UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

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FALL 1997 BOARD MEETING

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Hyatt Fair Lakes
1277 Fair Lakes Circle
Fairfax, Virginia 22033

Thursday, October 23, 1997

The above-entitled matter commenced, pursuant to notice at 8:30 a.m.

BOARD MEMBERS:

JARED COHON, Chairman, NWTRB
DANIEL BULLEN, Presiding
JOHN ARENDT
ALBERTO SAGUES
NORMAN CHRISTENSEN, JR.
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KARYN SEVERSON
DANIEL METLAY
SHERWOOD CHU
LEON REITER
VICTOR PALCIAUSKAS
FRANK RANDALL
PAULA ALFORD

ATTENDEES/PRESENTERS:

THOMAS DOERING
HUGH BENTON
JERRY COGAR
KEVIN COPPERSMITH
DAVID SHOESMITH
JOHN SCULLY
JOON LEE
DAVID STAHL
KLAUS KUHN
CARL PETERSON
STEVE HANAUER
GEORGE DANKO

ATTENDEES/PRESENTERS: [continued]

TAE AHN

JOE PAYER

P R O C E E D I N G S

[8:30 a.m.]

DR. BULLEN: Good morning. Could we all take our seats, so we can get started on time, please?

Good morning. My name is Dan Bullen, and I'm here to welcome you to the second day of the board's meeting, and I wanted to make an anecdotal note this morning that our chairman is here somewhat happy, so we can at least verify the outcome of last night's World Series game.

I have the honor to be chairman of this morning's session on waste package design, and as you know, waste package design has been an extremely important component, particularly in the early life of the repository.

Waste package is also an important cost component, and cost is one of the issues that has be addressed in the upcoming TSPA-VA and all of the VA analysis.

I think waste package, taken as a whole, will actually cost more than the repository underground facility. So, we have to keep the cost in mind as we take a look at the evaluation.

It's been about five years since the old thin-walled, low-capacity, bore hole and place waste package concept was superseded by a robust, high-capacity drift in place concept. The robust concept brought greater performance.

Arguably, it also opened the door to heightened heat transfer and criticality concerns, both of which will be addressed in today's session.

First, Tom Doering is going to discuss the current design. In view of the fact that about 70 percent of the board is new this year, we've also asked Tom to bring some history into his presentation.

Tom is a civil and nuclear engineer and manages waste package design activities. He is with Framatome Cogema and has been on the project for six years.

Our second speaker will be Hugh Benton, who will discuss criticality analysis. Hugh has spent almost 30 years in the Navy, mostly in the nuclear propulsion program. He is the manager of waste package development at Framatome Cogema. As such, he manages some 30 scientists and engineers and supervises people including Tom.

Our last speak this session will be Jerry Cogar. He will discuss progress on how the waste package will be made, closed, and closed securely after loading spent fuel into it and how it will be inspected.

Although I understand these topics have come up in a few previous board meetings, this is the first time a full presentation has been devoted to them. Also, Jerry has not made a presentation to the board before. We'd like to welcome Jerry. He is also a Navy veteran with some 28 years experience with Babcock & Wilcox, where his last position was superintendent of fabrication operations. He is the manager of engineering development and also reports to Hugh.

Without further ado, I'd like to turn the meeting over to Tom Doering.

MR. DOERING: Thank you very much, Dan.

As has been noted, we've been working on the program now for six years as the M&O first came on in 1991, and as has been noted, we'd like to go over a little bit of the details of what our design is today and go over a little bit of history and where we came from and how it's developed.

Again, we will talk a bit about the waste package and what we call the engineered barriers segments of the EBS or the engineered barrier system.

This is the engineered part of the activities, where we're dealing with what can we do in the engineering world to help isolate and then retain the waste before it goes to the natural system.

Jack Bailey talked a great deal yesterday about the different options and everything, so we saw a little introduction to that.

Dan McKenzie spent a very good time outlining where the repository is, and now what we want to do is go into -- the engineered barriers segment is the design side of it, and what we talk about first is the current design, some of the costs, because it really folds right into it, the history a little bit, where we came from and why we got there, and then also ending with what has been tried and where we plan to go, again reflecting a little bit more on what we heard yesterday from Jack Bailey and some of the things that we're doing there, and I was also asked to talk about some thermal and structural evaluation.

Hugh will talk directly toward criticality, but there's a difference, a little bit. The engineering side will implement the methodologies that are being developed in the methodology group that we have, we sort of organize around that.

So, I will address criticality a little bit but not in that much detail in how engineering does it.

Just to start off with -- I think we've seen this before. These are the basic waste forms that we have to deal with.

I'd like to start here, because this sort of maps right into the different waste packages that we have, and so, what we're looking for is pressurized water reactor, boiling water reactor.

We also have -- not classified waste. Somehow they don't like to have glassified or litrified, and the spell-checker in the system turned it into classified, but that is glassified waste. That's the pour canisters from Savannah River, from West Valley, and also from Hanford.

Now, Savannah River and West Valley are the same size. Hanford, similar to South Texas, decided to get longer, and they are 15 feet long. So, you'll find a waste package specifically aimed at them, because they came out with a larger pour canister.

Other DOE waste forms -- we are looking into them right now. We actually have a program in place that go down -- marching down the path of looking at three to four this year and roll into next year also.

Then we also have the Navy fuel on the outside of that, because it's a unique situation where this is classified activities, and those will be held out differently, and the packages are different and larger sizes for that.

So, those are the waste forms that we have to deal with. We have no opportunity to say no, we don't want the waste forms, so we have to deal with them.

So, what we're dealing with -- these are the waste package designs, the basic waste package designs that we're dealing with.

We have the uncanistered. That is when there is uncanistered fuel. There's a sense, when the utilities send us the fuel in shipping casks, they're unloaded and put into a waste package. So, they come uncanistered, they come as they come out of the reactor itself.

The canistered spent fuel that we're looking at is what would happen if we would get something like a multi-purpose canister or a dual purpose canister that we can take in and dispose of directly, and we're working with the industry to understand their design styles and having to work them into us.

We have the defense high-level waste. Again, that's the Savannah River, the Hanford, and the West Valley design, the pour canisters. They're around 24 inches in diameter, 10 feet long.

And then we have a whole host of DOE spent fuels. Some looks just like H.B. Robinson -- in fact, it is H.B. Robinson fuel -- and some are as much as the TMI level in little jars here and there.

So, that's the whole spread of waste forms that we have to deal with, and that will actually be -- if it's H.B. Robinson, it will go in one of the regular standard containers that we have. If it's like TMI, highly-enriched material, they'll have to go into a special design for that.

Again, the canistered Navy fuel will be coming in, and the canister has the same concept as the canister of spent nuclear fuel, essentially that we bring the canister in, we over-pack it into the barriers that we're dealing with.

Now, what were some of the drivers of choosing the waste packages that we have?

This year, we went through a great deal of effort looking at all the waste forms there. Until recently, we did a viability assessment through advanced conceptual designs, we said we were going to handle 90 percent of the fuel out there and see if there's a natural break there.

The thing that we were very concerned with, being good engineers, is that we don't want to over-design the package, and if we would design to the 1 percentile of the waste forms -- the worst waste form -- we would over-design the packages.

They might be a million dollars a copy, but you don't need that kind of neutron-absorbing materials, that heat rejection for all the waste forms that we have.

So, what we did this year is take a look at the criteria that we're looking and what is logical to put into the design side.

What we have from the Nuclear Regulatory Commission -- and this set the precedents on the storage and transportation -- is that the waste forms have to be coupled to the waste packages.

They will get the requirement that they're coupled and then you are allowed to load this type of fuel into this kind of package. If you go astray from that, you can't load it.

So, both the size and enrichment -- cool time is very important to us.

We heard about that yesterday, that the earlier years, the first 60 years, you get rid of a lot of the ceisiums, the cobalts, and all that, and that's the heat generation, and again, this is where some of the activities we heard yesterday about ventilation -- it's similar to aging, because it removes some of the heat.

We're also looking bounding. We're not looking to design individual little packages. We don't want 15, 20 different packages. We'd like to have a main set of packages and sort of the outlier packages.

So, we are looking at design bases, fuel design bases components of it, and each waste stream, again what we talked about earlier, will have its unique licenses, will have unique characteristics to it.

And the requirements we do have to address. The TMI fuel that we get in is different form, they have different requirements, different needs than the regular pressurized water reactor fuel. The Navy fuel has different requirements. So, those things we have to be sensitive to, also.

So, that's where this comes up, the waste form and how it behaves.

Now, to do this, we went off and did an evaluation of the waste stream, and there's a lot of different scenarios that you can go through.

In fact, we looked at 12 different waste stream evaluations that we worked with the M&O back in the east who is responsible for doing this, and so, what we did was look at the different nomographs that we can pull off of that and see where a decent break point was, and what we looked for is the package that we would like to design to capture thermally 90 percent of all the fuel, also criticality about 90 percent, also.

There's a clear economic break. At that point in time, it sort of gets more splintered.

[Slide.]

In fact, a slide that's not in your handout shows the scatter that we have. There is a lot of scatter on just the light water reactor fuel in here.

So, what we try to do is kind of cordon off a corner of this and say that will simply go into the waste packages without any todo, without any separation, and the outliers will be handled separately, and actually, the line that you see here is actually a K-effective line. Before, we always showed K-infinity line.

This is our waste package, our 21-PWR. It will capture 96 percent of the fuel for pressurized water reactor out there, and this is using what Hugh will describe more in detail, our burnup credit methodology. So, there's a lot that we benefit from that.

If we didn't have burnup credit, by the way, that line would go straight up, so it buys a lot.

Anyway -- so, those are the considerations we're dealing with. So, what we're dealing with the 90 percentile, so it goes in nicely.

The word "nominal" can't describe the average, because it is a distribution, and so, we look at the -- what is the design bases, and from there, we go down.

So, it's hard to design to an average design. If you design to the average, that means half of the waste form can't go into the package.

What we're dealing here is why did we choose certain sizes that we chose? Why didn't we chose a three-pack or a four-pack?

Well, back in '93, when we first started off doing the parametric evaluations, we said what is the logical size of the packages that we'd like to deal with? There are some natural packing densities that you can deal with.

Lo and behold, the 12, 21, 24 is not bad either, but now you jump up all the way up to like 32, are the natural packing densities that you get the most fuel in the package with the least amount of wasted space.

The least amount of wasted space is very important to us. It does a couple of things.

The package is smaller. It doesn't carry as much weight to it for that.

Also, if we're looking at disposal criticality issues, now there's less cavities, open space inside of it, so you don't have as much water holding capability of it, and again, being good engineers, we look for the dollars/KG disposed, and so, those numbers give you good dollar per KG disposed.

So, those are the numbers we landed on. We felt that 12 is a good size. We also felt that 21 is a good size due to the thermal reasonings.

[Slide.]

Now, the next slide shows you the whole set of packages that we're dealing with, and what we'd like to do is walk through a little bit. We can actually spend a lot of time on this one. We can go visit this one a little bit again.

What we'd like to do is show -- is that the evaluation that we did this year was very comprehensive.

We looked at not only the cost of the individual packages, but what is the total cost to the system, and then what can we gain out of the system by doing this, and one big thing that we are bringing into the system is that we're looking at thermal credits.

We're taking credit for the two intrinsic characteristics of spent nuclear fuel: it comes with radiation, it comes with heat, and from that radiation, that's where burnup credit comes from. That's the whole key into it.

So, those are the two things we have to deal with, and through the whole process, what you saw yesterday and also today, we're taking pretty good -- I think -- pretty good benefit of that.

We're trying to bound it thermally with the heat, and then, from the radiation, we're having to deal with it, but we're also looking at burnup credit.

The first three designs are all identical designs. All they are is different makes of it, different options of it.

But have no absorber. The real interesting thing is, if we take burnup credit, when we get that put in place, there's over a third of the fuel out there that simply cannot go critical again. There is no additional neutron absorbers needed for that package.

Neutron absorbers cost on the order of $100,000 per package. So, if we can save $100,000 per package, we will have this option available to the management to implement.

Now we had some that need absorber plates, and so we have a design of the absorber plates and we have the neutron absorbers.

Also, there are some that have more criticality potential than the plates can handle, so we've gone to putting rods in some of them. There are very few of them, as you can see.

Now, again, the cost -- on this one, unit costs might be similar, but when you multiply it by the number of packages that are anticipated, there is a significant savings.

So, that's the key. It's the combination of the number of packages and the unit cost will give us the savings.

But as we walk down here, we see that we also have number two with the no absorber for the pressurized water reactors that are thermally too hot, and by the way, these will take the South Texas.

That's essentially the South Texas design. They wanted a higher linear heat rate, and so, that's where you see this one, and this is actually the one with the plates in there, will be ones that, again, were rejected early, the assemblies, for some criticality issues.

So, similar with the boiling water reactors, we have a similar A and a B there. We also have a smaller package, a 24 boiling water reactor design to handle, again, the outliers, and you can see there are not that many of those, but we do have to handle those.

We looked at -- instead of having a different package to it, we also said what would happen if we would de-rate the package that we have? Well, if you look at the cost breakouts, we would have to have a separate argument to the NRC for that cost breakout.

If we start blocking off regions inside the package, then it would cost actually a little bit more from our cost evaluation than making the package that fits the size that we want, because again, you can't load fuel just haphazardly. You have to have a license, you will load this fuel into this package and nowhere -- and no other variation of that.

CHAIRMAN COHON: Cohon, Board. I'm sorry to interrupt. I have a specific question to help me understand this table. Are concepts one, two, and three different ways to dispose of the same amount of waste, or is concept two completely different for a different waste stream than concept one?

MR. DOERING: Ninety percent of the fuel will fall into concept one. Ten percent is required to handle the pressurized water reactors.

CHAIRMAN COHON: Different pressurized water reactors than the ones for concept one.

MR. DOERING: Right. They might have the same geometry, but they have different burnup and different enrichment characteristics. So, what we did, again, is try to minimize the number of options that we're dealing with.

So, we actually do have only right now 10 options, and you'll see that the commercial spent fuel goes down to six. As we go into VA and to LA, essentially we will also take a look at the different waste streams again to show that this is the right way to go.

We do have the number six down here, the commercial canister fuel, the Westinghouse design, the 44 BWR, that is that we took a look at it very quickly, and it sort of represents what size packages that we're dealing with, and you can see, we don't have the number of packages nor the cost of those, because that is still outside of our realm because that hasn't been given to us yet, because that's the commercially developed device and we're still working with the commercial vendors on those.

We have the defense high-level waste, again Hanford having to be different, we have the longer packages, and then we have the Navy fuel -- we have also two styles of the Navy fuel, where we have the standard or the short and also have the long, and then we also have the DOE spent nuclear fuel, the low-enriched, and there we don't actually -- we just started to work on the designs of those.

Now, that sort of briefly outlines where we are today in a sense that why did we come up with it?

Well, we came up with these designs because the waste stream is such that it breaks it down very nicely into these categories and also because it's cost-effective from a just fabrication point of view, also from a licensing and engineering point of view.

This is a variation of the theme. We can do a very economical design of that and just give options and put that into the SAR.

Now, I think we've seen this before, but I want to go visit this a little bit. There's a transitioning now into how do the packages look today, and we go into the history a little bit at the same time, and so what this is a representation of is engineered barrier segments.

This is the pressurized water one, this is the defense high-level waste, and you see sort of a brown in here, that would be co-disposed DOE waste, and what we have planned right now is to try to do it that way for the majority of the DOE spent fuel, and then we have to take a look at how that falls. It's unique fuel. We don't know if it will fit inside there. There might be another package for that. And then we have the boiling water reactor style.

As you see, they're very similar in design, they're very similar in diameter, so the cost of manufacturing is kept minimal, but they are designed such that they have independent failure mechanisms, and also, we do see those little supports right now, which is a very interesting design that keeps the pod package off the drift and keeps it away from the concrete for a while, so we don't have the hot spot in the package, also not the hot spot in the inverted concrete, that we start burning the concrete out early. So, that does it for those reasons.

Also, when you transport this package in, it's very heavy, and we don't want to damage the package, nor do we want to damage the liner inside the invert.

We use a design such that these are the failing points, where these can be replaced really easily, they're modular, just pick one up and throw it away if you need to.

So, if anything goes wrong, any off-normal event, those will take up the load, and so, we can put those back in, they're modular, because the support -- the pier and the support are separate, you just put them together, it's very straightforward, and they're all designed to -- right now, those piers and supports are designed to handle two to three hundred years, that's the design life right now, and that's for viability assessment. As we go into license application, we will look in more detail at what that design should be.

Now a little bit on the design of the waste package itself, I will show you the pressurized water and also the boiling water reactor. They're going to be very similar in design concept. There was no reason not to continue the design concept.

First, let's take a look at the environment. What are we having to deal with? This is what's in the requirements documents and in our assumptions document that we're dealing with.

We're dealing with 80 to 100 MTU per acre. So, what we're going to do is keep the container above boiling as long as we can to stop the corrosion activities for as long as we can, relatively low humidity in that timeframe, but we understand -- we also looked at the curves that we have there, we have to deal with that.

We do have water that can come in later in life, and we understand that the edges and faults we have to deal with a little bit separately and mode differently, and we understand that.

Water in the vicinity has been given to us as a designer. They're looking at -- all outside that boundary. From a designer point of view, this is what has been given to us to at least work within that boundary condition.

Now, the materials a little bit, and we'll go into just a little bit of that. I think David Stahl will go into more detail of that later on.

What we're doing with the design right now is a defense-in-depth design, where have the A516 on the outside. It's 10 centimeters of that material, and then we have a corrosion-resistant material, a high-nickel material that's now listed as 625, and a further evaluation is going on with that, and this is our baseline design.

We heard yesterday a little bit that we're looking at different concepts also, and we'll talk a little bit about that a little bit later. But the design is such that you have a defense-in-depth. Those two materials have different failure mechanisms.

And also, the carbon steel can be predicted very nicely, the general corrosion rate, and it can help us in the performance assessment.

The corrosion-resistant material has a different failure mechanism with less pitting and stress corrosion cracking, those natures, and it's more probabilistic, and so, we have two barriers that fail differently from different causes.

Now, as we go into the baskets, the basket looks very common for me -- I don't know if we can spend a lot of time here either, but it's a very simple straightforward design where you have an egg-crate material that simply slides together, and that's a stainless steel boron if we need the neutron-absorbing material or it's just simply stainless steel.

That simply slides together, forms an egg-crate, it's not welded, and what we do then to gain the strength for the surface and the handling facility -- we slide tubes -- and right now, they are carbon-steel tubes -- in between that.

That provides us our strength for the surface, and also, it provides us good thermal characteristics, and also there are some thermal shunts in here that remove the heat that's in between the stainless steel egg-crate, so we slide some thermal shunts inside there.

It's a very simple design, very little welding required inside the baskets, but significantly stronger than other baskets that we see, and also is very thermally competent, essentially, removes the heat very nicely, doesn't provide any kind of insulation, it really moves it away very quickly in that area.

So, it's a simple design, but each component has a lot of requirements that they have to meet, and that goes to the defense-in-depth slide.

So, each one of them -- each component actually serves more than one function.

The basket itself, the carbon steel -- when we first proposed carbon steel, we all looked at each other and the purist people said no, you don't want to do carbon steel, it doesn't last. We said that's the point. We want it to corrode, we want it to grow in volume.

As it grows in volume, it can sort of help displace or preclude any moisture from getting in, helping us with criticality.

Also, we've also learned through some performance assessment activities that the carbon steel, the iron, likes to hold on to some of the heavy metals, the neptunium, likes to combine with this at a later time. So, we've solved a lot of issues or helped a lot of issues in that same sense.

So, there's a lot of defense-in-depth in this area, a lot of components that do the same thing, and the stainless steel boron, which is a neutron-absorbing material -- the stainless-steel boron was chosen because it doesn't fail through general corrosion.

It fails through cracking, through pitting and things of that nature.

So, as the material breaks and sort of shards come off, it still has the boron embedded in it, and we over-bore the panels right now at 20 percent because we understand, as the material breaks, you do get some boron on the shards, and that could be dissolved, but still, it's carrying the neutron-absorbing material where you want it.

So, combining the neutron-absorbing material with the burnup credit, with the geometries, again it's building in -- you know, it's defense-in-depth in the design, and we understand what a safety margin is.

The thing that we've also done -- we'll talk a little bit later -- the development of burnup credit, there's effective thermal conductivity, and also a less placid design, so we can go to the regulator and say we know what a nominal case is as engineers, we know where the nominal is, now let's put the factor of safety on, instead of building the factors of uncertainty into the design and then not knowing where we are in the safety margin.

It's very important to us in the design group that we know where we are in the safety margin, and from there, we can make a logical decision.

The boiling water is a very similar design. You have interlocking plates and similar other support structure guide right now. The support structure guide is there simply because we have square things going in round holes, and we've got to make up the difference.

Now, that's the design as we stand today. We believe it's a very simple design. The design processes are underway right now. We've done a lot of evaluation thermally, structurally, and also criticality-wise, and we've done a lot in that area.

So, what we'd like to do now is talk a little bit about the history. This is the one slide on the history that I have. There's a lot more that we can talk about in this area. I think just with the time that we have --

In 1988, when we first got started in this area, there was pre-conceptual design concept, what might it look like, there was bore-hole design, thin material, less than one centimeter, actually one centimeter of material that was stainless steel.

That was later changed to an Inclenal-825 that had absolutely no criticality control material in it.

They had talked about consolidating rods, which would help you during the early timeframes for criticality, but again, if you put more material in there, it precludes any kind of moderator to get in, but the problem was with long-term.

These materials will break down, bore-hole design, everything will settle to the bottom, and the probability of showing that we won't have a criticality is going to be more difficult.

So, what we've done there is moved away from a design that we know we have a criticality issue, we have a performance problem, also a heat problem, very interesting.

The bore-hole design had much fewer assemblies in there, had three PWRs and four BWRs, was the maximum design they had, and it was actually -- it got hotter inside the bore-hole than the 21 pressurized water reactor in the drift.

Again, we talked a little bit about that yesterday. That's because the radiant heat -- it can reject the heat into the drift very nicely, and I'll show you pictures later on of how that really helps us.

So, that was a big step from when we went to the '88 design to the '92 design that we actually defined the advanced conceptual design.

This is where the larger packages that came in. 1991 was the first time we proposed the larger packages and started to look at them, and again, the dollar per KG disposed was very favorable in this area, also, and also, this design provided us -- the robust multi-barrier design provided us a longer-life package early on, where the natural system was thought of as being the complete carrier of the performance. That's where this design came from.

Now, as we understood the system a little bit more and understood that we are bringing in a defense-in-depth philosophy, we looked at what we can do in the engineered barrier segment.

Multi-purpose canister was a large influence on us. The utilities, again, are looking for packages that they can put on the site and can ship to us and then becomes

MPCs and we simply over-pack.

They're not looking for smaller packages, the onesie, twosie kind of thing. They would like to have the larger package they can simply put on the pad and have a few of them. So, that was a large influence on us.

We did complete an advanced conceptual design, and that design was reported back in 1996. It had, I think, about 1,000 pages of design description in there and detailed calculations evaluations that we did, and it actually -- it looks very similar to the design we have today.

We have changed a few materials, we have learned a lot from the evaluations we've done, and so, we saw some material change in the slight internal basket modification.

We now work on the viability assessment design. That's what I showed you today. That's where we are today.

This is what remains to be tried. This is something that Jack Bailey has talked to. You can see that the management and also engineering is sort of working in tandem.

We are looking at drip shields and ceramic coatings on the package. We have done some calculations on those.

Structurally, we still have to take a look at what kind of material will last and survive, but we had good positive statements on some of the materials that we're looking at, the ceramics, that will take rock drops and not chip.

We do have to have a definition of what porosity we're still looking for. We're looking at applying a technology that exists, and the technology that exists is not looking for high-density materials and also high thermally-conducted materials.

They're looking for more abrasion and also heat buffering. So, they actually put it in diesels, and so, they want to make sure that you're actually insulating the diesel engine from the ignition.

So, we're applying technologies to our needs. We're not trying to invent new technologies.

The interesting thing is that, from an engineering point of view, we've also realized that, as you load a canister -- and this has been shown at some of the spent fuel pools -- they become more barrel-shaped.

So, finding a material that actually can expand and contract with the package is very important to us.

We don't need the China kind of syndrome where we, you know, kind of grow and it all pops off. That would not be a decent material for a drip shield, because the drip shield is really there to last to 10,000 years.

That is why we're looking at ceramics. We're looking at a 10,000-year kind of coating.

So, we're looking at that.

We're also looking at the backfill closure time. This does bring out an engineering requirement, because you do -- once you put the backfill on there, you do heat things up.

Even with the cool-down time we have in there, we're going to blast cool some activity and try to draw that temperature down so it starts at a lower temperature and then starts to rise, but we're still looking at blending and lag storage and ventilation to make that backfill happen for us in the 50-year, even at the 100-year timeframe.

Again, we're trying to deal with the cladding, where we're saying, to make it happen, we're trying to protect the cladding temperature at 350 degrees.

Again, there's a lot of reasons for that 350 degrees. We don't want the cladding to rupture for more than one case, not only for just simply containing the material in there but also from a criticality point of view.

If I can go to the regulator and say, gentlemen, I know what the configuration of the spent nuclear fuel looks like from now to 10,000 years, I have a better chance of making that argument, but if I go into there and say, gentlemen, I'm going to blow the cladding away, they say, well, where is the material going, I say, well, someplace inside the package, the argument becomes weaker now.

So, if I can work a design such that I know the configurations, I have a better argument with the Nuclear Regulatory Commission, and that's what you see also in the engineered barrier segment, where we have different components doing different things for us in the criticality.

As it goes, as it starts degrading, the system degrades, and also we have as the invert media down here -- what we'd like to do is also capture the media like a civil engineering filter so we know exactly where it is and we can make a nice argument, it will be a slab kind of configuration, I can do a calculation, and it makes a strong argument.

Again, if we can design in the configurations, it will be an easier argument.

Okay.

Also, we're doing -- is totally shield -- I think we talked about that, totally shielded. We did take a hard look at that in the sense that we used a higher shielding dose. Again, we don't want to come and start breaking down and having different licenses for it.

So, we did take the worst shielding configuration that we had, and it came up very large.

Extending the life -- there's different mechanisms -- barrier materials, cladding failure -- we're looking at that. Again, the neutron-absorbing material, also looking at different neutron-absorbing materials.

Now, I want to talk a little bit about the thermal regions. We've done some interesting things in this.

We've done criticality. We've actually extended the technology a bit in this, and we're going to go through this relatively quickly in the sense that we've developed the four-model process by which we get the boundary conditions for each lower model.

Here we have to look at the larger repository, how it's behaving. We get the end effects and how it behaves in the center. With that, we do more of a slice-out.

Our models now are four to five packages long. We get a boundary condition for the package itself, so we can look at the cladding temperature.

And the interesting thing that we've done now is that we've looked and do have the methodology by which we can calculate the cladding temperatures within plus or minus four or five degrees, and this has been benchmarked against the TN calculations.

This is the model that actually look at the drift, and this is what I said -- this is sometimes known as a champagne bottle, because it does look like a little bit of champagne, but what we did is we took a 21 pressurized water reactor design which has the design basis fuel in it, we took a defense high-level waste, we looked at -- there's one right here that actually has lower thermal load that's just not ejecting them on the sheet, and also we have a boiling water reactor here.

You see, with the drift emplacement, it's very nice. We showed it at 10 years, because it's interesting. At 50 years, this is all orange all the way across, so it becomes very uniform. That's what we want.

If you look at a bore-hole design, the bore holes would become somewhat uniform here, but it would be a lot higher stresses to the rock. Here it distributes the heat very nicely to the rock mass, lowering the stresses to the rocks.

Also, you notice that the lower package also has some benefit from the higher package's thermal umbrella. So, what we're doing is we're bounding the design -- bounding the lower thermal one.

Now stepping through a bit, this is our waste package model. We do half symmetry on it. This is an ANSYS model. What we have done also, instead of trying to develop our own finite element methodology, we have used a nuclear standards technology of dealing with ANSYS.

As you can see, this is the 21 pressurized water reactor design, and we can see again where the hot assembly is, which is no surprise it's the center assembly, and how it also leaks the heat away.

Again, the basket design is such that it works very nicely, is one of the more efficient designs we've come up with.

Now, the effective thermal conductivity evaluation -- now we go down to -- we take one of these locations right here and we actually have a report out on this one. It was 1996, it was issued, and the NRC has seen it, and actually, the industry is picking up this methodology now.

This is effective thermal conductivity methodology, remodeled each rod to the point where we have -this is the fuel, this is the cladding, we even put the clad gap in there, and we also put an oxide layer on it. So, we made sure we covered all the bases on it.

And we have developed a matrix of temperatures and effective thermal conductivities, and we can predict and have benchmarked against experiments down to plus or minus five degrees, and again, that gives us a nominal design, we know exactly where we are, and now, as engineers, we can decide where the effective safety -- what kind of margin we might put onto the system.

Again, this is a methodology that's been adopted by the industry and is being moved out into it.

Structural evaluation: Again, the requirements -- all these things come with requirements and we have based on requirements. As engineers, that's where the basis is. And some of the evaluations that we've done is for handling, design basis events, and then post-closure.

We've come up with a requirement of a 2-meter handling height that they can deal with, and with that, we have to look at different geometries, similar to transportation casks, and so, this is the beginning model set-up of that one, and that's our 2-meter start point.

We're pushing it on a non-yield surface, and as you can see, the handling skirt actually does help us out a bit on the spring, so we do bend over, but as it hits, that's the center of gravity of a corner drop. It is typically a high-load configuration.

We are taking a look at other angles off that to make sure we did capture that correctly.

We're also doing horizontal drop, also doing a horizontal on a pin or a rail system, since we have a rail system going on, and again, this is done with the finite element model of ANSYS, it's well-benchmarked, well-maintained, and what we've seen here is we correlate very well with some of the tests that have gone on there, also. Again, we're very keen on that.

One of the real interesting ones that everybody seems to come back to is the design -- how big of a rock do you need to make the package break? Well, we look at that a year ago. You need a 3-meter rock and a 3-meter drift to start the failure mechanism on the package.

That means that's where ASME is not met, where we start the crack growth. So, the package initially is very stout, and we actually walk through time on this to make sure we get it.

[Slide.]

And with the last slide, I'm going to have -- people always talk about, how are you going to do the off-normal, can you actually pick up this waste package if something breaks or fails?

This might be hard to understand, but this is the skirt again and these are the lifting holes.

We have three lifting holes, three points define a plane, so we have three holes, and we've actually looked at these loads such that you can grab a chain fall through that one and drag it through the drift and it works.

So, we can go in there and get it out if it gets buried.

So, we have done a lot of work in these areas. We can go into a lot more detail when we need to.

The engineers have had fun with this, and just with ending, these are the three main areas that we've developed in technology also, while being in the design of the repository, to make sure that we can find the nominal configurations.

So, with that, I'd like to end, and open up for questions.

DR. BULLEN: Thank you, Tom.

We'll first open with questions from the board.

Knopman?

DR. KNOPMAN: Knopman, Board. I hope at that the time that you're speaking with the NRC you'll address the chairman of the NRC not as a gentleman. Make sure we cover that.

More importantly, I was wondering if you could perhaps give the board some bounds on the current design without the enhancements.

If you assume that there is no water at all and the canisters are sitting in the drifts, as you've set it up, what is your expected lifetime without any water? Let's just say zero relative humidity over an indefinite amount of time.

MR. DOERING: I'm looking into the audience, because I don't do the performance assessments, and I think Joon Lee will address those. Is Joon Lee here?

DR. KNOPMAN: Well, from an engineering point of view.

MR. DOERING: We have a requirement that says our packages have to have a minimum lifetime of 3,000 years.

DR. KNOPMAN: Okay.

MR. DOERING: And to keep the moisture off the waste form for 10,000 years. That is a relatively new one. The last one is a relatively new requirement that was imposed upon us, and that's why you've seen us go through more of the drip shields and also the coatings, the ceramic coatings.

Until then, we didn't believe we needed the ceramic coatings, but with that kind of longevity, we were looking at another option to extend it at that time.

DR. KNOPMAN: So, because you're not doing performance assessment, you can't say right now what you've got in terms of performance?

MR. DOERING: Our group doesn't do performance assessment, exactly, and we work closely with performance assessments, but it's not right for me to answer one of their questions with their models. So, Joon Lee will --

MR. STAHL: David Stahl from M&O/Framatome Cogema Fuels.

We work closely with Joon Lee in looking at some of the performance of the barriers, and there were calculations done for dry oxidation for both the corrosion allowance and corrosion-resistance materials, and they're very, very low penetration rates, and the system would last -- if not tens of thousands of years, perhaps hundreds of thousands of years, and hopefully, Joon, when he comes, will be able to give you more definite numbers, but they're very, very low oxidation and alteration rates.

CHAIRMAN COHON: Cohon, Board. I just want to jump it, because it follows directly on that. Is your design life, though, 10,000 years?

MR. DOERING: The design life of the waste package itself is not 10,000 years, it's 3,000 years, with the moisture, again, not -- there's a requirement that we have of the moisture not contacting the waste form, and the waste form is the spent nuclear fuel itself.

CHAIRMAN COHON: Thank you.

DR. NELSON: Nelson, Board. Can you explain to me the importance or the impact of pH of moisture on the package?

MR. DOERING: The pH -- I think David Stahl will address a little bit more of that, but the pH really deals with the corrosion rate that we're dealing with on the package itself and how quickly it will corrode and how quickly it will regenerate, how deep will it go into it.

Dave, do you have anymore to add on that one, on the pH?

MR. STAHL: I'll talk about it later, if you don't mind.

MR. DOERING: Okay.

So, yes, the pH has a big play on what the corrosion materials will do.

DR. BULLEN: Sagues?

DR. SAGUES: Yes, Sagues, Board.

There will be like definitely about 10,000 packages, maybe less than 20,000. The packages are going to be welded. The packages have a diameter of between 1 and 2 meters. We are talking about an enormous amount of lengths of weld of something which may be several inches thick.

What is -- can you -- can someone quantify the probability that at least one of those welds will be defective from day zero? That is, that we may have one or two leakers, if we may use that expression, even before the repository functions begin.

MR. DOERING: Actually, that leads into Jerry Cogar's talk very well, and I can ask him to speak to that.

DR. BULLEN: Why don't we wait on that one, if it's coming up?

MR. DOERING: Okay.

But just basically we can take that into consideration. But Jerry will go into the details of the welding and the quality that we have on that one.

DR. BULLEN: Parizek?

DR. PARIZEK: Parizek, Board. On the necking of the champagne bottle, you can maintain the temperature all down one whole placement drift, according to that diagram, but do you get cool spots somewhere in there where you might have refluxing coming back in on the drift?

MR. DOERING: What we did on this one is we took what we consider one of the worst configurations that we would get into as having a very hot one and a line of cold ones, and from this, we do see that we can even cover the 100 degrees C for a long period of time.

DR. PARIZEK: In a long distance down the placement drifts.

MR. DOERING: Right.

DR. PARIZEK: Another question regarding the nature of the redundancy in the waste package, it sounds like a car I bought once, and according to the redundancies, if one thing failed in my car, my car still runs, but sometimes one dinky thing goes wrong and nothing works.

In terms of your redundancies with the metals and all the different parts, the component parts you described for us, do they work in tandem or independently, or if one fails, does the whole thing kind of short-circuit its design life?

MR. DOERING: I think, from a design point of view, the answer would be no, and we'd try to prevent that from occurring.

DR. PARIZEK: That's what my car salesman told me.

MR. DOERING: The goal is not to have that. What we're looking at is the barriers that we're dealing with and how each component behaves.

Again, the five corners are only there to maintain its configuration during the handling configuration and through the surface and subsurface activities.

They could actually go away and systems could slump, and that's why you see those, but for materials A516, it's not that important to it.

So, what we've done is concentrate on the components that are very important to us. The egg crate design is inherently stiff and long-lasting, because there are no walls on it. So, we're not embrittling it, we're not causing any kind mechanism or any stresses in that material.

In fact, it's a load that only comes to as a load as an engineer, and that's why the carbon tube is there for the load bearing and the thermal, also.

So, what we try to do is break those things down so we don't have a common mode failure in those, and that's why you see a couple of materials in that, also.

We could have made -- we can go in there and change the material, the stainless steel boron to another material tube, but the stainless steel boron seems to provide us the most strength for the longest period of time, and still, we can deal with it.

So, we try to break it down so there is no common mode failure inside.

If you come up with other ideas as a weak spot, we gladly ask for that feedback.

DR. BULLEN: Arendt, Board.

DR. ARENDT: Arendt, Board. On the -- I guess the second or third view-graph, you have classified waste, and in the interests of communicating with everyone, classified waste and defense waste, high-level waste are synonymous, are they not?

MR. DOERING: This is a typo. The spell-checker got to us. That's supposed to be glassified. So, you can put a little bar across this.

On Friday, when I had to -- I had to leave on Monday to be here, and I turned this in to graphics, and graphics was really trying to help an engineer spell right. Unfortunately, I really wanted it to be glassified.

DR. ARENDT: You might use litrified, also.

MR. DOERING: Yes. I think that's the lesson I just learned.

DR. ARENDT: Okay.

The second question, in the list of the waste package designs, is there going to be one standard diameter, or am I to infer from this table that there might be more than one diameter?

MR. DOERING: There will be, by definition, more than one diameter, because there are different waste forms that are coming to us, and so, we can only reflect to what the waste forms are coming to us.

What we try to do is keep the diameter similar or try to minimize the number of packages that we have.

DR. ARENDT: Is that going to be a problem in handling the packages with the gantry, or are those people aware of the fact that the waste packets may be of several different diameters?

MR. DOERING: Actually, Dan McKenzie's head is nodding up and down, and our offices are very close, and so, the answer is yes.

DR. ARENDT: Okay.

MR. DOERING: What we do on that is we pick it up on the ends. That's why you see the skirts, also, because all the handling is done through the skirts, so we don't perturb the barriers. That's the whole point of that, also.

DR. ARENDT: The other question is are you limiting the study of depleted oxide to DU-235 or are you considering it for all enrichment waste spent fuel?

MR. DOERING: We're looking at the whole range of the DOE spent fuel, both low and medium, 20 percent, what we call medium, and also very, very high.

DR. ARENDT: You're looking at all --

MR. DOERING: Yes.

DR. ARENDT: But on your view-graph, you only indicated for high-enrichment --

MR. DOERING: We kind of called that out because that's the interesting one.

DR. ARENDT: Okay.

MR. DOERING: The low ones would be very easy --

DR. ARENDT: Last question is are you in contact with the Canadians in their waste package development and design?

MR. DOERING: Yes.

DR. ARENDT: So, you speak quite frequently? You know what they're doing, and they know what you're doing?

MR. DOERING: Actually, on that point, that's a very good point, because what we have is also what we consider an expert review team that comes in and looks at us every so often, and David Shoesmith came last month and looked at us and provided us good information.

DR. ARENDT: Thank you.

DR. BULLEN: Questions from the consultants?

MR. KUHN: Klaus Kuhn. What is your assumption for the long term, let's say for 10,000, of the mechanical stability of your emplacement drift, and what is the interaction during this time with the waste package?

MR. DOERING: That's an area that we're looking at. I think that's one of the slides we've got back at the home office, and we are concerned with that, and we understand there will be some rock falls. We're trying to quantify those rock falls.

What we did in another evaluation is look at the maximum through time, but we understand there will be rock falls and how it all comes in and insulates the package, but by then the thermal pulse is very low.

The repository level is off somewhere between 40 and 70 degrees centigrade. That's where it's all going to come down if you have high or low thermal load.

So, what we're dealing with a system in that range temperature with some rocks on top of it, so we don't believe we'll get a higher thermal loss.

We have also looked at what would happen if you have a bare assembly sitting there and a rock would fall and what size rock does that take to fail. So, we have gone into those issues and how the basket behaves and how it degrades, also.

That's a different talk, and we'd be glad to talk on that. That's something we have concern with.

MR. DANKO: George Danko.

The waste package size and the spacing represents an area of mass load to the repository, and then from the burnup rate and the age of the waste will translate this area mass load into an area heat load, and the heat load is what determines the temperature distribution, and I would like to ask you how reliable is this translation from mass load to heat load?

In other words, can you verify the burnup rate? Do you exactly know the heat output for a blend of waste within the packages? Can it be verified, or it is all paperwork? That's my question.

MR. DOERING: The quick answer is yes. The utilities have records of what the burnup is.

Those records will come along with the assemblies, let's understand, so we can actually know exactly what their age, you know, since discharge and also what the initial enrichment -- what the final burnup is. So, all those records are there, so the correlation is straight in there.

MR. PETERSON: Carl Peterson with two probably simple questions.

Is this vessel required to act as a significant pressure vessel other than the fact that the gas inside gets hot, and the other one is, is the gas inside controlled at the time you weld it shut?

MR. DOERING: Right now, what we're looking at is a helium -- one atmosphere helium inside simply to inert it. So, the intent is not to have a large gas buildup, even though 12 centimeters probably could handle a lot of pressure.

It's not the N-stamped or not a ASME boiler pressure. The vessel doesn't get that N-stamp for the pressure, but it can take it, and we have done some calculations on that, and I've forgotten exactly what the number is, but it's pretty high before it fails.

DR. BULLEN: Any questions from the staff?

[No response.]

DR. BULLEN: Seeing none, thank you very much, Tom.

MR. DOERING: Thank you very much. I've really enjoyed it.

DR. BULLEN: And we will move on to our next speaker, who is Hugh Benton, and he will speak to us about repository criticality analysis.

MR. BENTON: Good morning. I'm going to talk about the approach the repository criticality analysis. Of course, I am emphasizing that my talk is on the analysis. The objective of the analysis is to avoid the approach to criticality.

I want to first mention the regulation out of 10 CFR regarding criticality and its effect on the design. We'll talk just for a minute about the possibility locations of criticality, then discuss burnup, which is an intrinsic part of the fuel and is important to our design.

I will mention the key features of the disposal criticality analysis methodology and the design options that we have for controlling criticality and finally show what the consequences would be if, in spite of all, we did have a criticality in the repository.

Criticality control regulation comes from 10 CFR 60, and it is generally deterministically worded, and it says that criticality shall not be possible absent to unlikely and independent events.

Clearly, that is appropriate for the pre-closure period where the safety of workers is paramount, and our designs will prevent criticality during that period -- during the containment period prior to waste package breach.

The probabilistic approach is appropriate in evaluating risk of criticality after the containment period, since it is difficult to understand the independence of events that might occur during those very long timeframes. The events tend to be interconnected.

Now, where could a criticality occur? We must evaluate the potential for criticality in all possible locations, starting with in a canister, which would be perhaps inside a waste package, as Tom Doering has described, a canister which might DOE-owned spent fuel.

First, we could have the initial condition where both the basket and the fuel are intact. This has our lowest reactivity level.

We can then have a condition where, after waste package breach and after canister breach, where we get water in the canister, we can still have an intact basket but the fuel can start to degrade. This is a condition which requires the maximum amount of neutron-absorbing material.

Finally, we could get to where both the basket and fuel are degraded.

Then, in the waste package, after the canister of fuel, DOE-owned spent fuel or whatever, has degraded, we can still have the possibility of a criticality within the waste package with degraded basket and canister or the fuel can be fragmented in the bottom of the container.

After failure degradation, we have to be concerned about the possibility of criticality in the near-field where we may have fuel fragments that are dropped out of the waste package and into the invert or further degradation of the fuel could result in fuel degradation products seeping into the invert.

In the far-field, we could have degradation products traveling in fractures or we could have fuel degradation products which proceed through the matrix and pass through reducing zones which can potentially give a critical mass.

This is a schematic of what the degradation processes in the waste package will occur.

The initial configuration, of course, everything is intact, and then, perhaps through dripping water over many years, perhaps centuries, we can fail the outer and inner barriers and fill the waste package with water. The rest of the waste package is still intact.

Then the side guides that Tom Doering mentioned are made of carbon steel, they would start to fail. The corner guides would fail. Then the long criticality control plates would begin to degrade and bend at the ends under the weight of the assemblies.

Then the criticality control plates would further degrade, allowing the assemblies to start to drop toward the bottom. Finally, we would have a fully degraded basket where all of the basket structural materials and the criticality control materials have essentially degraded.

Now, in this condition, we would expect the iron oxide from the large amount of structural steel and stainless steel in the waste package to be generally dispersed, and as long as it is, there is no possibility of criticality.

We would also expect that the supplemental neutron-absorbent material, the boron, to generally be still in the waste package, and as long as it is, as long as at least about 10 percent of it, we would not have a criticality.

The limiting condition is where, through some flushing action because of holes in the side of the waste package, some, over a very long period of time, water movement dissolves the boron and gets it out of the waste package and the iron oxide, instead of being dispersed through the package, has dropped to the bottom.

In that condition, which is a very low-probability event, there is a possibility of criticality.

I want to look for just a moment at the time effects of a -- on a criticality potential for a 21 PWR waste package.

This is with no additional neutron absorbers added, and on the left -- sorry -- not quite shown is K-effective, the measure of criticality, the neutron multiplication factor, and any K-effective of 1 or above would indicate a criticality, a critical condition.

We have two lines.

The lower line is the actual case for the 3.75 initial enrichment 37-gigawatt day burnup fuel, and this shows the effect of taking credit for the burnup on K-effective. It depresses that line below 1. This is the actual case.

This curve is governed generally by what is happening to plutonium, plutonium 241 decaying, plutonium

240 decaying, plutonium 239 decaying.

Up here at the top is a line which shows what the K-effective would be for that fuel as initially fabricated, without any burnup, and the difference here, from here to here, is the credit for burnup that we are taking in our analysis.

So, burnup credit is a basic. It's a control feature that is built into the fuel. Were there not burnup, the power plants would not be sending us the fuel for disposal.

It's a function of the initial enrichment as far as the time and power level of the reactor operation while that fuel was there, accounts for the net depletion of the fissile isotopes.

We are using 29 principle isotopes to analyze the amount of burnup. These are the principle neutron-absorbing isotopes that are long-lived, will be there for a long period of time.

There are, of course, hundreds of isotopes that have some effect on K-effective, but we are only tracking the 29, and in that respect, burnup credit that we're taking is in the conservative direction.

Burnup credit is a basic feature of waste package design. Absent burnup credit, our waste packages would be far smaller, maybe of the order of three or four assemblies instead of 21.

The NRC has recognized burnup credit, has not generally sanctioned its use for transportation and storage, its use away from reactors.

I want to just mention two of the significant number of models that make up our criticality control analysis methodology.

The neutronics models are really the heart of that entire assembly of models which are designed to give us the bias and uncertainty associated with burnup credit. These are the criticality model and the isotopics model.

What we basically need to do, what we are doing, is to validate the models, establish the input parameters that go into the models, and define their range of applicability.

We are doing the validation through benchmarks of three general types -- laboratory critical experiments, commercial reactor criticalities, and chemical assays -- and we need all three.

Laboratory critical experiments are used to measure the critical conditions in an actual critical assembly in a laboratory or extrapolate a critical assembly. They are used to validate criticality safety codes such as MCNP and KENO.

We will be using in excess of 300 laboratory critical experiments in our validation.

Commercial reactor criticalities are measuring actual critical conditions that occur in an operating reactor, and they are used generally for reactor design code validations, such as CASMO and NEMO.

We expect to have about 80 commercial reactor criticalities analyzed as part of our total validation.

Finally, chemical assays, which measure isotopic concentrations in actual samples of spent fuel used for depletion code validation such as REGN or SAS-2H, and currently, we expect to have about 65 chemical assays supporting the validation.

Let me show some of the design options that we have for controlling criticality.

The one that we are most using the supplemental neutron absorber material, boron stainless steel. For some of the DOE high-enriched fuel, we are looking at gadolinium or a combination of gadolinium and hafnium.

Some of the commercial fuel may come with the control rods still inserted, which are silver-indium-cadmium, and if they do, we will obviously leave those in.

Our criticality control material is either in plates or, in some cases, we may manufacture rods which would be zircalloy-clad B4C rods inserted into the assemblies.

We, as Tom Doering has mentioned, add more criticality control material than we need initially by at least 20 percent to account for degradation.

We can also control criticality through geometry. This was the method used in the multi-purpose canister flux-traps in which the assemblies are physically held apart to prevent criticality.

This is not general an optimal way of doing it for a repository, since you are asking to defy gravity for tens of thousands of years, which is somewhat difficult.

Filler material could be added. Moderators such as steel shot, which is an excellent neutron absorber in itself and also, as it corrodes, it expands and displaces moderator. We may be, for some of the DOE high-enriched, adding depleted uranium as a dilutant.

The filler material does add cost and time and also significantly increases the weight of the loaded waste package.

We also limit the fissile mass, and this is particularly applicable in the case of the highly-enriched DOE spent fuel. Obviously, the limiting of the mass does increase the number of packages.

We have other criticality control material options, such as adding material to the invert, which might aid in controlling criticality there.

Let me turn to the consequences in case a criticality event were to occur.

After K-effective got to 1 or just above 1, we would start generating a small amount of power. This would increase the temperature of the water.

If the top of the waste package was essentially open but the rest of the waste package was intact to hold the water, you would have a boiling effect, and the water would eventually boil off.

If the water had come in through a fairly small hole, there would be a small pressure built up in the waste package, and steam would be expelled, which would result in some localized increase in relative humidity for a short time.

There would be a slight increase in the fission products, a decrease in the fissionable material.

After we've gone through the initial steps, the water would be reduced, and when the moderator goes away, the criticality stops.

Under all reasonable scenarios, it would be several years before we could collect enough water in that same waste package, open waste package, in order to reach criticality again, and the event would not only not be noticeable outside the repository, it would not be noticeable in adjacent drifts.

We have looked at the worst case scenarios of degraded fuel and degraded waste packages, and within the waste package, 85 percent of the PWR fuel cannot support a criticality.

Now, this 85 percent differs from the one-third that Tom Doering mentioned as fuel that could not go critical.

The difference is here I'm talking about could actually support a criticality, K-effective of 1.

Tom is talking about the one-third as a design number, including the 5-percent safety margin minus an uncertainty and so forth that the NRC requires.

In order to get a criticality, we have to have at least two, maybe four assemblies immersed in the water, and it's only possible if the neutron absorber is separated from the fuel, and as I mentioned, the iron oxide does act as a very good neutron absorber on its own.

Outside the waste package, we have looked at a broad range of geo-chemical interactions, and we have not yet been able to find one which could accumulate a critical mass.

We are not totally through with that analysis. So, we're saying that geochemical interactions are very unlikely to accumulate a critical mass.

And for a long period of time, there is no mechanism for ponding of water underneath the waste package. We might have a short period of some ponding, but for a long period of time, that, we do not believe, is a credible scenario.

So, in summary, criticality analysis will be documented in the topical report for submission to the DOE and then to the NRC. It is dependent on achieving credit for the burnup that is in the fuel, and it is based on a probabilistic approach.

Designs will prevent criticalities before waste package breach. Probability of a criticality event sometime long in the distant future is very low but not zero, of course, but the consequences would be small and highly localized.

Subject to your questions, that's all I have.

DR. BULLEN: Thank you, Hugh.

Questions from the board?

Arendt?

DR. ARENDT: I'm not sure this is appropriate for Hugh here, but who is responsible for determining the usefulness of depleted uranium oxide for the M&O. Who has that responsibility?

MR. BENTON: Anything like that, the basic responsibility would be the Department of Energy. We might assist with some specific analyses of what depleted uranium might or might not do, but a major policy issue such as that would clearly be handled by the Department of Energy.

DR. ARENDT: But that has not been given to the M&O as an assignment, then, from DOE?

MR. BENTON: It has certainly not been given to me, no, sir.

DR. ARENDT: Well, we're talking about it. Somebody must be doing some work on it.

MR. BENTON: We have had technical discussions concerning depleted uranium. Paul Harrington from the Department of Energy will respond.

MR. HARRINGTON: This is Paul Harrington, DOE.

We have had the M&O working with PM and MB, looking at possible uses of DU in the repository.

We had a multi-day workshop back in July, I believe, of this summer to pursue possible uses of that. There's a report that's come out in draft form looking at what may be used or the uses of that may be available for the repository.

DR. ARENDT: But there isn't anybody in the M&O, then, that has specific assignment. Is that right?

MR. HARRINGTON: I believe that the M&O is tasked to look at potential uses of that, yes.

DR. ARENDT: Okay.

Now, on filler material, you say that it adds cost and time for loading. That's a disadvantage, obviously, but I'm wondering, has there been a cost-benefit analysis made yet, or is that just an off-the-top-of-the-head comment?

MR. BENTON: There has been some analysis of the additional time.

We have done testing of the ability to fill all the crevices in a waste package with iron shot. We've constructed dummy assemblies, put them in a case, poured the shot in to see what fill we could get, and we got very good fill, above 95 percent.

So, that proved that the concept would work.

There has been some analysis done of what that would do to the time in the surface facilities but not a detailed analysis yet, because we haven't yet decided how many packages are going to require it, if any, which ones, and we're still doing that analysis.

DR. ARENDT: I understand that there could be some benefits in addition to criticality in the use of depleted uranium oxide.

MR. BENTON: Dr. Hanauer would like to discuss that.

DR. HANAUER: Steve Hanauer, DOE.

These depleted uranium considerations are much more complicated than the effect on criticality or anything else on our repository. There is roughly a half-a-million tons of this substance stored around the isotope separation plants in Oak Ridge, Paducah, and Portsmouth, Ohio.

This is a national problem, how to dispose of this material. The discussions about what to do with are, in fact, not very far along.

Where it's going to end up is certainly not decided, but one of the places is obviously the Yucca Mountain -- one of the possible places is the Yucca Mountain repository, and people are starting to look at ways to solve the two problems jointly, the two problems being what are we going to do with high-level waste for which Yucca Mountain is, by statute, the current candidate and what we're going to do with all this depleted uranium.

This is not very far along, because the people who are dealing with the depleted uranium problem are looking for customers so far.

They think this is a valuable substance, and they have not yet come around to seeing this as a problem to be solved jointly with the high-level waste, which I'm convinced has to be considered as a national problem and has not yet gotten much consideration.

DR. BULLEN: Questions from consultants, please?

MR. DANKO: George Danko.

Is criticality more likely to happen in a hot

container or in a cold container like in the defense waste?
MR. BENTON: In a hot container or a cold container?
MR. DANKO: Yes.
MR. BENTON: Well, given the fact that we expect the containers to last and not breach until after they become cold, basically cold, then we would say that the probability of a criticality in a cold container, thousands of years, is more likely than while the heat pulse is still there.
MR. DANKO: My question was that the material inside the defense container is a candidate for going to be critical or is not? It's in a different waste form.
MR. BENTON: Generally, the moderator is more effective if it's colder and denser, so from that standpoint, we would say generally cold.
DR. BULLEN: Bullen, Board. I have kind of a followup on that.
I'm a reactor manager with some argonot fuel that's aluminum-clad that's going to be put into the center of your five DWPF waste can with DOE spent fuel in the middle, or maybe it's not, but the question I have is are you inventory-limited there?
Do you essentially do the criticality analysis and keep less than critical mass in those center sections, or is

there a potential that you're going to reject some fuel and say it has to be processed in another manner so that you can accept it as a waste form?

MR. BENTON: I think both of those are possible.

There may be some waste forms -- and I am not addressing your particular waste form -- that may, because of sodium bonding or some other reason, fall outside the envelope of acceptable waste for disposal, perhaps pyrophoricity or whatever.

The DOE spent fuel that we have analyzed so far -- and we have not analyzed very much of it -- we are not limiting ourselves to less than a critical mass in any single waste package.

We are using supplemental neutron absorbers and taking advantage of the iron oxide and all the other things so that we could get a reasonable amount.

In the small 17-inch canister that fits inside the five, some of the fuel we've analyzed we're capacity-limited, not fissile mass-limited.

DR. BULLEN: Thank you.

Sagues, Board?

DR. SAGUES: Yes. Sagues, Board.

You indicated in one of the transparencies that there was no known mechanism for ponding of water in the drift, and by that, you mean that it's an unlikely event or that they couldn't figure out some way in which that could happen?

Because I could mention at least one way in which that could happen, but go ahead.

MR. BENTON: Well, we believe that it is very unlikely, possible maybe in some short timeframe, but in the long run, given that, for instance, the drifts are sloped.

So, it would be difficult to imagine that there could be a significant amount of ponding, and of course, by ponding, I'm interested in the amount of ponding that would be necessary in order to achieve a criticality.

So, a small film of water or something in the floor of the drift would not create a criticality event anyway.

DR. SAGUES: Because you could have, for example, a caving down-slope from there and either a caving or you could have, for example, accumulated phoresence through the concrete in the part in which the container that would be a problem happens to be in good condition, and indeed, there may have been enough carbonation and associated phenomena to make the bottom in that area quite impermeable, and then, if you have a localized drift nearby, you could conceivably begin to build a couple of feet of water down there.

MR. BENTON: Yes, sir. I understand that we could have -- given 100 miles of drift, we could have concrete falling in almost any configuration. Generally, we are assuming that the invert, the matrix below it, will allow, generally, water to pass.

So, there may be some possibility for some period of time, until the concrete disintegrates, that we could have some short-term ponding.

DR. BULLEN: Craig, Board.

DR. CRAIG: Craig, Board.

To follow up on that, what would happen if the integrity of the cask were lost and there were a flash flood that totally covered the residual material? What's the worst case accident under some circumstances, such circumstances?

MR. BENTON: It's not significantly more difficult if the water builds up in the cask rapidly or it builds up in the cask slowly.

The worst case condition that we've -- scenario that we've been able to define so far is that, with the iron oxide distributed throughout a degraded cask, the supplemental neutron absorber material has been flushed out, but you still have the iron oxide, and then through a severe earthquake or something like that, a sudden shaking, the iron oxide settles to the bottom quickly, you can then get a significant increase in reactivity.

This is a sequence of events each of which is very unlikely, and the total string is extremely unlikely.

DR. CRAIG: I absolutely agree that it's unlikely, but I find it yet appealing for what is the greatest energy release that could occur even in such an unlikely event.

MR. BENTON: We have done some analyses which shows that, if that worst case condition occurred, you could have a criticality event that would last on the order of 10 minutes. It might peak at several hundred watts, but it would be over a very short period of time.

DR. CRAIG: So, you have been unable to think of any events that would release more than hundreds of watts over 10-minute periods.

MR. BENTON: Our analysis so far, that's about the range.

DR. CRAIG: Thank you.

MR. BENTON: Maybe up to a megawatt but nothing beyond that.

DR. BULLEN: Any questions from the staff?

[No response.]

DR. BULLEN: Seeing none, thank you very much, Mr. Benton.

MR. BENTON: Thank you.

DR. BULLEN: Our final presentation before the break is going to be made by Mr. Cogar on waste package fabrication, closure, and examination.

MR. COGAR: Good morning.

Engineering development has the responsibility for the fabrication of the waste package, the closure of the waste package, and the development work necessary to do that, so that's what I'm going to address this morning

Starting with fabrication of the waste package and looking at the outer barrier first, the outer barrier is made of two plates of A516 carbon steel, its current design. Why two plates? Because that's the size we can buy and essentially the size we can work with.

These two plates are preliminarily cut to size, pre-bent on the ends or, if you will, bulldozed. The plates are then put in a furnace, brought up to heat, and put in rolls and formed into cylinders.

When the two cylinders are formed, we then make a longitudinal weld seam on each one. That's represented by the little white line in the center there.

Once the weld seams are made -- and it can made by a number of weld processes -- we grind or prepare both sides of the weld seam and do non-destructive examinations on it.

These non-destructive examinations include ultrasonic inspection, radiographic inspection, and a surface inspection, which in this case is a magnetic particle inspection.

At that point in time, the outer barrier is actually done.

The inner barrier, which currently is alloy-625, is formed in much the same manner. It's preliminarily cut to size, bulldozed or pre-bent on the ends, and put into rolls and rolled. It does not have to be heated.

The long seam or the longitudinal seam, again represented by the white line, is done in much the same manner, can be done by a number of weld processes.

It's then ground or prepared on the inside and the outside, and the non-destructive examinations are done, once again ultrasonics and radiographic inspection for the volumetric and, in this case, dye penetrant for the surface inspection.

At this point in time, we have four cylinders, two outer barriers, two inner barriers.

At this point, we would seam the surface -- the outside surface of the inner barrier and the inside surface of the outer barrier. These are machined to approximately a 70/1,000ths interference fit.

The outer barriers are both put in a furnace and heated up, approximately 1,000 degrees, brought out of the furnace, and the inner barrier is deposited into the outer barrier with stops to set it in place.

The assembly is then returned to the furnace and allowed to cool from 1,000 down to about 800, at which time we can air cool it. This is commonly known as shrink fit. Let's talk just a little bit about shrink fit, because it has been a topic of discussion.

Shrink fit is not a new process. It's not a new and exotic thing. It's been around sometime since they put rims on wagon wheels.

Currently, it's being used in industry for drawbridge hinges, copper foil pickup rolls -- those were made for Gould -- extreme contour changes, and more recently for shipping casks. There's a company in Pennsylvania that's actually doing this to make shipping casks.

So, it's not something new and exotic. It is a reasonably cost-effective way to get a tight fit. It's simplistic, and it's easy to do, but there are a lot of different opinions on it.

Once the shrink fit is completed, we need to put the two pieces together, and this is done by making a circumferential seam in the middle of it, it's done by making the carbon steel first, the outer barrier first, it's welded from the inside.

Then the inside is ground and the outside is ground to sound metal, we do our inspections on there and back-welding, if necessary. The inspections are the same as before. They're ultrasonic, radiographic, and magnetic particle inspection.

After that's completed, then we make the inner barrier weld. The inner barrier weld is basically done as a back-clad-type operation. It actually welds the inner barrier to the outer barrier, much the same as done in the Navy nuclear industry or the commercial nuclear industry.

So, at that point, we have the two barriers placed together into one long cylinder.

We then send this to machining and, in essence, turn the assembly in that position, and start the lid assemblies.

The inner lid is placed on and it's welded. There's a number of weld methods that can be used for that. It's more or less at the discretion of the fabricator.

After the inner lid is welded, we do the ultrasonics and a dye penetrant inspection on it, put the outer lid on, weld it with virtually the same method and the same setup, do the ultrasonics and magnetic particle inspection of that.

At that time, we stress relieve the entire assembly.

After the stress relieve of the assembly, we come back and do, to the maximum extent possible, all of our ultrasonic tests again. The ones I described initially are at the discretion of the manufacturer unless they're inaccessible after stress.

Yes, sir.

DR. SAGUES: Excuse me. Could you explain that graph?

MR. COGAR: This one right here says the inner barrier with the outer barrier -- the green is the inner barrier. This is the inner lid, and you're probably confused just slightly because I didn't put the one with the outer lid up.

After the inner lid, which is the blue line, which doesn't show really well, and then the outer lid is on.

DR. SAGUES: Yes, but what is the bottom part of it, the rounded portion?

MR. COGAR: Oh, this here?

DR. SAGUES: Yes.

MR. COGAR: It's a cutout that shows the inside. It shows both the barriers. It's a cutout of the assembly that shows the carbon steel outer barrier along with the incanel or high-nickel inner barrier.

DR. SAGUES: Oh, I see. Okay. Thank you.

MR. COGAR: After both lids are on, then we simply machine the barrier -- the cleanup on the inner part of the assembly and machine a register on the bottom and a register on the bottom sides to assist us in setup for the closure welds.

The closure welds are done in the same method, same setup, except that they're done at the factory -- or at the waste repository, waste handling building.

The package is placed on a turntable. Why the turntable? It's a very stable base, it's low maintenance, and it's easier to track the weld groove.

There's been a lot of talk on both sides about whether it's easier to rotate the waste package or it's easier to rotate the weld head. Our opinion is that it's easier to rotate the package, it's easier to track.

This is state-of-the-art in most of our large vessel manufacturing facilities.

They do use a rotating weld head in some of the shipping casks, but those are places where you have limited access, you can go in there, you can set it up, very difficult to set up in a totally remote environment.

This weld is made with automatic tungsten arc process. It's a very slow process, admittedly, but it's also a very clean process. You have very little chance of defects.

Currently, we're using the hot wire method, because it allows us to deposit a lot more weld in a shorter time. I'm going to talk about development programs and you'll see how we got to there.

After the outer -- the inner lid is put on and ultrasonically inspected -- and this is all done remotely --

we put the inner -- the outer lid on, it's welded with the same process and ultrasonically inspected the same way.

I'll talk just a second about the development programs that we have done over the past. Most of them are related to the closure weld.

We started in 1996 and we performed a process comparison. It was done by a lot of the weld engineers at the home office.

They did a comparison and came up with auto gas tungsten arc method as being the preferred method, mostly because it is a very clean weld, a very high acceptance rate.

We produced straight-line mockups to check the groove configuration. The groove configuration is a narrow grove. It is a very narrow weld, about 3/8ths of an inch, so we deposit very little heat, and it works rather well. It is state of the art in the industry.

At that time, we were using A516 carbon steel as our materials and 825. We have since gone down to 625. And we did all the normal tests, the bend and tensile.

In 1997, we produced a mockup, basically looks like this. It is an exact duplication of the diameter of the waste package, but it was only about 44 inches long.

This was produced with a shrink fit method. We did the welding on this with the automatic gas tungsten arc method and a narrow groove configuration. The materials for this were alloy-625 as an inner barrier and A516 as the outer barrier.

We did ultrasonic tests on these. We also did ultrasonic tests to try to ascertain what the gaps were between the cylinders. We were able to only establish about 13-percent contact. That's all ultrasonics would tell us.

We performed thermal tests on this, basically just to see if there was anything about the two barriers that we didn't know. We recorded all the residual stress measurements. These were given to our structural people, and they're looking those over.

The mockup itself was sent to Lawrence Livermore for testing, and they are going to test and we're going to see what the gaps are and what the residual stress measurements and everything are.

We've had a lot of talk about the shrink fit, about whether it's good or bad, whether it has residual stress.

We have a lot of intelligent and educated people on both sides of the question. So, our answer was let's send the whole mockup there and let them test it to see what's actually there.

For 1998, we're going to continue to test at Lawrence Livermore. That's not actually part of our development program, but it's our mockup that we sent them, so we put that in, and we plan on building another mockup using the shrink fit method.

We want to do more development in the ultrasonic area to see if we can ascertain what the gaps are in there a little better. We want to do a stress relieve on this vessel to see what happens when we stress the shrink fit or stress relieve the shrink fit.

We want to do this remotely. It's very important that we prove that we can do this remotely. We've proved that we can make the weld; making it remotely is a little different story.

We want to do dimensional inspections to see what happens during the shrink fit, and the mockup will basically the same except they'll have lids on both ends.

Current tie-ins were to use 6.5 and alloy C-22. Because there was a lot of talk about C-22, probably we'll be more into the C-22.

And that's the development program and how we fabricate the waste package.

DR. BULLEN: I'm going to use the session chair prerogative -- Bullen, Board -- and ask the first question or two, then I'll defer to the other board members.

With respect to the stress relief of the lower vessel head lid after welding, it's not something that you plan on doing in fabrication, is it?

MR. COGAR: Yes.

DR. BULLEN: Can you protect the 350-degree C clad limit of the internals of the package by doing stress relief?

MR. COGAR: I thought you asked the lower lid. The lower lids we're going to stress with the package. The upper lid we do not intend to stress relieve.

DR. BULLEN: Okay. So, no upper lid, no post-weld stress relief after you've closed the can, because you can't -- you would exceed the 350-degree C clad temperature.

MR. COGAR: We would not like to try to do that. I think we would, yes. There are some ways we could try to do it. We don't want to have to do that.

DR. BULLEN: Okay.

The other follow-on question is, in the shrink-fit, is there a plan to do it in an inert environment, are you always going to do it in air? That hot metal in open air kind of oxidizes a bit, and I was wondering if that was a problem.

MR. COGAR: We did get a thin layer of oxidation on the mockup, it was there, so we're aware of that problem. We've talked to a number of manufacturers, and we know that it is possible to do that in an inert environment.

The question and the reason we want to test this mockup is to see is it a problem, is the small layer of oxidation we had a problem or is it not, and that's what the structural people, the materials people have to tell us.

If it is, then it is highly possible we can do this in an inert atmosphere.

DR. BULLEN: Okay.

Last question, and then I'll defer to the board.

In dry cask storage environments around the country, delayed hot cracking or delayed cracking seems to be a problem with some of the closure welds. Could you comment on your ability to avoid such a problem?

MR. COGAR: I won't comment on the cracking.

DR. BULLEN: No, your ability to avoid such a problem is the question.

MR. COGAR: We have a different weld design, much different from what they have. We have a narrow groove weld. We put it on with a different process.

We've done something in the order of 6 PQs and two mock-up welds, and we haven't had any problem with cracking. I don't anticipate that we will. I think the cracking problem can be resolved by the process you use, the parameters you use, and the joint design that you use.

There was a lot of interest in the closure weld we made on the mockup due to those problems, so I'm aware of them.

DR. BULLEN: Okay.

Other questions from the board?

Arendt?

DR. ARENDT: A couple of important items, of course, are weld joint design and then the welding procedure, and I guess you just alluded to the weld joint design.

Have you finalized the weld joint design yet, or are you still working on that? Do you have the welding procedure developed? Are the welds defect-free, or are you permitting some defects, and if you are permitting defects, what would be the size of the defects? And I guess that's it.

MR. COGAR: We have finalized the weld design, the weld groove design, basically. We will tweak it just a little bit in this, because we learned something off the last mockup.

We found no defects in the PQs we ran in '96. They were totally free of any defect. There was not even any recordables in it, so -- there's a difference in recordable and defect, but there's no recordables in it.

The alloy-625 weld we made last year had no recordables in it, it was clean as far as any inspection technique we can use.

The carbon steel did have three small defects in it, and it had -- they were -- the reason I said we would tweak the weld groove, the weld groove was slightly too large for a single bead.

We want to bring single bead all the way up. We didn't start double-beading in time, and we did have non-fusion on one of the walls which we did pick up.

So, the answer to your question, yes, we're still tweaking the weld groove. Basically, it's designed.

We do have the procedural qualifications. We've done ASME procedural qualifications on every weld we've made, so we have a record, and it is a procedural qualification. Anybody can pick this up and go make this weld.

There was a question before -- I guess it's in that same vein -- about manufacturing defects, referring to the welds. We believe that we will find anything that's in the weld.

We have looked at the Navy nuclear industry, we looked at the commercial nuclear industry. We've been unable to find a through-wall defect in any of those places.

We can never say never about anything, but there is some studies going on, and I don't remember the number of the probability of the defect, but it was very small.

DR. ARENDT: You said reportable defects. Does this mean that -- have we determined what the smallest defect is that you could see by the various methods of non-destructive testing you're using?

MR. COGAR: I don't think answer -- your question is the smallest one you can see. Ultrasonics picks up down in the neighborhood of 1/16th of an inch linear, so it's very small, and I'm not sure that that's the limit of it. That's the limit of my knowledge of it.

We currently are using the ASME acceptance criteria for these welds.

DR. ARENDT: Okay.

MR. COGAR: We don't know that that's going to be finally what we use, but that's what we're using now, and that's a good starting point.

DR. ARENDT: You will make an engineering analysis of those defects, the minimum defects.

MR. COGAR: Yes. How small can they be?

DR. ARENDT: Yes.

MR. COGAR: I didn't mention -- we do make a concession to defects and corrosion in the welds, especially the closure welds, in that the plates themselves are -- the outer plate is 110 millimeters thick, the inner plate is 25 millimeters, so we've increased the thickness of the plate to compensate for the weld, and we have minute small defects in there that we don't pick up, and we may have some -- you know, welds do corrode faster than any other part.

DR. BULLEN: Cohon, Board.

CHAIRMAN COHON: I'm just asking this question because Dan Bullen told me to.

Could you say something about the stress on the welds and the seams during lifting in the repository?

MR. COGAR: I'm a manufacturing person and not a structural engineer, so I'm not going to try to answer that from an engineering term, but obviously anytime you weld you do have stresses.

We did see the -- in the measurements we took of the closure weld on the mockup, we did see stress, we did give that to the structural engineers, and that need to do the analysis on that.

I know it's there, but I wouldn't want to address it.

CHAIRMAN COHON: So, it's something that someone is working on.

MR. COGAR: Yes.

CHAIRMAN COHON: It's being analyzed.

MR. COGAR: And that's why we do the normalization in the fabrication process, too. I mean that takes care of -- that does relieve some of that. Since we can't do it in the closure weld, we limit that to a very small weld groove, very low heat.

DR. BULLEN: Doering?

MR. DOERING: Tom Doering, the M&O.

The structural group that we have actually is my group, and we have taken those stresses and we're looking at those stresses right now, and during our accident and off-normal situations, we take those stresses in consideration.

So, that's why we're feeding back and forth. That's why Jerry is actually doing some stress measurements on it, so we can get that and feed that back into the design.

DR. BULLEN: Other questions from the Board?

[No response.]

DR. BULLEN: Consultants?

MR. KUHN: Klaus Kuhn. Did you or someone else in the program look into the possibility to fabricate the container by using cast iron?

MR. COGAR: The materials people looked at all the materials, as I understand it, and Dave Stahl would probably answer this better. He's our materials person. As I understand it, they don't stand up as well in the corrosion.

Dave, is that correct?

MR. STAHL: Stahl, M&O-Framatome Cogema Fuels.

The corrosion resistance is about the same for the cast and the wrought carbon steels.

I think there are other considerations, mainly in the fabrication, that Jerry could respond to regarding the selection of the materials, but we have both of those materials under test, and the response so far has been about the same for either cast or wrought.

MR. COGAR: From a fabrication standpoint, we can use either one. Obviously, we can fabricate anything you want to do, kind of like Jack Bailey said yesterday, you can engineer anything and we can fabricate it. They are more expensive. They cost more to do. But I have seen them used as cylinders, yes.

DR. BULLEN: With the chairman's prerogative, Mr. Snell?

MR. SNELL: If I may, I wanted to offer a couple of quick comments.

I'm going back to some of the earlier presentations, but first of all, with regard to materials selection generally and all of the things associated with it, the requirements that we're faced with are very, very demanding, as you might imagine.

We're talking about performance on materials for thousands of years.

So, the logic or the sequence, at least in my mind, is first find a material that you believe will behave adequately for the durations that you have to contend with.

Once you can identify that material or those materials, then you can begin to look at some of the steps that follow, namely fabrication and do allowances for the influences of fabrication -- residual stresses, welding quality, and so forth.

There will be a full array of quality control programs imposed on everything that we do in this facility, and necessarily, there will be defects, if you will, or weaknesses, whatever.

Weld joints, typically, are not as good as parent metal unless you do something to compensate for that. Those will be cranked into the performance evaluations that we make for the long-term performance in the repository. So, that's a point I wanted to make.

There was a comment about allowances for rock fall and degradation scenarios that we might have to contend with, and I would just comment that we're developing a set of degradation scenarios for the underground which include degradation of the emplacement liner and degradation of the rock behind the liner once the liner has begun to fail, rock fall, and some of the expectations that might result from that, and we'll be able to show that to you later on. I don't have anything in the program right now.

And lastly, we had a number of questions of various kinds about other experience.

One aspect of what we're going to do is take full advantage of lessons learned, and that comes from several areas -- lessons learned, you mentioned, in surface storage, degradation experience on weld or weld failure mechanisms.

There are a number of other places where we can get lessons learned experience and where we'll need to take full advantage of those lessons learned as we develop the repository design.

Thank you.

DR. BULLEN: Thank you, Mr. Snell.

Now, chairman's prerogative -- we have 45 seconds left, but Dr. Knopman had a question that would have been answered by Joon Lee had he been here.

I'm going to take the last 45 seconds and give it to Dr. Lee for a brief comment. This was with respect to oxidation in a dry environment long-term, what's the waste package life? Is that accurate?

DR. LEE: Assuming that the dry environment all the time and carbon steel will under-load dry oxidation, the rate is extremely slow, but my guess is maybe several hundred thousand years.

We have corrosion-resistant material in there, and the dry oxidation of that corrosion-resistant material is even lower. So, my guess is it's kind of infinite.

DR. BULLEN: With that, I would like to declare this session closed. We have a 10-minute break, and then we're back for the second morning session. Thank you very much. Thank you to all the speakers.

[Recess.]

CHAIRMAN COHON: I would like all present to be reminded that there is a public comment period immediately after the end of this next session at approximately one o'clock, and when he comes back in the room, Alberto Sagues will be convening this session.

DR. SAGUES: Good morning again. I'm Alberto Sagues.

We are going to have quite a long session here. It's going to take us beyond one p.m. So, let's get started, and I would like to ask the speakers, as well, to, indeed, keep within the allocated time.

At our summer meeting in Las Vegas last June, we heard the summary of the expert elicitation for the unsaturated zone, and that was very successful.

Since then, another expert elicitation project has been completed. This one is for the degradation of the waste package. That is what we will hear about today. We will follow an agenda similar to the last time but with some differences.

Last time, Dr. Kevin Coppersmith explained the elicitation process, and this time, Kevin will summarize the process and is going to give us an indication of how he proceeds.

Last time, we had a couple of experts showing their individual views.

This time, again we're going to have two outside experts who are members of the expert elicitation panel, will give their views, and the experts are Dr. John Scully, an Associate Professor of Material Science and Engineering at the University of Virginia, and Dr. David Shoesmith, who heads the corrosion and electrochemistry activities at the Atomic Energy of Canada, Limited's White Shell Laboratories in Penowa, Manitoba.

We're going to close with two talks.

Dr. Joon Lee will tell us how the results of the waste package or the degradation expert elicitation will be reflected on the TSPA-VA, and Dr. David Stahl will update us briefly on the status of the materials research program and how it is changing to reflect recommendations made by the expert elicitation panel.

Dr. Lee is both a chemical engineer and a materials scientist. He has been with the project some three years, and he develops waste package performance models for the TSPA.

I'd like to add Dr. Lee's Ph.D. is in material science from Penn State.

At the last meeting, we had Penn State scientist

Dr. Dela Roy, who spoke about concrete. Concrete is not part of the immediate waste package, but it's just about the next thing, nearest thing to it.

We're going to have, I understand, a few meters cube of concrete for every linear meter of our repository, and the concrete is quite important from the point of view that it determines both the environment that these materials will be exposed, and also, the concrete may play an important role on what happens to the radionuclides once they are released from the package.

We want to keep in mind what happens over there. We want to see the connection and the impact that the concrete will have in the near-field environment, and I think that that may impact on some of the conclusions, some of the findings that we're going to be discussing today.

Our last speaker, Dave Stahl, is also a chemical engineer and materials scientist. He is with Framatome Cogema and has managed waste package materials efforts for six years.

Since the waste package is made of metal, I don't suppose his responsibilities pertain to concrete, as well, but maybe he will be able to address something along those lines.

Now, then, our first speaker, Kevin Coppersmith -- he is a geologist with Geomatrix and conducted and facilitated the waste package expert elicitation project.

Take it away, Kevin.

MR. COPPERSMITH: Thank you very much. It's a pleasure to be here in front of the board again. Forgive me if I mistakenly talk about earthquakes, volcanoes, or unsaturated zone hydrology. I have to remember where I am and what we're up to.

I want to go through the purposes, the objectives of the elicitation process, and I will be talking very, very briefly about the process, only one view-graph.

The process followed is very similar to that described last time in the context of the unsaturated zone expert elicitation. So, I will briefly go through that.

Then I will talk about some of the results. I'm going to be talking about some of the more qualitative results.

Joon Lee will talk about some of the more quantitative expressions of uncertainties and certain key parameters and some of the distributions or ranges in those uncertainties across the panel.

The objective of this elicitation is to quantify uncertainty in key aspects of the waste package degradation. Uncertainty is the theme throughout these processes. They're designed to help in this process.

We use a panel of experts to help quantify, express uncertainties, and in my mind, uncertainties need to come from experience, not only site knowledge or detailed knowledge of the subject matter at hand but also a perspective that comes from a broader range of experience from work on other projects as well as Yucca Mountain, and so, we use a panel of experts to provide that perspective.

This is a snapshot in time. It's a snapshot of uncertainties that we have at the present level of information.

This is -- we tried our hardest in this process to let everyone know about the present data that exist, that have been developed for the Yucca Mountain project, as well as other pertinent data sources, but it's only a snapshot.

It does not replace data. It's an opportunity to get an expression of uncertainty at a certain point in time.

As we'll talk about, we have members on the panel who are considered to be empiricists, John Scully, the type who feels that data are the best drivers in developing expressions of uncertainty, and we also have those that are more used to developing a subjective expression of uncertainty.

I think that blend across the panel provided a good opportunity to see how uncertainties are expressed.

Right now, all the members of the panel would conclude that, in fact, the data that exist at the present time for certain aspects of waste package degradation modeling are deficient, are scarce, and some particularly related to the corrosion-resistant materials.

Our ongoing test, as Dave will talk about, should help in helping fill some of those gaps in information and data.

This is kind of a series of expert elicitations. They're designed to be part of the schedule for the viability assessments.

They're done very quickly. The experts are not given much opportunity to review the available data.

They're not given much opportunity to interact. We have three workshops, and they need to develop their interpretations, I would say, quickly so that we're on schedule with the viability assessment.

These are the members of the expert panel. It's a blend of corrosion scientists, materials people, those that have been involved with performance assessments and those that haven't.

I was pleased to hear that John Scully and David Shoesmith will be representing the panel. I think they're essentially the anode and cathode of the panel, and I'm not sure what the throwing distance is in this solution, but we'll talk about that later.

David Shoesmith always wanted to begin his discussion with an anecdote, some sort of discussion of a similar experience related somehow to beer in every one of those discussions.

John Scully, who can go on talking in his 96th minute of a 30-minute talk at seven p.m., with the same energy that he had when he began.

Quite a panel.

Here is my one process slide that deals with the waste package degradation expert elicitation, or WPDEE, as we call it.

It goes through a process that essentially conforms with all recent guidance related to expert elicitation, including the NRC's branch technical position on expert elicitation.

We have essentially three workshops that take us through available data, dissemination of that data, discussion of alternative models and interpretations. We had a field trip to the ESF to get a feel for the geology and the nature of the facility itself.

Elicitation training -- an opportunity for the experts themselves to present their preliminary ideas to other members of the panel and get some feedback in the third workshop.

Elicitation interviews -- these are formal interview sessions, day-long sessions with the experts.

Feedback -- they have an opportunity to see the assessments that all the other members of the panel have made before they, in turn, finalize their own interpretations.

The documentation that exists at this time is preliminary. I want to be sure that, when we go through these conclusions and those that Joon Lee will see, that these are preliminary in the sense that we're still trying to understand the results and their application to the TSPA, how they'll be used.

Some parameters were provided to us as cumulative distribution functions, others as other types of distributions. So, we'll still in the process of working that out.

We do have an opportunity, though, in this discussion, to talk about some of the key conclusions and the range of assessments that have been made across the panel.

These are the items that we will step through, the combination of my talk and Joon's.

We dealt quite a bit with the corrosion allowance material.

Let me change the sequence here, if I could. Let me talk first about assumptions.

The environmental conditions, design conditions are our responsibility as members of the methodology development team.

We provided these assumptions to the panel, so they would not have to worry about the potential of, let's say, both environmental conditions and temperature and relative humidity.

We told them to assume various temperatures, assume different time histories of relative humidity through the lifetime of the repository.

We also told them to look at the potential for drip, for water seepage into the drifts and what that might mean if it occurred early on during the thermal and RH history of the facility or if it happened later.

We told them basically the same thing with water chemistry, pH, assume that the possibility of alkaline or pH's of 10 or greater are possible as water drips through the concrete but also consider more neutral conditions.

Likewise, looking at chloride concentrations and other aspects of the water chemistry, to look at those ranges or given situations when you make your assessment.

With design conditions, we laid out essentially the basic waste package designed. We had them consider carbon steel or Monel 400 as the CAM, or corrosion allowance material, to consider alloy 625 or C-22 in dealing with the CRM.

And likewise, other aspects of the design were summarized to them early in the project.

So, they are not responsible for the presence of drips or high pH drips. They were told to assume those, and other parts of the project will deal with the likelihood of occurrence of those conditions.

These are the key assessments. Let me just finish.

We'll go through some of the key assessments related to the corrosion allowance material, then the corrosion-resistance interval error, and other key issues that we've dealt with, like the potential for microbiological influence corrosion, ceramic coatings, and so on.

And finally, the last item on here, which is very important, especially to experimentalists and purists -- we asked them for recommendations, since they had defined this uncertainty, what recommendations they would have for ways to reduce it, what focused studies could be carried out that would specifically address those uncertainties and reduce them, and normally this type of question to a peer review panel, as opposed to an expert elicitation process, would be the first and foremost item.

Here we asked that question only after they've gone through screaming and kicking in some cases, giving us their assessment of uncertainties.

So, these, often, are very specific and, I think, provide additional information to the project that could be very helpful.

I want to go through just some of the aspects related to the corrosion allowance material, and again, it's impossible, across a panel of six people, to give a meaningful summary unless they all said the same thing, but we're not looking for a consensus across this panel, there's no need for it, and in fact, it would be artificial unless it happened accidentally.

So, what I'm getting here, in reading through the summaries and the discussions, are some of the common themes that have come through. Every one of these would have someone say -- express this somewhat differently.

Determining the period of dry oxidation of the carbon steel during the assumed initially hot and very dry environment, most of the experts expect a very thin oxide layer to develop during this hot, dry period.

There were some who felt that, in fact, it may be thicker, but there were very distinct differences of opinion about the spalling potential of the oxide layer that develops, ranging from a very thin, tenacious, non-spalling oxide layer to one that would be subject to spalling, large parts of it spalling off of the CAM.

Oxygen depletion during this early period was something that particularly Peter Andreson felt should be considered.

It's likely that oxygen and perhaps carbon dioxide levels will be low during that period, but it was not considered -- given the lack of information about or knowledge about how much the mountain breathes, it was not considered to be something that was an assumption, assumed, for example, low-oxygen partial pressures, but it was something to be potentially investigated.

I will skip over the temperature thresholds and relative humidity thresholds. Joon will talk about those assessments.

Drips -- I would say, out of all of the things that we dealt with, the importance of drips is foremost in waste package degradation.

Drips provide an opportunity for a lot of things to happen. They provide potential nutrients for microbiological-influenced corrosion to occur.

They provide an opportunity for elevated pH's that might lead to a different corrosion mechanism for the carbon steel. They are very important, not only their location but frequency of drips and persistence through time.

Do they occur randomly, or do they occur for a while?

During the process of drips occurring early is a period where they will immediately evaporate as they hit the surface of the canister, leave behind salt deposits, lead to different corrosion mechanisms of the canister itself.

So, I think, as you will see in the recommendations, all the experts felt that the projects to be looking to, the frequency, location, and persistence of drips is a very important aspect for additional study.

Corrosion modes of the CAM -- in neutral conditions, general corrosion is expected, relatively uniform corrosion of the canister, but under high pH conditions the potential exists for what we call high-aspect ratio pitting.

High in this case -- the mean, I think, across the panel of aspect ratios is about three or four, so we're not dealing with very long, narrow types of pits, ones that are, though, somewhat distinct from general corrosion, but the aspect ratios are not very high, and as we'll see when we get into the concept of galvanic protections, because of that relatively low aspect ratio and the potential for neutral conditions, that in fact the galvanic protection is not seen as a very effective mechanisms for protecting the inner CRM.

We asked them also about the geometry of corrosion processes, where do we expect drips to occur, what do we expect humid air corrosion and other corrosion mechanisms to occur?

This is the top of the waste package, is the area that would be subject to drips, also high pH and potentially salt solutions and so on.

The bottom of the package also -- remember the discussion this morning -- the pedestals are designed to last for a few hundred years. The canister then may be inverted, and there's a possibility of ponding or bulk water conditions at the base of the canister, as well.

More on the corrosion allowance.

The outer barrier, pit density, and pit diameter were assess from the panel, and Joon will show those.

A quick thing for the discussion at this meeting in terms of corrosion rates -- we put them all into a common form.

Corrosion rates were provided by the panel in various ways, but in general, this rate term is a function of the general corrosion or passive dissolution rate plus a localized corrosion rate times time raised to the N value, what specifies the nature of decay of that rate with time.

This is a generalized form that allows us to look at corrosion rates provided by the panel as a whole.

In general, for the CAM, we dealt with the general corrosion rates -- there was endorsement of the TSPA-95 approach and corrosion rate methods that were used in that process.

When we get into assessments of localized corrosion, particularly the CRM, our data sets go down, the ability to use a lot of experimental data goes down, and of course, these are areas that were the focus for suggestions for additional work.

We asked them about an alternative to carbon steel for the CAM, Monel 400.

The corrosion modes -- first of all, general corrosion is expected under these bulk environmental conditions that we have talked about without deep pitting.

There are various pros and cons, but in general they look like this, the corrosion rates.

The general corrosion rates are slower than they are for carbon steel.

It's passive in most environments, including the high pH type of environment, which might be expected from drips through the concrete, seems to be resistant to stress corrosion cracking and hydrogen embrittlement.

Some detractors are that it may undergo dealloying in the presence of microbiological influence corrosion. There are fewer data to establish -- to defend this type of outer barrier than there are for carbon steel. We know a lot about carbon steel.

Also cited was the fact that Monel 400 costs quite a bit more.

Under the CRM, the corrosion resistant material area, the issue of galvanic protection -- the extent of that galvanic protection, the galvanic couple, is determined by the throwing power which, in turn, is a function of the conductivity of the solution, and given the conditions that we're dealing with here, it was felt that, in these cases, because of that conductivity, we're dealing with relatively short throwing distances, on the order of millimeters or centimeters.

Also, the geometry of the pits, as I mentioned before, were not very high aspect ratio pits, so we're dealing with galvanic protection during the high pH conditions, potentially.

During neutral conditions, general corrosion is occurring at the CAM, and potentially, there would be very low protection of the inner barrier.

So, under neutral conditions, where we might exposing larger surfaces of the CRM at a time, it was felt that galvanic protection would only be effective over relatively short periods of time.

In the case of high aspect ratio pits coming from alkaline conditions or drips, longer -- maybe on the order of hundreds of years -- but in general, because they're not dealing with extreme geometric shapes, it was felt that galvanic protection would not be an effective long-term process, and I'll ask that, somewhere in the course of the discussion, that John and David mention their assessments of galvanic protection.

If you don't already have it in there, then maybe someone will ask you a question.

Corrosion modes for the CRM are -- basically, the general corrosion is expected under the bulk environmental conditions that we're looking at in terms of pH and temperatures and so on, but localized corrosion is possible, probably crevice corrosion would be the most likely mechanism, pitting is possible, as well, but we've asked them, in looking at this, where does that potential for localized corrosion come from, and it's primarily due to high chloride, more aggressive environments in terms of pH, the presence of Fe^{+3} and oxygen and so on.

Most of the conditions that will lead to localized attack of the inner barrier come from the presence of drips in one way or another. So, once again, it highlights the importance of assessments of that particular condition.

One thing that was noted, as we talked about before, the present design deals with a shrink fit between the inner and outer barriers.

That gap was the subject of some discussion by members of the panel. It was felt that, in fact, that gap may serve as a conduit or pathway for moisture to move along, much the same way as a crevice would.

Pit density and corrosion rates were also assessed, and Joon will summarize those.

Let me deal with some of the other issues.

We were fortunate on our panel to have Brenda Little, who is an expert in MIC. The others made some assessments in this area but were relatively limited. This is Brenda's area of expertise, and she provided a number of assessments that we could use in terms of quantifying uncertainty.

It's controlled by the variability of nutrients, water, and electronic acceptors. You need all three of those to make it work, and we also need to be down into temperature and relative humidity environments that allow for development of microbes and allows them to populate.

Iron-reducing bacteria are the most likely organisms for attack or MIC of the CAM.

For the CRM, drips are required, primarily to provide nutrients but also to set the proper conditions for MIC to occur.

It was interesting, in discussing MIC, it looks like the effect is primarily one of dealing with the probability of initiation of corrosion, initiation of a localized attack or initial pit density, rather than a control on the rate.

MIC helps the conditions both help initiate pits and gets the process going. Once that gets going, the process would proceed at rates that are comparable to what we would expect for that type of material.

The issue of welds -- it was noted that there is an enhanced potential for stress corrosion cracking unless full stress anneal-type techniques are used, which they are assumed that they will be, but nevertheless, that potential always exists when we deal with a welded type of connection.

It as not expected that there would be much increased potential for localized corrosion or for MIC given the welding techniques that are anticipated.

Ceramic coatings -- I'm not sure we had any real strong ceramic experts on the panel, and correct me if I'm wrong, but those who did have expertise in this area felt that there were potential problems related to cracking of that, both due to mechanical loads, potentially, or due to volume expansion as the CAM corrodes itself, leaving corrosion products that are of larger volume and lead to cracking.

In general, it was concluded that the ceramic coating is not recommended as an approach.

Finally, a little bit with stress corrosion cracking.

It was noted that the potential exists for residual stresses related to the shrink fit process itself, which obviously uses a thermal load type of process, and it wasn't clearly how that would work, and I know additional work is going on to look at that from the discussions this morning.

As I mentioned before, the weld stresses using the techniques that are anticipated can mitigate much of the residual stresses.

Radiolysis -- it was felt that, in general, given the time history that had been provided, most of the radioactive decay will have lowered the dose by the time conditions reach 100 degrees C and relative humidity of 65 percent.

We're interested in finding out from you, Dan, your experience where radiolysis has been a problem, because they felt that, in fact, it probably isn't.

Recommendations for additional work -- again, you hold off these guys, they always want to go to this first. We asked them that last. What things could be done or should be done to help reduce uncertainties? I just summarized them into four major categories here.

Number one, which isn't -- wouldn't be done by these experts, is to establish the environmental conditions.

What's the likelihood of drip? What's the likelihood of high pH conditions dripping through the concrete? These are the focus of ongoing studies in the rest of the program.

Testing the carbon steel and looking at high pH conditions, there is very little information that tells us what these high aspect ratio pits look like, what the modes look like, what the rates are. That is an area of additional testing.

Experiments, obviously, related to the CRM -- localized corrosion initiation, how do you deal with the probability of initiation?

Almost all the information that we have now shows that initiation under very aggressive conditions, extremely low pH, high temperatures and so on -- how can those be used to give us some information about what we expect or under the expected conditions within the repository?

And finally, for MIC, it was suggested that a mass-balance inventory be done looking at all the -- the availability of nutrients going into the system, the various types of, again, temperature threshold, other information that's important to the establishment of MIC as a way to get an idea of its potential and effect on the waste package.

And that's my summary.

DR. SAGUES: Thank you, Kevin.

Before we open this for questions, I would like to indicate that the quantitative application of the information developed by the panel will be given in quite a bit of detail by Joon Lee later on, and I think that you, Kevin, have centered on the qualitative aspects of the recommendations of the panel.

We'll start first with any questions that the panel may have.

Paul?

DR. CRAIG: Paul Craig, Board.

One of the things that the board needs to worry about is potential showstoppers, and last spring, I began to go through TSPA-95, and the thing that jumped out at me at that point was the corrosion problem, looked like an important potential showstopper.

So, I spent a lot of time trying to understand what's going on, and the more I'm learning about it, the more I feel that that may move to the very top of my list of potential showstoppers.

So, what I want to do is to explain why and put a question which I hope will also be addressed by John Scully and David Shoesmith.

The issue is this. As the mountain changes, the engineered barrier clearly becomes more important and the corrosion is critical to the success of the engineered barriers.

When I think about criteria for deciding whether something is going to be acceptable to me when we go out into the very distant future, the concept of scaling laws emerges right up at the top, because we are extrapolating into unknown regimes.

Now, some areas we understand very, very well.

We understand how to look at radioactivity. No one argues about radioactive decay or the heat release thereof. That's really on solid ground.

Activation processes, where you have good theoretical reasons to believe that some process goes to Z to the minus something over KT you can test in the laboratory -- those are pretty fine.

Volcanism and earthquakes -- the arguments are quite different, but again they look very sound.

On the other hand, when I look at corrosion -- and I went off to the expert elicitation panel, a portion of it, and I went to a portion of the WPDEE panel and talked to the people there -- what I seem to be finding is -- and this is a critical point -- there does not seem to be a sound theoretical framework for understanding corrosion processes, and what that leads to is that the extrapolations which get used in the analysis are empirical, they are not based on theoretical grounds, and if you're doing an empirical extrapolation into the distant future, it seems to me this is a recipe for real problems.

I don't know how you deal with that, and maybe there are theoretical bases for understanding corrosion that I haven't learned about, but the question I'd like to put to you and to the other panel members is, can you provide a scientifically sound basis for doing extrapolations in the very, very unusual regimes in which you're asked to do extrapolations?

MR. COPPERSMITH: I will not even attempt an answer to that other than to say, in general terms, obviously everything related to Yucca Mountain and its performance is long-term.

So, those aspects, like you mentioned before -- volcanism, earthquakes, other disruptive events -- have a frequency of occurrence or probability of occurrence that comes from a certain data set, either observed, theoretical.

I think that, from my point of view, the best is a combination of the two.

Obviously, a corrosion test, 1,000-hour corrosion test extrapolated out 100,000 years blindly will be a problem, but on the other hand, I think that the -- and hopefully these two -- these are probably the best to deal with that issue.

Hopefully, you have a theoretical basis that is tempered or uses the empirical observation but gives a reason for, for example, a particular growth law and its extrapolation out well beyond experimental or observational information. I think that the combination of the two are critical.

I don't know if you would like to respond to that now or later on.

That's a key part of the problem.

MR. SHOESMITH: I have just one comment on that.

I think extrapolation is always going to have to rely on, effectively, an empirical measured database, but the justification for its use involves a whole series of surrounding experiments which justify that you understand the assumptions that you've made.

So, the great bulk of the work is not actually going to show in the performance assessment. It's going to show in the quality of the understanding that you have to justify the assumptions that allow you to make that extrapolation.

That's not easy, but I will be confident that it can be done.

DR. CRAIG: Let me pursue this for one more point.

I don't ask that things be easy. I do ask that they be possible and that they be done.

And it seems to me that what does need to be done in order to convince a skeptical scientific community is that there be clear statements of the scientific underpinnings of these extrapolations.

DR. SAGUES: By the way, the previous speaker, for the benefit of the record, was David Shoesmith.

Dave, maybe we should wait until your presentation for additional questions.

Dan, did you have a question?

DR. BULLEN: I have first a quick comment and then a question for Kevin, so don't go away, Kevin.

In response to your comments on radiolysis, my data are, indeed, interesting, but they are for room temperature and 100-percent humidity.

So, I always ask Dave Stahl this question. Are there radiolysis experiments planned to verify that, indeed, at conditions of 100 C and 65-percent relative humidity, that's probably not going to be an issue with respect to the corrosion allowance material?

David.

MR. STAHL: David Stahl, M&O-Framatome.

The answer is yes, we do have experiments planned. Unfortunately, the budget being what it is, these have been deferred into FY '99.

DR. BULLEN: But will they be available before LA?

MR. STAHL: Yes.

DR. BULLEN: Okay. That's the key concern. If you could put the issue to bed before LA, then we've probably got it resolved, and I don't necessarily disagree. I just would like to see data that would support the 100-degree C, 65-percent relative humidity issue.

Now, with respect to Kevin, having done a number of these expert elicitations and taking a look at sort of a comparison between the different ones, how does this one compare with respect to range of variability of the experts' opinions?

Some of them have had very broad disagreements, might be a way to put it.

How did this one compare?

MR. COPPERSMITH: Number one, there are a lot more engineers in this mix. So, it tends to be a more subdued crowd, more ties.

Geologists get dirty. Of course, these guys did go down there.

Actually, I think this was an interesting blend of disciplines, really a very extreme difference, almost pure researchers to those that are asked to make decisions almost every day.

So, I think it was difficult, in some cases, to get those who would want to study the problem more to make an assessment.

You know, I think, for example, John Scully will give a probability distribution and we commit to do another experiment, and I think that's important to the process that there's a commitment made that, in fact, this won't be the end-all, that if I give you a distribution, that won't be the end of it.

In fact, you need to actually quantify these assessments, particularly for some of the observables that aren't observed -- the corrosion of C-22 -- very, very little data that relate to this, particularly pertinent data to the conditions we're dealing with.

We need some commitment to deal with that and go beyond just this probability distribution that I've given you.

CHAIRMAN COHON: Cohon, Board.

Am I correct in believing that this panel was smaller than most of the ones that you've done?

MR. COPPERSMITH: It's one smaller that the usual six.

CHAIRMAN COHON: Okay. So, this is fairly typical.

DR. SAGUES: Very quickly, any questions from the experts?

MR. PETERSON: Carl Peterson.

This question is prompted by the prominence of the drip shield in the conversation not necessarily on corrosion, but let me ask it while I think of it.

Yesterday I learned that radiation is the primary heat transfer mode and, therefore, it's desireable to have a relatively large bore compared to the package, you need the surface area. What does the imposition of a drip shield do? It shuts off most of that window.

MR. DOERING: Tom Doering, the M&O.

We have looked at the drip shield and also a coating onto it. The drip shield does add a little temperature to it, depending on the material properties of it.

Also, if we do a coating onto the waste package, we do gain some temperature internally, but again, dealing with the materials that we are dealing with and the initial evaluation that we've done, it still meets our 350-degree cladding temperature and also the wall temperature.

There are still more evaluations needing to be done in the drip shield and the coating, so those numbers are preliminary, and we haven't chosen the material nor a drip shield design yet.

So, that's where we are.

DR. SAGUES: Board members? Board staff?

MR. REITER: Leon Reiter, staff.

Kevin, I just want to follow up on a question from Dan about comparing this panel to other panels. I think what Dan was getting at is how the conclusions of this panel -- were there were variants among them, as compared to some of the other panels?

I would add to that, how does this panel compare in terms of sufficiency of data? In other words, did you walk out of this panel feeling, in this area, there is relatively more data available to reach a conclusion than with other panels, or the opposite?

MR. COPPERSMITH: I think, just in dealing with your latter questions, I think that, in general, there are abundant data related to carbon steel, there are limited data related to corrosion-resistant nickel-based alloys, and then when we deal with the conditions that we're dealing in the repository, which are not aggressive, potentially, certainly not the bulk environmental conditions, there are very few data.

I would say that, in general, contrast it, let's say, with 12 years of volcanism studies that had gone on, the amount of work that's gone on in the tectonic area and so on, I think there are actually very few data that are available for this purpose.

Now, one of the arguments that I think John Scully will make is that we don't need 20 years of corrosion testing data to be able to answer some of these key, say, rate parameters and some of the other aspects, the pit initiation probabilities and so on, that, in fact, more limited data can do that, and I guess, then, Dave will respond after that, that in fact now that the corrosion testing program is underway, those data will be gathered.

But I think, in general, what this gives us is a snapshot at the point that we asked these experts these questions, I think, for some aspects, we have very few data related to Yucca Mountain, and they had to rely heavily on their past experience in other projects, other alloys, other experience.

MR. REITER: How did you feel absent the volcanism, seismology, or some of the hydrology things? Did you feel the same way or differently?

MR. COPPERSMITH: I think you'll hear in January about the saturated zone hydrology. I think that spans a spectrum, too, the difference between a saturated zone and unsaturated.

Unsaturated, obviously, has been an area of heavy work, research, quite a bit of data, a lot of modeling. I would that far exceeds what's been done so far in the corrosion part or waste package degradation area.

I would say saturated zone probably will be less than what has been done so far here in waste package.

DR. SAGUES: Any other board staff?

[No response.]

DR. SAGUES: Thank you very much, Kevin.

We are going to continue, then, with the next presentation by Dr. John Scully on his individual opinions on the corrosion performance.

DR. SCULLY: Good morning.

I want to take a lesson from the political science profession at UVA, Larry Sabato. You may have heard of Larry.

He's asked every time it's a presidential election or gubernatorial election in Virginia to give a political comment, and he says that, when professors speak in public, they should try to avoid being irrelevant and not be so long-winded, and so, I will try to be short-winded and be relevant.

Of course, I'm no Larry Sabato.

What I'd like to do is I'd like to surgically, very surgically, hit on some of the -- what I thought were some of the key issues, and I've borrowed one of Kevin's overheads, so I'll leave this one up, and I'm going to hit just some of those issues and not every single one of them chronologically.

So, the title of my presentation is key issues identified that have uncertainty or that have great impact and also have uncertainty, and at the end of this presentation, I would like to follow up on Paul Craig's comment.

The first comment that I'd like to make from my perspective -- certainly, a plausible set of corrosion scenarios have been defined for the current waste package design, and that is to say that the corrosion phenomena governing waste package evolution have been identified, and there aren't any great mysteries as to what the corrosion phenomena will be given a given set of environmental conditions.

I think everybody agrees that, if there's a high pH drip condition, that we have to be concerned with his aspect pits on steel, and everyone agrees that, if the pH is 4, we'll have uniform corrosion of steel. Everybody understands what the factors are that control the dry oxidation rates of steel, etcetera.

And so, a flow chart, a logic tree defining the conditions that make certain corrosion scenarios more likely has been developed and is reasonable.

I'd like to say, secondly, as you've heard before, that a lack of accurate definition of the near-field and local on the waste package -- that is, environment, composition, its evolution, time, the drip rate and spacial distribution of drips, frequency and persistency -- is probably the greatest overall shortcoming to accurate waste package degradation performance assessment from a corrosion viewpoint.

That is to say, if you tell me -- if one knows what the pH is, what the composition is of the drip, how frequently it drips, so on and so forth, that corrosion information either exists for steel or could be developed. It's not an intractable problem.

The field of localized corrosion doesn't have to be embedded in order for these corrosion problems to be -- corrosion assessment and data to be provided this input into Joon Lee's probabilistic model of damage evolution.

So, that is the greatest issue that's a shortcoming.

So, I'm going to make some comments on, you know, some of the assumptions of modeling, but there are some good first-cut assessments, and they pale in comparison to this uncertainty, and so take that in the construction spirit in which it's intended.

And so, there must be an accurate definition of the water hydrology as well as the effects of the concrete liner, wet-dry cycling, and the effects of corrosion products interactions on the waste package itself, and this is needed.

Also, as part of my overview, I want to go into some details, is that there is general agreement on the steel corrosion allowance material, its dry oxidation rates, heated air corrosion rates, and aqueous corrosion rates are well-defined, and I think that that's -- they're reasonably defined, and the relative percent thresholds -- the threshold relative humidities are reasonably defined, and there are some differences based on whether or not we have hydroscopic salts, based on whether or not we have debris, clean surfaces versus dirty surfaces have different threshold relative humidities for these various processes, but I think they're reasonably well-defined.

The localized corrosion processes are not well-defined, mainly because there is a dearth of data, there is a shortage of data.

Lastly as an overview, uncertainties in the nature of probabilistic and deterministic localized corrosion damage functions do not present intractable scientific issues for the corrosion community to address, and that is to say that we don't need to invent the science, and Paul, probably to address your comment, the field of electrochemistry, the scientific underpinning of corrosion is extremely well-developed, extremely well-developed, and it wasn't probably 40 or 50 years ago, when most data was empirical, but the electrochemical science, the scientific underpinning of most corrosion processes is very well understood, and there are arrhenius functions for corrosion processes, there are thermal-activated processes that are very well-behaved, and there is great danger and extrapolation, but the dangers are the same sort of pitfalls, I think, that exist for other fields.

As an additional comment, the more knowledgeable one is about corrosion processes, the less stocastic people think that they are, and I work on this borderline.

We look at deterministic and stocastic aspects of corrosion, and I can tell you that people that aren't well-educated on the subject typically are going to say, oh, it's just stocastic, it's random. It's not random.

If you can define all the variables, more and more of the corrosion problems go from the random aspect into the deterministic bin once the work has been done, once people have done the work.

And just to back that up, you know, not only do people all use batteries in their cars, but an electrochemical timer is one of the most accurate timing devices known to man, so it's a very well-behaved process, and electrochemical timers are one of the earliest forms -- one of the earliest ways to keep accurate time.

And so, there are some issues -- there are some data sets that are needed, both deterministic and probabilistic, and some of the probabilistic probability distribution functions and key probability distributions that are needed, some of the deterministic growth laws, could be defined after a one-to-four-year period of study.

There are a few unresolved scientific corrosion issues that I think require a greater period of study, and I'll just tell you what they are as this goes along.

To make some comments regarding TSPA-95, again, I'll say once again, an accurate definition of the near-field local environment is the greatest shortcoming.

As I said before, dry oxidation rates, humid air corrosion rates, humid air aqueous corrosion rates under drips, hydroscopic salts, and thin aqueous films for steel seem reasonable, the literature has been mined.

I don't think that a 4X pitting factor should be applied to uniform penetration, and we discussed that in the workshop, and we gave reasons -- we gave technical justification for why leathering would occur.

Mainly, that is that aqueous corrosion of steel is controlled by oxygen reduction, and so, the peaks in any sort of shallow pits would tend to then be leveled, and so, you'd end up with, eventually, uniform corrosion, and that was our technical justification.

I think that a more conservative estimate of local waste package electrolyte composition is a near saturated salt solution from evaporated drips, and that differs from the utilization of the J-13 well water times 100 or times 1,000 concentration.

Now, admittedly, the time evolution of this is complex.

At first you have drips that evaporate when the package is hot, and then you have more water, you get 99-percent relative humidity, and now you have this concentrate salt solution that I speak of, and then, eventually, with time, that might get diluted again, because now you've got a situation that's 99-percent relative humidity, near room temperature, and eventually perhaps that concentrated salt solution gets diluted, but my comment is that I felt that more concentrated solutions should be considered especially for localized corrosion processes.

Okay.

Now, you've all heard that galvanic suppression of the corrosion-resistant material depends critically on the thickness of the electrolyte or drip frequency, corrosion allowance material, corrosion morphology, etcetera, but there was consensus -- and I also will speak mainly for myself, and I have one caveat, I have one asterisk next to this, but generally, under a dripping condition, if you had persistent dripping, the period of galvanic suppression would be short, would be on the order of tens or hundreds of years, and I think that's well-justified based on the galvanic corrosion data that exists in the literature and the rates that you would expect.

However, I have an asterisk to put next to that, and I'll talk about that in a little while, when I talk about the work that I think is valuable.

In the TSPA-95 assumptions of localized corrosion penetration rates, pit area densities, and diameters for the corrosion-resistant material, we really would benefit from more experimental data that would allow these to be quantified better, and I think everybody has said that.

Okay.

Some comments on the ongoing research.

The ongoing research efforts would benefit from modification of objectives and outputs that real data that can better contribute to the probabilistic damage evolution type model that usually is developed.

And in terms of localized corrosion data, the localized corrosion data I saw, the snapshot that I saw, does provide, at best, a ranking. Okay. The ranking of 625 isn't as good as C-276, and C-276 isn't as good as C-22.

Well, unless you give me a drastically different environment like work I did at Sandia and, you know, lithium batteries and a chloride environment, unless you give me some totally bizarre environment, you know, a lot of people, probably 200 corrosion people probably could have guessed that that's the ranking, or more, and so, it isn't the ranking of which is the best and which is the worst that's important.

What's important is that the data cannot be readily extracted to a probabilistic model like Joon Lee is using, and that is the critical point that I saw there.

Now, regarding galvanic corrosion, the galvanic corrosion studies need to interact -- they need to address galvanic interactions in a thin aqueous film under dripping conditions, and so, full immersion data, if you couple 625 to steel under full immersion, that is, again, going to lend itself to provide the type of data that would be beneficial to the type of model that is being developed that seeks to take credit for galvanic suppression, and I do have one more comment about that later.

Again, MIC studies emphasize the role of nickel and uniform corrosion rate, and as I went through this, it seemed like most of the MIC studies, microbially-induced corrosion studies, focused on MIC as if it were a different mechanism, and there are isolated cases where that is true, like with Monel 400 you would get dealloying, whereas you wouldn't normally have dealloying when you have MIC, as Brenda Little point out, but mainly -- my main concern with MIC is the effect on MIC on other operative corrosion processes.

In other words, one of the things that MIC does -- and we see this in seawater and Navy work -- was that films will produce a potential ennoblement that causes alloy-625 to crevice corrode whereas it would not crevice corrode normally in room temperature seawater, and so, the effect of MIC shouldn't be treated like it's a new form of corrosion, but its effect on other corrosion processes, particularly localized corrosion processes, should be considered, and so, the all critical question there is how many drips due to nutrient conditions exist for MIC corrosion, do you get the ennoblement, and therefore, do you get the initiation process to occur, and that affects the probability of initiation, which again could be input into Joon Lee's model.

Okay.

So, when I talked to Carl on the phone, he said don't focus on the corrosion phenomena that don't have great impact.

I haven't run the simulation, so it's a little hard for me to say which corrosion phenomena -- I haven't run through a complete, what if we, you know, assign this corrosion rate, what if we assign that corrosion rate, what the result will be on TSPA, but I tried to consider what I thought would be important, and what would be the most important -- so, I've divided this in three categories, what is the most important, most significant impact, and what has some impact, and we'll skip what has no impact, of course, because I won't waste your time, but I'll do it that way -- most significant, significant, and some impact.

It seems to me the thing that has the most impact would be probabilistic and deterministic aspects of the corrosion resistance, localized corrosion resistance of the corrosion-resistant material, and those must be better defined for the materials that are of interest, and you can see here what I've recommended, and if we go down here to the bottom, really what needs to be developed is this curve.

Now, the way to read this curve is -- this is accumulative probability distribution, and this is applied potential. So, the applied potential is like applied stress, it's something that you subject your system to, depending on what the conditions are.

And then these solid lines with the curves are the response to the material. In other words, that's the intrinsic way the material behaves.

And the way to look at this plot is that -- this is very hypothetical, but steel is certainly much less resistant to, say, crevice corrosion initiation that has 625, and C-276 is better than 625, and C-22 is better than C-276.

So, the way to look at this curve is that one needs to develop these probabilities -- this happens to be for crevice or pit initiation -- one needs to develop these probabilities, and the dotted lines indicate that, if you take 625, a higher temperature, the curve is going to be shifted a little bit to the left, if you take it a lower temperature it's a little more to the right, and once you develop these curves, then you say, okay, where do I have MIC, where do I have oxygen, where do I have ferric ions, and as you look at this curve, what you can see is that, if you choose 625 and you have MIC, you have a high probability of crevice corrosion.

However, if you choose C-22 and you have MIC, you have a low probability of crevice curve initiation. All right? So, that's the way you read the plot, and that's the kind of data that I think would be valuable for this process.

Okay?

One of the things that are valuable on this list are to establish a rational technical basis for conditions and conditions for whether or not repassivation occurs.

In other words, one of the comments that is a reasonable comment is do you see 2-centimeter pits, do you see pits that grow 2 or 3 or 4 centimeters, or will pits just stop? In other words, you've got this thing sitting there, but the pits will just stop.

And I think that a rational technical basis needs to be developed for that, and that's possible. There are many mechanisms, as I discussed during the workshop, for why you might have a rational technical basis for these processes stopping, but the need to be developed.

Otherwise, there will be other blokes like me that will say, well, you haven't proven to me that a pit stifles or a crevice site stifles.

The things that have some impact, that will have significant impact, of course, would be defining the corrosion rates of steel in the alkaline environment, where the steel will pit, and here we also include a passive dissolution rate, because it's important that passive dissolution rate of the steel is finite.

I mentioned earlier that are some scientific issues that have impact, that I think are going to take more time to study, and the scientific issues that have impact would be do localized corrosion sites interact positively or negatively?

In other words, if you have two adjacent sites, do they know each other exist? Do they work against each other, or do they work in favor of each other?

What is the coalescence of localized corrosion sites? If you want to develop a damage function, you need to consider localized corrosion sites coalescing. So, that's a scientific issue.

There isn't a whole lot of work. There is some work where that's been considered.

Then a third issue, what I think is an important scientific issue, is how many monolayers of water do you need to support high-rate corrosion process?

I'll, just to summarize, say, if we have high-rate corrosion process, like pitting corrosion, like crevice corrosion, how many monolayers of water do you need in order to support these high-rate corrosion processes? And that is not well-known.

I went to the electronic corrosion field, because the electronic corrosion people are concerned with how many monolayers of water do you form at 95-percent relatively humidity, how many monolayers of water do you form at 80-percent relative humidity, and what they generally say is that you form an electrified double layer and that you reach the conductivity of bulk solutions when you have something like 20 monolayers of water.

However, I would argue that, to support high-rate corrosion processes, you need more than 20 monolayers of water, because if you look at some of these corrosion processes that we are considering here, like crevice corrosion, you use up all that water really fast, I mean you use it up, and it will be gone.

So, that's a scientific issue that I don't think getting humid air corrosion data from the literally is going to necessarily help that, and those are the ones that have significant impact.

I've run over time-wise.

There is a list of things that have some impact, but it's not as important, so I won't spend much time on it.

Spalling -- there was some disagreement with the committee -- with the experts based on spalling.

However, it should be recognized that spalling is not that important, and the reason for that is that dry oxidation really doesn't define the penetration rate of the corrosion allowance material, and the second reason for that is that, if you have non-spall oxide, they do not render the steel protected against subsequent corrosion processes.

So, even if you don't have spalling, it's not like that the lifesaver for the corrosion allowance material.

And there are some other issues there that I won't go into.

So, let me stop there and take your questions.

I did feel that -- and this is a self-criticism -- that the timeframe was very compressed, that we would have gotten better input from the experts, including myself, if the timeframe for the whole process hadn't been so compressed, and I'd lastly like to say that there are -- I'm involved in the aging aircraft scenario, and if you look at the aging aircraft scenario, there are a lot of government agencies, a lot of industries, and a lot of universities working this problem, and there are whole scientific symposia that go on where people talk about aging aircraft, also pressure vessel steels and petrochemical industry, there are whole technical symposia, and boy, if you want to have an external audit on the work being done and if you want to work some of these issues, the unresolved corrosion issues -- and I realize it's a funding issue, but have a large enough effort where people are really battling these things out in the scientific literature, and that will provide you with the largest external audit on the whole process.

I'll be glad to take any of your questions.

Paul, if you have anymore questions about corrosion processes --

DR. CRAIG: Let's do it on the side.

DR. SCULLY: Sure.

DR. SAGUES: Thank you, John.

Do we have any questions from board members?

DR. BULLEN: Bullen, Board.

John, just one quick question.

DR. SCULLY: Yes.

DR. BULLEN: Realizing you were constrained with a certain design, you had to do this expert elicitation.

If you weren't constrained, what design would you pick for the environment as you now know it, what package design? What materials would you choose, what package design? Would you change it?

DR. SCULLY: One of the questions that comes up is the corrosion allowance material.

One of the only ways you can think of to corrode something like 625 or C-276 or C-22 is to put it in boiling ferichloride, and I brought a sample, because we want to do a crevice corrosion test in one of our labs at UVA that the students can see crevice corrosion in like a day, and that's one of the ways we can supposedly do it, and so, if you had uniform penetration of the corrosion allowance and you had -- since it's an oxidizing environment, you oxidize first to ferric, you'd have a ferric environment, maybe have salts.

That would be one of the ways to undo such a corrosion-resistant material, and the other thing is that -- the other comment that's related to that is that I spoke of in my workshops getting into windows of environmental conditions, temperature, where the material just plain is almost immune.

People don't like that, because you can't do a probabilistic damage function evolution, but on the other hand, it's more attractive to me, because if you're going to do it for materials that are not immune, you have to have good data, as Kevin has -- you have to have reasonable data, otherwise the extrapolations do start to become questionable.

DR. SAGUES: Any other questions?

DR. PARIZEK: Parizek, Board.

What's the difference between, say, a film of water of, say, a high relative humidity inside of the drift versus a drip, versus maybe more than a drip, if water is streaming in?

DR. SCULLY: That was one of my scientific questions, and the way it goes is that first you're going to have sticking of molecules of water, and then you are going to have islands of water. Then you're going to have layers of water, monolayers of water, different monolayers.

In 95-percent relative humidity, you get up to 20 monolayers, and people that have done studies of the conductivity in that environment say that you perform the conductivity of a bulk environment -- in other words, if I have 5,000 monolayers of water -- by the time it reaches 20 monolayers, and so, people that do electronic corrosion are concerned with that.

However, I would put another criteria on that, and that is that there's separation of anodes and cathodes in materials, usually on a metallurgical basis.

In other words, for steel, it could be the FE3C in the alpha iron phase, and so, let's say they're separated micro-structurally by a tenth of a micro-meter.

Then I would say that this electrolyte layer, in order for that anode and cathode to communicate to start a corrosion process, like I said it has very solid scientific underpinning that the well water has to be -- in order to support a corrosion process between those two metallurgical phases, that the aqueous layer has to be on the order of a tenth of a micron, which is many, many monolayers.

So, that's the way you do the process, and that is as to exactly what rate of corrosion process can be sustained, and under 99-percent relative humidity with no drips, or whether you even have drips -- in other words, say, what I call definitely bulk water -- is an interesting scientific question.

DR. PARIZEK: Then the bulk water could flush the salts off, could be helpful in slowing it down?

DR. SCULLY: In the evolution of the process, I would argue that you would have evaporating drips and concentrate salts, and then eventually, over geological time, you would have dripping and dilute salts. So, you'd have some sort of spike in the concentration.

DR. PARIZEK: What about the inverts? You've got these little feet and you've got these great big weights sitting on these little feet and water eventually accumulating in that contact point. Is that bad?

DR. SCULLY: Yes.

DR. KNOPMAN: Knopman, Board.

It seems to me that this is a sequential kind of -- there are a whole series of steps of things that could happen. When they happen and when they happen in relation to one another depends on what your corrosion pathway might be.

Is it fair to say that the first 50 or 60 years of emplacement is the critical time in the sense that you have -- you don't have total dry-out yet at that point, you still possibly have fairly high relative humidity -- well, even anything over, let's say, 50 percent -- and that you could have the conditions for microbial-induced corrosion at that time and that you sort of -- the material, in effect, gets -- its susceptibility to future corrosion thousands of years out is, in effect, defined by those first few decades of activity before you get the high heat and low humidity?

CHAIRMAN COHON: Debra, you meant first few decades after closure?

DR. KNOPMAN: After closure.

CHAIRMAN COHON: Assuming no ventilation.

DR. KNOPMAN: Assuming no ventilation.

DR. SCULLY: I'm not sure I follow the scenario as well as I'd like to.

DR. KNOPMAN: Okay.

DR. SCULLY: And I should also say that I'm not the MIC expert, but if I think what you're saying is -- first of all, what the MIC experts tell me is that, when it's very hot, even if there's -- you're going to have dry oxidation but with some humidity, you're going to have oxidation with some humidity, and actually, as I wrote in my report, except for the people that have done studies on ultra-high vacuum that have leaked in a little bit of partial pressure of oxygen and looked at the surface science technique at the initial monolayers of oxide growth, most oxidation studies inadvertently, because they were just done under non-well-controlled conditions, were done with a mixture of oxygen and water vapor, and so, those early conditions have been simulated by the wealth of data that exists for steel oxidation, and then I'm told, as I started to say, that MIC is probably not going to be operative at high temperature but between 200 and 100 degrees centigrade, and the last comment I'd like to make about that is that I don't think you plant sort of a seed that undoes the material.

As a matter of fact, if you view it as a layer-by-layer penetration, which is not terribly long for steel, the layers that would be affected by MIC, even if they were, would then subsequently be renewed.

If it's 8 centimeters of steel, once you're down 1 centimeter -- so, it's not like you plant some seed for some problem that's the undoing of the whole thing. It doesn't work that way.

DR. KNOPMAN: Okay. That was the question. Before you get your really high-temperature conditions in the repository itself, are there things that could be happening there that would then substantially affect --

DR. SCULLY: Again, I would say the MIC process there, let's say before the temperature ramps up, the MIC problem there -- there are a number of studies on corrosion of steel where people that don't wish or seek to include the effects of MIC have had MIC inadvertently, and so -- in other words, there are no surprises. There aren't huge surprises. You understand what I'm saying?

In other words, over the last 40 or 50 years, where corrosion tests have been conducted, if you take -- mine that literature, there are some of those studies where they had MIC whether they liked it or not, see what I'm saying, that have done humid air corrosion studies on steel.

For instance, some of the work in the Panama Canal zone is literature that was mined to provide rate information.

DR. KNOPMAN: So, it's present. So, this is when this is going to happen. That's the point.

DR. SCULLY: No, because MIC of the carbon steel I don't view as a showstopper issue. MIC of the carbon steel is a 4X enhancement or something of the corrosion rate, and so, it's a fairly well-behaved process.

DR. SAGUES: Dr. Nelson has what no doubt is a very brief question.

DR. NELSON: What do you think is going on with ceramics? I realize that it was said by Kevin that perhaps the expertise that you might want to have there to comment on that wasn't on the panel, but I'm sure it was discussed. What input do you have on the ceramics issue?

DR. SCULLY: Again, trying to be not long-winded and relevant, my view -- I did do a little bit of work on ceramics as a corrosion barrier coating.

I actually am interested in this for Navy propellers, for a reason I won't go in, in the interest of not being long-winded, but we did put ceramic coatings on propellers and actually found paints to stick, and my opinion on ceramics is that you're going to have cracks in the ceramics, and any coating has a failure in it.

The time of the first penetration of a waste package is probably not going to be diminished significantly, but the corrosion process won't be radically -- so, what's going to happen is you're going to get a credit for the area where the ceramic is not cracked, and then, once you get corrosion products, the wedging action of the corrosion products will spall the ceramic, and it's the way concrete spalls when steel corrodes in concrete, it spalls the concrete, and so, you're going to start to spall the ceramic.

So, generally, you get some period -- I do not know what that period would be -- where you could take credit for reduced error fraction of a package degrading because of the ceramic, and that's my view of it.

You won't radically slow down the rate to first penetration, and eventually, it will catch up, because the low fracture toughness will be spalled.

If you look at some things in material science -- I am a material scientist -- in some aspects of material science, if you ran 1,000 tests or 100 tests, the processes are very well-defined, you don't have the stocastics or the randomness, and corrosion is a little bit more random, though, a little bit broader distribution, but it is by far not the broadest, the most scattered in the material science field.

Probably composites or ceramics have the greatest scatter. Like if you look at fracture toughness of ceramics, that has the greatest scatter.

So, corrosion, as a sub-field of material science, is not even the one with the greatest scatter and uncertainty. There are other fields.

DR. SAGUES: I don't want to be responsible for several angry people saying things about half-an-hour or one hour from now, so we are going to stop the proceedings at this moment. Thank you very much, John, for a very detailed presentation.

The next speaker, in spite of my attempt to rename him a little while ago, is still David Shoesmith from the Atomic Energy of Canada, Limited, and Dave is, of course, another expert in the panel. We look very much forward to his presentation.

MR. SHOESMITH: This is a pleasure for me, because this is probably the first time in my life that I've ever given a talk in front of so many people where everybody knew -- where most people knew where Pirawa was, but for those of you that don't, it's north of North Dakota, and yes, there is life up there, though it's hard to find between Halloween and Easter.

Pirawa is an Indian word for calm waters, which describes not the political climate but the general climate around there. Whiteshell is an Anglo-Saxon word which is misspelled, which also defines the environment around there.

First we'll deal with the question of -- I'm going to follow a similar format to John, except I'm going to try and illustrate with crude chemical drawings some of the chemical -- or corrosion processes that I think are important.

We will start with what I think is critical to any assessment, and that is what will the evolution of the repository environment be?

That looks complex, but there are two sets of lines here. There are those which are not highlighted in red, which I like to think are gimmes.

Those are things that you know fairly well -- the temperature, the relative humidity, and the fact that oxygen will be generally available at about a uniform amount -- and those which will be affected by the drip scenario.

Without the drips, the salinity will not go through this increased peak as it develops and deposits, you not get too many deposits on the container, and you will not get the high pH scenario.

So, the evolution of the environment depends very much on the drips and their number and where they are located.

The drip scenario is, without a doubt, the biggest unknown and the biggest danger, and I'll try and illustrate that by just showing you how I would envisage the corrosion processes to go.

In the no-drip scenario, we would go through the dry oxidation period, through humid air, aqueous corrosion, you'd fail the outer barrier, you may get some galvanic protection, then you would move on to passive corrosion, eventually you would fail, but you should get long lifetimes.

If you introduce the drips, you introduce a number of significant extra possibilities which I have indicated here with a red asterisk, and those are that you could get microbial corrosion, not that it will be absent here, but here it will be much more prominent, you could pit in the outer barrier.

Once you get through to the inner barrier, you have to deal with the dangerous possible scenarios of crevice corrosion and cracking, which you could eliminate, I think, if you don't have drips.

So, whether or not you do have drips, where they are, how probable they are, is a big problem.

Quickly, I think that the corrosion allowance material in the absence of drips, the present analysis used -- the previous one used in TSPA-95 is very good. The criteria established for temperature and threshold I wouldn't argue with.

I would argue that you will not get pitting, but if you start to pit, because this is not a passive material, the pits will spread, as well as penetrate, and eventually it will just look rough, it won't look pitted, and therefore, the use of a pitting factor is an over-conservatism which I don't think you need.

Just to emphasize that I think the quality or variability of data for the corrosion allowance material is good, I show you two plots with many data points which already are in TSPA-95. This is good. We can start to believe something where there is a significant amount of data.

For the corrosion under the non-drip scenario, I think there are two things which should be looked at -- the possibility of spalling. I don't think it's dangerous. This curve turns over because the corrosion product starts to protect the underlying material.

If there is a means of removing it by stress flaking or whatever it might be, then you could up this rate.

The second one is that you should include microbial corrosion, but the inclusion of a factor of four throughout the lifetime of the COM without the supply of bulk water by drips, to me, is a grossly conservative scenario.

So, I would suggest that you might include it but include it with something which decreases in time, as a parameter whose influence decreases with time.

The problem with drips -- and I hope you'll pardon the artwork. The problem with drips is it supplies everything that you don't want to some of the things that might help you.

So, a drip will supply the nutrients for the microbial corrosion, it will supply calcium which will form the deposits, it will drain the alkalinity, which will give you the passivity which makes the corrosion allowance material susceptible to pitting, but it may also give you things like silicates, which make this deposit inert and protected.

It's essential to know what the distribution of these drips are and what they are, and if that cannot be known, then alternative solutions, engineering solutions become more important, things like drip shields, backfills, or even changing the material to something else.

If you do have drips and establish alkaline conditions, then what I learned at the workshop from John Scully was that penetration laws for the depth is equal to some constant time time waste of power and are quite universal, and I think that they should be applied, as opposed to a pitting factor and also that you could include some other factors in that constant K.

This has got to be a limited period, because eventually you will wash this concrete out of alkalinity, you won't be able to supply it anymore, so the pitting process will go for a certain amount of time, but the microbial nutrients will be supplied forever.

So, you have to go with a pitting factor -- I beg your pardon -- the microbial factor if you have a pitting scenario.

What I think the implications of that will be -- this is my crude summary in a qualitative sense of what TSPA-95 is.

If you use the data that I've showed before, you will have a certain depth of penetration due to general corrosion.

It was then assumed that microbial corrosion would add to that a factor of about four or five, so this pushes the penetration rate up by that factor, and on top of that, a pitting factor of about four was also used.

This means that you end up multiplying this by anything up to a factor of 20, guaranteeing that lifetimes will be fairly short for that material.

My assessment, if it proves to be valid -- and data are still required to demonstrate it -- would suggest that the microbial factor is a function of time and, therefore, this effect will not accelerate forever and that you can add in a penetration depth for that period of pitting which you get due to the alkaline period and that the consequences will be much longer lifetimes.

So, I'm out on a -- I have been to Las Vegas 10 times this year -- I'm out on a limb with that until and Joon Lee proves me correct or incorrect later.

The failure of that outer barrier will expose the inner barrier. In the absence of drips, you'll see that oxygen there to drive corrosion, but it's going to give a passive layer, and I don't think that the underlying material will then curve very quickly.

Those passive corrosion rates, in the absence of bulk water, will be extremely low and very long lifetimes. Tens to hundreds of thousands of years or greater should be achievable for something which is 2 centimeters thick. We think we can get 10 to the 4th, 10 to 5 years out of .6th of a centimeter of titanium under passive conditions.

If there is water, then unfortunately there is the potential for the aggressive environment that John suggest.

You supply oxygen, which will continue to drive the corrosion of the outer material. The fericide will be oxidized to feric, which would hydrologize to give you the combination of feric iron and acidity.

This, coupled with the water supply of further alkalinity, will give you that nasty solution right on the inner barrier that you don't want.

Whether it will achieve the saturation that we all fear is another matter, but nevertheless, the potential for an oxidizing, acidic, saline environment would be present in the presence of bulk water.

The establishment of those conditions could render the inner barrier susceptible to localized processes, which I'll deal with in a second. It could also give you the galvanic protection. That is, that the corrosion of this iron material will protect this underlying material.

This is the anode, or the aggressive John Scully, and this is the passive or somewhat more benign cathode, cathartic Dave Shoesmith, down here, and you will get some galvanic protection providing that opening is fairly narrow, but as it opens out, you effectively will split it, and the galvanic protection will be divided between the two contact points, and the middle will no longer be galvanically protected, and my estimate is about the same as John's. I don't think you've more than tens to hundreds of years for that kind of protection.

That would just be great in normal industrial processes, but when you want 10,000 years, a few hundred years is not going to do you any good.

I think that crevice corrosion is the most likely failure of the inner barrier.

There is not a good database, but there are people that have done very aggressive experiments to try and establish whether a particular material is susceptible to a problem or not, and from Peter Andreson's assessment, I took this table -- I think it's also from some Lawrence Livermore report -- where we have set critical temperatures for crevice corrosion.

So, if the temperature is above this, it's susceptible in this particular aggressive saline, ferichloride, low pH environment. If the temperature is below that, then the testing said it's not susceptible.

So, you can use this as a criterion for when it should -- whether it will or will not crevice corrode.

And then you take a look at the temperature-time profile -- and I apologize for this. This was the only one I could find when I did this. It may not be the appropriate one, but the point is that if you don't fail the outer barrier until the temperature is below the threshold value, then you have a criterion for saying that the underlying barrier will not undergo the localized corrosion process, and on that basis, of course, which is intuitive to corrosion scientists, you can rate the potential materials.

So, perhaps with C-22, the outer barrier would have to last a couple of thousand years for this criteria, bearing in mind that this is an aggressive environment that you are taking the test from. C-276 would be about 3,000 years; 625, 10,000 years.

That effectively defines a period of protection that you require on the outer barrier if you want to accept this criterion for eliminating the possibility that the localized corrosion process will occur on the inner barrier.

You can then take that and put it into an assessment. I have an overhead describing that, but I will leave it out because of the time limitations, and just illustrate it on the next one.

You can then look at the distribution of sites that you have coming through the CAM and you can draw a number of conclusions.

For instance, this particular site fails while the criterion -- the temperature is above the threshold. That site could then be taken to initiate a crevice.

This one does not fail before the temperature gets below the threshold, this site will not initiate a crevice, so you could take a distribution of penetration depth from the CAM and use it as a criterion for specifying whether or not you will initiate sites in the underlying material.

And again, a crevice that starts here will have a certain penetration law. There's very little data for this, but what data exists suggests that they are very similar to the ones that John described for pitting.

We have one for titanium. It's very conservative. You could use that data if wish, not because the material is anything similar to the one you're going to use, but that is a material which, in the pure commercial form, crevice corrodes more rapidly than any of these nickel-based alloys. So, it would be an extremely conservative damage function.

The potential crevice corrosion scenarios -- one is that you will crevice corrode to a certain depth and that, beyond that time, residual stresses or some kind of stress in the material will force a crack to go through this, and I would suggest you have about two-thirds of the allowance for the localized corrosion process and one-third for the crack.

The conservative case is that the crevice corrosion will propagate to failure but it won't crack, so you have the whole CRM thickness as a barrier.

What I think is a realistic case is to choose a material like 625 or higher, perhaps not 625 but C-276 and C-22, is that you will change the composition of the material. So, once it starts to crevice corrode, it will adapt its local chemistry to repassivate.

Now, there are mechanisms by which it has been proposed that that happens in high methilinum-containing nickel alloys.

We feel that, in titanium materials, we know what those processes are, but this is a point in question dealing with the question that Paul asked -- Paul Craig asked before. If you wish to say that crevice corrosion will be limited, you must understand the mechanism which limits it.

So, I would suggest that a fair amount of work should go into looking at those corrosion-resistant materials and trying to define what the mechanism of stifling or repassivation would be.

Alternative corrosion allowance materials -- generally, I don't think it's a good idea.

One, it's expensive. That may not be the major one.

The second one is I would hate to give up on that good database for carbon steel. That's a rarity in this corrosion -- attempt to do this corrosion performance assessment, is that there is a good database already.

I would not change the material unless you cannot demonstrate that the outer barrier will give you this required period of protection to protect the under barrier from the localized corrosion, and if you're going to go Monel, why not go all the way and go to something like titanium, which has many advantages?

It's unaffected by drips, it's immune to microbial corrosion, and you don't require a thick wall.

I understand that there are alternative problems, like it's a thin-wall material, so you don't have the same kind of radiation shielding. It might be more impact-sensitive.

There are issues, but from the corrosion point of view, it would eliminate the problem of worrying about carbon steel.

Microbial influence corrosion -- in the absence of drips, I think it's limited by the supply of nutrients, and I would be amazed if anybody would argue about that. Certainly, Brenda Little wouldn't.

My suggestion is you would put in a time function for the microbial factor which makes it important in the beginning but not so important eventually.

In the presence of drips, you can't do that, because you perpetually supply nutrients and potentially feedstock for this process, and it could continue to go. You have no -- probably no other voiding except in the factor for microbial corrosion.

The underlying or inner barrier materials -- this process is generally not seen on those barriers.

Now, that doesn't mean to say that it will never occur or it's impossible.

It means that it's not seen, its probability of occurring is very low, and if it did, all it would do is initiate the process of something like crevice corrosion, which would then propagate at a rate independent of the fact that it was started microbially as opposed to being started some other way.

Finally, some very general conclusions.

To me, the primary one is a much better understanding of the movement of water is required so that you can decide whether you can accept the drip scenario for this container design or whether you have to go to engineering solutions like drip shields and whatever, change of material.

I think the database of carbon steel is good. I feel that a protection for the behavior of the corrosion allowance material is quite justifiable.

The absence of a good database of candidate corrosion-resistant materials means that you probably would have to accept that conservative scenario that I presented, and I would suggest that lots of experimental work is required here, and I think -- I agree with John, I think it's doable in the timeframe of licensing, if not viability, and I think you should take care not to overemphasize the effects of microbial corrosion.

Thank you for your attention.

DR. SAGUES: Thank you very much, Dave.

Do we have any board questions or comments?

DR. BULLEN: Bullen, Board.

At the risk of not treating the experts equally, if you could change the design, David, what would you do? Obviously, the answer is in your view-graphs, but I'd like you to say it.

MR. SHOESMITH: First of all, I'm not going to suggest, because I have an alternative possible design, that the one that's presently proposed won't work. I think it will work.

But I would definitely go with the titanium outer barrier, and I would give up on the possibility of shielding.

Then I think you could probably go with 625 on the inside, because the lag time of the outer barrier would be sufficiently long that you could choose a cheaper material on the inside, you'd reduce the weight, and all those kind of factors, you put up with higher radiation fields on the outside of the container.

CHAIRMAN COHON: Cohon, Board.

When you say you think the current design would work, do you mean that a 3,000-year package is credible?

MR. SHOESMITH: Yes. I find it difficult to think of a credible scenario, other than very persistent dripping, that will fail this container in -- this combination of two walls in 3,000 years.

DR. SAGUES: Any other board members' questions?

We do have a board staff question.

MR. McFARLAND: Yes, David. Russ McFarland, board staff.

You have been exposed to the broad concept of the repository in the board of consultants. Do you have any comments with regard to the use of a granular backfill material to enhance the life of the waste package?

MR. SHOESMITH: I have some personal opinions on it, yes. I think it has some significant advantages, mainly from the engineering point of view.

The possibility of impact from degraded concrete is one possible advantage.

The other is, if you designed it correctly -- and I can't claim credit for this idea, this is an idea which comes out of the program -- if you have two layers of different aggregate size, you can deflect a lot of water and it will soak up a lot of water. So, I think you avoid the bulk water scenario.

The kind of thing that I would like to see calculated, though, would be if you put a thick backfill over the top of this container, what do you do to the temperature inside the container?

I noticed from Tom's overhead that temperatures in the core of the container are somewhere up around 320, 330, and you have a cladding upper limit of 350. So, you're getting close to the limit.

Putting something with a low ability to allow thermal transfer on top of the container might affect that.

So, it's not a gimme. I think you would have to start calculating what the thermal impacts were, but maybe you already have done so. But I would worry about that a little bit.

MR. DOERING: Tom Doering with the M&O.

We have done thermal calculations with the backfill, and the way we had performed them was that we went to different effective thermal conductivities to make it work, and so, what we did was do a variation of .3 watts per meter Calvin, all the way to .8, which is a very good thermal conductivity. The rock is around 2.

So, it's a matter that we'd have to find the right material to work it with, otherwise the cladding temperatures at 50 years could come close to 400 degrees C internally.

DR. SAGUES: Thank you.

In the interest of time, we will go ahead with the next presentation by Joon Lee from the M&O Intera on waste package degradation modeling in the total system performance assessment for the viability assessment.

LEE: Thank you.

In this presentation I will be discussing PA modeling exercise for waste package degradation for the PA.

PA modeling exercise is different than other kind of detailed modeling exercise and testing. We try to get the information from the detailed modeling exercise and testing to capture the key essence of the process.

Also, the PA modeling exercise is to try to capture potentially why the range of temporal and spatial near-field exposure conditions that occur in the process.

So PA modeling exercise tend to be probabilistic.

Also, I was asked to prepare a brief history of that waste package degradation modeling in previous TSPAs, telling me I have 20 minutes, so obviously I cannot cover that scope within 20 minutes, so I put a lot of materials in the handout so you can find those things, and then if you have any questions I would be happy to discuss them with you.

For outline of my presentation, I will be discussing very particularly about waste package degradation modeling in TSPA-95. Then I will discuss the TSPA-VA base case. This is base case waste package degradation model. Then I will discuss key parameters for waste package degradation model derived from expert elicitation.

I want to make two points here.

One thing is the result I will be showing for those key parameters will be part of the results we get from expert elicitation. Experts provide a lot of information about that, but I will be showing only part of that.

Second, as Kevin Coppersmith mentioned that those results are still in draft version, and then we will be --

although those results should be very close to final. But there is maybe some chance to, you know, change slightly later on. So, don't be surprised if later you see some slightly different results.

This view shows the logic diagram for waste package degradation model in TSPA 1995 which is based on a probabilistic approach.

Our thermal hydration model provides the waste package surface temperature and RH, and in TSPA-95 we used a waste package temperature of 100 degrees C as a threshold to initiate the outer barrier corrosion, and then covers to outer barrier undergo either humid air corrosion or aqueous corrosion depending on the RH at the surface of the waste package, and the humid air corrosion and aqueous corrosion of carbon steel was modeled using data from the literature.

Localized corrosion of carbon steel was modeled using a pitting factor approach, assuming the pitting factor has a normal distribution with a mean of 4 and a standard deviation of 1, and once we accomplish the rate, we assume that the inner barrier undergoes aqueous pitting corrosion, and in TSPA-1995 we also incorporate the galvanic protection of the inner barrier and modeled using a model provided by the project experts.

The results of the TSPA-95 modeling exercise was the time history of the waste package population.

I don't have time for all the details of this modeling exercise in TSPA-1995. Again, you can find more details for individual models and the modeling approach in your handout.

This view-graph shows that logic diagram for the base case. This is base case of the waste package degradation model.

This model based on current waste package design and then current understanding of the near-field exposure conditions and then current understanding of waste package degradation process.

Those boxes with the colors indicate the key model components or key model parameters, and then I will discuss later about the information about those key model parameters which were developed from expert elicitation.

In the VA model, the near-field thermal hydration model will provide package temperature and RH and then in-drift dripping information, and near-field chemistry environment model abstraction will provide pH and chloride concentration of the drip model, and then oxygen pressure evolution in the drift, and waste package temperature will be tested compared with a threshold temperature to initiate the outer barrier corrosion.

Then this base case model depends on whether waste package will be dripped on or not, and for the waste package for the drips, if the pH of the dripping water is greater than 10, we assume that the carbon steel undergoes a high aspect ratio pitting corrosion, and if the pH is less than 10, we assume that the carbon steel undergoes general corrosion.

Once outer barrier penetrate, we assume that a crevice forms on the inner barrier between the remaining carbon steel and inner barrier or the crevice forms between corrosion product and inner barrier, so we will test that condition inside the crevice to the threshold barriers, and if that crevice conditions is greater than our threshold value, then we assume that the inner barrier undergoes localized corrosion and general corrosion.

If the crevice condition is less than the threshold, we assume the inner barrier undergoes general corrosion only, or alternatively, we can fit this box, this box conservatively, just assuming that once outer barrier penetrate, we assume the inner barrier undergoes localized corrosion and general corrosion only.

We can skip this box. This box has not been finalized yet.

And for waste packages, we use our rates at the waste package surface to initiate the outer barrier corrosion either in aqueous environment or humid air environment, depending on the RH of the waste package surface, and in either case we assume that the general corrosion is only in process, and once the outer barrier penetrate and we assume the inner barrier undergoes general corrosion only.

The outcome of this base case model will be a time history of the structural failure of waste package, and then for waste packages with localized corrosion with drips will be a time history of pit perforations and then structural failure.

I'll leave this up here and then I'll go through each box with the information we developed from expert elicitation.

Now I move to the key parameters for the VA base case waste package degradation model, and the first thing is the threshold for the canister corrosion initiations.

As I mentioned, we have three corrosion initiation threshold, a temperature threshold, an RH threshold for humid-air corrosion initiation, and RH threshold for aqueous corrosion initiation.

Those thresholds are dependent on the surface conditions such as presence of dust, oxides, and salt and whether dripping or not, location on a waste package such as top, sides, and bottom.

This view-graph shows the distribution for temperature threshold with the outer barrier to initiate either humid air corrosion or aqueous corrosion, and this result for this box here.

Five experts responded to this issue, and X axis is the temperature threshold in degrees Celsius, Y axis is cumulative probability, and the red line here is combined distribution of that individual expert, and the 50th percentile of this combined distribution is about 102 degrees Celsius.

In TSPA-1995, we used a single value of 100 degrees C for initiating for the temperature threshold.

The next one is the distribution for RH threshold for carbon steel outer barrier humid-air corrosion initiation.

Again, the five experts responded to this issue, and then the X axis is the RH threshold in percent for humid air corrosion initiation, and the 50th percentile of the combined distribution is about 72 percent

In TSPA-1995, we used RH range for this threshold between 65 and 75 percent, assuming a normal uniform distribution.

The next one is distribution for RH threshold for carbon steel aqueous corrosion initiation. The result is actually this box here. Oh, no, no, this box, sorry.

Again, five experts responded to this issue, and then the 50th percentile of the combined distribution is about 92 percent, and then, in TSPA-1995, we used RH range between 85 to 95 percent, assuming uniform distribution.

Those are some examples we developed from our expert elicitation.

The next copy is the key parameters for carbon steel out barrier corrosion models.

In humid air or neutral pH aqueous condition, we will use the TSPA-95 model for general corrosion of carbon steel in both environments, and we assume that general corrosion will be dominant mode, with some low localization factors or variations, and in the alkaline pH condition with a pH greater than 10, we assume that carbon steel undergoes high aspect ratio pitting corrosion and the corrosion will be modeled with the pit growth law and the pit growth will be basically a combination of the general corrosion rate model and localized corrosion rate model, and the general corrosion model will be general corrosion rate of carbon steel in alkaline environment, will be modeled using modified TSPA-95 model, which basically incorporates the scaling factor to give us the general corrosion rate of carbon steel.

Let me over onto page 11 first and give you the distribution for constant "n" of pit growth rate model, which is "n" here, and the result I'm going to show you in the next three view-graphs basically covers this box here.

For the "n" values, four experts responded to this issue and then the 50th percentile of the combined distribution is about minus .58.

Next view-graph is for the distribution of the constant C-sub-L of the pit growth rate model for carbon steel in alkaline conditions.

Again, four experts responded to this issue, and then the 50th percentile of the combined distribution is about 4 here.

The next view-graph shows the distribution for pit density of carbon steel in alkaline conditions.

Four experts responded to this issue, and then the X axis is carbon steel pit density in number of pits per square centimeter, and the 50th percentile of the combined distribution, which is the red line here, is about one pit per square centimeters.

Those information will be incorporated into the PA model for carbon steel outer barrier corrosion degradation.

Now I'm going to discuss about key parameters for the inner barrier corrosion model which was developed from, again, expert elicitations.

We assume the general corrosion of inner barrier under humid air or non-dripping aqueous conditions scenario, and we assumed marginal galvanic protection of the inner barrier, a few hundred years at most, but Livermore is testing to confirm this galvanic protection issue of CRM, and until we have that result from Livermore, we will assume that the VA base case model wouldn't take credit of galvanic protection of the inner barrier, and as the expert mentioned that the localized corrosion requires a drip with elevated chloride concentration and low pH within a crevice, and then localized corrosion of the inner barrier will be modeled using a pit growth rate model which is, again, the same format as for carbon steel.

This view-graph shows the distribution for constant C-sub-G, which is basically general corrosion rate of the inner barrier, and this result covers this box here and this general corrosion rate here.

Four experts responded to this issue, and then the X axis here is the general corrosion rate in millimeters per year, and 50th percentile of the combined distribution is about 8 times 10 to the minus 5 millimeters per year.

The time constant "n" of the pit growth rate model for CRM -- two experts responded to provide input to this issue, and then, again, the 50th percentile of this combined distribution is about minus .58. Experts suggested that the value for CRM for this time constant time constant "n".

The last view-graph I have is for the distribution of the constant C-sub-L of the pit growth rate model here. Two experts, Dave Shoesmith and John Scully, provided the input to this issue, and the 50th percentile of this combined distribution is about 3 and 4.

So, I just, you know, covered some of the information we developed from expert elicitation for those key model parameters, model components, and concluding my presentation, the expert elicitation will be incorporated extensively in the VA analysis and then in the base case and the sensitivity analysis to develop scenarios for the base case and sensitivity analysis and also to develop or drive the key model parameters.

Also the base case and sensitivity analysis of waste package degradation modeling for the VA will be focused to evaluate the effect of waste package performance in the area of waste containment and isolation and providing such as a time history of waste package failure and the time histories of waste package perforations.

Also, we will be looking at alternative options for waste package design and effects of alternative EBS designs.

That's all I have for this presentation. Thank you.

DR. SAGUES: Thank you.

Evidently it's an attempt to quantify all the results from various authorities and opinions in some instances.

I'm going to take the chairman's prerogative and ask the first question here.

If you have, say, two streams, one of them with a pH of 7 and one with a pH of 14, the average pH is going to be probably going to be more like 13-something, because of course, it's magnitude and so on.

Has any attempt been made to take into account the sum of these magnitudes, for example, exponential coefficients and the like, may not be amenable to straight averaging in order to obtain the cumulative distribution curves?

DR. LEE: For that pH of the --

DR. SAGUES: No. I just gave the pH as an example of what happens when you are trying to average things that may not be averageable in a straight fashion.

DR. LEE: Actually, the work for addressing this issue is being done in the near-field --

DR. SAGUES: What I'm trying to say is the following.

In order to obtain average values or median values, you have taken the estimates from each expert and then obtained a median or obtained an average, and what I'm saying is that some of this magnitude, such as for example, pitting densities or exponents for growth and the like, may not be the kind of things that one can just go ahead and simply average in the sense that the average may have to be done on a logarithmic basis as opposed to a linear basis.

DR. LEE: Kevin, might be --

DR. COPPERSMITH: Coppersmith, Geomatrix.

Let me just comment in general.

Number one, while I have a chance and I have the microphone, let me point on that, on several of John Scully's assessments, you'll see "initial" and "final." He deals with a time-dependent process both for pit density as well as general corrosion rates, and so on.

So, they are time dependent. It wasn't that he changed his mind. They're not discrete alternatives.

DR. SCULLY: I changed my mind by 5 percent.

DR. COPPERSMITH: Okay. So, I just wanted to be sure that was clear, and the only way we could portray a time-dependent process was show at the beginning and then later.

Going back to how we're combining these, these were all probability density functions for particular parameters, and we're combining the density functions properly into a composite.

In that combining process, we're assuming equal weight among the experts. We're not providing differential weights. But it's the density functions that are being provided. You're right. We need to look at those to see if they make sense. But it is the density functions that are being provided.

The only reason we put the median on there was to draw attention to central estimates. I think the plan in TSPA is to look at that entire cumulative distribution function and to use that and sample from those distributions entirely.

So, again, we're only showing the central estimates just for the sake of discussion.

DR. SAGUES: Yes. What I wanted to point out is we are dealing sometimes with magnitudes which have been estimated with a logarithmic or exponential approach, and in that case you may end up with mathematical artifacts in the way in which that has been done.

DR. COHON: Cohon, Board.

I have a different kind of concern though.

When you have differences that large, you've got one expert saying the world is black and the other one is saying the world is white and your probability distribution says it's grey -- I need some help with that.

Could you justify that, when you've got orders of magnitude discrepancy between the experts?

DR. COPPERSMITH: Let me qualify what Joon presented.

It's very difficult for him to present all these in a vacuum because often the logic structure that got you to that point hasn't been presented.

In other words, to deal with, for example, the localized growth rate of the CRM, for a particular expert, there is a logic that gets you to that point.

Drips may be required, spalling of the outer CAM may be required, a whole process that gets you to the aggressive conditions that would lead to localized corrosion, and then we show a localized corrosion rate and compare it expert to expert.

In reality, I think the logic structure that gets you there provides much of the interpretation of why, in fact, there are big differences, but I think these differences that exist, let's say, in the localized corrosion rate for the CRM are related to some of the fundamental uncertainties that exist to this point.

DR. COHON: Let me pursue this. I think it's really fundamentally important, that applies not to just this part of the TSPA but all of it.

When you've got two experts who are disagreeing by that much, it's exactly the logic, the two different bodies of logic that led them to those two different conclusions that concerns me.

They may represent two fundamentally different views of the world and whatever is actually happening physically.

To average them is to say that there is some phenomenon going on that isn't possible, in fact, because it's going to be this or it's going to be that. You see my point.

So, what might be a more credible approach when you have such disagreement and such uncertainty is to take both distributions and flip a coin -- I mean you flip a coin anyhow using uniform distributions -- using uniform distribution to choose one of them and then flip a coin again and use some other.

I think you get a very different result than you get from what was done here by averaging the distributions.

DR. LEE: If I can add something on that, concerning all the uncertainties and probabilities in the near-field environment and corrosion process, the range of an order of magnitude between the expert elicitation, I thought, is kind of reasonable.

DR. COHON: That's another explanation.

DR. SAGUES: Any other questions of board members?

DR. VAN LUIK: This is Ed Van Luik from DOE.

I think, when we do the probabilistic sampling over the whole range of likely estimates, we will look and see whether, between one estimate and the other, that basically there are two different world views, and in that case, we would go to a bimodal distribution and probabilistically estimate it that way, and of course we would have to clarify that on the write-up, tell exactly what we did and why we did it.

DR. COPPERSMITH: Let me make one more comment related to that. Coppersmith, Geomatrix again.

We hope that the logic structure that was on to Joon's left, to our right, provides those links. In other words, the dripping scenario -- we should separate out yes and no, or localized corrosion, yes and no, or initiation probability.

Those are opportunities, then, to then follow a particular path and have the appropriate probability distribution for that path. If there are different scenarios, then the basic logic structure for the TSPA should split and follow those separately.

DR. SAGUES: We have a couple of board staff questions.

MR. REITER: Leon Reiter.

I just want to make sure -- question, will the median be used in some way as a central estimate, or is it here just for display?

DR. LEE: We will sample off the whole range.

MR. REITER: So, you're not going to use the median as a central estimate.

DR. LEE: No.

MR. DI BELLO: This is Carl DiBello, board staff.

I want a clarification of the use of galvanic protection, NPA.

I think you said quite clearly in your slides you would not be using it in the base case, yet yesterday Jack Bailey said, describing what is being turned over as a reference case to PA, that galvanic protection was included, and then today we heard from Jerry Cogar about how the waste package would be fabricated, where shrink fitting is going to be used, and I think the major, perhaps only driving force for shrink fitting is galvanic protection, but I may be wrong.

So, could you clarify again that PA's base case will not include galvanic protection?

DR. LEE: Not on this current model.

We didn't take credit for galvanic protection based on what we know from expert elicitation, but I think what Jack Bailey referred to yesterday, I think, Dave has told me, you know, explain better than me, I think he may be referring to the current testing underway at Livermore to clarify the issue more clearly.

DR. SAGUES: Any other board staff questions?

[No response.]

DR. SAGUES: Any of the experts at this time?

[No response.]

DR. SAGUES: Okay. Well, then thank you very much, Joon.

We are going to proceed now to the last presentation of the session, which is going to be by David Stahl of Framatome Cogema on the materials research program status and changes.

David?

MR. STAHL: Thank you, Dr. Sagues. Good afternoon.

I'm going to talk about materials research program status and changes, as indicated.

I'm going to briefly cover the role of the waste package materials testing and modeling program, very briefly cover the environmental assessment -- assumptions, rather -- you've heard about those. I think you pretty much know about the containment materials.

I'll cover that very quickly, talk about our strategy, where we are right now, and how we've modified the program to incorporate input from the expert elicitation effort and other boards.

Basically, as indicated here, the role is to provide a scientific basis for our waste package and engineered barrier design and performance assessment, and we have a whole host of materials that we're evaluating to provide performance data to design and PA.

You can read that at your leisure.

You've heard about the -- basically the weather report for the repository.

We assume that we have early hot and dry conditions, followed by cooler, more humid conditions, with the potential for dripping of concentrated groundwater onto the waste packages, and we think that that's a conservative situation, as I'll talk about a little bit later.

We feel that that's not going to last a very long period of time.

We have a very wide testing range, from 10X to 1,000X J-13 water, our pH range is in 2 to 12, which encompasses the design range and the control design assumptions document, which is 4 1/2 to 10 1/2, and our temperature range is 60 to 90 degrees C.

As I mentioned, we don't believe that the corrosion degradation is closely coupled to the local conditions. Excuse me. It is more closely coupled to the local conditions at the surface of the waste package than to the seepage flux chemistry, and Dr. Scully and Dr. Shoesmith have addressed that area.

Now, what we're doing in the corrosion test program is basically following that scenario. We start with some TGA and relative humidity chamber tests, where we've exposed coupons to determine the effect of low and high relative humidity on corrosion allowance materials and the surface conditions, including clean, oxidized, and salted conditions as a result of that splashing of concentrated waters onto the package.

Then we go into the long-term corrosion test facility, where we expose coupons, standard weight-loss coupons, U-bend, and creviced specimens under a range of conditions, and we're looking at vapor phase, waterline, and full immersion both for the corrosion allowance and corrosion-resistant material.

This is a listing of materials. We're looking at several kinds of corrosion-allowance materials, as indicated. We have a broad range of corrosion-resistant materials, including the titanium Grade 12 and the titanium Grade 16 that Dr. Shoesmith was mentioning with palladium.

We have other materials in the program. We're looking at 304/316 stainless steel with and without boron to support the design effort that Tom Doering talking about.

We also have some zircaloy which will be added to support the Navy program, and we are just starting up our ceramics program, looking at various oxides or combinations of oxides.

I'm not going to dwell on this, and I discussed it at length in a former board meeting, but I'll just indicate that we're in this stage here, we've developed detailed plans, we're performing tests and developing models to give us PA design information.

We have input from technical experts, including the waste package degradation expert elicitation panel, and we do all kinds of different tests, characterization tests, service conditions, accelerated tests, later on we'll be doing confirmation tests and also be inputting some information from analog evaluations to help us in our long-term predictions.

This is a laundry list, basically, of the kinds of testing that we're doing in materials. We have engineered barrier material studies, and we also have some waste form studies.

I think I've mentioned in the past that most of these are being done by Lawrence Livermore Lab. The waste form studies, for the most part, are being done by Argonne National Laboratory and Pacific Northwest National Laboratory.

Now, I've just chosen a couple of areas to focus on. One is on the long-term corrosion test facility. These, unfortunately, don't copy very well. This just shows the laboratory.

We have 24 test vessels here, as indicated. Eighteen are general corrosion tests, and six are galvanic test group vessels. They're about -- little more than a meter square and about a meter-and-a-half high, and they hold about 250 liters of water.

Each of these vessels has six rows of racks. These are the racks with -- you can see the U-bend and the crevice and standard weight-loss coupons.

This is a picture of some of the carbon steel specimens before and after corrosion, and as you can see, carbon steel rusts, which is what we knew. It's just a question of what the rate is.

As I indicate over here at the bottom, the carbon alloy steel showed corrosion resistance in the range of expected values, about 100 microns per year. In the vapor phase, it was about one-third of that, and at the waterline, it was about twice that level.

Let me just show some pictures quickly of some of the corrosion-resistant materials.

Let me show first alloy 825, which is the only specimen here that we did some crevice or pitting attack under this crevice Teflon ring. It's a standard corrosion test.

In contrast, 625, C-22 did not show any attack. We see just a little discoloration.

As far as the two nickel-based -- I should say titanium, as well. I didn't have a picture of it.

As far as the nickel-based materials, copper and nickel-based, Monel and 715 nickel-copper material, very little localized attack. Basically, we have general attack.

This is the weld. Most of these specimens are welded. There's a variety, welded or non-welded. I just happened to pick the welded ones to show you, because there's no weld susceptibility that we found in any of the samples.

Going on to the humid air corrosion tests, we've done a variety, as I mentioned, of TGA and relative humidity chambers.

Let me skip right to the results. We've got just the beginning of some six-month data.

Basically, for clean surfaces, no corrosion evident for carbon steel or titanium alloys that we have to have in that test.

For salted surfaces, we do see a reddish-brown oxide corrosion evident for carbon steel. This is consistent with our TGA critical welds and humidity test that was performed earlier, certainly nothing evident for titanium.

Let me get on to the modifications to the program that came about as a result of input from the expert elicitation panel and other panels, because I'll include some other areas that you may have an interest in, and just alphabetically, I'll talk about alternative materials, carbon steel pitting.

I won't talk in much more detail except that, for the general corrosion testing, we will be putting in some thicker specimens to study pit propagation and geometry, and we've already started up some vessels with concrete modified pH.

I'm just going to treat these alphabetically.

Firstly, for the alternative materials, the expert elicitation indicated that C-22 is preferred over 625 based on available short-term corrosion data. The problem is there isn't a lot of data out there.

We have, as I mentioned, the six-month data for the vessel tank or the vessels which show that 625 and C-22 were basically immune, at least at that point. We will be looking at the one-year data about the February or March timeframe.

What we plan to do and we're starting up very shortly some short-term tests under adjusted chemistries to obtain parameter values for the crevice corrosion model that's been developed by Joe Farmer, and he's had considerable discussion with Dr. Scully on this particular issue.

Another interesting experimental effort that we're getting going is a micro-sensor device which is basically put into the crevice to determine the pH suppression and the chloride content enhancement as corrosion proceeds, and this will be input to the model.

This is a device that was developed at Livermore, actually, for some biological testing. It's being given new application.

So far as the ceramic testing is concerned, the experts were concerned about flaking or spalling of the ceramic coating.

We feel that that will be eliminated by the utilization of backfill prior to closure, and that is, if, indeed, you wanted to go to a ceramic or a drip shield, that you'd more likely backfill the repository, and I believe that Jack Bailey discussed this yesterday.

So, what we've done is then modify the program to look at some of the other issues in regard to ceramics and that is the density of the coating and its impermeability and the resistance to handling loads.

One of the things that you can do is to modify the composition of the coating to match the thermal expansion, and we feel there's many different oxides -- alumina titania oxides, for example, and zirconia, stabilized -- that have a very good thermal match with the carbon steel.

There's also many different thermal spray mechanisms that can be used -- high-velocity oxy-fuel and detonation gun, and we have been able to get coatings in excess of 97 percent theoretical density.

We have made some samples up, and they've been put into the corrosion vessel that you saw in the picture, and we're actually looking at corrosion resistance for both notched and un-notched specimens.

So, we put some notches in those specimens to evaluate the impact of localized corrosion and whether, indeed, you will get flaking or spalling of the ceramic and how far that extends around the surface.

So, hopefully that will resolve some of those concerns.

Another subject for the last elicitation and will be covered in another elicitation had to do with cladding credit and was brought up by other experts.

They suggest that a credit be taken for cladding performance. However, we don't have very good database under prototypic conditions.

We are initiating some tests at Argonne to study the restraint of cladding on fuel expansion. These will be humid air and dripping water cases.

We are planning to do also some mechanical tests to evaluate the response of broad segments to rock loads. Actually, we're thinking of doing this at Argonne, and they have the companion study that's going on jointly for EPRI and the NRC.

And lastly, as I mentioned, we will be studying some zircaloy corrosion in our corrosion test facility.

Galvanic protection -- that's one of the more interesting ones, as indicated by Dr. Shoesmith and Dr. Scully and discussed just recently by Joon, Dr. Lee, and that is that you have pits and corrosion allowance material and they're like to be wide and not offer much galvanic protection to the corrosion-resistant material.

As shown by Dr. Shoesmith, as the corrosion allowance material corrodes away, opens up larger areas, you then become nonprotective.

However, the experience based on thick-wall carbon steel vessels and pipes is not conclusive regarding pit geometry. The pits can be narrower or wider depending on the chemistry of the water in that pit.

So, we have initiated long-term experiments under several environment conditions to determine the degree of galvanic protection.

As indicated, throwing power and corrosion geometry, particularly the pit geometry, will be evaluated.

We also have couples of corrosion-allowance, corrosion-resistant, and corrosion-allowance sandwiches, basically, that have been put into tests.

These vessel chemistries are the same as the other tanks, and we also have a concrete modified chemistry, and there we're trying to look at the impacts at high pH.

In MIC, I think most of the experts believe that MIC is only possible when the repository cools less than 80 degrees centigrade and relative humidity increases above 60 percent. So, this kind of a scenario follows the approach that Dr. Little suggested in the expert elicitation.

What we need to do first is get an answer, basically, are microbes present or can they enter later? If they are present, for example, after 1,000 years when the repository cools, are there sufficient nutrients to prevent microbial colonization?

For example, if you're able to keep out phosphorous and sulphur from the concrete, for example, or from the water itself, you may find that the microbes will not be able to colonize.

If those nutrients are present and they colonize, will they colonize on the corrosion-allowance material, and studies to date indicate that, in C-22, there's been no evidence of any microbial attack and just one isolated evidence of some attack in 625 that may or may not be MIC-related.

If they do colonize, will they enhance corrosion rates, and again, that's part of our thermal program. We're doing this jointly or in cooperation with performance assessment.

So, to summarize, we have long-term tests underway. Six-month data have been collected, and one-year data will be available shortly.

In response to the waste package degradation expert elicitation and their concerns, we've modified the testing program to address those concerns. We're focusing on corrosion-resistant material selection, pitting of carbon steel, and the viability of galvanic protection, and we're looking at the viability of ceramic coatings and, of course not related to this expert elicitation, but we are looking into cladding credit.

So, with that, I'll close and take questions.

DR. SAGUES: Thank you very much.

Do we have any questions from the panel?

Dr. Knopman?

DR. KNOPMAN: Perhaps a quick question. Knopman, Board.

You've gone through lots of testing plans. What would be your top two or three if you only had the resources to do two or three test runs of the kind you describe? That is, what are your highest priorities?

MR. STAHL: Well, certainly, one is to look at short-term tests, to get the data needed for modeling of the performance of the corrosion-resistant materials, the 625 versus the C-22, and the second would be understanding the pit generation and the possibility of galvanic protection from the corrosion allowance material.

DR. BULLEN: Bullen, Board.

Dave, just a quick question about the geometry of your ceramic coated specimens. Are they just simple test coupons that have been notched?

MR. STAHL: No, these are about one-inch-diameter circular bars that have been coated.

DR. BULLEN: Okay. So, the inside is a metal and the outside is a ceramic?

MR. STAHL: Yes.

DR. BULLEN: And the outside has been notched so that it has a pre-crack in it?

MR. STAHL: Correct.

DR. BULLEN: Okay. That's good.

I was worried about geometry, because even a little corrosion can give you a stress concentration a distance away that would give you a fracture and a spallation, and so, I guess you've got the worst case condition there, because you've got a very small radius as opposed to the large radius.

MR. STAHL: That's correct.

DR. BULLEN: Okay. Thank you.

DR. PARIZEK: Parizek, Board.

To my knowledge, there have been no free drips yet seen in any of the excavation work done at Yucca Mountain, but because of the fast path of it, you have to almost allow for it.

Is it possible that the program would go another year and say we still haven't found any dripping water and, therefore, bypass that part of the problem?

MR. STAHL: I would doubt it, but I think there are others that would have to answer that question.

We do have an in situ thermal test program. We have the single heater test, and in this November-December timeframe, we will be starting up the drift scale heater test, and we'll be looking, during the heating and cooling cycle, at where that water goes and whether we do have the potential for water coming back, what its chemistry is, and what rate it might re-contact the waste packages.

DR. PARIZEK: You almost have to design to allow for drips based on what's known even if you never seen any free water.

MR. STAHL: That's correct.

DR. SAGUES: I have a question. On the experiments in which you're going to sample the pH and chloride content inside the crevice, is that a mixed metal crevice, like a corrosion allowance metal on one side and a corrosion resistant on the other?

MR. STAHL: Yes.

DR. SAGUES: How big of a gap are they trying to use in that? Is it like a fraction of a millimeter type of gap?

MR. STAHL: With the standard crevice samples, there is a prescribed gap. You have teflon and then you have a certain pressure that you put on it, and so, you get something in the range of -- a few microns, is it, David? I forget what the number is.

That's the same dimension as the micro probes, these very fine hair-like optical fibers that are used in these tests. So, at least it's a consistent geometry.

DR. SAGUES: I look forward to see that.

MR. STAHL: Yes, I am, too.

DR. SAGUES: We have one question from one of the experts.

MR. DANKO: George Danko.

You showed crevice corrosion test results using this teflon?

MR. STAHL: Yes.

MR. DANKO: And I would like to ask if that gives you enough data to predict corrosion between the container and some potential backfill material?

MR. STAHL: That's an interesting question, and I'm not sure I can answer that directly. With that, you don't have perhaps the pressure that you have in a normal crevice, but certainly you might expect to have some crevice attack on the surface of the carbon steel.

The crevice attack in carbon steel is not a major problem, because it's really controlled by the general aqueous corrosion.

So, I suspect, as Dr. Shoesmith had said, that you will get areas that will preferentially corrode at the beginning, but as time goes on, it will level out.

So, I wouldn't anticipate that to be a major contributor to the corrosion of a outer barrier.

MR. AHN: Tae Ahn, NRC.

This question is to all the presenters this morning. From our CTA exercise, we identified a very critical path in the radionuclide release -- that is, the radionuclide leads to the failure of the container mainly through the pinholes.

Two questions.

One is what kind of materials will be inside a pit? I don't think anybody today addressed the characteristics inside a pit.

Second question is how do you elicit the uniform corrosion rate? For instance, if I take, let's say, the current density of 1 micron per year, there will be no diffusion barrier after 10,000 years, because all are corroding uniformly.

Those two questions are related to your long-term testing, but I don't think that rationale or background was discussed today. This is very, very important.

MR. STAHL: Well, we've made some assumptions in both of those cases. I'll let Joon respond, but let me answer the first part of your question.

We do have a small study at Purdue that's looking at transport through pitted materials, and we're particularly interested in the pitting -- excuse me -- the transport if you have occluded pits.

So, at least we'll partially evaluate that issue, but the question, of course, is can you guarantee that, once the pits form, they'll become occluded? But at least you will have a basis for an assumption.

Joon, do you have anything you want to add in regard to the second part of the question?

DR. LEE: Joon Lee with the M&O.

In fact, we have a technical meeting with the NRC on November 5th and 6th, and we will be discussing that issue in more detail, but the corrosion rate of the inner barrier will be time dependent, and if we have like a .1 microns per year rate, it would give like 200,000 years lifetime of CRM, basically, and during that time period, we still have some kind of a pit formed on the CRM, and then what -- the assumption in PA waste package model is that we feel the environment will provide a lot of species, and those species will precipitate as minerals in that pit area and that the precipitate will form more like a porous kind of clog inside the pit, so that provides some kind of a diffusion barrier, but we can discuss with more detail.

DR. SAGUES: I would like to say that we are still within questions for board members, and we have a general public question in a minute, but do we have any questions from board staff?

MR. DI BELLO: This is Carl DiBello again. This is really a followon from my last question, Dave. Excuse me. Maybe I can direct it to Dick Snell.

Does the reference design that is going to be used for VA include galvanic protection or not?

MR. SNELL: I'm Dick Snell, M&O.

As of now, the reference design for VA does include galvanic protection.

You've heard here a lot of information to suggest that galvanic protection is of limited value, and that will be taken into account when we assess the overall performance of the design, and the experts here can correct me if I am wrong, but I think the galvanic protection is more helpful when you're talking about pitting corrosion mechanisms than it is when you're talking about the spalling-type corrosion mechanisms.

There are circumstances, I think, when we look at the design performance over time where the pitting mechanisms is of importance, and I think, for the time being anyway, we will, as I say, leave it in the reference design, take what credit for it we can derive.

The door is not closed on the issue.

Let me add that we did start a big galvanic test that I showed last month, and our hope is that, in the February timeframe, we have at least about six-month data to help support that kind of analysis.

MR. DI BELLO: May I just continue? One more thing.

It sounds like one component of the VA is the design, it's going to have galvanic protection; another component of the VA is the TSPA-VA, it's not going to have galvanic protection. I'm wondering if somebody from DOE could explain that dichotomy.

DR. LEE: For the base case analysis, we would not take credit for galvanic protection.

The experts indicate that, at most, we would have like a few hundred years of benefit of galvanic protection, which is very, very marginal in terms of modeling 100,000 years waste package degradation modeling. So, we tend to not take credit.

MR. BARNARD: Bill Barnard, board staff. I've got a couple of questions for different presenters.

The first question is for David Shoesmith.

David, if I understood you correctly, you said that, if we used the dual-barrier design with our carbon steel, high-nickel steel, that we could probably get about 3,000 years of waste isolation out of that. Is that what you said?

MR. SHOESMITH: No. What I meant was I don't think that 3,000 years -- I think that's the minimum you will get.

MR. BARNARD: That's the minimum.

MR. SHOESMITH: Yes. I don't think that's when it will fail.

MR. BARNARD: If we use that as sort of a baseline, what if we went to a titanium canister? What would be the minimum isolation time?

MR. SHOESMITH: It would be presumptuous of me to suggest that there are no tests that should be done to justify some of the claims I'm going to make, but -- that's not as facetious a comment as it sounds.

There are many things that you can eliminate with titanium, and theoretically, you should be able to get to the 5, 10 to the 6 years, about a million years.

MR. BARNARD: A million years.

MR. SHOESMITH: Yes.

MR. BARNARD: John Scully, do you believe that we could get a million years out of a titanium canister? How about 100,000? How about 10,000?

DR. SCULLY: Scully, UVA.

I haven't thought much about titanium in this application, so I'd like to defer that question to further thought.

You know, I think that titanium is going to be more in the realm of either being in the window or out of it, not a damage function evolution, and so, if you're out of it, the answer is yes. If you're in it, it will fail quickly, and that's based on a lot of work that I've done recently on beta titanium alloys.

There is technical justification, incidentally, for the titanium alloys that we study, one of which is a orthopedic hip implant alloy. That only has to last 40 or 50 years, like for Bo Jackson, but in that alloy there's a technical justification for stifling of crevice corrosion for that alloy, and there's a scientific technical justification.

And so, I would say that, if you're outside of the windows of susceptibility, titanium will last a million years, yes. If you're in the window -- if you get into a window of susceptibility, then failure modes could lead to fairly rapid failure.

So, it's that type of material. It's like falling off the edge of a cliff.

MR. SHOESMITH: You've got to chose the alloy appropriately, and I don't think that you can stand -- there are certain alloys which, if they start, it will go very quickly, but for the two mentioned, the 12 and the 16, we can get titanium 12 to crevice corrode submerged in a chloride solution at 150 degrees, but we do minimal damage. We can't actually get the process to start at 150 degrees in sodium chloride on the titanium 16, and we feel that we have a good explanation.

Come out to the University of Virginia tomorrow. It's in the seminar that I'm going to give there. But I have a report in my briefcase which outlines why we are so keen on titanium. I was going to give it to John, but if you would like it, I'll leave it and send him another one.

MR. BARNARD: Yes. Thank you.

I've got another question for, I guess, Tom Doering or Hugh Benton.

As I recall, overall estimates for waste package costs for the repository are somewhere in the neighborhood of $4 billion. If we went to a titanium package, how much would those costs increase? Do you have any idea?

MR. BENTON: Hugh Benton, M&O.

We really haven't costed that. There are issues with titanium. We have a very heavy package. The fuel is very heavy. We've got to make sure we can pick this thing up.

We haven't really designed but will evaluate the effects of titanium, either with or without the carbon steel. Titanium and carbon steel have some incompatibilities.

So, the answer is we're not sure, because we're not sure how thick the titanium is going to have to be in order to give us the structural capabilities that we have to have.

I think it's safe to say the price would be significantly up but may be well within reason.

MR. BARNARD: What's well with reason?

MR. BENTON: I guess we have sort thought of a general threshold that if an individual waste package for 21 assemblies had to get -- approach a million dollars, that might exceed the threshold of reasonableness.

MR. BARNARD: How does translate into waste package cost --

MR. BENTON: Well, that would be something like a doubling or two-and-a-half times the current cost.

MR. BARNARD: Okay.

I have a question for one of my board members.

Dr. Bullen, would you like to comment on the comments that you've just heard on the titanium waste package?

DR. BULLEN: Bullen, Board.

I think the comments would be that he's right, that it would entail a significant cost increase, meaning a factor of two or more, and that the analysis would have to be done with respect to the thicknesses required or the changes in design necessary, but if you looked at -- I mean my pet project is the C-22 clad with titanium as a corrosion-resistant material over another corrosion-resistant material, precluding MIC, precluding other failure mechanisms, but I know that I haven't done the completely design analysis necessary to decide if I can pick that can up, so --

I mean I know I can probably make it in estimated cost, but if I can't pick it up, then it's probably not a worthwhile can, so I would have to defer to the designers in that case.

But you and I have had this discussion before, and you know, this is my favorite, and that's why I asked the question of both of the presenters today.

DR. SAGUES: Okay.

Do we have any final questions from the board or board staff?

[No response.]

DR. SAGUES: How about the experts?

[No response.]

DR. SAGUES: Okay.

Then I'm going to thank all the speakers again and transfer the meeting to Dr. Cohon.

CHAIRMAN COHON: Thank you, Alberto.

We now move to our comment period, and I'd like to call on board member Paul Craig.

DR. CRAIG: Paul Craig, Board.

I have a comment that goes back to the meeting that was held on Tuesday, which was on the interim performance.

Back in my early career, I worked at Los Alamos, and my paycheck came from the University of California but the money came from the AEC.

In due course, the AEC was split up into what eventually became the DOE and the Nuclear Regulatory Commission, and the split had to do with regulation versus advocacy.

That is exceedingly important.

I want to make a remark that is probably not a remark within the purview of the Board. Nevertheless, I make it as a private citizen.

At the meeting on Tuesday, Steve Brocoum showed a view-graph which read, since the 25-millirem-per-year limit is an all-pathway limit, a separate groundwater protection limit is not needed.

Now, this is not the first time I've heard this, but this time it really got to me, and the reason is not that I disagree with his statement. In fact, I agree with it. I believe, personally, that a separate groundwater protection limit is not needed.

However, I do not believe that the Department of Energy should be explicitly attempting to influence the regulation standards-setting process. The Department of Energy should be an independent organization.

I want the people in it to be enthusiastic about what they're doing, but I am very disturbed when I see them publicly advocating what the standards ought to be, and I believe that this is damaging to the overall process.

The damage may by now be hopelessly done. Nevertheless, I would be personally very much encouraged if the posture would change during the next phase.

CHAIRMAN COHON: Thank you, Dr. Craig.

Is there anybody else who would like to make a comment or ask a question about anything related to this meeting or this topic?

MR. VAN LUIK: This is Ed Van Luik, DOE, and I wanted to clarify to Mr. DiBello that the TSPA-VA will have a thorough discussion of the importance of galvanic protection or the non-importance of galvanic protection, but Joon Lee is correct in that the base case will only invoke galvanic protection when there is dripping causing pitting in the outer corrosion barrier, and even then it will only be an effect that is, as the current modeling goes, for a couple of hundred years.

As far as the other issue about advocacy, we were public commenters on the National Academy of Sciences report and made the same statement in our public comments on that report, and so, basically, we have taken a consistent stance, but we will look at your comment and take it under advisement.

CHAIRMAN COHON: Thank you, Ed.

Yes, sir.

MR. PAYER: I'm Joe Payer. I'm on the peer review panel for the TSPA, DOE. In my real life, I'm at Case Western Reserve University in Cleveland. Go Tribe.

Two issues.

One, I'm not sure that it came through in some of the discussion of the corrosion, because we get down into a lot of details, but a very critical issue is will localized corrosion of the inner barrier currently, the corrosion-resistant material -- will a thin film of moisture or droplets of moisture, drips, sustain crevice, corrosion, or pitting, and as John said and David would agree and I think the expert panel said, that's an open question in the corrosion community.

We don't know much about pitting. Most of the pitting and crevice corrosion work is done under fully immersed conditions, with some accelerated -- accelerated either chemically or electrochemically to drive the corrosion.

The general feeling is that it will not sustain pitting or crevice corrosion, but the evidence of that has not really been well-developed.

That's a big issue. If these materials are passive, that's the difference between the 10 to the 5th and 10 to the 6th life for titanium and failure in a matter of tens of years.

It's a big, big issue, and that addresses a little bit the second point, Dave, and this is, I think, to Dave Stahl and the matching of the experimental program to support VA and, subsequently, LA.

A strong message in the expert elicitation is that a big unknown is not only the amount of water but the chemistry of that water, and if the project is required to come up with a material that is resistant to boiling ferichloride and highly-reducing hydrochloric acid and a strong basic caustic condition, then, you know, we don't have any materials that are going to do that.

It's highly unlikely that that range of conditions is going to exist down in the repository.

So, I guess my question is -- and I know what you commented on today is how the program is going to -- how is the program going to address those critical issues -- database on corrosion-resistant materials and then, secondly, the environment in the long-term? You're not going to be able to impact VA, I understand.

MR. STAHL: David Stahl, Framatome Cogema Fuels.

We are trying to address each of those issues, Professor Payer, first by looking at the corrosion vessels themselves.

As I mentioned, we have samples exposed in the water and also in the vapor phase, where the vapor is close to 100-percent saturation.

So, there are liquid films that are on those specimens in the vapor phase, and so far, we've seen no crevice or pitting attack on those samples. Certainly, that will be followed.

We're hoping, also, that the micro sensor that I mentioned, where we get pH and chloride information on the crevice, will help us understand what's happening in the crevice and be supportive of the short-term test that we're performing to get model parameters for the crevice and pitting corrosion model.

So, those two things together should be able to give us an understanding of the impact of some of those bad actors like chloride and pH and some of the perhaps mitigating anions like bicarbonate that will be able to lead to a predictive model.

Certainly, I agree that we're not going to have the very aggressive conditions that some of these materials have tested against.

For example, the chart that was shown earlier, which is kind of what they call the green death solution -- you don't really get that in real life, but it's a way of screening materials for chemical reaction vessels.

What we need to do is have a set of less aggressive conditions, understand the behavior, and then hopefully better quantify what we anticipate so that we match, as Dr. Scully said, the window of susceptibility with the conditions that those materials can withstand.

So, I hope that addresses your question.

CHAIRMAN COHON: Would anybody else care to comment or to ask a question?

[No response.]

CHAIRMAN COHON: Well, this meeting over the last two days, focusing on design from the repository to package, I think, has been a very good one.

I don't think the word "fun" could be applied to such a meeting, but there is a certain satisfaction in talking about things that we can tinker with, as opposed to wondering and guessing about the mountain, which is what many of our other meetings are about.

I want to close by thanking all those who participated.

To our speakers, we thank you for all the effort that went into your preparation and your effort to be here and answer our questions.

To my colleagues on the board, especially Priscilla Nelson for chairing, and John Arndt, Daniel Bullen, and Alberto Sagues for convening individual sessions, and to the staff for their help in preparing for this, especially Carl DiBello and Russ McFarland, who took the lead in preparing for this meeting.

We stand adjourned.

Thank you very much.

[Whereupon, at 1:35 p.m., the meeting was concluded.]