to release from that essentially all of it to get .7 out, but transporting -- I assume we're going to retain half of it again, like we did before, rule of thumb. So I have to put .35 in that spot right there.

Now just to comment, I don't think that the increased fission products, if there are any, which there aren't in my case anyway, would affect the transport significantly.

Okay, go down to the next one. So down here it looks to me, from what we had before, we're releasing about 80 percent of the material that leaves

the vessel one way or another, and I don't see any reason to change that. So I'll hold that at 25 percent, .25 again, and come down here, and I don't see any reason to change that, .2 again.

MR. BOYACK: Okay, Tom, have you reworked your numbers?

MR. KRESS: Yes. I'm going to still be weird and stick with a .06 just as an indicator. Early in-vessel, my numbers work out to be about .45 instead of .06.

Then ex-vessel is about, well, because I'm releasing more in-vessel in places, it comes down to be about .15.

Late in-vessel turns out to be about .2 that we had before.

MR. GIESEKE: So you think 90 percent of the core is involved with the damage molten in -- to get your .45 number?

MR. KRESS: My numbers were 50 percent of the core as being MOX and --

MR. GIESEKE: But if you look at the known gases, which we've already said that we're only involving like, what, 70 percent of the core --

MR. KRESS: Let me redo my numbers again because I don't think that's right either.

MR. GIESEKE: The rest of it is still standing relatively undamaged.

MR. BOYACK: While he's doing that, can somebody, would somebody care to hold forth on whether or not there are any data needs or needs that go along with this particular area, halogens for MOX fuel?

MR. POWERS: Everything's necessary. We don't know how that stuff degrades. We don't know how it releases. That's right. We probably don't even know what the inventory is very well.

MR. BOYACK: I mean the statement's great. Now I've got to figure out how to translate that.

(Laughter.)

That would just go across the board, right?

MR. POWERS: Yes. I don't think you need to say that with respect to the melt-concrete interactions.

MR. BOYACK: With respect to what?

MR. POWERS: The melt-concrete interactions, because I think we understand how the halogens are behaving down there, and any MOX-ness of the fuel has been wiped out as soon as we melt it, put it on concrete.

MR. BOYACK: Okay, so basically okay in

these areas here?

MR. GIESEKE: The one above there, the early in-vessel --

MR. BOYACK: Okay, you started to say on early in-vessel, Jim?

MR. GIESEKE: I think it may be worth noting there that we said before, dealing with noble gases, how important the damage progression was, and the same things drives a lot of this, at least from my perspective, the damage progression. So I think that's particularly important there.

MR. BOYACK: So we're talking about noble gas releases as a function of the manner in which they --

MR. POWERS: Halogens.

MR. GIESEKE: We said that before, and I'm saying again here for halogens that melt progression or damage progression is crucial to --

MR. POWERS: As would be expected.

MR. GIESEKE: Yes.

MR. BOYACK: I must be getting tired. Just give me a few words, and I'll write them down.

MR. GIESEKE: Damage progression information is crucial.

MR. BOYACK: That's a few words. I can

get that.

MR. POWERS: And what I would say down in the ex-vessel is the core concrete is okay, but any part due to the continued damage of the vessel fuel, we've got the same problem as the air ingress. If not in spades, we don't have any air ingress data.

This is no different than our basic uncertainty about revaporization. There is nothing peculiar about MOX here. We have very poor information about revaporization, nothing peculiar about that, lack of information here with respect to MOX.

MR. GIESEKE: I think air ingression is important here. If you're going to cite it specifically in the other, it certainly is important in the next one.

MR. POWERS: Yes, it's very important down here.

MR. BOYACK: Jason must be having an interesting discussion on this, but if he stays away long enough, we'll be done.

MR. GIESEKE: Did you want to add air ingression on the --

MR. BOYACK: On the last one?

MR. GIESEKE: -- on the last one?

MR. BOYACK: I didn't realize that's what you were telling me.

MR. GIESEKE: The late in-vessel, which is what we were saying.

MR. BOYACK: I had air ingress up above.

MR. POWERS: Well, the revaporization, it really plays a number on iodine, if you get it. If you get air in iodines, it turns all of the iodines into iodine gas.

MR. BOYACK: Is the air ingress data, it's not the amount of air; it's the effect of the air on the -- okay? Everybody but Tom can take a break.

MR. KRESS: Good idea.

(Laughter.)

MR. BOYACK: Do you need a little bit more time?

MR. KRESS: Yes.

MR. POWERS: He's doing a new correlation.

MR. KRESS: No correlations.

MR. POWERS: He's going to have a whole code here for us.

MR. BOYACK: Why don't we come back at 2:30?

(Whereupon, the foregoing matter went off the record at 2:18 p.m. and went back on the record at

2:34 p.m.)

MR. KRESS: The number I come up with for early in-vessel is .35.

MR. BOYACK: I believe that.

MR. KRESS: I don't believe that.

MR. BOYACK: I trust you.

MR. KRESS: I don't understand it, but it is.

MR. BOYACK: That's a good number.

MR. KRESS: And for ex-vessel, I'm assuming that's what went down with the melt and got out with the MCCI, and that's only about .15.

MR. BOYACK: All right.

MR. KRESS: And late, .2.

MR. BOYACK: Okay, so we changed one number?

MR. KRESS: Yes.

MR. BOYACK: That's good.

MR. KRESS: And it all adds up now.

MR. BOYACK: And you gave everybody a break, too.

MR. KRESS: Yes. I don't know how it came out that way, but it did. I took 70 percent of both the MOX and 70 percent LEU, undergoes the melt, of the MOX, what gets released from the fuel is all of it;

what gets released from the LEU is about .7, and then that gives us the release. I assume about half of it plated out, and it adds up to these numbers.

MR. POWERS: So the way you were doing the numerical evaluation of the electrical interval, I think that's not a suitable approximation.

MR. KRESS: I think you're probably right.

MR. POWERS: I know you were linearizing it to make it easier, but I don't think you can linearize it that way. I think you should have done an asymptotic expansion.

MR. KRESS: As part of that resumption, we'll now have an announcement from Jason.

MR. SCHAPEROW: Okay. Well, I'm kind of curious as to how far you got.

MR. KRESS: Well, we're down to outgoing metals.

MR. SCHAPEROW: I grabbed somebody else with more experience and expertise in this area, Charlie Tinkler, and we discussed it a little bit. He made two very good points.

One was, when we get to the low volatiles, what do we do, because we've broken it up into a bunch of groups now? We're starting to go into more -- we've broken plutonium out, for example, in a separate

group.

MR. KRESS: I think we're going to throw up our hands when we get to low volatiles.

MR. SCHAPEROW: Well, the other point was that, I guess we kind of talked about, was the usefulness of 100 percent MOX for a table for that. If we did something like that, we would need a clear path to get to a different percentage of MOX.

MR. KRESS: You ratio it by the amount of MOX that's in the core.

MR. SCHAPEROW: Well, if the panel's willing to agree on that and write it down on one of the tables, fine. It's just that it's not obvious to me how you go from 100 percent MOX core to a 40 percent MOX core, but maybe that's me actually.

MR. KRESS: You take the MOX table and you modify it by .4 and add to it the LEU table that we had before and modify it by .6.

MR. SCHAPEROW: We don't really have -- which LEU table.

MR. KRESS: The L-1465.

MR. SCHAPEROW: I'm not sure we can -- write that down. That's fine. If that's what you want to do, that's fine.

MR. BOYACK: We can do anything we want?

MR. SCHAPEROW: We can do 100 percent MOX fuel table if we write down how to multiply what to get to the 20 percent MOX table.

MR. KRESS: If we tell how to use it.

MR. SCHAPEROW: It's just that nobody is going to build a core with 100 percent MOX fuel. I mean nobody's going to put that in a core.

MR. KRESS: Yes, but it's more illustrative, in my mind, what the differences are between a MOX core and a non-MOX core, even though people know you're not going to have a full MOX core. It's illustrative of what the differences are.

MR. POWERS: I mean you could look at it as an assembly, an average assembly.

MR. KRESS: That's the way you could view it.

MR. SCHAPEROW: We're not objecting to that. All we're saying is we'd like to have a clear path to get to a 40 percent MOX table. It sounds like it's fairly clear in your mind what to do, but I'm not there yet, and Charlie didn't understand either.

MR. POWERS: He's just saying a simple weighting.

MR. SCHAPEROW: If you do a simple weighting and the rest of the panel says that's fine,

then that's fine, because then I can use a 40 percent MOX table or a 60 percent MOX table, or whatever somebody might do.

MR. BOYACK: Let me just get a reading from everybody. A reading? Any comment? The suggestion is that we do this for MOX I guess assembly is what was said. That's what I put down. The idea was that downstream somebody would weight on an assembly fractional basis the MOX and the LEU. Now for high-burnup fuel elements, you could do that. Somebody's going to have to go back and redo or accept 1465 values, which the panel would redo, if they were --

MR. SCHAPEROW: It sounds like we're moving away from that anyway.

MR. BOYACK: Well, what I'm saying is that the high-burnup fuel source term, in effect, has those changes integrated into it. That's for high-burnup fuel. For the regular burnup fuel you don't have anything that reflects that change now. It's just what NUREG 1465 would cover.

MR. LEAVER: Do you mean for LEU fuel it's not high-burnup? But I think the industry is -- I mean, you know, anybody that does alternate source term is probably going to do high-burnup from now on.

I mean everybody doing high-burnup.

MR. BOYACK: So the question is, and this is just a question for the panel -- they've left it up to us, right, as long as we provide a clear pathway?

MR. SCHAPEROW: Exactly.

MR. POWERS: The trouble is I think Jason and I think a little bit -- because it's not clear to me what the pathway is. If I have a loading pattern in which the center of the core is uranium fuel and then I have a checkered pattern, checkerboard pattern out there, I can do this 40/60 split provided the numbers have been developed anticipating a 40/60 split.

Because my concern is this: that when I do the in-vessel release and I damage like 50 percent of the core, that's predominantly no more uranium fuel. Then when I degrade the peripheral region, which has a mixture of MOX and whatnot in it, the fraction is just different out there. So I have to be very careful about how I formulate the numbers.

MR. NESBITT: Just assume that the MOX and the uranium are equally dispersed in the core.

MR. POWERS: Just have a complete checkerboard?

MR. NESBITT: Yes. It's not going to make

that much difference.

MR. POWERS: For my numbers it will make almost zip difference for the major releases because I'm not getting that big of change.

MR. KRESS: We do have to make some decision on how much of each of those we're going to participate in now, like we did before. I think the source term represents a substantial core melt. We have to define what that is. I think Jason said it was 70 percent, didn't he?

MR. SCHAPEROW: That's what the panel proposed as the amount prior to lower head failure, because --

MR. LEAVER: I don't think that number is stated anywhere, but we --

MR. KRESS: Yes, it's stated.

MR. LEAVER: -- we backed it out of the idea that, if you get about 35 percent of the iodine release keeping roughly, retaining a factor of two in the RCS, that you must have gotten significant thermal damage to about 70 percent of the core.

MR. KRESS: I think we ought to make that an explicit observation because either that or say do it for a full core melt and say, whatever -- do the same thing as we're doing for the MOX, say whatever

fraction of the core you think melts, you adjust these to that.

MR. LEAVER: NUREG 1465 has always been sort of --

MR. KRESS: I think we'd better put the fraction in because the other one has the fraction in.

MR. LEAVER: It did, and also I think it's consistent with the notion of 1465. We consider a spectrum of sequences, but when we talk about phenomena, we sort of go back to a low-pressure sequence. In the case of the high-burnup, we assumed that we had roughly a third of the core high-burnup, which is kind of in a generic sense.

It seems to me it's consistent if we say, okay, what we're looking at here is a mixed oxide situation which at the present time in the United States we can be representative or typical if we assume that about half of the core, and it wouldn't be any more than that, are MOX assemblies and we'll just intersperse them. I think that's a good notion, rather than try and -- what are you shaking your head for?

MR. LYMAN: The public and the licensee are here, and what you're talking about is just making -- we're just talking about it, and you're providing

intermediate steps so that people can read the report and understand what you're talking about. You're talking about mixing an intermediate step in some obscure way, so that no one is ever going to understand what you're talking about.

MR. LEAVER: What's obscure about half the assemblies being MOX and half being UO2?

MR. LYMAN: Because some of the assumptions you are making are a little bit obscure as far as our relationship --

MR. NESBITT: Look at what he was trying to do. It would have been a real simple calculation if you were just --

MR. KRESS: It was a simple calculation. It's just a matter of keeping track of where everything was.

MR. NESBITT: And if you just had to consider the MOX core for that, it would have been very straightforward.

MR. KRESS: Yes, so my algorithm would be let's focus on strictly MOX systems and assume -- go ahead and put the 70 percent in, and make that very explicit that we're only talking about each assembly of MOX, only about 70 percent of it participates in this melt, because that's in the old table, and it's

going to use the two tables together.

Then just say this MOX fuel undergoes the core transient just like the LEU would, and use a fission product release relationship that's in our head or something and say what fraction of release you get from that MOX fuel only, and then that's the number we put up there. Then if somebody wants to use it, they have to go in and say, now how much MOX fuel do I have, and how much LEU do I have, and conglomerate this table with the high-burnup when we dealt with that before, just simply by weighting the fractions, which assumes a homogeneous distribution and that each part of the core undergoes a similar type of thermal transient.

That I think would be highly transparent to most everybody. The only nontransparent in there is we're only assuming 70 percent of it participates. We can make that very explicit.

MR. GIESEKE: I can see that and I can go along with that. It's important to point out that there's an assumption made in doing that, and the assumption is that the mechanics, if you want to call it that, of the fuel damage is the same regardless of the amount of MOX fuel in your core, and it's the same for both kinds of fuel, the MOX and the --

MR. KRESS: That's kind of consistent with the design basis accident source terms anyway.

MR. GIESEKE: Okay, but I think it may be important to write that down because that is implicit if you go that way, to just state that the assumption is the same, I mean the assumption regarding, I guess progression and damage are the same for MOX --

MR. LEAVER: So if we did this and just tried to come up with a source term for a MOX assembly, would we also be saying in the same breath that this is applicable for up to half the core being MOX and nothing beyond it?

MR. KRESS: No. No, that's not necessary.

MR. LEAVER: Not necessary?

MR. KRESS: Because I don't think it makes that much difference.

MR. GIESEKE: Or you could say what I just said, and it doesn't make any difference, I think.

MR. KRESS: I think the more MOX you've got in there, the more accurate it is, frankly. It's the other way around. The more MOX you've got in there, the more homogeneous the core is, so the more good our assumptions are.

MR. BOYACK: We've got to have a decision and go forward here, and it's really not mine to make.

So I'm going to just quickly poll the panel to see whether the majority of it says go one way or the another.

Tom, full MOX, right, with a condition where you're listing that?

MR. KRESS: Yes, I say go with the MOX.

MR. CLEMENT: The way of arranging, so the problem is the special distribution. If your MOX assemblies and your LEU assemblies are separated, you should just arrange by the fractions, you will have some bias. In my opinion, it depends on what you want to do with these tables, which degree of accuracy you want to attain.

We were discussing this morning about 1.3 or 1.4 halos and things like that. If you want to go down to this degree of accuracy, this is a difficulty. In my opinion, when you look at what is a source term at the end of the day, it's not worthwhile to discuss one halo experience. So it depends on what you want to do with these tables, because if you want to have a deterministic and real fuel average, I mean you have to make a mechanistic goal much more than what we have up to now.

So I am wondering whether we are not discussing the details that are far away from what

we are able to do.

MR. KRESS: I think we are, and I think that's one reason I want to go to just the MOX.

MR. CLEMENT: That's the way of our approach for the evaluation of source term, where we are systematically introducing some conservatism, so that this conservatism -- we're always asked to cope with such situations as we are debating now. When you have enough conservatism, you can just make the weighting that you propose without any trouble.

MR. KRESS: I think a distributed MOX core that's other than homogeneous would give you a more source term than what we're using the other way. So I think we're biasing it in a direction that's conservative, I think, if we just use the full MOX.

MR. LEAVER: I guess I could go either way because I don't think it's going to make much difference, frankly, in the number. But I'm uncomfortable with the notion that somehow we're coming up with numbers that could be applied to a core that's a significant majority of MOX assemblies, because I don't -- that makes me more uncomfortable than thinking of it as a majority of LEUs.

MR. BOYACK: That is this point made a little earlier that we have less insight into this

whole area of MOX fuel. Now you're asking the panel members to make a larger extrapolation because they don't have this information for a full MOX core. Is that --

MR. LEAVER: I guess another way of saying it is, with a core that's a majority of LEU assemblies, if I'm wrong a bit on the MOX, then that makes that effect less significant.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: Sure.

MR. KRESS: Sure what?

MR. GIESEKE: I'll go with the MOX, just look at the MOX. I think that makes the data more understandable and more transparent in the long run, because then you can build it up any way you want.

I think there is some concern that -- so I'm voting with Tom basically, but there is some concern I have whether it's okay just to state the assumption that core degradation is the same in either case. Then I think it would cover a wide range.

The MOX doesn't affect -- the MOX degradation mechanics is the same as LEU.

MR. BOYACK: You may have held forth already, Dana, but let me -- so what I've heard is two MOX, one I can go either way.

MR. CLEMENT: For me, it doesn't matter so much.

MR. BOYACK: It really doesn't; I understand that.

MR. CLEMENT: Yes, because you have seen for high-burnup fuel proposed values that were in general higher, in general, than this panel. That means they introduce some, let's say, some more conservatism. Also, for the separation in different phases, in our approach we don't think it's worthwhile for this source term to go into so much detail.

MR. BOYACK: Right.

MR. CLEMENT: So that I would say, whatever the solution you propose that is concentrating on MOX, given our approach, I think I will get the same values, except some exceptions.

MR. BOYACK: It's almost an abstain, yes. Okay.

Dana?

MR. POWERS: I'm very sympathetic with Dave Leaver's view, from I don't understand much to I don't understand anything.

(Laughter.)

On the other hand, maybe the purpose is to say, look, here's the best guess. Go out and get some

experimental data, and at least we know what we're refining. That could be a legitimate value. You would at least know what you were looking for. Quite frankly, I think that's the biggest purpose of this exercise, is really to define what you don't know and then set about going after that.

So I guess I could do either one of them.

MR. BOYACK: Okay. All right, so basically all I heard was two MOX and two either one of them. I think that's what I --

MR. KRESS: That's four for MOX, right?

MR. BOYACK: Well, it's close enough.

(Laughter.)

MR. BOYACK: Okay, so the approach, now I want down the information, though, so that it is clearly characterized. So this is a full MOX core?

MR. KRESS: No, assume the MOX is distributed uniformly.

MR. BOYACK: Okay.

MR. KRESS: So that all parts of the core undergo a similar thermal transient, those parts that participate.

MR. GIESEKE: No, I don't agree with that. There's outer and inner. It's the same whether it's MOX or LEU.

MR. KRESS: That's what I mean.

MR. GIESEKE: Yes.

MR. BOYACK: That's what he meant, yes.

MR. GIESEKE: But not all parts of the core --

MR. LEAVER: LEU and MOX undergo the same thermal transient, yes.

MR. KRESS: So we don't have to differentiate. Then I would say we do have to explicitly include the fraction of each of these that we think undergoes the definition of a substantial melt, and I would be consistent and use the 70 percent, 70 percent of each participates in the melt.

MR. GIESEKE: The numbers that we're going to put in our tables assume 100 percent MOX.

MR. KRESS: The table is going to have 100 percent MOX in it.

MR. SCHAPEROW: This is how you come up with the different fractions of MOX.

MR. KRESS: Yes.

MR. GIESEKE: It is more than just a MOX assembly because you have to deal with the retention. I mean you're assuming a full core release.

MR. LEAVER: Yes, but I'm not assuming a full core of MOX. I'm estimating the release

characteristics for the MOX assemblies, period, okay. And this is in a core.

MR. GIESEKE: It's got to be in a full core because of the losses and the effect of concentrations on losses. You don't want to say it's --

MR. LEAVER: I understand. It's a full core accident, yes.

MR. GIESEKE: A full core accident.

MR. LEAVER: It's a full core accident, right.

MR. GIESEKE: And it's a full MOX core accident, the numbers we're going to give. You're going to take releases and multiply it by the number of assemblies, and that's the --

MR. LEAVER: It's a release fraction.

MR. GIESEKE: It's a fraction.

MR. KRESS: When you get to considering deposition and things like that, that might be in your mind how much because --

MR. GIESEKE: That's what I'm saying.

MR. KRESS: But I think we're going to just throw a number in there, like half.

MR. BOYACK: I don't understand, I've got to tell you. Assume MOX is distributed uniformly.

Release fractions stated apply no matter what the fractional split between LEU and MOX. Somehow that doesn't connect with me.

Dana made the statement that this is a 50/50 core, and I sort of get the same thing.

MR. KRESS: What I'm going to do is take a MOX assembly; I'm going to run it through a temperature transient for a core melt that represents temperature transients of severe accidents. From that full MOX assembly, I'm going to estimate the fractional release that comes out for each of these things, and we'll multiple each of those by 70 percent. That's the number I'm going to put in.

MR. BOYACK: Okay, MOX assembly temperature transient for a core melt. Then you're going to --

MR. KRESS: I'm going to use some sort of fission product release relationship and get the fission product, the releases of all the fission products from that temperature transient for the full assembly, but then I'm going to multiply each of them by 70 percent. That's what gets released from the core, the fractional release from the core.

MR. GIESEKE: Seventy percent is only applicable at the end of the early in-vessel. It's

not applicable at the gap early in-vessel --

MR. KRESS: I'm assuming 30 percent of the fuel is residual fuel that doesn't participate in this release. I think that's consistent with what we assumed for the other.

MR. GIESEKE: At the end of the early in-vessel.

MR. KRESS: Yes, but then it might --

MR. GIESEKE: Not at the beginning of the early in-vessel?

MR. KRESS: Then it might do something else.

MR. GIESEKE: I'm saying that's only at that one point in time. It's not true at the junction between gap and early in-vessel because that's a different definition. That's the matching of the curves and such, and that might be 20 percent; it might be 40 percent; it might be, you know, some other number of the amount of core that's been involved at that point. But when you get to the end of the early in-vessel, you're saying 70 percent, is all I'm saying. The 70 percent --

MR. KRESS: All you're worrying about is the timing. I think the timing is almost independent of all this.

MR. BOYACK: What is the nature of this, "assume MOX is distributed uniformly."?

MR. KRESS: I am assuming that it doesn't matter where it is in the core. I am assuming that equal amounts of MOX and LEU are everywhere in the core. It's more that the core is uniform in terms of how much LEU and how much MOX is located where.

MR. LEAVER: What he doesn't want to do is have to worry about the fact that MOX is out on the outside of the core.

MR. KRESS: And LEU is in the middle.

MR. LEAVER: Yes. He doesn't want to worry about that.

MR. LEAVER: No, because when I do then make this assumption, I think I bias things on the high side of fission product release.

MR. GIESEKE: I don't think you want to say equal amounts. That assumes 50/50.

MR. KRESS: That's right, you don't want to say equal amounts.

MR. GIESEKE: Homogeneously distributed? You want to say homogeneous?

MR. KRESS: Uniformly distributed is what I wanted to say.

MR. BOYACK: All this sounds to me to be

the same thing. I am just not quite sure. Tell me, if you built that core, what are you going to do?

MR. POWERS: My suspicion is that what Tom is saying is that, when you go to apply this, assume that MOX is uniformly distributed. Multiply the results it gives you times that fraction that's MOX. Multiply the results from a different table times that fraction that is LEU.

MR. KRESS: That is exactly what I am saying. That is exactly what I mean with that.

MR. BOYACK: Okay, would you say that one more time, and I'll try to get it down?

MR. LEAVER: You want to say, assume MOX assemblies are distributed uniformly throughout the core.

MR. KRESS: That's good.

MR. POWERS: I would predicate it by saying, when you go to apply the results of these tables.

MR. BOYACK: Okay, now that's where I'm at. The application --

MR. POWERS: When you go to apply these results, the results of these tables, define the fraction of MOX fuel in the core to be "f."

MR. KRESS: No, no, just define it to be

whatever you want it to be.

MR. POWERS: "f."

MR. KRESS: What?

MR. POWERS: "f."

MR. KRESS: No, no, I meant define it to be whatever it is.

MR. POWERS: It's "f."

(Laughter.)

MR. KRESS: Oh, you said "f." I thought you said, "High."

MR. POWERS: No, "f."

MR. KRESS: "f"? Okay, I'm sorry.

(Laughter.)

MR. POWERS: It is the sixth letter in the alphabet.

MR. KRESS: Okay.

MR. POWERS: And preferably a lowercase "f."

MR. BOYACK: Okay, now go on.

MR. POWERS: Okay. Define the fraction that is urania fuel, conventional fuel, to be one minus "f."

MR. BOYACK: Oh, great.

(Laughter.)

MR. POWERS: Multiple the results in these

tables for MOX fuel by "f." Add that to the results for the appropriate conventional fuel multiplied by one minus "f."

MR. KRESS: Very good. That's exactly what I had in mind.

And that's highly transparent, right?

MR. LYMAN: And if the licensee thinks that's too conservative and they want the MOX differently --

MR. KRESS: Then they can justify how to do it.

MR. POWERS: They can also do that.

MR. SCHAPEROW: The only problem I have with that is that I don't have the results for conventional fuel.

MR. KRESS: Yes, we do. It's that other table.

MR. SCHAPEROW: Can you show me that? I don't know which table you're talking about. We have a lot of tables.

MR. KRESS: It is the high-burnup PWR table that we just developed yesterday and the day before.

MR. POWERS: Not high-burnup.

MR. SCHAPEROW: Tell me which table it is.

That's all I ask.

MR. POWERS: It's the appropriate one.

MR. KRESS: It's the appropriate one. It's the 1465 adjusted for Dana's problem.

MR. POWERS: For high-burnup fuel, the values are those in the tables generated by the panel. What are they for conventional?

MR. KRESS: I don't think any choice but to go back to 1465, although I think they're wrong.

MR. POWERS: No, I mean, it seems to me that you did the high-burnup fuel, you had a two-thirds mixture of conventional fuel and a one-third mixture of fuel up around --

MR. KRESS: You have an algorithm to adjust that back. There is an algorithm we could adjust that table back to what we --

MR. POWERS: There is an algorithm that we could, except we qualitatively changed things.

MR. GIESEKE: You can never go back to 1465.

MR. POWERS: Yes, stay away from 1465 --

MR. GIESEKE: Because we've said there are changes that have occurred in the meantime, and we even cite in there that a new view of the releases at this point in time, as compared with what had been put

in 1465, and that was kind of the basis which was then adjusted for high-burnup. So I'm not sure there's any way to get back anymore.

MR. KRESS: Let me ask you something. If we use 1465, would it be conservative --

MR. POWERS: No.

MR. KRESS: -- except for tellurium?

MR. POWERS: No, because we jacked up some releases on some of the low volatiles a lot. In the face of quantitative release of cesium and iodine for the worst two hours, no, it doesn't make any difference at all.

We've got a comment back here.

MR. NESBITT: If the licensee who has a core composed of all LEU fuel, it's not high-burnup, can use 1465 --

MR. KRESS: Why can't they use it here?

MR. NESBITT: -- then we can use 1465 for the portion of our core that's LEU fuel if it's not all high-burnup. But if we go to high-burnup fuel, we'll use whatever you all came up with.

MR. KRESS: That's my feeling. Even though we think 1465 is wrong, I think in regulatory space you have to use it, and it won't make much difference. It's a way to go that gets us out of this

conundrum.

MR. POWERS: Let me ask you a question. You keep talking about 70 percent core damage. What fraction of that core melts and penetrates the lower head?

MR. KRESS: I'm assuming all --

MR. LEAVER: What did you say, Tom?

MR. KRESS: I'm assuming all --

MR. LEAVER: I don't think so.

MR. POWERS: See, I was assuming about 50 percent of the core. I mean I think I can track back on a lot of work the NRC has done and come to that conclusion, that about 50 percent of the core comes down --

MR. KRESS: This will impact on what you do for ex-vessel and late in-vessel, I think.

MR. POWERS: Then there is some 20 percent that's damaged, thermally-insulted, but it hasn't melted. It hasn't melted in the core now.

MR. KRESS: Well, whatever those things turn out to be, and I'm not too fixed on it yet, I think we ought to make it explicitly clear what we're assuming and talk about the ex-vessel releases and the late vessel, what substances go into them, partially for design basis accidents, I'd probably just say all

of it goes out. All of the full 70 percent goes out to participate in MCCI, and there's 30 percent left in the core to do whatever it's going to do.

MR. POWERS: Do I have to do that or is that part of the variability in the results?

MR. KRESS: It's part of the variability in the results. The 30 percent that's left, my assumption would be that it doesn't do anything, because I think that's the assumption we've been making previously. It's just left behind, and that part of the core doesn't participate in any way in any of it.

MR. LEAVER: Are you saying it's intact?

MR. KRESS: It's left in there and doesn't raise fission --

MR. LEAVER: If that's true, then I don't see how you are going to be able to melt 70 percent of the core. I mean, you may melt -- even Dana's 50 percent seems to me to be high. I think it would be more like 30, 35 percent.

MR. KRESS: That's pessimistic. This is the design basis phase. I think if you looked at all the severe accidents --

MR. LEAVER: Early in-vessel release is design basis, but once you go beyond that, it's not.

No, it's not.

MR. KRESS: Yes, sure, it is.

MR. LEAVER: No, it's not.

MR. KRESS: None of this is based on a single accident sequence or specified accident sequences. It's all an agglomeration of accident sequences. In all design basis --

MR. LEAVER: But the word "design basis" means something very specific, and the staff has stated that that portion of 1465 that's up to the early in-vessel is to be used for the radiological design basis accident calculation.

MR. KRESS: Sure.

MR. LEAVER: And no one has ever said that the ex-vessel release or the late in-vessel release is design basis.

MR. KRESS: No, I didn't intend for it to be that. If you're going to use it for anything, you would use it in a design basis sense.

MR. LEAVER: But no one's contemplating using it in a design basis sense. The only thing people are contemplating using is the release up to the early in-vessel.

MR. KRESS: If I had my way, we would get regulations that had to do with late containment

failure and land contamination, and we would use it in a design basis --

MR. LEAVER: Well, if and when, yes.

MR. BOYACK: Is this here this 70 percent? Is that where you're taking issue?

MR. LEAVER: I was taking issue with the assumption that 70 percent of the fuel is molten and the other 30 percent is pristine in the vessel. I don't think that's realistic. I don't see how that could possibly happen. I don't think it matters, but I think it's certainly reasonable to think of -- I like Dana's word of "thermal assault" or core damage, if you will. Seventy percent of the core is badly damaged. Some portion of that is truly molten, and some portion is shards, debris, in a pile somewhere.

MR. BOYACK: Can you live with that, Tom? Seventy percent of the core is badly damaged?

MR. KRESS: I don't care because the question is what participates in MCCI, but that's not part of the design basis generally.

MR. BOYACK: Right.

MR. KRESS: So I don't care what they do with it.

MR. POWERS: Well, the next question I have is, why do I have to assume that the 30 percent

that's left in the fuel in pristine suffers nothing in the subsequent phases of the accident?

MR. LEAVER: I think that's a good question. I think that it's hard to argue that the late in-vessel, that that would heat up and undergo further damage.

MR. KRESS: Well, one asks yourself, in choosing design basis events, does one factor in everything or does one try to have a risk and frequency in mind in choosing it, and one concept is that air ingression accidents that might influence -- if that stuff's left in there and the bottom of the head's off, you're either going to cool it and it's going to stop releasing or you're going to get air ingression in there, and you're going to have an air zirc reaction that's going to heat it up and drive up everything off.

The question is, do I know enough about the accident to make some argument that it ought to be considered in design basis space, or is its frequency low enough that I can relegate it to severe accident space and say, "I don't have to make the designer or the licensee deal with it, the design basis space? It's a judgment call usually, and it is a question of, is this going to make -- is my system robust enough

with what I have or do I have to make it more robust?

You know, I could anticipate all sorts of things. Like I would add in what fraction of this 70 percent that means some fraction of the iodine didn't get released to the side. I might say all the iodine because the minute it hits core-concrete interaction, it's going to come out real fast. So it might as well be part of the in-vessel release except for it doesn't get plated out.

So you can make all sorts of assumptions, and I think these assumptions here are just consistent with what the old design basis source term is.

MR. BOYACK: Let's take a quick look at what I have down under the approach and see if there's anything else that needs to be done.

Assume MOX assemblies are distributed uniformly throughout the core. Whether MOX or LEU assembly, it undergoes the same thermal transient. MOX assembly passes through a temperature transient that represents the core melt. Core disruption -- is that better? That creates -- that damages.

Estimate fission product releases for a full --

MR. LEAVER: Fuel.

MR. BOYACK: Pardon me?

MR. LEAVER: Probably the word "fuel" would be better than "core."

MR. BOYACK: Estimate fission product releases for a full MOX assembly. At the end of the early in-vessel --

MR. LEAVER: Release fractions.

MR. BOYACK: -- release fractions. Okay, thank you.

MR. LEAVER: You're normalizing this. So it doesn't matter how many assemblies.

MR. GIESEKE: Isn't the third one of those bullets covered by the second one?

MR. BOYACK: I didn't think so. I thought there was something explicit that I had asked a question about, and it says this is what we're doing. So somewhere I think we -- at the end of the early in-vessel phrase, release 70 percent of the core's badly damaged, some molten and some otherwise damaged. Thirty percent of the core doesn't participate in the release through the end of this phase.

Anything else?

MR. GIESEKE: I still think the third one is covered under the second one. Otherwise --

MR. BOYACK: Does it matter?

MR. GIESEKE: -- I would like for you to

add to the third one that says, "LEU assemblies pass through a temperature transient that damages the fuel."

MR. BOYACK: So, okay, you want me to also put the --

MR. GIESEKE: Well, if you're going to put one, you have to put the other because --

MR. LEAVER: We're just estimating MOX.

MR. GIESEKE: Yes, but what happens --

MR. BOYACK: But the release fractions that we're presenting are for a full MOX assembly. That's all they are. They aren't for the LEU. That was the point.

MR. GIESEKE: Oh.

MR. BOYACK: So the fission product release fractions developed are for a full MOX assembly.

MR. KRESS: I don't know what the "full" means. It's just for a MOX assembly. It's not a single one.

MR. LEAVER: Get rid of the word "a" and put "MOX assemblies."

MR. BOYACK: Assembly can be one fuel -- it's the same thing.

MR. LEAVER: Right, for MOX fuel.

MR. BOYACK: Yes, for MOX fuel.

Anything else that's needed post to end of early in-vessel?

MR. NOURBAKHSH: I have a comment. Is that definition of 1465 source term, early release by definition is up to the vessel failure. So that assumption is 70 percent is implicitly, is one of your conclusions, is not explicitly by definition of LEU source early release.

See, you bring the end time of the vessel failure. So it is an implicit kind of conclusion that you make that 70 percent should damage before the vessel failure. You saw that explicit definition of that source.

MR. KRESS: But making it explicit makes that clear. I mean, it's the same practical -- you get the same result. You get the same result. You might want to clarify that and say the 70 percent is a result of how much it takes to melt through the vessel. That might be a clarification. In practice, it's the same thing. It just assumes the same.

Terminate the in-vessel phase.

MR. BOYACK: Okay? Are we all clear?

MR. POWERS: I'm still unclear what happens after the vessel has been breached.

MR. KRESS: You no longer have steam going out. You've probably got some pretty hot fuel left in there, and it didn't get down yet because you need 70 percent of it to get down. That fuel's, I think it --

MR. LEAVER: Are you asking what happens to the 30 percent that's up in the vessel?

MR. POWERS: Yes.

MR. KRESS: I think when the bottom -- when the hole gets in the vessels, whatever kind of hole you get, you're going to expel a lot of the 70 percent because I think most of it is molten. Most of it is going to go down to the core concrete.

MR. POWERS: Well, I understand that you think that most of it is molten, but I certainly don't think so.

MR. KRESS: Do you think a substantial fraction of it is crusted and --

MR. POWERS: I think a substantial fraction of it is fuel rods that are oxidized to some extent, but not been hot, grading out to when you get to the complete perimeter they're in pretty good shape. They may have ruptured their clad, but otherwise not much has happened to them by that time.

MR. KRESS: That's the 30 percent?

MR. LEAVER: No, that's a portion of the

70 percent.

MR. POWERS: If I take a look radially, starting at the center of the core, and I go out about 70 percent of the distance, that's gone. Okay. It's slumped --

MR. KRESS: It's 70 percent of the --

MR. POWERS: Seventy percent of the radial distance.

MR. KRESS: PI-r squared is --

MR. POWERS: Point seven times .7 is .49. So it's about half the volume of the core that has slumped down and hit the vessel head. There is an uncertain range on that I will agree to; it depends on the accident. It is probably higher in this particular accident than others, but it's a good number and I think it meshes with other studies that the NRC has done.

From that .7 to the perimeter, the degradation goes from rod stubs all the way up to almost pristine fuel. I suspect that it's broken its clad, has a hole in its clad, but by the time you get to that outer row of assemblies, it's pretty much intact fuel at the time of vessel rupture. Okay?

Now I want to know what happens after that. Does Saran Wrap get put over this thing and

nothing happens?

MR. KRESS: My assumption is not that you've got 70 percent of the radius, but you go out far enough that you capture 70 percent of the fuel. That's what it takes to melt the vessel, and the 30 of the fuel is around the periphery. Pretty much like you say, it hasn't melted yet. It's probably damaged, and it may have even released some of its, well, evolved to fission products, but it's there. You have a hole in the vessel. You've probably gotten rid of all the steam and water by now, and whatever melt, this hole goes down to the core concrete, and now you've got this stuff sitting around the edge which is undergoing decay heatup, but doesn't have much of a way to cool itself except by radiation.

I don't know what happens to it. I think it might continue to heat up and continue to melt and fall down in what residual lower head there is and continue to release its fission products.

MR. BOYACK: I am going to ask a question here. I hope you can satisfy my curiosity.

An hour and a half ago, we were marching through tables at a pretty rapid pace without these definitions. It appears to me that, although we're talking about MOX specifically, and trying to come

through this prescription, we were doing without this prescription before, right or wrong. Was it wrong? Because now we're into a level of defining the scenario that could have been applicable to what we were doing before also. No matter what the fuel mix is, we could have had this definition.

So I'm curious. We were going through the process before, coming up with answers. Now we're having a rather protracted scenario description. Is it necessary to go forward? And if so, why is it different from what we were doing an hour and a half ago?

MR. KRESS: That's exactly what I was doing.

MR. BOYACK: So the specificity is showing that others were doing different things?

MR. KRESS: Probably. I don't know what the others were doing, but that's exactly what I was doing.

MR. POWERS: I was thinking about 50 percent core melt, some damage, and that broke through the vessel head. Then after that, I had the rest of the core coming down over a two-hour period.

MR. BOYACK: Okay.

MR. POWERS: And that was augmenting the

core-concrete interaction. For 10 hours I was running gas up through the piping system revaporizing stuff.

MR. KRESS: That is probably more realistic than what's going on in what I was saying.

MR. BOYACK: Then what I would like to do, then, is sort of see if we can't just get down. So this is a 50 percent of the volume of the core, right? What you had was 50 percent of the volume of the core? Fifty percent of the core doesn't participate in release through this --

MR. POWERS: Some fraction of it --

MR. BOYACK: Okay, so why is it not within this 50 percent?

MR. POWERS: You've got 50 percent of the core that penetrates the vessel.

MR. BOYACK: This is the 50 percent that's going to come down? Okay.

MR. POWERS: The residual part of it --

MR. BOYACK: And will be released immediately upon failure of the lower vessel, right?

MR. POWERS: Uh-hum.

MR. BOYACK: The remaining percent of the core, 50 percent of the core remaining in the vessel, some portion is also damaged.

Now at this point I think what you said

was, for the next phase, so for the ex-vessel phase, the 50 percent discharged, released, in the core concrete interaction, and then what did you say about the rest of the --

MR. POWERS: Understand that some portion has been damaged, but during the ex-vessel phase 100 percent of the core eventually ends up on the floor.

MR. BOYACK: Okay. Then you had a last statement, which was to do with the gas flows, and this was the late phase, right? That's called the late in-vessel phase? What do we call it?

MR. POWERS: It's late in-vessel.

MR. BOYACK: Just let me go with this for a moment.

MR. POWERS: This is the 10-hour period where you're taking the material off that you put on the piping system, you're revaporizing fractions of it.

MR. BOYACK: Deposited material, okay. Is revaporized?

MR. POWERS: Uh-hum.

MR. GIESEKE: Why don't we call that -- oh, I don't know what to call it -- RCS internals, as in the piping system?

MR. POWERS: Yes, you can say RCS. A lot

of it's on the upper core structures.

MR. GIESEKE: That makes it a little bit more applicable to a TWR.

MR. BOYACK: Okay, Tom, can you live with that?

MR. KRESS: Well, I can live with it except your "some portion" in the bullet halfway up. I don't know what that portion is.

MR. BOYACK: Oh, right here?

MR. KRESS: Because what I think you're saying is that that's going to be added into the early in-vessel release.

MR. POWERS: That's right.

MR. KRESS: So I need to know what that portion is, and I think your intent was to make it 20 percent or something like that?

MR. POWERS: In fact, what I did was 30 percent of the fuel was involved and it released half of its inventory in the volatile materials.

MR. BOYACK: Okay, I was typing.

MR. KRESS: I said 15 percent, and the other 35 percent was part of the core concrete that went over a longer time.

MR. POWERS: Thirty-five percent probably lost the gap release, but it hadn't released very

much. Then it collapses down into the reactor cavity and it releases everything. It does that over a two-hour period.

MR. BOYACK: So if you were asking, some portion of the 50 percent of the core main vessel was also damaged and participates in the early in-vessel release. Did a number come out of that?

MR. POWERS: What I had said, what I had been doing was saying 30 percent of the core loses on average half of its volatile inventory. So if I look at the core and ask something like the, say, cesium content, how much had come out of the core, not onto the containment, but had come out of the core, it essentially amounted to a 65 percent release fraction of the core as a whole.

MR. BOYACK: Is this 30 percent of the 50?

MR. POWERS: No.

MR. GIESEKE: No. It's of the total core. It's 30 percent of the total core, but it comes out of the 50 percent left standing, so to speak.

MR. BOYACK: Give me some words.

MR. POWERS: Three-fifths of the remaining core releases one-half of its volatile inventories.

MR. KRESS: Yes, I don't know whether one-half --

MR. POWERS: Well, a straight line from 100 percent release to zero release for that part of the diagram.

MR. BOYACK: So is that three-fifths of the main core loses one-half of its volatile inventory? Is that what was said?

MR. POWERS: That's what I heard.

MR. BOYACK: Yes, that's what you heard. You said it so well. Does this do it?

MR. GIESEKE: I have a little bit of a -- well --

MR. BOYACK: Does this put everybody on the same description?

MR. GIESEKE: Well, that's basically where I came up with the 65 percent in the very first place, is this kind of logic. I don't know if we want to -- I guess we could say --

MR. BOYACK: The reason a prescription like this is good is, when it goes in the report, if something changes markedly, then people can adjust accordingly.

MR. GIESEKE: I guess my question is if you want to use something other than -- well, I guess volatile inventory is as good a way as anything.

MR. BOYACK: You're going to get another

chance to change this. As long as we're communicating all right about it --

MR. GIESEKE: Yes, it's close enough.

MR. BOYACK: I can't print this because Jason's got all my disks. We went back again to reprint again. So I don't have a transfer media until tomorrow on this. What I can do is I can just keep it where we can get at it.

So now we go back -- first off, tell me, is it going to affect duration?

MR. GIESEKE: From my perspective, it's build on this. Any numbers I give you are built on this basic assumption anyway.

MR. BOYACK: Okay, for you the answer is no. I'd like you to think about that and whether anybody needs to change the duration input you gave me.

MR. KRESS: I am still bothered about the three-fifths of the remaining core and one-half of its fission products.

MR. POWERS: It's not one-half of its fission products. This is half of the volatile --

MR. GIESEKE: That's important because that's the point I was trying to fuss with, and then I just said, well, as long as he carries the word

"volatile" with it every place he goes, if that's maybe noble gases, you know, if you look at it that way --

MR. KRESS: It still bothers me. I don't know whether one-half -- I understand what Dana is saying, but it seems awfully arbitrary to me.

MR. GIESEKE: It is.

MR. POWERS: Anything we do on this is going to be completely arbitrary.

MR. KRESS: I think you're right, but both the three-fifths and the one-half seem arbitrary to me. I'm convoluting two arbitrary things to get another arbitrary thing, and that's why it's bothering me.

(Laughter.)

MR. BOYACK: The real key is it's arbitrary, but it's very specific.

MR. KRESS: Oh, I agree it's specific, and specificity is very important.

MR. POWERS: I think I'd go through the actual analyses that have been done in the last 10 years and show you that thinking about 50 percent of the core is not a bad, as core melt, it's not a bad -- it fits the definition of 1465. It may not be bounding, but it's a pretty severe situation. I can

find accidents that initially melt a lot less, and I can find a few, not too many, that melt more.

Now among the ones that melt more tend to be these large breaks. So I don't think the 50 percent is all that arbitrary. I think there's a basis for that.

As I say, the 50 percent, all I'm doing is saying, look, the outer ring of assemblies may have ruptured its clad, but it hasn't released -- and lost its gap inventory, but it hasn't done very much now. So since I don't know all the details of release, I will assume that, starting at my .7 radius to my .9 radius, it's essentially linear. Half of linear, I mean it's roughly half. You can do it very exactly, but you're kind of fooling yourself.

MR. BOYACK: So, Tom, are you just feeling that it's a different value? I mean, because we need the prescription to have people doing the same thing. So are you just feeling it's different values or what?

MR. KRESS: This three-fifths of the remaining core losing one-half of its volatile inventory, where does that go? Does it go through the primary system that undergoes plate out or does it go straight into containment, in your mind?

MR. POWERS: It's part of the in-vessel

release.

MR. KRESS: So half of it plates out, or whatever our assumption is on the --

MR. POWERS: I was using basically half plate out.

MR. KRESS: So three-fifths of 50 is 30, and if I added that to the original 50, I would have gotten 80, and 80 percent --

MR. GIESEKE: I am seeing the same thing. I mean, I don't know where you're going to 80 --

MR. KRESS: We used 70 percent before to calculate the release in-vessel.

MR. POWERS: No, it is 50 percent plus half of three-fifths, which is essentially 15. So you come up with -- so if I take --

MR. KRESS: So 15 plus 50; 65 instead of 70? I don't see that that's substantially different. It just gives a better rationale for why you would use 70 or in this case 65. So I'll go along with what he's saying if --

MR. BOYACK: Do I have to do some rewording here?

MR. KRESS: No, no. I think it's --

MR. POWERS: The issue is what you do after the vessel ruptures.

MR. KRESS: That's right, and you were asking me what I did with the 30 percent, and I said, I don't know. This gives you some rationale for what to do with it. It all goes in over a two-hour period to MCCI or something, and it gives you a rationale for dealing with the ex-vessel part of it. From that standpoint, it's a fairly good specification. It doesn't violate my general rule of 70 percent very much. It's 65 instead of 70.

MR. BOYACK: Well, that's good because the uncertainty is much bigger than that, right?

Okay, so are we ready to go on?

MR. POWERS: Yes.

MR. BOYACK: That's the folder file. I'm kind of thinking it would be good if people had a copy of that.

Okay, so the duration we said was all right.

MR. KRESS: Yes, I don't see that any of this change affects my concept of the duration.

MR. BOYACK: Okay, let's go to the noble gases. I'm starting to get tired because I can tell I can't remember who did what, but we'll just start over here. I guess that's how we started last time.

Dave, do you want to change any of the

values based upon now this new definition?

MR. LEAVER: Okay, and this is now all MOX, and we've kind of come up with a some sort of a standard core damage progression model, which I think is very reasonable. It's a badly damaged core, but it's in the representative or typical range, somewhat conservative in terms of the amount of damage, when you look at the probablistically important sequences.

I guess the only thing I would do is I might just lower my 80 percent a bit, maybe make it 70, 75 percent, and then increase the ex-vessel to 20 percent.

MR. BOYACK: Okay, Jim?

MR. GIESEKE: I like it right now.

MR. BOYACK: Okay, Dana?

MR. POWERS: Okay.

MR. BOYACK: That's right. Tom? Here comes the moment.

(Laughter.)

MR. POWERS: He likes them the way they are.

MR. KRESS: We're still on the gap release.

MR. BOYACK: Oh, actually, I thought we went through the whole thing with these others.

MR. KRESS: Did you?

MR. BOYACK: Yes. So you can just go through the whole smear here.

MR. KRESS: My gap release was designed to say 40 percent of the core was high and it was MOX, and the other 60 percent was LEU. Now I'm going to all MOX. My gap release would go up considerably to about .1 if I'm just using all MOX.

I think the inventory in there is -- you know, if you multiply .1 by 40 percent and add that to -- no, that's not right. .6 was a combination of .4 times some number plus .6 times some number. .6 times .05 is .03. So the .6 is .03 divided by .4. So it goes up to about .7 or .75, somewhere around there.

MR. BOYACK: Do you want that 5 there?

MR. KRESS: No. Let's make it 7.

MR. BOYACK: Okay.

MR. KRESS: And that's to reflect what I think the inventory change will be --

MR. BOYACK: Okay, early in-vessel?

MR. KRESS: Early in-vessel?

MR. BOYACK: Yes, you get the whole thing now.

MR. KRESS: Well, I'm going to take .65 for sure because that's the amount that participates

in essential the melt. Now the question is, am I going to release any of these noble gases from that other part of the fuel that didn't? I think surely it gets damaged to the point that it's going to release some noble gases. So I'm going to go back up to the .93 because I think the .07 takes care of --

MR. BOYACK: So everything goes out?

MR. KRESS: Goes to .93, yes.

MR. BOYACK: And the next two go to zero?

MR. KRESS: Yes.

MR. CLEMENT: Don't change .05, but you could add this, we'll say that the margin is reduced. This is the original for the experiments. I don't change the .05.

MR. BOYACK: Okay, so --

MR. KRESS: He's saying that it is always the same.

MR. CLEMENT: Yes, but you can indicate that now margin is reduced.

MR. BOYACK: For the .05?

MR. CLEMENT: Yes. Then for the other, .95, no change, because basically in our studies we consider 100 percent participating to the release. So never mind for the 40 or 60 percent. So no change.

MR. BOYACK: Okay, noble gases are done.

I have to color code or I'll lose track of what I'm doing.

How are you doing, Jim? Are you ready to go on to the halogens?

MR. GIESEKE: Yes. I don't see any reason to change because I kind of made these assumptions when I did it in the first place. They're close enough.

MR. BOYACK: Dana? Anything you want to do with the .325?

MR. POWERS: I put my .325 in there to reflect a belief that the releases of halogens from the fuel are a little more rapid than they would be for conventional fuels. As a consequence, the partial pressure in and immediately above the core region was a bit higher. Consequently, the driving force for fission product halogen condensation on the particulate and structures was a bit higher, and you got less out. The precise numerical value I think is not so important as the indications that, release things more rapidly; you deposit them more rapidly.

MR. BOYACK: In effect, we're just employing the logic for everybody now that you were employing before? That's why your number doesn't need to change?

MR. POWERS: Well, I mean, it would seem like it ought to change because now you're doing a full MOX fuel, but since I think the overall release fractions tend to be driven more by damage progression for these volatile materials than by the details of kinetics, the only place I reflect that as higher kinetics is in the fraction of the deposits. So I kick up the fraction of the deposit, and consequently, I kick up the fraction that subsequently revaporizes in the late in-vessel phase.

MR. BOYACK: Okay, so your ex-vessel number and your late in-vessel?

MR. POWERS: I guess I'd just leave them alone.

MR. BOYACK: Okay. Tom?

MR. KRESS: My number is going to be the same, and I'll tell you why. The .65 fraction that we essentially assumed undergoes the transient, releases all of it, and you get .65, plus you add the gap release, which is basically another .5 or so. I finally just played it out, and it leaves me with .35 running the vessel and .35 on the other right now.

MR. BOYACK: Okay, so those numbers stay the same?

MR. KRESS: Uh-hum.

MR. NESBITT: The gap release phase for Tom is now, I think, inconsistent with that for the noble gases.

MR. KRESS: Oh, yes, I would convert that to 7 also. Thank you very much. You know, once again, that's hardly distinguishable from the 5.

MR. NESBITT: Well, I agree, but --

MR. KRESS: But just to be consistent and to show that I think there's more in the gap inventory because of its MOX fuel.

MR. BOYACK: Any comments now that I need to change? Any different numbers?

MR. CLEMENT: No.

MR. KRESS: Now ex-vessel, this is the 50 percent of the core -- I mean the three-fifths of the core that went down and released half of its content. The other half is going to get released very quickly. So it's that number that. It's coming up to be .15, I think.

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

MR. KRESS: Well, I've got half of the -- I've got .35 plated out. I don't think it all gets released, because once it starts releasing, it releases its heat source. So I'm going to stay with my .2, just to be consistent with the things that were

late in-vessel before.

MR. BOYACK: Okay. This is where we left off. And, Dana, it follows your lot on alkali metals now.

MR. POWERS: Okay, it's 0.05 to begin. I am conceding that the inventory can be a bit higher, but I still think the .05 has enough margin to reflect that, especially when they're limiting the burnup to something around 40. So I just don't see any reason to change that.

MR. BOYACK: Okay. Early in-vessel?

MR. POWERS: Early in-vessel release, I have 0.30.

MR. BOYACK: Okay.

MR. POWERS: Ex-vessel, 0.25.

MR. BOYACK: Okay.

MR. POWERS: And the late in-vessel release, 0.15. Some of this may need a little explanation.

MR. BOYACK: All right.

MR. POWERS: My general belief is the deposition of cesium in retention of the piping system is a little better than for iodine.

MR. BOYACK: And the fractional release is --

MR. POWERS: The fractional releases are about the same, but it strikes me that there is more chemical diversity available to cesium to deposit on the parent piping system, and that it forms more refractory compounds, so the revaporization is less efficient. So that you see some bias down from cesium and iodine here.

MR. BOYACK: Okay, now one of the general statements -- I don't know quite where to shuffle them into the thing here as to which phase. So if there is something that you feel like you want in the report, I need to have the phase or I get lost immediately, and then I loss track of the conversation.

MR. POWERS: I would say down in the late in-vessel, just say more refractory surface species, so the extent of revaporization is less than for iodine.

MR. BOYACK: Okay. All right, Tom?

MR. KRESS: Believe it or not, my numbers are just about like Dana's, but I would put the .07 in there just to be consistent, and the rest of the numbers are about the same. The difference --

MR. BOYACK: The same as?

MR. KRESS: As Dana's. The difference in the total doesn't make any difference substantially,

and I agree with him on -- no, .15. I agree with him it does revaporize as easy.

MR. BOYACK: Bernard?

MR. CLEMENT: .05 for gap releases, and then .65 for releases, the usual.

MR. BOYACK: Dave?

MR. LEAVER: I'd say .05. I think the cesium is, for the same reason as I said on noble gases and iodine, I think it should be a bit higher than what we used for the LEU. So I would say .3 would be a reasonable estimate there, and I'd say .3 for ex-vessel and .1 for late in-vessel. Actually, I would, yes, I would say .15 for late in-vessel.

MR. BOYACK: Jim?

MR. GIESEKE: .05. Looking and comparing with iodine again, and accounting for transport and deposition differences, I get -- I'll round down; I had a little bit more, but I'll round down to .30 and make your averaging easier. I had .3 and .1.

MR. BOYACK: What about research for the alkali metals?

MR. POWERS: What?

MR. BOYACK: Research.

MR. POWERS: Oh, research.

MR. BOYACK: This is for anybody who cares

to hold forth.

MR. POWERS: Well, it seems to me that we do have a couple of data points for the cesium release, and there's still some work to sort out what all the peculiarities of those data points. That effort to sort that out is clearly needed to be done. What you have, then two points make a straight line, and Tom can build his model.

MR. KRESS: That's right. I can do it like that.

MR. POWERS: And you don't have to worry about scattering the data that way.

MR. KRESS: That's right.

MR. POWERS: When you just have two points.

MR. BOYACK: This is VERCORS test?

MR. POWERS: Yes. Don't do any more tests. Just sort out what you've got.

(Laughter.)

MR. KRESS: If we do a third test, I'm in trouble.

MR. POWERS: Yes.

(Laughter.)

MR. KHATIB-RAHBAR: Coming back to the VERCORS tests, the one that was not presented

yesterday were RT 1 comparison to RT 2. This data you have already presented in public. You have a paper that was presented this exactly, this comparison.

MR. KRESS: It's RT 7.

MR. KHATIB-RAHBAR: RT 7 is the one you have in place.

MR. KRESS: That's the one I need because it's got the whole range of fission products in it.

MR. KHATIB-RAHBAR: It was already presented, and I believe that your institute has indicated to NRC that they will providing those data to us.

MR. KRESS: Right, that's fine with me.

MR. KHATIB-RAHBAR: It's just a matter of timing. So we will have access to that.

MR. CLEMENT: We hope it will be soon.

MR. KHATIB-RAHBAR: Soon, yes.

MR. BOYACK: Any other research needs for any of the other phases? Any other research needs in this area that have occurred to you?

MR. KRESS: There was this question of validating the VERCORS data with the real fuel, but after listening to all of it, I'm of the opinion that it's applicable to what they're going to use and we don't have to run this.

MR. CLEMENT: The data will be exactly the characteristics of the tests that has been used because it's important.

MR. BOYACK: Okay, tellurium, right? Did I hear somebody groan?

(Laughter.)

Was that you who sort of let that out?

MR. KRESS: Yes.

MR. BOYACK: Well, you get the first shot at this one as it turns out.

(Laughter.)

I'm ready to stand up for a second.

(Whereupon, the foregoing matter went off the record at 4:05 p.m. and went back on the record at 4:06 p.m.)

MR. KRESS: Tellurium was a tough one for me because if I just look it its volatility, it actually gets released as much as the halogens, but then there's this business that it gets tied up with the metallic elements in the fuel, and then it's not as volatile as you think it is.

My opinion is that eventually this gets tied up in metallics. It eventually gets released anyway from that fuel that undergoes the damage part, and I would put it exactly the same as the halogens,

which is .3. I think we used .35 for the halogens here.

The gap, I think it's probably the same as -- I think it's zero in the gap. I think it's tied somewhere else in the fuel.

MR. BOYACK: So you're referring back to what, the halogens?

MR. KRESS: For the early in-vessel, I'm referring back to now. I forgot when we were talking about gap. I'm still going to use the zero gap release, but for the halogens early in-vessels I would still use the .35.

MR. BOYACK: Okay.

MR. KRESS: I think ex-vessel tellurium is probably less releasable than the -- it's going to get released ex-vessel, so I think the .15 is good there.

Late in-vessel, I think once it gets tied up with on the surfaces, it's not going to get released there. So I'd go down to .1. That's about it, I guess.

MR. CLEMENT: Gap release zero, and then for the early in-vessel, but, in fact, total release, .7.

MR. BOYACK: Dave?

MR. LEAVER: Gap release zero. I guess

early in-vessel, on LEU I think there certainly was a strong feeling that tellurium was -- the 1465 tellurium estimate ought to be increased. There may be some disagreement as to how much, but I think that notion applies here, but I felt that the 30 percent number that a number of people came up with was uncomfortably high. So I was in the range of 10 to 15 percent. So I guess here I would say maybe the upper end of that, .15.

MR. BOYACK: Okay.

MR. LEAVER: And I'll go with .4 ex-vessel and .2 for late in-vessel.

MR. BOYACK: Jim?

MR. GIESEKE: I think it needs to be a little bit there; .005 perhaps.

(Laughter.)

Put a little in there. What's wrong? You don't want to put a little in there? How about .3, .4, and then .2, which are basically the same numbers as we used for the high-burnup.

MR. BOYACK: Dana?

MR. POWERS: Like Jim, I'd like to reflect a little bit in the gap. What I put in is .005. Now with the proviso that I don't understand tellurium at all, here's somewhat how my thinking goes:

We're going to use M5 cladding. That means we've got no tin. To form tin, telluride should be released in the tellurium. It's my impression that the MOX fuel runs a little higher in oxygen potential than conventional fuel because the plutonium nodules have a less capacity to sustain and buffer oxygen that's being liberated by the fission process.

So I suspect that this manifests itself in seeing a little higher releases of molybdenum, a little higher releases of ruthenium than what we're used to. The same phenomenon ought to lead to a little higher releases of tellurium than we're used to.

On the other hand, we're going to have higher concentrations of the reactive forms of tellurium in the flow out of the core. To propose that I have any capacity to doing this integral in my head is to overstate my capacities by a lot.

MR. KRESS: My assumption there was just aerosols plated up --

MR. POWERS: I just didn't want it to reflect the potential of reacting with the upper structures, and you might not see too much movement of this material. So I took the in-vessel release, and this is the release to the containment. You have a

higher release fraction than the fuel itself. But to the containment, I have .1.

MR. KRESS: You're really plating this stuff out?

MR. POWERS: Yes, I plated out a lot of it.

Okay, the ex-vessel release, I had .40. As I said, tellurium is the one thing during melt-concrete interactions I think I understand. I don't understand in-vessel at all, but I understand ex-vessel a lot. So I think it gets released, oh, fairly extensively ex-vessel.

MR. KRESS: I'd like to change my .15 for ex-vessel to .4. I agree with Dana on that and the rest of the people. I don't know how I came up with .15.

MR. POWERS: The reason I'm fairly confident on this is that, when you put a melt on concrete and let it chew away for a while, there's a strong smell of rotten eggs, and that rotten eggs is the gypsum used in the concrete, which is the calcium sulfate being turned into hydrogen sulfide and vaporized out. While sulfur chemistry and tellurium chemistry are sufficiently close that you know they should be volatile, we have done experiments

explicitly looking at tellurium release because of a bet Dick Vogle intemperately made with me on the release of tellurium during melt-concrete interactions. He said it couldn't be released. I, having smelled the rotten eggs, knew that it would be released. So we did the experiment, and we found VANESSA matched exactly the release, and we were getting a slow, steady, and what would be eventually complete release. So I'm fairly confident on that.

Now the late in-vessel release, I get .2. That's because I put a lot of tellurium out of fuel and I put it on the piping system, and then I believe that with reactors that have air filter containments you'll get air in there and you will turn any tellurides into TeO that's volatile and it will come out.

MR. KRESS: Can you add up when you look at where everything is?

MR. POWERS: I hope not.

MR. KRESS: Well, let's see, you've got -- how much have you got?

MR. POWERS: What Tom's asking is, have I vaporized more off the piping system than what I had to vaporize? And I don't think so. No, I released essentially 60 percent from the fuel itself.

MR. KRESS: And 40 percent ex-vessel?

MR. POWERS: Yes.

MR. KRESS: And that's 100 percent failure?

MR. POWERS: Right, and I put 50 percent of the core inventory on the piping system, and I subsequently revaporized a fraction of that. I think my mass balance is okay. Now the quality of the numbers, of course, is not worth one spit.

MR. NOURBAKHSH: All of the tellurium comes from core-concrete interaction?

MR. POWERS: I get about 60 percent release from the fuel during the in-vessel transient, 60 percent of the core inventory; 50 percent of the core inventory, which is five-sixths of that released, deposits on the piping system. Ten percent of the core inventory actually comes in the containment. Subsequently, in the late in-vessel phase, about 40 percent of that deposited material subsequently revaporizes and comes into the containment.

MR. NOURBAKHSH: So all of the releases are overlaid water or --

MR. POWERS: Oh, I see what you're asking. Was there any transient for that degraded fuel?

MR. NOURBAKHSH: Yes.

MR. POWERS: I really didn't --

MR. NOURBAKHSH: You made that assumption such a release of tellurium is there.

MR. POWERS: You're right, and I just didn't correct these numbers to try to horse that out. See, there's a big debate on what happens with the rest of this residual fuel. If you let air come in and interact with that residual fuel, and it's very rapid, it gets so hot that it melts down very quickly, and almost nothing gets released. It doesn't have time to get released before it gets down.

If, on the other hand, you meter in the air, it comes in very slowly, and the clad just melts off, so that you expose fuel to the oxidizing environment, then you get a lot off. I have tried to model that with things like the MELCOR code. As I assumed dictated the results. I mean, if I had real good high flows in there, I could get the stuff to melt, flow out, and never had a chance to release anything. If I slowed down the flow a lot, it had a chance to burn off the clad and release lots. It's so-so good. I mean, your assumptions are dictating things.

And that's why we had hoped that the PHEBUS program would be able to do an air ingression

accident, so that we could have some idea how the competition between things is working. I mean, there doesn't seem to be able way to do this in my capacity to model these things. I just don't know.

MR. NOURBAKHSH: You have the same case for high-burnup?

MR. POWERS: It's almost exactly the same case there, except you've got, with the high-burnup conventional clads, as opposed to the M5 clads, you've got a little thicker oxide coating, so you know it's a slower oxidation process, unless you get a breakaway.

I don't discount it. I mean, I think it is one of the premier uncertainties in the late phase of the accident. Is it consequential enough to investigate? Well, it has more interest now that we worry about spent fuel pools because the same phenomenon happens there. If you melt down fast, your releases are going to be low. If you melt down slow, your releases are going to be high.

MR. BOYACK: Are there any comments on research needs?

MR. LEAVER: A couple of questions.

MR. BOYACK: Sure.

MR. LEAVER: It sounds like what you were

saying is that you don't believe that tellurium has tin telluride, and therefore, it will tend to have a higher deposition velocity than what you had -- you had written that short paper yesterday where I think you were thinking in terms of tin telluride, is that correct?

MR. POWERS: Yes, well, my argument in the paper goes that in the gas phase it's tin telluride, and in the condensed phase it's silver telluride. It can never react with the surface to form nickel telluride.

Here you've got no tin telluride because you're working with a niobium clad, and there is a niobium telluride, but I don't think it's whompingly stable the way tin telluride is. There are other tellurides that could form. I mean I could be just dead wrong on this. This could come out just the same as we assumed for the high-burnup fuel with conventional cladding.

You do have the silver indium cadmium control rods, which can -- I mean cadmium telluride is a real nice compound, real stable, but we just don't see that much of it. We see more silver telluride, but only in the condensed phase. We never seen it as a gaseous species.

So I come saying, well, it looks to me like it is more likely to have deposition in the upper internals and primary piping system for this case than with conventional clads.

MR. LEAVER: Okay. Another question is, how much do we know to say that the oxygen potential is, in fact, higher for mixed oxide fuel than urial --

MR. POWERS: I really don't -- I'm reasoning that we're going to have some evidence that you get a little higher ruthenium movement. The only way I can move ruthenium around in these MOX fuels is if there is more oxygen available.

MR. LEAVER: Do we have such evidence for higher ruthenium in oxygen --

MR. POWERS: Yes.

MR. LEAVER: What is that?

MR. POWERS: It's a VERCORS test.

MR. LEAVER: Okay. I wasn't aware of that.

Do you know which test is that, Bernard? The evidence for more ruthenium, higher ruthenium release in MOX than in UO2, do we have such evidence? I wasn't aware of that.

MR. CLEMENT: I don't say so.

MR. LEAVER: Well, yes, we talked about

this yesterday, and the 15 percent is like it's different for these different numbers, and he wasn't specific about any of these things. But, I mean, that's an important point if that's true. Is there higher ruthenium release for MOX than there is UO2.

MR. CLEMENT: It's too early to say that.

MR. LEAVER: Okay. I thought maybe you were thinking that if we visualized the mixed oxide as these 30, 40, 50 micron particles with a lot of local, more concentrated fission events and, therefore, fission products in the smaller volume, that you just would tend to see higher oxygen potential, at least for the fission products. That's not what you're saying?

MR. POWERS: No, I mean my reasoning goes this way: PuO2 has inherent desire to go to PuO2 minus X. We have never ever seen PuO2 plus X. I guess I said that too strongly. To my knowledge, there is one report in the literature of a 2 plus X forming.

So that means you can't accommodate a lot of interstitials in the plutonium where what happens is the interstitials get balanced by the vacancies. But you're forming oxygen when you fission things. I mean you've got to put it someplace. So there's only

one place for it to go, and that's over into the oxygen lattice, the uranium lattice.

So if I take this thing up and I have a lower yield of molybdenum in these systems anyway, I no way to buffer it. So I reach my buffering capacity. I have seen these higher releases of ruthenium, and I said, well, if I'm moving ruthenium around, I'm sure I'm moving tellurium around. That was the rationing.

MR. NESBITT: In the discussion yesterday we talked about ruthenium, but it was in the context of the predicted quantities produced by the fission yields. That was one of the elements that had a significantly higher production in the MOX than LEU. So there might be some confusion about what that was about. That was just the amount that gets produced in the fuel matrix. It does not have anything to do with what happens to it once it gets produced in terms of being released.

MR. POWERS: It's about a 15 to 20 percent increase in production. In this world that's not very significant.

MR. NESBITT: It was higher. It was like 74 percent higher.

MR. POWERS: A factor of two isn't going

to make any difference to us. It increases the activity a little bit, but that's all. Roughly a factor of 74 percent.

MR. LEAVER: I would maybe make a comment. There is, in one of Bernard's slides he presented yesterday, a statement that in RT 7, which is a reducing environment MOX test, right, that there was, quote, "low, but significant release (less than 15 percent) of niobium, lanthanum, europian, ruthenium, moly, cerian, neptunian.

What Bernard said yesterday was that these six or seven elements all had different numbers, all less than 15 percent, so to think of it as between 10 and 15 is really not right because it's different for different elements. What he just said a moment ago is they're still trying to, I guess, define those numbers.

MR. CLEMENT: It is less than 15. It is less than 15.

MR. LEAVER: It's less?

MR. CLEMENT: It's not more than 10. It's less than 15. That's all.

MR. LEAVER: Ten to 15. That's right. That's what I said. It is less than 15. That doesn't mean it's more than 10.

MR. CLEMENT: No.

MR. LEAVER: It just means it's less than 15.

MR. CLEMENT: Yes.

MR. LEAVER: Now ruthenium, as I recall, there was from the fuel, well, some fairly significant releases, perhaps I remember 6 and 7 percent.

MR. CLEMENT: We didn't give any figure for this test for ruthenium.

MR. LEAVER: No, for the UO2.

MR. CLEMENT: Oh, for UO2, yes. Yes.

MR. LEAVER: Yes. So I just don't see any data that would suggest that you get higher ruthenium releases -- maybe you do, but I don't see any data that suggests higher ruthenium releases from MOX fuel.

MR. CLEMENT: At least from this data there is no change in the order of magnitude. Okay?

MR. LEAVER: Yes, okay.

MR. CLEMENT: For these data.

MR. BOYACK: Okay, on tellurium now, what we're looking for I think is more complete exposition on data needs in your letters, but is there anything else that you want to flag here for the tellurium group in the way of data needs?

MR. POWERS: Well, for the tellurium, we

just need to know what it's doing. The problem is it's virtually impossible to measure it.

MR. CLEMENT: Still on tellurium, I think it's important to assess, not forget to assess for gap releases and for other points, the effect of the absence of tin, to look, to confirm or not the hypothesis because this may change things.

MR. POWERS: It may be that something else steps in and acts in the same way, in which case if you get release, you'll get it out.

MR. KRESS: I know that for regular LEU fuel, we took some tests, just took the clad out. There wasn't any clad there. But tellurium got released just at the same rate, in fact a little faster than the iodine. So with LEU fuel, if you don't have the clad there, why, you get it released.

I'm assuming the M5 clad would act like, for tellurium, like there wasn't any clad there, but Dana may be right; there may be other things in the fuel that could latch onto it for MOX, as opposed to LEU.

MR. GIESEKE: So it sounds like there may be a different, significant difference between MOX and the other, the M5 clad and the other cladding for tellurium. So it warrants experiments to check

into --

MR. KRESS: It warrants experiments with M5 clad in MOX fuel.

MR. GIESEKE: Yes.

MR. BOYACK: Okay, tomorrow we have four groups to take care of, and there was some discussion at lunchtime with Jason that it looks like we can go through these reasonably well. We've had good progress today in the afternoon on this.

MR. LEAVER: I don't know. I mean, low volatiles are going to be tough. At least up to this point we've thought of these elements as volatile, which they are, but now they're not.

It may take more than an hour on each of them?

MR. KRESS: It may take six months. I don't know how --

MR. LEAVER: The difference may be that there can be no answers.

MR. KRESS: Yes, I think that's the tough part.

MR. CLEMENT: It depends how we agree to take into account the uncertainties that are much higher on these groups, because anyway it will be more difficult to have a precise and definite value with

actual --

MR. BOYACK: I propose to bring those up tomorrow in any case.

MR. LEAVER: The problem is that for something like say iodine or cesium, you can to some degree take uncertainty into account, and though not all of us did, you can increment it, and in doing so you may increase the amount by 10 percent or 20 percent, or something like that.

For the low volatiles, you could take uncertainty into account and increase the release by a factor of 100 or 50, and that can be, begins to be a very significant effect on dose and really gives you a source term that's just greatly different than what people have been using for UO2 fuel, and I'm not sure that necessarily solves any problems or is what we want to do. We can say things are uncertain, but for the low volatiles, now you're talking orders of magnitude instead of 10 or 20 percent.

MR. KRESS: And I think RT 7 sheds some light on that.

MR. LEAVER: If we had --

MR. KRESS: If we have the results for the low volatiles.

MR. LEAVER: Right.

MR. KRESS: It would be useful --

MR. LEAVER: Richard is not here. He said something.

MR. KRESS: He said those will probably -- he said there are negotiations are underway, and his understanding was that that RT 7 data will be released soon to NRC. He didn't say when, but they will. They are talking about getting it, and they will probably get it.

MR. LEAVER: I think it was going to be soon.

MR. KRESS: Yes, it sounded like probably soon, but they didn't give a time. So that's going to be available, but it won't be available in time for us.

MR. CLEMENT: I can confirm that discussions are underway. We don't know when it will be finalized.

MR. KRESS: We can do a lot better when we get that data, but until we get it, well, I think we're kind of in the dark on these.

MR. BOYACK: Tomorrow the way the day's going to unfold is, evidently, they have a real challenge scheduling rooms here. So there's a group that's coming in here at 11:45 tomorrow. They'll be

here until 1:15. It's a Toastmasters' group, so you're welcome to stay and participate in giving talks, if you wish.

(Laughter.)

But there will be that.

I'm going to take 45 minutes earlier. I have a meeting to go to, and Jason Schaperow will take over the discussion here at 11:00.

MR. LEAVER: Do we need to go two-thirds of a day tomorrow?

MR. BOYACK: Well, that was where I was starting to ask some questions. It's not clear from your answer where we'll end up tomorrow.

The thing that Jason had said, which was where I was starting to go, was that it would be nice of there were any time left tomorrow where the panel towards the end of the day could take a little bit of time and say, to reflect a little bit about the meetings thus far, what we've learned about PWR, high-burnup fuel, and BWR, and if there were any set of bottom-line items that sort of come to mind through this learning process, this learning/doing process, to get those down, which would be made available to most thus far to ponder and put in the final report.

MR. LEAVER: Bottom-line, what does that

mean?

MR. BOYACK: Summary, conclusions, things that have been learned. Just what has been learned by going through this process in the source term applicability on the three areas?

So I think the answer is that we'll probably use all the time available. You're going to leave at 1:00, I believe, was it?

MR. LEAVER: I think if I need to get there an hour and a half ahead, I probably need to leave about 2:00, maybe 2:30.

MR. BOYACK: Okay, that would be fine. I think we'll be done by 2:30.

So, with that, any other questions or comments? Then we'll turn off the tape machine.

(Whereupon, the foregoing matter went off the record at 4:36 p.m., to reconvene the following day, Thursday, February 21, 2002.)