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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE AND MATERIALS

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185TH MEETING

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VOLUME I

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MONDAY,

DECEMBER 17, 2007

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Dr. Michael T. Ryan, Chairman, presiding.

MEMBERS PRESENT:

MICHAEL T. RYAN, Chair
ALLEN G. CROFF, Vice Chair
JAMES H. CLARKE, Member
WILLIAM J. HINZE, Member
RUTH F. WEINER, Member

NRC STAFF PRESENT:

CHRISTOPHER BROWN

LATIF HAMDAN

ANTONIO DIAS

NEIL COLEMAN

DEREK WIDMAYER

MYSORE NATARAJA

GARY COMFORT

MIKE FLIEGEL

DENNIS RATHBUN

BILL von TILL

ALSO PRESENT:

JOHN KEMENY

AMIT GHOSH (via telephone)

ROMAN KAZBAN (via telephone)

LUIS IBARRA (via telephone)

ASA CHOWDHURY (via telephone)

GOODLUCK OFOEGBU (via telephone)

CHARLES PENNINGTON

A G E N D A

P R O C E E D I N G S

(8:33 a.m.)

CHAIR RYAN: Okay. The meeting will come to order, please. This is the first day of the 185th Meeting of the Advisory Committee on Nuclear Waste and Materials. During today's meeting, the Committee will consider the following; the Electric Power Research Institute's report on drift degradation at Yucca Mountain, an update on NRC rulemaking on groundwater protection in in-situ leach Uranium mining facilities, and the NAC International views on transportation-aging disposal performance specifications. The Committee will also discuss ACNW letters and reports.

Neil Coleman is the Designated Federal Official for today's session. We have received no written comments or requests for time to make oral statements from members of the public regarding today's session. Should anyone wish to address the Committee, please make your wishes known to one of the Committee staff. It is requested that speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so they can be readily heard. It's also requested that if you have cell phones or pagers, that you kindly turn them off. Feedback forms are available at the back of the room for anyone who would like to provide us with his or her comments about the meeting.

I'd like to begin the meeting with a couple of items of current interest. First, Professor Hinze, one of our members, is in the Indianapolis Airport due to weather, desperately trying to make his way here, so we'll look forward to his arrival. In lieu of Bill being the Cognizant Member for this morning's session, Professor Clarke has agreed to step in for him and cover those presentations.

Ms. Barbara Jo White, who has been with the ACRS ACNW Office for about 40 years and is off to my left - Barbara Jo, good morning - is retiring on January 3rd, 2008. All these years she has provided outstanding administrative support to the members. She's always insured that members have a good place to stay when they attend ACNW meetings in town, or out of town. She has been exceptional in insuring that the Federal Register notice to the Subcommittee and Full Committee meetings have been issued consistent with FACA requirements. Her outstanding administrative support for the members, hard work, dedication, professional attitude in dealing with not only members and staff, but also with members of the public are very much appreciated by both Committees and the staff of the ACNW&M and the ACRS.

We thank you, Barbara Jo, and look forward to wishing you every success and pleasure that life has to offer in the days and years ahead. And it's always a comfort to know I can call Barbara Jo, and I won't be out in the cold, and I'll be where I'm supposed to be. So thank you very much, and we appreciate all your service. Thank you.

(Applause.)

CHAIR RYAN: With that, I'll turn the session over to Professor Clarke.

MEMBER CLARKE: Thank you, Dr. Ryan. Is our speaker present?

VICE CHAIR CROFF: He is.

(Off the record comments.)

MEMBER CLARKE: Okay. Our first speaker is Professor John Kemeny of the University of Arizona, who will speak to us on the Electric Power Research Institute report on drift degradation at Yucca Mountain. John, it's all your's. Thank you.

MR. KEMENY: All right. Well, thank you very much. I'm from the University of Arizona, and also I work for EPRI, and I want to thank Nick Apted from Monitor Scientific also involved with this, and John Kessler from EPRI.

Today I'm going to talk about the problem of thermal spalling due to the heating of the nuclear waste in the underground repository at Yucca Mountain. I thought first I'd just mention that EPRI recently has been involved in a number of rock mechanics studies, and this one being the most recent. But also in 2007, we've been involved in looking at some rock mechanics issues associated with expanding the capacity of Yucca Mountain. And then if you look, you'll see that last year we did some studies on the effect of multiple seismic events, which is something quite important, and also long-term performance. We actually tried to look as far as a million years to see what we thought would happen to the repository. And then back in 2005, we were also looking at seismicity and rock fall.

Okay. So I'll start sort of simple. What is rock spalling? It's, basically, a failure around a boundary of an excavation, and typically it could show up as some sort of bulging, as it does here. This is the test drift in Yucca Mountain, and you can see it sort of bulges here. But if there isn't any support, then the rock often will produce rock fall. And you can see here, this is from a paper, Rajmeny, and you can see this sort of initiation of some spalling, then some sort of minor amount of spalling. You can see a piece of rock on the ground. And then here you can see the drift is starting to get sort of oblong shape, because this quite severe rock spalling.

Okay. In terms of what causes rock spalling, basically, it's a high tangential stress combined with a low radial stress near the excavation boundary. I give this example from the Canadian URL, where basically they have very high horizontal stress, as high as 55 megaPascals, and so they end up with high tangential stresses on the boundary.

Now because the radial stress is essentially zero on the boundary, it's like a uniaxial test on the boundary. This is just an example of a uniaxial test that we might test in the lab, and you'll notice that you're getting these sort of -- these high angle fractures, what we call these splitting fractures. Anyway, so that's the idea that the spalling will initiate on the boundary, because that's where the rock strength is the lowest. And as you go in towards the boundary into the rock, then, of course, first of all, the tangential stress goes down. And secondly, you develop a radial stress, and so it becomes stronger. So, typically, the extent of the spalling is limited, as you can see here for the URL case.

In terms of what spalling depends on, it depends on many factors, maybe the most important of which is the in-situ stresses, the magnitude, and the orientation of the in-situ stresses. Of course, the opening, the shape of the opening, the size of the opening, and the orientation if it has a particular shape. Also, the excavation method for --

CHAIR RYAN: John, I'm sorry to interrupt you.

MR. KEMENY: Yes.

CHAIR RYAN: I forgot one opening item, and that's, is there anybody on the bridge line? Is there anybody on the phone bridge line? Okay. They'll announce when they come in. We'll get a beep. I just wanted to make sure we got that on the record. Thank you.

MR. KEMENY: So will people be asking me questions while I give the talk? Is that --

CHAIR RYAN: No, I just wanted -- I had the obligation to make sure our record reflected the bridge lines.

MR. KEMENY: I'd be happy to answer questions during the talk.

CHAIR RYAN: If folks want to break in,

I'm sure they will. Thanks.

MR. KEMENY: All right. Thanks.

All right. So, anyway, also the excavation method. TBM is probably the -- produces the most stable opening, so you probably see less spalling in that case, as opposed to fill and blast type. And, of course, the properties of the rocks, so the in-tact rock strength properties, as well as the joint properties are very important.

The underground environment can also have -- such as the temperature, the humidity, the rock saturation, these can also affect how much spalling you get. And the type and properties of the support. And I know this is an important issue, but support can reduce the amount of spalling. Typically, a lot of times, typical kinds of support will be rock bolt and shot-crete. A lot of times this will not affect the amount of spalling so much as it just supports the loose ground. It keeps it from falling. But in extreme cases where you have tunneling and very adverse conditions, actually, if you put enough support in, and you put it in soon enough, you can actually reduce the amount of spalling.

And then lastly, I talk about this thing called progressive spalling. And this is this issue that you get some spalling, and it sort of continues to -- rock fall continues to occur, and the spalling gets bigger and bigger. And this can occur, in particular, because of the time-dependent nature of the rock behavior. And, secondly, because of changing stress conditions. So, for instance, you might be driving drifts close by, and that caused a change in stress. Certainly, a seismic event would cause the change in stress, or the thermal loading that we're talking about at Yucca Mountain would also be a time-dependent effect, so we could see progressive spalling over tens or hundreds of thousands of years.

All right. Now I want to first talk about sort of the extreme cases. Now just in here is progressive spalling leading to total drift collapse, which basically occurs in very extreme conditions. It's certainly not the norm. And I list sort of two conditions where this could occur, and one is where the in-situ stresses are extremely high compared to the rock strength. And this is a graph from Everett Hoek, and he basically has a parameter down here which is the rock strength over the in-situ stress, and he's got different sort of categories. He's got a few support problems, minor, severe squeezing, very severe, extreme squeezing. And sort of the boundary between the minor and starting to get to severe sort of these spalling conditions would be at a value of .2, and that means the in-situ stress is five times the rock strength.

And if I go back to the previous figure, you can see that these are quite severe. Like here is a case where I have quite a high stress, 55 megaPascals, but this is a granite. The strength of this rock over 100 megaPascals, so this stress is only half of the rock strength, so I'm talking about when the stress is five times the rock strength. So for this rock, this would mean that for this kind of stress, it would have to be a rock that's very weak, on the order of 10 megaPascal; or, conversely, for rock with 100 megaPascal strength, it would have to be a stress of 500 megaPascal, so, again, these are very severe conditions, that we get this sort of severe squeezing condition.

And you can also talk about in terms of the tangential stress, so it's the max tangential stress on the boundary of about 10 times greater than the rock mass strength, then you could get this severe squeezing.

These are famous cases where they tried to put in tunnels. The TBM gets stuck, or a mile of drift may take five years, with all sorts of kinds of support, and grouting and things like that, so these are very extreme conditions.

The other condition where we sometimes see a total collapse of the drifts is the opposite of that. It's a very low stress condition in a weak rock mass. And this would typically be very shallow, so maybe only 50 feet below the ground surface or something like that, very weak rock mass, maybe the joints have no strength, and you can get a collapse all the way to surface. So this is typically the two conditions that we see, this sort of total collapse. And we're not expecting these kind of conditions at Yucca Mountain, and I'll demonstrate that in a second. I'll go to the next slide, and I'll show you that we're not really near this Condition 1, and not Condition 2, either, because Condition 2 would involve a much shallower excavation than the drifts in Yucca Mountain.

So I think what we really have at Yucca Mountain is what I call slight to moderate over-stress conditions. And what I mean by that is that the depth of the spalling is less than one radius away from the boundary. So, for instance, let's say this is an excavation, has a radius R, and then I might have some spalling around it, and if that spalling is less than or equal to the radius, then I'd call it slight to moderate over-stress conditions. And these are very common in underground excavation. We see this all the time, almost every underground excavation has some kind of failure. It may be slight, or it may be moderate, and it's very typical that we can stabilize these excavations with very standard types of rock support, like shot-crete or rock bolt. And if it gets more severe, we can use concrete liners, and so on.

And this is what I would expect at Yucca Mountain. Again, this is before I did any analysis, but just basically looking at the rock strength and the in-situ stresses, and the amount of stresses created by the thermal, this is what I would expect. So here is kind of the -- I guess we're going to focus today really on the non-lithophysal units. And I've done -- this is a similar categorization that DOE has done. They've separated their lithophysal into five types, five categories, the weakest being Category 1, and the strongest being Category 5. If you look at the strengths, the unconfined compressive strengths, they vary from let's say 10 for the weakest, up to let's say 30 for the strongest.

The other thing that's very important here is the Young's modulus. Again, all these results come from the DOE's drift degradation analysis report of 2004. Look at the Young's modulus in gigaPascals, you'll see that the Category 1 has very low Young's modulus, only 1.9. The Category 5 is very -- almost the same as the non-lith, with about 20 gigaPascal. And, of course, this has to do with the lithophysal porosity, which varies from 35 percent for the weakest, to maybe less than 7 percent for the strongest.

So what's going on at Yucca Mountain in terms of stresses? If we forget about the thermal, first of all.

CHAIR RYAN: I'm sorry. Is someone on the bridge line, please?

MR. CHOWDHURY: This is the Center.

CHAIR RYAN: Could you tell us who you are?

MR. CHOWDHURY: The Center.

CHAIR RYAN: Okay. And you are who, individually?

MR. CHOWDHURY: This is Asad.

CHAIR RYAN: Okay. Thank you. Anybody else with you?

MR. CHOWDHURY: I've got Roman, Luis, and

Amit.

CHAIR RYAN: Okay. We'll have to get those names from you maybe after the session. Thank you.

MR. KEMENY: All right. So, again, back to this Young's modulus. So if we take the case where we don't have any thermal, so just basically what's going on now, there's a few drifts at the repository level now, and they're being subjected to the in-situ stresses. So we're getting maximum tangential stresses that vary from 3 to 18, and the 18 is coming on the walls, and the 3 is coming from the roof, so the walls have more stress right now than the roof. And if you look at these, and you compare these numbers to these, you can see that already we should be seeing some kind of spalling on the drifts for these Category 1 and 2 lith, which is the case. There has been some documentation of some wall spalling already in the drifts. And that's consistent with the in-situ stresses that would cause these kind of stresses.

Now when we add the thermal, and the thermal is very tricky because the amount of thermal stress depends on Young's modulus. If the rock is stiffer, you will create -- you will generate much more thermal stress. So what happens is, the reason the range is so great is because for this low Young's modulus, it's going to generate very -- just a few megaPascals of thermal stress, so the thermal won't really make much difference at all.

On the other hand, for the strongest lithophysal units, the Young's modulus is 20, and it will result in as much as maybe 45 megaPascals of increased tangential stress. And so what that means is it will exceed this value, so we can expect when we heat up the drifts that we're going to see some thermal spalling in maybe the last three categories, probably not in these first two because the Young's modulus is so low. And this would be both in the wall and the roof. Maybe the roof actually has a higher -- -- ends up with a higher amount of stress than the walls.

Anyway, so if we compare these numbers to the previous, just to show you again, here I'm using this criterion that I get this extremely high stress when the tangential stress at the boundary is 10 times greater than the rock mass strength. Well, this never occurs for either the static stresses, or the thermal stresses. It would occur under seismic loading, but it does not occur under thermal.

As you can see, if I took a value of 30, I would be -- if I took 10 times that, I would need a max tangential stress of 300 megaPascals, or the maximum of about 50, and so on. So, again, just to reiterate, we're not in this sort of what I consider these extreme conditions that would lead to total drift collapse. We're in what I consider this slight to moderate over-stress condition.

All right. So now the issue that really has come up with regard to this is what happens if there's no rock support? Because it's true that even though this is very common in mining, and also tunneling, almost in every case, if there's going to be some over-stress, then they'll put some rock bolts in, or they'll put some shot-crete in.

Now in this case we have something different. The Yucca Mountain drifts is going to remain under thermal loading for thousands of years. There will be rock support that will be installed as part of pre-closure, but it will deteriorate over time. And this is kind of a heavy statement, because we don't know ----- as far as I know, there hasn't been a lot of work on exactly how long rock bolts will last. Typically, rock bolts, for instance, are made of sort of low-grade carbon steel. They deteriorate very fast. It doesn't mean you couldn't make a rock bolt that would last longer. But overall, DOE is not taking any credit for the rock support, so similarly, we basically assume that there's no rock support left at the time of the maximum thermal loading.

So without rock support, there's kind of two possible scenarios, and this is where really the crux of the whole thing comes down to. The one scenario is that rock reaches the peak stress, which there will be some spalling. And if it immediately fails and falls, so I get some failure, and it falls out, of course, that means that that stress has to redistribute. The stress behind it will get loaded, and it can probably fail, and so we'll get sort of a progressive spalling. So the one is that the rock strength is exceeded. That rock falls out completely, and then the stress redistributes, and then I get this sort of progressive spalling. And in the end, I can get a much larger failure zone than the initial over-stressed region.

The second scenario is that the rock fails, but still remains -- still has some residual strength, which is typical for most rock. Most rocks, even when it reaches the peak stress, and a little bit after, the rock strength does not go to zero immediately. It goes gradually over strain, and so if that's the case, if it retains some residual strength, then the rock can remain in place. It will -- some stress will redistribute, but not as much, because the rock doesn't actually fall out. It remains in place, but it's damaged, so we end up with damaged rock that stays in place. So then the failure zone really doesn't grow. We don't get this progressive failure. So those are kind of the two extreme scenarios. And as it turns out, those are the two opinions, the first opinion by the CNWRA, the Center, and the second one by DOE. So that's kind of where we're left with.

And if I look at where they get these opinions, the DOE is basically based on modeling. They used some discontinued modeling, UDEC and PFC, and that gives them confidence that there in a sort of second category where the rock is damaged, remains in place; and, therefore, you don't see progressive spalling.

On the other hand, the Center has done an elastic analysis, and they get some slight to moderate over-stress. And they just make the assumption, they make the worst-case assumption that that rock will completely be removed, and so it's really an elastic analysis with what I call a worst-case assumption.

And just to sort of support both these ideas, that I think that if you go back in rock mechanics 20 years ago, we didn't have very powerful numerical models, so really the elastic analysis was kind of where we stopped. We would do an elastic analysis, and we'd say okay, we'd sort of have to wave our arms about what would happen after that.

Now in the last, particularly in the last five or so years, there's been a lot of development in being able to model into the post-failure region with these underground excavations. But it's still relatively new, so I think what I'd say is the Center approach is the conservative approach, saying we don't feel confident going into that post-failure region, if we're going to make the worst-case assumption. Whereas, the DOE, I think, has done some very novel work in doing just that.

All right. So let's talk about what I did, what EPRI has done. We did some modeling ourselves to try to also look into this. We did standard 5.5 meter diameter. There's a typo. You guys have -- it said radius in that. So 5.5 meter drift, separated by 81 meter pillars. I took all the material properties, basically the temperature, ground conditions, everything has come from the drift degradation analysis report. There's two kinds of analysis. I felt like one contribution that EPRI could make is to do something different than what DOE did, so that we could see another kind of analysis, and see what it gives.

So I did a continuum model, where I focused on the strength softening. Again, this goes back to this issue here on the previous slide of, if the failed region could retain some residual strength, so that all has to do with the strength softening properties of the materials, so I did some continuum modeling with a program called FLAC . And I varied these softening properties to sort of see the range of possible behaviors. And then also I did some discontinuum modeling very similar to what DOE did, but I also added a time-dependence. I added a time-dependent drift degradation and simulated stress corrosion cracking for 1,000 years, because that was another issue that was brought up by the NRC, or by the Center, was that you'd get a progressive time-dependent spalling over time, so I looked at that.

This was the mesh I used for the FLAC. It's only a quarter mesh, because for continuum analysis, it really is fine. All the other quarters are going to look about the same. But for the UDEC, I have what's called a block model. I'll explain more about this later, but it's a full mesh all the way around. You can see this is the big mesh, and this is just the -- focused on what's right around the drift. I focused on this block model. It's trying to simulate, realistic simulation of what actually occurs in the lithophysal properties.

So let's talk about the FLAC modeling. Again, it's what we call a strain softening model. So let me just explain what strain softening is. This is a typical stress strain curve for rock, and rock typically we get sort of an elastic region. We get a slight non-linear region near the peak, you get a peak strength, and then we get this what's called the strain softening region after the peak, and then we get some kind of residual strength at the large strains. And so that's the stress strain curve. And, in particular, we're interested again in this post-peak, because that's where this difference between being able to retain some residual strength. That's the key, because you can see that out to the peak, the strength goes down, but only after some amount of displacement, and so it doesn't go down immediately.

So what I did, I used -- this is the strain softening model in FLAC. Basically, I have three things, cohesion, friction angle, and tensile strength, and they all decrease as a function of plastic strain down to some minimum value. And the key parameter for the cohesion and the tensile strength, the minimum value is pretty much zero. So the key parameter for me is this gammaC, and that's the strain over -- that's the plastic strain over which these values decrease to their minimum value. And I'll just -- here's an example of a simulation of a uniaxial test using this strain softening model. We've got stress on this axis, I've got strain on this axis, so again I get this sort of linear region, and then I get the strain softening.

I just want to explain a little bit about the details of this, kind of waviness. The FLAC is actually a dynamic model, so when I applied the load initially, this actually creates a little bit of a wave, so that wave is what we see here. So that's why you see this little bit of a waviness rather than a straightness, is just because when I first apply the load, it's actually got this small amount of waving properties through there.

And, similarly, you see this kind of thing at the very end. And that's I get this instability, and then it sort of rebounds a little bit, because, again, it's a dynamic model, so that's why. So if you see that, I just want to explain about that.

All right. Well, this is kind of the core result that we get from the FLAC results. So I tried many different values of gammaC, and what I tried to do here was find a value that's on one side, and a value on the other side. So what ends up happening is, this is the Category 5 lithophysal tuff, so just remember it's the strongest. It's got a strength of about 30 megaPascal. It's got a high Young's modulus, and it's got a low lithophysal porosity, so it's got the highest thermal stress that's generated. And what happens is, I end up producing a large spalling when this gammaC is .002, which are these two left set of figures. And then I produce a very small spalling region for gammaC of .005. So there's sort of a transition in-between these two strain levels.

And just to explain this figure. The little Xs all represent places that have gone plastic in the FLAC model. And then there's also this kind of orange, and the orange just shows the most recent failures that are occurring there. And then there's actually some tensile failures that occur here. And you can see this is the uniaxial behavior for the gammaC equals .002. And this is the uniaxial behavior for the gammaC equals .005. So, again, a big difference.

I've created a scenario where I get the large spalling. I've created a scenario where I get the small, and the difference of these two strain levels. And the reason for this has got to do with the fact that when I look at the -- in the case that's unstable, what's happening is the cohesion, and the tensile strength are going to their minimum values, and so all the stress has to be redistributed. And it's that same effect that I talked about, so then that gets -- the load builds up there and fails again. So it's really very similar, these two are very similar to these two scenarios. So I've sort of created the case where I want to have the scenario where it does fail, and it's like it's being removed. And the other case, it retains some residual strength.

Now what I could -- just as a further explanation, here's the case, the stable case. And if I look at the cohesion values, you can see that the key here is the cohesion has not dropped to the minimum values. Initial cohesion 7.5 megaPascal, and then at most it's gone down by about 20 percent. Whereas, if I look at the unstable case, you can see here the cohesion goes all the way to zero, and that causes this advancement of the spalling. I really have captured the two scenarios, one a stable case, and one unstable.

So now the real key now is, let's figure out which one the actual Category 5 lithophysal tuff, which gammaC best fits that one, so here I do that. Now what I'm going to look at is the key parameter here is the strain it takes to go from here to here. So if I look at this axis of strain at the bottom, how much strain does it take me to get from the top to the bottom?

Now if I look at the unstable case, the peak to residual strain is less than .0001, and if I look at the stable case, the peak to residual strain is about .0004. Now here's some actual results for the Category 5 - well, this is actually Category 5 lithophysal, so this is actually the non-lith, and you can see, first of all, these are very much more gradual. And, in particular, for the case with the lithophysal, actually very gradual. And if I measure the peak to residual, so for the non-lith, the peak to residual is about .001, and for the Category 5, the peak to residual is about .015, so you can see here that it's -- in this case, it's 15 times as much strain as the unstable case. So I make the conclusion from this only minor spalling is expected in Category 5 because, certainly, the strain that you get is much greater than this minimum strain that you need in order to get this unstable.

Now you can make -- similar conclusions can be made for the other categories of lithophysal tuff. I'll just show you. This wasn't in the thing. I threw in the Category 3. This was in the report, but it wasn't in the handout. And very similar results for Category 3. Here's the uniaxial, here's again an unstable case, and a stable case. And, also, the Category 1, this is a Category 1, same idea, an unstable case and a stable case.

Again, the stable case you see still has wall spalling. And, again, as I talked about before, that's because that's a spalling that doesn't even -- it occurred even without thermal loading. That's because it exceeded the strain. So that's an interesting case, so I'll repeat this last one.

I did a FLAC analysis for the Category 1 without any thermal loading, because that's what's going on now in the drifts. And there's some drifts at Yucca Mountain that are under static stresses, but they're not under thermal loading. And you can see that for the Category 1, I predict spalling even without thermal loading. And if I look at the cohesion values, you'll see that they don't drop very much. Even though I get a spalling region, if I look at the loss of cohesion, you can see that it's at most around 10 percent. So what you'd expect to see, you wouldn't expect to see this big spall if you went underground at Yucca Mountain, because the cohesion values -- maybe you'd only see a little bit of spalling right at the center, and that's exactly what you see. So I think this matches very well with what you see. So, again, this supports the idea that the strain is significant enough, the strain softening of the actual Yucca Mountain rock, so that we're not seeing this large spall, we're only seeing a very small spalling region.

So then the question, just to say well, at least with Yucca Mountain, I haven't seen any case that would predict this large amount of spalling. Do rocks exist with a steep strain softening slope? And I make the statement here, in general, as the heterogeneity in the rock increases, the peak to residual strain would also increase. And so, for instance, if I take a granite, this is typical granite, and it's got very small scale micro cracks on the order of the grain of granite, which might be a

10^{th} of a millimeter, or might be a millimeter. And if I compare that to the lithophysal tuff, you can see there I'm getting heterogeneity on the order of inches. And so what I'm saying is that because of that, you can expect a lot more peak to residual straining post-peak, than you would in something like a granite. And I show some examples of stress strain curves. For instance, this is a basalt, and has very fine grain, volcanic rock, and also sometimes granite. And so there are rocks where you can expect to see maybe this kind of progressive spalling that wouldn't be in the Yucca Mountain rock, and I'd say that's because the heterogeneity is just too big, and so that's creating this large strain. And that kind of shows in the difference here.

You can see here's a case of the non-lith. Here's the lith. By putting all these voids in there, you can see how it's flattening out the post-peak. And that's exactly what should be causing stability in these cases. All right. That was the FLAC model.

Now I did another model with UDEC, and this model is trying to closely simulate what actually happens in the rock at Yucca Mountain. And, so, in the lithophysal. And if I take a look, here's an example of a picture of some of the lithophysal rock, and here's some of the numerical modeling. And what it shows is as I load this rock up, you essentially get cracks that connect the lithophysae, and that's the mechanism that you expect to get failure in the lithophysal zones, these cracks that connect the lithophysae. And so how would a block fall? If I'm going to get a block to fall in the lithophysal, what's going to happen is that all sides of that block, the cracks have to propagate, and it creates this free block that can fall. So I say here that rock failure occurs by cracks that connect individual lithophysae, and removable blocks, those are the ones that can create rock fall, are formed by cracks connecting all sides of a block.

So what that means is if I have cracks on three out of four sides of a block, that may not be enough. That block will stay in the roof, it won't actually fall out. It'll be damaged, the rock will be damaged, but I won't get, necessarily, a rock fall event. So a rock fall event means that I have to have creating a crack around all sides.

Now this is a lot in one slide, and this is a lot of work that went into this time-dependence, and I've written many papers on this, and I can give people some more references on this, but I thought I would be pretty brief about how I do the time-dependence.

Now I have this thing called a rock bridge. It starts with what I call a rock bridge, so what I'm calling the rock bridge is the in-tact rock between the lithophysae, so if you see a lithophysae here and here, then that little piece of in-tact rock is what I'm calling a rock bridge. So that what we're really doing is we're breaking the rock bridges, time-dependent degradation of these rock bridges. And it creates a cohesion guide, so what I have is a block model, and on all sides of the block I have cohesion. The cohesion is there because we have these rock bridges. And then as the rock bridges crack, then the cohesion drops to zero on the boundaries of the block. And if the cohesion drops to zero on all sides, then I have the potential for this rock to fall out.

Now based on the information in the drift degradation report, I've taken average rock bridge sizes of five, ten, and twenty centimeters for Categories 1, 3, and 5 tuff. And then I have a formula here, again, this comes from -- this has been derived in some papers that I've written. Basically, I have a cohesion that's based on the fracture toughness, the size of the bridge, and the size of the block. And what I end up getting, which is interesting, if I took all this information on the lithophysae, obviously, when I'm in the higher porosity lithophysae, like the Category 1, it's going to have a much higher concentration. And based on that, I calculated all the cohesion values, and when I go through the analysis, I get for the Categories 1, 3, and 5, I get cohesion values of 3.8, 6.2, and 11.25.

Now as it turns out, this is very similar to what DOE got in their UDEC model. They also put cohesion. They matched the cohesion by doing some kind of a matching procedure, but it's interesting. I went through calculating the cohesion based on the actual spacing of the lithophysae, and I got these cohesion values, which turns out to match the DOE quite well. And then I used power-law stress corrosion formula for the cracking of the rock bridges. And, again, I have material properties that we determined for all of these values. So that's basically how that works.

I'll show you some results now from the UDEC. Again, it's a block model, so you can see here I've got all these individual blocks. And on the sides of the block are basically the cracks that will connect the lithophysae. And, first of all, I've got the first plot is the number of rock bridges that failed versus time. And it's in seconds, so basically this goes to 1,000 years. And so what I did was in the modeling, is I loaded it up to the max thermal, and I held it there for 1,000 years just to see what the effect of having it at maximum temperature for 1,000 years. So that was kind of -- that's how I did the thermal analysis.

And what you can see here is on this axis I've got number of rock bridges, and you can see the maximum is about 140. And what you can see is that most of the damage occurs in the first hundred years, fifty to a hundred years. And then it sort of flattens off, even though there's still a little bit of damage. This shows the actual damage around the excavation. This is Category 5 tuff. And you can see, if I look very closely, you can see blocks, and you can see, the green shows where the bridge, it's cracked. And what we see is, if I have a block where it's green on all sides, then that's a potential block that could fall out.

On the other hand, what I see mostly, though, I see some green, but in a lot of instances I still see some unbroken bridges. So in a lot of cases

I get -- you can see I get a damage zone. I predict the damage zone, but you can see there's a few blocks that are actually falling. But, overall, the blocks are not falling out because -- there's two reasons why the blocks aren't really falling out. One is, because I haven't -- because the bridges have not broken on all sides, and so, therefore, they're still hanging on. And secondly, because there's still frictional forces, because the thing about friction is the cohesion in the post-peak region basically drops to zero, but the friction does not go to zero. Even when I fail, I create a crack, there's still frictional forces along that crack that's created, so the friction may be lower than its initial value, but it doesn't go to zero. And so because of that, I could have a big wedge like this that's still in held in place because I still have some stresses.

So one way to see that, and let's go to the next slide. I'll show you some more. Now this shows some of those frictional forces, so this shows -- the green on this top plot shows open cracks. Now the open ones are the ones that certainly there's no stress on them, and they're ready to fall out. But you'll notice that on the sides here, I've got some ones in red, and red is the ones that have friction on them, forces on them. So what I end up getting is a lot of these blocks are in place just because of frictional forces, even though the rock bridges might be broken.

And one way to see that is to look at the stresses, because what happens is I get a damage zone, and the stress will move outside the damage zone. And how much stress moves outside is a reflection of how weak, or what's the kind of cohesion of that damage zone. So if I look at the bottom figure, different colors represent different stress, and you can see -- the highest stresses, as you can see, have moved out, so right around the excavation, the stresses are lower. And you can see that red kind of band around the outside. That's where the stresses have moved, so there has been some damage, the stress has moved away. But what's interesting is the stress has not gone to zero by any means, and you can see that in a lot of places, I still have stresses on the order of 20 megaPascal, so that's plenty enough to hold these blocks in place with friction. So the only places I see where I'm really probably going to see rock fall are the stresses at the very bottom here, the very dark blue. So if I look at these very dark blue ones, I think those are the ones I'd expect to see the rock fall.

And so what I'm saying then, is based on my UDEC analysis, we're predicting some rock fall within the damage zone, but not the whole damage zone. Overall, the damage zone seems to hold itself up, except for a few places where the stresses have gone to zero, or near zero, and they may fall out. So that's kind of my sort of conclusion for UDEC modeling.

Now Category 1 is a little different, so I thought I'd show that. Category 3 ends up the same as Category 5. Again, the Category 1 is the weakest one, and that one is interesting because, first of all, I get a bigger damage zone. You can see the damage zone here is quite a bit larger. Again, the rate is 2-1/2 meters, and I'm getting maybe up to 2 meters of damage zone. But, again, if I look closely, you'll see that there's a lot of unbroken bridges in the damage zone. And, again, that's because there are still stresses there. Because of the orientations of those, they're not likely to break. And you can see there's some rock fall events occurring, but overall, I'm seeing some integrity of the damage zone. So, again, I don't expect that this whole thing will just collapse.

Now what's also interesting about the Category 1 is I'm seeing some continued creep, even up to 1,000 years, so at least for the Category 1, it might be a case where given another 1,000 years or so, there could be some additional damage. I submitted up to 1,000 years, but certainly, even though most of the damage occurs in the first 50 years, there seems to be steady increase in damage over time in Category 1, so that's the weakest lithophysal unit. I expect any place that's going to see large amounts of rock fall are probably going to be -- it's going to be there.

And if I look at these other two figures, again, you can see there's a lot of this green represents open. But again, there's red zones that represent the friction. So, overall, again there's a mechanism for this rock to be damaged, but not falling into the excavation. And I can see that again here. Again, you see these big red zones on the side. That's where the stress is pushed out into these, but there's a small region with this very dark blue, where I expect to see some rock fall. And that's where some rock falls are interfering. And so, again, I would predict some isolated rock fall within the damage zone, but not the whole damage zone, for the reasons that I mentioned.

Now I did model the non-lith. This isn't in the thing. I had this in the report, but I didn't put it in the thing. I did model the non-lith. This is what I call regular. This is kind of this average properties of the non-lith, and it's very stable. This is very similar to the DOE results, basically predicting that almost no rock fall in the -- this actually increases the stability. I get the same results. The thermal stress actually causes to push on those joints, and actually increases the frictional effects, so it's very stable. If I look at the number of rock bridges, you can see it's very small. You can see there are still rock bridges that have failed, but overall, there's not really -- there's no open zones. You can see there's no green zones representing open zones. And if I look here, there's not much load stress even, so very stable, the standard, what I call the average non-lith.

Now I also did, just for -- because I know when you go underground at Yucca Mountain in the non-lith, you do see zones that have a density of fractures, and they're referred to as these fractured zones, or I forget what I call them. It could be a fault zone, or it could be a highly fractured zone. And there, I modeled them, and that was the worst, of all the things I modeled that was the worst case, was that in this highly fractured zone in the non-lith. The reason is because the failure mechanism in the non-lith is different. These are actually joints, and there's hardly any cohesion on those joints. Whereas, in the lith, the lithophysal zones, you have to create those cracks between the lithophysae, and that gives it some cohesion that you don't have. And in these fault zones, I predict a lot of -- here it's got 800 rock bridges broken. You can see it's a very -- and here's a case where a lot of the bridges are broken, so the only thing -- there's really no cohesion left in here. The only thing that's holding up is frictional effects. If you go to this one, you can see a lot of open, and this is a case where I would expect to get some rock failure. And, again, a lot of zones are very low stress near the boundary, so the only place where I really did find the condition where I might see a lot of rock fall is this highly fractured zone in the non-lith.

All right. So conclusion, again, I did two kinds of models. I did the FLAC and the UDEC. The FLAC results was a strain softening model, a continuum model. It was based on varying the steepness of the strain softening slope, and I show --

it showed that if the slope is steep enough, sure enough, you will get a large spall. But for the material properties of the Yucca Mountain tuff, I never got a steep enough strain softening slope, so I come to the conclusion from the FLAC that I wouldn't see these large spall regions because there's always -- the strain softening slope is not steep enough in any of the rocks at Yucca Mountain.

The UDEC results I simulated by this crack growth around these lithophysae, creating free blocks that can fall out. And it showed that you will always -- you do create a damage zone, in every case there is a damage zone. It's bigger in the weaker rock, it's not as big in the stronger rock. Also, I did time-dependence, and it showed that there is time-dependence, but it generally stabilizes at about 150 years. And the only kind of actual rock fall I saw was isolated blocks within the damage zone, not the whole damage zone, just isolated blocks.

There was a good correlation between the FLAC and UDEC. There was some little differences. I noticed that - this is a Category 1 tuff - but I didn't vary the -- I kind of made the standard orientation of block which consisted of sort of these three sets of joints. And because they were fixed, I think that sort of influenced the exact location where the weakest zone was. I noted that in the report. But, overall, other than that, I think there was good correlation between the FLAC and UDEC results. And the results I showed are in agreement with the DOE in the drift degradation analysis, and they're in partial agreement with the Center's results. And they're in partial agreement because it does predict damage, certainly. It does predict some failure, some spalling, but it doesn't predict total drift collapse. So for the last statement here, neither the FLAC, nor the UDEC predicted large amounts of spalling that would fill the drifts with rock blocks.

MEMBER CLARKE: Okay, John. Thank you. Dr. Ryan, I know you need to leave soon. Do you have any questions?

CHAIR RYAN: No, go ahead, Jim.

MEMBER CLARKE: Okay. Professor Hinze, as Dr. Ryan mentioned, is fighting his way to Rockville. He's had some serious travel difficulties, but he did send in some questions.

MR. KEMENY: Okay.

MEMBER CLARKE: And I want to make sure we have time for those, but let's take a couple from the Committee, first. So, Ruth, do you want to go?

MEMBER WEINER: First of all, thank you for a very, very thorough presentation. I have two questions that are not related to each other.

The first is, are there natural analogs for this process? I mean, I know that we have done -- Banolier National Monument, for example, is full of caves that people have lived in in volcanic tuff. Does that kind of situation afford you any sort of natural analog to what you're modeling?

MR. KEMENY: I think so. I think the problem always come in the thermal. You know, the Center is very insistent that the difference -- there's a lot of places that have high stresses, that don't show complete drift collapse, and the DOE said, well, look at that. And then the Center says well, those don't have thermal loading, so that's really -- and, unfortunately, I can't think of a maybe an analog where you've got the same kind of thermal loading. I think that's probably the problem with the analog.

MEMBER WEINER: Yes. I don't think you have the same kind of thermal loading. People did build fires in those openings.

MR. KEMENY: Sure. True.

MEMBER WEINER: And that gives you, more

or less, a continuing thermal loading, but the temperatures, of course, aren't as high. There's no spike.

MR. KEMENY: Well, certainly, the best analog we have is the heater test. The heater test in the non-lith, which is good because according to my analysis, the non-liths would have steeper strengths off the slope, so if you didn't see runaway spalling in the non-lith, you would definitely not see it in the lith. And then the only -- the Center is basically saying well, that's not a good test because it's got rock bolts and mesh. And I would say again, you know, DOE said the same thing, if you look at the stresses, the kind of support stresses given by rock bolts, there is some. I mean, typically, I mean, think about the stresses at Yucca Mountain, you've got 10, 20 type of megaPascal static, and then it could go up to like 50 megaPascals thermal. The support pressure of a rock bolt is probably half a megaPascal, 10^{th} of a megaPascal, it's very small. And, again, that assumes that you put it in before you get the deformation, which is the case at Yucca Mountain, probably. I assume they put the rock bolts in before they heated it up. I assume that, so if that's the case, then yes, you could get a slight amount of support stress from a rock bolt, but it would be so small that I wouldn't think it would change the dynamic of the spall region. So if you're getting a very small spall region in the heater test, I would think that's a pretty good indication that that would similar, even if you didn't have any rock spall. That's something you could analyze, because again, rock bolts just don't apply that support pressure, probably a 10th of a megaPascal, compared to the kind of 20 to 50 megaPascals that's running in there, so it's a very small amount of support pressure.

MEMBER WEINER: My other question really ought to be addressed to the Center, I guess. There's a big difference between your results, and the Center's results. And I remember that we discussed this some years ago in visits to the Center, and also in presentations to this Committee. What do you see is the resolution of that?

MR. KEMENY: First of all, I don't see it as a big difference.

MEMBER WEINER: Oh, that's interesting.

MR. KEMENY: You know, the differences are coming from -- it's coming from the conservatism, I would say, because they -- the Center ran an elastic analysis, which is fine, and the elastic analysis showed some failure, a small amount of failure. It's very similar to what I showed, and what DOE showed. They are just saying since we don't know what's going to happen in the post-peak, let's take the worst case scenario. So, again, it's not as if they modeled, and they created large spall from the model. It was really from an assumption. And, again, if I'm wrong on this, let me know. But I was assuming they did an elastic analysis, and then they made an assumption. And since they didn't want to make -- they just took the worst case assumption about what would happen in the post-peak. And what I tried to do is say okay, let's do better than that. Let's just see if we can model the post-peak, similar to what DOE did. And when you actually model the post-peak, it looks like it's stable. So, again, I would say it's not necessarily a huge difference in terms of the starting point. It's just this kind of sort of an assumption that's made.

MEMBER WEINER: So you would say that the differences, just to repeat what you just said so I have it straight, the differences are that the Center's assumptions were conservative. But within their assumptions, your models would yield basically the same or very similar results.

MR. KEMENY: I think they would admit that their assumptions are - I think, I'm not positive - that they are conservative, because again, it's true, that modeling post-peak rock behavior is a relatively new thing in rock mechanics. I mean, we haven't been doing it for 20 years, we've been doing it for maybe less than 10 years, so it's not necessarily totally out of line to say, okay, well, we can't model post-peak; therefore, we just not going to. We're going to make a worst case assumption. But I think the Center would admit that they were making a worst case, a conservative assumption.

MEMBER WEINER: Okay. Thank you.

MEMBER CLARKE: Okay, Ruth. Thank you. Allen?

VICE CHAIR CROFF: A couple of things. If you get damage in the rock, as you project, and apparently DOE projects, and then a seismic event comes along, is the rock a lot more susceptible to spalling?

MR. KEMENY: Definitely. The real question then, if the rock is damaged, then when a seismic event comes along, it's more likely to -- the rock fall would be worse than if it wasn't damaged.

VICE CHAIR CROFF: Okay. And is the

seismic event that would be required the magnitude of it, particularly large, without defining what large is?

MR. KEMENY: Yes. And it could be that the seismic event, it could create drift fill. It might be smaller than if the rock wasn't there, so that's right.

VICE CHAIR CROFF: Okay.

MR. KEMENY: But, again -- yes, that's probably true. And it goes back to how much damage there is. I'm not predicting like a meter to two meters of damage around that --

VICE CHAIR CROFF: Has anybody looked at superposition of these two, I mean, assuming the damage occurs, and then at some point well down the road you get a seismic event. Has anybody looked into that?

MR. KEMENY: That's a good question. We did not, because we did the seismic, but we didn't do the thermal. And, similarly, DOE did the seismic and thermal separately, if I remember right. I think we'd all agree that it would be -- I think it's reasonable to agree that a damaged rock is going to fare worse in a seismic event than an undamaged rock.

VICE CHAIR CROFF: Okay. Second, I'm a

little intrigued by this mention of stress corrosion cracking. I don't think of rock as being susceptible to that kind of thing, but if I understand what you mean, the rock under ordinary conditions is fairly stable in the presence of the percolating water coming down from wherever. But what you're implying is that under stress, it becomes more susceptible to, I guess, corrosion or dissolution, if I can use that word. Is that what we're talking about here?

MR. KEMENY: Yes. It's a similar phenomenon that's very common. You guys speak that language with the metals, I know, but yes. I mean, the crack -- the way we talk about it is, we call it sub-critical crack growth. So what's happening is the rock is under stress, and so all the little cracks are under stress. And they're not enough stress to break, because if we take a sample in the lab, I have to -- in order to reach the tensile strength, I have to pull a certain amount, and so it's under less than that. But given enough time, the crack will continue to grow, and it grows slowly but surely over time.

VICE CHAIR CROFF: And it's growing because corrosive --

MR. KEMENY: Well, the main mechanism for stress corrosion cracking in rocks is -- again, it's stress enhanced, so it's at the tip of the crack, and it's typically -- in quartz, you get it from the silica, the switch, you have a silica oxygen bond and it re-arranges and becomes a weaker bond, and then it's able to break. It probably a similar mechanism -- I don't really know much about the metal stress corrosion, but it's a chemical reaction that's essentially taking place at the tip of the crack. It's enhanced at the tip of the crack. And you need water to do it, you need some amount of humidity, at least, in order to drive it.

VICE CHAIR CROFF: Has that been studied very much in tuff?

MR. KEMENY: Oh, yes, it's been studied a lot. And I should say that DOE's model is the same. The DOE has a time-dependent model in their drift degradation, and it's also stress corrosion cracking model. It's a little different than the one I used. I used this what's called a power-law. They use an exponential function. But other than that, they're both -- they devote a lot -- there's a whole chapter devoted to this in the drift degradation analysis report about their model. Yes, and it's a stress corrosion model.

VICE CHAIR CROFF: Okay. And are the time constants on stress corrosion cracking - I mean, you've run through your models, and you basically conclude the tunnel is reasonably stable, some spalling but not much. Are the time constants on stress corrosion cracking such that it wouldn't have a lot of effect during the time you looked at it? It might be 100,000 years, or --

MR. KEMENY: Well, let me say first, it's not a standard type of test to determine these time-dependent parameters. I am a little uneasy on those properties, just because if you look at how I determined those, a little bit of back-calculation there, so I actually reduce the properties. I mentioned this in the report, I did the analysis where I just reduced the properties, just to see what would happen. Because let's say I'm off on this one parameter, so I reduced it much farther than I thought it would be, and just to see the difference. And it wasn't a huge difference, but there was a difference. But yes, I think what it's showing is, with the kind of stresses we're seeing there, 1,000 years isn't really enough. You're right. When you go to a million years, you start to see a lot more definition of that kind of mechanism. You can, it depends on the level of stress. I think everybody knows, for instance, that like -- let's say I have a rock, and it's got a strength of 100 megaPascal. If I sit it at 80 megaPascal, let it sit there, probably after a few months it's going to fail. So if you're close to the failure strength, then you might only have to wait a few minutes, or a few months, or a few years. But let's say I only put a load of half the stress I need, well, then it may take 10,000, or may take 100,000 years to fail. It will still fail, but it may take a lot longer, and it's sort of a logarithmic type of thing. So as the -- and these tests have been done in the lab. They can do the test where you put it at 90 percent because it fails within a few hours. You can do the test at 80 percent of strength, maybe it takes a few days. But as soon as you get to like 70 or 60, you can't wait that long. It may take years, it takes tens of years, so, unfortunately, the test results don't go down the very low values because it would take too long to have them fail. So we're extrapolating all this, and they also do this in the DOE, they have to extrapolate to these lower values.

VICE CHAIR CROFF: Okay. Thanks.

MR. KEMENY: Yes.

MEMBER CLARKE: Okay. Thank you, Allen. I want to get to Bill's questions, but one for you, John,

MR. KEMENY: Okay.

MEMBER CLARKE: I'm looking at the second bullet in your conclusions, and it says that the UDEC results indicate that damage would occur within 50 to 150 years. Is that the case with the FLAC results, as well?

MR. KEMENY: Well, the FLAC was not time-dependent.

MEMBER CLARKE: It's a different kind of --

MR. KEMENY: I didn't do a time-dependent FLAC result.

MEMBER CLARKE: Okay.

MR. KEMENY: The way I do time-dependence in UDEC is, it's actually the stress corrosion cracking, but there are really no cracks in the FLAC. FLAC is a continuum model, so, unfortunately, in a continuum model, you don't really have a crack, so I don't really know how to do the time-dependence in FLAC.

MEMBER CLARKE: I guess what struck me with that is that that would be during the period when the repository would still be open.

MR. KEMENY: That's true. And I feel that same way, that in some sense - and that's what these results show. We would be able to see -- let's say we were wrong. Scientists are not -- we're wrong sometimes, so let's say we were wrong, and you did get big spalling. Well, you'd see it, because it certainly would happen right away. If you look at those curves, I mean, all the damage, you get a very steep amount of increase in damage in just the first 10 years, so you would see it.

MEMBER CLARKE: Okay. Thank you. Neil Coleman of the ACNW&M staff has been working very closely with Bill on these issues. He has Bill's questions, so Neil.

MR. COLEMAN: Okay. Could you comment on the in-tact rock strength versus rock mass strength, and the scaling relationship, and how that was addressed in the modeling that you did?

MR. KEMENY: Right. Now it's right at the beginning. I hope I'm not wrong in this, but the typical -- a lot of rock mechanics has been developed for the hard blocky rock containing joints, including your typical three-joint sets. In non-lith, there's this whole new thing, this is swiss cheese rock, and something that we were not used to. But, anyway, I think if I'm not wrong, these -- for these Categories of 1-5, these are rock mass properties, but it's a different kind of rock mass than you typically see for a hard blocky rock. Because what happens with hard rock is we test a little piece of granite in the lab, we get 200 megaPascals for the strength. Well, when we extrapolate that to the scale of a large slope or something like that, it can go down by a factor of 100. The rock mass strength can be 100 times lower than the in-tact strength. In-tact strength is a little piece that we test in the lab. The rock mass is the big piece we see on a highway, or underneath an excavation, and it can be a difference by a factor of 100. It depends on how many joints you have, the spacing of the joints. But this lithophysal rock is different, it doesn't have a lot what we consider these classical low-cohesion discontinuities, and so because of that, when you test a sample that's big, this big, it's a little bit weaker than a small sample, but it's not the factor of 100. So as far as I know, these are the rock mass, and there is no kind of equivalent in-tact, because - well, you can, but if you tested the material in-between the lithophysae, I mean, this is essentially -- the 30 is probably essentially the in-tact strength. And then by adding the lithophysae, reduced it to 10, but if you didn't have any lithophysae at all, you're probably just a little bit stronger than that one, so maybe 35 or 40 megaPascal. So that's essentially the in-tact. And then as I add this heterogeneity, it keeps getting weaker and weaker, so these are what I would call for the lithophysae.

Now this bottom one I put, maybe that's what he's referring to. You can -- this is basically using one of these Hoek and Browne rock mass strength criteria, so based on R mar. R mar is something that he's calculated in the non-lith, you get R mar values of like 50 or 60, and based on that, you take in-tact rock, you can calculate it, and that's where that is coming from.

MR. COLEMAN: Actually, the staff are here, and I just wanted to confirm something by asking Raj, as I understand it, the current design, and you won't see the final design until an LA is submitted, but the current design involves stainless steel rock bolts, and a thin layer of a perforated stainless steel mesh. Is that still your current understanding?

MR. NATARAJA: Yes, but that's not being discussed here, because that support is only supposed to be helping you during the pre-closure period, and no credit is being taken for the performance of the support system during post-closure.

MR. COLEMAN: Right. But I'm just asking what the current design was.

MR. NATARAJA: Yes, what you just said is what we have seen in the current conceptual designs.

MR. COLEMAN: Certainly, stainless steel rock bolts would last much longer than the carbon steel that's typically used.

MR. NATARAJA: They could, but as the presenter said, I don't think there are too many studies documenting the life span of ground support system.

MR. KEMENY: The key is -- it's quite a corrosive environment for the rock bolt. It's in the rock, fluids are going to travel. Well, they can be grouted, I shouldn't say that. Well, they can't be grouted because you can't use cement. I don't know, so if you grouted them and protected the rock bolt from the environment, they would last longer, so I don't know if that's going to -- if they're going to be grouted or not.

MR. COLEMAN: Of course, there's also going to be a thermal period with extensive dry-out of the rock that would be keeping a lot of moisture away.

MR. KEMENY: That's true. So if the

moisture is away, then yes, stainless steel rock bolt, I would think would last a long time. And I would think that -- I think there are some studies going on. When I just did some Google search, I found some various studies in the perforated sheet, and on the rock bolts. It looks like people are studying the long-term behavior of those things, but they haven't entered into any of these initial documents, the results, I guess. But it does appear there are some studies going on on this stuff.

MR. COLEMAN: Also, there's a question about the basic testing that the data are based on, the uniaxial compression testing. Now this, of course, is different from the actual conditions underground. Initially, you have a cylindrical tunnel that's unconfined on one side, and a lot of these conclusions about the rock strength are based on a completely unconfined test. Does this add to conservatism of the analysis in what one concludes about the strength?

MR. KEMENY: I don't know. I'm not sure. That's a typical -- if it's a typical, we just assume that's the uniaxial strength. I don't think that's necessarily that conservative.

MR. COLEMAN: In identifying the depth of spalling that takes place, I believe some of the materials you've written refer to a self-limiting process in this depth of spalling. And, although, I think you've addressed that at several points in your talk, could you just briefly summarize the things that limit the depth of spalling in the worst case.

MR. KEMENY: Here, these two figures - well, these two, so if I take a look at these two, here's a case where I do see a lot of spalling, here's a case where I don't. And the difference is -- the key difference, it's in the post-peak behavior of the material.

Now if I look at the stable case, what I see here is, I see, again, I see a very narrow region where I'm getting some softening. But if you look at these numbers right here, it starts at 7.5, only goes down to 6, only about a 20 percent decrease in the cohesion, and in the tensile strength. And because of that, it's basically -- it still maintains some residual stress. So the key is if I go -- maybe I didn't do that before - is start at the peak, and that material in the boundary is not down here, it's right here somewhere. It hasn't gotten all the way down. And the reason is because there hasn't been enough strain. Because this kind of -- it's all got to do with this kind of extra stuff right at the peak here, so it ends up sitting right there, instead of dropping down.

If I go to the unstable case, you can see it's a very sharp drop. It doesn't have any strain at the top there, so as soon as there's a little strain in the post-peak, it drops. So what happens is, you can see it started actually on the wall, but what happens is it completely reaches bottom, so all the stress has to go to the neighbor. That gets bottomed out, and so on. And the reason it sort of starts at the wall and sort of progresses toward the roof is because that what happens to the stress rate over time. As you heat up an underground excavation, initially you're just heating up right at the boundary, so the high stresses are equally around, all around the boundary. And it's going to fail first right here, because that in-situ stress is the highest in the wall. But then with time, what happens is there starts to be some interaction between the drift here and the drift that's 81 meters apart, and those temperatures start to expand that pillar. And that creates higher stresses in the roof. So you get sort of -- it starts here, and it kind of migrates over to the roof. And if you look at this figure here, you actually see -- this one, you can actually see the same thing, because what happened was, these are green, which represents yield in the past. And then the active yielding is in the roof, so what happens, again it started in the wall, and it migrated up to the roof, and that's sort of the process that we see there.

But anyway, so the big factor is that the fact that it's not very steep in the post-peak means that it can retain residual strength, and it doesn't have to fall out. It can just sit there with slightly -- it's a stable -- it turns out this is a stable equilibrium condition, with just a slightly yielded zone here, and it's like you said, it's self-limiting in this particular case. But if it was a steeper strain softening, it's not self-limiting, it causes that progressive spalling.

MR. COLEMAN: And here, you're talking about a case where there's no consideration of ground support at all, just the natural case.

MR. KEMENY: Right. Right. Sure. I mean, it makes sense. If you create a damage zone and the rock doesn't fall out, it retains a little residual strength, then that's the best support. It's like a natural support system, in a way, because the stress moves away from it a little bit, so there isn't much stress in it, but it doesn't fall out. That's right.

MR. COLEMAN: The NRC has recently published TPA 5.1, the User's Guide, and the current best estimates by the staff on collapse of the tunnels. And the current estimate in there is that full collapse of the tunnels would occur in less than 2,000 years. And that certainly seems very different from what you're talking about here. It does not appear to take into account the variation in the rock properties, the lithophysal.

MR. KEMENY: You're right. Right. Well, that's another -- that's true. That's another thing that -- these are -- you would expect -- I mean, there's a significant change in the properties between this lithophysal 1 and 5, so you wouldn't expect everything to just happen the same across the board. You would expect there to be differences between the different materials.

And what I'm kind of showing, if there's any rock that's the most susceptible, it would be this lithophysal 1. It's got the highest porosity, it only represents 2.5 percent of the repository, but that's the one that, if anything, may show some more significant rock fall than the other one.

MR. COLEMAN: I'm trying to think of a very simple way to describe this. The liths that you're showing, the lower right in that figure shows this is the thermal phase, and you're showing a greater depth of spalling in the roof?

MR. KEMENY: Well, no. Actually, I should be careful, because this is what an elastic analysis would kind of show, because what an elastic analysis shows is that there's several phases of this temperature cycle. Initially, you're heating up right around the room, and then in the end, you're heating up the whole pillar and the whole rock mass is expanding. In the end, the elastic analysis, this is very similar to what -- maybe what the Center came up with. But what I kind of show sometimes, and that's very similar to the stable case, I get here. You can see I've got there's more cohesion lost in the roof than there is in the wall, so I also get that. But in this case, you'll notice it's a little bit different, because in this case, because I get failure of the wall first, it actually changes the stress state, so you end up with sort of a different kind of a failure in the end. And I mentioned this in the report, that in a true non-linear analysis, the stress path does matter. And you can see that here, the stress path results in a different failure than if you just did an elastic analysis. You might predict something like this, but because that's elastic, so there's no failure anywhere. But when you actually take into account the non-linear of the rock, you might get a different result.

MR. COLEMAN: One last point here. You describe how in the thermal period there's a transition in the maximum stress more to the overhead in the tunnel than in the walls; whereas, in the present day, there's a greater stress in the walls. Sort of a simple way to look at this, if you get initial spalling from the overhead in the drift, then more of it begins, from a stress point of view, to look like walls. Is that one of the things that in a simple way is limiting the overall spalling, where it eventually completely bridges over, and you don't get much additional failure?

MR. KEMENY: I don't know. Well, I mean, the -- initially, the stress is higher in the walls because this is a normal faulting regime. Normal faulting, you've got your highest stress is your vertical stress, so initially, the highest stress is in the wall, because this is your highest in-situ stress is the vertical stress, because it's a normal faulting type of thing. But the transition is really -- first of all, when you start to heat, if you look at solution for thermal stress due to putting a hot canister, it gives an equal thermal stress pretty much all the way around, and that's true. So then what you're doing is, you're still not creating this effect. But that final effect comes from the fact that you heat those pillars, 81 meter pillars up, and that expansion creates this sort of third phase which has got a high stress component in the roof. And that's got to do with -- because it's allowed to -- the mountain basically moves up, but it can't really -- because you've got all these parallel drifts, it's kind of forced to not expand laterally, so it creates the stress. But that's when the full heating of that pillar -- I didn't bring any -- I should have brought some figures on that to show you, but that's really what's creating the high stress on the roof, is the heating of the pillar. It wants to expand, but because I have parallel drifts, it can't expand, so it creates a high horizontal stress. And that's kind of the third phase.

The first phase is before you heat, you have the highest stresses here. When I thermally heat, the thermal heating is kind of equal all the way around, but you'll end up with the higher stresses in the wall because you add the in-situ stress to the thermal. And then finally as the pillar starts to heat up, you'll start to see a little more -- maybe a little more stress in the roof. But you were giving some analogy which I couldn't quite --

MR. COLEMAN: Just as a final clarification here. Your first bullet there, "Depth of spalling less than 1 radius", it would predict less than 1 radius away from the boundary. And is that for the lithophysal Category 5?

MR. KEMENY: Well, I'm not -- I put this mainly -- I'm trying to just say well, what do I mean by slight to moderate over stress? I'm not really speaking of Yucca Mountain so much here. I'm just speaking about any place. I'm saying if the over-stress is greater than the radius, then we might -- it might start to get into this category here, what I consider to be -- we used the word here, we call it squeezing ground conditions. We have this super high stress, we refer to that as the squeezing ground condition, because the stress is so high. So what I'm saying is, once this D is greater than R, or maybe even 2R, then you've kind of -- now it's at the squeezing ground condition, as opposed to this, what I'd call slight to moderate over-stress condition.

MR. COLEMAN: Well, from your modeling, what insight would you have about -- not looking at the most highly fractured case, which is a severe case, but of the different categories of lithophysal versus non-lithophysal rock, what sort of radius depth of spalling --

MR. KEMENY: Well, the FLAC is predicting this to skip. I call it like a skin effect. So, again, you get sort of a different result between the FLAC and the -- the FLAC is predicting various thin skin of spalling, less than -- way less than a meter, less than a half a meter. And that's shown by basically this. So, again, we predict that you always get this sort of stable case. But you're getting some small region where you're over-stressing the rock, and it's just what we call a skin effect. Now that's what you get from the FLAC.

Now what you get from UDEC is a little bit different. UDEC kind of predicts this kind of result, where you've got a damage zone that could be quite thick, but you're only predicting very little isolated blocks to come down within that damage zone. So the who damage zone does not come down, and that's because the definition of damage zone is different than the definition of what the FLAC sees. The definition of damage zone means that at least one side of that block is cracked, but that doesn't mean all sides are cracked. So just because you have a damage zone, these blocks won't necessarily fail, because they're held up, and there's only some failure on certain sides of the blocks, and not all sides. That's why. Again, if you refer to the damage. So this is very similar to what DOE was getting, the damage zone was on the order of a meter, but again, it wasn't really showing that much rock fall within the damage zone. Just maybe isolated blocks here and there.

MR. COLEMAN: Thank you very much. A very informative presentation.

MEMBER CLARKE: Thank you, Neil.

MR. COLEMAN: We have five minutes. You might want to invite any comments from the staff.

MEMBER CLARKE: I was going to do that, but Ruth has a quick one, and hope it's a quick one. And I want to invite the Center to participate, as well. We just have a few minutes, and we do need to --

MEMBER WEINER: This is a very quick question. You're assuming that the effects that you see are uniform along the entire drift.

MR. KEMENY: Sure.

MEMBER WEINER: That's correct. Is that a good assumption?

MR. KEMENY: Well, I think, again, this is the same assumption that DOE makes, that the lithophysal makes for an okay, two-dimensional approximation, because it's kind of like the swiss cheese rock, so it's really okay.

Now when you go to the non-lith, you notice they did a full 3-D analysis, because really you run into problems there by making a two-dimensional assumption. But for the non-lith -- for the lithophysal, then I think it's a pretty good assumption, yes.

MEMBER WEINER: Okay. Thanks.

MEMBER CLARKE: Okay. Antonio has something.

MR. DIAS: Yes. Hi, this is Antonio Dias with the Center staff, and it's somewhat to follow-up from what Ruth was saying. In your model, UDEC model, you basically assumed at the start of your simulation that that was a pristine drift. What would be the consequence, and how difficult would it be to incorporate actually the effects of the drilling that cause on those walls, so instead of starting from a clean sheet, you actually have already some induced cracks.

MR. KEMENY: Okay. From the TBM, you mean?

MR. DIAS: Yes, the tunnel boring. How much of a penalty is that? And the other question I have would be, your thermal loading, you applied it for 1,000 years. Is it a constant load?

MR. KEMENY: Yes. I'm sorry. I didn't quite do -- I actually -- the way I did the thermal, I'll answer that question first, was I loaded up to the maximum thermal, and then I just kept it constant for 1,000 years.

MR. DIAS: Because it's --

MR. KEMENY: But I realize -- and I did some of the models where I did that, and it wasn't that much of a difference.

MR. DIAS: Really? That's very interesting.

MR. KEMENY: Yes. But, again, I didn't repot on those, because I just did one run, I think, on that. Most of them I held it for constant for 1,000.

MR. DIAS: Okay.

MR. KEMENY: But to answer your first question, yes, I think so. I think that TBM will do some damage, probably within -- maybe a half a meter or something, so that could be put into the model.

MR. DIAS: Yes. Okay. Thank you.

MEMBER CLARKE: Thanks, Antonio. Let me now invite the Center. Are you still on the bridge?

MR. IBARRA: Yes, this is the Center.

MEMBER CLARKE: Any questions?

MR. IBARRA: Okay. This is Luis Ibarra from the Center. I have a question about the FLAC model. Mr. Kemeny has mentioned several times that it was key behavior that made the key difference in the potential spallation of the rock. And he mentioned that a factor of gammaC, which is the strength after the -- the peak strength, it will be the key. I don't have the figures here, but in one of the figures on the report, he shows that you have a larger gamma C value, he has on ductility after the peak strength, so my question is there's no experiment, at least no available experiments showing what is the performance of this tuff material after the peak strength, and the question would be what are the reasons for believing that you shall have -- that shall be ductility or less ductility after the peak strength in the confinement area is reached?

MR. KEMENY: Okay. That's a good question. First of all, he brings up a very good point, which is mentioned in the report, that these are actually not laboratory test results. As it turns out, when do lab tests, it's been traditional to measure the peak, and to measure this slope so we know the modulus. But this is -- in all the reports that I have, I do not have the information from actual tests.

Now there were a couple of tests presented by DOE, I don't have them, that did show this International Lab test, but they're from some report that I don't have, or unpublished. But I think the key is, and I mentioned this in the conclusions, in the future we need to really start reporting the post-peak because it is so important in this problem of spalling. But I think the question that he asked is really the answer here, that why do we believe that in the real -- when we test the real rock, we're going to get those big strain softening slopes? And it's really got to do with the size of the heterogeneity, that when we tested granite, and it has a .1 millimeter grain size, we sometimes get a very steep post peak. But in all cases, when you start to have this heterogeneous cracks, it's a very torturous path the crack has to take to get its way through there, and it always results in more strain. You can see that in the results.

This is without the lithophysae, this is with, and you can see that it produces a much bigger post peak. And I believe that in general, the larger the heterogeneity, the more strain, post-peak strain you're going to -- they're going to produce. And since this is the rock that we're dealing with --

MR. IBARRA: All right. That's all our follow-up questions.

MEMBER CLARKE: All right.

MR. OFOEGBU: John, this is Goodluck. I have the response to Luis' question, that in looking at the calculations you did, what we find is that the parameter, what it intends to control, actually, is not stress, often it is the amount of strain that your material can undergo without a significant change in the strength, which is the ductility that Luis is talking about. And you know that brittle rock, like basaltic, volcanic rock is typical brittle rock based on information that has been published in the literature, for it to have this kind of ductility, you have to increase the confining pressure to probably tens of megaPascals. The best thing, of course, would be to show whether the rock has this kind of ductility, that without a test, the question is what do we expect, based on studies of rocks that we have published in the literature. What kind of rock usually is the peak when you measure the ductility at the peak of the stress. Would we observe this kind of behavior from volcanic tuff?

MR. KEMENY: I see what you're saying. I've seen some non-lithophysal tuff that's very fine grain, like you said. It could be quite brittle, but that's not what we're looking at, in general, in the lithophysal. It's very heterogeneous. But I would say, my biggest support, I think, would be the two test results that I -- the two kind of actual field results that you have. One is, that if I go to the Category 1, this is the Category 1, if it was very brittle. But this is not what we see in the drifts today. What we see in the drifts today, looks more like what we predict when it's not very brittle. So I guess this would be the one piece of evidence we have, is that this exists now in the tunnels. We can go look at this, and we do not see this massive type of wall spalling. What we see is something more consistent with this, where we see a region with just a little drop in cohesion, maybe a little bit of bulging right near the center.

The second case, I would say, would be the heater test in the non-lith, because again, there are rock bolts there. I don't believe the rock bolts are really adding much support; so, therefore, I would say that, again, that's another piece of evidence that it's behaving not like this basalt that has this super steep post peak, but rather like a more -- a rock that has a lot of heterogeneity to it on large scale.

MR. OFOEGBU: Can I see -- I don't know what the schedule is there, but this behavior of Category 1 material in the drifts that you mentioned, actually helps confine the strength of the material, that it doesn't really tell us what would happen if the stress were to -- if there is a constant sort of loading, like thermal load, that tends to increase the stress in the rock beyond the peak strength.

MR. KEMENY: The result I show, as I said before, because the modulus is so low, this has not got to do with thermal at all. The result I show for Category 1 is all based on in-situ stress, because the thermal - it's like one megaPascal increase due to thermal, and so this is the result of static loading only.

MR. OFOEGBU: Yes, but I mean all the analyses come and do show that for Category 1 rock, you don't get -- you get failure on the side wall, but not in the roof, because of the low modulus on the thermal load. So what I'm wondering is how the behavior of Category 1 rock out in the field is evidence of ductility. It only gives you the -- what happens in an opening immediately after it's commissioned? That tells you how the stress, the in-situ stress, or the stress concentration around the opening relates to the strength of the material, but it doesn't really tell you how failure may progress, if it didn't occur. And the example you mentioned for the heater test, I thought that the heater test showed a lot of bulging in the wire mesh, indicating that there is a load of rubble weighing down on that material, and that it is possible that the existence of the material essentially removed the space available for more -- for rock to fall into, that's likely if the opening were to be filled with rubble. So why would you use that as an evidence of ductility for the non-lithophysal tuff?

MR. KEMENY: Well, again, I know how much force one of these -- this is a split-set, it looks like to me. If that's a split-set, it probably --

when I said .1 megaPascal, that was for actually like a resin ball. There was a split-set, which that looks like it is, I have to calculate it, but it's probably 100^{th} of a megaPascal, is the amount of support force it's going to apply. So I can't imagine that the difference between massive rock fall and it's just a 100^{th} of a megaPascal of rock bolt support, so I would -- I think it -- I just think that compared to -- again, this is the non-lith, so again, if anything is going to go, it would be the -- if anything is going to be brittle, it's going to be the non-lith. So the fact that you're seeing such a small zone of spalling, to me, would indicate that --

MR. OFOEGBU: Well, the importance of the heater test, is that it's live evidence that we have of how the rock might behave on that thermal load. I'm surprised, though, that you're discounting the role of support. Support system does -- the role of support system is not really reduced related to the amount of pressure it applies in this particular case. What is critical is for something to -- the wire mesh, what it does is that as the rubble comes down, instead of falling off the surface, it is held by the wire mesh, and the wire mesh essentially creates -- limits the amount of space that is available. And once that space is filled up, then there isn't room for any more material to fall. This is speculation, of course. The right thing would have been to cut out the wire mesh, and let whatever is there fall off, and then we see how much it is, and then see whether there's any tendency, and this is why I think it's virtually incomplete in terms of the behavior of the rock under heated conditions.

MR. KEMENY: Well, let me just say that if I went into a deep underground excavation with this amount, same support rock bolts, mesh, and it was unstable in the sense of -- I would see this full of loose rock. I mean, it wouldn't just be in the middle, it would be all across, it would be sagging the mesh in-between the bolts, it would be popping the bolts out. I mean, there are many examples of underground excavations where this is a support, and it's just popping. It's just popping it out. So I would just -- if it really -- if you really thought that this was the kind of -- going back to this figure. If you really thought this was the kind of failure we're getting, then I would expect to see more evidence of that behind the wire mesh. We've seen that in deep underground excavations.

MR. OFOEGBU: We've spent a lot of time on non-lithophysal, but for the lithophysal rock, I think the main thing you present is that with some amount of ductility, that you can expect the rock -- the overloaded rock to simply fracture, and shed the load. But your analysis all show that even if you reduce this ductility, then you would expect the material to fracture and fall. One of the test results, you showed two tests by FLAC, one with .05 and the other one with -- okay, .002 and .005. So that small difference, and you also show that the difference is really in the ductility at the peak point or the stress break-off. In one case, there is measurable ductility. The other case, there is ductility, but not much.

MR. KEMENY: Right.

MR. OFOEGBU: Okay. So the first case showed what you interpreted to be spallation tended for the spallation to continue. Whereas the other one showed that in fact the ductility of the rock is enough to prevent it. So it boils down, really what needs to be determined, if we are going to -- if one would rely on this kind of behavior to predict what might happen in the drifts is to look at test results that will show whether this rock has sufficient ductility to change the behavior under thermal conditions.

MEMBER CLARKE: Mr. Chairman, I'm going to need to jump in here. We were scheduled to end at 10, and it's now past 10, after 10. Raj has one more question, then I'm going to have to call it. Thank you.

MR. NATARAJA: This is Nataraja, NRC staff. I don't have a question. We didn't intend to have any kind of a debate or anything like that on this presentation. All I can say is that it's an excellent presentation, and what we have learned today does not seem to change our approach to the review. If you remember what we said, we were looking at the various possibilities, the range of possibilities that might be predicted by various models, alternative conceptual models, and it looks like that this presentation confirms that we have a number of possibilities, as he showed the two extreme cases, and we are looking at all the things in-between in our performance assessment. So I think it actually strengthens our approach of review, and it's an other good data point for us. Thank you.

MEMBER CLARKE: Thank you. John, thank you very much.

MR. KEMENY: All right.

MEMBER CLARKE: Thank you all. Allen?

VICE CHAIR CROFF: Thanks for an interesting presentation. At this point, we'll go off the record until 1:00, where we'll reconvene and hear about in-situ leach mining and TAD. Thank you.

(Whereupon, the proceedings went off the record at 10:14 a.m., and went back on the record at 12:58 p.m.)

CHAIR RYAN: The meeting will come to order.

The cognizant member for this session, the session after the break, is Dr. Weiner on issues related to first in situ uranium mining facilities; and second, vendors' views on transportation, aging and disposal performance specifications.

So Dr. Weiner, have at it.

UPDATE ON NRC RULEMAKING ON GROUNDWATER PROTECTION AT IN-SITU LEACH URANIUM MINING FACILITIES

MR. RATHBUN: Thank you.

We are scheduled to have an update on the rulemaking on groundwater protection at in-situ leach facilities. And I take it our speakers are Gary Comfort and Mike Fliegel. Divide it up however you want to.

Gary, you're in charge, our lead speaker.

MR. RATHBUN: Dr. Weiner, let me say - Dennis Rathbun from the Division of Intergovernmental Liaison and Rulemaking.

We are pleased to be here. We worked this up with Mike and Gary.

(Telephone interruption)

MEMBER WEINER: I think it's just us.

Dennis, if you want to continue. Sorry for the interruption.

MR. RATHBUN: Oh, sure. Thank you.

Anyway, we responded to ANCW's request. Talked to Mike Fliegel and Gary Comfort.

Gary will be - he has a set of slides that he's going to go through. But we've had a lot of interactions over the past several months with Environmental Protection Agency, and that's been kind of challenging I guess is a gentle way to put it.

And we had originally planned that e would have a draft proposed rule in late spring, April or so of next year in 2008. Whether or not we will actually make that date or not, given the current status and the complexity of the ongoing discussions that we had with the Environmental Protection Agency I think frankly is a little problematic at this point.

But ANCW wanted to have a status report on where we are with respect to the situation, and in particular the technical basis. And so Gary will go through what we're doing.

I'll turn it over to Gary and Mike.

MR. COMFORT: Good afternoon, everybody. My name is Gary Comfort. I'm a senior project manager in the rulemaking branch and division of inter-governmental liaison and rulemaking.

I'm here to update you on the status of our rulemaking for the groundwater protection at in situ leach facilities.

I plan on between Mike Fliegel who's to my right and I are going to basically plan to give you some information on the background of the rulemaking just to remind you of what it's about; the status of our current rulemaking effort; and then to respond to the comments that were sent to us last summer.

Just a reminder, we gave you a previous presentation on this on April 11th. During that presentation we discussed the technical basis of our rulemaking.

Included in that discussion was legislative and regulatory background, the efforts that we were - the commission's efforts and desire for us to try to eliminate dual regulation. Currently we're in a situation where the Environmental Protection Agency will provide permits to the underground injection - or the injection wells, and we provide licensing to the ISLs themselves for the uranium processing operations.

We discussed our rulemaking strategy at the time. And prior to the April 11th meeting we were having our discussions with the Environmental Protection Agency at that time also.

We were having some more high level policy issues about what the basis for our regulation was; whether - were the Uranium Mill Tailings Control Act, or if we could use the EPA's underground injection control for these ISL facilities.

We discussed our path forward, which as Dennis had mentioned we were hoping to get our proposed rule to the Commission by April of this year.

We had meetings shortly after we had resolved where our technical basis - or our basis for the rulemaking was for the UMTRCA, the Uranium Mill Trailings act, and we discussed that in final meetings with EPA, and also the National Mining Association to make sure they understood that the direction that the Commission had originally pointed us at was not going to work because of EPA's new discussion with us about what the actual statute required.

But we basically got agreement from both of those groups to move forward. Then we basically discussed what our next steps at that time were.

Just to remind you that our initial commission direction was given in March, 2006, where the Commission directed us to initiate rulemaking for groundwater protection at ISLs. The big thing on that was to focus on eliminating dual regulation of groundwater protection at those ISLs.

We were hoping that we could potentially get our regulation situated consistent with EPA's UIC program for Underground Injection Control program, to allow us to defer our regulation of certain aspects of our program to EPA; basically the day-to-day monitoring, that kind of thing, to either EPA or their state programs.

We were also directed to actively engage the stakeholders, and we were supposed to get a proposed rule at that time to the Commission in January of 2007.

That date wasn't met because of our difficulties with determining what the basis for the rule really should be.

In June of 2006, after we'd gotten the

initial direction from the Commission, we sent out a letter to EPA which requested that they confirm with us that the Commission's intent that the UIC rules were the appropriate standards for which us to conform our NRC regulations; that was going to be correct.

We got back a letter in August of that year that basically said that they were very concerned with that proposal, and they suggested that we go ahead and meet with EPA to discuss that standard a little bit more intensively.

So we started going down the path of rulemaking because we had a very expedited schedule, but we were starting to meet with EPA at the same time.

In those meetings in August that we had with EPA, basically EPA stated that it had two major problems. The one first was that the groundwater standards in 40 CFR and 192 which basically were the UMTRCA standards are applicable to ISLs, and they were stating that the UIC standards were not applicable.

What this ran us into a difficulty is what our restoration requirements, that was where the biggest difficulty came out. UIC basically wasn't quite as conservative as what the restoration requirements were for UMTRCA. UMTRCA requires the first attempt for you to go back to background.

Additionally they pointed out that if we did want to defer, EPA didn't think they had sufficient resources to regulate the ISLs under an expanded UIC program to include what they thought NRC would need at the time, particularly in non-authorized states. The states themselves may have the resources, but EPA themselves at that point said they didn't.

So they were actually intending potentially maybe to even defer it to us, or rely on us for some of the work.

We continued those meetings all the way through late 2006, and finally EPA made it clear that they didn't believe that the use of the UIC standard as a basis for the ISL rulemaking was going to be the adequate standard. They said that the UMTRCA standards were the correct ones, and EPA however expressed a willingness to continue to work with us in this rulemaking process.

Now up to this point is basically things that you'd kind of seen in the previous presentation back in April. Now I'm moving up to things that happened after that date.

The first thing is, we updated the Commission on all this new information that we had gotten. Basically we told the Commission in COMSECY-07-0015 that we were resolving these issues with EPA; what the difficulties were; and what EPA had basically said should be the underlying standard.

And basically we said that we believed though that with that new information from EPA we could still go forth with a rule.

The staff requirements memorandum from that paper basically directed the staff to continue rulemaking; said they continued to remain diligent in working with EPA and the states also, and they wanted us to make sure we were establishing standards to protect the public and environment, as well as they still had an intent for us to reduce and eliminate dual regulation.

So that's where this rulemaking is going forth right now is to meet those goals.

In response to making sure that we worked sufficiently with the states and EPA, we added two working group members from the Environmental Protection Agency, one from their Office of Air and Radiation, and one from their Office of Water.

And we also added a person to help represent the states. We had already had an agreement state member from the Organization of Agreement

States, so we added somebody from the CRCPD.

MEMBER WEINER: I am going to interrupt you just for a point of clarification. What now is EPA's role in regulating this?

MR. COMFORT: EPA set the original standard.

MEMBER WEINER: Yes, but they have said that they don't think the underground injection standard is appliable.

MR. COMFORT: Right.

MEMBER WEINER: So what now is their role?

MR. COMFORT: Well, they still have a role in providing us - basically we have to - they basically confirm that our regulations meet with UMTRCA says. They have a role in that.

MEMBER WEINER: Oh, okay.

MR. COMFORT: So making sure that it's consistent with UMTRCA. So we almost have to do a concurrence to them to a certain level that they say - and we've got that developed into our process, that they will basically pout a statement at some point, probably in our statements of consideration, that they agree that the rule we are putting out is consistent with UMTRCA.

We are also using them in other ways to

make sure that we are not - anything that we develop in regulation is not going to be in conflict with any of their regulations that they have to regulate the same facilities with, again, to help eliminate the dual regulation issues, and other complications that could cause.

So since that point we have these group members; we've also begun, or further developed our draft ruling, which the language is basically being pulled from our NUREG in 1569 which we currently use to help regulate the ISL facilities that we have.

We also did still grab a lot of language from EPA's UIC program, and you'll see in a few minutes that that caused a little bit of problems with EPA.

We are also adding focus on additional - or basically the rule we are writing is going to focus primarily on additional requirements that are specific to the groundwater protection at ISLs. The Commission was very clear that tehy don't want us to expand this rule outside of that role of developing it for groundwater protection only.

There'd been talk in the past of doing a bigger, broader role that would be specific to the conventional mills and ISLs, and the Commission basically decided they don't want to put the resources into that at this time, and just add this hole that they feel is missing in our regulations on how to better protect and regulate the ISLs.

Our recent interactions with EPA have been complicated to say the least. After we added the working group members, we provided this new version of draft language to all the working group members. And that was in September.

The EPA staff raised some concerns about that language, particularly our use of UIC. There were also some other difficulties that it felt that we were pointing more toward UIC instead of the UMTRCA regulations.

Part of it was because we'd provided them additional language to go into our existing regulations of the mills, in Appendix A of Part 40, and they didn't quite understand how it fit in there as well. So they were looking for an independent rule, and they saw no language related to UMTRCA rather tahn in the rule we were referring to the existing regulations, because they are still going to be applying to these facilities.

Again, our intent was just to add additional criteria that we thought were necessary to ensure the proper safety of these facilities.

EPA nad NRC have met a couple of times since that point after the working group meeting. They brought it up to their management, and brought their concerns related to the rule, particularly the use of the UIC, and the complications that they foresaw with using their language.

And we've been going through a process of trying to resolve those issues. We had a meeting in October, and our latest was in December.

Basically there are issues surrounding, as I said, the UIC being used as a standard. What they were concerned about is by us using some of their language, we were going to usurp their role in running their underground injection control program by basically taking away their ability to do permits, or doing something to impact how they were going to be writing these permits for these injection wells.

And they thought it would actually influence not just the ISLs but any that they did under this class of facilities.

They also thought that anything we interpreted by using their language would result in a difficulty that tehy would have to interpret the same way.

So what they really preferred us to do was to change our language to do direct reference to their UIC program language.

While NRC saw that there were some problems with that direct reference - one, we didn't think anything such as the interpretation problem went away, it actually got worse; it would still be citing from our regulations. We have to interpret it, and now we are using their exact words.

Second of all their UIC program encompasses a lot more things than just ISLs, so trying to do a direct referral to aspects that would only regulate those - that was going to be specific to ISLs out of the larger scope was more difficult, and what we had done in our draft language was only taking that language that went specifically to the ISLs.

And then additionally we ran into a difficulty that the UIC program - lost my train of thought, apologize.

So they had a lot of difficulties though with us using UIC language, but we basically we were able to, in this December meeting, resolve that it would be our language; it was specific to our uses; and the other difficulty was that, or explanation that we had was, we were running under two different statutes that we are regulating under, so our use of their language was meant to basically make a consistent pathway forward to resolve similar problems, but because they were under different statutes there shouldn't be any difficulty if we had slight differences in interpretation, and we felt there was no reason to develop totally new language, and reinvent the wheel that could cause further conflict.

The other big difficulty they saw the current language was that despite us telling them that there were referrals back to the existing Appendix A, they didn't believe that the UMTRCA standard was represented in this language enough. And so we are trying to work on the rule language to better emphasize to make sure that people realize the UMTRCA is still the underlying standard.

Again, our last meeting was in December with EPA. We got it up to the management level of both EPA offices, as well as they got their lawyers, as well as our NRC lawyers, and managers were in the meeting.

As I stated we explained why the use of the UIC language, and why we wanted to use the UIC language, and what the problems with the direct referral were.

We did get most of the outstanding issues resolved that we believe we can move forward now with the rule. And this was only two weeks ago, so we're just at that point that we are reestablishing, making changes to the rule.

We are hoping to provide the working group a new version sometime in early 2008. Again, we will continue to work with EPA and provide those. They've got the working group numbers on the working group.

Our next steps are, we'll through the next couple of months continue drafting the rule package. There are a couple of other policy type issues that we have to deal with on handling some things in UMTRCA such as their use of the drinking water requirements and how that applies to the groundwater restoration, or Safe Drinking Water Act requirements.

The way it's been done in UMTRCA and Appendix A is that we've done basically a direct citation of what this Safe Drinking Water Act requirements were back when Appendix A was developed. That's now changed, and we're looking at a route that we can perhaps do a direct reference in this case, since they have to meet Safe Drinking Water Act.

But since UMTRCA itself and the EPA

implementation doesn't have that, we've got to do some negotiating with EPA.

We hope to continue this, and as Dennis said, it's likely going to be delayed. We are expecting we probably won't be able to get a proposed rule to the Commission until sometime around fall.

Part of that is because we still have to do a few more meetings with EPA to make sure that they are okay with the path that we are going; they feel it will meet the requirements of UMTRCA. And also we do want to make sure that we can get further input from stakeholders.

We have been planning on trying to do some public meetings early in the process. And we're hoping to, once we get some draft rule language developed, and to a point that the working group agrees to it, then we can either get it, some of it put on the web for comment, or go out and do some meetings at the potentially impacted sites.

So that's really where we're at with the rulemaking. Mike will now go on and basically respond to the comments that you had provided back in your letter.

MEMBER WEINER: Let me interrupt briefly again. Do you have a slide that shows in some way what the language was that EPA objected to, and how you ahve resolved that? Or could you give us some more details?

It's a little difficult to follow just saying, they didn't like this. Do you have that in your presentation?

MR. COMFORT: That's really not in the slides. But I can talk to that.

What we had done is, as we laid out the rule, the first draft of the rule, we had sections in there about characterization of the aquifer, about requirements to ensure that wells are properly drilled; and that there is integrity testing for wells; things like that.

Well, in looking at what hte language ought to be, we borrowed some of that from rule language, EPA's rule language under the Underground Injection Control program. For example certain statements about well integrity testing.

And we only borrowed pieces because EPA had within their rule had requirements that didn't really apply. So we had pieces that came from the UIC program. We also had pieces that came from our guidance. And while EPA was upset about us essentially borrowing the language for certain aspects from their regulations.

As Gary stated, the concerns were, well, why don't you just point to our regulations? Well then we'd have to - every time we borrowed a piece, have to point to that specific location in the EPA regulations. It just would become very cumbersome, and it wouldn't resolve the problem that we interpreted something a little differently than they, wehther we cite it in their regulations, or just write it in our own words, replacing EPA with NRC.

So that's basically what it was.

MEMBER WEINER: Thanks for hte clarification. That's good. That's fine.

MR. FLIEGEL: I am Mike Fliegel. I'm senior project manager in the Uranium Recovery Licensing Branch in the Division of Waste Management and Environmental Protection.

I'm going to pick up on the ANCW's recommendations.

After the April meeting, the staff briefing, ANCW wrote to Chairman Klein on May 9th. The letter presented five recommendations.

The EDO wrote back to ANCW on June 10th and agreed with the recommendations. It was a very short letter basically saying they agree.

The rest of my presentation will go through the recommendations and discuss how we plan to address them.

I'd like to say that we have already identified all the items in the recommendations. What I'll say is, we have identified most of them.

There are a couple of suggestions, they were all good, but there were a couple we hadn't thought of. Next slide.

The first recommendation was that the staff should proceed with developing the proposed rule, including codification of appropriate standards specified by EPA and we agree, and we are proceeding with the rulemaking as Gary as just discussed the recent activities.

The rule should provide specific guidance on the three-dimensional location of the point of compliance; groundwater monitoring requirements; methods of demonstrating compliance; and financial surety considerations.

I'll take them one at a time. The point of compliance: that's discussed in the present rule in the existing Appendix A with respect to conventional mills. It's location in the uppermost aquifer where the standards must be met.

It's also used to identify how we identify leakings from an impoundment at conventional mills. For ISLs, well, first of all the mining zone we know will exceed standards during operation. The rule will define where the standard is to be met; that is, in the mining zone after restoration.

The rule will also require excursion monitoring to detect contamination moving away from the mining zone during operation.

It's not clear yet whether the rules will specifically use the term for that point of compliance, but we will have those requirements in the rule.

Groundwater monitoring requirements, both in the mining zone and outside, including aquifers above and below will be in the ruling.

Methods for demonstrating compliance, both for excursions and for restoration, will be in the ruling.

Financial surety, financial surety is already in the rule, in the existing Appendix A criterion 9. However the ISL rule will discuss additional surety requirements that are specific to ISLs.

For example currently our guidance

discusses the situation if there is an excursion the licensee has to do certain things, and if he still doesn't contain the excursion and wants to continue operating, the guidance suggests that he's got to provide additional surety.

Well, things like that will go in the ruling. But again, surety in general, we feel is already in the rule.

The rule should establish guidance on measures to reduce the likelihood of contaminant excursions outside the mine zone and the site property, and for remediation outside the mine zone is excursion occurs.

To prevent excursions the rule will require a net inflow into the mining zone, and will require monitoring above and below the mining zone; and the rule will also address actions that must be taken if an excursion occurs.

Rules should be risk informed and should consider groundwater use, onsite effluent disposal, and deconditioning and license termination.

Restoration: requirements for restoration must be one of three standards. And these come from UMTRCA, EPA standards in 40 CFR 192, and are now in Appendix A, criterion 5. And the three standards are:

first, premining background; second, essentially drinking water standards; and the third is an alternative concentration limit.

The third requirement, an alternate concentration limit, incorporates risk in groundwater use. And so that's how we believe that it will be risk informed, and consider groundwater use.

Onsite effluent disposal: a licensee basically uses one of two methods. They use evaporation ponds, and that is already addressed in Appendix A criterion 5. The rule will refer to that, will make it clear than an ISL an evaporation pond has to meet the requirements that already exist for impoundments in criterion 5.

And deep well injection, which really is a requirement by EPA or the state.

Decommissioning and license termination: the decommissioning of wells will be addressed in the rule. The decommissioning of the site and the license termination we believe are beyond the scope of the rulemaking, but those subjects are already in Appendix A.

An ISL site has to be decommissioned to cleanup criteria that already exist in criterion 6, and license termination is already in Appendix A.

Finally the rule should provide requirements for establishing premining background and baseline level quality. The rule will provide requirements for establishing premining background, baseline quality, for all aquifers potentially affected by the ISL activities.

That includes the surfacial aquifer. We will in the rule point out that an ISL, you still have to look at the official aquifer, and the licensee is responsible for leakage from surface facilities into the surfacial aquifer in addition. And so it has to give some background for that, in addition to the aquifer that the mining zone is in and aquifers above and below it.

That concludes my presentation.

MEMBER WEINER: Thank you very much.

Dr. Hinze, why don't you start off.

MEMBER HINZE: Good afternoon, ma'am.

MEMBER WEINER: Glad to see you made it.

MEMBER HINZE: Thank you.

It's really heartening to hear all of this response to the comments that the committee made in the letter.

I do have a couple of questions at this time, and I'll ask a couple more afterwards.

The matter of dual regulation is one that we've heard, and we have also heard some of the state people not be overly concerned about state regulation; about dual regulations; about NRC regulations overlapping from the state's.

The states certainly differ in their regulations on this issue, and one wonders if in the advance to eliminate dual regulation that there may be holes in this process.

What are you doing to assure us that there are no holes as a result of the varying state regulations and trying not to overlap them?

MR. FLIEGEL: Well, we are looking at - I view it as a two-step process. The first step is writing our regulations to cover what we conclude NRC has responsibility to regulate.

Once we have that in place, we can go to a state; we can look at what the state is regulating, and if they are amenable reach an agreement whereby one party regulates that aspect for the two of us.

For example, once our regulations are in place, and we have a regulation looking at, say, well integrity, and testing of new wells, if the state has a requirement under their UIC program for the same thing, we could write an MOU that essentially has the licensee meeting its NRC obligation by having the state approve the well integrity testing.

There may be, depending on what's in the state regulations, there could be a lot of areas where we could defer to the state, or if the state wants to defer to us and we are amenable to it.

MEMBER HINZE: Mike, I really appreciate that response, and it gives me assurance that there won't be holes.

But I guess the other thing, the other side of the coin is, I wonder how generic this regulation is. Is that every time you have to deal with a different state or a different licensee, that we will have to adjust the regulations; or adjust the license or the committee to accommodate the state's regulations.

MR. FLIEGEL: That's certainly possible, that once the regulations are in place, we may have to negotiate understandings with each individual state and with EPA if EPA is amenable, although in our earlier meetings they told us that they didn't have the staff to do it for states that are not EPA authorized.

But we may have to do it state by state, and depending on what the legal implications are, we may also have to do it licensee by licensee.

MEMBER HINZE: Well, that seems like a pretty cumbersome approach, but it may be necessary. I'll defer to Dr. Weiner on this, but we did hear the states not be overly concerned about dual regulation. They felt that the problem was more in the communication and understanding of where the states were going and where the NRC was going.

So I think this whole matter of dual regulation needs a very close look.

Let me ask you a question about this point of compliance, I forget now what slide it was in, 11A, something like that. In any event, the point of compliance. One of the concerns is, if that point of compliance is at the margin of the mining zone, and we have monitoring wells, we discover that indeed there may be some excursions, and some levels above that background; I'm concerned that the material that the contaminant would then be outside of the zone, because if you find it at the edge it's outside as well.

And I'm wondering if you have given any thought to buffer zones in terms of this point of compliance. I realize that they also impose a significant problem. But what's the thinking of the NRC on buffer zones?

MR. FLIEGEL: Bill, do you want to -

MR. VON TILL: This is Bill Von Till. I'm the branch chief for Uranium Recovery. That's a good point. Appreciate your comment.

The existing regulations with plant compliant is set up for a surface impoundment whereby you are trying to detect a leakage from an impoundment.

The in situ situation is more analogous to underground storage tank, a circular RCRA groundwater remediate plume, where the entire plume as EPA or EPA authorized states would have point of compliance wells all within the plume itself; not outside the plume.

So we'll have to work this, but right now how we envision it more, you have a mine zone, you have like mine unit #1, for example. You have number of monitoring wells within the mining area.

And when they go to restore it, those wells will be the actual point of compliance wells to which they are measured by. As far as trying to get down to background MCLs or ACLs.

Now in addition to that you also have other wells that are outlying wells, horizontally and also vertically within other aquifers to look for, detect for excursion.

So one of the things we are trying to do is put the point of compliance well strategy in this unique situation that is not really like a surface impoundment scenario.

So I think that kind of gets to your question more. The actual excursion monitoring well rings are about 500 feet or so, depending on the nature of the aquifer outside the mining area to detect excursions.

They are not really - I wouldn't say they are really point of compliance wells. They are almost more point of exposure wells.

I think the point of compliance wells, once they get into restoration, are really within the actual mining zone itself.

MEMBER HINZE: Is that in effect a buffer zone then?

MR. VON TILL: A buffer zone is in effect depending on how the aquifer exemption - we are seeing different scenarios with how that is done from state to state. For example in Nebraska -

MEMBER HINZE: And it depends on how you monitor it.

MR. VON TILL: Yes, sometimes in Nebraska, between Nebraska and Wyoming, some may have more buffer zones than other areas. But I think that's a good point, that it is important to have some kind of a buffer zone, so that by the time you get to the actual underground drinking water source, it's protected.

MEMBER HINZE: You used the term, in response to one of our concerns, net inflow. I think I know what net means, but what does net mean to you people? Does that mean that we can still have excursions, local excursions?

MR. VON TILL: I'm a hydrogeologist, too, so when it's a hydrogeology type of question, I'll kind of jump in.

There are different types of excursions. And the biggest one is losing control of the lixiviant and the mining solution horizontally. And the companies typically have a 1 percent, 2 percent what they call bleed. In other words they are pumping more than they are injecting, and that causes this net inflow.

And that in essence tries to prevent excursions. You still can have excursions even horizontally if they are not watching the ebb and flow of your injection. And we see that happening.

MEMBER HINZE: Not to belabor the point,

but relying on net inflow is to me still opening up the box for having something go wrong.

MR. VON TILL: And that's a good point. There are really a number of contingencies here. There is the net inflow, and then there is the horizontal excursion monitoring well, and then there is also the monitoring wells that are located above and below the aquifers above and below. So there is a network of controls in place to make sure that the groundwater that is being used for drinking water purposes or other purposes is protected.

MEMBER HINZE: Mr. Chairman, can I have one -

CHAIR RYAN: I yield my two minutes to you, Bill. Go ahead.

(Laughter.)

MEMBER HINZE: Just a half a question.

MEMBER WEINER: Only half?

MEMBER HINZE: Just establishing the premining background, what kind of guidance are we talking about here in terms of coming to a premining background, and the length of time that is going to be - that monitoring is going to have to be in place, and what criteria are you going to use to determine what that length of time is going to be?

MR. VON TILL: Well, we are looking at that in the rule. And you know, typically we like these licensees or these applicants to come in with at least a year of monitoring.

MEMBER HINZE: What is that based on?

MR. VON TILL: That's based on our Nureg 1569.

MEMBER HINZE: What science?

MR. HAMDAN: Four seasons.

MR. VON TILL: Four seasons, yes, to capture the fluctuations of seasonal fluctuations.

MEMBER HINZE: We also have climatic change, and you know, and I think they are - I think one should take a very close look at the length of time that you require the mining companies to establish background, or down to some asymptote.

Because I would like to have a feeling that in the states involved, that even covering these four seasons over a period of several years would give a reasonably good value.

I understand where Latif is coming from, the four seasons. But we also know that the four seasons change quite a bit.

With that, I'll pass it.

MEMBER WEINER: Allen?

VICE CHAIR CROSS: Bill, you got my question on excursions, so I'll give Mike back his two minutes.

CHAIR RYAN: Just a follow-on comment I think mainly to Mike. I applaud, and everybody I guess, I applaud the fact that you are working directly with states, particularly nonauthorized states. Because from a licensee's perspective, the fact that you are doing that means there will be a little more, hopefully, regularity state to state.

So even though, Bill, that is complicated, frankly I think that is preferred. I would hate to see all individual licensees having to go through that negotiation on their own.

So I think if you can get the bulk of it with the authorized states program, that's great. If you have a few other ones where you are working directly with them on collaborating or coordinating NRC regulations with states, working on behalf of whatever licensees might show up in that state, that's a real positive.

MEMBER HINZE: I think we are on the same team. We want to make certain that-

(Simultaneous voices)

MEMBER HINZE: So I thought I'd clarify my view.

CHAIR RYAN: Again, from a licensee's perspective I will tell you that is very positive to have the agencies working it out without using the licensees as lubricants in that process. So three cheers to that.

And again my other questions were covered well by Professor Hinze. So I'll pass it back to Ruth.

MEMBER CLARKE: Yes, thanks Ruth.

You mentioned a couple of regulatory programs explicitly, mill tailings, radiation control act, and the underground injection control which I think is under the Safe Drinking Water Act, is it?

And then indirectly the alternate concentration limits that I think come from RCRA; at least that was the first time I ran into them.

Are they part of UIC as well?

MR. FLIEGEL: Ultimate concentration limits?

MEMBER CLARKE: Yes.

MR. FLIEGEL: I don't believe they are.

MR. VON TILL: No, they are not. They originally came from RCRA as you were mentioning, then they were adopted at UMTRCA in our Appendix A criterion 5. But they originally came from RCRA sites.

MEMBER CLARKE: Yes, they could probably go in UIC as well. I mean it's all the same problem I think.

The other thing I wondered about, the evaporation ponds. Are they Clean Water Act, RCRA as well? Is that another, I guess -

MR. FLIEGEL: We would view the evaporation ponds as an impoundment with 11(e)(2) byproduct material. And so would be solely covered under UMTRCA and under - the basis for our regulation would be these UMTRCA regulations, 40 CFR 192, which envisions impoundments, impoundements in general, an evaporation pond. We feel that the existing regulations in criterion 5 are certainly adequate to address that. EPA has a separate responsibility for it.

MEMBER CLARKE: And EPA agrees with that?

MR. FLIEGEL: I don't know that it has been raised. But they haven't objected.

MR. VON TILL: No, they don't have a problem. I should mention that the industry where they can likes to use the deep well injection methodology for effluent disposal as opposed to evaporation ponds for economic reasons.

And even in our existing facilities, where they have evaporation ponds, like Crow Butte in Nebraska, they are trying to use more and more the deep well injection methodology.

So with the incoming applications that we have, and I should mention we received our third application today; we have three now, three brand new applications; they are looking mainly at deep well injection is their preferred means for disposing liquid effluent.

MEMBER CLARKE: Okay, that's helpful, thank you.

MEMBER WEINER: I have a couple of questions to take off on some of Dr. Hinze's comments.

First of all on the one-year monitoring, there are a number of EPA regulations and guidance that I am thinking specifically of power plants that require three years of atmospheric monitoring, three years of weather monitoring for example.

And I would just simply encourage you to look at the question of whether one year is enough, whether you shouldn't expand that to a couple more years. That is just something for you to look at. And there may be very good reasons to stick with one year.

On the inflow question, don't you really mean inflow and not net inflow? I mean net inflow implies that you are controlling - that there is still outflow if you will.

So I'm a little concerned about the language and how you intend to implement that.

MR. VON TILL: that's a good point, and we should work with the terminology. But in essence an inward gradient, hydraulic gradient, is what we're really looking at there.

We can work with the actual terminology and language.

MEMBER WEINER: That's very helpful.

Finally you did sort of slide past the question of financial surety. What are you doing if anything to prevent situations like the Atlas tailings pile from recurring? In other words where the company simply runs out of money, goes bankrupt. You have no financial surety, and you have to clean up the groundwater nonetheless.

What is going to happen - what are you doing to prevent situations like that, or to address situations like that?

MR. FLIEGEL: Well, we have to recognize why we ran into a problem with Atlas, and part of the reason that we ran into a problem with Atlas is that by the time the regulations were in place, the requirements for financial surety between the rulemaking and the challenges to the rulemaking, by the time we actually got to look at Atlas, Atlas Corporation didn't have the wherewithal to provide a larger surety.

So in the future, presumably, now that the regulations are in place, we are requiring the financial surety essentially up front, and what we do for all our licensees is, there is an annual review of surety. And the annual review is forward looking.

For example if we have a licensee, and they had a surety of $10 million, and they have done $2 million worth of remediation, they can come to us and say, well, our surety should now be $8 million because we've done $2 million; they have to come to us and show us, there's $8 million of work left, and here is how we judge those costs.

So that's what we're doing now. We recognized that Atlas was under insured, but we didn't have the means, or Atlas didn't have the means to provide more financial assurance.

MEMBER WEINER: That's a very helpful clarification, that your surety requirements are now always forward looking and reviewed. I think that does resolve the question.

I have one more, and then I'll get back to you.

I was a little bit confused about your discussion about point of compliance. And it looks to me as if you are getting rid of the ACL concept, or not getting rid of the ACL concept.

Judging from what Bill von Till said, your point of compliance is now going to be in the mine zone. Maybe I'm just confused about this. Are you still looking at an ACL - at the possibility of an ACL, where a point of compliance is outside the mine zone, is somewhere close to the boundary?

MR. VON TILL: We are going to look at that in more detail. But to answer your question, the existing requirements for remediation and restoration, the background MCLs and ACLs, we are going to use that concept.

In other words, the way our guidance is right now, and the way we envision the rule, is that the licensee would have to first attempt to get down to the background in the mining zone itself.

And when you say, restore to those levels in the mining zone itself, you have to have compliance within the mining zone.

Now if they can't get down to that, and they can show that they have gotten as low as is reasonably achievable, after using practicable technologies and so forth, we will look at, like we do today, potentially alternate concentration scenarios, as long as they are protective of human health and the environment, basically outside the zone.

And so that is part of the risk, you know, informed risk based kind of approach that we utilize even today, and we plan to utilize that.

But luckily the criterion 5 already has that in there. We will have to expand on that in the rule to the unique nature of ISLs as opposed to tailings impoundment.

But we are going to use ACLs on background, MCLs and ACLs.

MEMBER WEINER: Thank you very much.

Jim, do you have another question?

MEMBER CLARKE: Thank you, just a quick I guess observation on monitoring, retains most of the monitoring, but one of the things that we are seeing a lot of in environmental restoration, especially when you put something into the subsurface, and then you try to get it out, is the mass transport dynamics in a subsurface with any complexity are such that stuff diffuses into zones where water doesn't move as fast as other zones.

This is going to be very basic to Bill. But when you stop pumping and you measure concentrations, there is something called a rebound effect. Concentrations will actually go back up if you turn the pumps back off.

So I was just wondering, does that have any role in deciding when you have met the -

MR. VON TILL: It does quite a bit, and licensees can use a number of pumping and groundwater restorations, biorestoration modeling. And we have been looking at this situation that you refer to for quite some time. We have been working with the Office of Research and looking at this aspect, and I've got right in front of me this NUREG/CR-6870, which is the consideration of geochemical issues in groundwater restoration of uranium in situ mining facilities.

So we continue to look at this research subject matter to try to get a handle on the actual geochemical conditions at one of these sites, and what happens when you stop extracting uranium, what happens to the actual uranium as it moves forward, and as you are trying to restore it.

And it is a research area that we are looking at.

I should also mention that we have the SRM, the November 28th SRM that also tasks the staff to work with the committee on looking at various remedial technologies, you know, other types of remedial technologies, and how they could be used at these sites, too. And we want to start to meet with the committee on that aspect.

So this is kind of an ongoing research and development kind of situation.

MEMBER CLARKE: That's helpful, Bill. I was referring to the false negative, if you will, when you think your concentration is where you want it to be, and then you turn the pumps off and it goes back up again.

So there are - there's been a lot of work in this area. There are models now to handle this.

MR. VON TILL: Stabilization is very much on our mind, too. A lot of people can use biorestoration, and bringing the concentrations down temporarily, but how long would they stick is a big question.

And we want to make sure that we have done an adequate job of monitoring that for an adequate period of time such that we have confidence that it's not going to be a problem in the future.

MEMBER CLARKE: Okay, for what it's worth I just throw it out. Thank you.

MR. FLIEGEL: The rule will address it. The rule will not define restoration completion as it meets the standards when you turn the pump off. There will be a period of time that the restored well field will have to be monitored.

MEMBER CLARKE: Thank you.

MEMBER WEINER: Bill, you had another question?

MEMBER HINZE: Well, it's an observation really.

In a study of what's going on in the West today where these ISL sites are, there is a lot of mineral resource development in the area, you drive out through there, and there are drill holes, a number of drill holes being put down by the petroleum companies. There are mining activities going on, coal deposits. All of these could have an impact on the gradient of the groundwater.

They could all have an impact on leakage from one aquifer to the other.

And I know there are state regulations

that deal with a number of these things, but I think in developing new regulation, you should at least have in the back of your mind the possibility of mineral resource development, and making certain that the licensee really understands what's going on, and that there is some precautionary efforts made here to not have them interact with each other.

As we traveled to Grand Junction several months ago the number of wells going in was really outstanding. And they weren't far from many of these sites. And the same is true of the coal mines.

So this is something that you need to at least approach in the regulation in some sort of guidance to the licensee.

MR. VON TILL: I appreciate that. That's very timely. Some of the applications we have in Wyoming right now, we have a lot of coal bed methane production right in the area, and we are looking at that not only from a safety and groundwater aspect, but from a NEPA cumulative impact standpoint.

So we are looking at that, and we appreciate those comments.

MEMBER WEINER: Well, having touched on that, I will pick up on that. At the public hearings that were held on this rule, and I don't know what

EPA's take is on this, but at the public hearings there was a great deal of concern expressed about doing a generic environmental impact statement, and that this would in some way obviate or limit what was done regarding site specific environmental issues.

And I wonder if any of you could comment on that?

MR. VON TILL: Well, the generic environmental impact statement was designed to look at the generic aspects of this. But in addition to that there is going to be site specific environmental analysis through at least an environmental assessment, along with public comment periods; so that there is adequate time for the public to comment on these site specific issues, and so that the issues that are not generic, like some of the groundwater issues in particular, some cultural resource type issues, will be looked at through the NEPA process, and we received a number of comments about that. Governor Richardson and members of Congress as well.

MEMBER WEINER: What I think we were looking for as a committee, and perhaps you want to do this at a future time, is to identify what issues you see that are generic as the state found those that are site specific.

Because I think this is what the public is looking for. I recall at the public meeting, two public meetings that I knew about, it would have been - what people were looking for was a list. These issues are generic issues that affect all sites, and these other issues are ones that are clearly site specific.

And I would encourage you to look at that from that point of view.

And staff questions, I know that Latif will have some.

MR. HAMDAN: I want to first defend myself to Dr. Hinze the question was the basis for - I was not expressing an opinion. In Appendix A it mentions minimum of four seasons. 40 CFR 192 on the standards themselves, they say four seasons, at a minimum one year, and it goes back actually to RCRA, for CFR 264, same thing. So this is the absolute minimum.

And just if I could make another point, the point of compliance is actually in the case of mill tailings right at the mill tailings, and in this case it is in the aquifer.

The point that you were talking about away from the site is the point of exposure. So in other words you can have a higher standard, higher number so to speak, in the case of ACL, but you need the primary starter at a point of exposure, just to clarify for the record.

And I only have one question, Mike. Are you going to address in the rule the buffer zone for the ACL? Are you going to put a limit for how much area, or how much distance you will have between the point of compliance and the point of exposure?

MR. FLIEGEL: Yes, at this point in our address we don't have that in there. And there is a difference that there - at a conventional site what sometimes happens is that - especially because of past practices where impoundments were built before requirements for liners and the monitoring and the cleanup as soon as you see contamination; before that all happened there were some plumes at existing sites that have essentially migrated.

And one of the solutions that some of our licensees have taken is essentially to have a bigger piece of land that they could apply an ACL in and have their boundary as the point of exposure. And that's what is currently done in some situations, because the site goes to DOE in the end.

And an ISL it's not a site that is going to DOE, so what will have to happen is that if it's an underground source of drinking water, the licensee will have to make the argument that they are protecting the underground source of drinking water.

And I don't know that there is a mechanism to extend that out. Bill, is there?

MR. VON TILL: No, that's really somewhere, a lot of times, up to the licensee. We have to make sure that they adhere to the requirements of an ACL, or we cannot grant an ACL. And what they have done in some of the conventional sites is, they've purchased additional properties. They've used institutional controls sometimes.

That's up to them. What we have to prove is that it's going to be protective to human health and the environment where people will use that water.

And if there is not a buffer zone, that would steer more towards us not approving an ACL is the modeling doesn't demonstrate that it's going to be safe there.

But we can look at that, Latif. We'll have to work with EPA a little bit, because EPA actually a lot of times makes the aquifer exemption boundary. We don't do that here at NRC. And that in essence would set up the point of exposure, the aquifer exemption boundary, so we'll have to work with

EPA on that a little bit.

MR. HAMDAN: I think you should, because this is really a major issue, in this case an ACL, the ACL guidance is clear, but I believe we are involved - in the ACL we are involved with - a certain area that the licensee cannot go beyond it.

This way, the licensee would not go and buy a lot of land, and could have a lot of area that otherwise would not be contaminated. This is something that needs to be looked at carefully I think. If you read the guidance in the ACL you would find this is the case that you cannot allow a free hand on how much land and how much area you can contaminate.

MR. VON TILL: That's a good point. From a research standpoint you don't want to contaminate a lot of the resource.

MEMBER HINZE: That's the buffer zone that we have to be concerned about.

Latif, how is this point of exposure position determined? What criteria are you using?

MR. HAMDAN: Okay, the main criteria is, the ACL is a standard that is larger than the primary standard. So the point of exposure technically is determined as the point at which this contamination in the point of exposure attenuates to the primary standard at the point of exposure.

In other words - are you with me?

MEMBER HINZE: I'm with you. I don't want to be with you but I am.

MR. HAMDAN: Actually you can think of it as - it is a point of compliance, but it is also a point of compliance location. So you meet the primary standard at a different location which is the point of exposure.

MEMBER HINZE: And that could lead to these very large buffer zones, which could take land out of place and use and so forth.

MR. HAMDAN: Yes, but to complete the answer to your question, that is the primary measure of where the building should be. However, the licensee land is not large enough. Then they cannot take full attention of that. They have to contain it. If the licensee land is too large, the question becomes -- that's where I was coming from.

But the idea is to have the point of exposure where the primary can be met. But that doesn't mean that you can, in my mind, I think the guidance for ACL, original guidance would say, that doesn't mean you go buy all the land you can and have a buffer zone that is five miles.

MEMBER WEINER: If there are no further questions, comments, I'll turn it back to the chair.

CHAIR RYAN: Okay, thank you. That was very interesting.

I agree with Professor Hinze, it's gratifying to see our advice been read and accepted. So thanks, we'll look forward to your updates on future visits. Thanks.

With that we're at a point where we should take a break. And based on the schedule, I don't want to get too far ahead, we will reconvene at 2:45.

Thank you.

(Whereupon at 2:06 p.m. the proceeding in the above-entitled matter went off the record to return on the record at 2:44 p.m.)

CHAIR RYAN: Our last session was also Dr. Weiner. So Dr. Weiner, without further ado, I'll turn the meeting over to you.

MEMBER WEINER: Thank you, Mr. Chairman.

We had a request from NAC International to make a presentation on their views on the transportation aging and disposal canister, and the presentation will be given by Charles Pennington, who is currently the director of NAC's marketing and business development for spent fuel storage and transportation businesses.

And just to go on the record, we have already advised Mr. Pennington as we did the other commercial vendors that came to us that we do not advise the Department of Energy. So we only advise the Nuclear Regulatory Commission.

So having said that, go ahead, Charley, with your presentation.

VENDOR'S VIEW ON THE TRANSPORTATION-AGING-DISPOSAL PERFORMANCE SPECIFICATIONS

MR. PENNINGTON: Thank you, Ruth.

Let me just follow that wonderful lead and assure the committee. I know it will be a disappointment, but this will not be a sales and marketing presentation. I will intend to address in a little bit of a unique way I think from what you've heard previously a technical issue.

This is primarily intended as a technical issue discussion, but is also intended to be a little more than that.

We all know that technical issues do not exist in a vacuum. Technical issues are contextualized by the framework in which we find them. In fact you can see that the context often defines the boundary conditions as it were to what technical solutions may be possible to technical issues.

I've heard in numerous discussions, especially in Miami recently at the PATRAM meeting a number of people expressing opinions about what the TAD system design was going to be or what it was not going to be.

And I was struck by how these people who are outside of the packaging industry felt about TAD systems, and how we, at least some of us, within the industry feel about the TAD system.

So I though I would first of all try to take, and not be duplicative or redundant to other messages you have heard from previous suppliers, but rather try to take a few new issues that have some I believe resonance with TAD development; highlight those; then put those in a special context which I've called other issues and limitations.

Because I think from the committee's perspective you will see that those will play as great a role, if not a greater role, than the technical issues themselves.

So with that little bit of an

introduction, let me proceed with the presentation itself. Please ask any questions at any time. I am not sensitive to interruption or whatever.

I am going to cover these topics. This will roughly serve as the framework for the technical issues that I will discuss, and then putting them in a context, contextualizing them, as it were, into the conditions th8at presently exist in TAD system development space.

I think we need to understand a little bit, so I've got one slide about NAC's qualifications for having input, providing input to this committee.

I want to give a brief overview of TAD system development, then talk about key TAD performance specification issues related to Part 71 and Part 72. And let me further amplify, Part 71 and Part 72 is indeed all that I would be an expert on.

I do not claim expertise in Part 63. I understand some of the interactions at play here with Part 63, and the possible implications for Part 71 and 72 design, but my discussion is limited to Part 71 and Part 72 issues.

Then I'll move into that contextual area that I've mentioned briefly about other issues and limitations that are of a potential concern. And then if there is desire at the end to have a discussion, I'll be happy to do whatever the committee wishes.

Quick background, and I have chosen to present a background in terms of the business organization.

(Telephone interruption)

MEMBER WEINER: Is anyone on the bridge? Excuse me, we have a bridge line, and we need to determine if there is someone on the bridge.

MR. IBARRA: This is the center from San Antonio.

CHAIR RYAN: And who are you?

MR. IBARRA: This is Luis Ibarra. I have with me Asa Chowdhury, Song Witt and Niko Shikan.

CHAIR RYAN: Thank you.

MR. IBARRA: Sure. No problem.

MR. PENNINGTON: Are we good? Okay.

The way to present the background I've chosen here is that we have three basic business areas at NAC.

One is for spent fuel, spent nuclear fuel dry storage systems development. We do that routinely.

We also fairly uniquely ship spent fuel. We have been shipping spent fuel since 1970. We move an awful lot of spent fuel. We've got currently more than 3,600 cask loads of spent fuel that's been shipped. And we have developed a lot of spent fuel systems and delivered them.

So this is our expertise that I want to simply mention for purposes of introduction of NAC to the committee.

TAD system development overview. For a year and a half the industry met very frequently with the DOE and others, and we worked very well together in working through - in fact, having been in this business for a few decades, it was the best interaction with DOE that we have experienced with respect to common objectives, and getting the performance specification contents into what I call the, quote, reasonable range.

I think thanks in no small measure to those activities NAC believes that the performance specification contents directed for Part 71 and Part 72 can be met.

There are a number of things that were put into the performance specification rates with respect to the aging systems, and some of the criteria that they want met that will require additional discussion with the DOE. But uniquely I believe that right approach here is to take Part 72 systems and make them completely different from the aging systems.

Others have expressed a counter view, and I don't really see that right now. I think that that is not the right way to go.

Another point here is that I am going to be talking about issues. I am going to try not to be repetitive. You've heard from others. I'm going to bring up I think four issues that have either not been presented or not necessarily presented in the way I'm going to present them.

So I would move forward with the first one of those issues. The issue of borated stainless. Borated stainless steel is a material that has not been used widely in spent fuel casks for a number of years. Our good friends at Dominion Resources - it used to be Virginia Power - used borated stainless in a number of cask systems in the `90s. Not a lot of use of voided stainless steel currently.

There is - the next issue would be an increase of canister design life. The specification takes the design life of canister systems and more than doubles the industry average as they currently exist.

There is an interesting requirement for a

PWR disposable control rod assembly in the spec. That is curious, because it's a DOE design, and it impacts our design and licensing of the canister.

And finally there are some flow through requirements for the geologic repository operating area and the aging system that as may have been highlighted earlier by a previous presented, may have some impacts on Part 71 and Part 72.

So I am going to touch on these issues straightforward.

Borated stainless, a function of borated stainless is as a neutron absorber of a poison material. The main function in the basket, particularly at the quantities that are listed in the performance specification, is for maintenance in a physical location and chemistry for the long term repository service.

However, Part 71 and Part 72 really control how the borated stainless is to be deployed and fixed in the basket.

So the real duty of this material, the real challenge for the material, is from the repository, but the licensing, Part 71 and Part 72, control how it can physically be mounted and used in the basket.

Now there was a code case for the use of borated stainless steel, Code Case -510-1, in the mid-1990s. That issue has never really been tested with - that code case has never really been tested with the NRC.

The code case is very restrictive with respect to welding. Are there specific designs that may need to do welding of the borated stainless for their particular basket approach? Unknown, but it is an uncertainty in the licensing process with respect to borated stainless.

Again, what load bearing properties would the borated stainless be allowed to take with respect to normal accident conditions in the canister?

Again, uncertain issues.

Finally, because of the severe duty of the voided stainless in the repository, there is a very large volume, a very heavy mass of borated stainless required for the basket.

This has a couple of factors. One, it increases the weight substantially; two, with a very limited canister diameter it restricts flux path and it restricts moderation. So you are going to get - you have a tendency toward under-moderation in the canister design, which can be problematical, and may lead you in certain design directions with respect to the basket.

So that is a quick overview of some of the concerns with respect - some of the issues associated with borated stainless.

Canister design life: canister design life averages about 40 to 50 years in most industry applications. Performance spec calls for a canister design life of 60 years at the power reactors, and 50 years at the aging facility; roughly a doubling, or a little more than doubling, of the design life.

There really is - the only real design life limitation that NAC has ever been able to identify would be in a highly corrosive environment in which the shell, or the welds on the shell, or the welds on the lid, might be compromised. And that literally is out many, many years.

How that is to be justified, defended, demonstrated, with that dramatic an increase in the canister design life is unclear. NAC does not consider it a problem. It only becomes an issue if there is far greater defense of that than has historically been necessary required.

The disposable control rod assembly, and this applies to PWRs only, and it's for fuels that fall outside the post-closure criticality loading curves, which are contained within the performance spec.

The design of that DCRA, how many are required per canister, all of these aspects of the DCRA are not only not final, they are not even identified.

The DCRA as you can appreciate fits into a spent fuel assembly very much like a control rod, either a CEA in a CE plant, or an RCC, rod control cluster, in a Westinghouse plant.

That means it's displacing water. That means that you are again being driven towards under-moderation. And depending on how many of these DCRAs you're required to have, and what some of these special unique characteristics such as an extra thick Zirc cladding, extended poison coverage, a special spider design, all of this is in DOE's design core. We need that for input. We need that very quickly when we move into contract execution in order to decide what fuels we indeed can license. Because that's our decision.

So this is an issue really of, hopefully timing is okay, hopefully DOE's understanding of the DCRAs is sufficiently detailed. But the use fo these

DCRAs may drive basket designs toward either larger flux traps, or flux traps in general whether they have them or not.

Again, the licensing schedule makes finalization critical.

The GROA, aging system flow through requirements from the performance specification, there are a number of system drops, and system handling events, due either to seismic or mishandling at the GROA that are required by the performance specification.

Interestingly the acceptance criteria is specified and stated in terms of leakage rates. Now I think everyone would understand that when you start talking about leakage rates you by definition have to have a hole size or a crack size or a leak size in order to begin to make that determination; and the analysis methods that are used with the code, design codes allowed, do not give you anyway to do that.

So there are some discussions that need to go on amongst the vendors and the DOE on exactly what this means. We have a strategy on how this is to be done. We have a way to look at this we think that will work.

But this is, as mentioned I believe by one of my predecessors here, this could have some impacts on the canister designs for the storage and transport. I personally don't think it will, but it could.

At any rate it's one of the things that I think can be resolved with discussion, but it needs to be resolved, and it needs to be resolved very clearly, because this will be an issue that might come up in licensing space even under Part 71 and Part 72.

Now I mentioned earlier that technical issues don't stand by themselves. Technical issues are indeed rarely the control and technical organizations that do not understand the context in which they are developing technical solutions are in great peril of failure.

So I want to talk a little bit now about some of the context that surrounds us with TAD system development, because there are some significant boundary conditions I would call them that will impose types of technical solutions upon us.

First of all we have a number of unresolved transport certification issues, and the committee has heard about these issues. NEI and others have made a number of presentations on these issues. I won't bother to dwell on them. But high burnup fuel cladding properties, burnup credit, moderator exclusion, all are issues that weigh in right now.

And the key point is that while making progress on these issues, they are still works in process. So we are not at closure on these issues.

Now DOE intends to proceed in the near term on TAD development, which it would be hopeful that they would. Time has really expired for applying any new resolutions. Because once the flag drops, there is a very crisp schedule, and a number of deliverables that have to be met, and time for working on resolution and new approaches just won't be there.

So in my view, I think industry must proceed with the current situation with respect to any of these issues in resolving licensing issues for transportation. And by the way I'm going to get to some ideas in the end to suggest how industry might go about doing this.

Another big issue is what we call confirmatory testing that is needed for transport certification with the NRC. Confirmatory testing is not testing for licensing; it is simply testing that confirms the analysis methodologies that you have used with the system designs that you are trying to license.

This testing of cask systems is time consuming and costly. Testing facilities, how many of them are really out there? Well, right now I would only be willing to say that Sandia National Labs is available. There is not a lot of testing that goes on; therefore there is not a big business in testing; therefore there is very little incentive for people that have testing to maintain their testing capabilities.

And make no mistake about it, drop testing is a craft. And it's an important craft, and it's very difficult, and even more importantly, and I mention this in the next bullet, they need to be audited and fully approved supplies under vendors - approved vendors.

In other words they have to be a quality supplier with an appropriate QA program. I would dare say there is no supplier of casks out there that right now has an approved vendor for cask drop testing on its approved vendors list. Just my guess, but I would be willing to hazard that guess.

Next, those of us that have done extended testing and a lot of testing over the last seven years are aware of the expanded nature of NRC requirements for cask drop testing. In the 1990s cask drop testing was really limited pretty much to assuring that peak G loads in the analysis were matched by peak G loads in the testing.

Today that is not the case. Today it's a matter of a time-history G loading in the actual drop test that matches a similar time history analysis curve from your own codes that you are applying to this.

It's a very rigorous approach to confirmatory drop testing.

What this says is that phantom work cask confirmatory drop testing for certification will be a time consuming process, especially if there were, let's say, only one supplier offering that service, one supplier that has a QA program, one supplier that have been approved.

And another issue that provides the context here for our technical issues, the DOE licensing requirements and their licensing schedule, and the current delays that we seem to be experiencing in deciding to proceed with the TAD development.

Under the current DOE procurement documents, vendors bear all risks of NRC licensing including schedule. We all know that new designs, new system designs, are most challenging both for vendors and for the NRC review staff with respect to schedules. Anything new and novel requires a very thorough review. And so from a schedule perspective, that is challenging.

One doesn't have to think too hard about this to see that if there were four suppliers of TAD systems, each providing at approximately the same time, within weeks of each other, an application for transport certification, and an application for dry storage certification, all of those on top of everything else could end up taxing the system. That is a very likely scenario with respect to what we see the DOE approach to be.

And finally we are now about approaching three months behind the schedule that we anticipated, and it's getting to be a critical proportion, even vendors have to do something with resources. So the resource availability becomes a concern the longer this carries forward.

All right, so now let me try to put a little bit finer point on the context issues I've raised here that surround the technical issues.

First of all all these context issues I'm sure have been looked at as carefully by the other supplies as they have by NAC. Our reviews and reviews in general lead you to consider the following things.

Well, let's get rid of as much originality as we can. Let's make this TAD canister just about as much a duplicate as anything we've ever done.

Storage and transport: the next consideration that might come up is, let's really limit ou8r spent fuel. Let's make sure that we are not doing anything challenging with respect to licensing space. High burnup fuel and burnup credit issues being the way they are, if licensing is going to be an issue, licensing schedule, then let's be very careful about what we select to license.

Consistent with the abilities of outside contractors to provide testing services, one could say, okay, let's make sure that not only our canister but that our transport cask is bounded by recent testing that we have already done.

Let's get as much - let's get as close on a transport cask design impact, limit our design to current designs as possible, so that we restrict the need for this testing with respect to schedules.

And finally even one might consider that, let's make the TAD system a simple amendment to current designs that we have already certified.

So those are some of the implications that vendors might consider in looking at all of the context of the technical issues that we face.

So real quickly, I believe that the performance specification issues for Part 71 and Part 72 can certainly be solved.

We are looking at them, even the flow through requirements, we've looked at approaches that will address some of the grow up pass through requirements; believe those can be done. But again you are never certain until you've talked with the DOE and then met with the NRC.

So it's really from NAC's perspective these other issues and limitations that are more troubling. How shall we view those? And how shall we use those to bound and restrict o8ur TAD development so that it is likely - not only likely but highly likely to be successful in the time frame that DOE expects?

And finally with the schedule delays that we are experiencing with the DOE, I would say that most importantly time is of the essence. The schedules that DOE had laid out originally are aggressive. With the delays we've got right now they become even more aggressive.

So we believe that it's time to make our moves, and make them very quickly; start resolving not only the contextual issues but the technical issues.

Lady and gentlemen, that's my quick and very - I would apologize, it's not superficial, but it did not have a lot of technical detail. But at least it highlighted some of the issues that we in the industry think are important.

MEMBER WEINER: Thank you very much, Charley.

Jim? Mr. Chairman?

CHAIR RYAN: No, I didn't have any, thanks.

MEMBER WEINER: Allen?

VICE CHAIR CROSS: No, thank you.

MEMBER HINZE: Well, I won't leave you hanging.

How do you change the canister to achieve this doubling of the lifetime? You mentioned the corrosive environment, particularly on the welds. What needs to be done?

MR. PENNINGTON: Nothing. There is so much reserve margin in canister design as it exists. We have never really been able to find anything that limits the canister design from a lifetime perspective.

In fact we've only really taken any

analysis out for a very long time, and that would be a canister in a very aggressive marine environment. And even at that point you'd get, and you are clearly looking at thinning, thinning of welds, thinning of shelves and things of that nature, you are still looking at canister design lives of around 200 years before you would compromise your accident analysis and lose confinement containment of the materials inside.

So the real question is not what we have to do to the canister, but what we would be required to do for licensing space to demonstrate a rough doubling of the design life.

That's the uncertainty. It hasn't been done this way before.

MEMBER HINZE: Is there anything that could be done with the welds to improve their quality and their lifetime? Is there - is there any robustness that one can add to the welds?

MR. PENNINGTON: This is an opinion, and I'll express it that way: I believe the welds are actually as high quality as anything else, because you are using materials in the welds that are actually better than the canister itself.

MEMBER HINZE: It isn't the material. It's the weld.

MR. PENNINGTON: Well, the weld itself is so redundant and so I would like to say it is well tested. Now it is not as well tested of course as if it were a code weld. And this is just a lid weld. The other welds on the canister are code welds.

But it is so well tested and so progressively tested and since we've never really had a failure of one of those welds, I think the experience is that it's a good weld.

Now if you wanted, the way to make it fully high quality would be to use either a UT or an RT method of inspecting that at the plant. That becomes horrendously expensive, and I could virtually - I don't want to speak for the utility community, but that would be very difficult for the utility company to take.

MEMBER HINZE: How can you improve the testing or calibrate the testing so that one can achieve a validity out to 100, 110 years? How do you improve the likelihood of extrapolation? And what happens to the uncertainties as you extrapolate out?

MR. PENNINGTON: Well, let me just walk you through what I consider to be the life of a canister. You've got a canister that is closed, and it's been placed into service. And everyday that passes from a thermal and a radiation perspective, its life gets better and better. If the heat load goes down, source term goes down. So the threat diminishes, any threat from either temperature or radiation decreases with time.

The historical analysis work on canisters shows such high margins for anything like fatigue, thermal cycling, anything like that, such high margins that you really - there really is not a need to try to say, okay, instead of a 3/4 inch or ½ inch canister, I'm going to double the fitness. I'm going to up the weld thickness by this amount.

There really isn't - you are already talking about margins that are sufficient, so that you are not going to really threaten them very much by essentially doubling the life.

But again the very questions you are asking might be the questions that the review staff asks, and says, okay, do something different. I don't know what they will ask, but that is an uncertainty that presents itself to any vendor going in with a new design requirement.

MEMBER HINZE: Going to your slide #10, referring to the 3 g seismic peak acceleration design requirements, Kennedy from DOE was in last month and discussed some of the recent work that the DOE has done on preclosure. And it struck me in reading the transcript on that, I missed it at the time, but he was talking about the 3 g being important because they didn't want the aging casks to tip over, because the ends of the canisters were not as adequately protected from radiation as the cylinder.

Can you help me with that?

MR. PENNINGTON: Yes, I think what he was saying loosely was that the lid thickness of the cask is not historically as much as the shell thickness, skyshine is such a small component dose. I think that's what he must have been trying to say. But again you can make it as thick as you want. You can -

MEMBER HINZE: So one can solve this 3 g by doing something to the lids? Could you?

MR. PENNINGTON: No, I think what - at least as I understood what you said to me, he was saying, well, we don't want them to tip over because we are going to get higher shine out of the -

MEMBER HINZE: Yes, right.

MR. PENNINGTON: Well, I can fix that.

MEMBER HINZE: Well, the top and the bottom.

MR. PENNINGTON: if that is all you are worried about, I'll put 10 feet of concrete up there.

MEMBER HINZE: That can be fixed.

MR. PENNINGTON: That can be fixed. So I think, and I don't want to put words in his mouth -

MEMBER HINZE: No, I'm just trying to - because you have obviously looked at the 3 g sites -

MR. PENNINGTON: Yes, and 3 gs is not a problem. And keeping it from tipping over at 3 gs is not really a problem. It can be done. It's going to be a hellatiously, horrifically expensive cask, but it can be done. It certainly can be done.

I think, and I don't want to speak for everybody else in the industry, but I think we would prefer to see tie-downs. They don't want to go through the inspection of tie-downs. Okay.

But this is certainly a kind of dialogue that we need to have with the site people in coming up with, okay, what's really important here? Can we combine a new design idea with perhaps a performance specification modification? There are certainly ways to go about this to solve these issues.

The g loading in 3 gs is nothing for these canisters. It's not a threat on that. It's only a threat on tip over, and you don't want it to walk off the pad. So you are going to avoid cylindrical bottom plates. You are going to do something else.

But you will be able to solve these issues, expensively, but you can sure solve them.

Now your point is well made. Okay, if all you are worried about is a tip over, we'll put you a real thick lid on there and there won't be any shine off the top.

MEMBER HINZE: Thank you very much, Mr. Pennington.

MEMBER WEINER: I just have a couple of questions. Normally, or recently, in recent environmental impact statements, no credit has been taken for the TAD in looking at the risks of transporting spent nuclear fuel.

Could you comment on what the influence of the TAD would be on those risk assessments, if credit were taken for the TAD, or if the TAD were included in the risk assessment; any TAD.

MR. PENNINGTON: Well, I would certainly be happy to do that.

The TAD provides in essence double containment. The transport cask will be licensed as a containment boundary. We can assure by both analysis and test that the canister will not leak. It won't leak; it won't let water in either. So that's a different issue.

But the point is that you have double containment system. I don't believe that there really is no event that I can conceive of in the regulatory framework, present regulatory framework, that would cause failure of either containment system, let alone both.

So the risk assessment has to come down to your beyond-design basis assessment, and I'm not sure which environmental assessment you're talking about, and whether they took into account beyond-design basis events or not.

But beyond-design basis events, there really isn't, if you shift - if you were to use an beyond-design basis event analysis for instance, if you were to shift away from code allowable to shift to really an ultimate strain or ultimate stress or ultimate strain criterion, you can do almost anything to these casks and they won't fail.

If you look at a simple stress vein curve for simple stainless steel, and you look at the amount of that area under that curve that the regulations allow you to use, you are going to see that you have a factor of 10 safety just to ultimate on any of the materials you are using, including the weld material.

So I think that the presence of the TAD canister, if properly assessed, increased - decreases the risk order of magnitude, more, something like that, for releases, for releases or radioactive material.

MEMBER WEINER: Thank you.

Other questions or comments from the audience? Hearing none, I turn it back to you, Mr. Chairman.

CHAIR RYAN: All right. Well, thanks very much. We appreciate your time and presentation.

With that, we are at the point where we are going to take up a discussion of our letter writing activities, and I guess we can probably take a five-minute break before we initiate that session and get ready for that.

So thank you all very much. And we will close the formal part of the record today here, and we can be in five minutes or so.

(Whereupon at 3:19 p.m. the proceeding in the above-entitled matter was adjourned.)