UNITED STATES NUCLEAR REGULATORY COMMISSION

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SOURCE TERM APPLICABILITY PANEL

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MONDAY,
SEPTEMBER 24, 2001

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The panel convened at 8:30 a.m. in the ASLBP Hearing Room (T-3-B-45), Two White Flint North, Rockville, MD, 20852.

FACILITATOR:

Brent Boyack, Los Alamos National Laboratories

PANEL MEMBERS:

Thomas Kress (Consultant)
Dana Powers (Sandia National Laboratories)
James Gieseke (Consultant)
Akihid Hidaka (JAERI)

A-G-E-N-D-A

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

(8:37 a.m.)

DR. BOYACK: Well, we're at the appointed time and it looks like everything is now set up so we'll go ahead and begin. I think what we would like to do is there's a few introductory comments by the NRC.

Jack Rosenthal, I believe you're going to say one or two things. Then we'll go ahead and have some introductions. Then we'll begin the formal part of the meeting. Jack.

DR. ROSENTHAL: I would like to thank you for coming and participating in the PIRT. I recognize you are all busy people and it is particularly valuable to us in terms of guiding the future research so thank you.

You see the photographer around. We're just planning a little article for Inside NRC. It won't appear in the New York Times.

DR. BOYACK: Depends on what we say, right?

DR. ROSENTHAL: It is a public meeting. It's not a FACA meeting so we'll get everybody's individual news rather than a joint view of the committee. It is a transcribed meeting and it's open to the public. Although I believe nobody from the public is here today, they are welcome to be here.

We've been using PIRT or PIRT-like activities. This is a PIRT-like activity with expert elicitation, surely rather than maybe all the formalism that occurs in our fuels program and thermal hydraulic program. I think it's an excellent way to visibly develop the future path, so thank you for being here.

DR. BOYACK: Thank you, Jack. What I would like to do is just take a moment and let each of the panel members and people in the

audience who care to introduce themselves so that we have a sense of who we are.

I'll begin with that and then we'll move back over to Dana in just a minute and just go through.

I'm Brent Boyack. I'm a member of the staff at Los Alamos National Laboratory and I've been asked to facilitate this particular source term applicability panel's efforts.

Through the years I have been involved in a large number of PIRT activities. I think that is primarily the reason why I've been asked to participate.

Dana.

DR. POWERS: I'm Dana Powers. I'm a member of the staff at Sandia National Laboratories and sometimes show up for ACRS meetings. I guess I have some background in the area of source term and behavior of materials at high temperature.

DR. GIESEKE: Jim Gieseke. I'm a private consultant at this point having retired from Bechtel after 35 years, many years of which were involved in reactor safety of one sort or another starting with fast reactors and moving into water cooled reactors, and later years with particular emphasis on source term and fission product release and transport.

DR. KRESS: Are you through?

DR. GIESEKE: I'm through. Would you like me to tell you some more stories?

DR. KRESS: I thought you were going to say something about aerosol.

DR. GIESEKE: Well, I did that, too.

DR. KRESS: Tom Kress, retired from Oak Ridge National Laboratory and sometimes show up for ACRS meetings. I have a relatively large background in source terms.

DR. SCHAPEROW: I'm Jason Schaperow, NRC staff. I'm a CRX analyst who does a bit of work in the source term area. I'm the coordinator and organizer for the meeting.

DR. NOURBAKHSH: Hossein Nourbakhsh. I'm with a small consulting company outside of New York City -- New York at Stonybrook. Previously I was affiliated with Brookhaven, which we did some work on source terms.

DR. TINKLER: I'm Charlie Tinkler of the NRC research staff. I've been involved in severe accident issues and research, I guess, for about a dozen or so years and before that I worked on similar kinds of things in NOR.

DR. HIDAKA: My name is Akihid Hidaka from Japan Atomic Energy Research Institute. And my research is interested in VEGA release and transport and I saw some variation. Today I show you the results of my experiments.

DR. BOYACK: Thank you. Let's go ahead and have the guests in the back introduce yourselves if you will.

(Whereupon, there are audience introductions.)

DR. BOYACK: Okay. Fine. Thank you very much.

I guess just a few housekeeping. I'm less familiar with the layout here on the third floor but I presume there is a restroom somewhere here that we'll be able to take and use during the breaks.

Does anybody have any insights about moving back and forth under the new security regulations? I presume we can only move if escorted. Is that correct?

DR. POWERS: It may say on your badge if escort is required.

DR. BOYACK: Yes, escort is required even on the third floor here now as we move around. At the breaks if you want to go down to the cafeteria and get a drink, then we'll need to have somebody take us down and bring us back. At least those of us who don't have NRC badges.

DR. POWERS: I recognize that Tom and I have badges.

DR. BOYACK: Yes. A different class of citizen. That's true.

DR. ROSENTHAL: Not better or worse. You always say different I notice.

DR. BOYACK: I did leave it at that.

Let's see. Somewhere here I have a sign-up list for those people who are participating. Has everybody in the back had a chance to sign that?

Just a couple of other things that may be of some use to you. When we do go ahead and produce the final report for this particular activity, one of the things we do is put in brief curriculum vitaes.

By e-mail after this meeting I'll go ahead and send you an example of the type of things that we've done in the past and we'll ask you to

go ahead and respond and send your vitae back to us by e-mail. They generally run about half a page max. We try to hold them to about that.

We think it's important on expert panels to be able to provide some information in the report that goes ahead and identifies who the panel members were so that people have that as part of their process of evaluating the report.

There will be a document, a NUREG CR produced for this particular activity that will come out, of course, as a NUREG CR and I believe it will be an EPRI numbered document also. When we get done we'll have that. It's not, I guess, clear exactly how many meetings we'll have to have.

I think the maximum was intended to be three. It could be less, of course, if we are able to accomplish the work quicker. There was a stated end date for this activity which was we needed to be done by March of 2002.

Any other comments on any of that?

DR. ROSENTHAL: Just not EPRI but rather ERI. It's just that they are contracted to us.

DR. BOYACK: Yes, I said EPRI instead of ERI. Sorry. That's right. The record ought to be clarified on that point.

Anyway, those are a few of the dimensions of the panel activity. Are there any questions of that nature that deal with that kind of issue before we go ahead and begin with the agenda items?

DR. KRESS: Each panel member is expected to write up his own report that goes into this NUREG?

DR. BOYACK: No, I don't believe that's the case. The particular way this will go is that -- this will become a little more clear as we continue on through the activities of today.

In essence there are in NUREG-1465 several tables. It's the task of this panel, to the extent they are able to based upon their expert opinion and backgrounds, to literally update those tables for several situations. One of them is high burn-up fuel and the second is MOX fuel. Then, of course, for the two reactor types as applicable.

So the report will carry that information and there won't be a need for individual reports except as we define any needs during the course of the meeting.

I'm trying to think if there is anything else. Are there any other questions? Again, some of this will become a little more clear as we go through the presentations of the morning.

DR. POWERS: Let me ask a question about MOX. When we think about MOX we have two things that can differ. One is the isotopic mix of the plutonium that can differ between when they use weapons materials or when they use reactor grade materials.

The other is that they are coming to at least two and maybe as many as four different types of MOX having to do with how they are prepared and the homogeneity of the matrix and the amount of concentrate, the inclusions of plutonium dioxide that exist.

Do we have any targeted type of MOX for this exercise?

DR. BOYACK: Okay. I think this is going to be a question for the NRC. Let me first see if there is an answer to that question.

DR. MARTIN: Do you have a hearty type of MOX?

DR. BOYACK: Would you identify yourself in the back by name first?

DR. MARTIN: I'm Bob Martin.

DR. BOYACK: I should mention that the reason that I'm doing that is we are having a transcript prepared for this activity so we do have a court reporter who is taking that information down.

DR. SCHAPEROW: McGuire is one of the first plants which is going to be using MOX. This is my understanding.

DR. MARTIN: McGuire and Catoctin. As much as I can tell you now is that the prospective fuel blender, the lead test assembly of which Framatone US has produced a fuel qualification report which discusses this issue in at least some detail. They give you a sort of introduction to the fuel specification and talk about (unintelligible). I can provide you a copy of that.

DR. BOYACK: Let me just say and add this particular perspective. From being involved in panels like this in the past, it's better, I think, if we can focus on a particular fuel design for carrying out our deliberations.

Then if we want to do an incremental type of assessment of variations on different MOX fuels and ask ourselves would they affect any of the findings, then that is a reasonable approach to go but it is much better to focus on a particular MOX fuel as we have our initial deliberations.

DR. TINKLER: We should probably also talk to Richard to see to the extent that his MOX research plan includes one or more types of MOX fuel. In general I agree. We need to clarify that as we go on in the next

two days which of these MOX fuels we would like the panel to primarily focus on.

DR. KRESS: Similar question about high burn-up. Do we have a target burn-up level we're talking about?

DR. SCHAPEROW: The target level that has been thrown around, at least, is a 75 gigawatt per ton for the maximum assembly.

DR. BOYACK: Do you think that's what we ought to use as our target then?

DR. SCHAPEROW: I believe so.

DR. BOYACK: Okay.

DR. KRESS: I'm really more interested in the average burn-up average and not the maximum. Do we have something that translates a maximum into an average?

DR. SCHAPEROW: Nothing other than divide by two-thirds.

DR. KRESS: So it's about two-thirds.

DR. SCHAPEROW: It may be a little higher than that.

DR. TINKLER: I think on this point, though, I also ask the panel to consider perhaps the effects of 75 gigawatt per metric ton fuel by itself. DR. KRESS: By itself.

DR. TINKLER: Just so we had some -- so then we could weight the average distribution of fuel in the core. Otherwise, we end up coming back each time.

DR. KRESS: In order to weigh it, though, you'll have to have a correlation or something.

DR. TINKLER: Right.

DR. KRESS: On more than one point anyway. The other question I would have is we are excluding RIAs from this particular pattern because you've already dealt with those?

DR. TINKLER: Perhaps we should address heat up and melt-down.

DR. BOYACK: Okay. Any other questions?

DR. POWERS: What's -- one thing we have to consider is what kind of clad we're going to be using, especially for the high burn-up. Are we going to the niobium based clads or are we going to stay with the Zircoloids?

DR. ROSENTHAL: I think we ought to go with an M-5 zirlo and ideally we would know what the robust field program is developing. If you are ascribing zirlo then there's no sense in going back to the Zirc 2 and Zirc 4.

DR. POWERS: Okay. Now, my recollection is M-5 is a niobium zirconium and zirlo is a mixture of 10 niobium.

DR. ROSENTHAL: Yes. If you need the chemical composition, I can pull it off my board.

DR. POWERS: That's okay. The details won't make that much difference.

DR. ROSENTHAL: But it makes a difference. The group should be aware that just separately we do have a test program at Argonne National Lab which is right now Zirc 2 and Zirc 4 but we are trying to work with the vendors to get M-5 and zirlo hopefully both fresh and high burn-up and we'll be testing the clad separately.

DR. KRESS: Another question is are we focusing only on low pressure accident?

DR. TINKLER: No. I think we should consider a range of representative accident sequences not necessarily focusing on just the low pressure scenarios. The low pressure scenarios have a particular significance for design basis and we should consider that, the influence of those design bases.

DR. KRESS: 1465 is a design basis source term.

DR. TINKLER: Right. It's used for a design basis. At least the first two phases of the source term are used for the design basis. I think we should also consider higher pressure scenarios as well.

DR. KRESS: Does that mean we need to think about effects in the RCS?

DR. TINKLER: Yes. Absolutely. This is a source term to the containment, not simply the release from the fuel.

DR. KRESS: Does this then also maybe deal with late sources of Iodine?

DR. TINKLER: I think we should. I think we should because the tables include early and late. Our colleagues and brethren at NRR will still be looking towards those first two phases for their design basis source term, but 1465 includes the source term for laid-in vessel and ex-vessel as well. To the extent we have insights which relate to phenomena that occur late in the containment as well, we would invite the panel to comment on that.

I want to make one other point without getting too far into the objectives. I guess we are assuming here that origin or its manifestations in

other forms can be used to calculate the mix of radionuclides in the fuel, at least within the accuracies that we need for our source term calculations but not to say for all purposes. Perhaps people in the KS or shielding business maybe they want more.

But for our release to the containment, I think we are concluding at this point that we have generally speaking the necessary tools to calculate the inventory. Now we're talking about how much of that inventory in the fuel is released to the containment.

DR. KRESS: I think that's a reasonable assumption. There is some question about some of the origin treatment for plutonium but I think it's sufficiently well established that for these purposes you don't have to worry about it.

DR. BOYACK: These questions that are being asked now are entirely appropriate. At the time we begin to work through the tables themselves and come to some understanding of the effects of high burn-up and MOX fuels, we want to make sure we have a quite precise definition before all the panel members so that they are clear about exactly the circumstances, the fuels, and the scenarios that we are dealing with at the time.

To the extent we don't do that, then we'll get down the path further and there will be some confusion and people will say, "Gee, I didn't think that's what we were doing."

One of the things that we try to do is as we finish up this first meeting we will try to go ahead and prepare a skeletal document that incorporates the information that was generated in the first disperse meeting.

Rather than seeing meeting minutes, you'll have two sources of information available.

The first source of information will be the transcript that will be available to all the members. The second thing will be that sometime before the next meeting there will be this skeletal draft in which we try to draw up all the information that we gathered at the first meeting into the document which will evolve eventually into the final document.

DR. KRESS: Have there been any calculations using, for example, fast grass which has a burn-up correlation to see what kind of results you would get with just a calculation?

DR. SCHAPEROW: No. I remember you had suggested that earlier and I have not done that yet. It's still a reasonable approach but we haven't taken that yet.

DR. BOYACK: Any other first comments before we begin with the agenda items?

DR. LEE: I'm Richard Lee. I understand the questions about the grain for the MOX fuel was raised.

DR. POWERS: I think you need to know the fabrication path of the MOX. I think once we have the fabrication path we know a lot about it. The differences are multi-fold. How much plutonium is dispersed in the uranian matrix and how much of it is present in the inclusion is the first thing you worry about.

Then the second thing you worry about is if there are inclusions of relatively pure $U\text{-}0_2$, are they randomly oriented grains like you

would get from a sulgel process or are they long-grain materials that you get from a high temperature metallurgical process.

DR. LEE: I think from the fuel fabrication is that we would like to get all the information relating to those formations you just mentioned about, what does it look like and the grain size distribution from total particles.

We were told it would not be more than 100 microns because we are concerned if there are bigger particles when you analyze it for certain transients which could become a problem based on old data we have which has very large grain of close to 1,000 microns.

Those are the formations we are asking for because we would like to know what does the fuel look like. Those would be available when they manufacture the fuel exactly. They are very close to the French reactor grain plutonium type fuel. I mean the process is going to be the same one but what is the official (unintelligible) for this particular weapons grade MOX.

DR. POWERS: There are a couple of different types of MOXs that are attributed to the French if they will tell us. Interestingly enough, I mean fascinating to me, it makes a difference how the uranium was produced for that portion of the distribution as well. It's one of those things that just changes the microstructure.

DR. LEE: We understood that the Oak Ridge was conducting fuel irradiation and DTR reactors. Those fuels will be quite close to the weapon-grade MOX. That's what we were told. And they have data that will be available from DOE program for us.

DR. BOYACK: Just one final thought. Because the French information was mentioned, IPSN will be represented and serve as a member

of the panel. There are two individuals that will participate, Bernard Commont (phonetic) and Jay Arbard (phonetic).

Bernard Commont (phonetic) was going to attend this meeting but IPSN has travel restrictions from France here to the United States at the present time so he sends his regrets that he was not able to participate but we'll look forward to them participating in the next meeting.

Okay. With that, let's go ahead and continue on. We're going to have our NRC guide us through a little bit of background and history and the need and Jason Schaperow is going to provide that presentation.

DR. SCHAPEROW: I regret that this won't be a formal presentation due to other events that occurred last week but I would like to make some remarks, as Brent said, to the background and history and need for this.

As you are all probably quite familiar with, the NRC is a good part of NUREG-1465 for its licensing activity for operating reactors and also for future reactors.

We use the physical form fraction for aerosol where 95 percent of the ion is aerosol. We use the chemical form fraction where 3 percent of the vapor ion in the containers is converted over to organic vapor. We used the magnitude up through the in-vessel phase. We generally rely quite heavily on NUREG-1465 as our source term.

There are a lot of plants that are coming in with license amendments to implement it. We rely on both safety and cost benefits. It's well recognized throughout the US and probably throughout the world as a significant improvement on what we had with ETID-1484 for a source term.

What we are attempting to do here is to conduct what I would like to call PIRT like activity whereas we are asking the experts to take a look at NUREG-1465 and review its basis. We've got Hossein Nourbakhsh who is going to go through all the information in 1465, where it came from, what kind of calculations were used to come up with those numbers.

We're going to ask you to review the background and make some judgements as to whether we can use NUREG-1465 for high burn-up and MOX, whether it's appropriate to apply it to those conditions.

When we get into the sessions on high burn-up and MOX tomorrow, as you point out, what we'll need to get in more detail is exactly what do we mean by high burn-up. How high is the burn-up and then the same for MOX.

So after we review the basis of 1465 we are going to ask that any new data be presented. We do have some new data that Jerry has brought to the meeting which we are very grateful for that. My understanding is they have run four tests so far over the last couple of years with what I would characterize as a fairly high burn-up.

They are using 47 gigawatt base per ton for their initial tests. The highest test we have performed in the U.S., from what I have seen, is 47 gigawatt base per ton, so they are at the top of our scale.

The French I understand have also done -- I believe they have actually done a MOX test but I haven't seen the results. They did do a high burn-up test and again the results, I think, have not been published yet. Unfortunately, the French are not going to be able to be here. Did you have

any success? Were they able to provide anything ahead of time or do you want to wait until the second meeting?

DR. BOYACK: Well, I have this slight problem when I tried to download my e-mail at work. That is, if somebody has dumped a huge attachment into the folder, you know, it just takes forever to download. Tonight I will just pay the price and let it run until everything is cleared out and see whether or not they responded. I didn't get that done last night.

DR. SCHAPEROW: We may be able to present you at least with a handout of what the French have put together. My understanding is they will be here at the second meeting to formally go over their results so far in the test program.

After we review the new data we are going to ask the experts to make judgements as to the appropriateness of NUREG-1465, in all three of it`s areas. In the area of release fractions, into the area of the containment, and in the area of timing, in the area of physical and chemical form.

If you judge that some aspects of NUREG-1465 are not appropriate for use with the higher burn-ups in MOX reactor cores, then we are going to ask you for some numbers or some judgements in that regard and also if it is considered that any additional test data may be needed.

We have done a few calculations this past year with a couple of our dose coats to look at the relative importance of individual groups, individual isotopic groups, the iodine group, the Cesium group, etc.

I`d like to share one insight that I gained this year was that iodine really dominates in some cases. If you made these pies where you have slices for each group, with iodine in some cases the whole thing is iodine

except for one tiny little slice which is the sum of everything else depending on the health effect you're looking at.

That's all I have to say.

Charlie, would you like to add something?

DR. TINKLER: Yeah, I wanted to add something and Brent will probably be ready to throw me out of the room when I say this. As a proper coordinator, he has already clarified that he likes a clear target. You know, when we have experts in a room we like to ask them as many things as we can which is beneficial to us.

In that vein, the extent to which the panel is comfortable and can make comments and give guidance with respect to things that are a bit afield. By that we'll say things like different kinds of environments perhaps, highly oxidizing environments versus typical reactor environments. And the extent to which maybe different groups are affected by those things.

I would just point out we did some work with both Radrad and Max to look at the important groups typically for our licensing calculations. You know, we show the dominance of iodine which is not surprising.

Other kinds of accidents in other analysis show other less volatile groups become quite important. They are small relief fractions but when iodine is decayed away, then other groups become important.

We have, at least some of the members of the panel know, we have attempted to use 1465 for other kinds of accidents with varying degrees of success, I would say, to the extent the panel can provide feedback on other accidents and the appropriateness of the source term for them based on some experiences.

DR. BOYACK: Okay. Actually, during the discussions there will be many opportunities to collect those. We'll see that as we -- when I get to my presentation I'll offer some of the processes. At some point you have to have a kernel of focus here to really generate, I think, the maximum amount of specific information which is what ultimately those tables turn out to be in 1465.

Okay. With that, I would like to go ahead and turn the time over to Dr. Nourbakhsh. I'll go ahead and hand out his presentation and we'll continue with him.

DR. NOURBAKHSH: I am going to give a presentation on the basis of the formulation of NUREG-1465. In order to provide a background on the formulation of 1465 I will briefly go over the review of phenomenological aspects of severe core damage accidents and define the release phases of severe accidents in terms of NUREG-1465 terminology.

Then for each of the release phases I'm going to discuss the technical basis and the information that has been used for development of the NUREG-1465.

Basically these are the release phases; gap release, early in-vessel release, ex-vessel release, late in-vessel release which is basically the releases due to revolatilization of volatiles in the reactor vessels.

Then finally I'm going to give a brief review of the work that has been done in support of chemical forms of iodine. And some discussions on the simplification and actual development of NUREG-1465 source and basically summarize at the end.

In NUREG-1465 source there was intended as a representative source. It was more toward looking at no pressure devices. The whole processes and sequences of events is (unintelligible) and real accident sequences is defined here. This I'm just putting it like an influence diagram and causal relationship in a top level phenomenon. If we can actually break each of these (unintelligible) but I`m not going to discuss it yet.

From accident initiation we have a boil off due to loss of water to break in RCS until core uncovery. That would be one phrase. Then core uncovery could heat-up into another phase and that is due to heat-up (unintelligible) fade the fuel and that's where the gap release comes into the picture.

Then as you continue toward the accident progression we have severe core degradation which is you lose the integrity of the fuel in the core and you start melting the fuel. I just put in here RCS depressurization makes an impact on some of these processes. This could be actually be done in thermally induced failure of RCS due to natural circulation.

Then as you progress toward severe accidents, you have the relocation to the lower plenum and this would be late phase of vessel accident progression basically.

Then you have bottom head melt attack and failure. Again, even though the probability of a vessel still explosion is low but since it has been mentioned it was included in NUREG-1150 we have included it. This is lower head failure due to (unintelligible).

Basically, the most important thing is this would be the dividing point in vessel phase and ex-vessel phase, the bottom head melt

attack and failure. If that happens at high pressure, you have high pressure melt ejections and DCH and all releases associated with high pressure melt ejection.

Then if it is low pressure which (unintelligible). As far as NUREG-1465, as I mentioned before, it is more toward low pressure sequences. You must not consider releases due to high pressure melt ejection.

Then we have ex-vessel steam explosion. Again, the probability of having an energetic ex-vessel steam explosion is very low so the release is due to aerosol ex-vessel steam (unintelligible) explosion has been basically neglected.

Then at the end will be when the melt comes into the cavity or the vessel. The containment you have poor (unintelligible) fractions which is basically again some of these high pressure melt ejection phenomena has an impact on the physical ability (unintelligible) and that kind of causal relationship there but I`m not going to (unintelligible) that.

Anyway, in summary, the containment release phases would be at the beginning loss of primary system water. Associated with it you have coolant activity release. Then you have core heat-up and fuel cladding failure that basically (unintelligible) the content in the gap between the fuel and cladding will be released which will be the phase for -- I mean, the gap activity phase.

As you progress through severe accidents, it would be in-vessel severe accident progression. Due to having significant heat-up of the

fuel and melting and relocation, you have releases from the fuel and releases is primarily dependent on temperature and basically surface to volume ratio.

So releases from the fuel releases into the RCS. Then some of it is going to be retained in RCS due to some processes of aerosol removal from RCS. Then the outcome of this would be that some of it releases into the containment so that part of it releases into the RCS which is transmitted to the containment. We define this as early vessel release because this is happening before vessel failure.

Then you have core-concrete interactions following that which basically provides the sources of releases for the ex-vessel release phase. Again, I put these two linked parts, they are releases due to high pressure melt ejection and ex-vessel steam explosion that we are not considering here because, as I said again, we present the table of low RCS pressure sequence. It doesn't have high pressure melt ejection (unintelligible).

After vessel failure we have late revolatilization from RCS. It may not be significant as far as magnitude but is of importance because of the timing. If it comes late in the accident, it may happen, for example, (unintelligible) you have the benefit of sufficient cooling to retain that even if it is small.

Or you don't have the benefit of too much aerosol in the containment that you remove it so it's late so you may eventually have some implication (unintelligible). That's called late in-vessel releases. It depends on how much you really retain in-vessel. A fraction of it is going to be revolatilized and basically dominating the Cesium to some extent.

DR. KRESS: Is there any consideration there of re-evolution from water like suppression pool.

DR. NOURBAKHSH: These are all -- we are not looking at containment behavior now. We are following just a few to the containment. When it is in the containment there is discussions on impact of -- I mean, (unintelligible) pH control and other issues. That is part of behavior in the containment. So all we are discussing right now releases into the containment.

DR. TINKLER: It's important to note, though, that we have restricted release of chemical forms of certain radionuclides to a condition where you maintain some pH. You are only allowed to use the provisions in NUREG-1465. You can demonstrate that some pH is controlled. Otherwise --

DR. KRESS: You would probably put the same restrictions.

DR. TINKLER: We would put the same restrictions on it.

I have a quick question, too. The relationship between what we call 1465 in-vessel and NUREG-1150's language of large early release.

DR. NOURBAKHSH: Not every release is actually NUREG-1150.

DR. TINKLER: I understand but when they group them, when they group early --

DR. NOURBAKHSH: Even NUREG-1150 doesn't have -- this actually they call it revolatilizaton release. They are different in terminology. The parameter they use is late release. Then in-vessel release they don't have the terminology already. In-vessel release means that the release is before vessel failure.

DR. TINKLER: What I was getting at it's early release. Maybe not in NUREG but as we customarily would use the term, it's usually a few hours after and it may include up to a few hours after vessel failure up to maybe four hours after vessel failure.

DR. NOURBAKHSH: That's from the consideration where we call it large early release. This is basically an environmental release. That is coming from containment because of a lot of phenomenology of dynamic phenomenology happening at the vessel failure of DCS containment (unintelligible) . So we are looking at that release.

If it doesn't fail at time of vessel failure, it's not going to happen right away. It's going to happen late. Early and late releases from the point of view of large early release would be basically environmental release from containment. That's when we are including some of the vessel failure and containment failure associated with vessel failure basically at that time.

DR. KRESS: I have another question. My recollection is the source terms in 1465 were compiled mostly from low-pressure accidents and that there was really no retention in RCS assumed.

DR. NOURBAKHSH: Exactly. So what we -- there are some retention. The retention is significant in lower as we will discuss later on. There was a consistency in the assumptions when you are using, for example -- I'm going to go into more detail but you are assuming in vessel releases associated with high Zirc oxidations so you cannot at the same time go to pore completing fraction and assume high -- I mean -- which should use low oxidized Zircoloids for example. There is a consistency that has been applied in the pontification.

As far as the timing for sanctification a constant rate is assumed so if you look at the cumulative release into the containment -- it's not on scale, of course -- beginning in the order of a few seconds you have again (unintelligible).

You have coolant activity and then you have gap release and then you have early vessel and ex-vessel releases. Hours later you finish. Basically many hours later on the late in-vessel release. Finally it means (unintelligible) though releases may be continued for more than that but not a significant amount.

DR. KRESS: You have a different curve like that for each isotope?

DR. NOURBAKHSH: Pardon me?

DR. KRESS: You have a different curve like that for each isotope?

DR. NOURBAKHSH: I have it when developing -- I use some of the data of the source term code package in all this for each isotope.

DR. KRESS: For each group.

DR. NOURBAKHSH: Actually, I put the data and that simplification in order to get some idea of what constant rate would be appropriate. Some of it has been documented in NUREG-4881. Actually, we developed a small computer program at that time that pushed the characteristic of accidents as you see them. There was a draft before which was really never published but we included all of that in that too.

So in order to summarize basically before I go through that, the source of information is NUREG-1465. I'm using only this source. They

have referred to some of these sources and they use other information but basically the whole severe accident -- state a lot of severe accident source and has been incorporated (unintelligible).

Then for each phase what is important, for example, for the gap phase I used -- I mean, the onset of release is important, what it would be from the design basis accidents. It is important to know when it started release.

Then composition and magnitude. For example, for the timing of a composition and magnitude basically NUREG/CR-4881 and WASH-1400 has been used primarily basically for the gap, how much is in the gap information.

The source there was some work done on the timing in support of development of 1465. I'm not going to go through it. They use all this (unintelligible) and some sensitivity even with the track (unintelligible) .

Then on the vessel release the onset of release is important so the onset of release basically if you look at that, then you get some idea of what is the duration of release for gap release basically.

DR. BOYACK: Let me interrupt for a minute. We're having trouble with the recording.

COURT REPORTER: I just can't pick you up.

DR. NOURBAKHSH: So I have to --

COURT REPORTER: At least point your voice to the microphone.

DR. NOURBAKHSH: I'm a little bit uncomfortable. Okay.

In early vessel release we have the onset of release, duration of release and composition and magnitude of release. For the onset of release, again work that has been done by 5747 based on STCP calculations but may lead to the conclusion of (unintelligible) output. There was some other work supported that they looked at the timing of the release for early vessel release. Basically this is some mental calculation that has been done for few BWR sequences. NUREG/CR-5747 --

DR. BOYACK: Hossein?

DR. NOURBAKHSH: I'm going to go into more detail and information later on. Basically we use the information from result of source and code package calculations for reference plans and to quantify some (unintelligible) for these releases. Then having some ideas about the timing and the amount of release.

Then we use NUREG-1150 source term elicitation results, the cumulative relief aggregate of those distributions for 1150 expert. We use them instead of calculating environmental release, we use the LHS sampling to get the distribution into the containment for different release rates.

Then we provide some support for development of relation release and composition and magnitude and basically uncertainties for all these severe accident phases, early in-vessel, ex-vessel, and late in-vessel.

On the chemical for (unintelligible) the work that Oak Ridge has been doing was primary sources of information for NUREG-1465 formulation. Then for the organic form of iodine some old work of post-law 1972 WASH-1233 (unintelligible) .

Basically I'm going to go to each of these for gap release and I am going to have to go through each of them what was the information available at the time basically. That's NUREG-1465.

DR. TINKLER: I just want to make one point here. More recently we supplemented the work that had been done for the onset of release by doing calculations.

We did detailed (unintelligible) calculations for DWRs with a limiting double-ended resert line break to determine the onset of fuel failures because the boilers had argued that the timing that was imposed on them in 1465 which had addressed the repeat (unintelligible) too conservative and the calculations both by us, those calculations performed at INEL and the calculations performed by industry confirmed that the onset of fuel failures was a little over 100 seconds, close to two minutes.

DR. SCHAPEROW: I want you also to recall we had one NRC staff member, Tony Elsa, did some track B, I believe, calculations and got the same answer, about 120 seconds.

DR. KRESS: I had a question how you establish an onset of release for early indice, for example, because I picture this as a slowly increasing release rate. Where do you draw the line and say this is the start?

DR. NOURBAKHSH: We look at them and we draft them actually. At one point -- see they are temperature dependent -- at some point it's a (unintelligible) . We picked up that time. Actually we look at the containment concentration rather than vessel at the time that appears in the containment.

DR. KRESS: I take it a low-pressure release is slight.

DR. NOURBAKHSH: We look at that. Then that is basically what is the basis for onset of releases here. Actually, it depends on the timing. In this particular sequence if you -- some of them maybe are nowhere. It depends.

DR. TINKLER: I don't mean to suggest that we are looking for duration of early in-vessel release based on double-ended pipe breaks. Not at all.

DR. NOURBAKHSH: So this is really an important part as far as the extension to high burn-up (unintelligible) . What the composition and magnitude uses for the gap is NUREG-1465 basically based on WASH-1400. The gap between fuel and the cladding is no greater than 3 percent except for Cesium which was estimated to be about 5 percent.

We looked at some of the later work here that has been documented which is basically a comparison more recently, primarily of Lawrence's work if I recall correctly. We found out the releases may be a little bit less than WASH-1400. That's basically as far as composition and magnitude is concerned. We look at the onset of release and again I tried to put timing here that we know.

DR. POWERS: Back to the issues of the inventory. Do you know what kinds of linear power ratings you were looking at when you came up with those inventories? The reason I ask is we see a lot, especially the boilers, interested in running to a little higher linear ratings, a little more uniformity and that should affect the inventory for the gap release stage.

DR. NOURBAKHSH: We looked at that and actually we comprised some of it. These were basically NUREG-1465 or work that was

done in support of the development of that with not really that much emphasis on gap release at the time. Basically they took it from WASH-1400.

They were looking at the typical conditions that they were using. 4881 was basically in support of 1465 development. We looked at this and then there was some parametric calculation of that. We just provided the information. I believe it was not per se for 1465 that much detail.

DR. POWERS: I think the real point is that when you come to boilers especially when we're trying to get a focused problem, we need to know whether we're focusing on the boilers as they're run now or the boilers as they are run with the extended power upgrade. That's going to make a difference in the gap inventory.

DR. KRESS: My suggestion would be to use the power upgrade because they're all going to go to it.

DR. POWERS: I think they will all eventually -- I mean, a substantial number of them are going to go to an extended power upgrade.

DR. KRESS: Which means the maximum to average is going to be different. It's really going to flatten the amount.

DR. SCHAPEROW: What you're talking about would affect NUREG-1465 itself regardless of whether you're going in -- whether it can be applied to high-burn or not. I would like to separate that out. We can certainly talk about it. It`s a good point to talk about.

DR. POWERS: You have to understand what it's going to affect is the gap inventory and the gap release fractions. It is not going to affect the integral from there to the end of the scenario. It will change the importance of that gap release fraction.

DR. SCHAPEROW: The gap release is important. There are a number of design bases that rely on that.

DR. POWERS: That's right.

DR. SCHAPEROW: I'm particularly interested in that.

DR. POWERS: Somehow in your elicitation you're going to have to recognize (unintelligible) power upgrade and it is going to change their fuel management cycle.

DR. TINKLER: That is a good point because 1465 is generally core-average kinds of numbers. NOR has a clear need for non-average numbers for gap releases. To the extent we can extrapolate to different conditions for nonaverage fuel rods for worse high power, high burn-up rods or whatever, the extent to which we can clarify the influence of burn-up for the gap relief would be quite useful to NRR.

DR. KRESS: Well, when you do the origin calculation, I suppose we could take both the power distribution and the burn-up into consideration to give the total. The question is do we know how that affects the inventory and the gap. Is it a different fraction of that total? I'm not sure but I think that would be a flux of linear power.

DR. POWERS: I think the negative fact on the gap inventory is going to come from the linear power from the fuel oil pump. You've got to get power some way. You can get it by raising the power distribution at the ends of the rods.

DR. KRESS: A lot of that gap inventory is driven by the temperature difference between middle and the outside. That's the reason I think about that. It's the fraction we're worried about.

DR. POWERS: And the other point is I don't think you can rest on WASH-1400. In fact, WASH-1400 in some of their analyses done in Appendix 7 used rather high linear heating rates to get the gap into the high gap inventories. The subsequent work by Laurens used relatively low linear heating rates. There's a difference in the conclusion of the completed gap inventories.

DR. ROSENTHAL: I'm sorry. Ultimately, did the group comment on -- we're looking at high burn-up. Ultimately you have teleclay interaction. What do you mean by a gap at these high burn-ups?

DR. KRESS: I think you have to picture that the high burn-up that the gap just gets redistributed. It`s still there.

DR. POWERS: It gets redistributed up to the point where fuel come in. It gets retro- (unintelligible) is the gap release fraction as stuff gets blended or the clad ruptures and you really don't care where it comes from.

DR. KRESS: You don't have to picture it as the gap between the (unintelligible) and the clad.

DR. NOURBAKHSH: So as far as the onset of gap release is concerned, the work of NUREG/CR-5787 was work done in support of 1465 formulation. They use the calculation using FRAPCON2 and SCDAP/RELAP5 MOD3.0 and FRAPT6 computer codes for two PWR plants, B&W and the Westinghouse plant.

The minimum time from the time of accident initiation until first fuel rod failure was calculated to be 13 and 24.6 seconds for the B&W and Westinghouse respectively.

Basically what they did during most of this large (unintelligible) . But they changed the size of the failure for 6-inch line break and the time was increased significantly to 6.5 and 10 minutes. This was done basically for the one that they are using before break.

A review of FSARs has been done for BWRs at the time of 1465 and it was recognized that the fuel failure may occur significantly later, on the order of several minutes or more. 1465 recognized that and basically for simplicity they put it all at one time but I suppose the report mentioned that they can take a look at it on a case-by-case basis.

DR. POWERS: I think it's worth pointing out that neither FRAPCON nor FRAPT at the time of those calculations had models where fuel is injected for fuel cladding that had been subjected to hyperope so they probably didn't want to estimate fuel clad failure correctly for hyperope clad.

DR. NOURBAKHSH: But do we know the calculation was done by high burn-up?

DR. POWERS: It doesn't matter. They didn't have the data. It doesn't matter what they did, they didn't have the data.

DR. NOURBAKHSH: Actually, some sensitivity calculation to track PF1 was done at the same time and it was a little bit higher. It was discussed as the differences in the report basically.

As far as severe accidents source terms now with everything going and gap releases, severe accidents source terms -- because it is dominated by this, NUREG/CR-5747 in our report actually was done before the development of 1465.

It was done as supporting work for formulation of 1465. Basically all

the information of source terms prior to -- look at all the result of source term code package calculations at the time of those (unintelligible). They try to simplify it for formulation of revised source term.

The source term parameters which were quantified in this report basically their timing may be as a result of source and code term package calculations. Some insights were obtained from NUREG1150 (unintelligible) elicitation, basically we did, as I mentioned before, uncertainty analysis for releases into containment.

We kind of organized all the results of (unintelligible) elicitation and source term issues of importance in containment source terms and tried to see really what are the parameters which controls the releases.

As far as grouping of radionuclides, I come back to the regrouping that has been done on the part of 1465, but as far as grouping is concerned it is based on WASH-1400 which examined the spectrum of fission products and came up with 7 major groups on the basis of 54 radionuclides.

Efforts associated with the source term code package development further analyzed this grouping and expanded the seven fission products groups basically in terms of release characteristics and chemical behavior. They made these groupings that went from (unintelligible) noble gases, i.e.: Cesium, Tellurium would be the first species on each group. The first elements in this group to identify the group. Nine groups. This is basically the nine groups that has been used in NUREG-1152.

As I come back to this later on as a part of 1465, we looked at the (unintelligible), they were very close as far as release. We grouped them together as one group.

Based on some work that Sandia has done on the relative importance of each element, they came to the conclusion that there are some other elements that has the significance to sequences if it releases too high -- I mean, large degree so they were included in seven and eight.

I'm coming back to the regrouping later on. Basically the basis for the grouping of 1465 was 1400 and some regrouping has been done based on 5747 and the Sandia report on relative importance of each element.

As far as early in-vessel release --

DR. KRESS: If those groupings were lined up by primarily relative volatility, how do you factor in important (unintelligible)?

DR. NOURBAKHSH: I think they release each at one time for the same amount and then they look at the sequences, the rate based on if they release one at one point and then look at this sequence effect.

DR. SCHAPEROW: There was some work done by Sandia back in the mid-'80s when they were concerned about which elements we should be concerned about. They assumed each element was released in equal release fractions and they calculated consequences for max code. Everything in the max code and they sorted it out.

DR. KRESS: Yeah, but that just tells you what to stick in that line or you stick something in that line. As far as a release fraction, all you're worried about is the particle part.

DR. NOURBAKHSH: Yes. But as far as grouping, they wanted to -- when they defined their groups what else you have to put in that group. For sequence calculation you have to put Cobalt 2, for example, may be important.

As far as early in-vessel release, in terms of terminology of NUREG-1150, it's really simplified. If you make it simplified, it would be FCOR(i) times FVES(i). This is source in-vessel for a species (i). Basically FCOR(i) is fractured off initial core inventory which is released during the vessel phase of accidents from the fuel into the RCS.

FVES is basically the transmission factor. How much of it which is released basically, the fraction is going to retain. A fraction of it minus that is going to be transmitted based on the fraction of demand which is released to the vessel which is eventually released into the containment.

So the NUREG-1150 source and group, there was elicitation on FCOR and FVES depending on the different cases that they considered. If you look at the result of elicitation as well as mean and median, the detail of this distribution is available in 5747 so I just pulled out the (unintelligible).

Basically first make a distinction between PWRs and BWRs experts. Not all of the fuel experts, may one out of four make recognition emphasizing that nuclear observation may be important. They have cases for high Zr oxidation and low Zr oxidation.

Then basically the importance of Zr oxidation and release is more pronounced on (unintelligible) because unoxidized Zr react with the tellurium so that is one reason. As you will see the differences between high and low, as far as release is concerned, is not really that important again except maybe uncertainties.

DR. POWERS: What I'm looking at here, if we look at double gas for PWRs high Zr oxidation, you have 0.92 and then you have a parenthetical of 0.83. What are the distinctions between them?

DR. NOURBAKHSH: These are median values of the distribution. The practice is the mean distribution. If the uncertainty range is very low --

DR. POWERS: It seems to me that it would be surprising if I could have a median that is higher than the mean.

DR. KRESS: That surprises me, too.

DR. NOURBAKHSH: A lot of the tails of this distribution sometimes controls the mean. The mean is not especially. These are just using -- the mean is usually much higher than the median. I agree with you.

DR. KRESS: That's why I was --

DR. NOURBAKHSH: But for all -- I mean, as far as noble gas considered, you release all of them basically in 1465.

DR. POWERS: It's going to be skewed. It's going to have to be an interesting skew to have a median higher than the mean.

DR. KRESS: I've never seen a skew like that.

DR. POWERS: I don't even know what it would be.

DR. NOURBAKHSH: If you use only percentile, the rest of it was done by extrapolation.

DR. KRESS: I interpret this high and low Zr oxidation to have something to do with type of sequence you have.

DR. NOURBAKHSH: Yes.

DR. KRESS: That's the total energetics of the whole sequence.

DR. NOURBAKHSH: Exactly. When you have the presence of how much steam you have as far as (unintelligible) of course because some

of (unintelligible) anyway. They don't oxidize (unintelligible) some of these sequences.

Basically they came up with questions of high Zr and low Zr like the definition, if I'm correct, they use 21 percent as the borderline. If this is more than 21 percent Zr oxidation, it's high. If it is less, it is low. By definition they use high and low Zr oxidation. Again, it has been sometimes (unintelligible) reports.

Here you look at the structure and the release is for (unintelligible) because the uncertainty is much narrower so you see mean and medians as very close to each other. When you are going to (unintelligible) you see the difference between mean and median (unintelligible) magnitude, sometimes more. This is because you have a wide range of uncertainties. When you do calculate the mean usually (unintelligible) controls the mean.

DR. KRESS: So you're making a dire judgement that you don't believe the tails given to you by the experts when using the mean?

DR. NOURBAKHSH: No. All the judgement that I'm making is that using mean value, the bottom line number mean value may not be a good representative of the number. You have to look at the whole distribution.

DR. KRESS: That's paramount to saying you don't believe the tails.

DR. NOURBAKHSH: Maybe one of the experts.

DR. KRESS: Okay.

DR. NOURBAKHSH: Even if one of the experts --

DR. KRESS: That's fine. So you are

saying --

DR. NOURBAKHSH: I'm saying this to all experts and then look at the whole distribution.

DR. KRESS: Okay. I really don't understand the logic between choosing the mean versus the median.

DR. NOURBAKHSH: They don't question mean and median from the experts. All they use from the experts --

DR. KRESS: Is a distribution.

DR. NOURBAKHSH: -- from cumulative. What is the 5 percent, 10 percent on up to 100 percent and you have to fill up those numbers.

DR. KRESS: Yes.

DR. NOURBAKHSH: When you calculate the mean, then it's very much controlled by the (unintelligible) because it's order of magnitude.

DR. KRESS: I understand that but why throw it away is the question.

DR. NOURBAKHSH: But there was some recognition of that when we got some comments on that 5747. Actually, the same comment from 1465 that some thought that using median is more appropriate than mean.

DR. KRESS: That's the loss I don't understand but go ahead.

DR. NOURBAKHSH: If you go back now to --

DR. TINKLER: Well, were there any cases where we used median? I don't think so.

DR. NOURBAKHSH: Most of the cases we used mean values.

DR. TINKLER: I don't think we ever used --

DR. KRESS: But the 1465 tables are means.

DR. TINKLER: Those are means with a couple exceptions but we didn't use the median.

DR. KRESS: I misunderstood then. I thought he was going to use the mean.

DR. NOURBAKHSH: No.

DR. SCHAPEROW: Part of the rationale for using the mean is also they were larger than the medians.

DR. KRESS: You guys need to be conservative.

DR. NOURBAKHSH: Sometimes the mean for this sometimes were higher than 75 percent.

DR. KRESS: Oh, sure.

DR. NOURBAKHSH: That's why they decided to use 75 percent.

DR. TINKLER: In a couple 75 percent.

DR. NOURBAKHSH: So looking at transmission within RCS, again the cases that were recognized was the pressure -- I mean, the conditions for (unintelligible) the transmission were sequences.

In fact, sequences was recognized with significant pressure in the RCS. Of course, when you have a high pressure sequence, you have much more residence time inside the vessel for removal then when you have a large local you have very big high steaming rate in vessel and then you don't

have really that much residence time for removal. As you will see, the low pressure transmission is much higher than low pressure.

High pressure (unintelligible) basically would be for cycling. High to median pressure, first of all, actually categorize high and intermediate and then came to conclusion they should combine two cases together. It is between 200 and like 1,200 would be like high pressure and intermediate pressure. Then low pressure is anything lower than 200. This is RCS pressure.

DR. BOYACK: Let me go back and ask a question for Charlie. Earlier on in the discussion was a question asked about scenarios. One of the things I'll do as we get started, I would like to fill in the table that talks about the set of conditions that we're examining here for this source term applicability. I just barely got through writing down other scenarios. Would you, at least, by the time we come to that place thought a little bit more about that and make sure --

DR. TINKLER: Yeah, I've been thinking about it ever since we talked about it, as a matter of fact, because I guess in my mind, too, I distinguish between events that proceed at low pressure and those that fail the vessel at low pressure but which much of the core damage may have proceeded at higher pressure.

this is the presumption that core damage proceeds under these conditions and is representative of a release path to the containment. We have -- I mean, many of our accidents which at least proceed initially at high pressure actually is on a depressurization prior to vessel failure.

DR. NOURBAKHSH: But depressurization before significant core (unintelligible)?

DR. TINKLER: Well, the amount of oxidation, for example, which develops at high and intermediate pressure can be relatively high if you try to do the signature of oxidation. The signature of oxidation is quite --

DR. NOURBAKHSH: It's independent.

DR. TINKLER: It's quite -- well, it's quite consistent with if you were using core type models it's quite consistent with the volatile releases. The volatile releases track oxidation in our core models very closely. Releases from the fuel of iodine look pretty close to our oxidation fractions core wide. Lots of these sequences.

It's just that the iodine then hasn't been released from the RCS to the containment. Those same high pressure sequences evolve into low pressure sequences prior to vessel failure for many of those calculations, at least for TWRs.

But I guess given the fact that we use this for a design basis calculation, you're pretty hard pressed not to focus, I guess, on the low pressure scenarios the same way that you did before.

I would point out, too, that I think many of our high pressure sequences will evolve into low pressure at the time of vessel failure because we've depressurized the system.

DR. NOURBAKHSH: But you will depressurize even before (unintelligible).

DR. TINKLER: Yeah, I understand but --

DR. NOURBAKHSH: -- future reactors that always --

DR. TINKLER: For depressurization.

DR. NOURBAKHSH: The depressurization system there in the designs. That was actually one of the factors which was considered in using low pressure sequence in 1465.

DR. KRESS: Did I interpret that table right for PWRs low pressure sequences the only release about a third of the aerosols?

DR. NOURBAKHSH: No. This is 30 percent of what has been released into the core.

DR. KRESS: Only 30 percent of those make it out.

DR. NOURBAKHSH: Yes. The 70 percent retained inside the vessel.

DR. KRESS: For low pressure sequences.

DR. NOURBAKHSH: From the low pressure sequences. Again --

DR. TINKLER: That was another point. My recollection of this is that even for the low pressure scenarios we are retaining about half of it, a half to two-thirds of it.

DR. KRESS: My recollection was about 70 or 80 percent. That number surprises me. I'll have to go back.

DR. POWERS: Let me inject a note on this deposition fraction. When we have compared the best models we can, when (unintelligible) in the reactor cooling system against what is observed in the Opedis experiment typically the calculated deposition is twice what is observed experimentally. You're looking at the results of deposition calculations.

DR. NOURBAKHSH: These are not early calculations. They are not specific calculation. STCP expressed and have knowledge of experiments at the time when they put those --

DR. POWERS: They are --

DR. KRESS: There were very few experiments.

DR. POWERS: But they are based on experience that has gone into the tubes, which is now overproductive by a factor of 2 on the Opedis experiment.

DR. KRESS: I'm now talking about strictly the aerosol.

DR. POWERS: That's what we're referring to. I mean, these are aerosol deposition calculations by and large. Now, built into them, of course, is how much gets turned into an aerosol by that time, but it's nearly all of it in these calculations. They're essentially aerosol calculations.

DR. SCHAPEROW: These numbers do look a lot like the source term code package numbers also in your report, 5747.

DR. NOURBAKHSH: That 5747 basically puts STCP calculations, too. I'm not putting them here because they are not really not much use for 1465 except the bounding values.

DR. SCHAPEROW: Actually, the F-VESSEL number, for example, for the Surry AG -- I take that back. For the low pressure sequence, Surry AG sequence, the Cesium F-VESSEL number is point 87.

DR. KRESS: That's what I would think.

DR. SCHAPEROW: This was the high pressure data.

DR. NOURBAKHSH: No, these are not source term code (unintelligible) so it's the mean and median of the distribution so may they use the source term code package calculations as 75 percent. They will recognize the source --

DR. KRESS: Dana is saying, though --

DR. BOYACK: Could you pick a place in here for a break in the flow of conversation?

DR. NOURBAKHSH: In the flow of conversation? Maybe when we finish one phase of accidents.

DR. BOYACK: Just tell me when we get to that point.

DR. TINKLER: I would also, I guess at this point -- Dana, I was curious about PHEBIS and whether or not you distinguished between those tests that tend to simulate cold leg break and those that are recoupled from the steam generator.

DR. POWERS: Well, Charlie, as you well know, all of the PHEBIS tests have essentially run the flow up through what amounts to a steam generator tube. The dominant deposition of their calculation, calculating is a combination of thermopoetic and turbulent deposition.

It's in the hot leg of that, too. It's right at a factor of two higher. The assumption that's built into the code is that the flow within the tube is well mixed. You can get the right number by assuming it's not well mixed.

In other words, if there is a turbulent core in a deposition annulus, and if you calculate only the things that have a time and pass across that annulus, you'll get the right number.

DR. KRESS: Coincidence?

DR. POWERS: I have no idea.

DR. TINKLER: I don't know whether the panel will think this provides further illumination to the subject or not, but we have done more recent calculations, interval calculations and looked at the amount of deposition in the RCS using system-level codes, which, to varying degrees, model deposition in the RCS. MELCOR.

We did it as part of the re-baseline and we're doing it now as part of 5044. The re-baselining work is available. We did look at large pipe rates, which were depressurized events.

The only thing that we are now seeing a little more as we model the RCS in greater detail with our system-level codes such that we model circulation within the vessel and circulation of steam and fission products to cooler portions of the vessel and the small circulatory paths in the RCS, we do see deposition because we are now distributing things with natural convection flow more than we did before where we had single thermal hydraulic control volumes. That, to a large extent, influenced their deposition.

Now we are getting circulatory paths to the upper internals, upper head depending on the amount of significance you give those kinds of calculations, they can show relatively high depositions for these kinds of scenarios, too. I don't know.

The one report I know is a published report that we could make available that was done as part of the re-baselining which could shed some additional light on this. Perhaps we could present some additional information on some other work that is ongoing, too.

DR. KRESS: My impression was you only got that kind of enhanced recirculation and thermal hydraulics when the sequence pressures were relatively high.

DR. TINKLER: That's true.

DR. KRESS: And the low pressures were --

DR. TINKLER: That's true. That is quite true. As I say, in every baseline calculations we have somewhere, a pipe break was used as the initiating event. So it's low pressure and you wouldn't have seen the same kind of circulation set-up.

To the extent you think the models in the RCS deposition models are better than they used to be, they provide an update to those kinds of mental calculations that the committee used to put in the source term code guide.

DR. BOYACK: When the French were not able to attend, basically what I did with the agenda is just left that space between 3:00 and 5:00 and said "Participant Identification and Discussion of Other Data."

If you want to take a portion of that time later this afternoon and talk to us or have somebody on the staff talk to us about these other things that have been going on -- Boev (phonetic) calculations -- that would be helpful.

DR. NOURBAKHSH: So these are the drawings of the same high pressure, low pressure. There is basically (unintelligible) low pressure and then this was one of the experts in NUREG-1150 looking at CRV flow and ATWS, that sequence.

DR. KRESS: Now these BWR releases, were they into this suppression pool?

DR. NOURBAKHSH: No. These are releases into the contain -- what is getting out of RCS, not into the suppression pool for BWR. What comes up before going to suppression pool.

DR. KRESS: Okay. That's why these numbers are relatively high. You've worried about suppressions.

DR. NOURBAKHSH: Yes.

DR. GIESEKE: I have a question. Is it break time?

DR. BOYACK: I think I heard a request. We can take a break now. Let's take 15 minutes and let's come back at 25 after 10:00 by my clock. Fifteen minutes whatever it turns out on yours.

(Whereupon, at 10:17 a.m. off the record until 10:44 a.m.)

DR. NOURBAKHSH: Those two distributions that we discussed for FCON and FVES, releases into containment and transmission releases into the vessel and transmission to the containment, just multiply together to get a probability. The mean and median values of the FCOR in-vessel -- I mean, early in-vessel releases to the containment. The main multiplication meaning is there, so basically multiplication is gone and F-VESSEL would be the median. Again, if you look at all the permutations of RCS pressure and zirc oxidation for PWRs. So I put here for the sake of NUREG-1465 and look at the low pressure and high zirc oxidation basically. As I mentioned before, the in-vessel into the containment the uncertainty for normal times is quite a lot fewer of magnitude so that's why you see significant difference between mean and median.

DR. KRESS: If you did a high zirc oxidation low pressure PWR with MELCOR, what would you get?

DR. NOURBAKHSH: What do we get? I don't have the number in front of me but I am sure there are some calculation. For iodine and Cesium I don't think -- this may be a little bit lower I would say as far as source term code package calculations which I don't think would be that much different as far as the modeling of low-pressure sequences.

DR. SCHAPEROW: We had pretty high numbers for iodine for the low pressure. We had like 60 or 80 percent. We had big numbers. For the low pressure, we had to run a two-inch break. You were telling me you looked at those and said the numbers were a lot lower, like 20 percent.

DR. TINKLER: We just did the station blackout and we were getting through the major in-vessel degradation phase, we were getting about -- on that one we were getting about a 20 percent release into the containment of iodine and Cesium.

Similar volatilities. We were getting about 20 percent, which is considerably lower than we got for the large pipe break scenario. We were getting a lot of deposition.

DR. KRESS: Most of that was at high pressure.

DR. TINKLER: Most of that was at high pressure. Most of that was at high pressure. Most of the oxidation occurred -- the failure, the induced thermo failure was predicted as a result of the oxidation so it was about at the end of about a 50 percent zirc water oxidation and the loop is predicted to fail from thermal rupture.

We had already deposited a good bit of it. We don't resuspend a lot of it so what had already deposited is there. As I recall, yeah. When we did the re-baselining, we did two kinds of calculations: one where we

force fed 1465 into MELCOR and another where we let MELCOR calculate and we were getting high releases in the containment for the large pipe break low-pressure scenario.

DR. SCHAPEROW: They were never higher than 1465. That certainly got our attention but that's expected. It's not a double-ended break from magnitude. It's more of a station blackout or two-inch break, not representative of a severe accident.

DR. KRESS: Sort of taking the frequency into effect.

DR. SCHAPEROW: Yes. That's correct.

DR. TINKLER: Clearly, the timing was different. The timing was faster.

DR. NOURBAKHSH: Again, for BWRs there is a mean and median of numbers. Basically again these are more (unintelligible). If you look at the whole distribution, as I said before, it's much more representative of uncertainties than going to mean and median. These are the distributions.

Basically we used FCOR and FVES and multiplied them, then we came up with this distribution. These are 95 mean and median. As you would see, some of these releases, the mean acquired I, iodine, is almost 75 percent high. More times, the uncertainty is much lower. So that you see the means and medians are very close.

The same for BWRs. Again, there are more of these distributions for other cases. These are for low pressure high zirc oxidation. This is in the distributions in the appendix. Some of it has been reproduced in 1465.

DR. KRESS: I'm surprised that the tellurium releases aren't higher. Tellurium is relatively volatile compared to iodine Cesium. I've always considered it more volatile than Cesium.

DR. NOURBAKHSH: I think this is high oxidation. An oxidized zirc alloy may retain some of it in the rest of it.

DR. KRESS: It certainly would hold.

DR. NOURBAKHSH: That is maybe one reason.

DR. KRESS: If it wasn't for that, I don't think the tellurium would come out just as much as the iodine and zinc.

DR. NOURBAKHSH: A few words about these uncertainties. For the release of the low volatile nuclides, the mean value of the uncertainty distribution is controlled by the upper tail of the distribution and the details of the whole distribution may be more indicative of the uncertainty than the "bottom line" results, such as a mean value.

There was work done on the part of the Advanced Slide Model Reactor Program of DOE at the time. It was in the 1993 time frame. Also, Osetek did some calculations using the results of the in-pile experiments of PBF and some data from post-examination of the Three Mile Island accident and came up with some rate --

DR. BOYACK: Let me stop you for a minute. Are you getting this on the recording?

COURT REPORTER: When you drop, it's getting pretty difficult.

DR. NOURBAKHSH: I'm sorry.

For the post-accident examination of TMI-2, Osetek's estimated values for low-volatile releases came close to the median values of the uncertainty distributions.

DR. KRESS: I would have had trouble in connecting those.

DR. NOURBAKHSH: So, basically, what I did, there was ALWR program recommendation in the DOE report for advanced reactor releases for low volatiles. This is 5747, the mean and median, and the 75th percentile. This is ALWR program release estimates and also the values, basically.

Again, there was some assumption done on the release durations. But, again, it depends on how much is vessel release duration -- I think it's ten hours -- then the estimated rate based on that duration. It's very dependent on that assumption. The numbers are not that far from the median values of NUREG-1150 numbers.

Actually, we recommended 1465. When the mean is more than the 75 percentile, take the 75 percentile. That is basically what they did.

As far as onset of release as far as any vessel release is concerned, these are really the timing for when you see significant releases into the containment. We examined the specific calculation results available at the time. Then there was pitch fork of calculations of sequences not on Peach Bottom, so they used MELCOR to come up with basically the duration of the release.

Again, release to the containment depends on the sequences. Then for low -- and the pressure actually, whether you are -- onset of release depends on many factors. We just pick up a shorter stand which

was 10 minutes for the sequence that was calculated at the time for PWRs and an hour for BWRs after the onset of the accident as far as the timing is concerned. I think 1465 picks up thirty minutes for both PWRs and BWR.

As far as duration of in-vessel release, from the time of significant release to the containment to the time of vessel failure, again we look at the STCP calculation and we found out again that release duration depends on the reactor type and accident sequence. But duration was thought to be somewhat longer for BWRs than PWRs largely due to lower power density in PWR plants.

DR. KRESS: Let me ask you about that. It seems to me like most of the release comes during that oxidation phase and the rate of energy developed there has very little to do with power density.

DR. NOURBAKHSH: The amount of Zircoloid is a lot, too, in BWRs.

DR. KRESS: There's a lot more but a lot of it acts as a heat seal.

DR. NOURBAKHSH: Yes.

DR. KRESS: I would have thought it had something to do with thermal hydraulics.

DR. NOURBAKHSH: I should correct myself on that.

Another thing which is really important is the pressure inside. The overhead failure mode because when you have high pressure the way the STCP model is, it has the stresses included as inside pressure so you tend to have that for BWR. Maybe that and other factors. That's what I think.

DR. POWERS: Maybe I'll just inject a comment here. Most of these calculations are based on models that assume release as a function of the temperature and they don't look at release being a function of either mass transport of the core debris or mass transport of the gas.

One of the things that comes so clearly through in looking at the PHEBUS result is that temperature is just one variable. When you get fuel relocation you get a lot of aerosol generation and, consequently, a lot of radioactivity release.

If you look at a BWR as a bunch of channels which are many cores that can relocate kind of piecemeal wise, whereas a PWR is a core that slumps together and you only get big episodic relocations, that might change the release as you see from BWRs versus PWRs.

DR. KRESS: Wouldn't change the calculations.

DR. POWERS: Doesn't change the calculations but it changes the way you do things because here there's nothing built in here that says when I relocate fuel I get big aerosol burst. That has become the way they can tell when they are getting relocations. They get spikes in the opacity in the gas passing through the system.

DR. KRESS: Is that key to the stuff falling into the water?

DR. POWERS: There's no water.

DR. KRESS: Water is down?

DR. POWERS: Just taking in steam. I suspect what you are seeing is mass transport effect. Whether it is a mass transport effect due to accelerated gas flow or just surface renewal as material flows in I'm not really certain. Clearly what is important here is the BWR can be looked at as a whole

bunch of little PHEBUS experiments whereas PWR takes more collectness in its behavior.

DR. KRESS: In fact, when Steve Hodge looked at BWRs he treated them as things separating channel by channel.

DR. POWERS: Yeah, channel by channel. If you believe the flow does make a different in aerosol generation, you can do release fractions.

DR. NOURBAKHSH: Representative early in-vessel release durations were selected to be 1.3 hours and 1.5 hours for PWR and BWR plants respectively.

Just to give you an idea of this timing of the results, this has been taken from NUREG/CR-5747. For in-vessel release duration I haven't put the pressure here but 1465 has a pressure associated with each sequence as in the table so they are combining these two together.

This is early in-vessel duration for PWRs. One and a half hours was basically presented for release duration.

DR. POWERS: The low pressure ones have 1.3.

DR. NOURBAKHSH: 1.3, yes.

DR. KRESS: If you take something like adeimatic heat-up and the heat-up rate driven by steam oxidation and take all the fuel up core melt, you get about this time. Is that what drives this? What I'm trying to figure out is what is the AG? How does the AG end up being the order of magnitude? Why is it different than the others?

DR. NOURBAKHSH: I think it was different, correct. It has been more than 10 years. AG has some injection into it. That's why it is delayed, partial injection.

DR. KRESS: So you don't use any heat-up?

DR. NOURBAKHSH: It is very short, yes. This AG here is much longer in vessel release time of in-vessel. It was partial injection at the very beginning and then --

DR. TINKLER: It's just got to be a curiosity.

DR. KRESS: It has to be. You just have to discount it.

DR. TINKLER: Normally, and you made the point, in 1465 parts the ratio release is kind of like a composite of each duration and the onset. That's the way I -- frankly, it doesn't take an hour and a half for the dirt water oxidation to get this thing fired up and released from fission products.

Thirty minutes is closer to the duration at which you are really cooking and releasing volatiles in just the temperature models. That doesn't speak to the transport out of the RCS.

I think we kind of look at that release duration as something that covers lots of parts of this scenario, the delay of the onset as well as the release duration because, frankly, time periods of an hour and a half are long for a metal-water reaction.

DR. KRESS: I feel that durations as the core level, water level, starts going down, you are uncovering tops of the core first. That portion then starts undergoing a thermal hydraulic heat-up which is about half of what you get in the 80 matic.

Then it gets up to a level where it sets off the zircoid reaction and that increases the rate at which it goes up until that portion of the fuel actually reaches some sort of maximum temperature like a melt temperature.

At the same time the core is still going down and this is happening to different segments of the core as you go along. It's a combination of how fast you are uncovering the core and how fast the parts are uncovered go into adeimatic heat-up and light off the zircoid reaction. If you make those calculations, you get these times, except for that AG which I don't understand.

DR. NOURBAKHSH: AG has to do with the definition of the particular sequence or the way that they calculate it.

Another factor which is we are assuming a constant rate even though volatile releases isn't early, but for low pressure you have puff release, too, which is included in the in-vessel phase release. It is at or before because you are releasing -- I mean for high pressures -- you are releasing it into that. As soon as that vessel fails it comes out, some of the volatiles.

For simplicity we are putting a constant duration and a constant rate so it has to be more controlled by the vessel failure time, too, than the release -- I mean, how much of that time you are experiencing high temperature.

For Peach Bottom there are typical numbers.

DR. POWERS: Dr. Kress, on your mental calculation, would we suggest then that if we go to higher-power cores with Peach Bottom, LaSalle, and Grand Gulf that we would shorten these times by the appropriate amount but the duration would be the same because it's driven by the Zircoloid burnup?

DR. KRESS: That's exactly right. Actually, the times have also changed because I think at high burn-up you start releasing fuel at a lower temperature. I think the temperature release relationship changes also.

DR. NOURBAKHSH: So that was the early vessel release. Now, as I said, 1465 does not consider releases due to high pressure, melt ejection, or ex-vessel extreme explosion. The next phase would be basically ex-vessel releases due to core-concrete interactions.

Based on the terminology of NUREG-1150 the ex-vessel release for each species would be the amount of core which participated in core-concrete interaction. That parameter was there in order to treat the basic ability or the issue of how much actuated debris is retained in the vessel.

Then the amount of what is in the melt coming out due to core-concrete interaction, again we are not in this including the DF decontamination factor for overlaying water on the core-concrete interactions. It's a dry cavity, basically. You can consider it that way.

We are looking at it but what gets out of -- I mean, the core debris into the overlaying water, that DF has to be treated separately as behavior in the containment basically.

Then basically what is in the vessel, what is from the initial inventory in that melt is 1-FCOR because you have already released FCOR during in-vessel progression. So 1-FCOR times FPART times FCCI is a parametric presentation of releases into the containment due to core-concrete interactions.

Again, for our interest we assume FPART is equal to 1 even though we did some sensitivity in 5747 for using .4 or whatever. So FCCI times 1 - FCOR(i).

If you look at the FCCI of 5747 distribution, which is basically the aggregate distributions of -- cumulative distributions that NUREG-1150 documents, we just got the mean value of that basically.

Then the issue was characterized in NUREG-1150 expert panel as concrete type for PWRs and whether the cavity is dry or wet and Zircoloid content in the melt. Of course, that's the source for additional energy if you have highly unoxidized Zircoloid.

Then basically that's the conditions that you will see and they have produced distributions for all of these. Then basically all the volatiles are going to have to be released from core-concrete interactions to some extent.

Tellurium, and then again I think there is retention. Then we have a strontium nonvolatile which is much lower release. Again, just because we are discussing the grouping here in NUREG-1465, for all practical purposes the releases of strontium and barium groups are elements of the same, so it's not a bad assumption to combine it.

Again, on the ex-vessel release they didn't have really all the plants which were available, they didn't distinguish between basaltic and limestone. If you look at those distributions, really the difference in limestone and basaltic concrete as far as result of these distributions were not really that important. Maybe from what I look one of the distributions basically dominated the results.

Again would be (unintelligible) condition concrete type which is (unintelligible) and they come up with the zirconium content in the melt. There are STCP calculations done. We actually did ourselves some calculations assuming basaltic concrete for the PWR.

DR. POWERS: I think most of these core-concrete release fractions were actually calculated with the VANESSA module and COR-CON.

DR. NOURBAKHSH: The one that was under STCP calculations.

DR. POWERS: When will you have --

DR. NOURBAKHSH: They have mental calculation. They have this in front of them. These are expert elicitation.

DR. POWERS: When we have compared the VANESSA calculations against experimental data, they match pretty well. Everything except the ruthenium release. VANESSA tends to predicts extraordinarily low ruthenium releases and experimentally they observe things up in the vicinity of 1 percent. I would look with a jaundiced eye on these ruthenium releases.

DR. KRESS: According to this it doesn't matter how much zirconium there is. It doesn't matter whether it's wet or dry.

DR. NOURBAKHSH: If you look at them, yes, because even though they distinguished these cases when they came up with the number then aggregated it, it didn't show up that much.

DR. POWERS: I think that the reason the wet doesn't make any difference is that all the scenarios with water in the cavity it saturated and at the time there was a widespread relief that boiling water pools didn't retain. In fact, the experiment showed boiling water pools retained just fine.

DR. NOURBAKHSH: You see here, if you look at the median value, the ruthenium value is much lower than the rest.

DR. KRESS: I'm surprised that the zirconium doesn't matter. That's really what drives the energetics.

DR. POWERS: Well, it's worse than that. It drives the chemistry and it makes strontium and barium releases very high. These are expert elicitations.

DR. KRESS: Ah, that's why they don't.

DR. POWERS: Yeah.

DR. KRESS: If you did the calculations, you probably --

DR. POWERS: When you do the calculations, especially BWR sequences where you have a lot of zirconium metal coming ex- cavity, you blow the strontium out. You just can't keep it in.

DR. NOURBAKHSH: The result of the STPC has been grouped very similar to NUREG-5747.

As far as ex-vessel release duration is concerned, the release starts after the end of onset of -- it would be like after in-vessel release. You look at the STCP and even though the core-concrete interaction may happen for many hours but most of the releases as far as except tellurium and ruthenium, they have been 90 percent of release if you look at the results of the graph happens in maybe two hours PWRs and three hours for BWRs. The tellurium and ruthenium extends much more, five or six hours.

DR. POWERS: In fact, what you see here is a period of intense release is when you have a zirconium level present.

DR. KRESS: And then it quits.

DR. POWERS: Zirconium and chromium will drive these things because you are getting the additional chemical heat. Once you lose that, once you have knocked the metal phase down so it's primarily iron, then it becomes kind of a percolating --

DR. KRESS: It goes on for a long time.

DR. POWERS: It goes on forever.

DR. KRESS: I'm surprised that the experts sort of discounted that.

DR. NOURBAKHSH: This timing was not done to help it.

DR. KRESS: Well, I mean in the release fraction.

DR. NOURBAKHSH: Release fraction, yes.

DR. POWERS: Do we know that at the time there had been some experiments done at Argonne in which they had put oxides onto the concrete without the metal and I think that was biasing a lot of the expert opinions.

DR. NOURBAKHSH: And some of the result of MAP view of core-concrete interaction has dominated this.

DR. POWERS: Its influence.

DR. NOURBAKHSH: Especially when you have wet ones. If you look at them, this was the impact of the way that MAP looks at overlaying water.

DR. TINKLER: Subsequent tests at Argonne actually show that zirconium burns very quickly. I doubt whether they got any that lasted anywhere near as long as two hours in their test.

DR. POWERS: That's because they couldn't keep much zirconium in there.

DR. TINKLER: Well, that's true. That's true. I mean, they couldn't have more than about 10 percent metal in their test.

DR. POWERS: They probably lost half of that just in the heat-up.

DR. TINKLER: They saved a little bit under our insulator boards so they had something to start but it burned quickly. All evidence suggested if it was molten it would burn pretty quick.

DR. POWERS: Zirconium burns at the rate it gets oxidated prior to it. There's no kinetic limitation. What happens in these big pool-mouth dumps that you get from the in-vessel, you just can't pull water out of the concrete fast enough.

DR. TINKLER: They may be limited by the total amount they had in there.

DR. NOURBAKHSH: As far as late in-vessel release which is basically mostly due to revolatilization, we are assuming nothing left as far as debris in the vessel. Everything that is released is what is retained in the vessel.

If you look at the parametric representation of that release, it would be FCOR times 1 minus FVES is the amount which is retained in the vessel. Then you multiply to FREV. I just put FREV notation for that.

Fraction of radionuclide group (i) in the initial core inventory that is released from the vessel into containment much later in the accident.

Again, assuming it happens right after the vessel failure.

Again, these releases has to do with the temperature in the vessel at the time late in the accident and the ability of the flow in order to bring that into it. The cases that they have looked at basically for PWR is whether you have one opening after vessel failure or you have two openings of the vessel breach. This provides a path for natural circulation between containment and the rest.

Then for the BWRs, again the water injection after vessel breach was one factor because if you inject water you cool down the vessel and you don't really have that much revolatilization. That's what you see as far as numbers for these and these.

Again, these are the fraction of what is retained in the vessel. So 2 percent for iodine and Cesium and median values 5 percent. Tellurium, actually, the median was -- some of these experts didn't believe the tellurium revolatilization rate.

To summarize these late in-vessel releases, now you look at the in-vessel. This is again 5747. I took the one which has more implication for 1465 representative, low RCS pressure, high Zircoloid oxidation. You have a dry cavity because DF water is going to be treated separately. Two openings of the vessel breach basically is representative of low pressure.

When we did these calculations we look at the consistency of all this so we are propagating when we are using FCOR of high zirconium oxidation so for core-concrete interaction we use high oxidized zircloy. When

you have high zirconium oxidation in vessel, it means you have no unoxidized zircloy for CCI. We use the formula logically here on an assumption basis.

For working vessel releases, noble gases are almost always released in-vessel phase. For iodine in-vessel phase this is early vessel, this is ex-vessel, this is late vessel.

DR. KRESS: The calculations for revolatilization in NUREG-5747 strictly assumed the chemical species was the same and looked at --

DR. NOURBAKHSH: We didn't really calculate. These numbers basically are all propagation of expert elicitation.

DR. KRESS: NUREG-5747?

DR. NOURBAKHSH: Yes. What I'm talking about is these are all --

DR. KRESS: I'm strictly talking about the revaporization source.

DR. NOURBAKHSH: These are mean values. What is reported in these tables are mean values for aggregate distributions that NUREG-1150 (unintelligible) . So we just took distributions for all of this. We multiplied those distributions and we look at the mean values.

So for low RCS pressure you are retaining quite a lot because they are consistent when you are calculating.

DR. SCHAPEROW: Maybe I can add a little bit to this. If the work that was done in NUREG/CR-5747, which was put into NUREG-1465, this is the volume from the NUREG-1150 study which I know your names are all on. That's one reason they asked you to be the experts on this panel. What they

all needed was the propagated -- you combined the distributions by those simple formulas.

I have copies of this document for all of you. Also I have three other main documents. They are on this bench over here if you want to bring them home or I can mail them to you and you can look at them later.

DR. KRESS: Consider the size if you mail them.

DR. SCHAPEROW: The other ones are real small. This is the big one. This has all the dissertations where you wrote up all your dissertations for why the distributions went the way they do and the mean and the median.

DR. NOURBAKHSH: But we have some bonding values of this which I don't have it here based on source term code package calculations. These are values much higher.

So for BWRs the summary of releases in severe accidents would be in-vessel, ex-vessel, revolatilization of those distributions -- propagation of those distributions.

As far as release durations, basically we didn't have that much at the -- I don't think those STCP calculations. They stopped trap melt calculation at the time vessel failure and they didn't continue that so we didn't have really that much information.

I think, if I recall correctly, Julian did one calculation extending it and we found out it would be like typically 10 hours. Then 10 hours later I looked at some of them. In MELCOR we have the results now. I looked at the initial calculation of MELCOR when MPR was doing it. I thought 10 hours would be appropriate but we really didn't do detailed analysis on this timing.

DR. KRESS: That's strictly a thermal hydraulic calculation.

DR. NOURBAKHSH: No. We wanted to see how long is revolatilization.

DR. KRESS: The time is how long it takes the primary system to get up to a given temperature, given what's there --

DR. NOURBAKHSH: Exactly. When you cool down, you don't have that much revolatilization. At the beginning the system is much higher. After 10 hours you lose a lot of chemical energy, Zircoloid, and the containment becomes cool so you don't really -- if you don't release them early and --

DR. KRESS: But most of that's driven by what is already deposited.

DR. NOURBAKHSH: Exactly. Exactly.

DR. KRESS: So it's a thermal hydraulic calculation.

DR. NOURBAKHSH: We could do some thermal hydraulic calculation with that but we have to look at basically how the iodine and Cesium and tellurium behave after vessel failure.

DR. GIESEKE: What amount do they use for that time during the 10 hours?

DR. NOURBAKHSH: No, we continued trapped melt runs after vessel failure.

DR. GIESEKE: Oh, okay.

DR. NOURBAKHSH: Then look at it -- I mean, if you look there it's very simplified -- assuming containment at high temperature that

vessel stays there and some chemical temperature vessel failure and then continuing.

DR. GIESEKE: Oh, it stays the same temperature you say?

DR. NOURBAKHSH: I believe.

DR. GIESEKE: That's what I was wondering, what temperature you were using.

DR. POWERS: I know that Peter Bieniars (phonetic) did a lot of calculations on that.

DR. KRESS: Richard Dean did some of that for Indian Point.

DR. POWERS: That's what we were doing it for. What Bieniars and I started out doing was saying, did chemical form made a difference on the surface. Then, of course, we found a huge difference. If you had Cesium hydroxide it vaporized practically instantly and it was hard to keep it on the surface.

As you evolve through a progression of things from the ferrites until you got to the forates it kind of revaporized but real slowly. Ten hours seems about right. If you went to the Cesium silicates they didn't come off at all.

Then Richard Dean and those guys picked up those calculations and did it for Indian Point. We came to the conclusion we had a real problem because they were getting fission products into the containment atmosphere after the containment failed.

DR. KRESS: Partly because they deposited a lot in RCS.

DR. NOURBAKHSH: As far as the chemical forms of iodine based on Oak Ridge.

DR. POWERS: So that needs to be reworked completely.

DR. NOURBAKHSH: Maybe I should sit down. Anyway, I just produced three slides. NUREG/CR-5732, iodine chemical forms.

A chemical kinetic model used 20 reactions to determine the control volume that you have frozen equilibrium. By frozen equilibrium means that you cannot reach equilibrium in subsequent control volumes by assuming those concentration temperatures. Then you stop there and then you assume that was released to the containment.

They did separate the equilibrium calculations using the FACT system to obtain the distribution of iodine species.

DR. KRESS: That's because of the fact you can use a lot more than 20 reactions. You have characterized what we did very well. Congratulations.

DR. NOURBAKHSH: Thank you. The analyses were based on the STCP calculated results of seven severe accident sequences for LWR.

DR. KRESS: What that does for you is tell you the temperature, but it also tells you hydrogen steam ratio which is important.

DR. NOURBAKHSH: Then the main conclusion of this report, as well as chemical form of release from RCS the containment. Of course, this study looked at chemical behavior inside the containment.

The main conclusion as far as chemical forms from RCS, the containment was 5 percent as elemental iodine and HI with not less than 1 percent as either elemental iodine or HI. The remaining 95 percent would be CsI. Basically that was the main conclusion.

DR. KRESS: That assumed no boron species.

DR. NOURBAKHSH: As far as chemical behavior in containment, what would be the state of that chemical form in the containment of the release, the distribution with species throughout the gas and liquid phases was estimated from simplified models, the models for radiolytic conversion of I to I_2 in water, and evaporation and gas-phase formation of organic iodides. Actually, we are revisiting some of the postmark using only radiated experiments.

The bonding release fraction that we were assuming how much coming into the containment were used based on the bonding values of NUREG-5747 used. Just summed them up how much it is, iodine and Cesium in the containment with fraction of I.

Then the production of I_2 in containment was found to be directly related to the pH of the water pools. Then if the pH is controlled and maintained at a value of 7 or greater, very little of dissolved iodine will be converted to I_2 but the picture would be quite different if pH drops below 7.

The larger fraction of iodine will be quite high and will be converted to elemental iodine. The detail depends on where you have everything in the containment but it will be a significant part will be released to the container.

DR. KRESS: One of the basic assumptions there was no silver present in this pool.

DR. POWERS: That's one of the results that you get out of the Opedis experiments is that assumption probably is not correct to make. You get a lot of silver from the control rods and when you have that, it

substantially suppresses the radon concentration for reasonable pH ranges, say up to 4. You can't go any lower than that.

DR. KRESS: The other thinking was that some of this release came out of the airborne aerosols which had water absorbed because of the hydroscopic and there would be no silver in that to inhibit the release. It also assumed there was methane sources coming up through the water. That would greatly enhance and do we convert it to organic.

DR. POWERS: And what the Canadians are arguing now is that it's not only the methane that you have to worry about, it's methyl-ethyl ketone coming out of the paint that makes the difference and you get organic iodine from that methyl-ethyl ketone.

DR. KRESS: That actually wasn't actually factored into the postmark.

DR. NOURBAKHSH: As far as iodine behavior, WASH-433 and WASH-434, which is basically a compilation of 69 experiments from different sources, British, INEL. Then they look at the decay of rich organic iodine could form in the containment system.

It was concluded that less than 1 percent of iodine initially airborne could become converted to organic iodides by non-radiolytic means.

Overall, no more than 3.2 percent of airborne iodine could be converted to organic iodides during the first two hours. NUREG-1465, I think assumptions arguing that this rapid taxonomy implication, has important implication for design basis accidents, as far as 1.2 and 1.4. I think they made assumptions of 3 percent would be an appropriate.

What percent are they using right now, 1.2, 1.4?

DR. SCHAPEROW: We're using 4 percent.

DR. KRESS: WASH-433 circa 1968 or '69.

DR. SCHAPEROW: 1972, that is one of the four (unintelligible).

DR. POWERS: Certainly if we look at, flipping through the cumulative experiments to date, and what they argue is that what they call their aerosol phase, the iodine you find suspended in containments, inorganic iodide. They go through a washing phase and then they come to what they call their chemistry phase. They argue that the iodine in the containment at that point is all organic iodide.

DR. NOURBAKHSH: A few words on the development of 1465. The source terms were developed (unintelligible) a complete core-melt accident. The magnitude of the fission produce released was intended to be represented here (unintelligible) except for the low volatile nuclides which was estimated from the mean values for a typical low-pressure.

For non-volatiles, the 75th percentile was selected as an appropriate measure of the release fraction. The timing were selected for a typical low pressure core-melt scenario, except for the onset of the release of gap activity was based on the earliest calculated time, I could find, of fuel failure under accident conditions.

DR. TINKLER: I want to revisit. I was checking with Ralph Myer on a couple of things related to FRAPT and FRAPCON. It's clear that we're still in the process of looking at cladding failures and fuel rod failures which trigger the release of the gap inventory.

That works really just to get it going, test high burn-up fuel rods and build the foundation of those models for the LOCA release. It doesn't work on FRAPCON for release of fission products into the gap from the fuel but whether we're talking about 30 seconds or 10 seconds to 15 seconds, that's looking at high burn-up cladding rods still ongoing -- new ongoing actually. You may not be able to reflect on the data yet on that particular aspect of it.

DR. NOURBAKHSH: Some words on the grouping. As I mentioned before, we look at all source term code package calculations. NUREG-1150 expert elicitation release of (unintelligible) very similar. Actually, (unintelligible) relative older studies of Sandia in 1980 and 1986 that found other elements such as (unintelligible). Group 7 was revised to include Curium and Americium and group 6 was revised to include Cobalt. That's why if you look at the elements in each group in 1465 it is a little bit different than WASH-1400 (unintelligible). They defined the groups basically as (unintelligible). This was reproduced from 1465.

We look at now -- we have just reproduced (unintelligible). We had a little bit change on the total iodine and Cesium in vessel, the releases are very similar to mean values. A little bit (unintelligible). As far as duration for gap release (unintelligible) for simplicity PWR, BWR, they really didn't distinguish between releases of non-volatiles and volatiles (unintelligible) everything is two hours, this is for PWR, and late in-vessel 10 hours. That was the releases. There was a typo in one of those when I looked at it. Was it on --

DR. SCHAPEROW: BWR table.

DR. NOURBAKHSH: BWR, yes. This is basically, you have to correct it to (unintelligible). As far as the duration, except for excess (unintelligible) one half hour the (unintelligible) noble gas is 5 percent, gap releases of 5 percent. But there was a (unintelligible) the Japanese will be 3 percent if long term (unintelligible).

Again, for PWR the Curium has decreased in response to comments that they got to some extent but not that much but the numbers basically appear in 1465. We have time to put these numbers in uncertainty distributions (unintelligible) more than just to have some -- looking at the uncertainty of this based on explained observation of (unintelligible) 50 to see what those numbers stand are included for just the same representative sequences, uncertainty on the total releases in the containment. You have to combine all of those and then compare it to this total -- of the release.

That's for PWR. I have included in my viewgraph the same distributions for -- the same sort of low pressure sequence for BWRs. The summation of those releases should be compared to these uncertainties that perceive to be at the time of NUREG-1150 (unintelligible) situation.

In summary, a review of the sources of information supporting NUREG-1465 source terms was provided. And some brief discussion of sources, of development of source terms. That concludes my presentation.

DR. BOYACK: Okay. Are there any questions or comments you want to make now? We are going to come back and work through these areas several different times. Is there anything right now that you would like to go ahead and comment on?

Okay. It was pretty much done during the course of the presentation. I would like to go ahead and take lunch break now, one hour. We'll come back at 12:45.

At that time I'll go ahead and we'll pick up the agenda item which would be my discussion with you about panel objectives and the process. Then after that we'll move on to JAERI's presentation. Somewhere in the middle of that we'll take a break. Okay, one hour. Thank you.

(Whereupon, at 11:52 a.m. off the record for lunch to reconvene at 12:52 p.m.)

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

(12:57 p.m.)

DR. BOYACK: Okay. We'll go ahead and begin. What I would like to do is talk with you about the objectives of this, right now called, source term applicability panel. Sometimes described, we have a PIRT like characteristics, but I think this captures the set of activities that are envisioned more closely than using the term PIRT.

The two things I would like to do is talk to you about objectives and processes. The NRC has stated the objectives for us and they have done a pretty good job. I think they are fairly clear.

The process is much more open to discussion today and you should feel comfortable going ahead and offering your thoughts on that. I'll invite those as we go along both today and as we continue on through this meeting and the rest of the meetings.

Okay. Let's talk first about the primary objective, and that is to evaluate the applicability of the NUREG-1465 source terms for reactors using two things, high burn-up fuel and MOX fuel. There are two reactor types, PWR and BWR.

I think each of you received letters from FURCAL-1 (phonetic) at the time and that was pretty much it. In specific terms, you can say the following. Determine the applicability of values in the following three tables from NUREG-1465. That was back in February 1995.

Table 3.6 which is the release phase durations; 3.12 BWR releases into containment; 3.13 PWR releases into containment. I think that is essentially what we're after is trying to update those tables.

Now, one of the things I would like to emphasize is there is the table and then there is the information that tells you why these decisions are made. We want to provide the rationale for each of your determinations. There are several vehicles for doing that.

The first of them is meeting transcripts. It's been my experience with meeting transcripts that they sort of work. Quite often there is enough said that it's hard to ultimately figure out what the bottom line was. Although they are very, very helpful, they are mainly, I think, helpful for getting down the ideas.

Then either a facilitator or NRC takes notes during the meeting. Ultimately the panel member reviews the task documentation and those rationales will be the most important way of getting that captured.

I will say along the way to the extent that we're able to do it in the discussion, recorded discussion, if we can make bottom line statements and I'll try to facilitate that, I think we are better off.

The second objective is to identify additional work, if it's appropriate only, to remedy any identifying deficiencies. At this point I'm not exactly sure how this is going to proceed. We're going to go through these tables.

I've already heard a discussion this morning that at some level takes issue with some of the basic information in 1465 tables. I'm not exactly sure as we run these extensions to high burn-up fuel and to MOX fuel even though we're talking about expert opinion whether you're always going to be able to come to a point and say, "Yeah, this is what we think it ought to be."

I hope that's the case but if it's not, then that would lead to this particular area of endeavor which is identifying additional work we need to remedy any identified deficiencies, either experimental data needed, additional analyses needed and, of course, that we would want to go ahead and also provide the rationale for that.

That's the objectives. Is there any questions that anybody has? This is a good time to discuss objectives if you have any questions.

DR. POWERS: It seems to me that the biggest question that we need to clarify is that 1465 was cast at a particular level of technology and that level of technology antedated the 1995 publication date by several years.

In fact, we could probably say 1465 represents the technology that was available in 1989. A lot of water has poured over the dam and our level of understanding of the source term has evolved.

The question comes do you want to update 1465 based on 1989 understanding or 2001 understanding of source code?

DR. BOYACK: I think there is a clear answer to that.

DR. POWERS: I think so, too. But I want somebody to say it.

DR. SCHAPEROW: I'm the NRC. I guess I should say it. We see those as two separate issues. One is improvements since 1465 and the other one is the impact of maybe moving to the next phase into high burn-up or MOX fuel. We are inherently interested in the second thing which is applicability to MOX and high burn-up fuel. We are also interested in improving NUREG-1465. You're right, there is a certain tie-in there. Our primary objective is to apply it to the high burn-up and MOX fuel, but if there

are other divisions you identify along in the regs, you'll have to be careful and try to remedy them.

DR. GIESEKE: You have to go to the new constraints here. Fuels not with 2001 technology? That's the question you're asking. Our objective is not to update 1465 in general to bring that up to 2001 technology. That's not an issue, but it's an issue for the fuels. I guess the answer is with 2001 technology.

DR. SCAPEROW: We want anything new since 1989. Absolutely.

DR. BOYACK: I'm going to strike my presentation. Although my association with this kind of work has been peripheral, I was involved in the MELCOR peer review which was a number of years ago. Of course, MELCOR was the package that was put together to supersede source term code package. I was looking at how much of the information in 1465 was based upon that. It just goes back to the statement by Dana.

I've now heard two different statements. I'm not really sure of where we are about this issue of 1465. You'll notice that I didn't include that.

I really basically said we update 1465 for high burn-up fuel and MOX fuel and I didn't have any statement about the idea that somehow we would also consider the terms that were in 1465. Where are we on that? I'm not sure I yet have understood what we're planning to do there.

DR. SCHAPEROW: I don't know. How many experiments have there been, for example, since 1989? We've done a few regionally at least.

DR. POWERS: I think what you have is all of the PEDIS (phonetic) program going on. Even the scenarios are a little bit different now. Whereas there was a design within the source term code package to look at kind of global core collapse all at once. Now you have a more piecemeal collapse.

There's been a lot of work in looking at vessel head failures and the timing there. I think the level of technology for the ex-vessel source terms hasn't changed but the in-vessel and the core degradation aspects of it there is a different understanding.

DR. KRESS: I think there may be a different understanding of high pressure melt injection since 1465.

DR. POWERS: Well, 1465 really doesn't have a high pressure melt ejection source term in it. It's low pressure.

DR. KRESS: Oh, yeah. That's right.

DR. SCHAPEROW: Our objective is not to track a little better 1465 for current generation reactors and current burn-ups, but I think along the way we are probably going to have that. We are going to explore those issues. The financial thing to do as part of this process is to explore those issues. That's not our primary goal.

DR. BOYACK: You might want to take that as an issue because there was a little bit of discussion about that with Charlie Tinkler when we broke for lunch. It wasn't all that clear.

DR. SCHAPEROW: Those things are going to come up as you discuss this.

DR. KRESS: If I could paraphrase what my understanding is of what you're saying, we take 1465 and say that is the correct recommendation for representative source term at the conditions of the counted fuel. How would we change that for the burn-up and MOX?

DR. SCHAPEROW: That was our original plan but, as Dana points out, 1465 by itself may -- we can make improvements to that.

DR. KRESS: But you still want us to stick to the original plan?

DR. GIESEKE: I don't think Dana was talking about upgrading 1465. He's saying that when we extend it, do we extend it using the information available when 1465 was written, or do we extend it with what we know today?

DR. KRESS: Use all the information we have today.

DR. GIESEKE: Only referring to the extension.

DR. POWERS: As an example take an inherently high pressure sequence in which you follow it in my two scenario except you eventually rupture a vessel. Then you have an air intrusion scenario. That was not recognized at the time of 1465.

There was no recognition of air ingression in the time 1465 was put together. That will have immediate -- that has the potential of having an immediate impact on your ruthenium and molybdenum releases.

You would come in and say, well, during the period that 1465 parlance is called ex-vessel release, you are, in fact, giving a pretty healthy molybdenum and ruthenium release that is not coming from melt-concrete

interaction. It's actually coming from the fuel that remains inside the vessel because it's exposed to air.

So when you had your table as a result of that, this updated table, I have this peculiar thing because of a different understanding of the scenario.

DR. KRESS: The other thought, though, is that design basis specifications you don't necessarily have to include everything.

DR. POWERS: That's right.

DR. KRESS: But they have to be inclusive enough so that when they're used they capture the -- in the way they're used they don't capture enough of these things that the safety status of the plans may think. You're thinking about design basis specification and you kind of keep in mind the frequency or relative probability of these things. At least in your thinking.

For example, I don't know how probable it is that you are going to get an air intrusion accident and should it be such that you ought to protect the incident with design basis concepts. I don't know. I personally think you probably ought to.

DR. POWERS: But it depends on where you stop. I think the way they are using 1465 now for design basis analysis is the right way to use it. They take it up to the vessel release and they stop. Okay? And that probably wouldn't change.

An air ingression accident comes after that and it may not change that. What would change is at the time of 1465 gap release, so that is 1 to 3 percent. Now we're going to look at high burn-up fuel and we're going

to spend a lot more attention on fuel behaviors. The technology we are going to look at is going to be a little bit different.

We'll just look at it in 1465 and we'll go into more detail on that because it's a bigger term. It's just not so readily negligible. Plus you also know that it probably gets more attention now than the people that wrote 1465 ever anticipated it would get.

DR. KRESS: They didn't think it was a big deal.

DR. POWERS: Well, it's not relative to a full core melt accident.

DR. BOYACK: The discussion I hear is that back in 1989 NEI approximately, the database was collected and it was used to generate NUREG-1465 which was eventually published in 1995.

Sort of the question we're addressing that I want to make sure I understood is that basically what I've heard is, and this diagram doesn't do it, but there's a path through this that says take these values and what would you do with respect to high burn-up and MOX but using all information generated since the database that was used for this. But in the process you don't really do anything with this explicitly.

Now, there may be a lot of information generated through the transcript. We might have a section in the report but you're not looking explicitly for this table to be updated. That is, the NUREG-1465 tables for 40 gigawatt per day fuel.

DR. SCHAPEROW: That's correct. We did not have an objective to update to current technology.

DR. BOYACK: Okay.

DR. SCHAPEROW: But, as Dana points out, looking at high burn-up and MOX we should all the information we've got since '89. Why skip over any?

DR. BOYACK: Does anybody perceive a difficulty building off of these, in a sense, building off of these values but using all this information? I'm not sure that is all that straightforward to me. I just want to make sure.

DR. KRESS: It's like mentally updating NUREG-1465. You mentally change it and then you factor in the burn-up. It can be done but you just won't physically change it. You'll think about how they should have been.

DR. BOYACK: Well, one of the things the NRC should ask is if in the final documentation whether they want that declared this mental update, which is going to become clear, I think.

Okay. Just the panel process and a few comments about that. I'm going to suggest the process. The real thing is you needed strawman to work with. I said this is one of the things we can do. It's important to really get a process that works for the panel, not for me. This is very much open for discussion.

I'm assuming that as we work our way through this matter, we'll have to adjust and revise on the fly a little bit. Nevertheless, we ought to be prepared to do that. It's important to have a sense of what we're going to do as we start, even the changes.

Here's what I think I would like to propose as a start. First, the history, we wanted to give some basis for the source terms in 1465. The Nourbakhsh presentation did that, plus your conversation back and forth as a

way of getting everybody reattached to that work even though it is familiar to many of you.

The next step we wanted to engage in was to identify and review applicable data not considered in NUREG-1465 development.

We have the JAERI work to be presented here following me. We were going to have a presentation of IPSN but, as I mentioned also, they have travel restrictions on at the present time so they are not able to go ahead and be here but they will be here the next meeting and we'll have that.

I did ask Clement if he already had the presentation materials ready in presentation format, if he could either fax or send electronic copy to me and I'll check my e-mail tonight and see whether that transpired.

Then, of course, there may be some other information identified by you. I started to hear a little bit of that from Charlie earlier as they talked about various calculations that were being performed.

After the JAERI presentation for the rest of the afternoon we'll have a chance to talk about that.

Okay. So after we -- let me do this. This is the one that I was working on last night. We'll see if the graphical thing helps.

We have NUREG-1465. There's a composition pre-accident. There's coolant accident release, gap activity release, all of the things we've talked about. Those things then were reflected for PWR in Table 3.13. The timing information was reflected in Table 3.6.

There's a question about how we go ahead and generate the information that we will eventually use. I'm not sure this will be a straightforward process or whether it will be something where we cycle back

and forth. What I would like to do is for each of these phases I would like to think about and have some discussion about the similarities and the dissimilarities.

DR. POWERS: I thought this morning we had stipulated that the assumption is going to be compositionally weak central origin for that is the composition.

DR. KRESS: But there still may be similarities and dissimilarities in the actual --

DR. BOYACK: And I don't mind simple statements to this effect. What I do want to do is I want to have an explicit statement as to these types of things.

Now, I'm not sure whether we can do these separately or just go ahead and have the discussion. Some of that discussion was started this morning.

DR. SCHAPEROW: I'm not sure it was a preliminary topic in our discussions.

DR. BOYACK: Well, what about MOX? We do have to have some discussion about the composition.

DR. LAVIE: Can I say something?

DR. BOYACK: You sure can. Just identify yourself.

DR. LAVIE: My name is Stephen Lavie of NRR. While I agree that the origin code itself is suitable for MOX and high burn-up fuel, what changes, however, is the data logs (unintelligible) the code uses. I know of no MOX libraries built with the NRC at the present time. It is an issue. The code itself is fine. We don't have the data libraries necessary.

DR. SCHAPEROW: I don't think (unintelligible). Charlie, we're discussing whether we're going to talk about origins basically and then the results of those types of calculations.

DR. TINKLER: I wouldn't make that an object of this. The extent to which we need to answer questions about origin and library --

DR. BOYACK: I didn't realize what I was doing here. Okay. Basically you're saying strike this.

DR. POWERS: In following up, there are compositional things to worry about but they are not official product inventories.

DR. TINKLER: Right. Right. Exactly. We fully expected the panel would address those issues of physical characteristics and it`s effect on release fractions.

DR. BOYACK: Maybe I used the wrong term here. If I used fission product inventory pre-accident, would that have been a problem? For instance, with MOX -- there are a lot of areas I don't know much about. This is one of them. If you have MOX or if you're going to have any different fission product inventory --

DR. TINKLER: Absolutely. That's the area that would be addressed by origin and whatever data is needed to support the origin calculations.

DR. KRESS: The question is whether a difference in inventory will have any affect on the release runs.

DR. BOYACK: Okay. What I intended to show here was that we take each of the phases and we try to highlight the similarities and dissimilarities from 1465. That idea was -- I'm somewhat driven by the end

product thinking about the fact that when we get to the end we have to put out a document, right?

So it seems to me that this is a natural area that we want to cover in that document; that is, what are the similarities and dissimilarities and laying the base for the decisions you make about the alterations to the tables in NUREG-1465.

So that was really all that was intended to do is to say that in each one of these areas I hope we can have a specific discussion. Hopefully this will be focused on similarities and dissimilarities so that we can capture that.

DR. TINKLER: I don't know. I don't want to belabor this but I guess to the extent that the panel believes that origin or similar tools cannot fully be used to address the inventory, I guess --

DR. KRESS: I think ours is fine to get the inventory and the isotopic mix. The databases exist. You may not have them here but you can get to databases. It's all right to get that.

The other question is now what does that difference -- does that different isotopic mix and even inventory have any effect on the release.

DR. TINKLER: From that standpoint we just kind of reached the conclusion that origin could be used to determine the inventory in the isotopic mix. I say we, the NRC, but if the panel thinks otherwise, the panel is free to comment on that and would obviously.

DR. KRESS: I certainly think it can be used, especially for the purposes we're talking about.

DR. TINKLER: Right. Now, to the extent that isotopic mix and the inventory effects release fractions, sure. In that sense, I guess you could leave it on. It's a legitimate --

DR. KRESS: It's a legitimate thing.

DR. TINKLER: It's a legitimate thing to leave on the agenda and as part of the scope.

DR. BOYACK: I'm going to wait another minute here to see if we want another sign.

DR. KRESS: It's hard to erase that.

DR. BOYACK: This one is actually erasable.

DR. KRESS: You could put it on the screen.

DR. BOYACK: Have you ever seen that happen? I have seen that happen, too.

DR. POWERS: If you'll go down one floor, you will see a foil to Dr. Kress' presence on the ECR.

DR. BOYACK: I didn't realize it was so personal.

Let me ask a question about this. It's the upper right-hand corner there. If this comes about because of some of the prior PIRT experience. I've been vaguely involved in maybe seven or eight of these.

Generally in that process, as you will know, you took a facility, you took a scenario and you identified all the processes that went on and then you ranked them high, medium, or low, and we stopped there.

When we started on the high burn-up fuel, Bruck Alkwilla asked us to do one other thing and that was to address the problems. First let me tell you what the problem was and then tell you the resolution.

The problem was in this ranking process if the knowledge base was relatively low for a particular process or phenomena, then that got factored back into the ranking. The higher the uncertainty, that tended to elevate the ranks. It maybe something that ultimately turns out to be relatively unimportant but because the knowledge at the time was low, people ranked it high.

He tried to separate this out and said, "If the panel can just give me their best judgement. Forgetting their knowledge and tell me what they think about the importance, high, medium, or low.

Then in a separate category tell me what they think the knowledge base is, whether that's high, medium, or low. I'll know separately now your uncertainty. I need your best opinion on importance and then you tell me how certain you are."

One of the questions I just wanted to ask, particularly after I heard the initial presentations and the discussion here, and related to this later objective, if appropriate, identifying additional work, would you consider thinking about whether or not as we go through this process of somehow characterizing how well we know that it doesn't have to be a miracle but if we could provide that information, I think it's a natural adjunct to some of the things we're doing about trying to update these tables.

DR. GIESEKE: In prior similar kinds of exercises we have done this with current information base, the level of knowledge. As you say, you ask for opinions of the importance of that piece of data, high, premium, low. Then you play one against the other to see whether it warrants additional

work. If it's important and we don't know much, then we should take a look at it.

My memory is that usually these are directed toward pretty specific kinds of issues. I might even call them details. You're looking at very broad issues here, you know, to say whether -- are you going to say that early in-vessel release is unimportant or insignificant?

I think that is probably pretty broad to be painting those kinds of issues. Maybe it's not. I don't know. It seems like it.

DR. BOYACK: At this stage I don't know either. What I wanted to do is I wanted to present the concept, let you think a little bit about it, and as we go forward we'll evolve an approach for dealing with these things.

DR. KRESS: Well, I think we'll end up decomposing.

DR. BOYACK: Decomposing?

DR. KRESS: Like releasing fuel in essence will decompose and they have some sort of models of things in mind. Basically we can address the database and the knowledge of importance of why are we decomposing.

DR. BOYACK: And even if it isn't high, medium or low, if there is text or information there that says -- which is probably just as valuable or more valuable in the long term.

DR. POWERS: Let me address one more thing on your chart. You have the coolant activity release there. Correct me if I'm wrong but I believe every plant has a tech spec for the coolant activity that they will operate under. Those are different from plant to plant. Wouldn't that coolant activity just be whatever the tech spec is?

DR. BOYACK: I'm not going to answer these questions because I think the panel has to. I wasn`t invited to participate as an expert. You've got the question. Do you have a thought?

DR. POWERS: Well, it seems to me that everybody that's going to use this from a user's perspective, you know, no matter what we told him about the coolant activity release, he's going to say, "What was the tech spec for this plan? OK. That was the activity available in release."

He's not going to care what we said because the plan is saying sometime in our life we could operate this high and you couldn't take that number. If it was all the same, then, yes, the panel could come back and give him a different number that he might want to use. Since that number varies by an order of magnitude across the fleet of the plant, he's going to use whatever plan is working.

He's not going to use something that any expert came up with except the expert that wrote the FSAR and the tech specs. That's the only expert that counts for the coolant activity.

DR. SCHAPEROW: I agree the coolant activity is something determined by linkage or whatever. Things are floating around in the water and really didn't have any bearing on what we were looking at which is the core melt accident source term.

Actually when we went through the exercise of getting a new reg guy out, we didn't really consider 1465. The reg guy didn't consider it was coolant activity releases.

DR. KRESS: Well, I'd like to put a little different perspective on that. The amount of activity in the coolant has probably something to do

with both what's in a gap as well as number of failed tubes. If we're saying that the gap inventory is going to change, it is likely to have some effect on the coolant activity.

If we think that effect is big enough, that likely coolant activities are going to exceed what they have in the tests. They might be of interest. I don't know what plants would do. They would have to look at their experience.

DR. BOYACK: Let me stop you here for just a minute. So we're talking about process. I believe this is the kind of discussion we'll have in each one of these steps. Is it not?

DR. KRESS: I think Dana is basically right on the coolant activity. The tech specs will decide what they use and they are so far above what they are asking on experience that you don't have to worry about any effects on burn-up.

DR. BOYACK: Some of what we're going to do is we're just going to mention -- we'll launch off and try to make sure we really have a very orderly process. We'll start with BWR and PWR reactor. I'm assuming we'll start with the high burn-up fuel which is an easier one to start with.

Then we'll discuss and summarize the characteristics of high burn-up fuel that might impact the source term as it relates to timing, the five release phases, release magnitude, chemical and physical properties, transport and depletion mechanisms.

And I'm not sure whether this will all be tied together with the updated table or whether we can go through those discussions and then come back and use the discussions as the base for updating the tables which will be

36 of the NUREG document and either 3.12 or 3.13 depending upon which of the reactor types. Then we would repeat the process for the remaining reactor type and high burn-up fuel.

At this point I view it as sort of four passes. Pick your reactor type and do it for high burn-up fuel, the other reactor type, do fuel and then MOX and for the other two.

Question. I guess somewhere in the back of my mind I heard that is MOX intended for both reactor types or just one?

DR. POWERS: Right now the only plans are to use them in two ice events here, PWRs.

DR. BOYACK: And I had a faint recollection that might be the case. One of the questions I had, and that's why I didn't list all of these as to whether it was PWR and BWR for MOX but that's a question we have to answer about whether we go after both those.

Here is the MOX one. Consider MOX fuel. Identify and summarize. This is the same information. Judge the applicability. You see that I haven't listed whether they are PWR or BWR so I haven't gone through that because I wasn't sure.

Two notes. The first one you are familiar with. It's right out of 1465. Basically these release fractions characterize what they represent and stated whether they be represented or typical rather than conserving or bounding. These were associated with low pressure core melt accident. We've had that discussion. That's a phrase right out of 1465.

Then this is a plea. To the extent that the panel members really help me make sure that we get very clear complete rationales either on

the tape or we get them written down, we'll save ourselves a lot of work at the document stage because it will accurately reflect sort of a summary of pieces. I'll try to make sure that happens. Anything that you can do to help me keep that going would be appreciated.

The other thing, I have a high-quality slide. This one did not come last night. It was during the day. It's based upon what I started here from some of the discussion. Here is the high burn-up cases. We want to make sure we get a clear statement of what it is we're working on.

I heard as a starter of these discussions, which we hadn't had before, peak burn-up of 75 gigawatt per day per ton. An average of two-thirds of that. Cladding, I heard M-5 and Zr. Linear heat rate.

I don't know whether that was an issue for PWRs. We talked about it in the context of BWRs. The scenario, low pressure. But there was some discussion about whether we did other high pressure and they were like the other things we needed to fill in.

The real key here is that we all need to be talking about the same thing. The best way to do that is to make sure we get it written down and explicitly called out so that we do that.

I'm going to stop there unless there are any other questions or comments. What about the process? Is that okay as a starter or do you want to offer any alternatives? Since Tom is the only one that nodded his head --

DR. GIESEKE: Are you going to finish this before we go on?

DR. BOYACK: I don't have anymore slides. What do you mean finish? Oh, the table.

DR. GIESEKE: The table.

DR. BOYACK: Well, I thought what we would do is go ahead and -- what I thought we would do is we would come back and as we start our work -- after the discussion of the database, then we'll come back and work on this.

DR. GIESEKE: All right. First item of business.

DR. BOYACK: Yes, if that's all right.

(Whereupon, at 1:38 p.m. off the record until 1:48 p.m.)

DR. BOYACK: Let's go on the record. We'll have Mr. Hidaka present his information on work done at JAERI.

DR. HIDAKA: My name is Hidaka and today I will make a presentation of the overview and progress of our program on nuclear release from fuel. To begin with I would like to say the context of my presentation, scope and target (unintelligible) I presented (unintelligible) season meeting (unintelligible). Next I'll like to show the overview of previous (unintelligible) directives (unintelligible). Finally I`ll try to summarize my presentation.

DR. BOYACK: Mr. Hidaka, since we don't have a microphone, if you could just point to the view graph on the machine it would work better.

DR. HIDAKA: Okay.

DR. BOYACK: Thank you.

DR. HIDAKA: First is the scope and target. (Unintelligible) release experiment (unintelligible) having performed (unintelligible). Meanwhile, these experiments are mostly finished 10 years before. Meanwhile, the team (unintelligible) examination so that fuel temperature could meet the

American temperature (unintelligible). At present the experiment is in progress to investigate the radionuclides release up to 3,000K but under atmospheric pressure. However (unintelligible) the fission product (unintelligible) under elevated pressure. Also we don't have any release data from MOX fuel (unintelligible) so that special target of their program is to improve (unintelligible) for MOX fuel (unintelligible). VEGA stands for Verification Experiments of Radionuclides Gas/Aerosol Release. The program is radionuclides release from irradiated fuel at up to 3,250K under high pressure up to 1.0MPa condition.

The detailed objectives are to obtain radionuclides release data from fuel under severe accident conditions and to improve source term predictability. Also to clarify the kinds of mechanisms of radionuclides release from various forms of fuel.

Next slide shows the VEGA facility. This is a photograph taken from the bed grass. These are manipulators for the mode of operation. This is (unintelligible). In this (unintelligible) we have induction coil and furnace. (Unintelligible) deliver fission product inward and upward delivered to (unintelligible) and filters inside this box. (Unintelligible) cascade inductor line (unintelligible) from horizontal pipes.

(Unintelligible) the cascade inductor line has two vents to reduce some of the stress. Next slide shows the detailed schematic of the facility. The facility is installed inside a Hot Cell and these is a gas operated system. This red part indicates (unintelligible) is heated by an induction coil. (Unintelligible) fission product can be deposited (unintelligible) furnace. We have three sets of (unintelligible) depending on the fuel temperature

(unintelligible) sequentially operating from A to B to C and the volatile fission product is trapped at some (unintelligible) tubes and filters and the temperature between the furnace and some of the (unintelligible) is maintained at 750 degrees in the position.

The temperature of the filters is just 200 degrees C and the piping between filters and condenser the temperature is maintained at around 50 degrees to avoid condensation of steam (unintelligible). This is condenser and this is noble gas trap. Noble gas is trapped by coolant activated carbon due to physical absorption. We can know the official product of release and (unintelligible) at different traps by (unintelligible) major system. We have four Gamma ray detectors.

Next slide show the VEGA furnace for oxidizing condition. We have two kinds of furnace. One is for oxidizing condition and the other is for unoxidizing condition. This red part indicates the test fuel and the test fuel is set at crucible. This is inner tube and inside inner tube made of thorium or zirconium. In case the maximum temperature higher than 2,500K degrees we use thorium. Otherwise we use zirconium.

This red part contains graphite susceptor heated by induction coil. These are (unintelligible). This is induction coil. The temperature is measured by two pyrometers. One pyrometer measures inside of the crucible and the other pyrometer measures inside of the graphite susceptor.

We have several holes that stand by and crucible and to maintain the progress.

Next slide shows the furnace for unoxidizing conditions. The basic concept for design is almost the same but the main difference is there is no graphite susceptor because at first we tried to use graphite susceptor but

in that case always the Ta inner tube became more than 2,700K or 3,000K due to detected reaction between tungsten and carbon so we abandoned the use of the graphite susceptor. We decided to heat directly the tungsten inner tube. In order to avoid the vaporization from the carbon felt we set the Tungsten reflector here and here and they are almost the same.

The next slide shows the temperature distribution in VEGA apparatus. The horizontal axis mostly corresponds to the distance from the furnace crucible (unintelligible) temperature. As you can see, the temperature decreases more drastically as the distance becomes far from furnace. This temperature core is maintained (unintelligible) as the furnace temperature. The upstream part of the (unintelligible) tube the temperature is maintained at about 1,023K and the temperature inside the (unintelligible). This is just a concept for dropping the fission product separately and making use of the difference in (unintelligible).

DR. POWERS: Let me ask you a question about your reactor arrangement. You have a relatively complicated flow pattern for over the test fuel. Have you run the experiment with something with a no vapor pressure so that you can understand what kind of mass transport limitations you might have because of the furnace flow rate?

DR. HIDAKA: Excuse me. You're talking about (unintelligible)?

DR. POWERS: Right. You see, you have your flow coming up stagnating and taking a right-angle bend, coming up again and then taking a right-angle bend in over the fuel and upwards. That's a fairly complicated

flow pattern and we would like to get data which are not rate limited by the flow pattern which you're using.

I'm wondering if you have run something with a known vapor pressure so that you can assure that you would not be into a position where the flow itself was rate limited.

DR. HIDAKA: We have three holes in that direction. In the second direction we have four. In order to help the (unintelligible) coming into the crucible and to help (unintelligible) fission product we cover the cap here. We have not ever irradiated that (unintelligible).

DR. KRESS: But when you ran the test did you go back and look at your thorium and zirconium to see how much activity was left on the furnace? When you make your mass balances for before and after to get your fission product release, did you see how much was on the furnace, if any?

DR. HIDAKA: I usually present data that was with the results.

DR. KRESS: This is your temperature profile you have shown. It has no -- that's linear. I mean, that's a linear scale. How did you get up there in time?

DR. HIDAKA: Also I present data later. This is just the temperature distribution downstream.

Next I would like to show test sample. Size of maximum test fuel is just 6cm long and the maximum weight is 100g. Depending on the experimental conditions we use the fuel without cladding. Currently available fuel specimen is described here. In the future we will do the evaluation of this fuel by using NSRR or JRR-3 to accumulate short-life radionuclides such as (unintelligible).

Next is experimental conditions. Maximum temperature is 3,250K. The pressure can be changed from 0.1 to 1.0MPa. This is the first trial in the world. Ambient gas species we use He, H2, Air and Steam. The maximum fuel burn-up is 26-61GWd/tU.

Next slide shows the fuel to be used in future VEGA tests. Up to the end of fiscal year 2001 we use 47GWd/tU for Takahama, Japan. From fiscal year 2002 to 2004 we try to use BWR fuel from Fukushima, Japan. Also we plan to use MOX fuel from ATR. ATR stands for advanced time reactor.

After fiscal year of 2004 we provide some program called ALPS to stand for advanced LWR fuel performance and safety research. We try to obtain fuel from Europe. For your information, maximum burn-up currently allowed in Japan in the case of BWR the maximum assembly burn-up is 55GWd/tU and maximum pellet burn-up is 75GWd/tU. For PWR maximum assembly burn-up is 48GWd/tU.

Next is measurements. We doing measurement for fuel and filters, charcoal traps and cooled charcoal by using germanium SSD. Also we are doing the on-line oxygen/hydrogen concentration measurement. And for trapped mass at thermal gradient tubes and filters.

Also we do the off-line gamma measurement for crucible. In order to get (unintelligible) we do nitric acid leaching for these portions. Also we do the off-line gamma measurement for actinide. Also in order to quantify the noble gases we do analysis using quadruple mass spectrometer.

Next post-test analyses. We do metallography and SEM/EPMA for test fuel. Also SEM/EPMA and SIMA for TGTs and filters for the (unintelligible) which does not emit (unintelligible).

Next I tried to experiment about fabrication of $Th0_2$ tube. (Unintelligible) is a nuclear material and the fabrication is not comparable. Most (unintelligible) for the material is stability and high temperature of more than 3,200K. There are three (unintelligible) $Th0_2$, W, and $Zr0_2$. Only $Th0_2$ meets this criteria.

Next slide shows the method for $Th0_2$ tube fabrication. At first powder size adjustment by heating to improve mold strength. Next slip was prepared by changing masses of water and dispersant.

Casting of tube and crucible was tested by using centrifugal slip and drain casting. Finally, sintering at 1,970K after drying up of casted tube. This photograph shows the sintered $Th0_2$ crucible. We have three holes. Heat up test mostly finished. We cannot say because we have still some program

mostly (unintelligible).

The next slide shows the VEGA test schedule. Facility construction was finished at the end of 1988 and the experiment was initiated from the year 1999. First we experiment under unoxidizing condition. In 2001 we initiated oxidizing condition test.

In late 2002 we initiate the oxidizing test. From this experiment we do the test analyses and model development. After the verification we incorporate into THALES-2 application.

Next shows flowchart of VEGA post-test analyses. Heat up test, pip cutting, measurement for piping for FP distribution, nitric acid leaching for FP mass balance, and SEM/EPMA metallography for fuel. Most of the tests are finished except for (unintelligible).

Next slide show VEGA test matrix. Up to now we did a whole VEGA experiments. I will explain (unintelligible). The next VEGA experiment will be performed next January. Experimental conditions are almost the same as (unintelligible). The fuel (unintelligible) just before experiment. In the VEGA-6 test we used (unintelligible). Also in the (unintelligible) JRR-3. In the VEGA-7 test we used MOX fuel.

Next I would like to move on to overview of previous VEGA tests. We performed four experiments so far. In those experiments we used 6-year cooling PWR for (unintelligible) without irradiation. In VEGA-1 test, which was performed in Sept. '99, the purpose was to get the reference data and confirm facility capability. About 85 percent of Cesium release but no release of Eu.

In the VEGA-2 test, which was performed in April 2000, the experimental conditions are almost the same as VEGA-01 test. Pressure was raised up to 1.0MPa. The results show that about 61 percent of Cesium released but no release of Eu.

DR. POWERS: If you had done the VEGA-2 test with everything exactly the same, how much would you expect that release fraction to deviate? In other words, what's the experimental error associated with these kinds of tests? If you did the VEGA-2 test over again, different specimen but everything else exactly the same.

DR. BOYACK: You're asking the repeatability.

DR. POWERS: I'm asking what the experimental error is because I look at your matrix and there doesn't seem to be any measurement that would give us a handle for the experimental error. I'm wondering how you handled that. The question comes down suppose that Tom's lousy rotten old code predicts 72 percent. Is that an error that I should worry about for (unintelligible) or is that within the range of experimental error that would be expected here?

DR. HIDAKA: This was developed from the (unintelligible) measurement for fuel.

DR. POWERS: I have my doubts about the precision of your measurements. The question is if you did the thing over with a different piece of fuel how much of the uncontrolled variables are affecting the result?

DR. HIDAKA: This measurement was taken from the same (unintelligible).

DR. POWERS: Yeah. Control everything you can control but there are going to be a group of variables which could be anything. They are not controlled in your experiment because you can't control everything. How much does that change the release fractions?

DR. HIDAKA: In my test matrix I did not show the experiment with similar conditions but we think that we can evaluate again the effect in the future.

DR. BOYACK: I guess the basic question I heard Dana ask is do you have any plans for a test that would attempt to replicate all the

conditions so that you have two tests run exactly the same and then look at the differences.

DR. HIDAKA: That is expensive so we can run only two tests a year. Probably we can't complete the same experiment but we can do similar experiment in the future so we can (unintelligible)

DR. BOYACK: All right. Thank you.

DR. TINKLER: One question. Based on the tests you've run, do you believe there's a strong effect due to pressure?

DR. HIDAKA: Yes, I think so.

DR. KRESS: I can confirm that these tests are hard to run. We could only do two a year and they are expensive.

DR. POWERS: But, Tom, without experimental error you've got 100 percent insertion.

DR. KRESS: You need to run at least one. The experimental mass balance is pretty good. You know the results.

DR. POWERS: I think mass balance is pretty good for your 98.9 and things like that. Those are the most illuminating of the test. The ones that are really interesting are those resulting in 50 percent rates. And there to make maximum use of them compared to the codes, I really have to know how that release could be arranged. If I could have that in plotting the results against what is predicted by the code is just about used.

DR. GIESEKE: 60 percent.

DR. POWERS: Maybe the next time it's 43 percent. Well, I how I draw conclusions. I say the codes are not reliable for extrapolation because of the difference there and the difference is within the experimental

range that it could be, you know, I'm making work for gradulus that I don't really want.

DR. KRESS: Well, I think you can make judgements. Say, for example, between these two tests at different pressures everything else remained the same.

DR. POWERS: I don't know how you could do that.

DR. KRESS: You can look at the flow rates and the release rates and make decisions on what is controlling. In our test we had the flow rate at such a rate that the mass transfer was in the fuel itself. Then the way we determined the total amount of release was how much was in the fuel before and how much was in after and that was a pretty good measurement.

DR. POWERS: I have no doubts about the measurement. What I wonder is you run VEGA-1 at one atmosphere and then you run VEGA-2 at 10 atmospheres and you see a difference of 24 percent of release. If I ran VEGA-1 again and got 75 percent release, I've cut down my pressure effect by a factor of two. These are getting significant here.

DR. KRESS: We never did that either.

DR. POWERS: Unless you're going to measure the experimental error, you're really handicapping your so-called interpretation results. When the tests are very expensive, I insist that is when you absolutely shouldn't have a VEGA experimental error. The more expensive the test is, the more essential it is to know what the experimental error is.

DR. KRESS: Actually, we did have some reputable tests. I would be interested to go back and look at those and see how they agree with each other.

DR. POWERS: I think you'll come up with a mixed bag. It's like you just duplicated the results. I mean, they are very close and you'll find some others that have peculiarities.

DR. BOYACK: Okay.

DR. HIDAKA: The next slide is the effect of pressure on release rate. I plotted the results of temperature and pressure release of (unintelligible). Concerning the temperature, there is a small difference in duration of -- but then this is almost the same. The temperature is raised up to 2,773K. When you look at the pressure release, obviously the pressure release of $U0_2$ was suppressed.

According to the report, product release this fuel is further released through the process of (unintelligible). This division will be able to bring and a division will open for us. The division will be able to bring event or temperature (unintelligible) pressure.

A division in (unintelligible) temperature pressure and, therefore, in the case of real pressure the (unintelligible) to a division but a case of high pressure administered was located not only in division but also in division (unintelligible).

DR. KRESS: Was this a calculation?

DR. HIDAKA: I just -- calculated by using the (unintelligible) and I present the data. I consider the modicum.

DR. KRESS: I mean, you would certainly expect this kind of effect?

DR. HIDAKA: Pardon?

DR. KRESS: You would certainly expect that kind of effect?

DR. HIDAKA: Yes, yes.

DR. KRESS: I just wondered if you actually calculated it to see what the rates were.

DR. POWERS: If you have gaseous diffusion in the pores, don't you have newts in diffusion?

DR. KRESS: Yeah, along the surfaces.

DR. POWERS: Does that depend on pressure?

DR. KRESS: Yes, I think it does.

DR. HIDAKA: In case of diffusion -- it depends on temperature and pressure. In case of high pressure distance between -- comes close and, therefore, the (unintelligible) pass becomes small so it takes much time to diffuse.

DR. KRESS: I just wondered if you had some estimate to transport pathways and their distances unless you made a calculation and say, "Oh, yeah. The gas phase is controlling this and the solid phase is in this one." I never made any calculations. I didn't know what the pores looked like.

DR. POWERS: They look like sausages.

DR. KRESS: Yeah, I know that. I didn't know how to characterize them.

DR. GIESEKE: But you wouldn't have to calculate an absolute rate and only look at relative rates with the calculations to see the

effective pressure. You can just look at the mechanisms involved, just like ratio of the mechanisms to the appropriate dimensional groups or some kind of process like that.

DR. KRESS: That's a distance. The diffusion distance is in that equation and it's hard to get it out.

DR. POWERS: It is a hard calculation to do because you have to set the equations up very carefully because you run into this problem. If things don't get out, they build up pressure so you actually have a gas pressure forcing that is left out of most of the kinetic equations. For this calculation you've got to put it in because it's got high pressure there that's limiting the flow out.

DR. TINKLER: Dr. Hidaka, did you say the name of a code? Did I hear you say you used a code?

DR. HIDAKA: Yes, VICTORIA code developed by Sandia National Labs.

DR. TINKLER: Okay. I thought I heard him say he used the VICTORIA code to compare those two resistances. Just a point of clarification.

DR. KRESS: Yes. I would have trouble getting the right input.

DR. TINKLER: The same comments still apply but I just thought I heard you say that.

DR. HIDAKA: The next slide shows the effect of temperature release rate. In the case of temperature, the temperature increase rate is the same between VEGA-1 and 3 tests. We wanted to get the release rate data

and the conditions of continuous high temperature above the melting point. We set the final temperature at 3,123K in VEGA-3 test.

It's difficult to compare from this transparency but the release data before 60 minutes almost the same, but finally the VEGA-3 release is about 100 percent. I tried to make two remarks here. One is the temperature increase rate of VEGA-3 at about 3,000K became small. This is due to the latent heat. Also at the same time this rate increased because of Cesium was released.

The next slide shows the effect of atmosphere on release rate. In this slide is the results of VEGA-1 and VEGA-4. In the VEGA-4 test we had to throw out temperature increase rate because we used (unintelligible) in crucible. In order to (unintelligible) we had to throw out temperature increase rate. But some (unintelligible) there is no difference.

Data is difficult to compare on this transparency but release rate of VEGA-4 is also about 100 percent. There is a reason for larger release in oxidizing condition can be considered by using this schematic. It is possible that the fission product transport inside the grain through defect of uranium grain matrix. In case of inert condition a number of defects is not so large. In case of steam condition, a number of defects could increase and, therefore, the fission product could be transported easier and as a result the release could increase. In order to compare for four VEGA tests performed so far we created the release rate and diffusion coefficients. Horizontal axis indicates reciprocal temperature and vertical axises indicates the release rate coefficient and diffusion coefficient. The tendencies of release rate and diffusion coefficients are almost the same. All the data became

straight line on this figure. The data can be expressed by (unintelligible). That data of VEGA-4 became the largest. On the other hand, the coefficients of VEGA-2 became the smallest. There is striking difference in inclination but the result of VEGA-1 and VEGA-3 is almost the same.

The next slide shows the preliminary data on fractional release estimated from intensity changes of gamma peaks.

DR. KRESS: You have the release rate coefficient as a function of temperature to get an Arrhenius like relationship but you're talking about transient temperatures. Are these the periods where you held them constant that you've extracted those from? These are the plateau temperatures in the experiments?

DR. HIDAKA: Yes. For example, in the case of VEGA-4 test we predicted temperature plateau. The second one responds to this one.

DR. KRESS: Is the variable temperature used as an average or the logarithmic average? I don't understand how you extracted an Arrhenius curve out of the transient temperature. I can see how you could use the plateaus except that the release rate is sometimes thought to be a function of the actual absolute magnitude.

If you have different magnitudes of concentration, then it's not appropriate to extract an Arrhenius curve out of that. I'm not sure. I was just struggling with how you actually get this Arrhenius curve out of the data. I'll think about it. I had trouble getting it.

DR. HIDAKA: This is just the result of final (unintelligible).

DR. KRESS: Anything you would like to share?

AUDIENCE MEMBER: No.

DR. KRESS: How do you get an Arrhenius curve out of this kind data is not as straightforward as you might think. You basically have to have a model. You best get your model and back the Arrhenius data out of it. Since there is more than one parameter in an Arrhenius curve, you have to have more than one set of data to do it. I'm not sure how they did it.

DR. HIDAKA: If I can answer again, I don't remember but this rate coefficients are created by -- I evaluated the final pressure release by using on-line measurement. As I explained, we on-line gamma measurement and off-line gamma measurement.

In the case of on-line gamma measurement the detector is located inside hot cell and the resolution of on-line gamma measurement is not so good because inside hot cell we have induction coil so the resolution becomes bad due to -- by the interference of induction coil. We decided to evaluate the pressure release by using off-line gamma measurement.

Also we have, as a program, because due to the (unintelligible) we have to perform the gamma measurement by using different systems. This kind of assumption was made that there is reduction of Europium-154 is equal to zero. Generally speaking, the pressure release of VEGA-2 became the smallest and VEGA-4 was the largest.

For your information, I gave the result of VEGA-3 experiment and the experimental conditions are almost the same as VEGA-4. In the VEGA-4 test (unintelligible) was released probably due to -- in the VEGA-4 test (unintelligible) was four. There is a big difference between VEGA-4 and VEGA-3 test. Measure the VEGA test and it was quite small.

The next slide is results and analyses for VEGA-1 test. First I show the result. Next I show the analysis. Next I show the temperature, flow rate, pressure of the VEGA-1 test. And present our systems which is time, temperature, flow rate, and the pressure. (Unintelligible) indicate the temperature. We have three sets of (unintelligible) and filters. (Unintelligible.)

The cascade impactor was opened here. This blue line indicates the flow rate at furnace outlet. You can see that the flow rate changed drastically when the air temperature was at maximum. This is because at this time -- we have (unintelligible) the tube became melted at this time. Just after melting (unintelligible). But we have bypass for the crucible so we continued at this point. However, generally speaking for safety reason -- the temperature at the surface of (unintelligible) 80 degrees C. At this point the (unintelligible) worked and the (unintelligible) supply was cut.

The next slide show the gamma spectrum change before and after the VEGA-1 test. Left I show the gamma spectrum before experiment and right I show the gamma spectrum after experiment. You can see the broader (unintelligible) such as Cesium decreased drastically during the experiment. But I guess (unintelligible) such as Europium did not change. But if you look carefully, the gamma intensity of Europium after my experiment became less compared to pre-test. This is because we used different (unintelligible).

The next slide shows the on-line gamma ray measurement of VEGA-1 test. This pink line indicates temperature and the red line indicates the gamma intensity for fuel. You can see that the gamma intensity for fuel began to decrease when the temperature gets higher than 1,500 or 1,600K.

On the other hand, the gamma intensity for filter began to increase after some delay. Also that gamma intensity for krypton increased in proportion to the gamma intensity for filters.

The next slide shows the distribution of gamma intensity in VEGA-1 facility. The horizontal axis starts from crucible bottom and (unintelligible) gamma intensity. As (unintelligible) measured inside crucible. On the other hand in the case of volatile Cesium.

The next slide shows the microphotograph of VEGA-1 test fuel. This photograph shows the cross section of crucible from the crucible bottom. It consist of double tube of doubling tungsten. This is tungsten crucible and this is tungsten inner tube. This gray part indicates the test fuel. That gray part indicates epoxy resins which was poured after experiment to solidify.

I did not describe in this photograph but you can see this light gray part is, and this is increment, which was formed at the time of melting. This is magnified photograph of UO_2 fuel, you can see a lot of holes -- formed by gas -- released from fuel. And just for your information I put the photograph before test with small amount of holes. Also you can see grain bundling but it is difficult to find grain bundling after the test.

Next I would like to move on to the analysis. I compared the VEGA-1 result with existing models such as CORSOR-M or O or NUREG-0772. In the early 1980s this (unintelligible) of high temperature (unintelligible) was not available. On the other hand, the CORSOR-O was predicted with better predictions. These results still operated at high temperatures.

The next slide. Since the existing models uses the research coefficient and diffusion coefficient and just obtained every one minute -- every minute from the experimental data. You can see that obviously at this part it does not follow the Arrhenius form but (unintelligible) follows the Arrhenius form. I calculated the division formula by using the (unintelligible) vessel.

As a next step (unintelligible). In case of research coefficient the multiple correlation coefficient it 0.52. In the case of diffusion coefficient, the coefficient becomes 0.86, which means the diffusion coefficient agrees with Arrhenius form better than research coefficient simply because the division is smaller -- in case of division coefficient. They are based on the temperature (unintelligible).

(Unintelligible) that means division (unintelligible) it means is located at diffusion grain. And also, I just copied the ORNL test results into here and that point indicating (unintelligible). And ORNL was just prepared based on the copies of the results. In the case of Oak Ridge experiment that experiment was performed using -- with cladding and, therefore, you take the reaction up to 2000 Fahrenheit and that is (unintelligible). On the other hand, in the case of (unintelligible) we did not use cladding and, therefore, the degrees was not as (unintelligible).

The next slide shows the comparison of Ru and Ce release of VEGA-1 between measurement and calculation. And that -- measured them (unintelligible) was very small and, therefore, I compare the final results. In existing models -- experimental data but my testing, I measured was very small so I became suspicious. We evaluate using (unintelligible) variation.

The next slide shows the distribution of Cs deposition in VEGA-1 apparatus. This was obtained from the (unintelligible) reading. Contrary to our expectations, Cesium deposited inside the furnace because of (unintelligible). You can see that there is no difference between Cesium-137 and 134.

This figure shows the aerosol size distribution obtained from cascade impactor. And according to measurement the medium diameter was 4.7 micrometer and the GSD was 4.9. I checked the position at horizontal 2 by calculation, -- horizontal 5. There is no temperature difference between (unintelligible) and, therefore, some frequencies can be ignored. Also the (unintelligible), the rate of (unintelligible) is 12 or 13, something like that. Therefore, the distribution from laminar flow can be ignored. The major deposition (unintelligible). In the calculation I used the diameter obtained by cascade impactor measurement.

Also concentration of aerosol was estimated from the deposition (unintelligible). The concentration was measured by mass and flow rate and I put in the calculation as 3.9 percent. On the other hand, 5.8 percent was deposited. So the remainder was not certain.

The next slide shows the condensation of Cesium through the tubes. The major deposition is considered to be condensation. It is saturated vapor pressure temperature is low. The saturated vapor pressure cannot be resisted as gaseous form, so the partial pressure, can be condensed. I evaluated the saturated vapor pressure of various Cesium compounds such as Cesium and Cesium hydroxide and the Cesium iodide.

Next I evaluated the Cesium vapor pressure in TGTs. It was estimated from the test duration and mass condensed at downstream of TGTs. I calculated the (unintelligible) at 1.1 ten to the minus five atm. This Cesium vapor pressure just crosses saturated vapor pressure, this is the iodide, at 800K. On the other hand, this shows the experiment results. You can see all the trains peak here at about 800K. Therefore, Cesium inside the TGT after the Cesium iodide. This result does not mean all chemical forms of Cesium is (unintelligible) . Only inside some of the burning tubes (unintelligible).

Next I will move on to consideration on model for pressure influence on Cs release. This diffusion coefficients (unintelligible). As I explained before, the diffusion coefficient can be described by two diffusion stages. A diffusion coefficient inside grain in proportion to minus b divided by T.

In the case of diffusion in open pore -- diffusion coefficient is proportionate to $T^{1.5}$ divided by P. Diffusion in grain does not depend on pressure but experimental data diffusion coefficient became small because of high pressure and, therefore, in case of high pressure (unintelligible) is located not only inside the grain but also diffusion over pore.

As the next step I evaluated the diffusion time in UO_2 grain and pore where the diffusion time was evaluated using this expression. In the case of diffusion time in grain (unintelligible)is equal to 4.2 radius. Beta and alpha I (unintelligible). In case of diffusion time in grain, I (unintelligible) --

DR. POWERS: What did you use to calculate the Chapman-Enskog model, you have to have both well depth and a cross section, a diameter for the gaseous species. What did you use?

DR. HIDAKA: I don't remember. I used Chapman-Enskog model.

DR. POWERS: But most of those parameters are obtained by measuring viscosity of the gas but you don't have viscosity of these gases so what parameter did you use?

DR. HIDAKA: Okay. Chapman-Enskog model is just for binary system so I checked with the results -- in this calculation we assumed krypton and Cesium.

DR. POWERS: Ah, krypton.

DR. HIDAKA: Krypton and Cesium.

DR. POWERS: Okay.

DR. HIDAKA: I don't remember krypton --

DR. POWERS: Neither one of them is appropriate because those are nonpolar and whatever species you have in Cesium is going to be polar, that I know.

DR. HIDAKA: In the case of diffusion found in grain (unintelligible) measuring temperature in grain does not depend on pressure. On the other hand, in the case of diffusion in pore depends on pressure but it does not depend so much on temperature. This is just reason why gaseous diffusion depends on pressure. In case of high pressure, (unintelligible) becomes small.

According to this calculation the diffusion time closes at about 2,600K which means -- therefore, the pressure just appears above the temperature of about 2,600K but in the experiment we found the (unintelligible)

low temperate region to high temperate region, and therefore, the pressure effect cannot be explained only by increase of diffusion time of open pore.

In the next slide by using (unintelligible). This black line indicates the VICTORIA result in the case of ORNL and Booth model. I selected ORNL and Booth model. This was the result of 0.1 and 1.0 (unintelligible). It means could not calculate the pressure effect. Also we have preformed the calculation using the diffusion coefficient obtained from the (unintelligible).

Next slide shows the FP release model of VICTORIA2.0. Inside the $U0_2$ grain was (unintelligible) equation. In the case of -- in the Booth model just using this -- to simplify the equations. These equations was debated by assuming that the concentration of grain surface is equal to zero. As for the diffusion in open pore, this equation was used.

The (unintelligible) through pore. In this situation alpha total porosity of fuel is 0.05 and beta is interconnected porosity of fuel 0.0001. This was based on -- I don't remember (unintelligible). If we divide both sides by alpha, we can get the apparent diffusion coefficient and apparent flow rate. (Unintelligible.) This could explain the pressure effect.

Next I checked the point of hydration. This slide shows the concentration of Cs in $U0_2$ grain and pore. That concentration in grain gradually decreased. On the other hand, in the case of concentration of pore, concentration increased until the (unintelligible).

After that the concentration decreased gradually. There is a difference in concentration of pore between low pressure and high pressure. In the case of high pressure concentration became larger.

Next slide again is FP release model in VICTORIA2.0. This is inside the UO_2 grain and this is the pore. As I explained before the Booth model always assumes that the concentration grain surface is equal to zero. In the case of VICTORIA model the amount of (unintelligible) released to the grain surface is just given to the diffusion calculation to open pore as an increase.

Therefore, there is discontinuity of concentration in the present model. In the case of low pressure, the concentration in pore is so small and it's okay. In the case of high pressure, the concentration in pore becomes 20 or 30 percent of concentration in UO_2 grain so this cannot go on.

The next step I preformed calculations by using this (unintelligible). In reality, the concentration can be described by using (unintelligible). In the experiment just the concentration inside UO_2 grain is equal to m instead of a. Also, in case of a, by using this experiment the current existing model can be high pressure calculation.

The next slide shows the results. (Unintelligible) make better predictions.

The next slide shows also results. In this case just the diffusion coefficient was changed from ORNL-Booth model to (unintelligible).

I would like to summarize my presentation. Three tests under inert atmosphere including the highest pressure or temperature condition among previous studies and one test under oxidizing condition were performed in VEGA program so far.

VEGA-2 test under 1.0MPa condition showed experimentally first in the world that Cs release rate could be suppressed by about 30 percent compared with that under atmospheric pressure condition.

In VEGA-3 test with maximum temperature of 3,123K, almost 100 percent of Cs was released at time of fuel melting but no release of low-volatile Eu, Ce, etc.

VEGA-4 test under steam condition showed that Cs release could be enhanced to increase of defect in UO_2 grain by oxidation. Ru release was also enhanced due to formation of ruthenium oxide with high volatility.

Existing FP release models mostly reproduced Cs release observed in VEGA-1 but overestimated above 2,400K because enhancement of release due to eutectic reaction did not occur in VEGA-1 without cladding.

Diffusion coefficients of Cs obtained from VEGA-1 mostly follow the Arrhenius form between the rate determining step of Cs release existed in diffusion in UO_2 grain.

Decrease of Cs release observed in high pressure VEGA-2 was not reproduced by current VICTORIA2.0 but it could be predicted by considering increase of Cs concentration at grain surface due to decrease of diffusion velocity in open pore under high pressure.

Release of low-volatile or short life FP and actinides will be investigated using high burn-up UO_2 and MOX fuel just after re-irradiation.

DR. BOYACK: Okay. Any further comments or questions right now? All right. Well, how about taking a 10-minute break and then coming back and Richard Lee says he's got one slide presentation regarding the French test so he's obviously picked up a little bit of information somewhere

and we'll talk about that -- hear that. Then we'll see what other data sources we should consider.

DR. KRESS: I did have one question before you leave. The Booth model which is the approximate solution to your diffusion equation was generally derived for a constant temperature process where the concentration is coming down with time.

You have a varying temperature with plateaus of varying temperature. How do you actually convert that Booth model into looking at that kind of test data? Do you understand what I'm saying? The actual model has built into it a constant temperature.

DR. GIESEKE: You're saying there's a constant temperature in Booth?

DR. KRESS: It assumes the diffusion must proceed in constant temperature.

DR. GIESEKE: He's got temperature plateaus in here. I tried to match up the plateaus with the data points and I couldn't quite do that so I'm not sure either.

DR. KRESS: There are several possibilities of how you apply the Booth model and I'm not sure how you do it.

DR. HIDAKA: I don't remember but according to the (unintelligible) and the same comment was made. I know that with modification is difficult to explain.

DR. BOYACK: Okay.

DR. TINKLER: In VEGA-4 did you actually measure the fuel afterwards to determine whether or not you had further oxidation of the fuel?

You speculated that it could be oxidation of the fuel that enhanced it. Did you measure it? Did you analyze the fuel to see if you have further oxidation?

DR. HIDAKA: We only had (unintelligible).

DR. BOYACK: Gentlemen, we're having trouble with the recording.

DR. SCHAPEROW: So did you measure the fuel after the test?

DR. TINKLER: Yes. That was my question.

DR. HIDAKA: Not yet.

DR. BOYACK: Anything else? Let's come back in 15 minutes at 25 to 4:00, 3:35.

(Whereupon, at 3:29 p.m. off the record until 3:47 a.m.)

DR. BOYACK: We'll go on the record now. Richard Lee has one slide and a lot of words regarding the French test program.

DR. LEE: It was about a year ago we asked the French to tell us about the fission products release tests. They actually gave us a matrix but I wasn't sure -- over here so we can present the matrix itself but I summarize it here.

I think this information was okay. Tomorrow I will ask him whether -- I will send him an e-mail and ask him whether we can give you the test matrix. I don't think there's any secret in the test matrix. I hope not.

In 1989 to 1994 time frame they have conducted six tests. Those have been completed. They are mostly all $U0_2$ fuel and the burn-up rate is basically around 38 to 60 Gwd/tU. You have seen that they have done it in

the producing as well as steam rich environment. The maximum temperature reached by the fuel is between this range.

Actually two tests were in the low range and four tests was in that range. All the fuel was irradiated. They never mention about the fuel has been pressured -- I mean, the test bundle is being pressurized. I assume that they are done at atmospheric pressure. I don't know for sure.

I remember that back in the '80s when we were winding down the Oak Ridge test we have actually transferred the technology of how the Oak Ridge HT series does the very high temperature, teach them the technology how to do it. That was conveyed to them in the '80s.

They have started two series of tests which is the RT and HT. The RT stands for release and transport. These are the high temperature series which are ongoing.

Actually out of these seven tests, one of the tests was less than 2,002 nexus actually a very low burn-up fuel 11. I think it was credit. I don't quit exactly know what they had to do with that one.

As far as MOX test is concerned, there were two tests in the 40 GWd/tU.

If you look at the next series it's also around 40. There's no higher MOX test that I have seen in the matrix. This is really beyond 2002. There's only three tests that was mentioned with dates on it. In this series here in about three tests they did not irradiate the test before they did it.

Within this series they have comparison between MOX and UO_2 at the same burn-up. Also MOX versus MOX in the steam rich environment versus so-called reduced environment.

Now also here the two tests using $U0_2$ was also done with debris to compare to the PHEBUS because at PHEBUS one of the tests they had conducted MPT 4, was a debris test. They have used the uranium fragments and they did a test in that, two tests actually done in this series here.

Moving to this one here, actually here is really 46 and 53. One MOX test I mentioned is 41. They have all cladded fuel here. They plan to irradiate everything.

Now the tungsten submission in this one is the only thing that gives us really the indigo filter and only used that. From here they have radium tubes filled with to gas capacity measurement and so forth. Also in this series they also inject materials into the test bundle like control rod materials to see what effects it has on particle release.

They also use boric acid which actually if you look at what change in the chemical species. This series is really the latest one they have. In '96 they performed one test. The next test was done in April of this year and the year 2000. Next year we don't know when the other two tests. There are no dates.

DR. KRESS: Are these small segments of fuel about six inches or so?

DR. LEE: Yes. They are small segments. Then these tests are all done in the Chernobyl

Nuclear Center and the plan, I believe, after this series is finished they will shut down that facility and they constructed a new facility at Caderage.

I believe the segments will be longer but they couldn't start with the first few so it takes a long learning curve to get that facility up to

running. It is supposed to replace all this and Chernobyl is not going to do anything.

DR. KRESS: Why are they interested in fresh fuel?

DR. LEE: Which one?

DR. KRESS: Any of them. Why are they interested in fresh fuel at all?

DR. LEE: The new facility at Caderage they are going to start by doing fresh fuel first.

DR. KRESS: Why?

DR. LEE: Just learning.

DR. KRESS: Just to practice on the experiment. Okay. I can see that.

DR. LEE: It's the start-up of the facility.

DR. KRESS: Yeah, I can understand that.

DR. LEE: I have very little information about that facility at this time. We were told a few years ago it should have been started but we have never seen it. This is what we are asking the French to get us but there is a lot of linkage between test conditions. As you can see, the temperature has moved up in terms of the temperature they are testing it on.

AUDIENCE MEMBER: The fuel fragment test that was supposed to be like a counterpart to the PHEBUS, is that all oxidated material? Are there fragments of cladding in that group? Except for one test they have planned with 11 gigawatt data you say it's uncladded fuel.

DR. LEE: RT4 it says it has oxidizing so in RT4 we have some oxidized zirconium. I will try to see if I can get hold of and give you this

test matrix which gives more detail. If you want to ask a question to repeat any test of the same thing, I would say no.

DR. KRESS: That's not good practice. Is it? It leaves you wondering what to do.

DR. POWERS: You just don't know what to do with the data.

DR. LEE: The thing is I remember in Oak Ridge the test we did like once a year was more than a million dollars.

DR. KRESS: But we did some replicate experiments.

DR. POWERS: The question is whether you want to spend a million dollars to have something you don't know what to do with or spend two million dollars and you know what to do with it.

DR. LEE: Okay. So that's all I can tell you. This is what we find here and hopefully in Japan we'll get some more information.

DR. POWERS: Actually, what it is you spend a million dollars on testing and two million dollars on analysis or two million dollars on testing and a million dollars on analysis. It's where you want to put your money.

DR. KRESS: Zero sum game.

DR. BOYACK: Before we move on, there was a document which was just given to me that was referenced earlier today, the fuel qualification plan from Framatone ANP. It's a transmittal of updated MOX fuel qualification plans, this document. It's double-sided and this size. We won't get to MOX for a little while but this can be distributed. The question is who wants a copy? One, two, three, four, five. Six copies. Okay.

DR. TINKLER: I don't want it because I've already got one.

DR. BOYACK: Are you the NRC research? Okay. Fine. Thank you.

All right. For the agenda the item is participant identification and discussion of other data. I want to spend a little bit of time here. I know you're just going to love these little drawings. Okay. What we have is in 1989 --

DR. KRESS: A smart-aleck remark.

DR. BOYACK: A smart-aleck comment. Who was it?

DR. KRESS: It wasn't me.

DR. SCHAPEROW: Said what? I didn't hear anything.

DR. BOYACK: Okay. 1989 the database generated essentially for NUREG-1465. Here we are in 2001. We're down to NUREG-1465 applicability. What I think would be worthwhile is spending a little bit of time talking about what is the nature of the additional information we are going to be utilizing in this 1465 either by way of data or by other information such as calculations.

We've had two sources of data that we talked about, one in much more detail than the other, of course. That is, the JAERI VEGA program. Then there's the ISPN VERCORS. I'm consistent on that. In fact, I tried to send an e-mail and it bounced it with ISPN. Can you imagine why? Okay. I apologize and I'll change that.

DR. KRESS: Is the reason you wrote that because the data is planned? Is that the reason you wrote the second data?

DR. BOYACK: I did not. That's not the reason. What I'm curious about is if the panel could help me develop a list of what it is they are

using or going to use as part of their common understanding that would lead us to this outcome. Help me. What is there?

DR. POWERS: Well, my suspicion that we're going to focus our attention on the gap release and the in-vessel release because I think everything that follows from that is not going to change very much. It's arguable on the late in-vessel release. I guess it's going to be a magnitude change. I don't think the phenominology changes very much there. I bet we spend a lot more time talking about the gap release than we ever did in the source term code package data.

When you talk about gap release, you need to talk about gap inventories and things like that. I think we are going to spend a lot of time looking at the database, get them published on fission gas release to the gap on these hyper fuels.

DR. BOYACK: And there is data on that or is it calculations or both?

DR. POWERS: The answer is yeah, they are both models and experimental data. There is certainly going to be lots of attention paid to the rent factor. We certainly have a plethora of models for the rim effect.

Most of those are looking at development of the rim effect but there are those that calculate the efficient transport. I suspect if I looked in here far enough I could find a good general reference on outlining those available models for the rim effect. I'm sure we'll pay attention to those.

DR. KRESS: The database on MOX fuel ought to have that rim effect built into it.

DR. POWERS: The database for MOX.

DR. KRESS: I mean, if we're looking at effects on fission product release.

DR. POWERS: I personally him not aware of a lot of MOX data going up to a temperature to get a rim effect.

DR. KRESS: I was thinking about these French tests.

DR. POWERS: They are only 40 gigawatt fuel and though they have a rim region on there, you really can't see it.

DR. KRESS: You're not getting enough.

DR. POWERS: It's so small. The rim effect is really interesting because you really can't see it until you get over in 45/50 gigawatt days per ton. It's there but you just can't see it and then it grows exponentially so that you have a lot of data around 60 gigawatt days per ton where there's 100, 200 micron rim effect that you can see.

By the time you get to 75 gigawatt days, it's 1,000 micron effect. You really lose expeditiously. I just don't know of anything comparable to that kind of rim change. Minimal exist. The Train Sirenion Institute and the Belgiums seem to like to torment fuel to an ungodly high. Until they exist I just didn't have a clue.

DR. KRESS: Some other comments on Dana line is I suspect there's probably not much reason to question the chemical forms except possibly you might want to reflect on what the PHEBUS data tells us about that and put PHEBUS up there as one of the data sources.

DR. BOYACK: I used to know how to spell PHEBUS.

DR. KRESS: P-H-E-B-U-S.

DR. BOYACK: And that's for chemical forms?

DR. KRESS: Yes.

DR. POWERS: We should also pay attention to Charlie Clement's work on charged aerosols.

DR. KRESS: We're talking about retention effects?

DR. POWERS: Yeah, for the transport effects.

DR. BOYACK: Clement's work on what?

DR. POWERS: Charged aerosols. You are going to be in the transport and you run 10 to the 7th rad field.

DR. KRESS: You expect them to be charged.

DR. POWERS: They charge up. At least in the big pipes.

DR. KRESS: There's some analytical work in the sum. There's a parkay outfit called Mattel Columbus by some guy named Gieseke. I don't know if that is still around.

DR. GIESEKE: We decided they discharge faster than they charge.

DR. KRESS: I think that's what I remember.

DR. POWERS: Well, Clement takes you to task over that.

DR. GIESEKE: Well, that's all right.

DR. POWERS: You can make mistakes, too.

DR. GIESEKE: He has the right to be as wrong as anyone.

DR. KRESS: We ought to list that. It's something to think about, the Gieseke effect.

DR. GIESEKE: No, no, no.

DR. BOYACK: One of the things as I listened is it fair characterization that between 1989 and the present there's not been a huge amount of additional work done?

DR. KRESS: The Oak Ridge data factored into the 1989 didn't include some of the high temperature data. They do have a little bit of burn-up effect. They didn't get up to really high but they do have a couple of different burn-ups. Some of that might be worth relooking at.

DR. BOYACK: So we've got Oak Ridge high temperature data. Then you mentioned a second.

DR. KRESS: Well, some of the other data had two or three different burn-ups at like 30 to 45. It's not enough to make good data based on burn-up but you can look at it.

DR. BOYACK: So is that the same as the high temperature data or is it additional?

DR. KRESS: No, it's other data.

DR. BOYACK: Different burn-ups. The reason I asked the question is because it wasn't like there was an immediate allegory. We've got this, we got this, we got this, we got this. I don't know whether it was a thoughtful pause. To me it was like what do we really have?

That was why I asked the question when I did. You came back and responded with a whole bunch of information between 1989 and now. Is this list hard to capture or is it really a lot more than that?

DR. POWERS: I think where the biggest impact has been really on the modeling transport and retention in the piping system. It's really gone a long way since the 1989 time frame.

DR. BOYACK: So this would be things like the deposition factors. Now, you've got the modeling but you cannot consider this modeling to be validated. That is, is it just a matter of better models but not validated or are these models that you feel comfortable or significant improvements?

DR. KRESS: Well, PHEBUS, for example, has some data.

DR. POWERS: And the situation is this, if you'd asked me about how I thought about the models before they did the PHEBUS experiment, I would say they are probably pretty good, especially the aerosol parts of the models. They are pretty good. So they did the experiment and now I have to say they are a factor of two off. How does that make you feel?

DR. GIESEKE: Good if you're only two off.

DR. POWERS: Well, the problem is it's consistently a factor of two off and I don't see how to get a factor of two back without just putting in a fudge factor. The PHEBUS is a very simple experiment presumably for aerosol deposition. Of course, the model is simplified even beyond as simply as it is. We've had some British pile work deposition that was part of an ISP. I think it was called FALCON.

DR. KRESS: FALCON, yeah. Then there was STORM was Italian.

DR. POWERS: That's an aerosol resuspension test. I've never really -- I mean, I've seen some of the data in Lynch publications but I'll be damned if I know how to use it.

DR. KRESS: I haven't looked at it.

DR. BOYACK: This may be a question that jumps too fast and too far. We want to keep hitting as much of this now as we can. Is this

information all universally known to all the members of the panel or is it the kind of thing where we're going to have to talk back and forth about it and have some presentations?

DR. KRESS: I think we would have to talk.

DR. POWERS: The only PHEBUS test publicly available right now is zero.

DR. TINKLER: One should be available pretty soon. They're talking about making an ISP out of it. That would mean it would almost have to be. The VERCOR data I have seen the Cesium and molybdenum releases from the VERCOR 3 experiments. That's a low burn-up, higher burn-up, and a MOX.

DR. LEE: In the PHEBUS FPT1 because of the ISP --

DR. BOYACK: So we would have that information available by the second meeting. You said November, right?

DR. LEE: All the information for PHEBUS is available.

DR. BOYACK: Currently available? I thought you said -- okay.

DR. SCHAPEROW: NRC as opposed to the public.

DR. BOYACK: So the question I think I'm hearing here is the fact that the NRC has it, it may not be able to be used by this panel? What is it you're telling me?

DR. LEE: The FPT 1 will become publicly available (unintelligible).

DR. BOYACK: So how do you handle it in a "public" meeting like this, discussions of it? You don't?

DR. LEE: Public meetings anything we present (unintelligible).

DR. BOYACK: All right. So you're saying the publicly available information is a subset of the information that the NRC has available to it. You might want to ask yourself whether there is any mechanism by which you can have that information available to the panel members so they can use it in deliberations.

Okay. What else? This will look better. I'll type it up. You should have a copy of that tomorrow.

DR. LEE: (Unintelligible.)

DR. BOYACK: My perspective on here is if you have an expert panel convene, they are going to go ahead and look at whatever set of information they have. They are going to go ahead and come out with these revisions to the table hopefully and we are going to state the rationale. Part of this may be more softly linked than other parts.

DR. BOYACK: Absolutely.

DR. KRESS: The PHEBUS part for me would be transport and performance.

DR. BOYACK: What about -- what do you make of these other areas of calculations? We've listed fuel gap models, fuel release to gap models, rim effect models, transport and retention walls.

Is there anything else in the modeling area that you are looking for? Part of the reason I'm asking this is whether or not there's anything that we need to consider and see if it's possible to provide when we come to the next meeting.

DR. KRESS: Well, there is some burn-up models in FAST GRASS computer code. I don't know if we could actually get a calculation but we might look at the models and speculate on how those models for burn-up might effect the results. We might be able to do that.

DR. BOYACK: FAST GRASS is --

DR. KRESS: Argonne National Lab.

DR. BOYACK: How do you spell that?

DR. GIESEKE: FAST GRASS just like it sounds.

DR. BOYACK: Two S's, right?

DR. KRESS: Right. We might not exercise the code. We could actually dig into what it its burn-up model and speculate on how that might affect the results.

DR. POWERS: -- rest is. I guess he isn't the original author of FAST GRASS but he's the guy who took it over.

DR. KRESS: He's the caretaker that's for sure.

DR. POWERS: He's the caretaker of it. They actually published two high burn-up models. In the literature I have to look up the references.

DR. KRESS: It would be worth looking into.

DR. POWERS: But he is obviously going after the high burn-up effect and the rim effect.

DR. LEE: I think from past experience using the FAST GRASS --

DR. BOYACK: Richard, why don't you get a little closer? You can have my chair.

DR. LEE: It's a nice chair. Remember we have included these types of model in the VICTORIA but the number of parameters is required for the user to input to do these calculations are more complicated.

We found that the GRASS modeling was very limited in the application to VICTORIA. I am hesitant to have someone marching down that road with models that was very hard time of calculating or even measuring the fuel. Am I correct, Dana?

DR. POWERS: Yeah.

DR. LEE: That was my experience with VICTORIA.

DR. POWERS: What they did with the VICTORIA code was they tried to upgrade the GRASS modeling sequence to handle a whole bunch of fission products rather than just the fission gas itself. I think they just tied themselves in knots and produced something that is just not usable.

DR. KRESS: My impression is you could use those fission gas models for iodine and Cesium too.

DR. POWERS: I think they are useful for getting gap inventories. I don't think they are useful for looking at the temperature transient once you get into fuel degradation. I mean, I think I'm like you. I'm more interested in looking at the physics than I am the numerics.

DR. KRESS: Yeah, that's what I had in mind, looking at the physics there.

DR. POWERS: I think the numerics has proved to be just intractable. Just really intractable numerics. It's just poor numerics. Let's face it. That's why the original GRASS sequence was written for calculating

inventories, especially in fast fuel. What Rest did was he tried to improve the numerics in the original GRASS.

That's why he created FAST GRASS. That didn't work so they went to PARA GRASS. Then they went from PARA GRASS to whatever they put into VICTORIA. Eventually the peer review panel just said toss that sucker out. It's just more overhead than it's worth.

DR. KRESS: It would be worth looking at the physics.

DR. POWERS: The physics I think is interesting and I think what you do there is you look and you say can I make the translation we usually do now of saying okay, is not the krypton inventories of the gap about the same as the Cesium iodine inventories of the gap. Can you still do that for the high burn-up effect.

We have other things that say let's look at what oxygen potentials are. Is molybdenum going to be in the metal phase or is it going to have transformed into the oxide phase and things like that.

DR. KRESS: I think there are some calculations, oxygen potentials and how they would affect various feeding products. I don't have -- I've seen those curves and it would be handy to have them to look at.

DR. POWERS: There's been a fair amount of attention paid to the rim effect and its affect on oxygen potentials, local oxygen potentials. It's really quite interesting. You think this rim effect as having beaten up, torn up, banged up uranium backslide grains. It's not.

The rim grains themselves are really well formed grains because what they have done is they have discharged all of their dislocations and vacancies into forming these pores. The grains are tiny. They are

submicron grains but they are really well formed.

They are relatively devoid of any fission product even though that is where most of the fission is now taking place because they are plutonium rich and that's where the neutron hits first. It's really quite complicated.

What you then see is this rather interesting peculiarity. That rim is the fracture-tough region. You usually think of porous ceramics as being very friable and what not but this is fracture-tough and it's fracture-tough because the grain structure is tiny and the pores are big so it blocks out all the fractures.

That's going to give us something different in degradation than what we have seen in liquefaction before and you just need to think about it. What is that going to mean? This wetting behavior we all worried about back in the PBF days, that doesn't exist anymore, the weepage into the crack structure. That doesn't exist anymore.

DR. KRESS: When they talk about leaching out. Didn't you guys do some tests at Sandia on the fuel foam lot at higher burn-ups?

DR. POWERS: Yeah. That's something they've talked about in the PHEBUS test. When the original tests were done, we saw massive foaming of the fuel in fairly inert conditions. It was largely ascribed to the fact it was reducing condition. Now we've seen it a lot more. Of course, the speculation is that foaming could be the predominate mode of core degradation rather than candling.

As far as I know, no one has incorporated foaming model into the core degradation codes as an addition to or substitution for the candling

models even though the foaming models was, of course, what we were using in the fast reactor data because that's what fast reactor cores all do. They foam, they don't candle. Those are interesting things that could affect core degradation scenarios.

DR. BOYACK: I sort of have a model in my mind of how this is going to play out. I think Richard or somebody else has alluded to it several times.

As we look at some of these various sources of information, we are going to use either by way of data calculations or whatever else, and that is we have this panel and this panel is the processing agent for a bunch of different information.

Some of it will be directly applicable and some of it less directly. They all use the NUREG-1465 based on later data, methods they'll ponder and they'll have dreams and n nightmares and all this will sort of feed in together.

When it comes out it will be this NUREG-1465 applicability with written justifications sometimes which may be this is just sort of what you think it is. In other cases it may be by relationship to some of these ponderings on the various models or various data sources. We'll try to capture all that.

The fact that something may not be immediately and directly applicable is not as much concern to me because it's going to be processed by the panel as long as they understand what they're doing.

DR. POWERS: Maybe I should just toss in what I did before coming here because I had an inkling that we were talking about high burn-up MOX fuel.

I went through J nuclear materials, old volumes back through 1991 and copied out those papers that I thought were pertinent.

DR. BOYACK: All right.

DR. POWERS: Okay. For what that's worth.

DR. BOYACK: I'm assuming that you saw something you thought might indeed fall into that applicability area.

DR. POWERS: I mean, I found stuff on rim effects, MOX effects, and things like that.

DR. KRESS: The question is how to transfer that information.

DR. BOYACK: We'll just give Dana 20 minutes next meeting.

DR. KRESS: He can give a presentation on all of it. He can summarize all those papers for us.

DR. POWERS: That would be the best way. Wouldn't it?

DR. BOYACK: I'm not exactly sure how you want to do this minus the panel. What do you think, Dana? Is it the type thing where the information ought to be divided up amongst the panel members and each of you take a number of papers? What do you think?

DR. POWERS: I don't know. One thing I can do is for the next meeting I could prepare a digest of what is available in here.

DR. BOYACK: Yes.

DR. POWERS: And say if you want to find out the details of it, here are copies of the paper and you can read it and you'll know as much as I do about the stuff.

DR. BOYACK: Yes.

DR. POWERS: It's not exactly tough reading. Alternatively I think Jason is going to do this tomorrow. You're going to pass out the one that talks about the various types of MOX that exist?

DR. SCHAPEROW: Yes. I have copies of that already.

DR. POWERS: Okay. What I can do is I can pull out the papers that I think provide good overviews. For instance, there's one that is quite good in going through and getting a qualitative review of the status of the rim modeling. It doesn't go down to equations and things like that. Here's what this one tried to do and here's what this one tries to do and here's what this other tries to do.

DR. LEE: Brent, in your previous books for the high burn-up fuel, local and so forth, didn't you already discuss about the high burn-up fuel, the rim effect, the concern with fission gaps? Because I'm interested in the internal pressure of the fuel and so forth that was in summary from those books.

DR. BOYACK: Those documents I understand are probably in publication now. Obviously we have final copies of them. The whole thrust of that was the high burn-up fuel.

DR. LEE: But they also look into MOX, too.

DR. BOYACK: Yes, but I would say that the focus on MOX. Although we ask ourselves a question after you've done the basic work and while you're doing it. That's one of the reasons why I tend to be suspicious of letting it get too broad because it's hard to get the same sort of focus on that.

We came back and asked questions about applicability of the ranking judgements for importance relative to MOX and other situations in the baseline fuel and baseline plants. Obviously we got some result of that. I just don't have as much confidence. That information is worthwhile.

DR. KRESS: I tried to get it off the webpage, NRC webpage, and I couldn't. All I could find was the summary.

DR. BOYACK: Well, like I say, why don't we make available and mail to you the copies of all three reports.

DR. KRESS: I think there was one of them that had the full document and the other two that had the summaries.

DR. BOYACK: Right, because we were just working those very much at the last. Literally I sent it out two weeks ago, the last update pages, so they are ready to go. If nothing else, I can print them off and send them out.

What I guess I heard was this was post 1991. Is there anything else that occurs to you at this point? NRC people, anything that you are familiar with?

At some point what we do is we just sort of start and things will be added to the list so I don't want to belabor this if we've kind of reached the end of the productive phase.

DR. LAVIE: Let me make one suggestion. This is Stephen Lavie of NRR again. This pilot of the MOX immobilization program we contracted with three fuel vendors to do a safety evaluation. I doubt there's original research in those papers but it could be a good biography as to where they got their data.

DR. BOYACK: So this is called -- first it's DOE, right?

DR. LAVIE: DOE Plutonium Immobilization Project.

DR. BOYACK: Do you have any idea who the contact person might be on that?

DR. LAVIE: Bob Martin.

DR. BOYACK: Who?

DR. LAVIE: Bob Martin.

DR. BOYACK: Okay. Bob Martin. Perhaps an NRC person can help me on that. Okay.

As a start this is what I wanted to accomplish. I just wanted people to start seeing a little bit of totality of the types of things that we want to be asking panel members to consider and ponder as they think about this act of processing all this information to this NUREG-1465 applicability update. Okay.

Let me just come back to this. My guess is this is the last thing we'll do during the course of the day because it seems like a natural break after we do that to come back. I'm not even sure this is the format. We may just want to start fresh with another sheet and come back and do something different.

As I was sitting here this morning, there was a certain amount of discussion. Dana right at the start started to ask the question, "Can you tell me what it is that are the parameters that we're looking for?" Of course, the burn-up was one of those, cladding was another.

There was some discussion about the scenario. There may be others and we may want to clarify even more here. I started to try and write a few of these down because I think they are important things to capture.

The first thing I heard was that on burn-up the peak ought to be 75 gigawatt days per ton and the average would be two-thirds of that.

DR. KRESS: Unless we're talking about substantial power upgrades which require flattening both PWR cores and BWR. That's PWR there which may not go to those high upgrades. The two-thirds I think is the standard core like we have now without the cladding.

DR. BOYACK: Okay. So there's a comment here but I'm not sure I've quite got it yet.

DR. KRESS: The average should be higher than two-thirds if we're talking about cores that have power upgrades.

DR. BOYACK: Okay. Got it. Okay. Fine. Appreciate that.

DR. POWERS: We should appreciate the fuel management strategies people may use. In other words, fast burn-up to high burn-up or slow burn-up to high burn-up.

DR. SCHAPEROW: The two-thirds number that I proposed was just based on the idea that they would be refueling the thing in thirds. They put the stuff in, they burn it for 20, they put more stuff in and they burn that -- this they burn up to 20 but the other stuff would burn 20 to 40. They put another third in.

DR. KRESS: Oh, that's where the two-thirds came in.

DR. SCHAPEROW: It has to do with fuel management. They are fueling in thirds. Actually what you see is it is higher than two-thirds

because they do tend to move things around. Maybe like three-quarters might be the right now.

DR. POWERS: Basically there are two types of high burn-up fuel.

DR. BOYACK: So forget the median. It's slow or fast.

DR. LAVIE: Just another aspect of fuel management is that traditionally we have assumed that the higher burn-up fuel shuffled off to the perforated core where it doesn't have the same heat rate.

Westinghouse has recently in order to flatten the fuel put the twice burned assemblies in the center position where they are exposed to higher heat rates. That's an aspect of fuel management that may need to be considered because, as I understand it, the fuel power gap is a function of the heat rate.

It also degrades the decreasing burn-up. For a long time we've assumed that the most susceptible fuel would be a lower burn-up. We are now finding that putting some of those assemblies in the P power position.

DR. BOYACK: Okay. So does this generic term capture that for right now? You wanted to alert us to the reality of that particular approach.

DR. LAVIE: This is something that affected us. We were racheted into a contract of heat limitation because of that aspect.

DR. KRESS: Another aspect of that is I think you could consider at higher burn-ups you are going to have more clad embrittlement. This is likely to change your thinking about what temperature the clad fails to get the gap. I don't know where that fits in up there.

DR. BOYACK: But this is a question of cladding behavior and high burn-up.

DR. KRESS: You may get more clad embrittlement. You are also going to likely get more internal pressure.

DR. TINKLER: I'm kind of thinking though that's really going to be addressed in the other parts of the fuel program unless we think it will radically change the onset of significant --

DR. KRESS: My feeling is it may effect your thinking of when the gap release starts. I don't know that it will but that would be what I would think it might affect.

DR. TINKLER: Yeah, I know, but I guess I'm still thinking that's really under those other PIRT panels for high burn-up fuel, fuel performance. They are going to consider LOCA conditions as well.

DR. KRESS: They may already have talked about it. That's one of the inputs in the NUREG-1465 table.

DR. TINKLER: Yes, it is. I agree. It is. But to the extent it's controlled by -- it's going to be addressed by the other, I'm not sure how -- that's a whole different area by itself.

DR. KRESS: It is. It's a separate area.

DR. POWERS: It seems to me that I would be -- I think we ought to just stipulate that we start our gap release when they tell us to start our gap release.

DR. KRESS: That would be one way to deal with it.

DR. TINKLER: I mean, they're going to look at to see if full performance is comparable. To the extent they determine that full performance is not comparable, then we adjust that.

DR. KRESS: This may be a good way to look at it.

DR. TINKLER: I want to make a general comment here, too. I don't want to see you jump over a rail and grab me by the neck here, but to the extent that a lot of these things we're talking about now are gap release oriented, we should consider that. Bear in mind that for our Flag Ship Part 100 calculation it's gap plus early in-vessel. It's the combined total of those two.

I can change gap by a fair amount and not change the total of those two. I said in the beginning the panel should consider gap because gap releases are what is used for certain accidents. Steve, I'm going to say risk significant just once here.

The risk significant accidents are not those concerned with gap releases. We should consider those factors but we shouldn't let that be the boulder rolling down the hill that drives everything else. We shouldn't be paralyzed by questions about the gap. Really we need to be concerned about gap plus early in-vessel.

DR. KRESS: I think you're exactly right.

DR. TINKLER: Part 100 calculations. I said that once and I won't say it again today.

DR. BOYACK: Sometimes we have to be reminded.

DR. TINKLER: Gap issues are important and control and economically can be a penalty for other kinds of analysis. I'll cite those calculations control at least a lot of the conditions.

DR. POWERS: I'm willing to bet by the end of the day when you take that sum you're going to see radical changes. You are going to see changes in the parts. A lot of the in-vessel release is driven by fuel melting.

That which is coming off has come off and that which ain't coming off ain't coming off. I'm willing to bet that, yeah, you spend a lot of time on the gap because that's where all the change is.

DR. KRESS: I suspect as burn-up increases, your rights of release get bigger and you get more of it out before you get the melt. I think things cut off in the melt and obviously stops there. I think it will drive more of the volatiles off before you get up to that.

DR. POWERS: I think you'll have a couple of changes in kind because you're driving a high burn-up because you eventually saturate the capacity of the clad from the oxygen that is produced by burn-up. You will drive up the oxygen potential of the fuel and you will start seeing higher molybdenum releases than we've ever seen before.

DR. KRESS: That would be my guess.

DR. POWERS: We will probably discuss the fact that we keep seeing the PHEBUS test moving ruthenium around. It may not be releasing it in the piping system but they keep moving it around so you naturally suspect that after we've been in a little higher burn-up, we probably would have gotten the stuff off. There could be some changes.

Now when you go to zirconium, now you go to the niobium fuels, and I don't have the same intuition to the extent of clad fuel oxidation at the cladding fuel interface that occurs with the zirlos and the M-5s because I just haven't seen enough cross sections.

DR. LAVIE: I do believe in the philosophy of not putting emphasis but I do want to point out from a regulatory standpoint that until such time does away with requirement for the fuel handling accident. NRR currently has no basis for approving any fuel design above 60,000 megawatt days per ton rating. The fuel handling accident becomes limited.

DR. BOYACK: Okay. I don't know what I do with that.

DR. KRESS: That means you go back to the gap as the controller for the two handling accidents. You have to deal with the gap.

DR. TINKLER: It may be controlling for the control room operator or other things as opposed to --

DR. KRESS: The thing I don't know how to deal with right now is there was substantial credit in the NUREG-1465 for retention in the primary system as well as the fact that there were some vapors that condensed and there were some aerosols that played it out.

I don't know how to deal with that fraction here based on the new information of PHEBUS and based on the fact that we're probably going to have a higher concentration both for aerosols and fission product which will tend to dry it up more. Those are the effects. You almost have to have some sort of model for that. We may have to go back to MELCOR again.

DR. GIESEKE: One thing that bothers me with trying to sharpen your pencil in all this primary system, transport and deposition, is that you are building all these complicated models on top of very, very crude thermal hydraulics.

People have spent their lifetimes trying to predict aerosol research in a constant temperature bin, for instance and model the secondary

flows around that corner and try to put all that down where they can get reasonable predictions. They still haven't done that.

How are we going to put it in tranket around here with protuberances and irregularities and build up of deposits and non-isothermal conditions. If we expect too much from the predictions, I think we're --

DR. KRESS: It's going to be an issue, though. What are we going to put in for our fraction retained in the primary system in order to get our numbers? When I think about it, I don't know exactly how to deal with that now other than use the same ones that they use in 1465. I know those aren't right.

DR. BOYACK: You're going down sort of a path that I wanted to go down in the last few moments the data pivoted. One of the things that I found helpful in the past is to look to the end and then ask ourselves how we get there.

I tried to do that a little bit in the presentation but we've now had a day of information and discussion so I think it's a good point to come back. A moment ago we were talking about specifying a set of conditions for PWR high burn-up cases. We started to make this information.

Ultimately, of course, what we are going to do is e are going to come in and we are going to say we are going to try to find a way to ask ourselves, gee, what happens is it goes from the gap release phase to the noble gases for high burn-up fuel in the PWR plant on one or 10 transients. Right? That's what we're going to do.

What would be helpful to me now since, again, I don't have the source term technical background, I'm trying to think process. What would work best for the four members of the panel here as we get together

tomorrow? What would work best for us to try and start moving this process along?

Now, one of those other slides, the very gobbie one, basically said that I thought we would come and take the various phases and start just asking ourselves what is similar and what is different about high burn-up fuel.

That was my thoughts in advance of coming here. The question is that a reasonable approach or is there something else we ought to be doing? We may have to flail around a little bit here during this first meeting and half a day tomorrow while we try to figure out the best way to go about this.

Ultimately what we're doing here is we are going back and going to try to do that. Then we are going to try to make sure we capture in fairly clear writing why we have made these decisions. What I just heard you, Tom, say is, "Gee, I'm still scratching my head about some of this." As well you probably should.

Any thoughts? Jim, you want to tell me how we're going to go about doing this?

DR. GIESEKE: You're supposed to tell us how we're going to do it.

DR. KRESS: Well, I like the idea of looking at similarities and dissimilarities and then trying to figure out how they will affect it. For example, on the gap release duration, to me when does the clad get up to a point where it fails? Well, that failure is driven by how strong the clad is, how embrittled it gets, and what is the internal pressure and how much can it stand at the given temperature.

What is different between then and now? One thing that's different it's got a different kind of clad. Another thing that's different is at high burn-up you tend to embrittle it more. Another thing that's different is you tend to increase the internal pressure due to the fission gases.

So you might ask yourself are those significant enough effects to affect this .5 because now it's going to fail at a lower temperature and you'll get there faster.

You'll get there faster for two reasons. One is it's a lower temperature and the other is because your heat rating is probably higher. That's the way I would approach just one of them. Then I would do the same thing for the other parts. I think it's a good approach.

DR. BOYACK: Okay. I'm not quite running out of steam but I am running out of things to say. Before I do an imaginary gavel, is there any other comments or suggestions or guidance?

Charlie, are we going down a path you are comfortable with here?

DR. TINKLER: Yeah, sure. The only thing, and this is I guess related to something Jim Gieseke said. When we talk about some of these phenomena, while they are not thermal hydraulic phenomena per se, our prediction of them is more influenced by our ability to predict the thermal hydraulic behavior of the system than it is the individual models that we have for deposition or other behavior. Transport, for example.

The point I wanted to say is to the extent we have concerns about our ability to predict some of those things, we shouldn't let that weigh too heavily on this differential of high burn-up and MOX relative to lower burn-up

and non-MOX. Some of those things existed when we created 1465 and they exist today.

My own bias is I think we do those things better than we did back on source term code package days. If I offended anyone, I apologize but I think we do.

DR. POWERS: If you're not doing it better today than they did at the time of the source term code package, we wasted a lot of money.

DR. TINKLER: I think it's a safe statement but it could be up for dispute. I think if you really look hard you probably could find parts of the system that are -- lots of parts of the system that are colder than we used to think they were and some that are hotter than we think they were. That's no reason to think on balance the deposition is different.

DR. KRESS: We were waiting for you to do that.

DR. GIESEKE: We talked this morning and he set up a trap for you and you finally got caught in it.

DR. TINKLER: But the only other thing I could offer is we could give you a little more information on the trend of interval calculations that we see today, what they might suggest.

DR. KRESS: Anything would be helpful.

DR. SCHAPEROW: We have not done a comprehensive set of work as Jim had done back in the '80s for the source term code package.

DR. TINKLER: I don't want to suggest that.

DR. SCHAPEROW: We've just got a couple of calculations. I don't know. Maybe you guys have a bunch of them. We only have one or two from the LOCA which we did earlier.

DR. TINKLER: The ones I typically do calculations for are not large by LOCA calculations. I typically do them for sequences that are more representative of severe accidents. They are not high, high pressure. We have some seal leaks and things like that that partially depressurize systems.

DR. KRESS: One thing I would like to reconcile is when I look at the 0 pressure accident calculations in the transport code, I get 80 percent of the stuff that gets released from the fuel transported into the container. 80 or 90 percent sometimes. 70 is a low number.

Then I look in NUREG-1465 and they use numbers like one-third gets released. I would like to reconcile this to figure someway. What did the expert panel, elicitation panel use to decide that a representative source term for this thing ought to be reduced by that amount due to the RCES? What was the thinking? Is that in there?

DR. SCHAPEROW: It should be. It just tells you Expert A, Expert B, Expert C so you don't know the exact person. Each person's rationale is in here for each of those terms that we talked about.

DR. KRESS: They were standing out with them in these calculations. It was 80 percent and they decided they were wrong.

DR. SCHAPEROW: In some cases they had one person running a calculation and two different people looked at it or things like that.

DR. KRESS: If it's in there, I'll try to figure out why.

DR. BOYACK: Just give me one more moment here. One of the bigger challenges of these panel meetings is finding times when you can get together. We would like you to write this information down and then look at your calendars and tomorrow we'll sort it out.

What I've done is looked at my calendar so this is the open spaces. This is the time since the first meeting. November 6 through 9, and I'm talking about full meeting days, in this interval we could have another two or three-day meeting, whichever is appropriate. November 6 through 9 which is a Tuesday through a Friday. November 28 through 30, Wednesday through Friday. That's the week following Thanksgiving.

DR. KRESS: I can already tell you that's not good for me or Dave.

DR. BOYACK: Okay. We can take that one off right off the bat.

DR. POWERS: The November time should be out too, isn't it?

DR. KRESS: Yeah, I think November has the same problem.

DR. BOYACK: All of November?

DR. KRESS: No, just the first.

DR. BOYACK: The 6th through the 9th?

DR. KRESS: Yeah.

DR. BOYACK: You're going to look that up, right?

DR. KRESS: I'm just getting to right now.

DR. BOYACK: Okay.

DR. POWERS: I know that Richard and I are running off to a group grope on the 28th and 30th of November. Actually December 11th through the 14th sure is looking attractive.

DR. KRESS: November 6th through 9th and December are both ACRS full meetings. That sort of rules those out.

DR. BOYACK: That's why you go ahead and do this thing as quickly as we can.

Would the rest of you then take a look at these two dates.

DR. POWERS: I know I'm gone the 28th through the 30th.

DR. BOYACK: Now, of course, we have to check with our French group also.

DR. POWERS: The French will be out the 28th and 30th.

DR. KRESS: December 11th through the 14th looks good to me.

DR. POWERS: I am definitely open on the 11th through the 14th.

DR. BOYACK: So we'll talk about this more tomorrow but we whittled down the list considerably. That's why we do it sort of soon. Your NCRS calendar is about three months ahead?

DR. POWERS: One year.

DR. BOYACK: Okay.

DR. KRESS: Except for subcommittee meetings which are sometimes one day ahead.

DR. POWERS: One thing, Tom, we could look at those -- you get tied up in case you have to come to P&P.

DR. KRESS: That usually screws up the whole week for me.

DR. BOYACK: All right. Well, for the moment we'll revisit this but I'm sort of hearing right now that two or three days during the December 11th and 14th and maybe a three-day meeting at that time.

Any other comments or questions before we adjourn for the day?

DR. SCHAPEROW: For tomorrow morning come right to the front desk in the lobby of this building and we'll pick you up there.

DR. KRESS: We're meeting here again?

DR. SCHAPEROW: Yes.

DR. BOYACK: Same room.

DR. KRESS: Can we leave stuff in here?

DR. SCHAPEROW: Yes, as far as I know.

You've had meetings in this room, Brent, right?

DR. BOYACK: Yeah, but it's been some time.

(Whereupon, at 5:03 p.m. the meeting was adjourned.)