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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

140TH MEETING

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WEDNESDAY,

MARCH 26, 2003

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ROCKVILLE, MARYLAND

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., George M. Hornberger, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

GEORGE M. HORNBERGER, Chairman

RAYMOND G. WYMER, Vice Chairman

B. JOHN GARRICK, Member

MILTON N. LEVENSON, Member

MICHAEL T. RYAN, Member

ACNW STAFF PRESENT:

JOHN T. LARKINS, Executive Director, ACRS/ACNW

SHER BAHADUR, Associate Director, ACRS/ACNW

NEIL COLEMAN, ACRS Staff

HOWARD J. LARSON, Special Assistant, ACRS/ACNW

MICHAEL LEE, ACNW Staff

EXPERT PANEL:

DANIEL BULLEN, Iowa State University/NWTRB

ROD EWING, University of Michigan

RON LATANISION, MIT/NWTRB

MAURY MORGENSTEIN, Geosciences Management Institute, Inc.

JOE H. PAYER, Case Western Reserve University

ALSO PRESENT:

ANDREW C. CAMPBELL, NRC/NMSS/DWM

ATEF ELZEFTAWY, Las Vegas Paiute Tribe

JOHN KESSLER, EPRI, Inc.

TIM McCARTIN, NRC/NMSS/DWM

DON L. SHETTEL, Ph.D., Geosciences Management Institute, Inc.

PETER SWIFT, Sandia National Laboratory

ENGLEBRICHT von TIESENHAUSEN, Clark County, Nevada

ABRAHAM E. VAN LUIK, U.S. Department of Energy

I N D E X

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

8:46 A.M.

CHAIRMAN HORNBERGER: The meeting will come to order, please. This is the second day of the 140 meeting of the Advisory Committee on Nuclear Waste. My name is George Hornberger, Chairman of the ACNW. The other members of the Committee present are Raymond Wymer, Vice Chairman; John Garrick; Milton Levenson; and Michael Ryan.

Today the Committee will continue the working group on NRC and DOE performance assessments, assumptions and differences.

Mike Lee is the Designated Federal Official for today's initial session.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's sessions. If anyone wishes to address the Committee, please make your wishes known to one of the Committee's staff. It is requested that the speakers use one of the microphones, identify themselves and speak with sufficiently clarity and volume so that they can be

readily heard.

We are in mid-course here on our working group and I again will turn the meeting over to John Garrick.

DR. GARRICK: Thanks, George. I just want to again remind the participants that what we're trying to do here is develop increased understanding of the performance assessment work that particularly we have tried to put the emphasis the source term by which we mean the mobilization of the waste and the development of their radionuclide release conditions.

Also, I do want to emphasize that the orientation here is not so much compliance as it is trying to understand and I'd like to continue to emphasize that. I'd also like to be able to walk away from this meeting having some indication of what the experts think is a realistic approach to this whole issue, again as opposed to necessarily putting the focus on compliance.

We, as a Committee, we have given lots of emphasis to the issue of trying to establish a realistic reference point against which to measure things like conservatism and we've also given lots of emphasis to trying to stress the concept of evidence-based performance assessment as opposed to assumption

based in support of assumption based. And I use the word evidence as oppose to data because evidence takes on a much broader meaning than data and includes methods of analysis, analogs and a whole bunch of other inputs.

One of things that we were very much interested in trying to come to some grips with in the work shop was given that the performance of the repository is driven by a relatively small number of radionuclides, we start out with some 300 that are radioactive and we end up with some 3 to 5 that dominate the risk and depending on the time segment, it may be 1 or 2. So an idea that we have suggested to the staff and that we would like to hear more about and we're hopeful that we're getting some of that today is starting somewhat with the results, namely what the dose is and peeling back the model to see what's driving those results almost on an individual radionuclide basis. Some of that we expect to hear about today.

Two things came up yesterday that are clearly centers of discussion in regard to the themes that I have tried to articulate. One of course, the key questions about the source term and the uncertainties associated with the source term and what

are really the important drivers.

The other is the whole issue of the biosphere and the dose uptake, I think deserve some revisiting, again, not so much in the context of the compliance issue, but in the context of understanding what really constitutes a reasonable approach and results.

So with that, we're going to continue. I do want to do a little clean up of an item that was left open yesterday. Abe Van Luik did apparently some more homework on a couple of questions he was asked and I want to take this opportunity to get that cleared up and then we'll move on with our agenda.

DR. VAN LUIK: Thank you. Abe Van Luik. Yesterday, in response to a question on the design of the invert, I misspoke. I did some checking yesterday with the help of several individuals here. The structural steel to be used is going to be a carbon steel in the invert, just to keep things in place until after closure. After that, there will be basically that's a sacrificial material. It will rust and it will allow settling of the emplacement module on to the rock over time probably. So I just wanted to clarify that and I appreciate the opportunity to do so.

DR. GARRICK: Okay, thanks. All right, I guess we're ready to start the program and we're going to -- we're addressing the agenda item that's title simplified models of key contributors of dose traced through various modules, something I was just talking about. And I guess Peter Swift?

We'll have to make some adjustments on timing here given the imposition of the orange alert, so I'll look to the speakers to help us in that regard.

MR. SWIFT: Is this microphone loud enough? Can you hear me? It's okay? Here's the pointer.

By way of introduction, I'm still the same person I was yesterday here, Peter Swift. And for those who weren't here yesterday, I am at Sandia National Laboratories and I'm also a manager within Bechtel SAIC of the performance assessment strategy and scope group within which the TSPA is performed.

I'm going to try to follow the -- try to cover the specific items on the agenda for this session. May I have the next slide, please?

(Slide change.)

MR. SWIFT: So in particular here, I'm going to start off with the overall results of the

nominal performance scenario. And go through the total dose and the major contributors through time and a chronology of selected events. These are -- it's not a comprehensive chronology of everything in the performance assessment. That would be too much, but events, I think, are probably of interest to this group and then I'll trace neptunium and Technetium through the system, component by component and this is an important point here.

There are additional results in the backup presentation. I'm not sure what slide number the backups start on, around 20 or so. A lot of backups here. The backup slides are a presentation I made to the Technical Review Board in January for those of you who saw that they are -- there's been minor wording changes, not thoughts have been changed since then.

There's a lot of information in those backups that I'm not going to be able to have time to go through here, but in particular, some of those backups may be interesting for comparison to some results Tim McCartin will be showing later on.

In the interest of time, I'm going to stick to what's in the main part of the talk.

Everything I'm showing here are -- they're called draft examples here of draft in the sense of

these are not our license application results. These are preliminary results, but they are, all of them are taken from existing analyses that are available. Everything is shown as a mean value except the next slide. Everything else is simply the mean of 300 realizations, so you do not see the full display of uncertainty. It is there for every one of those analyses and it would just simply be too time consuming to show it for this meeting. You'll see in the next slide what I mean by that.

The list there, the documents, again there should be a list of references at the end and at the bottom a disclaimer here. We do not have models, certainly therefore not results yet for the LA work.

Next slide, please?

(Slide change.)

MR. SWIFT: All right, this is nominal performance, no REMs per year, dose and net access, a log time scale from 10 to a million years and this happens to be from our most recent configuration of the model, the one we used in the so-called one-on analysis that I have been presenting since September.

And this is the case 12 of that analysis that has everything working, all the components are turned on, so this is essentially our current best

estimate of nominal performance. The mean shown in red, I apologize to those of you with black and white back in the audience. This is tough and it's going to get tougher. I apologize for that. I thought we'd have color.

The 95th in black and median in blue and a 50th percentile also in black there.

DR. GARRICK: Peter, can you just quickly tell us the principle of changes in these results from previous results?

MR. SWIFT: Sure. You will see almost no changes between this and results since the summer of 2001, i.e., the so-called SSPA, the Supplemental Science and Performance Analyses and the analyses used to support final environmental impact statement. This looks very much similar. I'll explain -- well, I'll explain it right now.

In this particular run here, we have one early waste package failure forced to occur in every realization. As Bob Andrews mentioned earlier yesterday, our data base on industry surveys suggests less than one per realization, but in order to get a reasonable sampling here, we forced one in each one.

So the doses in this period here are due to those early failures due to weld flaws and they're

simply higher than we would have seen a year ago.

Now the other major differences that you ask about here, back in 2000 for the site recommendation performance assessment, we had a model that had no early failures. There were no doses at all for nominal performance before 10,000 years and we had a model which showed earlier waste package failure and a higher peak dose. So that mean curve looked more like that. And that will be familiar from -- well, from winter of 2001.

Further work in the spring of 2001 suggested that that was an overly conservative approach to corrosion. We produced a model that showed a much longer package life and had a curve that looked much like this but without even slower out in there. That would have been the SSPA, the Supplemental Science Performance Analyses of the summer of 2001.

On further thought, we decided we were not prepared to defend the temperature-dependent corrosion model in that, so we allowed corrosion to proceed at its higher temperature rate throughout the simulation and that pushed waste package failures back from about this time here, back to their current mean time of failure of 400,000 years.

Peak dose from back in 2000, 2001, was at somewhat higher, including secondary mineral phases in our neptunium solubility limit model, pushed the neptunium doses at later times down some and that accounted for slight lowering up here.

I apologize, I don't have a slide that shows those earlier results. Is that what you were looking for?

DR. GARRICK: Yes.

DR. EWING: One other, just for information, so there's a drip shield in this system as well?

MR. SWIFT: Yes. And there was a drip shield in all those results I just talked about. Yes.

DR. GARRICK: Can you pinpoint the change in the corrosion model that affected the dose between 10,000 and 100,000 years the most?

MR. SWIFT: I'm going to ask Bob Andrews to answer that one.

Bob?

This is between the TSPA-SR when we had failures occurring say 30,000, 50,000 years and present where failures were occurring around 100,000.

DR. GARRICK: The reason for this is that range, the best estimate dose is reduced by many

orders of magnitude and it's not --

MR. SWIFT: During that time period.

DR. GARRICK: Yes, during that time frame.

MR. ANDREWS: Just to clarify, it was the temperature dependency that we based on the corrosion rate between -- during the summer of 2001. There was some discussion about that late yesterday about the technical bases for that temperature dependency. We felt that it might be noise, might be real, might be noise. It was an arhenius type relationship that was used. And we took the temperature dependency back out in these calculations.

MR. SWIFT: That moved the main slope from about here that way. I think John is asking about what moved it from TSPA-SR to the right.

MR. ANDREWS: I talked about that yesterday. It was the stress corrosion cracking representation and the 20 percent of yield strength versus 80 percent of yield strength on the stress cracks.

DR. GARRICK: Okay, all right. So that's where most of it came from.

DR. EWING: A follow-up question and not such a simple one. I look at this as kind of a breakthrough curve. You know, when does material --

MR. SWIFT: I'll actually show some--

DR. EWING: Looking at this, have you thought about if instead of the repository, you were looking at a uranium deposit, do you think that a uranium deposit, similar geology within a few thousand years you could see it at 18 kilometers?

You don't have to have the answer, but it's something to think about, just to see whether this seems reasonable.

CHAIRMAN HORNBERGER: Aren't all those uranium mines transient?

(Laughter.)

MR. SWIFT: Yes. You're imagining a uranium deposit without waste packages around it.

DR. EWING: Right, right, just transport. I mean that's pretty fast to move anything.

MR. SWIFT: Sure.

DR. EWING: And I just wonder whether --

MR. SWIFT: Yes.

MR. ANDREWS: Let me try, Rod. This isn't uranium. This is Iodine and Technetium dominantly that are driving the advective transport and --

MR. SWIFT: It's Technetium. I'm come to that.

MR. ANDREWS: And also neptunium, so it's

not uranium breakthrough curve we're looking at.

DR. EWING: Well, neptunium, uranium --

MR. ANDREWS: There are differences.

DR. EWING: -- Technetium use arsenic. They're polyvalent so I mean just to -- when everyone looks at this, as John says, well what critical parameter did you change to shift it, but I am asking myself does it match general experience?

That's a very important question, I think.

MR. SWIFT: Bob, let me comment on that. These are -- you'll see in a minute here that these are very small numbers and the early period here is dominated by Technetium and not by neptunium and members of that decay chain.

There are small concentrations getting through.

DR. EWING: Now the concentrations, I don't focus on it. It's just the speed at which any element moves through the subsurface is a little bit surprising.

MR. SWIFT: Okay, let me keep going here.

DR. EWING: I'm sorry.

DR. BULLEN: Dan Bullen, NWTRB. Quick question, Peter. You mentioned that you took out the temperature dependencies and you went with the high

temperature corrosion rates or what? Actually, which corrosion rate did you go with is the question.

MR. SWIFT: We assumed that the -- it was the higher one is the answer to your question. As the repository cools in our SSPA model we allowed corrosion rates to slow after 2,000 years or so. In this model they don't. They remain at that somewhat higher rate. It's not a huge change.

DR. BULLEN: Thank you.

MR. SWIFT: Next slide, please.

(Slide change.)

MR. SWIFT: I've got three slides here. This is the -- the other two are just for your information. This is the one I want to focus on on the screen.

This is the inventory in the system. It's a slide we don't show very often, but I think it's a useful one.

Total curies on this axis and log time on that one. This is not necessarily the total inventory. This is the inventory that we model. So the very short lived, very high radioactivity things are not included in here.

This is the inventory that matters for long term performance and what I've shown on the first

of these three slides are the species that are closest to the total, the ones that dominate the total through time, plus two others, Technetium and neptunium in blue and green which most of the repository history are not the largest single contributors to the total, but they are important dose contributors.

So what you see here at early time, cesium and strontium, came off very quickly. This plot has no transport, no retardation. Imagine that the waste just sat exactly in one place for a million years. This is what its activity would look like. Those these are just the K curves and in growth curves.

Americium-241, it's a hugely important player. At a thousand years, it is essentially all of the total inventory.

Plutonium-239, a dominant contribution out at near 100,000 years. Plutonium-240. One of the important points of this is that none of those species I just mentioned, the ones that dominate the total show up as major contributors to dose. The system is effectively removing the dominant contributors to activity.

And you can come back to this or say this is a reference slide that -- it puts things in perspective when we see what it is that we're counting

the dose on compared to what it is that the system is containing.

Next slide.

(Slide change.)

MR. SWIFT: This just shows other species here. Note that we do have ingrowth occurring. That would be Lead-210 coming off from Thorium-230.

Next slide, please.

(Slide change.)

MR. SWIFT: Yes, there's Thorium-230 and Iridium-226 ingrowth also.

All right, with that shown, now let me show you what contributes to the dose.

Next slide.

(Slide change.)

This one I truly apologize to those people who have black and white. That's hopeless. I thought we'd have color there.

All right, the important things to see here are this curve here which I believe is brown, if my eyes are doing okay. And Technetium-99 in pink. There is Technetium-99 through 40,000 to 50,000 years. Technetium-99 is a total dose curve essentially. That's a logarithmic scale. The other things are much, much smaller.

After about 100,000 years, total dose curve is very close to Neptunium-237. Those are the things driving the dose. There are a bunch of other things that pop in here in between, but if we take that apart you'll see that both Technetium and Neptunium are pretty important on through there.

Other things worth noting here, the Carbon-14 shown in red there. That is essentially an artifact of our having chosen to model Carbon-14 as if it were nonreactive in ground water. We don't believe it is nonreactive. We believe it will be very active.

DR. GARRICK: Yes, so why do you do that?

MR. SWIFT: Simplicity. When we first made that assumption, we did not -- remember, in the long time scale, it's relatively short lived. We did not think we'd be worrying about it. And now it's popping up in our plots early time.

It would be costly to develop a reactive transport model for carbon in ground water. We haven't done it. We do not believe that -- we do not believe that's a realistic curve. It is surely an upper bound on the contribution of carbon to ground water dose.

DR. MORGENSTEIN: If the mountain is breathing, have you looked at C-14 relative to this

gas phase?

MR. SWIFT: You mean as a gas phase? Yes. We have.

And the contribution of that to an individual dose at 18 kilometers is trivial. Dilution and dispersion in air is enormous.

We have actually looked at the possibility of all the Carbon-14 could have gone into the air phase as well. We're not going to double count it both ways, but we looked at that possibility and this is the way it gets to a larger dose, put it all in the water.

DR. RYAN: Question. One of the important parts to me of this graph is the Y axis and for Carbon-14, for example, when the major contribution is a maximum of some are around 10^{-5} millirem per year which is trivial, and you've made a conservative assumption that it's all reactive -- I'm sorry, it's all nonreactive --

MR. SWIFT: Like Technetium or Iodine.

DR. RYAN: To me, that puts the question about Carbon-14 to bed.

I mean its contribution is so far below the radar screen that it seems like it should be brought to closure.

CHAIRMAN HORNBERGER: Why do the Iodine doses, why aren't they higher earlier?

MR. SWIFT: Well, I'm sorry, I can't -- I don't have an answer for that. That's where they fell. They are not being retarded in the system. They're coming through. It's the relative abundance of Iodine and Technetium is what we're seeing here.

DR. BULLEN: Peter Bullen here. It's only one package, that's why they're not higher.

MR. SWIFT: Yes, thank you.

DR. BULLEN: It's only one package.

MR. SWIFT: Yes, one package per realization until you get out here until you start to see it climbing steeply is all we're saying.

Until the drip shields fail, this is entirely diffusive transport coming up.

(Slide change.)

MR. SWIFT: The next slide, this is just there for completeness. We've reproduced the key species of Neptunium and Technetium on this and shown the rest of the inventory. We've also reproduced Iodine in that state. That is simply there for completeness. If somebody wants to find out where their favorite radionuclide went, it should be on one of these two plots.

DR. GARRICK: Was the solubility samples on this calculated?

MR. SWIFT: Yes, yes.

DR. GARRICK: For Neptunium?

MR. SWIFT: Yes, for Neptunium, yes. No, I'm sorry. That's a bit of a misstatement. It is actually calculated dependent on primarily pH, but also temperature within the -- it is not actually a sampled input parameter. It's a calculated one, but it has a fairly broad uncertainty range on it.

DR. EWING: So when you calculate it you assumed that it's NP-205?

MR. SWIFT: I'm not the person to talk about a solubility model, but we'll probably come back to that and I can show you at least -- you can skip ahead and look at it. The very last figure in that handout has our calculated Neptunium solubility curve.

DR. EWING: All right, thank you.

MR. SWIFT: So you can go ahead and take a look at those.

Next slide.

(Slide change.)

MR. SWIFT: All right, this is the chronology that the agenda asks for. I think this is my cut of what I think would be useful stuff to know

about. Many of these things you can actually see their impact. Some of these things you can see their impact in dose curves, others you can't.

The climate changes. At 600 years, we go from our present climate to a monsoonal climate. That does show up in some of the plots, particularly the backups. At 2000 years, we go from a monsoonal climate to a glacial transition. At 38,000 years, we have the first full glacial climate. That spike shows up very prominently on all the dose plots, so if you want to know where 38,000 years is in a log plot you can look for it in a dose plot. There's a little step in the dose which corresponds to that water table rise that boosts -- stuck in the UZ or the SZ.

Temperatures. And all of these results here, well, yes, the results, observations, are made with respect to the 2001 Supplemental Science Performance Analyses. That's the one where we had high and low temperature. There's one exception on the next page.

So the peak package surface temperature for the high temperature operating mode, 160 degrees C., low temperature operating mode which had a longer ventilation period, it was below boiling at 84 degrees.

The time for the high temperature operating mode at which temperatures fell below boiling on the waste package surface for CSNF, this is all actually for CSNF, commercial spent nuclear fuel. Seven hundred years on the package surface, it falls below boiling and at the drift wall at 600 years. And keep in mind that for the low temperature operating mode it was never above boiling at either of those locations.

Next slide, please.

(Slide change.)

MR. SWIFT: Drip shield failures. The first failures by corrosion occurred about 28,000 years and about half of the realizations showed drip shield failure by 100,000 years. And Mark can correct me on this if I'm wrong, but I believe that once the drip shield started to fail, they went quite quickly and they would all go.

The early failures here for the Supplemental Science Performance Analyses and for the final environmental impact statement analyses, we assumed there would be one or two packages failing in each realization of the system. There was actually -- Bob Andrews said we could expect a number of .26 packages. That's correct. On a per realization basis

that gives you a probability of .23 that any one realization would have either one or two failures in it.

In order to get better statistics on those early releases in subsequent work we've just gone ahead and forced one package to fail per realization.

We will for the license application, we will once again use some sort of data base assumption about waste package failure, I believe.

Waste package failure by general corrosion for the SSPA high temperature, the first failure is about 110,000 years and 40 percent of the packages were still intact in a million years.

DR. LATANISION: Peter, just a point of clarification. That is based on uniform corrosion rates measured at temperatures in what range?

We did talk about this a bit yesterday.

MR. SWIFT: And Bob Andrews gave you an answer yesterday that -- Bob, the temperature range at which the general corrosion data was collected was?

Did people hear that answer? I didn't.

Bob, can you -- did you hear it?

CHAIRMAN HORNBERGER: It's the reporter who has to hear it.

MR. SWIFT: Somebody else say it. I

didn't hear it, that's all.

DR. EWING: Twenty five degrees C.

DR. LATANISION: Thank you. And then what, it's extrapolated to look at temperatures over the range of above boiling for some extended period?

MR. SWIFT: Yes, the temperatures that I just discussed in the previous slide. Yes.

DR. MORGENSTEIN: Peter, this is in a chemistry base that's essentially a saturated zone?

MR. SWIFT: This is based on the evolved water chemistry from a thermal hydrology model. This is not --

DR. MORGENSTEIN: Right, we're starting the saturated zone waters.

MR. SWIFT: Yes.

DR. MORGENSTEIN: So essentially you have the repository in the saturated zone?

MR. SWIFT: No.

DR. MORGENSTEIN: Chemically?

MR. SWIFT: No, because water wouldn't -- if there were unlimited supplies of saturated water, it would evolve quite differently in thermal hydrology model. We have small amounts of water which do concentrate very much in our thermal evolution model.

DR. BULLEN: Bullen, NWTRB. My

interpretation was was that there wasn't an extrapolation of those rates beyond 95. It was if it was above 95, it didn't corrode at all. Is that -- Bob, do you want to comment on that one?

MR. ANDREWS: Yes, the initiation of aqueous corrosion was assumed to occur at the point where the most deliquescent salt was on packaged surface. I believe -- I'm not sure which salt was assumed, but that was generally at a relative humidity of about 40 percent and I'd have to verify that to be honest with you.

So once you hit the relative humidity of 40 percent and then you'd have to compute the temperature does occur at, then it was assumed that humid air/aqueous corrosion processes could initiate and their rates would be those sampled rates over the entire distribution of possible rates which are over a range of different chemistries. They were not saturated zone chemistries that were sampled in from the laboratory testing of weight loss and other corrosion rates, for the general corrosion rates.

The initiation criteria was humidity, not temperature.

DR. BULLEN: Bullen, Technical Review Board. There were no localized corrosion models in

that. It was all general corrosion?

MR. ANDREWS: No, as I said yesterday, and maybe I should be back up there because we did talk about this yesterday. The localized corrosion model is in the general degradation model for the waste package and the drip shield. However, the chemical environments on the package and the drip shield were such and the temperature were such that it was never initiated.

DR. BULLEN: Okay.

MR. ANDREWS: There is a localized corrosion model. It was just never initiated.

MR. SWIFT: It was not an assumption there was no localized corrosion. It was a model of result that there was no localized corrosion.

MR. ESH: This is Dave Esh, NRC. And it was sodium nitrate salt at 120 degrees C. I believe.

DR. PAYER: I'll just make the observation that when we say general corrosion in the way it's being handled here, it's the passive, the material in the passive state, its corrosion rate and there have been a couple of approaches to determining what that is. One is looking at the current density on electrochemical polarization measurements and turning that into a penetration rate. And also weight loss

and other spectroscopies trying to measure very small penetration rates with microscopy and so forth out of long term, longer term weight loss type specimens. So that's, I think, where the basis of this comes from. It's really the passive corrosion rate, yet passivity remains stable.

DR. LATANISION: Latanision. I agree with that, John. My concern is that if we're working above the boiling point, then the question becomes what sorts of solutions are we using as their representative environment. And obviously, they'd have to be concentrated because we're not pressuring the system. I don't think there are measurements of passive current densities under those circumstances. So I think the data that exists is -- unless I'm really unaware of data that exists, I think those experiments really haven't been done.

DR. PAYER: My understanding is some tests have been -- I mean there's some crevice corrosion results and that up to 120, 130 centimeters, but that there would be passivity, polarization curve measurements there. I think they're in the long term, but again, I don't know the full inventory of data either, but there's been some electrochemical tests up there.

MR. SWIFT: The last points here that the transport times in the natural system, I'm not going to describe them here in words. You can see them on some slides I'm going to show in a minute. You can infer them anyway and in the backups are some actual breakthrough -- mean breakthrough curves for the unsaturated zone and a whole distribution of 100 breakthrough curves for the saturated zone. It's upcoming.

Can I have the next slide, here?

(Slide change.)

MR. SWIFT: All right, I'm now going to walk through Technetium and Neptunium transport through the modules suggested here on the agenda which, in our world of the barriers that we're going to talk about, obviously we don't have radionuclide transport in the overlying barriers in the infiltration barrier or the unsaturated zone above or in the drip shield. So the radionuclide transport of interest here that I'm going to walk through would be from the waste form which in this case I'm going to show -- will include the cladding; the waste package, the invert, the unsaturated zone below and the saturated zone.

Next slide, please.

(Slide change.)

MR. SWIFT: All right, this plot here, one of those model result plots we don't usually show. But there are reasons why it's sometimes confusing to show things in this way, but this is a release rate of curies per years. This is not dose and it's not mass either which is important to keep in mind. Over a million years and again, I apologize for the color on this, but if you work your way down the lefthand side of the figure, they're in the same order they are there.

So this is the activity flux curies leaving each model component. These are the GoldSim model cells that we're talking about here for the waste form in the upper curve for the waste package, the invert, the unsaturated zone and the saturated zone which are very close together there, those two.

And the first thing you see here, what are we looking for? This is the total that's really here for reference and I'm going to show the Technetium and Neptunium in the next few slides. But overall, you can see that there is, for example, roughly a thousand year delay here before it's created by the unsaturated zone below. There's relatively little effect here from the saturated zone. That's because everything

that is effectively retarded by the saturated zone has also already been effectively retarded by the unsaturated zone as modeled. So if you put them in the model, you see very little difference when you add in the saturated zone.

That does not mean the saturated zone isn't doing anything. If the unsaturated zone weren't there, you would still see the saturated zone curve about where it is.

All right, keep this one in mind and if you have questions, I can explain that. Let's go to the next one here.

DR. LEVENSON: Before you leave that one, I understand the term leaving the waste form and leaving the waste package and leaving the invert, but do you really mean leaving the saturated zone? Where does it go?

MR. SWIFT: It enters the pumping well. This goes back to our 3,000 acre feet discussion from yesterday. The radionuclides, all activity in the saturated zone is placed into a 3,000 acre feet for the purpose of modeling.

All right, next slide here.

(Slide change.)

MR. SWIFT: Here we see Technetium

transport. So if you wanted to see what the Technetium, where the Technetium is in the system at any given time, this is it. And what you can see then is that -- and keep in mind, this is again a nominal performance, so out until somewhere about here we're seeing releases from those early failures and that's one package per realization and until about in here somewhere we are in an entirely diffusive environment so things are being driven by the concentration gradients.

So we see the effect here of Technetium moving from the waste form to the waste package to the invert going across there. That's plausible and acceptable to see it. We're getting more there than we were back over there.

And what we see is that the -- at later times the Technetium was moving quite effectively through the system.

Next slide, please?

(Slide change.)

MR. SWIFT: The Neptunium transport through the system, again, waste package -- sorry, waste form, waste package, invert, unsaturated zone, saturated zone.

There is a -- on the waste package curve

here before someone asks me about it, I'll call attention to it. It sure caught our eye when we first saw it. There's discontinuity in the plot. This is not a plotting error and this reflects -- it's a real model result, whether it's a real physical result, we can all be the judge of that. Well, not unless we actually have the real system.

The first thing I want to point out there, these are very small numbers. We're talking picocuries here out of the entire inventory of the repository.

What's happening in here is recall we're in an environment that is entirely diffusion driven and we are using calculated solubility limits, not sampled ones. We actually have back diffusion occurring here. We have a very small diffusion of Neptunium from the invert into the waste package occurring in a handful of realizations in the model in this case.

What's happened in the model is that -- if I could go to the very last slide in the packet, it's the last backup, number 41.

(Slide change.)

MR. SWIFT: This is our Neptunium solubility limits function of temperature and pH.

These are as calculated in the model. What's happened here is that -- you see there's primarily a pH function. Neptunium becomes considerably less soluble around a neutral pH and many orders of magnitude change in the solubility limit. You go away from roughly neutral.

In a handful of realizations, we have a sufficient effect at 625 years. It's described in the previous few slides in the packet in some detail. The pH climbs from somewhere around 3 to somewhere around 7, 625 years corresponding to consumption of iron within the package, in other words, one package has failed here.

As the pH climbs, the solubility limit within the package drops, however, the pH outside in the invert is not controlled by the in package chemistry. It's somewhere out in this range here. So the solubility outside the package is actually higher than that. In the package, the concentrations in the package fall and you actually get, as modeled, a very slight diffusion, very small numbers back into the package for a few hundred years and a few realizations and that's what created that effect in the model.

DR. EWING: Peter, what if you have the wrong phase for your solubility correction? Or

solubility calculation?

MR. SWIFT: Yes?

DR. EWING: How different result would you get?

MR. SWIFT: The --

DR. EWING: Almost certainly, is this the Np-205?

MR. SWIFT: yes.

DR. EWING: Almost certainly that's the wrong phase.

MR. SWIFT: Rod, you know I'm not the solubility chemist on this. I'm implementing the solubilities that I've got.

DR. EWING: Right.

MR. SWIFT: And from my perspective, you're going to counter this, but I'm concerned primarily about the concentrations in solution, not about the chemistry of the solid phases.

What I want to know is --

DR. EWING: Well, wait a minute.

MR. SWIFT: Is our distribution --

DR. EWING: Solubility doesn't mean concentration. Solubility is with respect to a solid phase.

MR. SWIFT: Sure, right.

DR. EWING: So if you have the wrong solid phase, the solubility will be wrong.

MR. SWIFT: What I want to know from my perspective as a systems person, are the solubilities that I'm -- are the concentrations that I'm transporting, there's a distribution of solubility of concentrations that I'm transporting, do those reasonably capture uncertainty associated with the uncertainty in the actual chemistry of dissolution and precipitation with whatever solid phases are present. And I'm not the geochemist on that.

DR. EWING: But is there someone who could address that question and say well, we've used this and this is conservative relative to the other phases and that co-precipitation will give you a lower concentration anyway? Is the discussion --

MR. SWIFT: Yes, the project clearly has a team of geochemists working on this. They're not here.

DR. EWING: Right. Okay.

DR. GARRICK: Where does the Neptunium solubility data come from?

MR. SWIFT: Bob, do you want to answer that one?

MR. ANDREWS: There's a wide range of

laboratory data. There's Los Alamos laboratory data on Neptunium in some controlling phases. There's Argonne data.

DR. GARRICK: So this does reflect the Argonne data?

MR. ANDREWS: In different controlling phases, these are not controlled for these results by the Np-205 as we talked about yesterday. We showed similar plot yesterday and showed the Np-205, if that were the controlling phase on there.

And I'd like to amplify on Peter's statement, I think. With the uncertainty in the controlling phase, as represented by the uncertainty in the solubility is addressed, is there additional uncertainty that needs to be evaluated? That's what we're still looking at. So we have to accommodate that uncertainty in the controlling phase as it affects the uncertainty in the solubility and the uncertainty in transport, using the risk-informed regulation. I know this is not compliance-based discussion in here, but we are concerned about the compliance aspects of this and meeting the expectations of the WMRP.

DR. EWING: Let me just comment to emphasize how great the uncertainty is. It's very

important to know what the controlling phase is and the mechanism by which radionuclide is retained and the simple way to do it and it's the way many people do it is based on thermodynamic parameters, do a log-log plot we've seen before and see what the stability fields are for different phases. And keep in mind for the uranium oxyhyroxides and silicates there are tens to hundreds of phases that one might imagine forming.

If you take the thermodynamic parameters and you just vary them by less than one percent, a kilojoule, let's say, and you do these log-log plots, the stability fields shift greatly because it's an exponential relation. And so it's very difficult to be sure of what the controlling phase is. And I would offer that whatever it is, it's not this. A very important part is to argue that it doesn't matter and that's what I'm pushing for.

MR. SWIFT: In that regard, what we see, what I see here is that we, in fact, have -- saw limits that vary with pH over about eight orders of magnitude here.

And we actually realize much of that range in this analysis. We do have a very broad range of uncertainty in the treatment of Neptunium solubility that ends up being propagated through the forms

assessment.

DR. WYMER: What pH do you expect?

MR. SWIFT: It varies from model cell to model cell.

DR. WYMER: At this particular point in the system, what --

MR. SWIFT: This could be applied in several different places, but within the waste package, we expect pH to range from somewhere in here to somewhere over here.

DR. WYMER: Somewhere from --

MR. SWIFT: Within the waste package, we see that entire range of solubility.

DR. WYMER: The stuff that leaves the waste package and drips into the invert, what pH is that?

MR. SWIFT: Again, that varies. Tends to be more alkaline over on this side of the plot here.

It's the pH in the packages that's most important to us, because there's more water when you reach the invert, so even if the solubility limits fall in the invert, in general, there's enough more water present that -- unless it's -- it does occur. There may be precipitation in the invert, but in general, the invert will support the transport of more

mass.

DR. WYMER: I would have said that pH as it leaves the waste package and goes into the invert is the most important because if it's around 3 or 4, then it will react with the iron and you'll get a reduction of the Neptunium.

MR. SWIFT: Yes. The volume of water coming out of the package is small compared to the volume of water entering the invert from other sources. So we do not have all that chemistry effect there. The invert chemistry is not dependent on the chemistry of the water leaving the package.

DR. GARRICK: Peter, it's obvious that this is a very interesting area and I hate to cut it short, but --

MR. SWIFT: I am actually done. If we go back to slide, 15, my conclusion.

(Slide change.)

MR. SWIFT: Slide 15. That's it. With that, I'm done. I think I have said anything already on this side.

DR. GARRICK: Well, I didn't mean for you to finish in seconds.

(Laughter.)

I was going to give you a few minutes.

MR. SWIFT: I can read the slide.

(Laughter.)

I appreciate the thought. The points here on this slide, we've heard this before. Technetium-99 and at early times prior to drip shield failure, it's all diffusion in our model.

Later times, advective transport becomes important and Neptunium-237 is the most important contributor.

Neptunium-237 also does release, by diffusion, in early times, but the concentration of gradient is not as steep as it is for Technetium because the solubility limits are lower.

And after the waste packages have failed, basically total dose is Neptunium.

At this point here, I said this right at the very beginning, the things that actually dominate the inventory, the strontium, the Cesium-137, Americium-241, Plutonium-238, 239, are not significant contributors because retardation in the natural system prevents their release while their inventory is high.

The long-lived Plutoniums are effectively retarded in the natural system. That's it.

DR. GARRICK: Thank you. There may be time for one or two questions beyond what we have

asked or are we in good enough shape to move along?

MR. SWIFT: I have one more point. I encourage people to thumb through those backups and I'll be here the rest of the day and I can field questions on those also.

DR. GARRICK: Very good, thank you. Okay, now we're going to hear from the NRC side, Tim McCartin.

Tim, since we haven't heard from you, I'd appreciate it if you'd tell us who you are and what you do, even though we know.

MR. McCARTIN: Good morning. I'm Tim McCartin with the NRC Staff. I'm a Senior Advisor for Performance Assessment in the Division of Waste Management and I've also worked on the regulations, part 63.

And I guess as a bit of an introduction, the work I'm presenting today is a little bit different, but very complementary to what Peter Swift has presented. It's a work in progress that the Committee is aware of. I've talked to this a couple of times already to the Committee, but for others, we're in the process of developing additional capability within the Division to assist us in risk-informing our review of a potential license

application and when I speak of risk-informing, I'm talked about that we have an understanding of how the components of the repository function in relationship to a potential exposure or consequence.

And I guess as Andy Campbell indicated yesterday, we've been doing performance assessment at NRC for approximately 20 years and I think we've spent a lot of time developing our models, understanding how they function, etcetera.

We have not done as good a job being able to transmit that understanding to other people, both NRC Staff, the ACNW, technical experts, etcetera and I know yesterday there was talking of a simplified model and I completely agree in the concept of we need to be able to explain this system better to technical experts, to stakeholders, to the staff, so they understand how the system is working and allow them to then go back and look at that behavior of the repository and decide for themselves whether they agree or disagree and why. And I recall -- I'll go back when we first started having public meetings in Nevada on the proposed Part 63, I remember getting asked a question and I thought I gave a fairly good technical answer that certainly all my PA buddies would understand, but the response was I don't believe

you and I don't think it was an unfair retort back that we haven't done a good job in explaining things so others can understand.

And what I'm hoping to present today is some of the analyses, some of the calculations that we're going to do in preparation of receiving the application and helping our prelicensing interactions with DOE, but also once we get the application, to do these calculations to provide risk-information to the staff to help us focus our review on the more safety relevant factors.

And with that, let me go to the next slide.

(Slide change.)

MR. McCARTIN: And today, I'm going to just talk briefly to the current results as we see them with our TPA code. Then I'll talk about really the bulk of my talk is the performance attributes and analyses that we've done to try to understand those results, make those results more transparent and then finally, I'll summarize with what I believe from that analysis, some of the risk information, the risk insights that we could derive from those analyses.

Next slide.

(Slide change.)

MR. McCARTIN: Current performance assessments and here I'm talking nominal performance so I'm not looking at disruptive events like igneous activity, merely the ground water releases.

Dose within the 10,000 years are influenced by very mobile radionuclides, principally Iodine, Technetium. Beyond 10,000 years, influenced by Neptunium, primarily a somewhat mobile radionuclide. But a few questions, what about the rest of the waste inventory?

Next slide.

(Slide change.)

MR. McCARTIN: Somewhat consistent with what you saw with Peter's slides, today I will be focusing on five particular radionuclides, Technetium and Iodine, principally because those are the nuclides that we see showing up in the 10,000 year period; Neptunium, somewhat a little bit later; but also Americium-241 and Plutonium-240, two nuclides that have a very large inventory. If you look at the inventory of a thousand years by curies, clearly, the Americium and Plutonium account for more than 50 percent of the inventory by curies.

Interestingly enough, you see Technetium and Iodine are relatively a small portion of the

inventory. In addition, if I look at the dose conversion factors, how significant are these curies and most of the radionuclides fall into a dose conversion on the order of 10^6 and that's rems per year per curie per cubic meter in the water. But you can see, Technetium has a relatively low dose conversion factor and Iodine also, a little bit lower. So if we looked at a -- if we did a health risk, you can see these curies actually would get weighted less because their dose conversion factors is less than the other radionuclides.

Just a perspective on the inventory and next slide.

(Slide change.).

MR. McCARTIN: Understanding the performance assessment. What does and does not cause those potential exposures? And I think it's very important. We certainly are aware that Iodine and Technetium arrive very quickly, but it's also incumbent upon in our review, there's a host of other radionuclides, some of which never make it. Well, why is that the case? Do they decay away? Are they held up in the source term? Are they delayed in the geologic barrier? That's part of the understanding of the components of the repository system and I know Rod

Ewing brought up how do you understand the different barriers and their contributions?

Hopefully, the analysis I'll walk through is a way to try to understand what the function and role of the different parts of the system and part of it is related to nuclides that never cause an exposure.

It is very -- part of the problem, although as I said, for years we would come out and show dose curves and try to explain little wiggles in the dose curves and step changes in the dose curves. It's very difficult. There's different nuclides occurring with different behaviors. There's a temperature dependence that also imposes a time sensitivity, because obviously the temperature is decreasing over time. And most importantly, there's certain masking effects. When you have a multiple barrier system, if you have complementary barriers or redundant barriers, depending on what they're doing and when, they can mask the effect of other parts of the system. Trying to get to this -- next slide.

(Slide change.)

MR. McCARTIN: What we are looking at are different calculations that we can do to probe specific aspects of the repository system and look at

not only the sensitivities within each particular attribute, but the relationship between different attributes, this masking effect, if you will.

The repository system works in combination, both the engineering and the geology, work in combination ending up with the final dose curve that you have.

One of the things I'll talk about today is potential performance indicators. To me, although the dose curve is the final result for comparison to the regulation and it's a good measure of health risk, it is not very informative in explaining how the repository works. When I see that oh, the repository is -- it's .2 millirem. Well, I know it's below the dose limit, but I don't have a sense of what that means. I mean typically when we present dose curves there's only two things that people come away with. The doses tend to be low, prior to 10,000 years because none of the waste package has failed. And that's it. That's the only information that generally -- we might spend a couple of days presenting performance assessment results. That's what people walk away with. There's a waste package and the doses are low because of it and that's it.

And I'll try to show the repository system

is far more complex than that and I think what I'm hoping to do is provide some perspective to understand and interpret the performance assessment results, why are we seeing those low doses.

Next slide.

(Slide change.)

MR. McCARTIN: I will apologize. I didn't pay strict adherence to the profile for the ACW outline. I'm relatively close to it and I think I will address all the points, but I didn't adhere to the exact titles, but I'm looking at five particular aspects of the repository system, the waste package, water flow into the waste package, the waste form, the unsaturated zone and the saturated zone.

In terms of performance indicators, as I said, I don't want to -- you won't see a dose curve in my presentation and I will use the different indicators that I'd like to think people could then use to inform whether they believe what we're representing or at least take that information and go back and see if it's consistent with their thinking how these different parts of the system might work. And a dose really, at least in my way of thinking, doesn't help me very much explain or allow someone to do a side calculation as to whether they believe in

the behavior as we're representing it.

For the waste package lifetime is obviously just years, how long does a waste package last? That's a simple performance -- it's a time value.

Water flow into the waste package and waste form for those two attributes, what I will use today is how many waste packages are necessary to release 15 millirem at the drift wall.

Question?

DR. PAYER: I'm just thinking about it now, so I'll make a comment. Joe Payer.

The waste package lifetime certainly is measured in years for the first penetration, but the form distribution amount of those presentations has a major impact on what happens afterwards. I'm sure you know that, but I'm just --

MR. McCARTIN: Yes, absolutely. Yes, yes, yes.

And this particular aspect is a geologic delay. I will do an analysis, look at the release at the drift wall, take the highest release rate and see how many packages would have to be failing at that rate to get a 15 millirem dose.

For the unsaturated and saturated zone --

DR. RYAN: Excuse me, Tim. Just for completeness, 15 millirem where? I mean is that transported through the system then and then out at the receptor --

MR. McCARTIN: I'm assuming it's instantly to the receptor.

DR. RYAN: So you're actually drinking what's coming out of the drift wall?

MR. McCARTIN: I'm using 3,000 acre feed.

DR. RYAN: Okay, all right.

MR. McCARTIN: I'm not drinking --

DR. RYAN: I just want to be clear and I don't mean that as a criticism, but I just want to make sure people realize that you're stylizing that calculation.

MR. McCARTIN: Absolutely, yes. Yes. It's an intermediate point of the PA. I'm using the PA, but I have a release rate coming out at the drift wall. I will use that release rate. I am assuming that it is going into 3,000 acre feet, yes.

For the unsaturated and saturated zone transport, I look at the time it takes from an initial release into either of the zones, whatever goes in, how long does it take before that amount comes out. So if one curie goes into the saturated zone, how long

does it take one curie to come out?

DR. EWING: Tim, just a question.

MR. McCARTIN: Yes.

DR. EWING: I like this approach very much, because it translates into things that people can understand, but you used the phrase performance indicators. Would this be similar to a safety case?

That's actually a loaded question.

MR. McCARTIN: There have been so many definitions of what safety case is, I'm hesitant to --

DR. EWING: That goes directly to the safety --

MR. McCARTIN: Absolutely. Yes, in the concept that I believe that we need a thorough understanding of how the repository works and how it might relate to exposures. And there are -- it is far more than just a dose curve. We don't see a dose curve, oh, it's below the standard, that's it.

You need to go back and as Dr. Garrick mentioned when he opened up, we need to peel back things and understand what really matters.

Next slide.

(Slide change.)

MR. McCARTIN: With waste package and I will apologize to the waste package people here. I am

not going to show anything with respect to the waste package. It is the initial component. There are no releases from the repository until the waste package is breached.

The waste package performance is straight forward to explain in a general sense. It's either breached or it's not breached. There are all kinds of complexities and technical bases behind how it might leak, whether it's through cracks, through small pits, etcetera. There is a lot of information behind that. For this meeting, I did not want to try to get into that particular aspect of the performance, but with respect to trying to provide a simplified view of the results, until the waste package fails, nothing gets out.

With our representation in the PA model, we have -- once the first penetration occurs, we assume water can get into the waste package. Now that may be, depending on if it's in a very small crack or a very small pit, that may be a conservatism, but you'll see how we try to account for some of the ways the waste package fails in later slides. But for now, I'd like to move on from there, trying to give an overall picture of how our code and how we might calculate these intermediate spots to give a

perspective.

That's it for the waste package.

For water flow into the waste package, I want to look at a couple specific sensitivities with respect to the way we represent it in the code. Although it says water flow, I don't want to divorce water flow from solubility limits. They're really -- the impact of water flow is certainly, has to be considered in the context of the solubility limits. You'll see for Technetium and Iodine we have one molar solutions. There's no variation, relatively high. The other three radionuclides, there are solubility limits applied. For deep percolation, in a very simple way we have an initial rate of 4 to 13 millimeters per year is the initial deep percolation. However, we do represent the variation climate over time, so this will change although not that significantly over 10,000 years. Over 100,000 years, you certainly get into some very large glacial cycles and it's much larger. But in 10,000, or around the order, it possibly could double.

Dave Esh talked a little bit about this yesterday and Chris Grossman also. Flow diversion or enhancement. What do we do with the deep percolation rate? We have a representation for -- we can get more

water to a few packages, less water to more packages. In essence, it's a multiplying factor that varies from approximately a little bit more than 10^{-4} to 8. When it's less than 1, obviously, we're getting less infiltration. When it's greater than, we're getting more infiltration.

At the high end, if I took 13 millimeters per year, and enhancement factor of 8 at our high end, we get approximately 2.5 liters of water going into the waste package per day.

You can see it's approximately, it's on the order of 10,000 times less than that at the low end. So we have a fair amount of variation.

The calculations I'm going to next show, what I've done is I've used the TPA code and sampled across the different values, but I am going to fix for a particular analyses, I will either pin things at the high or low value in this situation.

Next slide.

(Slide change.)

MR. McCARTIN: In this case, I'm doing solubility limits and what I'm showing here is once again, how many waste packages do I need to be releasing at the highest rate to get 15 millirem and that's at the drip wall. There's no geologic

delaying. And I'm assuming the highest rate I observe over that 10,000 year period, that's not to say that highest rate -- it does not persist for the 10,000 years, but I'm taking the individual highest rate, although I am using a mean result. I'm sampling and I'm taking the mean release rates.

Not surprisingly, you can see that for the Technetium and Iodine, there's no variation. We didn't change between -- it was the same value for both. What I was I guess a little surprised at. I hadn't thought about it before and that's the value in doing some of these calculations, it takes over 7,000 packages. Why 7,000? I just not only took 70,000 metric tons, 10 metric tons per package. It's not quite that, but it's on the order of the you need more than the entire repository leaking to get you more than 15 millirem.

You can see for Neptunium, there's quite a bit more variation and likewise for Americium and Plutonium, it is far less.

I also did this calculation at 5,000 years and a 1,000 years to try to get a sense of how much did temperature affect this. Our release rates are somewhat dependent on temperature, just to see if that made a big difference. You can see there is some

variation. It wasn't as much as I thought, but you can see 6, rather than 110. Part of this is due to the decay. You've got 5,000 years Americium-241. Its half life is 430 years, so you have part of that is merely due to decay. That gives you a sense of what solubility limits are doing in terms of impacting our calculation.

Next slide.

(Slide change.)

MR. McCARTIN: In terms of water flux into the waste package, the same kind of construct, but here I'm looking at both what is it at the lowest flow, what is it at the highest flow into the waste package and you see at 5,000 years, with the diversion we had we could not get 15 millirem out of the -- at the drift wall with the entire repository.

You can see high flow, not surprisingly. Neptunium. And some of the other Americium, Plutonium, which are certainly solubility dependent. They also are dependent on the amount of water getting in there. See, there's a fairly big difference. All the repository versus one package. So there's a fairly large sensitivity for these nuclides with respect to water influx and solubility.

DR. GARRICK: Tim, do you have a sense of

what the numbers would be if you used the means for the solubility or some other central tendency parameter?

MR. McCARTIN: No.

DR. GARRICK: That's probably in your calculation.

MR. McCARTIN: As you know, this is work in progress.

DR. GARRICK: Right.

MR. McCARTIN: That's a good suggestion. Dave Esh also had some suggestions for me in terms of ways to examine a better depiction of the uncertainty and range here. And yes, we need to do more, but --

DR. GARRICK: That's okay, go ahead.

MR. McCARTIN: Next slide.

(Slide change.)

MR. McCARTIN: In terms of the waste form, in terms of our particular calculation, there really are two terms that I wanted to look at. One, we have a pre-exponential term that modifies the dissolution rate and you can see it varies from 1.2 times 10^3 to 10^6 so it's a three order of magnitude variation.

The dissolution rate also has -- it looks at the particle radius of the fuel for a surface area and how quickly things will be released and so looking

at those two aspects and certainly there's a temperature dependence you'll see. I did a 1,000 year and 5,000 year to try to see is there an effect due to temperature.

Next slide.

(Slide change.)

DR. EWING: Excuse me. What are the temperatures for 1,000 and 5,000 years? I mean --

MR. McCARTIN: I'd have to get back to you on that.

DR. EWING: Is that a difference of a 100 degrees or is it a 1,000? Most of the thermal pulse is cooled quite a lot.

MR. McCARTIN: Yes. Off the top of my head, I just don't -- I don't think it's -- it shouldn't be that much.

DR. EWING: Your data suggests there's not much of a difference.

MR. McCARTIN: I don't think it's that much, but Dave, do you know?

MR. ESH: Yes. This is Dave Esh. I would guess it's in the 60 to 80 degree C. range between those two points.

DR. EWING: Thank you.

MR. McCARTIN: If we look at the release

rate and once again, by low rate and high rate, what I'm doing for the low rate, I'm setting those values to both to give me the lowest value and both those values, both to the extreme to give the highest value. And you can see there is a sensitivity in terms of the release rate, certainly for the Technetium and Iodine. It didn't show up for the solubility limits or the water, primarily because we have high solubility limits for it, but you can see there is an impact there for those two nuclides.

Likewise, Neptunium shows a fairly large sensitivity to the release rate. Down here, there's not that much. I mean partly what you're seeing there is the fact that there's a fairly large inventory of these nuclides, not so much for these. And the release rate is much more effective for the small inventory rating like Iodine and Technetium. Once again, you get an understanding of where are you getting some impact.

(Microphone adjusted)

You mean I've been talking this entire time and nobody has heard a word?

(Laughter.)

I thought it was going well.

(Laughter.)

Hardly any questions.

(Laughter.)

Next slide.

(Slide change.)

DR. MORGENSTEIN: Does it make any difference which waste form you're using?

MR. McCARTIN: For this exercise, I was merely using our base case model which was model 2 which as Dick explained yesterday, sort of in between the lowest and highest that we have in our PA code. We do not have a glass waste form in this particular model.

But once again, it's a good question. We're trying to get together a stable of calculations that we would perform to give us, to give the staff some insights on where is the -- where is the bang for the buck, if you will, in terms of where are the larger safety factors with respect to the potential repository that we need to be examining very focused.

In terms of the unsaturated zone, for our particular model, the Calico Hills nonwelded vitric unit is a very high conductivity porous unit and so it has the potential to significant retard some radionuclides because of the porous flow rather than fracture flow. Once again, Iodine, Technetium are

assumed to be unretarded, but you can see for Neptunium, Americium and Plutonium, there are some -- certainly for Americium and Plutonium, some fairly large retardation factors.

DR. MORGENSTEIN: Is this zeolitic-based sorption or clay based sorption?

MR. McCARTIN: It's on the vitric unit. I'm not the sorption person. It's not the zeolatage unit which is primarily a very low matrix permeability, so it's primarily fracture flow. We have a relatively simply pipe model for the unsaturated zone and for the vitric unit, we would be using essentially all fracture flow which is also assumed to be unretarded whereas the Calico Hills which is primarily porous flow, it's -- there is the retardation there, but I don't know if --

DR. MORGENSTEIN: Retardation is not a function then of sorption. Is that what you're saying?

MR. McCARTIN: No, it is.

DR. MORGENSTEIN: It is.

MR. McCARTIN: It is. But not in the zealitic unit. This is the vitric unit. The zealitic unit is principally fracture -- well, in our model it essentially -- the matrix permeability is so low it

always is fracture flow and where we don't assume retardation in fractures. The Calico Hills vitric unit has a very high matrix permeability, so the waters going through the unsaturated rock and so there's a lot of surface area and we do account for that but --

DR. MORGENSTEIN: So this is by diffusion?

MR. McCARTIN: No, no, it's sorption.

DR. MORGENSTEIN: Sorption.

MR. McCARTIN: On to rock surfaces, right.

DR. MORGENSTEIN: On to glass surface or other minerals?

MR. McCARTIN: Yes.

DR. MORGENSTEIN: Okay.

DR. BULLEN: Tim, this is Dan Bullen, before you leave this, the 50 percent footprint is for the SR design footprint, not the 5-lobed design where you're going east of the Ghost Dance?

MR. McCARTIN: Correct. And this is probably a little bit dated also in about I'll see two or two three years ago is when we updated the stratigraphy below Yucca Mountain for our analyses and it's approximately 50 percent.

DR. BULLEN: Okay.

MR. McCARTIN: But it's not the new

footprint.

DR. BULLEN: Thank you.

MR. McCARTIN: And as I explained, our unsaturated zone model is relatively simple. We assume vertical flow downward, so where there isn't the Calico Hills vitric unit, we generally see fracture flow exclusively to the water table, so that is an effect there that where there isn't the Calico Hills vitric unit, it's almost -- it's a very, very short travel time to the saturated zone. So this type of performance we see for about -- affecting approximately 50 percent of the repository. I used an average thickness of 30 meters. That's about what we have. I mean we have -- as Chris explained yesterday, we have 10 different subareas and we represent each one of them separately, primarily because of stratigraphy and temperature. Both are considered, but I did not, for this analysis, I did not. I just used a single unit. Not an all temp.

Next slide.

(Slide change.)

MR. McCARTIN: When we look at that, not surprising in terms of the transport time, variety of Technetium, 450 years. No difference between low and high. It's assumed to be unretarded for both. But

you can quickly see that Neptunium, it varies from 9,000 to 60,000. For Americium and Plutonium, I did the simulation for 100,000 years. It never got out. And so you can see in our model, for where there is Calico Hills vitric and 30 meters of it, for these particular radionuclides, significant retardation.

Now in addition to the retardation, remember one thing that compliments the delay is a short half life, that it can -- I mean you don't have to delay too long and it disappears, it's gone. So whereas these certainly are longer lived, but even the Neptunium is quite substantial.

Next slide.

(Slide change.)

MR. McCARTIN: In terms of the saturated zone, you'll see we have some of the same characteristics for the retardation. In this case we -- this is retardation principally in the alluvium. We're assuming fracture flow in the welded tuff units.

So another aspect is how, where is that point between where the water goes from the welded tough units to the alluvium? I considered a distance of -- the stretch of the alluvium along the transport path from 1 to 5 kilometers.

In the unsaturated zone, we did not take

account for matrix diffusion. In the saturated zone we do account for matrix diffusion. It's a much longer transport path relatively slow velocities.

One of the parameters we sampled is an "effective" fraction of the matrix. We are not assuming the nuclides can diffuse into all the rock pores. And so there's a fraction that varies from 1 percent of the rock is available for diffusion to 10 percent, .01 to 1.

So that's the variation there and next slide.

(Slide change.)

DR. MORGENSTEIN: Could you go through again on the other one, what's driving retardation? Is it a combination then of matrix diffusion plus sorption?

MR. McCARTIN: This retardation is sorption in the alluvium.

DR. MORGENSTEIN: And is it mineralogically controlled? What's driving it? What minerals are driving sorption?

MR. McCARTIN: In terms of -- I'd have to ask one of the --

DR. MORGENSTEIN: In other words, what are the assumptions?

MR. McCARTIN: Is someone at the Center, is it Dave Turner or someone that can speak to the retardations we have?

MR. TURNER: My name is David Turner in San Antonio and where the sorption coefficients came from for TPA code, the version Tim is talking about is it has been calibrated a sorption model, particularly surface compensation model to sorption on to aluminosilicate and then we ran it over the range in water chemistries that are absorbed in the saturated zone in the vicinity of Yucca Mountain. That set up the probability distribution function that are pulled into the function part, transport.

They're driven by the sorption coefficient particularly for Americium can be very high. They're calibrated using site specific water chemistry at the site.

DR. MORGENSTEIN: The aluminum silicates are dominantly feldspars and/or clays?

MR. TURNER: For uranium they are clays. They're based on clay. It's also generated down here in San Antonio for Plutonium and Americium. They are based on data from the literature with sorption on to I believe it's an aluminosilicate.

DR. MORGENSTEIN: A final question, so

this is based on the concentration of clays, essentially and aluminumoxy compounds?

What site information do we have on concentrations?

MR. TURNER: Well, okay, in doing this, the basis is it's scaled to surface area and we're using surface area estimates that are from the TPA code. So it's a little bit -- we're consistent in that respect. We're trying to use the same surface areas and porosities that are being used in the TPA code to scale our sorption information to produce the retardation factor.

DR. MORGENSTEIN: So we only have site specific information, is that it?

MR. TURNER: Mineral concentration along the flow path.

MR. BERTETTI: This is Paul Bertetti from the Center. We don't have that site specific information in this version of the model, but we now have quantitative x-ray diffraction data from bore holds, Nye County bore holes in the alluvium and we're incorporating that into the next phase of the modeling effort.

DR. MORGENSTEIN: Do you want to guess on how close or how different you might be?

MR. BERTETTI: No, not until --

(Laughter.)

MR. McCARTIN: Next slide.

(Slide change.)

MR. McCARTIN: And as you can see, the results are somewhat similar to what we saw for the unsaturated zone that the Iodine Technetium have the shortest travel times and then with the others quite a bit longer. You see Neptunium, a fairly large range between the highest and the lowest retardations.

One interesting thing here that these two columns are high and low for a one kilometer stretch of alluvium, these two columns are a 5 kilometer stretch of alluvium. And I was surprised there wasn't a larger difference and it was -- it's always interesting to see some results that you didn't really expect, but there just isn't that much difference. And certainly for some radionuclides, you can see because of the high sorption that is being used, a little bit of retardation for Americium and Plutonium go quite a long way.

Next slide.

(Slide change.)

MR. McCARTIN: I wanted to look at matrix diffusion, a similar kind of result. As I said, there

was that effective fraction of the matrix diffusion and I don't know if I would have guessed this before, but it somewhat doubled between just that 1 percent and 10 percent. It somewhat doubled the time it took that initial release to get out for both the Iodine and Technetium. You see can a little more effective for Neptunium. That's also because once it diffuses into the matrix, there is some sorption that can go on and likewise, once again because of the sorption and the long half life, there was certainly larger numbers there.

Next slide.

(Slide change.)

MR. McCARTIN: In summary, having gone through that, what do I come away with that? In terms of water flow into the waste package, certainly the solubility limit and water flow were important for Neptunium. Also, a large number of waste package failures, breaches, must occur for Iodine and Technetium, partly because it's a rather limited inventory of those radionuclides.

In terms of the waste form, the degradation rate seemed to be important for all the radionuclides.

Interestingly, at least over the time

period I looked at, the 5,000 to a 1,000 years, there seemed to be a relatively limited sensitivity to the temperature. Now that's assuming the lowest waste package breach I considered was a 1,000 years.

Next slide.

(Slide change.)

MR. McCARTIN: The unsaturated zone, the Calico Hills vitric unit, certainly was important for sorbed radionuclides like Neptunium and it also added significant delay times for both the Americium and Plutonium.

For the saturated zone, Neptunium was rather sensitive to the variation of retardation. There was some sensitivity, but limited sensitivity to matrix diffusion and surprisingly, there was limited sensitivity to the extent of the alluvium.

Next slide.

(Slide change.)

MR. McCARTIN: The question is what do we do with this information? And the Committee, I'll say many people in the audience may not know the long standing -- I'll say frustration with the Committee as I'll term it, that we have not used our risk insights. And I think that's two-fold. It gets to, I think, we haven't been able to transparently convey to the

Committee what's going on in the system and why. And as you can see, these analyses are an attempt to try to peel back the shroud of mystery that ends up as a dose curve at the end and that's -- this is the first step. We're going to continue this. You can see there -- we hope to prioritize some of our work in relationship to some of the things that we saw. We also -- there's two other parts that we need to get to is then with these things that seem to matter, we need to then go back to the data and information supporting it, do we believe that representation? Now that we know these are the -- these particular aspects are the most significant, look at that information. We also intend to -- we've done this with the TPA code. We are very familiar with the TPA code. We can do a lot of different analyses with it. Clearly, we have to do this. It's not what the TPA code has in it, it's what's in DOE's GoldSim model and we need to look at the assumptions and use the GoldSim model to do some of these same analyses to help us understand what's going on and why there. In addition, to obviously making benefit of the calculations that Peter has presented. But and in fact, some of these analyses, I'll say started out oh, six months to a year ago. Bill Ford and Hans Arlt at the NRC were looking at

GoldSim and they were looking at some of the output files that GoldSim produced. And they were trying to understand things by some of these output files that are very similar to some of the output -- some of the results that I used. I said well, that makes a lot of sense. We can try to use that. We need to go back and start to look at the DOE model, so you'll -- as I've promised the Committee, we will be looking -- we need to transition from looking at our results to what does this mean in terms of the DOE results.

As we go down this path, flexibility in the selection of an analyses, as you saw, I did different things, different ways. I looked at different performance measures. I think this is consistent with the Committee has recommended different pinch point. I'd like to think this is -- it isn't a simplified analysis in the sense that I'm still using the TPA code. But it's simplified in that I'm pulling out and getting into a bite size piece of the TPA code that people can look at and go away with well, do I agree that the retardation for Neptunium is going to be delay things from 90 years to 70,000 years.

An expert can go back and look at analyses and determine that. Likewise, release rates. There's

something there that someone can say why should I believe that?

I guess we're going to continue. I assume we'll be back here not necessarily for this panel, but for the ACNW. It is a work in progress. We're continuing and I'd be happy to hear any questions, comments and --

DR. GARRICK: Thanks, Tim. We appreciate your abstractions of the abstractive model.

(Laughter.)

I think this is very valuable. I know Mike has a question.

DR. RYAN: Tim, I concur with John. This is very insightful work and helpful and stimulation of the thinking process, it's great.

Maybe I could turn your attention to Slide 4.

I want to kind of focus in on the dose conversion factor part. I know in your analysis that's been kind of a fixed parameter and that 15 millirem dose has been fixed. There's a couple of aspects I'd like to just take a minute and talk about and then get your reaction to maybe the same kind of systematic exploration that needs to be done there.

Dose conversation factors are used, I

guess from ICRP in the analysis. I caution everybody to recall, those are dose conversion factors that were designed for worker protection. Plutonium, for example, it would surprise you to take a look at all of the reported GI tract uptake fractions which drives the factor. They range over several orders of magnitude and the 95 percentile of the distribution is what was used to set that dose conversion factor.

The tendency of these factors is not to be central tendency, it's to be conservative tendency because they were designed for worker protection. So that's something that would be interesting to explore because in many cases they can be magnitude influences on doses.

Now perhaps for Technetium and Iodine, not, because they're soluble and mobile in the body and so on. So I think there's a fruitful area to explore with the fundamental dose conversation factors.

I've looked carefully at Plutonium and that's one that's very surprising that it was set to be quite thoroughly conservative for the purpose of worker protection. So we're using them for a different purpose now.

The second is the pathway dose conversion

factors which include intake rates, food consumption rates, all those kinds of things. For example, water intake is two liters per day. How many in the room drink all their water out of one tap all day long every day?

Well, there's probably a conservatism in that assumption, so I think with the same kind of exploration that you've done for these other issues of waste release and other things, it would be as important and very helpful to understand what the biosphere component offers in terms of either conservatism or perhaps nonconservatism or the same kind of exploration.

I guess that's my comment, and I'd appreciate your reaction to that.

MR. McCARTIN: Well, certain aspects of the extrapolation from a concentration of curies to dose could warrant looking at. I mean there are certain parts that my understanding that we would expect the Department to use whatever federal guidance is current at the time of the license application and EPA does the federal guidance for what methodology should be used to calculate doses and we would just use that.

DR. RYAN: Let me just react to that one

point. I agree with you. You use what's required. However, exploring what that means doesn't mean you don't use it.

MR. McCARTIN: Okay. Sure. Good point. Yes. I would agree.

Now one ameliorating factor is things like Americium and Plutonium in terms of if you saw the retardations for the alluvium, even at the lowest value, they never got out and so there's certain things that once again, we want to make sure and focus on the ones that -- now certainly Neptunium, it's pushed a little bit beyond, but as you saw, the variation is relatively significant between the solubility limits, release rates, water influx and retardation. That's certainly a nuclide that I think we want to --

DR. RYAN: And as you see, it's got the largest dose conversion factor, so that's an exploration that might be interesting.

MR. McCARTIN: Yes, yes, I would agree.

DR. GARRICK: Go ahead, Rod.

DR. EWING: Just to follow up on that, as someone who is not very familiar with dose conversation factors and I just know enough to be confused, it would be very helpful and what's missing

and I like your approach is a comparison between the DOE, the TSPA and the TPA and what would help me is before you get to the biosphere, show the number curies at a certain part and place in the analysis for say Technetium and Iodine and Technetium and Iodine are very interesting because they're not retarded. So these are real tracers that can bring out the differences between the models that you see. So I was sitting here struggling with dose conversion factors. The DOE, TSPA, Peter has given us curves for one package failure. You've said we need more than the inventory to fail for Technetium. At a 1,000 years, it would be very interesting to see how close your estimates actually are in terms of the total number of curies released and there will be differences, of course. That's not to say either is wrong, but in those differences, I think, is a lot of value, if we understand the reason for the difference.

But once you go to the dose conversion factors, then I lose control over my ability to think about what's gone on in the repository waste form.

DR. BULLEN: Bullen, Technical Review Board. Along those same lines, I actually did the same type of conversion as my esteemed colleague from the University of Michigan and if you take a look at

Peter Swift's figure this morning and you look at the 5,000 year dose for Iodine-129 -- actually, it's Technetium-99, I'm sorry. So Tech-99 dose is on the order of maybe 3 or 4 times 10^{-5} and you say okay, I've got about 10^{4} packages. I got up 10^{4} and say well, do I meet the regulatory limit or do I exceed it and I'm off by about an order of magnitude, if you just take a look at it.

Now the question I have for Tim is that well, is the order of magnitude close enough for the kinds of calculations that you're doing or do we need to understand more fully the differences between the codes and try to explain why that order of magnitude is there? But right now, you're within an order of magnitude and as a performance assessment modeler from way back, I look at that and say well, that's the same answer, but maybe you don't feel that way.

MR. McCARTIN: Well, I guess there's a couple of comments with respect to that and both points and there's certainly comparison between ourselves and DOE. We aren't holding ourselves out as gee, we have it right. We are doing analyses to assist our thinking and ultimately it's what is the DOE model, what are the characteristics that are incorporated in their PA and have they supported those

characteristics?

Now the part -- the differences is well, that's useful in trying to understand better what DOE is doing. I guess I'm not -- the fact that we compare, as I mentioned yesterday, there was a -- I'll say four or five years ago, somewhere in that range we looked at release rates and we were pretty close, I'd say.

We had a lower release rate and no cladding credit and DOE had a very high release rate and a lot of cladding credit. And it's useful to know that, but our role is has DOE supported the basis for their cladding credit.

We need to know that the cladding credit has a significant impact before we can review it and in that context, the understanding both performance assessments are useful, but it's really a took for us to probe DOE's assumptions and we have never and I probably should have had a caveat and maybe we've gotten a little lazy over the years, but when we've had technical exchanges with the Department on performance assessment, we have always said that there are no parameters or approaches in our TPA code that represent regulatory acceptance.

DR. GARRICK: As --

MR. McCARTIN: The Department can't say NRC did it that way, well, here it is, feed it right back to us.

They have to have their own basis and it has to stand on its own. We're doing this as an understanding process.

DR. GARRICK: Excuse me. My opinion on this order of magnitude business, if we were 100 percent confident that our results were within an order of magnitude I would be very, very happy.

(Laughter.)

Rod, you had a question.

DR. EWING: Just a comment. I understand the regulatory -- well, I don't understand the regulatory framework. I have an impression of the constraints. But still, you know, in any other scientific or engineering field where you've got two models, people immediately compare them and it's not to -- sometimes it's to tear one another part, but besides that it's very instructive to see what the cause of the difference is and of course, the fact that they match doesn't mean that either model is correct, but it's a very revealing and useful exercise that in other waste management communities around the world, I mentioned this blind predictive modeling,

people do routinely.

And so --

DR. GARRICK: Yes, this Committee agrees with you and we've been pushing for this very hard.

MR. McCARTIN: Right, and my only point is there's no question that the ability of the NRC staff to comment and review during this pre-licensing phase and when we get the license application is completely enhanced by the performance assessment work we've done to develop our own independent model.

It's the understanding, and I think that's your point, the understanding that that brings is the important aspect, not necessarily whether there's a direct comparison.

DR. GARRICK: Yes, Joe?

DR. PAYER: Joe Payer. I, too, really support this kind of effort. I know that there's the issue of remaining independent and so forth, but also understand that NRC and DOE have been able to identify key technical issues and other aspects, that sort of thing. It seems to me -- and I also know you're both working to try to make these complex models, at least let people know what's in them and how they work and so forth.

It seems to me it would be a great step

forward if you could agree upon the sorts of lists that you have package, waste form, those types of things and so these exploratory and explanatory types of treatments could at least be in the same boxes, you know, so how are each of you treating penetrations and the waste package, whatever type of thing.

For one thing, I guess to follow up on that is the question, when you try to compare what DOE is doing compared to your analysis, looking at your TPA, how hard is that to do? I'm sure all the information is there somewhere, but does it take a major amount of effort to repackage it and put it together or is it pretty straight forward.

MR. McCARTIN: In theory, it's straight forward. In application, it can be a little more difficult and it's just -- it's going to take a little bit of time on our part. We have approximately a year or a year and a half ago, we got the GoldSim model in house and we have it up on people's computers. To be able to go in, obviously, with our code I can go in and pull out output and do runs, very flexible. With the DOE model, it is someone else's model, so we're coming up to speed. There are some people, Dave Esh, on staff, who is very proficient in it, but others are coming up to speed and that I think is one of our main

goals.

The one problem we have is that the TPA code is a very, by comparison is a very svelte model. It runs very quick. We designed it, when we started many years ago to develop it, we wanted a code that we could do around 400 realizations over night and so we put a very strict run time on each of the modules on the order of I'll say 30 seconds. I forget exactly, for each module.

So we have something that we essentially can run over night. We run it over night easily on a PC now without any trouble. Sometimes in a couple of hours.

The DOE model is much larger and right now we don't have any computers in-house that can actually run the code. We can get DOE to give us the results, and that's what we're looking at now. We have the results and we can do it, but it's not quite as easy.

DR. PAYER: I'm not asking do codes mesh up. I'm just asking that it appears to me that both organizations are trying to make, strive for transparency and explain these to different groups of stakeholders. And if you could just agree on the categories in which you're going to explain that, you know, for example, if you would have followed the list

here, we would have had to close the -- but for reasons, you do your own things. And everybody does that, but then it makes the third party, it's hard to -- there's overlap between the boxes and I just think it would be a step forward if we could agree on it and start explaining things in the same bundle of products.

MR. McCARTIN: Good point.

DR. GARRICK: George Hornberger.

CHAIRMAN HORNBERGER: Just to change gears here, Tim, I have a very specific question, so looking at your table for saturated zone, retardation sensitivity, and you looked at a 1 kilometer pathway and a 5 kilometer pathway and Neptunium-237, you had 950 and for the 1 kilometer path and you for the -- it's Slide 18, if you want to pull it up, Michelle.

And 1050 for the 5 kilometer pathway. So tell me why the 1 kilometer pathway and the 5 kilometer pathway are not very different at all? Just for the low retardation.

MR. McCARTIN: For the low retardation. What you're seeing, I mean there's two competing things going on there. One is the fact that the alluvium path tends to be slower than the fracture path.

Now they're essentially unretarded at the low end, the Neptunium is unretarded. But for matrix -- that's in the alluvium. For a matrix diffusion, you have a -- you actually do have a retardation when it goes into the fracture rock there is a retardation and so what you're seeing is the fact that the reason it isn't more in my mind is that the alluvium slows it down somewhat but your fracture path is father in the fractures and you actually are getting some slow down due to the retardation and matrix diffusion in the fractured path. So it's not as much as you think.

DR. GARRICK: Okay, thanks, Tim. Thank you very much.

We will now take a 15 minute break.

(Whereupon, the proceedings in the above-entitled matter went off the record from 10:41 a.m. to 10:56 a.m.)

CHAIRMAN HORNBERGER: Before we recommence with our working group, we are privileged to have with us the leadership from NMSS and I will introduce Marty Virgilio, who wants to take care of a little human aspect of the ACNW. Marty.

MR. VIRGILIO: Thank you, George. I just want to take a minute to recognize Ray Wymer's retirement, and the excellent service that he's

provided to the Agency through his six years service on the ACNW. And this is an occasion for us, and we really wanted to thank you, Ray. I had signed out a letter that I want to present to you. I signed it at the beginning of March, but the Staff suggested that I hold off to an opportunity where we could get together and say a few words, so this is really on behalf of the Staff. I'd just like to read a few things from the letter before you stand up and grab it out of my hand. You're not getting out of this that easy.

This is on behalf of the Office of Nuclear Material Safety and Safeguards, but really I think it's on behalf of the Agency when I think about your contributions to the NRC. We're commending you for your six years of service on the Advisory Committee, recognizing your knowledge, insights and contributions in the area of radiochemistry and materials technology, have greatly assisted the Agency and NMSS in the work efforts that we've done. Your retirement during your second term is a loss to the Agency. It's not easily regained, and I just want to acknowledge that and thank you for everything you've done for us.

There's a lot in this letter, but I want to cut to the chase as your style of interaction was

always polite and in good spirits, constructive, and often accompanied by realistic examples served with your Tennessee-honed humor. We appreciate your professionalism in dealing with the Staff always, taking time to get to know them, and never had a disparaging word for anybody. We're gratified to hear that you're going to continue to serve nationally on some of the prominent committees that we still interact with. And, thus, we believe we'll continue to benefit from your experience and your insights. Thanks, Ray. We appreciate it. Thanks, George.

DR. WYMER: I'm not going to make a speech.

CHAIRMAN HORNBERGER: If we had time, we would demand that Ray make a speech, but we do have to get back to our working group, so I will turn the floor over again to John Garrick.

DR. GARRICK: Thanks, George, and thank you, Ray.

Okay. One of the important parts of our working group session was to try to get as many expert views on the issues that we've identified that we want to consider as possible, and we've very pleased to have a very strong contingency from the State of Nevada. And we're now going to hear a series of

presentations from several people, starting with Don Shettel. And I would appreciate it if you would give a little bit of a background on who you are, and your affiliations, et cetera.

DR. SHETTEL: I'm the designated speaker today for the State of Nevada. My name is Don Shettel. I'm a consultant with the state working with Geosciences Management Institute in Boulder City, Nevada, and my primary contribution to this talk is the near-field environment, and the rest of our team that's listed up here has to do with corrosion; specifically, Drs. Barkatt and Pulvirenti with Catholic University, Drs. Gorman and Marks with Dominion Engineering, and you all know Roger. But this group has been instrumental in planning, executing experiments in corrosion and general brainstorming the issues of corrosion.

This is a schematic of Yucca Mountain. I'm only going to worry about the portion of the Mountain that's at or above the repository level, and because in order to get these waters up to the repository level, we have to invoke some discredited theories that were mentioned yesterday. We have precipitation, and what doesn't show here is number two, is the fracture flow water, matrix water. This

diagram shows waters and processes that occur throughout the Mountain. And we have a refluxing zone here, which is high temperature refluxing of vadose water mixtures and interaction with the rock at higher temperatures, whether caused by the emplacement of the waste.

What I'm going to concentrate on today are primarily indirect processes in the next diagram. First, I go over the water types that are above the repository level in general. First, we have precipitation as its water composition, fracture flow water. There's not a lot of samples, and there's some question as to whether these really are fracture flow waters. The main thing we're going to work with here are matrix or pore waters in the Vadose Zone, and I have found that there are two types. There are some diagrams in the backup slides that will convince you that there are two types of water here. There's a shallow flow water that's above the repository level that has a Calcium Sulphate Fluoride composition, and it also has significantly more Magnesium and Nitrate than the deep flow waters that are below the repository level, which are essentially similar to ground water and perched water; in other words, a Sodium Bicarbonate.

Yesterday, there was a statement by DOE people that these two waters were essentially very similar, but it doesn't look like that on the slide. Plus, the main point of this is when you boil and evaporate these waters, the Calcium Sulfate Chloride water, late stage of evaporated residuals go acidic, and you do that for the Sodium Carbonate waters they go alkaline, so these waters are not as similar as some people would have you believe. I already discussed refluxing to some extent. Next slide, please.

The indirect processes, the main primary way the water is going to contact the waste package is by dripping or intermittent flowing water from fractures. Now the DOE would have you believe that once the rocks get above the boiling point, which I believe they consider the boiling point for pure water, which is 96 degrees C, the rocks dry out and you get no water flowing through fractures onto the canisters. However, once you start to boil water and concentrate it, you have what's called a boiling point elevation so the temperature of the residual solutions can go up.

And the other point here is that there have been calculations by Karsten Pruess at Lawrence

Berkeley Labs and some experiments, the Hele-Shaw experiments conducted by Dr. Houston at the Center, that show that liquid water can penetrate if it's above a -- in a fracture it's above a hot zone of rock, essentially above boiling. The water can finger down through the boiling zone in the rock, and essentially can penetrate the rock even to above boiling and reach the canister, so just because the rock is above boiling doesn't mean that water can't get through, or an acarus solution I should say, cannot get through in the fractures to reach the emplacement.

Most of these events here are processes you're familiar with. The ones we're interested in are corrosion, but we have some other processes here that are a result of evaporating waters, have acid volatilization, and hydrolysis of salts. Next slide, please.

Acid volatilization, when we evaporate these solutions, when they get fully concentrated, these acids, Nitric, Hydrochloric and Hydrofluoric are driven off in the vapor from thermo evaporated solutions. Sulfuric Acid or Sulfate is volatile, will concentrate in the residual solution, and eventually precipitates the Sulfates in the solution. Therefore,

these residual solutions lose whatever beneficial inhibitors they might have, and essentially this would invalidate the DOE corrosion model of the clad versus Sulfate Nitrate ratio. And as I said before, these residual solutions become, and in their common states, as well, become acidic with thermo evaporative concentration. Next slide, please.

The hydrolysis of salts is intimately connected with the previous slide. The salts that form from this thermo evaporation of the dripping vadose water obviously precipitate various salts, a couple of the minerals I've listed here, but there are many compounds that are not minerals, such as Calcium, any number of hydrates of Calcium and Magnesium that form here, and these are -- one of the key ones that we found is Tachyhydrite, which is a mixed Calcium Magnesium Chloride Hydrate, and these deliquescing salts cause accumulation of liquid on the canisters. The salts are hygroscopic. They absorb moisture from the drip or from the drift, and if they dry out in-between drips, whenever a drip comes back down onto the salts, they hydrolyze, as well. And during this process they can form very acidic solutions. Brines are also highly viscous and have low vapor pressure, so they're not necessarily going to run off the top of

the canister if they happen to fall right on top of the canister. And if these salts happen to dry out, one observation in the lab, if we completely dry out these salts and then let them sit around at room temperature - although it doesn't have to be room temperature - let them sit around and absorb moisture from the atmosphere, they can, in many cases, give off Nitric Acid vapor, which is an interesting result.

Next slide, please. Okay. There is a table of corrosion results in the backup slides which I'm not going to cover in detail unless we want to get into that, but I just want to show you a couple of results here from the experiments at Catholic University. This is C-22 disk. I believe this is about a centimeter across in a wet residual paste at 140 degrees C. This was the temperature that the solution was boiling at, so you can see there's going to be quite a difference between 96 degrees, which is the boiling point at the Mountain, at altitude, and what these salts can concentrate to. The 29-day initial solution was a concentrate pore water. The PH of this paste near the end was 2.2, and we got a general corrosion rate based on weight loss of almost 700 microns per year, which converts to almost 30 years for a hole to develop in a two centimeter

thickness canister.

Now this raises another question. And if we can penetrate the canister in such a short time period, and we believe we can, what happens when these salts and everything get inside the canister? Obviously, you don't have the bathtub model any more. You have hydrolysis of salts and acid volatilization going on inside the canister. We haven't begun to explore that one yet.

Now in our experiments, we use a Soxhlet Distillation apparatus which has a cup where the condensate can run from the boiling solutions, can run back in, and they put a piece of metal up there. And the temperature of the Soxhlet is 77 degrees C, and we get -- you can see a very high corrosion rate. This is an SEM photograph of that. The PH is very low, -.5, and again this translates into almost a millimeter per year, which converts to almost about 21 years to penetrate two centimeter thickness. Next.

A schematic of what might happen in the drift. Some of my labels do not work on the Microsoft, but this is a dripping fracture up here. We might form a salt stalactite here with dripping water. This could break-off periodically, and we can also form salts on top of the drip shield. Eventually

the drip shield is penetrated, and these get under the canister. And I might point out there is also a slide in the backups that show that the effect of these evaporating solutions is not limited to C-22. It also has a very similar effect on Titanium-7.

And the conclusion is next. This fracture and pore water occur at and above the repository level, of course. We have no ground water compositions. Indirect processes are much more complicated than has thus far been admitted by anybody. Corrosion rates are significantly higher for thermally evaporating solutions and their condensates. The range we found thus far is .1 to 1 millimeter per year, and one experiment has been up to 10 millimeters per year, which translates to two years to penetrate the 2 centimeter thickness of the canister.

And towards the bottom here we have sub-boiling, immersion testing of EBS materials and ground water is both unrealistic and non-conservative. That refers to long-term corrosion test facility, which is most of the basis for DOE's model of corrosion and essentially, the repository is supposed to be in the Vadose Zone, but this testing is really putting it down into the Saturated Zone, and we see that that is a major error in logic.

And then this also raises a more fundamental question, is the current repository design a good one for the Vadose Zone, and we don't believe it is, at Yucca Mountain, I should point out. Question?

DR. GARRICK: Go ahead, Maury.

DR. MOREGENSTEIN: Could you describe what's driving the PH?

DR. SHETTEL: What's driving the PH is the formation of solids in evaporating solutions, which are primarily Magnesium Hydrates, and other Magnesium compounds. One of them is a Magnesium Nitrate. These form fairly early before the solution is completely dry, and then when they rehydrolyze, they generate acid on hydrolysis.

DR. GARRICK: Any other comments, questions? Okay. Go ahead, Joe.

DR. PAYER: Just, I guess one comment. The -- you've shown that it's possible to start with, you know, mixtures of ions and waters that are available here. And if you treat them boiling them down, refluxing, things of that sort --

DR. SHETTEL: We're not just starting with any composition of ions. We're starting with ones that are appropriate --

DR. PAYER: I understand.

DR. SHETTEL: -- at and above the repository level. Okay.

DR. PAYER: Yeah. Starting with ions that are present there and treating them, what I haven't seen yet, I don't say it can't exist, but how do those environments get generated on a metal waste package surface? Do you envision a small Soxhlet- type process?

DR. SHETTEL: No, just by the solution that's dripping onto the canister and being evaporated and concentrated on a hot metal surface.

DR. PAYER: I understand, but how do they get refluxed?

DR. SHETTEL: Well, the refluxing was up in the rock. That's a different matter.

DR. PAYER: The highly acidic brines are up in the rock. That's where they form, and then they drip onto the waste package?

DR. SHETTEL: That's a possibility, but the loss would probably buffer the pH to limit that.

DR. PAYER: I mean, I guess -- yeah, I've heard these presentations in many different presentations. The part that's missing in my mind - I don't say it doesn't exist, or where it is, or where

the boundaries are - but the description of how these environments form on a waste package-type or a drip shield, either on the top, or the bottom, or wherever.

DR. SHETTEL: Well, the reflux --

DR. PAYER: How they would -- sorry. Just how they form, would they persist, how much of it is there, if they go away would they reform? I mean, that I think becomes the real issue. There's no question that you can generate environments in a lab that will, you know, make C-22 and Alloy Titanium corrode very rapidly. And that's been demonstrated.

DR. SHETTEL: Right. Well, I think these solutions can concentrate in the refluxing zone above the rock, I mean above the drift in the rock, and then the concentrate -- the essentially pre-concentrated solutions to some extent then can penetrate the fractures and drip onto the canisters where it can reach that final evaporation approaching near dryness or even complete dryness.

DR. PAYER: It's that whole bloop there I guess that is not clear in my mind. The part that I don't envision is how the condensation occurs, to keep the acid vapors that are generated at that location on the metal surface, because it's an ambient pressure is my picture of the --

DR. SHETTEL: So is our experiments. But anyway, the vapor is low acidic we've just discovered. I mean, we found that they were acidic, but the residual solutions that would reside on top of drip shields and then on top of the canister, those aren't dependent on the -- they form --

DR. PAYER: Well, maybe -- but you've got a recondensing to keep bringing them back. That's the part that -- you've got acid vapors. It seems to me you've got an open system where acid vapors could go wherever acid vapors are going to go, but they don't have to come back into --

DR. SHETTEL: That's right.

DR. PAYER: And be captured in the solution.

DR. SHETTEL: That's right. They don't have to.

DR. PAYER: And that there are processes that --

DR. SHETTEL: Somewhere else in the drift, but you have to remember, you can still keep dripping water down onto the canister and build up the salt deposits, and add moisture to that.

DR. MORGENSTEIN: Joe, let me interject for just a second to help this out. If you just take

a fracture drip onto say Titanium drip shield - okay - the precipitate that you would get from the evaporation of that drip will have Tachyhydrite in it period. Don't go any further. You don't need recycling.

DR. SHETTEL: I thought that's what I said, but --

DR. GARRICK: Okay. Very good. Go ahead, Dan.

DR. BULLEN: Dan Bullen. One more quick question. I'm just looking at the residual paste and how you got to it. And I want to try to understand. You started with 12 liters of 1243X UZ pore water. Right?

DR. SHETTEL: Right.

DR. BULLEN: And so basically, I'm just trying to do the mass balance in my head to figure out how much you need. So if I wanted to get to this level, I'd have to start with about 15,000 liters of water, and then how long would it take me to get 15,000 liters of water concentrated down to this level. I looked at your calculations and your backup slides, basically. I cheated. I'm looking at them.

DR. SHETTEL: Well, there is one slide back there, how dry is --

DR. BULLEN: Right. But I'm looking at that, and I'm looking at average percolation flux in the one to ten thousand year range that are, you know, sort of 2-20 millimeters per years, and so I picked 10. And if I have 15,000 liters that I need to make, and so I divided by 10 milliliters or 10 liters per year per cubic meter, per square meter, I'm sorry, it still takes me about 1,500 years to get this concentration? I mean, I'm just trying to do the math to figure out.

DR. SHETTEL: Well, that's on average. You have some canisters that will have more dripping on them, and others that will have less or none, so you're speaking about an average time.

DR. BULLEN: Right. But then I -- and that average time --

DR. SHETTEL: It can be concentrated down on one out of every, I don't know, three, four, five canisters, whatever it is.

DR. BULLEN: Okay. Keeping that in mind, that's fine. But it doesn't stay hot for that long. I mean, I'm above boiling for whatever it is.

DR. SHETTEL: Two years. I mean, you only need -- some of the solutions only take two years to penetrate the canister.

DR. BULLEN: But how long does it take me to get enough water there to make the solution, is the question.

DR. SHETTEL: Well, you're not taking into account that you're going to heat up a certain volume of rock above the drift, which is --

DR. BULLEN: And mobilize the water, I understand that.

DR. SHETTEL: Mobilize the vadose water, and pore water. Plus, you have the percolation water coming down.

DR. BULLEN: Okay. Well, I'm just trying to get a handle for it. And thank you for providing this "How Dry is Dry", because I wanted these numbers, and you had them, but thank you.

DR. SHETTEL: I'm not sure that that's our calculation to make. I mean, that's --

DR. BULLEN: No, I didn't say it was. Just thank you in your presentation.

DR. SHETTEL: It's something that needs to be done. It's not necessarily something that is our job.

DR. GARRICK: What I'd like to do is to make sure that every speaker has opportunity to make their presentation. And if we have time at the end,

we can come back and ask questions of anybody, so we will with that try to move right along.

And also I'd like to comment, I have very impressive CVs on each of these speakers, but rather than take the time to read them, I'm going to ask that they be made part of the record so they will be part of the permanent proceedings. And continue the adopted practice of having the speaker introduce themselves. Our next speaker is John Walton from the University of Texas, El Paso. And he's representing Nye County.

DR. WALTON: That's correct. I'm a Professor of Civil Engineering at the University of Texas at El Paso. And Drew Hall, who did the work, is my Master's student.

DR. GARRICK: That's impressive that a professor would make that kind of admission.

DR. WALTON: Well, I'm prepared to take credit for anything good, and blame him for any problems you may have. Next slide.

Water chemistry is clearly important for corrosion model EBS materials. Everyone agrees on that. We need to consider all micro chemical biological processes that might determine that water chemistry, and we get to look at these other things,

but things that are likely we'd look at. To my knowledge those were looked at, physical separation processes in the repository, the subject of a Master's thesis. Next slide.

Evaporation occurs in the repository, and evaporation usually occurs when water moves. That is, water doesn't stay put. If you remove water from a part of a lock matrix here, then by capillary suction other water would move towards it, and so the water tends to move as it evaporates. And as it moves and evaporates, it becomes more concentrated. And as it becomes more concentrated, the least soluble minerals will precipitate first, and the more soluble minerals will precipitate later, and perhaps at a different location. And that's the essence of this work.

There are many potential situations where this can occur. I've got a couple of cartoons to show you some examples, and pictures to show what really occurs, but this is very common in arid environments. Next slide.

First cartoon is not intended to be realistic. It's intended to be simple so we could explain what we're talking about. We have a fracture, produce the drip, the drip goes down on the drip shield or water container, could be either one. And

the drip occurs in the center, so we have a wetted area here, and the water flows away in the wetted area, and as it flows it evaporates. And so potentially we have a condense situation, but potentially we have where the least soluble minerals will be precipitated in the middle where the drip occurs right in there, and the most soluble will be precipitated at the edge. And so we could get a physical separation of the original ions in the source water.

Next slide. This is a little bit more complicated cartoon, and perhaps a little bit more realistic. Here we have a dead-end fracture that serves as our source of water, maybe from reflux and condensate or whatever. Water comes down in the matrix here, and it sees the capillary barrier here, starts moving around the drift. That's what we want to see. And as it moves around the drift, however, vaporate diffusion could occur, and there's going to be evaporation, so it's going to concentrate as it moves around the drift.

So potentially as it concentrates, the least soluble minerals will be precipitated first, the most soluble minerals could be precipitated later in a different location, physical separation of the

original source. Here we have a little surface roughness off the ceiling. Don't worry, so that's how it formed. Water's coming down in there by capillary suction. The water can evaporate because there is contact with the drift there because it moves. It becomes more concentrated as it moves out to the end. And these soluble minerals precipitate here, the most soluble minerals down there.

The third example in the cartoon, we have a dead-end fracture here. Here it opens up into the drift, so presumably we have vapor diffusion going on in the fracture. Will have the greatest vapor pressure, highest vapor pressure here, lowest out here. Highest relative humidity, at the bottom -- where's that last one? Highest vapor pressure there, lowest there. Highest relative humidity here, lowest there.

Some of the water comes in here. It's going to enter the fracture wrought by vapor diffusion. Some of the water will stay in the matrix. This is more desiccated portion of matrix than that. Capillary suction moved the water that way while it's evaporating. Least soluble tend to go here. Most soluble minerals in this direction. Next slide.

This is just kind of a blowup of the same

cartoon I had before. We have our fracture with separation of minerals along in our little stalactite or surface roughness. And we have a drift area, or this could be just a place on the ceiling where the saturated hydraulic conductivity is just a little bit higher. It would serve as a source area. Dripping's not required in a physical separation process, in the rock as well as on the canister. Next slide, please.

Now, it's nice to draw some cartoons, but the question always is, does it really occur? And it turns out, this is very common in the desert. Here's just a picture I saw, I walked into subway at lunch, and this is a rock wall in El Paso. We have lots of rock walls. People irrigate their plants up above, here on the picture, seeps down inside the rock wall, leaks out through cracks, down the sidewalk.

So we have the source area right here where water moves out. As it moves, it evaporates, the minerals are deposited and we see banding here. It's evidence of the physical separation processes. Next slide.

Here's a picture from a desert spring, and you see the ground is wet right here. The water rises up, the capillary rise along some rocks there, and we can see some signs of physical separation right along

some of these rocks here at the spring. Next slide.

So, you know, we have the general concept of physical separation, and we see if it occurs in natural systems. And so now the thing to do is develop a model to try to look at what happens, so we developed a simple equilibrium model. It's adequate for at least semi-quantitative analysis. It's not real sophisticated, didn't intend to be.

There's two obvious end-points in the physical separation that we can look at. One is what we call single-cell mixing tank. That is, everything goes into a beaker, at least mathematically, and evaporation occurs right there. And then later in time if the repository wets back up, the rehydration occurs in the beaker, so we call that a single-cell mixing tank, no separation.

The other extreme we can go is that everything is completely separated as it goes, and we call that our infinite series of mixing tanks. And we wrote a model that can do the single-cell, it can do the infinite series, and it can actually do anything in-between, because we really input the number of mixing tanks to use. Reality is like to be intermediate and highly variable. For this simulation we stopped at a concentration factor of 10 to 6, 1 to

a million, just arbitrary but you have to stop the graph somewhere. Next slide.

Interpretation, you can look at some of the water chemistries in there, look at the ratio of aggressive to non- aggressive ions. You know, we can have source areas are caused by drips. We can have separation of rock. Separation of rock is probably more important as you get these things forming on the ceiling, and then later on they fall down as dusts on the canister, so that's when they come into effect. Next slide.

Source waters, we're pretty agnostic about what the source waters are. We have a simple model so we can run it a lot of times. We can run a lot of different source waters. What I'm going to show you today is we have precipitation. It's an obvious one. Pore waters from Paintbrush, pore waters from Topopah Spring. We did a 50/50 mix of precipitation with Paintbrush tuff, the idea you get some matrix diffusion or whatever as the precipitation is coming down through the fracture. You know, what else should we try? You know, Drew has not defended his Master's thesis yet, he could use more work to do, so we're open to suggestions. Next slide.

Here is a graphical presentation of

results. It takes a little explanation here because we couldn't put everything on it. We cut off the labels here because they're fairly self- explanatory. These are mols, so this is fracture of the mols right here. And we're showing anions because they're most interesting. We have two figures here. This is called the single-cell results, and this is the infinite cells results with good separation.

Now what's kind of confusing, if you have a single-cell within the bounds of our simple model, the evaporation sequence, you go up and dilute the concentrated, and when we rehydrate this reverses itself, this repository cools down, so it's pretty straightforward.

Now the infinite series, what happens is during evaporation, the minerals precipitate and they're not longer available, so the evaporation sequence is the same for both of them. But later on with the infinite series, everything is physically separate. The rehydration is completely different, so this is rehydration of the infinite series, and this is evaporation in the infinite series, and both ways on the single- cell. So let's look at some of the results.

We see here for the single-cell, there's

Chloride right there. There's Nitrate. We see about a 10 to 1 or thereabouts at this point of Chloride to Nitrate, so it's getting a little bit aggressive. And that it evolves into a more fable situation. If you look at the infinite cells results, these are physically separated, different locations now, so we're not really specifying the concentration.

We see in some places we have the Bicarb mostly, some Sulfate waters. One point we get a pure Chloride pretty much, called the anions, and out here farther we have a mixture of Chloride and Nitrate. Next slide.

DR. LATANISION: Just a point of information.

DR. WALTON: Yes, sir.

DR. LATANISION: You're characterizing the Nitrate Chloride mix as being aggressive. What do you mean? When it's 100 percent Chloride, you consider that --

DR. WALTON: Well, I'm trying not to be too specific about that and let you judge for yourself. Some people believe that when it gets over about 5 to 1, Chloride would be more aggressive, but I'm really not trying to make a statement there. I'm just -- okay.

Let's just go ahead and skip this slide. There are a lot of ways that we can present the results, and I think that's a less interesting way. We can also show the cations. In this case, the cations, it looks like it's been evolved towards a mag- chloride system if we let it go far enough. Next slide.

Here's the Paintbrush Tuff, the different source water, same sort of calculation. Single-cell mixing tank, we get quite a bit of Nitrate out there, some Chloride, ratio about 10 to 1. On the infinite cells it's a little bit more interesting. We get the physical separation. Out here we get some pretty nice waters we like, and out here we get, it's just 100 percent Chloride for the anion. Next slide.

Here's precipitation, another possibility. We look for the single-cell, and we get lots of Nitrate the whole time here. It looks real nice, like that one. For the infinite cells, we get the Chloride and Nitrate are pretty well mixed out here. And back in here in the less concentrated areas, we get bands of Chloride, so we get quite a bit of Chloride. Next slide.

This is a mixture of precipitation and Paintbrush Tuff. I believe what we did is concentrate

the precipitation a factor of 10 to 100 and then mixed the two together. Otherwise, just based to dilute the source water. Single-cell looks pretty good. Infinite cells, we get some area with some Fluoride, and we get one little band of pure Chloride. Next slide.

Another issue is how long do these processes occur? What's the timing? And I think there's a fair amount of uncertainty on timing which these will occur. Natural breathing of the mountain, I mean this was raised yesterday. It's not clear that natural breathing of the mountain is fully considered the model, so most of the models are designed to be fairly conservative, and most people believe it's conservative if you have more water, greater relative humidity. And construction increases air permeability, even if we seal the drifts, and so there is some question about how long this pure -- Joe Payer showed us the other day where we'll have significant evaporation will last. I think there's a good argument to be made that it'll last much longer in the projections we see.

Also, climate could be drier than anticipated. People don't tend to do down-turns in climates, does up-turn in climate. And you could question whether -- what's really conservative,

because for the most part, corrosion processes occur most rapidly in the mixed wetted area, where the relative humidities are at up to 100 percent. This transport occurs most rapidly when you get a lot of water, and so to my view, the worst case is when you get a long period of fairly low relative humidity followed by a wet period. Next slide.

And this is just a pretty picture that shows some nice banding. All of this is really temperate effect. Next slide. Now this is a transition to a little change of pace a little bit. This just looks at one of the assumptions that we're all making. We made the same assumption in the calculations I just showed you. Here we looked at precipitations. There's precipitation right there. And if we evaporate that precipitation, there's the evaporation line between Nitrate and Chloride.

Down below, applied the actual data from Nye County Wells, so this is what everybody is assuming. And this is what we see in the ground water, so you could debate how well that is, but I think it's instructive to at least look at what limited data we have, and they don't tend to match our assumptions very well. Next slide.

Group 1, Group 2, Group 3 is a different

subject. Conclusions, if you look at physical separation process. If you walk around where those are residing, you look in the overhangs, you look back in nooks and crannies that are protected from precipitation and you see this physical separation, just all over the place. It's common. It's going to occur. You see it along the Rio Grande in the winter down in El Paso, because the flows are very low and you get salts building up along the banks. Produces a wide range of water chemistry, potentially aggressive environments, certainly high spatial and temporal ability. How long is extended time, I think is an open question. And looks at a subset of the anticipated processes that could affect the water chemistry. Look at one simple one, physical separation. There are other things out there like biological processes that are also important, we didn't look at. Thank you.

DR. GARRICK: Questions? Yes. Go ahead, Joe.

DR. PAYER: John, just a question. Again, the approach and the goals of this work I think are right on, so I applaud you for that. The -- how do you deal with the issue of what is going to precipitate, and when it precipitates, and the

thermodynamic database and brines, and things of that sort? It's always a challenge, so my question is, just what do you do to --

DR. WALTON: Okay. What we did was, is that's why I was careful to label it semi-quantitative. What we did is, we had a very simple model, just assumes -- you know, doesn't tally for activity coefficients, just takes the common salts that people have said might be there, and we put those in the list. And then when they're super-saturated, precipitate immediately. Okay? So it's very simplistic.

DR. PAYER: As single salts or mixtures of salts?

DR. WALTON: Well, what happens is, is mixtures precipitate, and that's why when you rehydrate them you get like Chloride and Nitrate come together. So at each step, for example, Sodium Chloride and Sodium Nitrate are going to precipitate, they precipitate together, so things are allowed to precipitate together, but there's nothing like salt solution or anything complicated like that.

DR. GARRICK: Any other questions from anybody? Thank you very much. All right. Our next speaker, his name ought to have something like Baron

or Lord in front of it, it's such a great name. It's Englebricht von Tiesenhausen, representing Clark County. We've seen Englebricht at many, many of our meetings. He's no newcomer to the Committee, for sure. Maybe we'll make you a Baron.

MR. VON TIESENHAUSEN: First, I'd like to point out, I'm not an expert on anything. I just like to try to understand the system more as a generalist than an expert.

Don Shettel's presentation kind of stole some of my points, but reinforced others, so I want to thank him for that. And Dr. Payer, I think, made some really good points as to what we need to be worried about in the near-field environment. And one of the more important ones for us to consider, the mixed species effects, and not to look at particular species in isolation. Our concerns are basically repository temperature, it's effect on coupled processes, and I will only mention corrosion in passing. I won't go into details. Next slide, please.

That temperatures have been a concern for a long time is pretty obvious. The ACNW in their astuteness wrote a letter to Meserve, and exhorted the Staff to continue to look at chemical issues associated with repository temperatures designs. Now

this is kind of out of context, but it was a statement that was made. Next slide, please.

DOE recently updated a lot of their work, but they issued a White Paper in 2002 where they said the uncertainty in total dose is larger than the difference between operating mols. And this is the HTOM, or the LTOM or the high temperature/low temperature repository. And also, at the total systems level, the difference is not significant. Now to me saying that the uncertainty in total base is larger than the difference is not a very comforting statement, because it can be practically anything. Next slide, please.

The TRB has also been concerned about temperature repository modeling, the temperature differences. And this is a statement by Dr. Cohon which he made in 2001. I'll try to hurry through this. Next slide, please.

Our concerns persist, and that's really the only thing I want to say. I also want to add at this point in time that we share Nye County's concerns with the use of J-13 water for the corrosion tests. And we feel that this is a concern that we really need to address in a little more detail. Next slide, please.

This is just -- you've seen this slide before. I borrowed it from DOE. I'm not capable of putting together graphics that look that nice. And it is really just to show how high the temperatures are, and to understand that at these elevated temperatures, there are almost no kinetic data, and thermodynamic data are sparse. Next slide.

Lot of people like to quote famous persons from Antiquity. I quoted some individuals related to the NRC, and the understanding of coupled processes. You can read them for yourself. And those are actual quotes. The names shall remain anonymous.

The State of Nevada gave a similar presentation of the one they gave today on their evolution of waters, Vadose Zone versus J-13. This is an issue that's also been brought out in the paper by Rosenberg, Godowski and Knauss, also looked at this. And they looked at it at lower temperatures, below boiling temperatures. And the only comment I really want to make is that there seems to be enough data to show that the end points in J-13 water and the end points in Vadose Zone water or pore water are different. And that's as far as I want to go with that statement. Next slide, please.

When we look at water chemistry in the

near-field, I think it is very important to consider the influence of dust, and that really hasn't been addressed too much. And I am the proud recipient, I should say, of some preliminary data from the USGS where they have looked at this issue. The tables are in your handout. This is some compositions. I'll show you some data. I won't spend a lot of time discussing it, because it would take me all day to go through it in detail. I think it is something that really needs to be considered when we look at what the environment on the waste package is. We don't just have any water unaffected by dust on the waste package. Chemistry of the water will be moderated by the dust that is there.

Now just next slide, please. And these are just some compositions of dust analyses, and as I said, there isn't enough time to go through them. Next slide. You'll see that there are other compounds. Approximately one-half percent of the total dust is water soluble, so it will have an effect. That's an average number. Next slide, please. These are more of the water soluble compounds of the ionic species and elements that you'll find. Next slide.

This kind of, I think, clearly shows that

we have almost primordial soup sitting on the waste package. These are all trace elements that you'll likely find in the water when you look at -- when you have to look at corrosion processes, when you look at realistic corrosion processes. And I guess -- next slide, please.

My point is really that I don't think the knowledge base is there to look at fully coupled thermo hydrological chemical corrosion processes at these high temperatures. It isn't the data, either kinetic - definitely not kinetic, certainly not even within the dynamic data that's necessary. The environments are going to be extremely complex. And with that degree of complexity, I don't know if it's even possible to arrive at the reasonable bounding analysis. And Shettel already made the last comment, so I won't go into that any more.

But what's the solution, you know. If you're an engineer and you run up against the problem that you can't reasonably engineer your way around, you look for maybe a different location if you're building a bridge, you look at something else. And to me it would be to go lower temperatures, and do away with a lot of these very critical issues that affect base package performance. And that's really all I

have to say.

DR. GARRICK: Okay. Engelbricht, you've identified a number of areas where you think better data would put us in a much better knowledge with respect to the adequacy of the site. Do you have any views on the feasibility of such data being obtained in a reasonable time? What's your -- are we talking about a problem here that's, from your perspective is solvable, or are we talking about something that is -- would take 100 years to do?

MR. VON TIESENHAUSEN: I don't think it would take 100 years, but certainly with the time frame available, I don't think -- in the temperatures under consideration, I don't think it's possible to get that data. I think if DOE had started, I believe one, you know, funding maybe programs at the universities to look at thermodynamic issues and kinetic issues, maybe we'd get a little further ahead. I don't think now with license application supposedly going forward it can be had.

DR. GARRICK: Yeah. Go ahead, Dr. Bullen.

DR. BULLEN: This novel idea to go to low temperatures is very interesting. How low is low enough in your opinion, Engelbricht?

MR. VON TIESENHAUSEN: That's a very

difficult issue, and you look at DOE's low temperature design and the average is around 80 degrees Centigrade. And that may not, as an average that may -- as an upper bound that may be good. As an average, that may not be good enough. That's kind of my take on it.

DR. GARRICK: Maury, go ahead.

DR. MORGENSTEIN: Yeah. I was just trying to think about following up on Dan's question about how low? And I was going to try to get Don back here and ask him what the stability field for Tachyhydrite was. How -- Don, do you know? I'm sorry. Do you know what the low limit is on Tachyhydrite stability temperature-wise?

DR. SHETTEL: 22 degrees C. And that climbs up to 165 or more, so it has quite a large temperature range of stability.

DR. MORGENSTEIN: Great. Thanks. Well, I'll pass.

MR. VON TIESENHAUSEN: I guess the only comment I would have is if you look at everything that is there, what were uniform.

DR. GARRICK: Just a moment. Rod, you pass? Any other questions? Okay. Thank you very much.

Our next speaker represents the Las Vegas Paiute Tribe, Atef Eizeftawy. Is he here? Oh, there he is.

DR. ELZEFTAWY: My children tell me that I'm technically challenged. Two seconds about my bio. I was born in Alexandria, Egypt some years ago, and in 1964 I got a Bachelor Degree from the University of Alexandria in Ag Engineering. And `68/69 I got Ph.D. from there in Hydrology, and my profession was taken away by the police because he expressed his strong opinion against the war during that time in Egypt, and so I came, without getting the Ph.D. approved, and I went to the University of Florida to get another Ph.D. in Soil Physics. My Master from Egypt was also in Soil Physics, or what we call it, the Unsaturated Zone, Hydrology and modeling and all that. So after I finished the University of Florida, I moved on to the University of Illinois to become an Assistant Professor working with Civil Engineering for the program, and trying to modeling the water, unsaturated flow, salt, and temperature underneath the highways of the United States, especially in the midwest.

Then I got the opportunity to move to Las Vegas, Nevada to work as an Associate Professor with the Desert Research Institute. That's where I got

involved into the high-level waste and so on. And then I came to work for the NRC Staff for three years. Stopped back then, and I didn't like living in Washington, D.C. a whole lot because I always had this thing in my hand. My hands get sweaty all the time for the humidity, so I went back to the dry west, and worked for the state, a small consulting firm.

And just before I came, I wanted to see what I did, and I looked at the miscellaneous of these comments. And one of my comments way back there, says the DOE at the time, talking about Yucca Mountain, was saying the downward flow of the unsaturated zone was one millimeter flux, and then the upward vapor flow was more than that. I made the calculations and I thought oh, boy, the Yucca Mountain is drying out by itself, so that's good place to put the waste. Obviously, that was sort of a joke.

Anyway, I'm not here to present a technical presentation. I'm here on behalf of Gloria Hernandez. She's our Chairperson of the Tribe, and before I start, I think I need to give you one second or two, hopefully about the Native American Tribe. When I became a citizen 30 years ago, I had no idea about the Native American, their plight and so on. But today, we know that they do a lot of gambling. We

have a lot of casinos, and they have some money. Well, there are 600, for you who do not know, there are 600 federally recognized tracts across the United States. Most of them are on the West Coast. In Nevada there are 24 tribes, in California there are probably 30 tribes, and in Arizona might be about 10 or 15. Most tribes are recognized by the United States as it sits here as a federally recognized sovereign nation in the United States. In other words, they do whatever they want to do independent of the United States government. They have their own constitution ratified by the Congress of the United States. They have their own election process, and they have their own government. They pass their own law, and during the last six, seven years, they were given the -- well, the freedom from EPA to provide their own environmental programs and so on.

Well, to make it a little bit shorter than that -- oh, one other point. Some of those tribes have no land whatsoever, homeless, call it that way. Some tribes have an acre piece of land. Some tribes as the Hopi or the Navajo has less than one- fourth or 20 percent of the State of Arizona, so that gives you the range anyway, if you are from the east and you don't know what's going on in the west.

Two years ago, probably about two years ago the -- since I'm the Environmental and Water Consultant for the tribe, two years ago the chairperson thought that well, we should really look at this Yucca Mountain thing. Started to heat up and so the Interstate 95 is crossing the 4,000 acre piece of land that they're sovereign, or have their own sovereignty on it. Anyway, so knowing that I have a little bit background in that program, they said why don't you look at that? I said okay, I will, but who's going to pay my money. Said well, you're not going to have any money from us, so I looked for them, and I meet with them. They pay me in some other project, but this particular program I just don't get a thing. So a couple of weeks ago, Gloria said well, here's the money. You need to go and read this piece of paper that I give you over there. And if you are a lawyer, you're quite welcome to come because I think they hired a lawyer today, and they gave him some nice six figures contract for five years to come, smart guy.

So before I go on, I want to make one comment on her behalf. We would like to say thank you on the record for the Chair of the United States Regulatory Commission who generously gave about an

hour, hour and a half of his time in Las Vegas to meet with the Chairman of the Tribe and the Vice Chairman in the presence of John Greeves. And I'd like also to say thank you for Marty Virgilio, something like that. I don't remember his last name, and John Greeves for taking the time and meeting with us also. And another compliment for Commissioner Merrifield, who took the time and spent four or five hours with us visiting Las Vegas and visiting our land.

A couple of comments that she had written here, which are getting better. And it reads, "No government-to-government consultation or interaction according to the Presidential Executive Order." The Tribe of the United States Government likes to have their standard upgraded a little bit and be treated as equally to the states. They do in many instances, and she also wrote here that, "As a federally recognized Tribe, we should be allowed to play a major role in the Yucca Mountain Program as stated in the Nuclear Waste Policy Act."

Another point here, she said that, "We started to get some fragmented information now and then from the NRC. We haven't got a thing from the DOE, even though we knocked on their doors a couple of times." Some of the major concerns, not technical but

I don't want to claim to be a technical person here because I don't want to field your shots. And you're shooting at one another real nice.

Her point here was, when I explained to her about the background of the site, that if the site was put together as a geologic repository, it should be a geologic repository, not engineering repository. And they are firm on that.

Also looking at the DOE Total Performance Assessment, when I explained to her in layman terms about the modeling and the total system performance and so, and she wrote here, her words, "Accepting the DOE Total Performance Computer Assessment as a method of testing and evaluating the suitability of the Yucca Mountain site is not - underlined - acceptable to the Tribe." In other words, don't do it by the computer and say well, it looks fine. You should have data. You should have things that really supplement all that decision when it comes to the politics of it.

She also said, "They feel - that's the Council - they feel that the NRC and NRC Staff should play their independence role as specified in the Nuclear Waste Policy Act, which means that the NRC should not modify the CFR to fit the technical problem with the DOE Yucca Mountain Program. And if the NRC

does, that is not acceptable to the tribe." So with that, I will end my presentation to you, and if you have any question, I'll be glad to answer them. If you don't, I will just sit down, in a couple of hours fly back.

Thank you for the pleasure of being here. I left in 1987, and I never regret it. So thank you for the time. Come to visit us, and so that's all I want to say. Good luck to you. It looks like you have a lot of good brains and good people, and all that, so we'll -- I want to thank you again.

DR. GARRICK: Thank you. Anybody have any questions before he leaves the podium? Thank you very much. We hope to see you again.

Our next speaker is not from the State of Nevada, but from the Electric Power Research Institute, and is also somebody we've heard from many times, and always makes an important contribution, and that's John Kessler from EPRI.

MR. KESSLER: While I share Engelbricht's heritage in terms of last names, unfortunately the interpretation of Kessler is Kettlemaker, so Sir Kettlemaker doesn't come across.

I thought I want to say not quite something for completely -- that's completely

different, but I'd almost like to bring this discussion full circle back to some themes that I think Abe Van Luik started with yesterday morning, trying to touch on when realism is and isn't needed in TSPAs, how this fits back into the licensing environment that we're in. And one of my intents here is to provide you an example of a non-realism, how that works through, what the potential implications are, and why we make care or not care that we have that unrealism. Next viewgraph, please.

So I'd like to talk about why realism is useful, although I can certainly with this crowd skip that bullet. Why full realism is not always necessary is something I'd like to touch on, and then the question is how much realism is needed for a TSPA used for Yucca Mountain licensing purposes, and perhaps a bit on the process by which improved realism can be achieved. Next viewgraph.

Back where Abe went, because after all, while all this discussion of realism and getting models right is all nice, the point of all of this is potentially to develop a repository that has to go through a licensing process with a lot of approaches and baggage that goes with that. Repeating I think what Abe started here is TSPA regulatory requirements

in Part 63.2, what is it the TSPA should do? Identify depths and sequences of events over 10,000 years and their probabilities of occurrence, examine the effects of the above on performance. That's a subjective thing at this point except with a few quantitative criteria along the way, at least in terms of making a safety case it can projected. Probability weighted dose estimates, plus uncertainties to the reasonably maximally exposed individual. Identification in defense of multiple barriers is another thing that's in there. Tim McCartin had some analysis that talked about potential ways of defending, or at least identifying the multiple barriers, as did Peter Swift in his talks.

I want to argue that the main regulatory requirement here is reasonable expectation of compliance with individual dose limits, and maximum concentration limits, or MCLs here. This is really what it's all about in terms of realism versus potential lack of realism, is that in the end, NRC's going to have to have a reasonable expectation that Yucca Mountain is safe in terms of complying with individual dose limits and MCLs.

The "reasonable expectation" term, EPA tried to take some pain to distinguish that from

reasonable assurance, in the sense that they said they were words that EPA used about trying to develop more best estimate models. And as I think in Abe's talk also about not trying to leave out things just because they're difficult. However, conservative approaches are okay as long as there's still compliance, I think is a potential option that we have here.

TSPA is also a tool for management and understanding, we hope, to evaluate existing knowledge. We want to develop uncertainties and variabilities. TSPA is used to provide an estimate to the range of possible behavior, and when we do this, it's best if the uncertainties and variabilities are not biased. That is, when we're trying to come up with this whole range to develop our knowledge base, if we're biasing our uncertainty ranges or picking maybe a single value that what we think is pessimistic, then we tend to start biasing that in terms of evaluating existing knowledge. And that's more important when we're trying to identify which parts of the system or features, events and processes matter. I put "matter" in quotes there, because certainly that's partially subjective.

For example, does the particular behavior of a system, is there a significant change in the

probability weighted dose, or I call it the dose risk in terms of estimate. The BSC, DOE's contractor, used a plus or minus 1 millirem is potentially a significant change in the risk prioritization report. That's certainly subjective from our standpoint. That seems reasonable, as a somewhat arbitrary quantitative marker of identifying what's significant.

You can use this kind of thing to develop candidate barriers, and identify which ones are important candidate barriers. If that barrier effect matters, and the uncertainty is high, then it should be the focus of attention. And then the question is what about the others? Next viewgraph, please.

One can, as you've seen from some of the DOE presentations, counter some of the uncertainties with conservatism or pessimistic assumptions here. Can we do that? The advantages of doing it, I believe Abe mentioned, as did a few others. It's often easier to defend, especially during licensing. It could be sufficiently robust for the adjudicatory process; that is, that sometimes it's very hard to nail down what the real value is, or the real range is. But given that this is going to be a licensing process with an adjudicatory process at the end, it will be easier sometimes to defend a pessimistic assumption in some

cases.

It can serve to provide boundaries for license conditions. Again, John Garrick mentioned this idea of, you know, maintenance rules. There are going to be other licensing conditions. Potentially that's all used in the process. And then there's a connection to performance confirmation, and the idea is, is that you may want to just have a performance confirmation activity that tries to "confirm" that something is no worse than a certain kind of behavior, rather than trying to develop a performance confirmation activity that tries to identify what the true behavior is. Again, performance confirmation and license conditions are likely to be very strongly linked.

Pitfalls with using conservatisms or pessimistic assumptions is it may distort which part or parts of the system matter. It will distort the relative importance of individual parts or the individual barriers. And before we move on to the next viewgraph, I'm going to provide an example of the effects of one particular conservative approach that's on near-field diffusion.

When I go through some results and sensitivities of the next set of viewgraphs which are

from some EPRI performance assessment work, the point is that I'm trying to make a point about how this conservatism might bias the results, rather than necessarily giving my limited time going into the details of why the curves that we've got look the way they do. Next viewgraph, please.

Okay. One particularly conservative or pessimistic example is the diffusive release model in our recent IMARC-7 TSPA code. Background, I think you've probably got it already, so I'll whiz through this, but a few containers are expected to be actively dripped on, so that tends to limit the release due to advection where we would expect perhaps the majority of the containers would not get dripped on. However, most containers will eventually be in humid air conditions as we've heard about. These thin films of water coating exposed surfaces are a possibility, and this facilitates release due to diffusion if you have a continuous water pathway all the way through.

Our current pessimistic assumptions about diffusive release are here. We assume excellent contact between all the engineered barrier system regions. You can read them all there, and the surrounding rock. In reality, there's likely to be poor contact. We also assume that there's multiple

continuous water pathways through the EDS, where in reality there's likely to be more limited continuous pathways. Dave Esh mentioned this in his presentation yesterday, and in terms of at least for the TPA model, some assumptions they made about the amount of contact or continuous pathways that were different than what we've got here. Next viewgraph, please.

For a single failed container with advective and diffusive releases, what we have here is for -- we're looking at two different species, Iodine 129 and Neptunium 237, where the Iodine has a higher solubility than the Neptunium. And what we see is that for Iodine due to -- for Neptunium advection we get this amount of release in terms of mols per year. For Iodine advection we get here. For Iodine diffusion, this is the release. And why it's higher than Neptunium and why it's got the double hump, we have a certain amount of cladding that fails early, and then we have more cladding that fails later, because we do take credit for cladding. And for Neptunium it tend to -- you have a solubility limit here. The idea is that we have a higher solubility for Iodine that tends to drive more diffusive release compared to Neptunium. Next viewgraph, please.

So now we look across the repository. We

take into account that only some of the containers get dripped on, where more of the containers may be failed but are subject to diffusive release. So let's look at this Iodine 129 species, highly soluble, low absorption that tends to move through the system. What we see is in our model where we have all these well-connected diffusive pathways, we actually on a repository-wide basis have more diffusive release for this high solubility, low absorption species than we do for advective release in our model. Next viewgraph.

For Neptunium, the situation is the other way around. We have Neptunium, more solubility limited, and has more absorption. And here we see that advective release does dominant diffusive. Next viewgraph.

So putting it all together here, this is our primary result from our base case normal release scenario. It doesn't include igneous activity. We also do not yet have colloid transport in our model. The point is that for our nominal release scenario, at 10,000 years we're at something like 10 to the minus 3 millirem per year the RMEI, so in a way we've got this kind of margin. Actually, I should have brought this bar up to 15 which is up here, so we're something like 10 to the 4th lower than the Part 63 limit.

However, this 10,000 year peak is estimated strongly by our conservative diffusion model, because you see that those two radionuclides that dominate this early peak are Iodine and Technetium, and those are released predominantly by diffusion in our particular model. Next viewgraph, please.

So what's the effect of that particular conservative assumption? It affects the relative importance of the unsaturated zone and the saturated zone, because as it's been pointed out, this is a case where we have basically a pulse release at year 1,000, and we want to track through the system. Basically, what we're saying here is that Iodine comes through faster than Neptunium, and if we're already over-emphasizing the release of Iodine and Technetium, we're tending to under- emphasize the relative importance of the saturated zone and the unsaturated zone for retarding the species had we done a more realistic case of release from the EDS.

Plutonium here, we released it and it doesn't even show up. It gets attenuated in the UZ, primarily the SZ. Next viewgraph. So the summary of the UZ and SZ travel times for the unsaturated zone below the repository, we get ranges of travel times in the 1,200 and 3,000 years. The point for this example

is that it's radio element and infiltration rate dependent. For Iodine and Technetium, that in our model have diffusive release dominated, they're at the lower end of the range. For Neptunium and Plutonium that are more advective release dominated, it's at the higher end of the range. You can see that our conservative assumption in one area may be biasing the relative importance of another area.

Saturated Zone, we're seeing travel times of 5,000 to greater than 9,000 years. Again, same thing. Iodine and Technetium -- excuse me. Five hundred I should say here. Iodine and Technetium tend to have travel times in the lower end of the range, Neptunium and Plutonium at the higher end of the range. So the conclusion here that we would get better relative unsaturated zone and saturated zone performance if we had used a more realistic diffusive release model.

Okay. Do we care? Next viewgraph, please. I want to back up and say, you know, what's the relevance of these pessimistic approaches. I think it needs to be said, given the panel that's assembled here, the Yucca Mountain Project is not fundamentally a research project. We're not out to know everything about everything. We need to know

what is necessary to provide reasonable expectations, reasonable assurance, confidence that the Yucca Mountain repository is going to be safe. And in this case from a regulatory standpoint, we measure safety by the particular quantitative limits that are applied in this case. So the purpose is to provide this reasonable expectation that Yucca Mountain system will protect human health. Next viewgraph, please.

I would argue then that it's okay to leave high uncertainty or replace with pessimistic assumptions if it doesn't matter to overall performance assessment of performance. And the corollary that's important, and certainly needs to be discussed, and has been discussed here is that we need to be confident, reasonable expectation so we know some parts do not matter. So if we're going to apply some conservatism realisms in one place, we need to understand what the implications are to make sure that we know some parts do not matter.

Compliance can be -- it may be also okay to use high uncertainty in place of pessimistic assumptions if compliance can be demonstrated anyway. That's the concept of that use of margin. If you're well below, and if you can stay below the dose limit, why do you need to sharpen your pencil more, is the

basic question here. So you could replace with a more realistic model, would only result in more margin. I go back to that example of the EPRI conservative diffusion model. If, for example, we replace it with an approach like Dave Esh showed in his talk yesterday, we'd probably lower those 10,000 year dose numbers by another two orders of magnitude, so we're down from 10 to the minus 3 millirem per year, to 10 to the minus 5 millirem per year. One is really low, the other is incredibly low. I think at this point, DOE has -- it should be allowed to ask the question, why should we bother? Why should we spend the resources to do that? If there's another good reason to do it, fine. But it's not clear to me it has to be done.

On the other hand, additional work could be done to increase the confidence if it's desired for whatever reason. Performance confirmation activities are one way of doing it. Analog studies over the short-term and the long-term are other ways of reducing uncertainties, increasing confidence if necessary. And over the longer run, pessimism can be replaced with more realism at the time when more confidence is required, perhaps at a later stage of the repository development.

For example, say we -- at this stage, DOE might be asking for to construct the repository. They're not calling on the natural barriers to be relied on until a later time. They have more time to increase their confidence or increase NRC's confidence. That's what we're talking about in the sense that some of this can be replaced over the right period of the repository development given the relative importance of a particular barrier at the time that the repository is being developed. Next viewgraph, please.

So the conclusion is that pessimism or conservatism has its place. Realism is important for management purposes. If the management needs to identify what is important without bias, they need to do that to focus resources. Some pessimistic approaches will need to be built into the TSPA model for licensing purposes. DOE will need to establish robustness for the adjudicatory process. It is an adjudicatory process. That is reality, in a sense. That is what is going to be required, to provide boundaries for license conditions, and to provide reasonable expectation level of confidence and compliance with regulations.

The idea is that even the uncertain --

even when we have uncertainties that will always be there to some extent, in the end, NRC needs to be satisfied with the reasonable expectation that regulations will be complied with. And sometimes, that will involve the use of conservatism. Thank you.

DR. GARRICK: John, I think maybe to me an even more significant conclusion here is, you've demonstrated the value of embracing the notions of uncertainty. You've demonstrated the value of knowing that if something is four, or five, or six, or seven orders of magnitude uncertain, that if it's a couple of orders of magnitude below what is driving the risk, or perhaps a compliance requirement, that from the point of view of the analysis you're trying to do, the analysis that led you to the five or seven orders of magnitude of uncertainty is adequate. And to me, that's the most important issue. It's not so much knowing whether your pessimistic or conservative. It's knowing what the uncertainties are, it seems to me. Go ahead, Dan.

DR. BULLEN: Dan Bullen, TRB. I really enjoyed your presentation, although I have a question about your pessimism/conservatism analyses. As you do a TSPA like IMARC or TPA or TSPA, how do you convince yourself that you aren't masking an effect that is

over-simplifying the results, leading you to a conclusion that may or may not be physically real. And how do you address those types of concerns as you look at, for example, the source term issue that we're trying to address here?

MR. KESSLER: We do lots of sensitivity studies. We try to use expert judgment in the sense that in some cases you don't have a good handle on what the realistic value is, or the best estimate value is. In some cases, there's just -- you may have a better handle on not necessarily bounding, but near bounding cases. We'll use judgment to suggest well, it's probably in this range. We might use that value or range of values in what we think is probably a better estimate of what we think reality is, rerun our sensitivities and try to get some understanding then as to, you know, what got masked or what got improper -- got out of balance in terms of relative importance, if we care about, you know, understanding what are the most important parts of the system in terms of their effect on dose risk.

CHAIRMAN HORNBERGER: In other words, you do a more realistic analysis to see whether or not your conclusion is justified.

MR. KESSLER: In some cases we try to do

it. That's right. That's what I'm saying. You do that as a management tool, that we would like to argue the way you use -- you do two different performance assessments. You may wind up doing two performance assessments in the end. You may do one performance assessment that may fall outside some of the bounds of the QA classes that will need to be used in the regulatory proceedings to develop your management understanding of what's most important.

In that case, you might use a lot of expert judgment that wouldn't necessarily withstand the scrutiny of the regulatory process. Once you have that basis to understand what you think is important, then you develop your Sunday Best TSPA. Of course, that's in the eye of the beholder, that you think can withstand the licensing process.

One would hope that behind the scenes, DOE has been doing what they think are more realistic modeling to get their handle on what the important parts of the systems are, from at least a management standpoint.

DR. GARRICK: Ron, and then Rod.

DR. LATANISION: I, too, enjoyed hearing your comments. I'd like to take a very specific case and see whether or not, or how you would deal with

this. And I'm thinking particularly of the issue of the concentration of the environment, or likely environment, or reasonable expectation of what the environment would be in terms of the waste package. How would you deal with that? I mean, we've heard today from some -- gosh, who did we hear from?

MR. KESSLER: John Walton's talk was the last one that talked about that.

DR. LATANISION: I think in Don Shettel's.

MR. KESSLER: And then Don's. Right.

DR. LATANISION: Right, among others. But his view on the concentration phenomena is really quite different than, for example, the Project's view, or perhaps even NRC's view. I'm not sure. How would you deal with that? What level -- how would you deal with determining what is a realistic expectation in terms of the environment?

MR. KESSLER: Well, I need to back-off and ask myself first, why do I care? Why do I care to get the chemistry right? How does it matter to me? And again, I go back in our case to our own set of barriers which, you know, are similar enough to what DOE or NRC is thinking about in terms of barriers. I want to know what's the ultimate impact on those barriers, so in the global sense I'll say I care about

chemistry because it affects certainly, obviously the corrosion of, you know, some of the things in the near-field. It's going to affect solubilities and all things like that, so what I care about is how long does my waste package last? How much release will they get in terms of, you know, how it affects solubility limits? How it might affect retardation, in the sense that these are the main indicators of performance of some of the barriers.

So after that, then what we do is look at, you know, how might this impact corrosion. If we say it could, then it's something that we would want to look into. Now I'm not trying to say exactly how I would address this issue. I'm just trying to say would I look at this issue. Do we think it's potentially important? Yes. It certainly is potentially important in terms of --

DR. LATANISION: Well, given that there is evidence that the environments that are generated by these very, I would say what would appear to be extreme condensation, evap -- concentration are shown to be very corrosive. From your perspective, is this an issue that the Project ought to be exploring in a different way, perhaps, or in more detail than it is today?

MR. KESSLER: The project ought to be exploring what they think are plausible conditions that could lead to, you know, significant degradation of what they're thinking of might happen for their container performance, so the answer is yes. I mean, if they feel that this is plausible, they should have some sort of --

DR. LATANISION: Reasonable expectation.

MR. KESSLER: Well, of course, that's for NRC to decide. But the point is, DOE needs to come in with their own case as to why they feel what Don and John presented is or is not reasonable. Certainly, that would have an effect on what they're making estimates for container corrosion.

DR. GARRICK: Rod.

DR. EWING: Great presentation, but of course, I disagree I think with the results a bit more than some of the others. And that, I would say actually to me what you've described is not an iterative PA process, but more a circular process. And in the extreme what I mean by that is, if you design an analysis that's chemistry-free, and you do a sensitivity analysis, it's no surprise that chemistry doesn't matter. And so, certainly for licensing, you have to identify what matters most,

what the uncertainties are, be able to identify and recognize when you make bounding or conservative calculations, and then you do the sensitivity analysis. But behind all of that is the assumption that you have a useful model. Right?

You start with a model, and then if you do an analysis and you say well, X, Y and Z didn't show up, that doesn't mean that they're not important. It could be that the model is not very useful for analyzing the system. So at the end, you mention natural analogs but, you know, what I always propose is when we have these complicated models, why not pull out the modules and test them either against real laboratory data or natural systems, and design experiments to challenge the efficacy and usefulness of the models.

MR. KESSLER: I'm opposed to that.

DR. EWING: Yeah, but you put that at the end and with a little in Italics, "If necessary". It seems to me it's absolutely necessary from step one.

MR. KESSLER: It is necessary from step one in some areas. If you want to call this circular or whatever, I must protest to the comment about chemistry-free. That's --

DR. EWING: I didn't say your model was

chemistry-free. The example I used, many of these models nearly are chemistry- free. It was an example. If you leave something out and do a sensitivity analysis, don't be surprised that what you left out turns out not to be important.

MR. KESSLER: That's absolutely true.

DR. EWING: Right.

MR. KESSLER: If something is left out, and you don't do it, and it might affect your sensitivity results, that's a problem. Right.

DR. EWING: Because in a real system you have a chance to really see if you left something out.

MR. KESSLER: You're right. And my point would be, is if I care about it, in the sense that I could have some of these particular barriers, effects or whatever, I can have, as John was pointing out, a huge uncertainty range, and it still doesn't affect dose-risk very much.

Now that's partially -- that result is partially from making assumptions about the validity of all the other parts of my system. And that's another concern that we need to keep track of as we do all of this. But I would argue that if we have reasonable confidence in most or all of the other parts, and we still find that we can have -- you know,

we can be way off in one area, or we can live within some huge uncertainty band, I question whether that really needs to be done.

DR. EWING: Let me give you an example that's, I think, relevant to the discussion today. Today we're worried about source term, because if we got the source term correct, then everything that follows would be improved in principle.

Over the last 20 or 30 years, people have worked very hard to develop better Borosilicate glasses, better waste form glasses, better alternative waste form, crystalline ceramics and so on. They're on the shelf, there are a lot of improvements. And yet nearly every step of the way, particularly let's say 10, 20 years ago, the statement was well, we did our analysis, and the waste form doesn't matter, because the geology is the barrier. Okay? And now we're arrived at the point where the geology is not such an important barrier, and we're left when we look at -- think about the presentations for corrosion of spent fuels, models that are on six data sets. And that's, I would argue, a direct result of sensitivity analyses that made very optimistic assumptions about the behavior of different parts of, in this case, the geology of the system that haven't panned out. And so

I think it's important to really get into the details at every level. That's my speech.

MR. KESSLER: I think that it matters again only in some areas if we have some decent knowledge of a good chunk of the areas.

DR. MORGENSTEIN: Rod, I'd like to sit right behind you. I would agree. I'm having major problems with simplest things like natural analogs, going toward the concept of natural analogs when we haven't even actually sat at the site and done an accurate characterization. Don't you want to know and understand the site before you go to Africa to look at Oklo? Granted there's information at Oklo that would help us in certain aspects, but if we don't know what the chemistry of the site is, what the chemistry of the near-field is, what's the difference of what happens at Oklo?

MR. KESSLER: You may be right, you may be wrong.

DR. EWING: Here we disagree. I must interject that. But what I'm really proposing is that, you know, there can be many places in the world, you know, separate from the site itself where we could ask very specific questions, take parts out of the performance assessment, and try it out, see how it

goes.

MR. KESSLER: If those things are relevant to what we need to know to provide confidence, then that would provide additional confidence. It's not -- my take on what Maury said was that if there's something about doing a model, benchmarking against Oklo, that will give us what we need to know about our models, that provides confidence in a particular model that underlies an important barrier, then it's useful to do. It needs to meet all those criteria before we just go do it, because it's nice, because it adds some --

DR. EWING: But we could go to the library and see how difficult it is to do pure chemical modeling. Hydrology is not - -

MR. KESSLER: The point is that there's uncertainties in the model. You're talking about, you know, maybe the particular Neptunium species they used isn't likely to be the right one, or you're sure it's not the right one. I can understand why they may have chosen that, because they may feel that it's bounding in the sense that it provides them among the highest solubilities they get, even though it's not likely to be the right one. That, in my mind, isn't necessarily the wrong approach.

It will, however, give you perhaps some weirdnesses in some of your sub-system performance, like was shown by Peter, this idea that you get this, you know, drop or this discontinuity. That I agree is somewhat of a modeling artifact based on their assumption. However, a discontinuity in that particular -- running from one particular area to another is important? I don't know. It's a sub-system performance criteria. It's not really -- it's something in the middle that I'm not sure is necessarily related to overall safety. We need to be aware of why it's there. I think Peter pointed out why it's there. I came away with appreciating okay, it's based on their assumption about their solubility curve versus pH, and what happens at what certain time. And it's nice to know those things, so you understand what's happening in your model. Does that mean that using that Neptunium solubility distribution that they used is wrong? It doesn't necessarily mean that.

DR. EWING: Well, let me leave this an open question, the following. Now as a reviewer or as a scientist looking at any performance assessment, and not picking on any particular person, I inevitably would be able to find some difficulties. That's

natural in life, but how many mistakes do I have to find before we abandon the analysis or the site? How would I know when I've finally reached the point where I can say well, the analysis is not very good?

MR. KESSLER: If you talk about what's the importance of the mistake.

DR. EWING: There you use your model, and then, you know, if I don't accept your model, then we're in this loop.

DR. GARRICK: Well, one of the things that I'm curious about, Maury said a little earlier that -- I'll interpret what he says, that rather than running off and looking at other sites, we've got a site. Let's look at it, and let's collect data from it, and proceed.

What I guess my question is, are we saying that the four to six billion dollars that's been spent on site characterization was foolishly spent? That we're coming in late now and criticizing a program that may be forthcoming early on, and offered our advice?

DR. MORGENSTEIN: I'd love to speak to that. Yes. Except that we came in many years ago and criticized the program. In the early 80s we said a fracture flow is a fast path. No one listened, so

there's some flaw in the system, because we --

DR. GARRICK: Well, I guess what I'm getting at --

DR. MORGENSTEIN: We all agree today the fracture flow --

DR. GARRICK: The problem is site characterization --

DR. MORGENSTEIN: -- is a fast path.

DR. GARRICK: Yeah.

DR. MORGENSTEIN: So it's whether you deal with a program that's driven by scientific information, and whether you deal with a program that's driven by a political desire to put something in a certain place. I go no further.

DR. GARRICK: And the other thing, you know -- the issue here is, we've got a site and we've got information about a site, and we've got a model, and how do we bring these two together in a rational way to make a decision? We're hearing that site characterization was bad from some people, and we're hearing that the model is bad from others. Is there an opportunity here to pinpoint the problems with the both of these things, and such that our leaders can make a decision?

DR. EWING: My response would be as

follows. And it's in the theme of this workshop. The site characterization -- well, we have the site, we have a certain level of site characterization. In my judgment, we won't know a lot more about the site for the next 100 million or billion dollars. That's just an off-the-cuff judgment. But in the near-field, if we could establish a strong scientific basis for the argument that not much is released, then the deficiencies in site characterization, which will be there simply because the site's complicated, not necessarily because the work wasn't done well, or thoroughly. That might, I think, move the whole project to a more acceptable level.

DR. GARRICK: Yeah, but there's a bit of an inhibition on that strategy. We made the emphasis in this workshop the source term for this reason.

DR. EWING: Right.

DR. GARRICK: But on the other hand, if we emphasize the source term, we're emphasizing in most respects the performance of the waste package. And the image that's created when you do that is this problem of not adequately balancing the analysis between the engineered barriers and the natural setting. What's wrong with being able to demonstrate that the waste package is a million year package, or

100,000 year package? What's wrong with that?

DR. EWING: There's nothing wrong with that, but then the question is, do you have a series of multiple barriers? If that's the only answer, the waste package, the metal container then, you know, people can very legitimately ask for multiple barriers.

DR. GARRICK: Yes. Go ahead, Joe.

DR. PAYER: I -- there's some validity to what you said, but I don't think you can justify not doing work where work can be done effectively to increase understanding because you think you're getting out of balance. I don't think that just -- the sort of logic of that seems to me to be wrong-handed. The image be damned or whatever. I mean, fix the image and then go out and do some more characterization, or do -- you know, let's just do everything we can with the rock, and let's do everything we can with the near-field, and let's do everything we can with the waste form within these bounds. And, you know, if you could design a package that lasts a million years, great.

I think, you know, you still have to do the analysis of what happens, what if the end falls off? You want to know what those other things are

but, you know -- I particularly, I'm not troubled that 97 percent of the containments in the waste package, folks can look and say well, there's even more in the mountain that we're not taking credit for. That's the question. I mean, are we or aren't we, and will the package have that kind of life? Those are the questions. And Rod is saying, and I've been saying from the other side of the waste package, and I'm standing on the outside of it, is these kinds of things can be approached, and are approached, and there's work going on right now that is gathering further information, and helping us define where these boundaries are, and if there are boundaries. And, you know, we ought to get on with it, but there's been a -- in many cases, there's been -- because of the milestones, because of the critical paths, I mean, you know, the old saying on the project is, you know, a 20 year project, there's never been time to do a two year experiment, because milestone, milestone, milestone pops up.

DR. STAEHLE: John, could I add something? Is that possible?

DR. GARRICK: Sure. Give your name.

DR. STAEHLE: Roger Staehle. Rod just made a point that I realize has some interesting

analogies. You know, finally discovered that the fast path was sufficient that we had to depend on the waste package. And then Maury made this, well that was something that was something that was known some time ago. Well, there's a second step on that, another step on that, is the fact that for quite a while we've depended on the J-13 water as the concentrated water, but now it's pretty clear that that's the wrong choice; that, in fact, the right choice is to use the Vadose water, the pore water concentration. And so for a long time, we've used the J-13 chemistry, in fact, probably that's the wrong chemistry.

Now if we take step, this logical process of we did believe this, and we now have formed this, for example, this work that April Pulvirenti has done to show that you, in fact, can penetrate C-22 in something like a centimeter per year under a set of achievable circumstances, it's certainty. The result is true. Whether it works or not is something else, but the point is, we now have another step on that logical process that we used to think relying on a passive film which is sort of an alkaline- based passive film. Well, we're not talking about alkaline-based passive film. We're talking about a very acidic-based process, which is to me an analogy

which is the next step beyond the J-13 vadose step. But it was prompted by the rather insightful thought that maybe what we ought to think about doing, what you guys, somebody ought to think about doing, is to think about the logical process, the model for which Roger suggested, and re- examine how they're approaching this and say well, what about these things? If you take April's work, that says you could penetrate the wall in about four years if you can achieve that chemistry. And that's not with stress corrosion, that's just plain dissolution, so I think we need to kind of think that logic and see if that model of thinking, we need to apply somehow in some logical step-wise process.

MR. GARRICK: Thank you. Yes, Joe.

DR. PAYER: Roger, I don't know that the logic is what's wrong. I mean, if you look at the overall logic, but the environment certainly maybe not have been examined as completely as now what's being suggested. But the logic of identify -- just in the corrosion issue because that's what, you know, where I'm based. But the logic has been to identify what likely environments may be there. The logic has been to examine the behavior of Alloy 22 and Titanium in those environments, so I don't think the process

necessarily is wrong. I don't think the experimental technique is necessarily wrong. Perhaps it hasn't been opened enough to gee, it might be outside of the bounds of, you know, where they've been putting the boundary.

DR. STAEHLE: Well, at the point -- I didn't want to really make the corrosion argument as an argument, because we'll make that later. But it was the thinking that the step-wise process of recognizing things we already know that have not well enough sort of step back and say wait a minute. There's a point here, we just haven't done it right. We haven't examined it properly. And you can then move that to successive levels, as I just suggested, and that was the point. So I think there's a point here that maybe we ought to stop a little bit and think, that was a really wonderful idea.

DR. GARRICK: Thank you. This is the kind of discussion I was hoping for. Now we're not throwing things at each other yet, but when we get to there, I'll really be happy. All right. George.

CHAIRMAN HORNBERGER: Let me try, and I'll throw something at my friend Rod, and try to take some cue from what John Kessler said.

It strikes me, Rod, that at the extreme

end the way I would characterize your view, I think we have to make this a research project, because understanding is the only way to go. And from what I would have taken from John Kessler's opening remarks would be to warn us against that, and that there may be some arguments that we don't have to do that. And so if we look at something like the kind of questions you were asking on Neptunium solubility and what solid phase is controlling, obviously, we would like to do good scientific work, because we would like to understand these things better.

At the end of the day, even if we did the scientific work, I have a suspicion that our lack of precision about the environment might lead us to have big uncertainties as to which solid phases were controlling, because as you pointed out, you can move those stability fields pretty widely. And so I could -- I think that I can make the argument, or I would be willing to make the argument well, if I can in and acknowledge that I have a very wide uncertainty in Neptunium solubility, and I can do an analysis, or John can do an analysis, I can't, to suggest that it doesn't matter all that much, that uncertainty. I can still make the case for reasonable assurance.

I don't see why we have to stop the

process until we get all the scientific knowledge we need. It doesn't mean that we stop the science. We still do want to have the understanding, but I don't know why we have to stop the licensing process to do it.

DR. EWING: Okay. First, I haven't said stop the licensing process. And we're good friends, but I'll say you've been unfair in your characterization of my position.

CHAIRMAN HORNBERGER: Of course.

DR. EWING: Describing it as extreme and research- oriented. I don't think it's extreme to look at the performance assessment and see that actually there are almost no real field tests, at least in the chemistry part. And recognizing that these can be done, I mean, and they're being done in other countries. These aren't original ideas. There are publications, so I think it's not extreme to note the absence of chemistry in large part, the absence of exercising the codes against real natural systems to see what we don't know.

I think the extreme position is, you know, compared to other communities who are involved in modeling, is that we haven't. We're in the extreme position there. And I don't think the licensing

process has to stop; although, if I were in charge of it, I would be concerned to go forward without a few more bells and whistles, but that's not my call.

Now why worry about Neptunium? Well, that's a small enough thing to worry about, but my point is this. The reason it comes out in the analysis as not important is because we put a lot of credit on the waste package. And in previous performance assessments, there was a lot of credit for the cladding, so the optimistic assumptions about different parts of the system or other parts of the system are what are leading to the conclusion well, this isn't so important. We can simply bound it. And if I were in charge of the project, I'd be very concerned that my optimistic assumptions don't pan out.

DR. GARRICK: It seems to me, Rod, what you're saying is that we're not doing a very good, or they're not doing a very good job of addressing the parameter uncertainties.

MR. KESSLER: I think that they've tried, and not in particular this meeting, but I would say the closest we had to trying to understand how might Neptunium solubility, for an example, become more important if certain things were not the way the

project panned out was in Tim's talk. You know, Tim talked about this idea of, you know, how many packages do we need to fail to get to a certain dose? And, you know, how high does the solubility have to be with a certain kind of flow through those containers to get the kind of dose? I think that that's a way to get at, you know, when -- under what circumstances might we care more about Neptunium solubility in that example, if the package is or isn't there, or behaves in a different way.

DR. EWING: But it's more than parametric uncertainty. It's a conceptual uncertainty that I'm worried about, because --

DR. GARRICK: I'm not talking just about parametric. I am including conceptual modeling uncertainty, as well.

I think what I'd like to do is allow some time that they want to have for rearranging things a little bit, because the next session is going to be devoted to hearing from the distinguished expert panel we have. And we want to make that as productive and efficient as possible, so I'd like to call a 15 minute break, and we'll go promptly at 1. Thank you.

(Whereupon, the proceeding in the above-entitled matter went off the record at 12:50

p.m. and resumed at 1:07 p.m.)

CHAIRMAN HORNBERGER: We're going to have a discussion session, but I was advised to warn everybody that we had to rearrange schedules to have lunch at two o'clock, and we neglected to think that the cafeteria closes at two o'clock. So if any of you are particularly hungry and have to run off and grab a sandwich and come back, I will understand. Otherwise, you are going to be on your own with a closed cafeteria at two. John, it's now yours.

DR. GARRICK: Thank you. Where is my panel?

PARTICIPANT: They're in the cafeteria.

(Laughter.)

DR. GARRICK: All right. This is going to be a highlight session. What we want to do is devote the next hour to the panel, the distinguished panel, and we'll keep the Committee reasonably quiet during that time. So the approach we'll take is I'd like to suggest that each of the panel members take the microphone and spend a few minutes telling their impressions of what they've heard and whatever other comments or views that you care to make. And then we will open up the discussion to everybody, including members from the audience and members of the

Committee, DOE, NRC, to ask whatever questions with whatever remaining time there is.

Also, to avoid any dozing within the distinguished panel, I'm not going to indicate the order that --

(Laughter.)

-- that you'll be called on to speak. I'm going to -- so with that, I think we will proceed, and I think I'll ask Professor Latanision from MIT to be the lead-off speaker.

DR. LATANISION: I'm going to use the overhead, so if we could just set it up, for just two transparencies.

John, let me first make a very important statement, and that is that -- disclaimer is probably the better word, that although I'm here as a member of the Technical Review Board, and this is true of Dan as well, the comments we will make during this session are of course our own and not necessarily Board positions.

DR. BULLEN: And have made. And have made in the last two days.

(Laughter.)

DR. GARRICK: So much for disclaimers.

DR. LATANISION: I want to make one

observation, and then I want to focus my comments on one issue. The observation is that having been a Board member now since, I guess, June of this past year, I almost can predict what the people around this table and in room are going to say when they have an opportunity, and that's not all bad. But on the other hand, it really raises an issue that I'm concerned about, and that is we continue to bring forth concerns about such things as the concentration phenomena that may or may not occur in the repository and which could have tremendous impact on the waste packages, but the reality is the next time we meet, whether it's in this forum or a Board meeting or an Appendix 7 meeting in which key technical issues are talked about, we'll talk about them again. And I just think we need to find a forum in which we can address these issues where all the interested parties get together and instead of presenting what we've already seen before we really do have this sort of knock down, drag out discussion that I thought we were heading towards about 20 minutes ago.

DR. GARRICK: Yes. We've been having this debate for ten years.

DR. LATANISION: And, frankly, I think that may have been the most interesting part, and I

don't mean to demean what we've done here, but I found that conversation to be really, I think, really important. And I think there are many other issues that deserve the kind of detailed scrutiny that I just haven't seen. I mean I've heard a lot of these issues a number of times, but I think it's time to get really serious about them, and I don't think that will happen with another series of formal presentations that, as I said, I can almost predict what people are going to say.

DR. GARRICK: Yes.

DR. LATANISION: So I'm just imploring those interested parties that we need to do something like that. I'm not sure what the best forum for doing it is, but I think we need to do that.

CHAIRMAN HORNBERGER: Could we ask if somebody wants to predict what you're going to say now?

(Laughter.)

DR. LATANISION: Well, you might actually have been able to predict. I've mentioned a couple of times my concern about some of the temperature issues, and from the point of view of corrosion engineering, temperature is obviously a very important issue. It affects all of the modes of degradation that are

typical in a corrosion engineer's lexicon. By that I mean the uniform corrosion rates, the rates of localized corrosion, all of those phenomena are affected by temperature, along with the environmental chemistry and state of stress of the material and so on. All those issues play a role.

And I won't repeat what I said about uniform corrosion yesterday and then earlier today, except to say that I don't -- I think we've collected data at temperatures which are not inside the envelope of the high temperature operating mode. And I mean I just don't think we've done that, and until we do I think the issue of a reasonable expectation, to quote John's comments a few minutes ago, I think there are going to be uncertainties in just exactly what corrosion rates are likely from the point of view of uniform corrosion.

And that affects -- the first order decision is whether or not there is a sufficient mass of material in terms of the drip shield and the exterior of the waste package to sustain 10,000 years or whatever it might be. And without having accurate projections of uniform corrosion rates, although my intuition tells me that the rates are likely to be low enough that that isn't the problem, I still think

there's a reasonable uncertainty based on the fact that most of the testing has not been done, from my perspective, in the temperature range that's important. I don't think it's impossible to do, it's very manageable, but I think it needs to be done.

I want to just show -- just to follow the temperature issue to a certain extent and talk about one form of corrosion that we haven't said very much about.

DR. PAYER: I predict a hydrogen comment.

(Laughter.)

DR. LATANISION: I know this guy. I've known him for a long time, but he's wrong, I'm happy to say.

The issue of the -- well, no longer a debate, I guess, but the issue of low temperature operating mode as opposed to high temperature operating mode has been mentioned a number of times, and I'm showing you here some data that was shown to the Board at a meeting in January of this year for the first time, and I think it's really very important data. What we're looking at here is what are called cyclic polarization test data for Alloy 22 in concentrated brines. These are brines that range in concentration up from somewhere around ten to 18 molar

so they're very concentrated. And what you're looking at here is the difference in potential between what is called, in terms of corrosion engineering, the open circuit potential and the protection potential, or repassivation potential.

Without going into a lot of detail, the essence of that difference is that when the difference becomes zero the material becomes susceptible to localized corrosion, in this case, the crevice corrosion. These are crevice samples that were exposed to this brine solution. Now, what you see here is that the temperature at which the difference in potential extinguishes is around 140 degrees, and this is in concentrated calcium chloride brines without nitrates, and nitrates are known to actually act as an inhibitor for crevice corrosion.

But what this shows is that when you exceed 140 degrees, the susceptibility to crevice corrosion is manifested. That means that if you have an engineering system which is designed or has crevices present, those crevices are likely to be activated when you exceed that temperature. The same crevices at lower temperatures will remain inactive. I mean that's the essence of this data.

There's one more transparency which shows

a similar set of data with nitrates present. And in this data the conclusion is that the nitrates act to inhibit to a certain extent, but once again you still see the zero point is somewhere around 150.

So we have a waste package that has closure wells, there are likely to be crevices present if those wells are defective at all. We have, in addition to this data, data emerging from the folks in San Antonio from the Center for Nuclear Waste --

PARTICIPANT: Regulatory Analysis.

DR. LATANISION: Right, Regulatory Analysis, which shows that welded surfaces are even more susceptible to crevice corrosion in similar brine solutions, and so it just raises in my mind a flag that says that we're heading off in a direction with the high temperature operating mode, and we're seeing the evolution of data, some from the project, that seems to be contrary to a high temperature operation. And I think this is an issue in terms of the question of uncertainties or the question of reasonable expectations that has to be dealt with.

And so I think I just wanted to focus on that one issue. There are many other issues that we could talk about in terms of localized corrosion, but I think this one is a very important one. It seems

really contrary to the direction, as I understand it, that the project is heading, and I think there needs to be some serious conversation about this.

DR. GARRICK: Good. I think what we'll do is we'll proceed right through the remarks before we ask questions. Joe.

DR. PAYER: One of the advantages of having a presentation on my laptop is you can make slides as you sit here. And one of the disadvantage is you can make slides when you sit here.

I just want to summarize a couple of things we've said, and I think it's a reasonable follow-on to what Ron was saying and the concerns that any of us that have deal with corrosion have about these issues. We showed this little cartoon yesterday, and I think it's still real, it captures the reality. If this is the environment, the population of environments, and if this is the population of the corrosion resistance of a material, the whole issue is where do they overlap, because that's where corrosion can occur? If that corrosion can occur, I think what we're interested in is, one, showing where those environments are, but that's not far enough.

The next question is can we correlate

those conditions with real repository conditions? How do they form? When, where and how much is formed? Will the environments persist? Because one of the things that's lost in most of our testing modes and most of the thermodynamic modeling on a potential PH diagram, people point to a given potential in PH and say here's what happens. In real systems, they're trajectories of potential in PH, the solutions aren't constant.

And so this is a starting point, but then we've got to build on that. So if there's something in there that's consuming the acidity, then it's become more alkaline. If there's something that's consuming the hydroxyl ions, it's going to become more acidic. And we know about these processes, it's just a matter of working them in.

So will these environments form? How much, where and how many times? Will the environments persist? If they don't persist, if they stifle or rest or go away because the package becomes dry in that area, could they reform and start again? Next slide.

And this just reminds that there are predictions of the temperature/time behavior. Next slide. Those predictions can be coupled with other

information to give us a relative humidity as a function of time behavior. And based on our knowledge and assumptions and analysis of what's on the package, we can make judgment at a given relative humidity. Will it be dry or will it be maybe dry or maybe wet or will it almost certainly -- and this variability comes in and what's on the package surface? If it's something that's highly hygroscopic, it's going to be wet at lower relative humidity. So that's information that we've had and that people are looking at. Next slide.

Then if you take these two populations, the environment and the material, and let's just for the scenario here say we've got a given material. So we've got Alloy 22 and that's not moving in either direction; that's fixed. Well, at some temperature, high temperature, I would say that we went from high temperature to low, at some temperature, wherever that is, it's going to be dry. So essentially these environments are removed from the material, and we would expect no corrosion.

At some other temperature, though, we're going to reach the location where in fact we have a wet environment, and there's going to be perhaps an area of overlap. As the temperature decreases, we

would expect that area of temperature of overlap to get smaller, and I think we would agree there's some lower temperature, wherever that temperature is, where those fields separate again. So we've got no corrosion, no corrosion, and in this scenario, we've got a temperature range which could be correlated with a time range that is the range of vulnerability to corrosion.

So if we have overlap, though, again in this question mark area, again, if it occurs, how large that is, how long it will prevail depends on the water chemistry in that area. If that location is there, is there water availability? Will the chemistry persist? And so I think we've got a logic and a rationale for dealing with this. The question is do we have sufficient data and understanding. Next slide.

The other thing to recognize, I think, if this is the range of environments and this is the range of materials that we're really dealing with, and I think it's come out pretty clearly here from the various presentations of DOE, NRC, the State of Nevada and some others, that we really could be talking about a family of waters. And I just suggest here that the ambient waters would be skewed to this side -- this is

all qualitative here at this point, but the family of waters would be skewed to this side for the ambient waters, the carbonate/mixed ionic brines, the types of things that a lot of the testing on the DOE project has been done and quite a bit of testing in the project, would be someplace across there. And these very acidic concentrated halide brines would be probably the farthest population to the right. So, again, this is qualitative, but that's the general movement.

If you look up here at the material, that material can -- and what we're trying to think about here is how this overlap is formed and how large it is, the base material, solution annealed, is probably over here. What will move that further to the right is more chromium, nickel and molybdenum, and the examples of that are the corrosion behavior, a 316, 825, which is a lower chrome, nickel, mali (ph) alloy, and C-22, which is the most resistant we've looked at. And that's going to shift that field over there making the likelihood of overlap less. Things like weld and heat-affected zone, Ron mentioned this just a moment ago, it also comes up if thermal aging occurs, and we do get precipitation of phases or ordering, that could shift this field to the right. But the logic, I

think, prevails. The question is do we get overlap or not? Next slide.

Just now in summary comments, what I've taken home out of this, and sometimes you come here just to reinforces your biases and other times you learn things, but this is a combination of both, I think. But just underlying again this whole issue of water as being the primary accessor, meaning it's the primary thing that's going to open up a package and cause penetrations. The question then is when, how much and what gets in? Once it get into the package, it's going to be the -- provide access by going through clad that's not already open and mobilizing form.

Again, the question is when, how much, how often? It's going to be the mobilizing species, either in thin films for diffusive transport or droplets in flow by advective flow, and it's also going to be the determinant, one of the key determinants in the transport process. What kinds of radionuclide transport will we see? So it all comes -- it's not the only thing that's important, but water when you're talking about the source term I think is a very critical part of it. Next slide.

The black here are things I said at the

end of my talk, and the blue italicized are things I added as summary comments here. So this is the summary of some of the things I've taken out. I think we're still talking about water contacting the waste package, the waste package lifetime, releases of waste form and alteration, mobilization and transport. Those are logical boxes. You could break them up or add other ones, but that's a reasonable flow, I think.

So using this water contact in the waste packages we know that condensation on cooling is going to occur, we know that it's likely that dripping will occur sometimes in some places. How much, where, how often? Waste package lifetime, we know we're going to get full containment for some period of time. Is that a long time or a short time? We know that penetrations will ultimately occur if we're looking over certainly hundreds of thousands and millions of years, and with localized corrosion much sooner than that. Water will access the waste package internals, but it's going to access all of them. It's going to get at the carbon, it's going to corrode the carbon steel that's in the package creating large volumes of iron oxide. It's going to attack the aluminum that's in there, it's going to attack the zirconium, and it also will get at the UO2.

Once it gets to the spent fuel, it's going to release radionuclides by UO2 corrosion and formation of alteration products, and Rod's been telling us a lot about that, and could tell us a lot more. What's going to happen then as far as retardation in those waste products, in the corrosion products and as it goes through the invert? And then once these things are mobilized, we seem to have a pretty good handle on the inventory of radionuclides and how that inventory changes over time. The issue is where are they solved, where are they dissolved, where are they sorbed, are they sorbed, in any case, and so forth.

I would say that it's my opinion that the DOE and NRC models have identified these relevant processes, and they've identified a lot of detail below that set of processes. The question is, the issues are how sound is the technical basis in the data to support models of data to support that analysis? How solid are they in providing us understanding and confidence? But I think the structure makes sense. I would not suggest that we abandon this and start again. Thank you.

DR. GARRICK: Thank you. Maury?

DR. MORGENSTEIN: Leave everything up.

DR. GARRICK: Microphone.

DR. MORGENSTEIN: I'd like to essentially concentrate on the vadose zone environment. My feeling is that it's a very complex area that we don't understand at present the very basics of a hydrogeochemistry, we don't have a good handle on water entering -- the chemistry of the water entering the system in soil zones. There's no reason to presume that water in the soil zone in 40-mile wash has the same chemistry as water in the soil zone on top of Yucca Mountain. The soil parameters are different.

Rain water entering the soil does so and reacts with the soil immediately and picks up a soil signature. It's that signature that starts out as Vadose water and goes down the system and eventually reaches the near field. We don't have a mass balance between what water chemistry looks like at the surface and water in the saturated zone. This is a basic lack of understanding.

When we look at the behavior of the EBS items, such as C-22 and Titanium-7 as barriers in the environment, they can react with water that's perturbated by both the temperature of the system and the variations of the dynamics of the system, the

environment, as it changes through time. If we go and look at the variety of different water chemistries that could occur and we go to Joe's excellent diagrams of realistic range of environment and range of material susceptibility and we look at the realistic range of environments that could occur through time, we have a minuscule understanding today of what some of those environments would look like.

I feel that the project is probably moving too fast, and if we haven't to date been able to collect and acquire these information, I don't know what kind of confidence we have, we would get in understanding prior to licensing. And I say prior to licensing or initiation of licensing in that it seems to be inappropriate at best to go into the licensing arena without a basic understanding of what is offered in the system, how the system will work or how it could work or what the variations are. TSPA is dominantly based on the EBS today. It's not based on a natural system that we can rely on due to the fast path.

There is obviously degrees of retardation offered by the natural system. It is not clear that this degree of retardation is sufficient to meet licensing requirements. There is clearly a

desirability to have a strong EBS in place to justify the site, yet we don't have the basic understanding today of what the parameters are, the basic reactions that might take place.

For example, we have a deliquescent tachyhydrite that we see that forms as a precipitate on whatever surface water evaporates on. This forms from pore water but certainly doesn't form, to our knowledge of these, from waters that might look like saturated zone water. Yet much of the project has concentrated on water chemistries that one might find in the saturated zone.

Not saying that you can't find any saturated zone water compositions in the Vadose Zone, I'm saying that dominantly it's one small composition that you might find. More likely you will find a variety of compositions that we have not talked about at all today. There's some sort of variance of pore water, some sort of variance of pore water in addition to mixes of pore water and the elusive soil zone water, which we have no idea about. And unless we understand that chemistry, we have no real assurance that important barriers, such as C-22, will function was we envision.

So I'm not confident at this point that we

have, or that the project has, the ability within a short time frame to acquire the information that is really required. If, however, the time frame might change, I do have confidence that the talent has the capability to acquire information. I'll leave it there.

DR. GARRICK: Okay. Thank you. Dan?

DR. BULLEN: Thank you, John. Actually, when you called me about three or four months ago to invite me to come and consider sitting on this panel, I started thinking about source term and uncertainties, and then actually you mentioned this morning sort of the biosphere and uptake, and I'll talk about two of those issues.

Having followed two distinguished material scientists, maybe there's not much that I can say except that I would like to remind you of a comment that was made by one of my predecessors on the Board. When I first met Ellis Verinka I asked about, "Well, you know, kind of what material would you pick to contain the waste in a repository environment," and his first question to me was, "Well, what's the environment? You know, depending on the environment, I can pick a material that will probably last and perform pretty well." And thinking about that you've

got to look at the history of the repository design and testing.

I'll go back to sort of the late -- maybe mid to late 1980s when they were talking about unsaturated zone and placement and a thermal environment that was going to be greater than 96 degrees C for thousands of years; it was going to be very hot. And very limited water content, tenth of a millimeter per year. We've heard that before. And, obviously, we found out that there's much more water available.

But the early waste package design was a bore-hole emplacement, one-centimeter, 304 stainless steel container that you put in the ground and it got very hot. I actually did some performance assessment modeling on that type of design for early EPRI work and tried to figure out how to do a surface diffusion transport pathway out of a perforated container at the weld interface, and it's a hard calculation to do, so I have a great deal of admiration for these people who've been doing diffusive transport.

But it points to the evolution of the waste package design as we learned more about the Mountain. We learned that there wasn't a tenth of a millimeter of water per year, and so they went into --

well, actually, I'll blame the Board for a bore-hole emplaced large waste package, which is the next thing that we did. Our predecessor said there should be a drift and not a shaft. So that waste package got a ten-centimeter carbon steel outer barrier over a two-centimeter 825 inner barrier. That carbon steel outer barrier was a corrosion allowance barrier; remember hearing about that.

Unfortunately, and in fact at that time I had joined the Board, the Technical Review Board, and we found that there was more water available at the Mountain. In fact, there was much more water than a tenth of a millimeter per year, maybe tens of millimeters, maybe in the pluvial conditions hundreds of millimeters per year. So I was fortunate enough to be one of the Board members that was asked to go to the Director's Office, Director of Office of Civilian Radioactive Waste Management, to brief him about a letter, and my comment to the Acting Director at the time was, "Lake, your waste package is inside out." Okay. Well, later they changed the design so that it was actually two centimeters of 825 over. At that time it was 316 stainless. And my next meeting at Lake's office was he told me I was right. I should have quit right there, that's the only time Lake ever

told me I was right.

(Laughter.)

Now, it's evolved from 825 to 625 to Alloy 22 based on the need for the perception of improved performance. And part of that's due to the fact that we've learned more about the environment. We've learned that maybe there's more water. We've also learned that maybe our predictive modeling of the environment isn't as good as it should have been. And I want to harken back to some of the underground tests that have been done, specifically the drift scale test. As the drift scale test was envisioned, they were actually making calculations to predict the near field environment next to the waste package.

And there was a prediction that said we're going to boil enough water that we're going to drive away all the O2. So the partial pressure of oxygen is going to go down so low that it won't be there and it will last. And so I remember asking over and over again what's the PO2 of the drift scale heater test, and I think it was Dr. Bill Boyle who always answered that they didn't have the data or ultimately it ended up being the concentration in air, which was probably not too much of a surprise. But they had made predictions and the predictions were presented in our

meetings that said that we were going to drive away the oxygen.

Now, they also predicted ponding of water above the repository, and in some cases they were correct; in other cases they had fracture flow, so it drained below. And so there were changes in the perception of the understanding of the environment. Now, all this kind of ties into what the Board has raised over the past six years that I've been on it with respect to the reduction in uncertainties, which Dr. Garrick mentioned earlier today. And these have been a key issue for the Board.

The problem that we run into is that you can't deal with uncertainties if the models that you're trying to use to model those uncertainties don't address the issues like Dr. Ewing said this morning. For example, the Supplemental Science and Performance Analysis, LTOM, HTOM Analysis, has no temperature dependence on corrosion in some of those, and so you don't get a big difference in whether or not there's a corrosion effect. And in fact, there's no localized corrosion, because the localized corrosion model isn't kicked in, because there weren't data to support it at the time. Not having data they decided there wasn't any corrosion. Well, my esteemed

colleague on the Board, Dr. Latanision pointed out that, "Well, maybe we've got some data now, so that might not be the issue there."

So without relevant deliquescence data basically we asked as a Board for a high temperature and a low temperature analysis. And, obviously, my colleague has already talked about the issue with respect to the deliquescence of the salts and the low-class corrosion, so I won't revisit that.

I would like to offer a personal opinion and agree with Baron Englebricht von Tiesenhausen, and say that I think he was correct that a cooler repository design may be desirable, not only because it's less difficult to model but it's more closely related to the current ambient conditions at the Mountain. And so the less you perturb the Mountain maybe the better off we are. And maybe we don't get to the high chloride concentrations and high salt concentrations that we see, and I'm not saying that deliquescence doesn't occur and all that, but it may not be as aggressive an environment.

I want to change gears just for a second, and then I'll let Rod Ewing have the last 20 minutes, because I think he'll probably need it.

(Laughter.)

We had talked about biosphere and uptake. I in a previous lifetime have been working on low-level radioactive waste management and actually am very familiar with the biosphere code that they use for dose assessment, which is the GENIE code or GENIE-S code. And I've done some low-level waste performance assessment modeling for about 15 years, and I've participated an independent performance assessment model for a compact license application in the Midwest, and I'm very concerned about the 3,000 acre feet of water dilution factor, because I think that that might be masking some significant problems associated with the biosphere model. Predominantly, because if I have a plume that's coming by and I decide that I'm only going to draw my drinking water or maybe my irrigation water for my small patch garden that I'm going to grow my tomatoes in, and I have a tomato and cucumber diet because that's what I eat, I think I have a potential for a significantly greater dose than if I take 3,000 acre feet and dilute it with all the radionuclides that are in the plume.

And so even though I know it's the regulatory requirement that you do these things, I think that the ACNW, and certainly our Board when we start talking about issues related to the biosphere,

I'm going to raise the issue of I don't think that that is -- first, it's not realistic, but, secondly, it's not conservative. And it's not conservative because I can have a small source of water with a high concentration that's not significantly diluted that may give me significantly greater dose than, and I'm not saying I've done the calculations, but it may give me significantly greater dose than what is predicted with that great dilution factor.

Now, with that, I've raised a couple of issues, I've written down a couple questions. I'll wait till the last speaker goes, and then I'll ask my questions. But thank you, Mr. Chairman.

DR. GARRICK: Thank you. Well, I don't want you to speculate about why you're last, Rob, but we'd like to hear from you.

DR. EWING: Well, as the constant critic of performance assessment, let me start with a confession. If I had DOE's job or if I had the job of the NRC, the very first thing I would do is a performance assessment, because the performance assessment informs one about how things are connected. I think where I part company with many is that having done the performance assessment, it would be a long time before I'd believe the results. I think the

exercise is informative, but the results almost certainly are wrong, and the question is are they so wrong as to be not useful?

And to talk about this a little bit, John was kind enough to give me the first viewgraph of the workshop. And I like this very much, I use it in classes, but what I want to say is that although it's a good beginning, I think it really doesn't emphasize the challenges we have when we do a performance assessment.

First, the idea that we had discrete packet we can work on is not very useful. In fact, these discrete packets are highly couple in a non-linear way system, and so when we do the one-off/one-on analysis what that is telling me is that they're probably not coupled enough because it's done too easily.

I know what John means by initial conditions, but on top of getting the initial conditions right, which are assigning probabilities to seismic events, the real challenge is to get the boundary conditions for the different stages here, because the boundary conditions, as these units interact with one another, evolve over time. That's the water chemistry, temperature, the poracity, the

permeability and so on. And the boundary conditions, the environment in which the metals or the waste form have to survive, are really the first step in the story and perhaps the most important step.

Also, from the discussion yesterday between John and I comparing a nuclear power plant to a repository, we left the discussion where there was a challenge of describing a passive system. Well, the point I want to make is that a geologic repository is not a passive system, it's a very active, dynamic system, and I think this is maybe cultural. Depending on your training, if you're a geologist, you look at the Mountain and you see all the parts working, and if you're an engineer, you go and it looks like a static system in which we should be able to take a part off and add a part. And I think this is part of the difficulty.

Then I would also say that if you listen or think carefully about the TSPA or the TPA and what's actually said, the physics of the system is what is generally modeled, and I want to suggest that actually the chemistry of this system may be the dominant driving force in terms of the end result. I mean there's chemistry in the model but from a geochemical point of view, it's at a pretty primitive

level.

And then, finally, there's the remarkable extrapolation over time, but what's also remarkable is the extrapolation over scale where in the total system's performance assessment sometimes we're at the atomic scale, we have models at that scale, and then over time we amplify those processes so that we're at scales of kilometers. And this isn't actually very often done. So that's the starting point. We have really a tough problem here.

Now, the question then is in what context can we deal with this problem? And I think an impression that I have from this workshop is that if I look at the TSPA and the TPA in a very natural and understandable way, I would say in terms of modeling they've evolved into a corner, talking one to the other, but what's missing, and it's not part of the license application process, is the broader context in terms of what can be done by modeling.

Keep in mind that now the whole world is modeling. There are lots of people with complicated problems and trying to find ways to do things that aren't too different from what we're doing here. And this will seem like a digression, and people generally don't believe when you hear what I'm about to say, but

I'll go ahead and say it, let me try to put what we're doing into a context.

Several years ago I became very interested in the impact of the fuel cycle and the carbon cycle in global warming. The question was simple: What impact can nuclear power have on global warming? So I began working with people who doing carbon cycle modeling, global warming modeling. It was very similar. Same scale, atomic scale to global scale, lots of physics, lots of chemistry, non-linear, lots of boxes all connected to one another. Actually, in terms of the computation scale not too different, I think, from what we're attempting here. Depends on which model you're talking about. And in fact similar in the sense that there was usually just a single end point -- what is the CO2 content or what is the temperature, if you think in terms of our end point of what is the number of rems at a certain point in time and space. And it's very interesting to just -- well, it's very stimulating, but very interesting just to look at what they're doing when they have this problem and how it's handled.

Well, first, they have an advantage. We have a geologic record and so we can buy numerous proxies, would be the term, oxygen isotopes or tree

rings, whatever. We can model or we can come up with past temperatures or CO2 content. So for their modeling they can develop models and run them backwards. A thousand years is no problem. Typically run back 10,000, 20,000 years. Can be run longer but that becomes very speculative. But a solid base of reverse modeling with lots of different proxies and different kinds of models.

Now, how far do they go forward in their predictions? Well, the period of interest is about 100 years, so with that database of thousands of years of model checking, they go 100 years into the future. Now, think about that compared to what we're doing. We have data for materials on the scale of years, for waste form on the scale of six years, eight years, ten years, and yet our regulation requires us to run our model out to 10,000 years. And if you graph this, I didn't make the nice overhead, you see the grand difference.

The other very interesting aspect that is of the climate modeling is -- of course, there are studies that go for thousand of years, but for the next 100 years people have asked, well, given the uncertainty in the model -- and here they have many models, they have probablistic models, deterministic

models, lots of people are doing this, uncertainty is discussed and calculated. The question has been asked, "Well, how can I extrapolate my results before the uncertainty keeps me or hinders my ability to make a policy decision?" Well, in our arena, the question should be, "How far can I extrapolate my results before the uncertainty is so large I can't reasonably say that I've complied with the regulation?" And what's interesting for the climate modeling that time period is 20 or 30 years. It's very short, even given this long time period.

And so what I would like to suggest, no one has time for this, but for our modeling efforts it would be very informative to look around at other systems, look for complex systems and ask, well, what are the tricks and what are the limitations and see if we're fooling ourselves. And if we're not fooling ourselves, can we at least fool someone else with what we're doing?

Now, speaking -- have I used my time?

DR. GARRICK: Go ahead.

DR. EWING: Okay. Now, speaking specifically, going to say -- and I could pick on either the TSPA or the TPA, it's not difficult, but as an example I'll pick --

DR. CAMPBELL: Pick on John Kessler, that's what we're here for.

(Laughter.)

DR. EWING: I'll move to TPA now. But as an example of how uncertainty should be considered in what we're doing at, not grand scale, but specific scale, let's take the recently added corrosion model, glass corrosion model. That model comes from a Ph.D. dissertation of a German named Burt Granbow, it's about 20 years old and it's fairly standard now. There's a long-term rate and a short-term rate. Short term doesn't matter very much. But in France now, the French being very critical of the German work, the real issue in their thinking about it is that long-term rate is very difficult to measure in short-term experiments because it's so low. And so if I'm on a panel there and when I'm in France we're discussing how long does the experiment have to run in order to reduce the uncertainty of the extrapolation for thousands of years?

That's a very reasonable and logical question to ask, but in this discussion for the past two days I haven't heard anything of that form. And I think that's -- you know, there's the grand uncertainty, but within every part of the model I

think one has to look and -- you know, if you have parametric uncertainty or conceptual model uncertainty, look at that uncertainty, extrapolate it over time and propagate it through the other parts of the model.

And I'll just as an aside say what I didn't learn very much about during the past two days is uncertainty. It's on everyone's lips but no one calculated it, I didn't see it evolve over time, I don't understand how we're going to handle this.

And then, finally, going back to one of Abe's bullets where he says one of the things we want to do is provide the basis for judging the adequacy of the models or the modeling, I applaud that, but I didn't hear any discussion on how we judge the adequacy. Is it against some scientific standard? Is it against a standard that we meet the regulation? Is it against some reduced uncertainty in the models? I don't know. Clearly, I think judging the adequacy of the models, from my point of view, means using the models in real systems, real geologic systems, real experimental systems and seeing how well they work. And that part of the program is less than I think is desirable. So that's my speech.

DR. GARRICK: Very good. All of the

comments were excellent. What I had hoped we'd be able to do, and unfortunately we're going to have to end promptly at two because we have a commitment that we have to deal with, I had hoped to after the remarks, and this is not a criticism of the length of remarks, they were all appropriate and timely and of the right length, but I had hoped to give DOE and NRC an opportunity to ask a question or two on the basis of what they'd heard, because the whole discussion has been sort of beating up on these models, and maybe in a couple minutes -- or just for a couple minutes we can at least start that. Abe, would you like to respond to anything you've heard? And then I will ask the same thing of the NRC, Andy.

DR. VAN LUIK: Abe Van Luik, DOE. I think that the presentations made by the panelists were very interesting, and several of us were taking notes. I think there are some things that we obviously have to go home and work on a little bit, but all in all this is not -- nothing that's said here today is really a surprise or an "Oh, my gosh, we never thought of that," type of thing. So I'm looking forward to the input from this meeting, but I think that as far as what these gentlemen have said, basically there's no disagreement. We need to provide the NRC in our

license application the basis for our modeling. Some of the statements made go directly towards that our basis is insufficient and we're going to go home and do our homework, and you'll see the results sometime in the future.

DR. GARRICK: Thanks. Thanks, Abe. Andy?

DR. CAMPBELL: I think that for our purposes what is very useful about this sort of interaction is it gives us more understanding of issues from a different perspective and that we can factor that into our review of what DOE is doing. And certainly in the area of the higher temperatures on the waste package we've actually been looking at that for a while. The Committee was briefed on that I believe last June by Dave and Tae Anh. That's certainly an area that was identified as requiring more understanding because that was considered an area that could lead to more extensive corrosion of the waste package.

In terms of things like fracture flow, the NRC has been following this issue of fracture flow versus matrix for, what, 25 years, some period of time since the '80s, and it's been an issue and a concern of ours. So to us this type of information is extremely useful in terms of helping us better probe

what the Department is doing. And I think that unless Tim has something to say, that's probably all we'll say at this point, but it's certainly provides a useful new insights or reaffirmation of insights that we've been following up on.

DR. GARRICK: Okay. The workshop is not concluded, we're not concluding it until later today, but some people are going to have to leave. And for those I want to thank them attending and participating. I think it's been an outstanding exchange, and I would like to see us be able to somehow find a forum, as Ron suggested, where we can extend some of these discussions to where the inhibitions disappear, not to the point where we do physical damage to each other but at least to the point where we can really vent the opinions and the comments.

So with that, I think, as I say, we thank those who are not going to be able to rejoin us after lunch, a late lunch indeed, but we will now adjourn until, what is it, 3:15. Thank you.

(Whereupon, the foregoing matter went off the record at 2:01 p.m. and went back on the record at 3:38 p.m.)

CHAIRMAN HORNBERGER: It's 3:15 so we have

to reconvene, please. Okay. So we're ready to go, and, John, this is I think the final session. Go ahead.

DR. GARRICK: All right. Before we get into the Committee's comments, I think Andy Campbell has indicated to me that as a result of some discussion that took place about the agreements and what's being done therein have some information to add to that topic. Go ahead, Andy.

DR. CAMPBELL: Okay. Just for the record, I'm Andy Campbell, Section Chief with the PA Section in the Division of Waste Management. I mentioned in my talk yesterday that there was a series of technical exchanges over a period of two or three years that dealt with key technical issues. Among those were evolution of the near field environment, container life and source term and total system performance assessment. Looking at those agreements with Dave Esh during lunch, we identified at least 30 of those agreements that deal specifically with the kinds of issues that have been raised in the workshop. And during those technical exchanges we not only had specific discipline staffed there, attending and running those tech exchanges, but also PA staff was attending those. So there was a high degree of

integration among the different disciplines.

A lot of those deal with, for example, brine chemistry and testing on the surfaces of the waste package, thermal, hydrological and chemical, coupled processes, uncertainties, propagating the uncertainties in the geochemical models, the brine chemistry and chemical divide phenomena, which Joe Payer mentioned, and the importance of very small differences in water chemistry resulting in probably significant differences in the chemistry of the brine that might end up on the waste package, issues about the range of chemistry of water dripping on the drip shield or the waste package itself, why sodium nitrate may or may not be conservative when it's considered the main deliquescent salt, looking at mixtures of salts, uncertainties in the waste package and drip shield projections in terms of performance and a whole series of other issues dealing with the corrosion testing of the waste packages and the long-term performance of the waste packages, validation of a couple of processes, kinetics, dust, the impacts of dust on the waste package, support for the model and the validation of the model.

So that gives you an idea of these are all agreements that the NRC and DOE has agreed to provide

us with information on these. I haven't read them verbatim out of the agreement, but those are available publicly. And I won't give you all the numbers for the agreements because they won't mean anything to you, but, again, the information that we're looking for and that was discussed here, there's a high degree of alignment between those types of things. So that's all I had to add to the record.

DR. BULLEN: Mr. Chairman, quick question.

DR. GARRICK: Yes.

DR. BULLEN: Andy, is it your understanding that all of those KTIs will be closed before license application?

DR. CAMPBELL: Almost all of these are rated high in our estimation of importance to risk. That means they need to be addressed by DOE prior to license application. That doesn't mean that every single item will be completed; however, there's a -- these are very significant to our ability to review the license application. That's why they're rated high.

DR. BULLEN: I understand. Maybe I'll reword it. Prior t issuing the license do you think that the commissioners will have to have all of these issues closed?

DR. CAMPBELL: If through the process of doing our further analysis it is determined that a particular issue is necessary to provide reasonable expectation of performance, then the answer would be year.

DR. BULLEN: Very good answer for answering for the commissioners. That was great.

(Laughter.)

That was a good answer.

DR. GARRICK: You're learning quickly up there.

(Laughter.)

PARTICIPANT: He can be trained.

DR. GARRICK: All right. What I'd like to do now is do what we did with the panel earlier do for the Committee, and I'd also like to continue the practice of random selection, except out of respect for the Chairman I'll think I'll ask him for his comments first.

CHAIRMAN HORNBERGER: Thanks, John. I guess, first of all, let me say I really appreciated the input from everyone who participated in this working group. I thought it was a stimulating day today. Certainly, we got a lot of information.

I'll cut right to some summary

observations. I like the comments that Ron started off with this morning in the sense that a lot of times we hear some of the issues, shall I say, about what we don't know, and we can go around and around and around and hear issue after issue after issue, and what we really need is to have the kind of things that Andy just described, and these are the technical exchanges where in fact the information gets conveyed.

I will say the ACNW had a look at the issue resolution process, and the members did attend several of these meetings, and I, for one, was very impressed with how the technical exchanges between the Department and the NRC went. I will also say that in the -- and I think that everybody knows this who's here -- talking about the waste package. The people in the NRC and the people at the Center for Nuclear Waste Regulatory Analysis are outstanding, they are really good people. They really, I believe, understand the issues and would have appreciated all of the complexities that were described, and I really believe are giving the NRC staff, the Performance Assessment staff very good insights into how to treat these technical issues.

Having said that, I think I sort of exposed my bias by my overstatement to Rod Ewing. I

always like to make overstatements to elicit a reaction. But I do worry about the balance, and I think that we do have to have a balance, we do have to come to some kind of agreement, even if it's an agreement to disagree on some things, an agreement on how much science is needed versus how much we can rely on some kind of an analysis that bounds the problem. And I believe, I think like a lot of others who have looked at this program for years, that we are not going to have complete understanding of the natural system, and we're probably not going to have complete understanding of the engineered system either or nearly complete understanding of the engineered system either.

And we somehow have to find a way to balance the need for science and understanding with a way to accept how the Department would demonstrate a reasonable expectation. And that's, I think, the tricky bit that we've been working with here on performance assessment, and I think what the ACNW has urged to have as much realism as possible in these performance assessments and also to stress that we need to gain greater transparency in some of the investigations, so that we come to understand the problems better.

So I don't have any solutions, but I thought that it was informative what we heard, and I think that we still know some of the things that need to be done, we just need to keep on top of having them done.

DR. GARRICK: Thank you. Ray, out of respect for this being your last opportunity to --

DR. WYMER: I thought you were going to say something about age.

(Laughter.)

DR. GARRICK: Me say something about age? That doesn't get you anywhere.

DR. WYMER: That's true. I defer age-wise to my senior. I have just a couple of observations. One is there was almost the presumption in some of the things I heard, especially this afternoon, that we go into these performance analyses and assessments as though we were newly born, that we don't know anything. And in fact we know a great deal, and there's a great deal of information, a great deal of knowledge, and you don't really need to do everything ab initio in this world, there are starting points. And we can build on that without having to go back to ground zero.

More specifically, I think that what has

come out, what's emerged, is something I've been telling these people on this Committee to the point where I sort of have to duck every time I say it, but the whole thing is chemistry.

DR. GARRICK: Let me write that down.

(Laughter.)

DR. WYMER: I don't really think that we do understand much of the chemistry as much as we should with respect to corrosion, although we know more about corrosion than some of the other parts of the chemistry. We don't know as much about the solubilization of the waste form and of the various species that are going to be formed and the solid phases that will determine solubility and thereby determine source term. We don't know as much of that as we would like to think we know in some of the analyses that are done. But on the other hand I don't know how much of that we need to know, and that's where the great uncertainty comes in. Just how much is enough?

And I think that we never will quite answer that question, and in the final analysis I think whether or not this license is granted will come down to a judgment call on the part of the people who are making the final decisions as to whether or not

they believe that the information, however complete or incomplete it may be, whether that is adequate to go ahead and license the repository. So everything we do and all of the deliberations we make and all of the refinements we seek and all the uncertainties we have are going to finally fall on the heads of a few people who are going to have to make these judgments, and they will be very tough judgment calls indeed.

DR. GARRICK: Okay. Milt?

DR. LEVENSON: Well, I think it's safe to say I've been involved in this longer than anybody. At the end of this year it will be 60 years that I've been involved in nuclear energy. I've also been accused of looking at things at about 90 degrees to the way everybody else does, so I may disagree with Ray and some others. In those 60 years, I've made quite a number of major decisions involved engineering facilities, designs, projects. I don't think ever did we have all of the information we would have liked to have had. The real world of getting things done never gives you the privilege of having all of the data and the information.

Some people think that there should be no uncertainties and there should be no risks. That's a different world than the one in which we live. My

view of the TSPA or the TSP or anything equivalent is that's not a way to calculate quantitative values for anything. It's an extremely important valuable tool to get insights, and the decisions have to be made by responsible people taking those insights and combining them with everything else we know, that we just don't have the capability to really model the entire physical world.

I mean if we take something simple like one of the talks we had this morning, there's a good chance that in the time periods that are of interest the waste packages are going to be covered with very, very thick films of rock dust and so forth. I'm not sure how you or if you can maintain a highly acid thing on a few drops evaporating in the middle of that.

I'm not saying yes or no, all I'm saying is that the systems are so complex that -- and I'm advocating, I'm a strong advocate for things like TSPA and TSP for doing different evaluations, doing different studies, not trying to decide what is exactly the right module or code but just changing it really helps provide insights. And so for this meeting and this workshop I really think it's an important ongoing effort, but we shouldn't lose sight

of the fact that it's really just a tool, not a way to get quantitative answers.

DR. GARRICK: Thank you. Rookie, the rookie of the Committee, Mike Ryan.

DR. RYAN: Thank you, John. I refrained from making any comments about age.

(Laughter.)

First, I'd like to thank all the presenters and the panel members for these couple of days. It's been very informative I think for everybody, certainly for me. It's been a little bit like drinking from a four-inch fire hose on geochemistry and geology and some of the engineering aspects, but that's okay. I'd also like to take an aside and thank Ray Wymer for his mentorship and collegiality on the ACNW. He's been a mentor of mine for quite a long time and I appreciate his counseling and his leadership on this Committee, and his career, his body of work is formidable for those that know about him.

And as I thought about all the presentations today, I took note of a couple of figures in a couple of the later presentations. One was John Kessler's graph on his base case dose normal release scenario, and David Esh's curve where he

showed some spent fuel dissolution model sensitivity analysis. And what caught the health physicist's attention is the y-axis. The flat part that goes over the range of around thousands of years is ten to the minus three millirem per year. That's ten minutes of cosmic ray exposure as we sit here in this room. Ten minutes of cosmic ray exposure. So it's a very small fraction of a part of background.

Now, I don't say that to say we should trivialize any aspect of all of the science that was discussed in the last two days; in fact, I applaud the science. But I think that we can be informed by perspective, by the term that John used of margin and then trend analysis, some of the things that Milt mentioned in terms of insights, and we can be informed by that. And bouncing off lots of things against what does that do to the margin, what does that do to our measure of performance against the dose standard I think is something we have to visit regularly in the process.

To that end, I think even though it's prescriptive in regulation on the biosphere part, I think we should examine that for its conservatism or lack thereof. Dan Bullen mentioned about the 3,000 acre feet, I mentioned about dose conversion factors,

so obviously I think that's a fruitful area to think about how the conservatisms or perhaps non-conservatisms need attention. Not that we would calculate it or present it differently from a license application standpoint because of the requirement, but that it would better inform our thinking and our knowledge in terms of the dose calculation.

So, ultimately, and, again, I come at this from the health physics point of view, the radiation protection aspects of it, we're looking at what do all of these things mean in terms of dose. Several times, I think, several of the panel members touched on this idea of what does it mean in terms of impact, and we've asked the question what does it mean in terms of performance. Well, ultimately, that rolls out to the dose calculation. And when you're calculating annual doses that are equivalent to ten minutes of cosmic ray exposure in the lowest exposure area of the U.S., that's something to consider. I think that's an important margin to recognize. I don't offer it as a value judgment that everything is fine, just the opposite. I think we're on the right track of intellectually examining these questions and moving forward with that rigorous and vigorous examination from all points of view. So thanks very much for your

attention.

DR. GARRICK: Thank you. Are there any comments from the staff, the ACNW staff, that they'd like to make at this point that would dovetail in? Neil, do you have anything to add to what the Committee members have said?

MR. COLEMAN: Neil Coleman, ACNW staff. Just one thing that -- fracture flow was discussed at length or the expression was used. There are parts of the flow paths that are not fracture flow. The farthest extent in the valley fill alluvium and the Nye County wells have been very important in identifying how much of that there is. Also, Calico Hills non-welded vitric unit has sections that are porous flow. These are very important in the flow system and as far as potential retardation.

DR. GARRICK: All right. I'm looking at the agenda and it's a strange agenda from here on in that we are supposed to be together for a little while and then have a break, and I'm just trying to figure out how's the best way to tie all these things together. John Larkins, did you want to make any comments while the Committee and staff are reacting?

DR. LARSON: Yes. I thought there were some interesting concepts that were raised that

probably need to be explored further, I think, as the Committee decides on what type of advice to provide the Commission staff on the subject.

DR. GARRICK: I should say that John Larkins is the Executive Director of the Advisory Committees, the Advisory Committee on Reactory Safeguards and the Advisory Committee on Nuclear Waste.

DR. LARSON: Thank you. The comment was made on the use of margins where there's large uncertainties or information may be missing, and the staff has always used margins in reactor licensing when we knew what the -- had a good feel for what the margin is. Here in some of these I'm not sure we really know what the margins are and what's acceptable and what's not acceptable. Probably it needs to be given some thought.

And the same thing I think when you think about the uncertainty what criteria do you use to judge the uncertainty? And when is it acceptable and not acceptable? So some type of looking at maybe acceptance criteria in light of large uncertainty is something that needs some further thought or discussion. Those are some things that sort of stuck in my mind.

DR. GARRICK: Yes. Okay. I'd like to just make a couple of comments. They don't -- I think most of the comments that have been made by the expert panels and by the Committee have pretty well covered this spectrum of things that have come out of the couple of days. But the issues that I have been especially interested in, of course, are whether or not the notion of a risk-based approach or risk-informed approach to something like a natural setting was a feasible thing to do. It has always been something of great challenge. You'll recall me mentioning the first day that this diagram that I used kind of grew out of a discussion I had with Norm Rassmussen at least ten years ago when we were in my company debating the boundaries or the extent to which the risk assessment thought process could be applied. And Norm was reasonably skeptical at the time about its application with respect to the waste business. And for those of you who don't know, and I doubt that that's anybody, Norm Rassmussen, of course, was one of the discoverers, co-discoverers of the whole concept of probablistic risk assessment and led a famous reactor safety study that was performed in the mid-'70s.

I am of the opinion that the fundamental

thought processes apply to any kind of issue and that particularly if you take the approach of a scenario-based approach as kind of the cornerstone of what a risk assessment is, namely a structured set of scenarios that answer the question of what can go wrong with your system.

The issue of uncertainty, I think, was kind of brought into focus quite well by John Kessler's remarks as to what it provides in the way of opportunity and flexibility to convey the performance of a system. I think this is a point that's often missed by people who are less than confident about the use of the risk sciences. The idea of being able to account for the absence of information or the ineffectiveness, if you wish, of a model in the analysis is very fundamental and very important to being able to anchor the analysis to the supporting evidence.

We often -- I remember many years I was teaching a reliability course at UCLA in the -- a short course for about 20 years, and I would start the course with a display on the blackboard of two sets of data. The one set of data was a set of point values about certain critical parameters, and the second set of data was the distribution functions on those same

parameters. And I would challenge the students to tell me which of these two presentations was the most quantitative. And I was always shocked by how many of them said the point values. And I think that whole concept of quantitative analysis as it relates to risk is anchored to the way in which we attempt to bring uncertainty into the process.

And that's why it's kind of an oxymoron to me to hear the term, "conservative risk assessment." It doesn't make sense. It isn't why the discipline was invented, the point being that the risk assessment really ought to be the very best shot of the experts as to what the risk is and let the regulators and the decision-makers decide how much conservatism and how much safety, how much margin they want to add to that. But without that, they have no high confidence place to start.

And so that's one kind of characteristic of this that I think is extremely important, and we have as a Committee have been trying to make that point and I think with some success with the NRC and the staff, again, not to just suggest that the values that are calculated from the risk assessment are to be the values that serve as the basis for regulation, but that they serve as the basis of the best information

available as to what the safety case really is and therefore a baseline in reference from which to use it in a decision-making sense.

You hear about other engineering projects and what have you, and you often hear the argument that, well, we didn't have that problem in that project. And the reason very often that problem that didn't exist is that these uncertainties were ignored. And so here we have a transition in the engineering community that I think is critically important of no longer dealing in terms of just safety margins or performance margins but genuinely attempting to quantify what we mean by that. And as we do this, I think a number of concepts will begin to take on a much more scientific basis, including the much discussed basic regulatory tenet known as defense-in-depth. I think we've advanced to a point where we don't have to have the concept as much of a mystery as perhaps it has been in the past and that we can begin to express defense-in-depth in more quantitative terms. Generally, defense-in-depth, or at least one motivation for defense-in-depth, has always been to account for the uncertainties. And as we learn how to account for them and embrace them and put them in our fundamental models and propagate them in some sort of

systematic and transparent way, I think we're in a position to be much more clear on what we are getting from our so-called defense-in-depth.

So the one that that I was pleased to hear in the various discussions was a growing interest in bringing uncertainty, recognizing that there's many kinds of uncertainty, and the one that we probably know the least about is the modeling uncertainty, but recognizing that we have a long ways to go before we can feel 100 percent confident that we can count on the results for decision-making.

I agree with the comments that have been made that the most important thing here, and I was pleased to hear Rod Ewing admit that the first thing he would do is a performance assessment, although I have to see it to believe it. But I also tend to agree with him that you shouldn't necessarily be overwhelmed with the results, that you need to be guided somewhat by them and you need to have them as a basis for helping ferret out some of the issues and the problems, and it is a continuing process, but it is not the end itself.

As far as the discussions about -- I found the discussion, and this has been an ongoing discussion, not only with respect to Yucca Mountain

but with respect to the Waste Isolation Pilot Plant involving some of the same people, the ongoing discussion of research or science versus engineering and adequate science. And I think that, again, one of the most important vehicles for measuring adequacy is being able to quantify the uncertainties. I think that's a healthy ongoing debate and should continue. But I remember once being a witness at a public hearing and debating with an intervenor and finally the judge, out of frustration, said, "Look, we have to make a decision here. And we can't just continue to debate this issue." And I think that's the context we sometimes don't give enough emphasis to when we're doing these kinds of analysis and models, casting it in a forum that makes it possible to make a decision.

The reason that the performance assessments have a long ways to go before they can be risk models in the sense of probablistic risk assessments, particularly the large scope probablistic risk assessments that were performed in the '80s and early '90s, is that I still think that the performance assessments are principally compliance assessments much more than they are risk assessments. And while there's progress that has been made by such activities as the elimination of sub-system requirements, there's

still a lot of prescriptive features of the license requirements that obscure, if you wish, at least the performance assessments that's performed in the name of licensing, a lot of things that obscure and stand in the way of really developing a somewhat unconstrained risk assessment of a geologic repository. So we're a long ways away probably from having an example of a risk assessment of a geologic repository that could be compared with the risk assessments that have been performed on nuclear power plants.

Now, the risk assessments that were performed particularly in the '80s and '90s on nuclear power plants were unconstrained in the sense that they were not driven for the purpose of license application. They were driven only for the purpose of answering the question of what was the risk of the individual plants. And I think that's the big difference between the advancements that were made there and the advancements that are being made in the waste field as it relates to the progress of the risk assessment thought process as applied to geologic repositories.

But I think in certain specific areas we are making quite a bit of progress, and that was one

of the reasons why we were very interested in focusing on one aspect of the PA perhaps more than the other, namely the source term on the basis that that lends itself as much as maybe anything to applying these ideas and principles. And as I say, I think we've made progress, but we certainly are nowhere near where we need to be to really test the concept in terms of whether or not you can build that kind of a model on something like a repository. I'm convinced you can but I'm also convinced we haven't done that yet.

Okay. Now, let's see, according to this agenda, we're supposed to have a break at 4;15. Can we go directly to our next agenda item and move into comments, public comments? All right. Let's do that. Let's turn the meeting over to anybody who wishes to make a comment now, particularly the public.

DR. ELZEFTAWY: Hi. In the same of time, since I'm going to leave in about two minutes, I would like to just make one -- is that thing on. I can speak loud. Again, I'm Atef Elzeftawy, I'm with the Las Vegas Paiute Tribe. This comment --

DR. GARRICK: Is that working? Excuse me a minute. I want to make --

COURT REPORTER: Yes. Stand a little closer to it but it is on. Don't get too close to the

mic, though, it blocks.

DR. ELZEFTAWY: Well, I speak loud anyway, so probably you guys can hear me. I just wanted to say two things. One, a word of thanks. And the second is just a little small story, food for thought as you guys go home. I wanted to tell on behalf of the Chairwoman and the Council and the people of Las Vegas Paiute Tribe, 120 of them, thank you very much for inviting us, we appreciate the invitation. So for the Chairman and for the Committee and for John Larkins, I think, who signed the letter, for Neil Coleman, and the best I've ever done with NRC is to tell Hub Miller that "That's a good guy, hire him." So good for you. Good for you.

You guys have a lot of good brains, a lot of good discussion. I heard a whole lot of good things from the Department of Energy, from NRC and all that, and I think, in general, you are on the right track. And one of the gentleman, I think the Chairman, made a comment, and the other person too, John -- I guess I can recall the names now -- we've got to make a decision. And that's really what scares me a little bit in terms of the performance assessment.

And here's a little story. Oppenheimer --

you probably know the names -- Oppenheimer, Edward Teller, Alvarez and Server and somebody else in June '42 they sat down together in a room in the County Hall in Berkeley two days and they wanted to find out how much it's going to take in terms of uranium to make the bomb. It's public knowledge now. They came up with -- after all their discussion between all of them, it's physics not chemistry, and they came up with 100 kilograms. Now you rest of the rest of the story. After Los Alamos and thinking done, with all the work they have done, with all the billion dollars they spend, that 100 kilograms went down to ten. That's a public comment also. So at least they looked at the uncertainty in their theoretical work, call it performance assessment, and then finally the way they did it. So here's food for thought.

The other thing is the quantum mechanics theory. Albert Einstein passed way not believing in the quantum mechanics theory. The late Feynman with his Nobel Prize winning said this: We don't know what it is. We don't understand it in all details. But we know one thing: It works. And if we can come up with performance assessment models that it works, then I think that helps the decision-making process. And with that, thank you again for everything. And

hopefully you will come to Las Vegas, so come and I don't promise any -- but come and don't waste your money there.

(Laughter.)

DR. GARRICK: Thank you.

DR. ELZEFTAWY: Best wishes to you. Thank you again.

DR. GARRICK: Thank you.

MS. TREICHEL: Judy Treichel, Nevada Nuclear Waste Transport. I'll give you my fortune. It says you will be rewarded for being good listeners. I found it really interesting today when the conversation got around to the fact that everybody or people who have been doing this for a long time could predict what they were going to hear from whoever was speaking. That certainly goes for me. You know exactly what's going to come out of my mouth.

But it was refreshing to hear the, as you call it, knock down, drag out that really didn't last long enough. Yes, that stuff has to be hashed over and it has to be hashed over hard, but I don't know that you can get people to change their mind. Because with everything that's going on here, you've got a terrible glitch, and you've got a glitch for being able to do a good probablistic risk assessment which

I know is where you live, John, but because that's generally done with something that everybody wants. And if there's a risk involved, people decide they want to take the risk because they want the result. And you don't have that in Nevada.

And when it's just very sort of cavalierly thrown out, well, who drinks all their water out of the same tap, well, that answer is easy. A family out in Amargosa Valley that farms. That's where they get the water, that's where they pull it out, and, Dan, they don't have to just eat tomatoes and cucumbers, they can eat pistachios, they can drink the milk from the cow who drinks out of the same tap, and they can do all sorts of stuff or they can go down the street to get something out of essentially the same tap or one of the same. So you're dealing with people who will be assigned a risk by someone else.

And I don't think the argument stops and starts with whether or not you realized it was fracture flow or it was through the matrix or whether or not you realized the chemistry of the water or -- you need to almost ratchet back. And one of my big problems is I'm never talking to the right audience. But nobody ever really decided what the repository does, why it's there, what it's for. And nobody can

really give you that answer. That's sort of in the eye of beholder as well, because according to the Vice President in his task force, it's essential for a resurgence of nuclear power. Want to build a lot more nuclear power plants, so we're going to have to have Yucca Mountain, so we have to be able to say that we can do this. For somebody else, it's something else.

There's continual arguments about what this thing is for, why we're doing it, how much waste it's going to have to hold, who benefits, who takes the risk, and I would like to see some of those things decided before anything else. But my real fear is that when you have discussions like this and we watch who the presenters are and where the biases are and who's coming out with what, that I'm terribly worried that NRC is sort of pushing to make this thing okay.

I really think NRC would like to have Yucca Mountain, and there's got to be compromises made, there have to be -- uncertainties have to be acknowledged and then either accepted or not, and I'm worried that people who don't have to live with this are going to be way more eager to have uncertainty or to feel that it can be accepted then other people. And I would just love to be able to leave here thinking it was totally fair but I don't so far.

Thank you.

DR. GARRICK: Thank you. Roger?

DR. STAEHLE: I want to say a few things. I'll tell you first about a consulting problem I'm working on where a helicopter set of lights failed and killed three people. And I looked into this and discovered that the engineers who organized the device that holds the blades on had done a fatigue test and had concluded that the rotor had infinite life. And so when I looked into it, I discovered that the reason they concluded infinite life was the fact they ran these experiments in laboratory air. You know the rest of it, that the thing didn't fail in laboratory air, it failed in Houston industrially polluted air.

The second experience I wanted to mention was the fact that I looked at the first BWR pipe failure in 1967 and having looked at this, and I was a young guy then, I said, you know, this is going to happen again. Someone said, "Don't worry about, Roger, that was bad heat."

And so what I'd like to say here specifically is that it seems to me that we have a problem that can be identified as being very complex. We have complexities in the surface chemistry, we have complexities in the Mountain, we have complexities in

temperature, and there's no way we can solve that problem. I mean with all the mathematics we can all think of, we can't write a set of equations and modeling that will solve that with any kind of exactitude. And so what we have to do is figure out how we can approach this in some way that makes sense.

And it seems to me that there's a need to approach a -- have a bounding approach where we can say that at least we can bound our problem with certain kinds of quantifiable ideas. And so step one is to figure out what it is we're going to bound, and that's a discussion of we're going to bound temperature, we're going to bound the availability of water, we're going to bound how long we have to predict for, and we're going to bound whether or not the site is going to be air-cooled or not air-cooled.

And it seems to me that we need to kind of develop, first of all, what are the set of things we have to bound in order to make predictions? The second thing we need to do then is to approach these bounding situations and say what is a reasonable worst case, not what is the worst case, but what's an intelligent, reasonable worst case in each of these bounding categories? And then with that set of problems, we can, like the work the helicopter people

should have done, we can then begin to bound the environments and the temperatures and the question of the availability of water.

So I think that we need to develop something like that or whatever can we in fact work with, because, as Joe Payer is struggling with this, and I know that others have struggled with it, that, well, they can't figure out how this is going to work, how the environment's going to react and give us plus or minus one or how the environment is going to -- how much water do we need? Well, it's a struggle, because we don't know, and so we've got to recognize we don't know and step back and say we've got to bound that some way.

So I would like to see that process organized somehow that we develop the categories of bounding, develop a set of worst cases for bounding and then see if we can't make progress with modeling on that kind of a basis.

DR. GARRICK: Thank you. There he is, Steve. I've been wondering where you were. I can't see you behind that post.

MR. FRISHMAN: I've been wondering where you were. I can't see you in front of the post.

(Laughter.)

Steve Frishman, State of Nevada. Just a couple of quick observations that I think are maybe important for you to at least notice, maybe not the same way I did, but at least notice. One is there's an interesting line on one of John Kessler's viewgraphs, and that's on Page 7 at the bottom. He says, "Pessimism can be replaced with more realism at a time when more confidence is required, perhaps at a later stage of repository development." Well, I think that that's fine in the sense that I noticed a few people seemed to agree with that in one way or another when he was saying it and when it came up, at least in part, in discussion later.

But I think you also have to remember that there's no room for this concept under the current regulation. And that is that when more confidence is required, the way the regulation reads the confidence that is required is to support the decision about whether a license or whether a construction authorization is issued or not. This is not a staged program of building confidence to the point -- and I've been through this with you collectively a number of times, and it needs to be remembered, because this type of talk is becoming sort of more built into the system once again as the concept of staging is

emerging.

Well, the NRC's rule as it stands today is not a staged rule under the concept of you build and build and build. And I think it's necessary to understand once again that the confidence that is necessary is the confidence that can be elicited through demonstration at the time a construction authorization is issued, if it is to be issued. So that's a point that I think you can't forget, even in your zeal to say that through time we will know more and the implication being, and in fact, actually, it was stated explicitly yesterday, that through time we can expect our understanding to be better and our confidence to go up. Well, that's not necessarily true. Through time we can expect that we will know more, but what we might know in the addition of more is that we have less confidence rather than more confidence. It's just as possible.

Now, just one other point and that's that at least two of the members today just in the last few minutes pointed out that TSPA is, yes, a very important component in the whole effort that is underway right now, but it must be remembered that mainly TSPA is a very useful tool. And the purpose and use of that tool, described differently but all

comes out to about the same, a useful tool for understanding not only what you know but more importantly what you don't know.

The thing you have to remember, once again, in your considerations around this table and what advice you may pass on to the Commission, is that the licensing rule doesn't have room for that either. The licensing rule says that the outcome of the performance assessment is the statement of compliance or not. And you in fact somewhat endorsed that idea in the past. So, yes, this discussion is wonderful and I think it's been a very good discussion to have had, it should have been had a very long time ago by a much broader base of people with a much broader scope, but, yes, it's a good discussion too far afield at this point where all of a sudden you're sort of giving way to the idea that the performance assessment somehow can be compromised by some other measure in a decision of reasonable assurance or reasonable expectation or whatever.

The way the rule, whether you like it or not, whether I like it or not, and we're trying to do something about that, by the way, what the rule says right now is that the performance assessment is the statement of compliance or not. So if you want to do

something about that, there may be others way to do it, but I warn don't encourage that the concept of reasonable expectation begin building in what I circle back to from John Kessler, the idea that when more confidence is needed. Well, that's just not the way the structure is right now.

I don't like the structure the way it is, and I've told you about that many times, and, as I said, we're trying to do something to change that. I don't know if we'll be successful. But at this late date I don't think that it is wise to reconsider the concept of performance assessment that will only build some new level of vagueness into what people might think is an acceptable way to make a decision about a construction authorization.

So, essentially, you guys participated in building the regulatory bed that we're all in right now, for good or for bad, and I know that through past things that it is possible if you begin talking about how performance assessment is a tool, which we have all been saying for years anyway, that can get translated into a decision for reasonable expectation or reasonable assurance or whatever it is called at whatever time it is used, that can lead to another level of subjectivity some new great idea once again

that is all working very much to the detriment of safety as far as I see it because each one, in my mind, is a new way to compromise on the applicant's original responsibility, which is to demonstrate safety on the front end and demonstrate it to the extent that it has a scientific basis to it. So that's my warning for this week. Thank you.

DR. GARRICK: Thanks, Steve. One of the things I think I mentioned at the beginning and somewhere along the way is that the Committee does its best to address the technical issues and is not the body that makes the decision about whether or not a license is in compliance. We are not license experts, we're not regulation experts. We're here to complement the regulatory process but be focused on what is going on from a technical standpoint. So there is that point to make.

And in that context, the idea that some of the things that have been said about confidence and uncertainty are clearly appropriate. I agree with you that in the end the decisions have to be made on the basis of compliance with the regulations and the legal structure that is involved.

Okay. Are there any other comments? I think what I'd like to do -- I think people are kind

of wrung out. I'd like to wrap this up. We have systematically been losing our panel, and you've not -- you don't need to hear anymore speeches from me especially. So let me just in about two minutes just throw out a few thoughts that are in the nature of sound bites, if you wish, on the meeting and bring the working group session to close. And then we'll take our break and the Committee will reconvene and get into our report writing discussion session, et cetera, et cetera.

We've heard a lot about the issues that we identified as themes for the meeting, particularly the issue of realism, and we've given quite a bit of discussion about why we're interested in realism, and I don't think we need to build on that anymore, and I think that to a large extent the goals of the workshop or the working group session have been fulfilled in that regard.

The DOE staff identified degradation modes of waste packages as a major source of modeling uncertainty. We pursued this issue of where are the principal sources of uncertainty, et cetera. NRC identified source term release as a major source of uncertainty, and of course we've known for a long time that this is an area of considerable concern to the

stakeholders.

We did our usual campaign for simple models and the usefulness thereof, and we were cautioned in this discussion about the need for balance between simplicity and the drive to add complexity to our models. We heard lots of discussion of areas of considerable disagreement, such as disagreements about the potential for extreme corrosive environments to exist on the surfaces of drip shields and waste packages. We heard considerable discussion about the assumption that all soluble radionuclides will be captured at the 18 kilometer boundary and some of the opinions within the group about the extreme conservatism involved there. We discussed the assumption that juvenile failures of waste packages will be extremely rare, and they should be examined based on the non-uniformity of welding and annealing skill levels in the industries that do this sort of thing, such as the steel industry.

We had some very interesting discussion about the waste package environment and such matters as how solubility depends on the mineral phases present, and the point was made very clearly that if the assumed phases are wrong, the solubilities will also be wrong. We also had a good discussion on the

need for field scale data on radionuclide retardation in particular and a number of other issues that I think we've adequately discussed in this last half an hour.

So unless there's somebody that wishes to make some final remarks, I'd like to thank everybody that was here and who participated. I thought the comments that were made were made freely and openly, and I agree with Judy and others that we need to have more time on some of these issues to more appropriately address them in an increasingly uninhibited fashion. And we'll have to figure out what's the best forum.

We also want to thank the contribution made from San Antonio and the staff from the Center and everybody else for attending and showing the patience to listen to a lot of discussion and deliberation on a very complex issue but an issue that's an extremely important to our nation. And let's hope that we can continue to ferret out the issues in a manner that indeed at the appropriate time will take the form of a useful basis for decision-making.

Neil, do you have any closing? I want to thank Neil Coleman again for his assistance in putting

together the working group session as well as Mike and anybody else that was involved, because these are difficult things to arrange considering the level of people that are involved and all of the other things that are going on at this time. So have I left -- Andy?

DR. CAMPBELL: I just wanted to thank the members of my staff, Chris Grossman and Dave Esh and everybody else, and the folks at the Center, for the tremendous support that they've provided in giving you information that you needed.

DR. GARRICK: Yes. Yes. Thank you very much. So unless there are people wanting to say more, I'm going to turn the meeting back over to our Chairman, and we'll take our break now, I think, and then we'll come back in for our report writing session.

CHAIRMAN HORNBERGER: Okay. Thank you, John. I don't see any other hands up, so we will take a break until -- how long a break do we want, Milt, 20 minutes, 15 minutes? Five o'clock? Fifteen-minute break.

(Whereupon, at 4:41 p.m., the ACNW meeting was concluded.)