UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SUMMER 2000 BOARD MEETING

Piñon Plaza Resort
2171 Highway 50 East
Carson City, Nevada 89701

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Scientific and Technical Issues and
Total System Performance Assessment

NWTRB BOARD MEMBERS PRESENT

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Dr. Daniel B. Bullen
Dr. Norman L. Christensen, Session Chair
(Repository Safety Strategy)
Dr. Jared L. Cohon, Chair, NWTRB
Dr. Paul P. Craig, Session Chair (TSPA/SR)
Dr. Debra S. Knopman
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I N D E X

P R O C E E D I N G S

8:30 a.m.

COHON: Thank you. It's my pleasure to welcome you to this second day of our summer meeting. Yesterday was a very full and productive day, and we look forward to the same for this day.

I want to make a couple of introductions before we get down to business. I'm very pleased to note that we've been joined by Bill O'Donnell, a member of the Nevada State Senate. Senator O'Donnell, thank you very much for being with us today. And we're pleased you could be here. We hope you can spend a little time and get educated and maybe participate.

I'm also very pleased to introduce to you a new member of our staff. Her name is Joyce Dory. And, Joyce, if you'd stand up so people can see you? There's Joyce. Joyce has just joined us as Director of Administration for the Board. She's succeeding Mike Carroll, who many of you know. Mike, as you may recall, moved on to a position at the Department of State.

Joyce, before joining us, was Chief of Budget, Finance and Administration Services in the Office of Federal

Contract Compliance at the Department of Labor. And prior to that, she worked at various high-level budget positions at the Equal Employment Opportunity Commission and at the Department of the Army. We're very pleased she's with us and look forward to many years of working together.

Welcome, Joyce.

Relish that applause, because it probably won't come again. In the nature of your job and the nature of this Board, that might be it.

One scheduling note for today. To accommodate two members of the public who have to depart early today, we're going to add a public comment period at 11:45, which was the time we had scheduled to break for lunch. We will break for lunch immediately after that public comment period. Lunch will be at least an hour. Don't worry, we're not going to be that grim. I currently expect that the lunch break will commence at 12:15 or so, and we will reconvene at about 1:15. But we'll update that at that time.

I want to emphasize, though, we will still retain the public comment period previously scheduled for the end of the meeting. That is on the schedule at 4:50. My guess is it will be around 5 o'clock, not too much after that.

With that attended to then, it's my pleasure to introduce to you Paul Craig, a member of the Board, who will Chair this morning's session. Paul?

CRAIG: Thank you, Jerry. My name is Paul Craig, and I'd like to welcome you back for the second day of this meeting of the Nuclear Waste Technical Review Board. This morning, we'll continue our discussions on TSPA for Site Recommendation, commonly known as TSPA/SR.

As our chairman and Dan Bullen pointed out yesterday, TSPA/SR will provide the primary technical basis for any decision on the suitability of Yucca Mountain as a repository for the nation's spent fuel and high-level radioactive waste.

The Board has emphasized the need for transparency, that is, that readers should be able to gain a clear picture to their satisfaction of what has been done, what the results are, and why the results are as they are. That's a quotation from the Nuclear Energy Agency, 1998.

The Board has also emphasized the need for the DOE to quantify, describe and display the associated uncertainties.

We'll begin today with a continuation of the presentations on individual components of TSPA/SR and related sensitivity tests.

Yesterday, we heard about the unsaturated zone, the engineering barrier system environment, and the waste package and drip shield. This morning, Christine Stockman will discuss the waste form, that is, the radionuclide inventory, degradation of the spent fuel, high-level cladding, high-level waste cladding, radionuclide solubilities and formation of colloids. This is a lot of important chemistry that helps determine the source term, that is, the types, amounts and timing of radionuclide release from the engineered into the natural system at Yucca Mountain.

Following Christine, Bruce Robinson will discuss saturated flow and transport, that is, how released radionuclides travel with the groundwater from the unsaturated zone beneath the repository to the accessible environment some 20 kilometers away.

John Schmitt will then discuss the biosphere, or how the living world of plants and animals can take up any released and transported radionuclides. All this will end up in an estimate of amount and timing of the radioactive dose that a member of the so-called critical group will receive.

The last presentation will be by Kathy Gaither on disruptive events, that is, on the effect of earthquakes and volcanic activity on the repository. We've already seen that according to TSPA/SR, volcanic activity provides the only dose during the first 10,000 years of repository lifetime.

There's one more speaker before lunch time. It's Abe Van Luik, who will tell us about the DOE's efforts to get a firmer grip on uncertainty in TSPA/SR. He'll discuss both general plans for estimating overall uncertainty, and some specific results for individual components.

As discussed yesterday, uncertainty in TSPA/SR is of great interest to the Board, and was the subject of a recent Board letter to DOE. We're especially looking forward to Abe's presentation.

I'd like to remind everyone that we're trying to limit ourselves to questions of clarification during these first four presentations. There will be ample opportunity to ask other questions or provide comments in the panel discussion this afternoon. We've allowed 30 minutes for each one of these presentations, and as you start to approach too closely on your limit, I'll speak up.

So our first speaker is Christine Stockman. Christine is from Sandia National Laboratories where she's the project leader on the Waste Form Degradation Model Report. Christine is a chemist by training, and has spent more than ten years working on performance assessment and waste disposal.

Christine?

STOCKMAN: As he said, I'm Christine Stockman, and I'm the Waste Form lead for Waste Form Degradation. But I wanted to first off thank Rob Reckard, he's the PA lead for Waste Form in the project, and he prepared all these slides for me while I was off at a family wedding.

This slide shows the eight components of the waste form degradation model, and it shows their interconnection. In-package chemistry is here on the left. It is a controlling factor on all the other components. It controls the CSNF, or commercial spent fuel degradation rate, the cladding degradation rate, the DSNF degradation rate. In reality, that would be controlling. We don't have an arrow here because we've bounded this so high we didn't need to have that connection in the abstraction. Then there's the high-level waste degradation rate, the dissolved concentration limits, and the colloidal component. Those are all dependent on chemistry. The only thing that is not is the radionuclide inventory, which is just a straight feed into the model.

The process model factors that Bob Andrews showed yesterday are pretty much the same as those eight components. We have the in-package environment, the cladding degradation, the three different waste form degradation rates, the dissolved concentration limits, the colloidal concentration, and then also here in-package transport. We've hatched that because this is partly in waste form and partly in EBS transport, and this one we very much bounded in the current TSPA presentation.

So we're going through the assumptions and some of the results today, and first is the assumptions of the chemistry component. First of all, the bulk chemistry is what we're considering here, not localized chemistry. And in our modelling, we found that the bulk chemistry was controlled by the cladding, coverage of the CSNF, or the degradation rate of high-level waste glass in a co-disposal package, and the steel degradation rate for the basket materials holding the waste, and it was also by the assumed gas pressure that we used in the calculations. We assumed ten to the minus three, atmospheric CO2 pressure, and atmospheric oxygen pressure in our calculations. And when we did this, these controlled the bulk chemistry.

In turn, as I just said, the bulk chemistry does affect the other components. And the other thing in the bulk chemistry is we assumed a well mixed, fully oxidizing, full bathtub model. There are other scenarios with thin films of water where you could allow the inside of the package to go non-oxidizing at early time. We did not do that. We had a full bathtub, well mixed and fully oxidizing, which we felt was conservative for the bulk chemistry.

We are continuing to do sensitivity studies with our codes now, varying the amount of water to solids. We don't believe that's going to make a large difference, but we will see. And we have also added in sensitivity studies on the type of water we add. In the last bullet here, we used J-13 water as the input. We'll be using concentrated J-13 as well to see if that makes a big difference. We don't believe it will.

This shows the uncertainty in the TSPA calculations of the resultant pH that came from our abstraction. And the title here is actually a little misleading. It's saying that the pH for the commercial fuel has a larger spread of uncertainty than for the co-disposal. And this is true for the TSPA abstraction, but for the process model reports, it looked the other way around. For the process model reports, we varied the corrosion rates of all materials inside the package. We varied the seep rate of water entering the package. And the seep rate was a very important factor.

Now, let me go through some of this in a little more detail, and let me also point out that the time scale here is time since first package failure. This is not time, absolute time. If the first package breaches at 50,000 years, then this would be 51,000 years here. The reason we did this is there's no reactions going on until a waste package breach and water gets into the package, and then during the first thousand years or so, we have reaction of the materials within the package, and in particular, the sulfur and the carbon steel will oxidize and produce sulfuric acid which depresses the pH in the early period.

Following that, and as more seepage comes in, and the CSNF reacts with the water, it comes up more neutral. In the co-disposal package, you also have a period where it goes acid because of the carbon steel. But then as the high-level waste degrades, it's quite alkaline and it brings it up to about a pH of nine.

Another feature that you can see here is based on the other things you've seen yesterday, there is not much seepage until about 40,000 years. And you can see here in the co-disposal, that this is all pretty flat and straight until about 40,000 years. Then the pH starts to dip down. That's where seepage is actually diluting the chemistry and bringing it more towards J-13.

The other thing is what we did in this abstraction, we tried to be conservative and we tried to be simple so that it could be easily implemented in the TSPA. So what we did is depending on the time period and the waste package, we had different assumptions. For the commercial fuel, this period shows the range of the minimum pH seen in the first 1,000 years. Whereas, in this region, we used the average over the whole time period for the pH, and that's why that's a lot flatter.

If we had actual pH shown in the actual runs, they would be horse tail plots, they would be jumping up at different times, they'd be wiggling around. But this makes it much easier. This captures the most important effects and is much easier to handle in TSPA.

Similarly for the co-disposal, this can go even higher, and the time at which it jumps varies depending on the rate of steel corrosion and the rate of glass corrosion.

This is just a plot of the corrosion rates for the three kinds of matrix we had in the PA, and these are all quite conservative. The DSNF, we used a constant rate which was equal to the fastest rate observed for the uranium metal dissolution rates. And then here is the commercial spent fuel. It's very similar to the TSPA rate. It's a function of pH. And here is the high-level waste glass, which is very similar to the TSPA/VA rates. Also, a function of pH.

You can see also this is versus 1/T, that the high-level waste glass is more temperature dependent than the commercial spent fuel.

This shows the uncertainty that was actually used in the PA for the glass dissolution rate. I showed you the nominal case, but each of the terms in the equation actually had significant uncertainty, and this broad uncertainty is due to the three terms. The forward dissolution rate had about an order of magnitude uncertainty. The pH dependence term had about a half an order of magnitude dependency, and the activation term had about two orders of magnitude uncertainty. So we had quite a large range of glass corrosion rate.

For the cladding, this is a more complicated model, and there were quite a few assumptions. First of all, we broke the degradation of cladding into two components, two steps, the perforation step and then the unzipping step. Quite a few perforation mechanisms were included. It says four here, but there's actually more than that. We have the initial perforations that occur in the reactor and in transportation. Then we have the type that occur quite early, the creep, which could happen during storage and transport, or during the early heat-up period of the repository. We have stress corrosion cracking that can occur on the inside of the clad before any water gets in there.

And then we have what happens later on when water interacts, we have the localized corrosion, and this we have as a function of seepage into the package where you can get aggressive species like fluorine and chlorine into the package. So that doesn't really kick in until 40,000 years at the earliest.

Then we also have a seismic factor where the very extremely rare earthquakes that happen ten to the minus six per year are strong enough to just rattle that package enough that we assume that all the clads have cracks in them and start to unzip.

And after we have the perforation, we then release the radionuclides in two steps. There's the fast release fraction, which is the gap fraction where cesium, it's about 1 per cent, and for iodine it's about 4 per cent. And then we also release the fraction of the rod that dissolves before the unzipping would occur.

When you have the perforation, you have a porous matrix inside the cladding, it takes a while for those surfaces to react, and then they'll fill up a lot of the porosity within that package. Once they fill up that porosity, they start to exert pressure on the clad and start to open it up, unzip it. And during that period, we assume that all radionuclides that reacted on those surfaces would be released at that time, and that ranges from about 0 to .4 per cent of all the radionuclides. So that's the fast release fraction.

Then at the unzipping step, we assume that to occur between 1 and 240 times faster than the CSNF dissolution rate. This is, as we say, it's assumed here, it's because we haven't seen unzipping in a wet situation or environment type humid situation below 100 degrees. But we do have dry unzipping at higher temperatures that we use by analogy, and we have zircaloy properties, and so we made the judgment that it would unzip between 1 and 240 times faster than the forward dissolution rate.

Finally, the inventory was assumed to be released as the clad unzipped. If the clads one-tenth unzipped, we assumed that one-tenth of the radionuclides have been liberated from the matrix and available to be dissolved or reprecipitated as required. And except for the fast release, it just means that we've already liberated that right at the beginning.

This shows the actual performance for a given run, which was Bin 4, which is one of the infiltration bins, the infiltration bin that had the most packages and average infiltration scenario. And this shows versus the function of regular time. This is not post-waste package breach. This is normal time. This is the amount of clad that has perforated, and what we can see here is that it shows about 8 per cent at early time, and then as seepage comes in, we start to get breach of other rods from localized corrosion.

If you look at the range of calculations behind this average one, the creep, which was the major contributor, ranged from about 2 per cent to about 16 per cent.

Okay, the unzipping rate is shown here, and you can see it ranges from about 800 years to unzip a rod to over 100,000 years to unzip a rod, quite a large uncertainty. And this uncertainty comes from several effects. First of all, the uncertainty in pH gives some of this uncertainty, the uncertainty in the matrix dissolution rate, which is about one order of magnitude, and the uncertainty in the unzipping rate multiplier, that 1 to 240 multiplier.

So we have quite a large range for the unzipping, and actually that does turn out to be one of the important factors later on.

Solubility component. We made quite a few conservative assumptions. First of all, we selected pure phases only to control the solubility. In other words, we neglected co-precipitation or solid solution. We also neglected sorption. And then we conservatively fixed the gas pressures for the calculations we ran. For C02, it was 10 to the minus 3 atmospheres, and for oxygen, it was atmospheric.

Here's some of the actual abstracted solubilities used in the TSPA. We had several types of calculations. For some elements, we had distributions. For instance, for plutonium, we used an amorphous plutonium hydroxide phase to control our solubility, and we ran it under a range of chemistries predicted by the chemistry model, and what we got is this broad range of solubility. Notice that the range is broader than before, but the mean is about the same as 93 in the VA.

Similarly, we did that for protactinium and lead. Then for the elements that we had a lot more information on, we derived empirical functions where we determined solubility is a function of pH or CO2 or temperature. And for neptunium, I'm going to show you that in the next slide, it ranged from about 10 to the minus 1 to 10 to the minus 7 molar. The same thing for americium and uranium, about 10 to the minus 4 to 10 to the minus 7.

Finally, we had the elements where there were not many good controlling solids in the database, and they're quite soluble. So we just used one molar as upper limit, and that, in effect, makes it inventory limited in our calculations.

All these calculations that were done were done with an EQ3/6 with a new database that was based on recent NEA data and literature. That database was to be verified when it was run, and it should be qualified within the next week or so.

Here's, it's a little bit busy, but this shows you what we did with Neptunium, one of the most important elements. The red boxes here are actual data. They're from under-saturation by Efurd, et al. And that data was used to adjust thermodynamic database. We then used that database to run calculations at these blue triangles. That's the calculations we got. And then a line was fit, and that's the abstracted function for the TSPA, is that line that was fit.

Well, how does this function compare with actual molarity that is used in the PAs? Over here, we can see 1995 had this range, and the TSPA/VA had this range. Well, in this calculation, we have two time periods, the early 1000 year time period post-package breach, and then the remaining time period from that pH plot I showed you before. And what we see here is that at early times, the pH is quite low, it's acid, and we have this range here for the Neptunium solubility, 10 to the minus 3 to 10 to the minus 1, very high solubility. And for high-level waste glass it's similarly quite high solubility. But at later time when the pH has become more neutral, the solubility drops quite a bit.

Still, all these, the full range from here to here is not that much different from the bottom of TSPA/VA to the top of TSPA-95. The only real big difference is that in the very acid regions, we've gone to significantly higher solubility. But that only lasts for a thousand years after breach in the CSNF.

This shows the uncertainty of the solubility of Neptunium in the actuals runs, and you can see looking between here and the pH, that the uncertainty in pH is what's determining the uncertainty in the solubility. We have no additional uncertainty terms in our equations. The equations were direct deterministic from the pH. And as I said before, we assume pure phases. We assume a pure phased control, and there were a lot of things that could make the solubilities be lower than what we have. So the real uncertainty would include lower solubilities as well, but given our conservative assumptions, this is the uncertainty range in the PA.

This is the colloid model, and there's quite a few pieces to the colloid model. As shown in this cartoon here, this is your backup Slide 30, and this was done by Hans Pakenbooth (phonetic). Basically, this shows how the in-package chemistry affects the ionic strength and the pH of the system. And the three kinds of colloids have a different stability, depending on the pH and the ionic strength. And so in this part, it's determining the concentration of colloids as a function of chemistry, which is this first bullet here.

The second bullet is irreversible colloids versus the reversible colloids. We had two types of attachment of radionuclides onto colloids. We had irreversible, which is what we see in the Argonne tests where as glass dissolves and it makes clay colloids, there are discrete phases of actinide bearing phases such as thorium phosphate where all the actinide is in these discrete phases. They co-precipitate with the clay and then settle out, or it gets transported. We believe that those are irreversibly attached. It's not a simple desorption that would remove them from the colloid, and that's what the irreversible colloids are.

For reversible, for any colloid, clay or iron oxide or other groundwater colloids, if you have dissolved radionuclide, they can attach and sorb onto the colloid, or detach.

As you can see here, for the irreversible, the attached plutonium and americium onto the high-level waste,

waste form colloids were used, and that was from the experiments we saw.

Then for reversible sorption, we had a larger range of elements, because there's quite a bit of experiments on the sorption of these elements onto the various materials. We conservatively left out any filtration or sorption within the package, although that is somewhat counted in the concentration. For the concentration, we have the maximum mobile concentration. If you go above that, colloids tend to coagulate and settle out. But once that happens, we do not allow them to be filtered any more, or sorbed onto the stationary materials.

And then for diffusion coefficient, we used what we feel is very conservative. It was only 100 times slower than free water diffusion. And that would be true only for the very smallest colloids. Most colloids would probably diffuse 1,000 times slower than free water, which is what we used in the VA.

Okay, that was all the assumption section, and now we're into just pretty much results. And one of the first things that they noticed in PA was that most of the release is coming from the commercial spent fuel, as it had in all of our previous PAs. This is the base case, the black, and then they just cancelled out the co-disposal inventory or the commercial inventory. When they cancelled out the commercial inventory, it dropped down to here. When they cancelled out the co-disposal, it dropped hardly at all.

Here is the barrier performance for the cladding. I don't know if you can read it well. The degraded barrier is the 95th of the unzipping velocity, 95th of the matrix dissolution rate, the 95th of the initial failure uncertainty. And I believe that includes the creep uncertainty, which was that 2 to 16 per cent, and the 95th of the clad localized corrosion rate uncertainty.

That's the degraded, and then there's the enhanced is the opposite. You can see there's only about a four-fold change in these. And I believe what we're seeing here is that the creep, the amount that's failed at early time by creep, which is about 8 per cent, goes up to 16 per cent, which is only two times higher. And it goes down to two, which is only four times higher. So that's what we're pretty much seeing here, is the effect of how much we assume has failed by creep right away.

There is another slide, but it's not in this packet, where cladding actually just all of it failed at original time, and it's about an order of magnitude higher than the base case, which makes sense. The base case has about 8 per cent failed, and with 100 per cent failed, that's about an order of magnitude higher.

NELSON: Can I ask a question? Nelson, Board.

What is the time scale here relative to the time scale that you had showed before regarding waste packages?

STOCKMAN: This is the real time scale. This is not relative to first breach. Now, I have a mix throughout, so on each one, you have to remind yourself to look carefully to see.

This is the dose to the accessible environment. And the reason we don't have any dose up here is there's no waste packages failed at that point. And in this period of time right here, it's mostly diffusion, and then finally seepage gets into the package, and this is diffusion and evection out here.

NELSON: Thank you.

STOCKMAN: Now, this one, it's a little bit mislabeled, and it's a little bit difficult one to convey. The problem is we wanted to show the barrier for the radionuclide concentration, the barrier analysis for that. Well, radionuclide concentration is of some of the solubility and the colloidal radionuclide concentration, but those things aren't input parameters to be sampled at the 5 and 95. Their output is a function of the pH. So when they did this run, what they did was in the invert, they set the colloid stability to be the maximum concentration for colloids, and then they set the Kds for colloids at their 95th.

But for solubilities, they couldn't set that to

95th, so what they did is they used the solubility based on the pH in the package as opposed to the solubility based on the pH in the invert. And in the package, the pH is a little lower from the acid from the steel, and so the Neptunium solubility is a little higher. That's why there's almost no change here.

This one I could talk, and I have five minutes, but I could talk for quite a long time on this one. I'll try to hit the salient points, and maybe you can ask more questions this afternoon.

First of all, the most important thing to say here is that colloids are not a big deal. They're an order of magnitude less than non-colloidal release. And this is release from the EBS. These are complicated partly because there is a release from the waste package, and then there's release from the EBS, and where the limiting step is is not quite clear in this, and we're going back and looking at those results and should be able to give you more detail on that soon.

But what you see here is that there's quite a bit of Plutonium-239 release, even as soon as waste packages are breached. And this is diffusive release, and I believe that this diffusive release is not necessarily that of plutonium. It may be its parent. Plutonium-239 comes from Americium-243, and in these calculations, Americium-243 can go up to 10 to the minus 1 molar.

So it may be that what we see is diffusion of americium from the package into the invert, where it then decays to Plutonium-239, and then travels more as dissolved Plutonium-239. So that's the first thing, is the total release.

Then we have the reversible release, which is this blue line, and you can see that that happens, it's quite a bit lower than the dissolved, which is probably due to the lower diffusion coefficient of the colloids. And then there's the irreversible colloids here which start when the seepage starts, partly because these are just travelling and they have to diffuse, whereas, the reversible, it's in equilibrium with the dissolved, so it could be dissolved travelled a little, and then become colloidal and then stick and travel slower, and then redissolve and travel a little further. That's why the reversible make it out before the irreversible, which are just moving along as themselves only.

Then for the source of the reversible colloids, we have the three types of colloids, the waste form, the groundwater, and the iron oxides. And we can see that the waste form is dominant. The groundwater is next, and the iron oxides is the lowest. These are based on quite conservative Kds, I believe, and quite conservative concentrations. And even so, they are much lower than total plutonium release.

So we believe with our very conservative colloid model, we've pretty much put it to rest, that it's not going to be a major deal.

One thing you might notice, if I'm not out of time completely, is that the black line here, the waste form colloids, is the same as the blue line here. This is the reversible colloids. Which is basically saying that these waste form colloids that are making it out are the reversible ones, and the irreversible ones, which would be quite a bit lower, and I believe that this is a very conservative model where we have in reality when we look at the experiments at Argonne, the colloids are irreversibly attached, and from that, we were able to get concentration of colloids.

Well, we then took Kds for that type of material, clay, and said that's the Kc of that would be about a thousand. So we have reversibly attached about a thousand times more plutonium than what we actually measured. So that's quite conservative, and that's what we're seeing here.

And I think that's all I have time for. Any questions?

CRAIG: Thank you, Christine. We're just about out of time, so we'll take only emergency type. Jerry?

COHON: Cohon, Board. It can't wait until this afternoon because I'll be even more confused by then.

I don't understand this last curve, last presentation, or what you said about it, or what you concluded about it. First of all, which dose release curve does this release rate curve correspond to?

STOCKMAN: Well, this is actually the release in grams per year from the EBS.

COHON: I understand that. But isn't there some release curve, dose curve that this--some case this comes from? Is this the nominal case?

STOCKMAN: I believe this is the nominal case, and maybe Bob can help me out on that. It's the mean case? The mean of the 300 runs.

COHON: Doesn't the blue line and the red line contribute somehow in some additive sense to the black line?

STOCKMAN: Yes.

COHON: Then how could you say that they don't matter very much? They're a very large fraction of the total release after 30,000 years.

STOCKMAN: Well, they're about an order of magnitude lower. So they're only 10 per cent, or so.

COHON: What does it look like past 100,000 years? Did you go that far?

STOCKMAN: I don't have that plot.

COHON: And I missed something. I must have missed something from yesterday. You said seepage doesn't start until it looks like 30,000 years, 40,000 years?

STOCKMAN: Yeah, about 40,000 years.

COHON: Why?

STOCKMAN: I'd have to ask Bob that. I believe what it is is the stress corrosion cracking lets water in, lets water vapor and water in.

COHON: They said drip shield.

STOCKMAN: Drip shield will not let actual seepage in. So what you're getting is water vapor getting into the package, condensing and forming a diffusive connection to the outside world, so you can have diffusive release.

COHON: Finally--well, actually, the other question can wait until this afternoon.

CRAIG: Don?

RUNNELLS: Don Runnells, Board. Could you refer back to Slide Number 5? When you introduced that slide, you said that in comparing the variability of the pH for CSNF to that of co-disposal in the PA, we see these results. But in the actual process model, the variability was reversed. if you could explain that to me, I might be able to understand a little better how we use the process models to get into the PA. What happened that in the PA, the variability was reversed from what you observed in the process model?

STOCKMAN: Several things happened. One is that in order to put it into PA, we needed to make it into discrete time periods after waste package breach. And if you looked at the process model version of this, you would see, for instance, here that the time period when it goes up to this average ranged quite a ways. So if you looked at the plot, it would be just a very--it would be a horse tail plot. And that's just the uncertainty in the time between the two.

Whereas, for the PA, since we only had two times, the second time is the average for this period. And if you did get up here, then the average would be right in this area. So it was the way we just discretized the problem as we put it into TSPA. We probably could have made three time periods and we would have seen a little more of that uncertainty of the jump between the two modes, and that may have been doable, but that kind of complexity is difficult to put into the TSPA. We certainly could not have, for each run, have a time dependent pH. It would just be too complex for the code.

RUNNELLS: Thank you.

CRAIG: Bullen promises to be brief.

BULLEN: Bullen, Board. On Figure 9, this is an indication that 8 per cent of the cladding has perforations from 1,000 years and beyond. What fraction of cladding is failed at emplacement?

STOCKMAN: It's between .1 and 1.

BULLEN: So .1 and 1 of the fuel rods in every package is failed?

STOCKMAN: Yes.

BULLEN: Why don't we find those and put them all in one package? Why do we have to agglomerate it? And this was a problem in VA, because we have a couple of percent that were failed, so any waste package had immediate release. And if you want to really take clad cut, why don't you at least do the math and the inventory so you can take clad cut.

STOCKMAN: Well, in this run, this is a run where it was of normal CSNF. It wasn't the stainless steel clad, which in VA, as you remember, we put stainless steel in each of them.

BULLEN: In every package; right.

STOCKMAN: We didn't do it this time.

BULLEN: Okay. So you separated it. But you still have failed fuel?

STOCKMAN: We still had some failed fuel. I could look up in my notes. It's about .1 per cent or 1 per cent.

BULLEN: The last question is that when you did the unzipping, when you take a look at the kinetics of the transition from UO2 to U308, that's temperature dependent?

STOCKMAN: Uh-huh.

BULLEN: If the packages were cooler or the cladding never got to that temperature, would you see that temperature dependence in your calculations, and would you have a significantly less transformation rate, a significantly lower transformation rate?

STOCKMAN: In our unzipping, we're assuming it's going to metashopyte, because it's in less than 100 degrees, and it's in high relative humidity. So we're assuming that there is condensation of water, and we're going from UO2 to metashophyte.

BULLEN: Oh, okay. So you're not going all the way to U308 right away.

STOCKMAN: No, we're not going to U308 at all.

BULLEN: Okay, thank you.

CRAIG: Thank you very much.

KNOPMAN: Just related to this, can I ask one quick question? Thank you.

Knopman, Board. Could you just quickly explain why, for the always drip case, you would have less cladding perforated than with the intermittent drip?

STOCKMAN: That's a good question. The reason why is because the always drip case actually has lower flow than the intermittent drip case.

CRAIG: Okay, thank you. Our next speaker is Bruce Robinson from Los Alamos. Bruce has a Ph.D. in chemical engineering from MIT. He leads a team of hydrologists at Los Alamos, and he's going to talk to us about the saturated zone.

ROBINSON: Good morning. I'm pleased to be able to report on the saturated zone flow and transport modeling, both from a process model point of view and also the TSPA abstractions.

Now, the model is significantly different than the TSPA abstraction in the VA, and so I'm going to spend some time on the process model as well to give you a good picture of how we're using the process model and abstracting it to perform the radionuclide calculations.

This is a slide that many of us have been showing, showing basically the model being talked about, and also boiling down to the input parameters that wind up in the TSPA calculation. We're talking about saturated zone radionuclide transport, which involves elements of flow in the saturated zone, and also transport processes of radionuclides as they travel through the volcanic tuffs and the alluvial valley fill.

So we have basically as the output of the process model, breakthrough curves. The transport time and breakthrough curve of different radionuclides that are released at the repository level at the saturated zone, the breakthrough curve meaning the concentration versus time that would be arriving at a compliance boundary, the 20 kilometer boundary. Those depend on the sort of flow processes that I'll be describing, including the flux in the saturated zone, where you put the radionuclides into the saturated zone, which is tied to the unsaturated zone modeling, the flow fields themselves, which are controlled by fluxes and permeabilities in the aquifer.

And then you get into some transport processes in addition to the flow processes. In order to describe each of these to you and how they influence things, I'll have to get into some detail on the process model itself for radionuclide transport, and I'll be doing that in this talk. Finally, there are some colloid transport models and processes in the saturated zone flow and transport model as well.

Radionuclides that are released from the near field waste package and engineered barriers, and percolate through the unsaturated zone via the unsaturated zone flow and transport model arrive eventually at the water table, and they are carried in the saturated zone with the flow field that is predicted to occur in the saturated zone, down to a downstream location, where then at a given concentration utilizes that water at a given concentration, and that's where the biosphere modeling takes place.

So the input to this model is the output of the unsaturated zone flow and transport model. The modeling itself predicts the concentration versus time history at the compliance boundary, which is then picked up by the biosphere component.

This is a schematic which shows the key transport processes that are in the conceptual model for the saturated zone. Large scale flow and transport is governed by the flow field that's predicted using the process model, and so that transport occurs along the flow paths of the saturated zone down to the model predicting the Armargosa Valley as being the ultimate arrival point at a 20 kilometer boundary.

You've got processes occurring at a variety of scales which are going to control the rate of movement of radionuclides in the saturated zone.

Let's go from larger scale to smallest. On the large scale, we have dispersion, both longitudinally along the flow path, and also transverse to the direction of flow. And those are processes which would tend to spread out in the aquifer the radionuclides, so that even if it's a point source beneath the potential repository, you will have a spread-out distribution of concentrations downstream.

Going to smaller scales now, we have sort of a dual system, with fractured volcanic tuffs comprising the transport pathway for perhaps the majority of the flow path length, and this medium would be characterized by an effective porosity that would be governed by the flowing fractures.

So of the entire amount of rock available for transport, water is travelling through the fractures, and that comprises only a small fraction of the total volume of that rock. That implies shorter groundwater travel times if nothing else was occurring in these fractured volcanics. However, as you go to smaller scales, in addition to advection in the fractures, matrix diffusion will occur. These are processes that have been determined experimentally at various field sites, including at the C-well site at Yucca Mountain, and at the present, in the process model. Sorption also can occur for radionuclides that diffuse into the rock matrix in the volcanics.

When you get down to the alluvium valley fill units, a porous medium approach is taken in the modeling. That would give you a larger effective porosity than the fractured medium case, and perhaps longer groundwater travel times. But we know sort of from the first principles and lots of observations around the world that we're going to have preferential flow paths within that system as well. And so that's accounted for in the model through the distribution of the porosity that's used for this medium. So those are the key elements that we want to capture in our calculations.

This slide outlines our general approach for the transport abstraction that's used in TSPA/SR. We're using the saturated zone site scale flow and transport model directly to simulate radionuclide mass transport, and that transport occurs to the 20 kilometer compliance boundary from four source regions that are taken based on where the radionuclide mass is predicted to reach the water table from the unsaturated zone modeling. So that forms our choice on how we place radionuclides in the saturated zone model, and then the saturated zone model itself takes over, and the calculation occurs within the saturated zone.

We use a particle tracking model within the three dimensional flow and transport model to generate breakthrough curves of radionuclides. Those are carried out using the process model, and a catalog of these breakthrough curves are provided to the TSPA calculation, and we use the convolution integral method, really an expedient to speed up the calculations and allow us to do these calculations beforehand, so that the TSPA calculations themselves can just draw from this catalog of breakthrough curves. And so that's how that is done.

Then for concentrations, the radionuclide concentration is gotten from this breakthrough curve at the compliance boundary by dividing the radionuclide mass flux that crosses the boundary by the average annual groundwater usage of the hypothetical farming community.

So we're taking the radionuclides that reach the compliance boundary, no matter if they're spread out or very compact, and we are mixing that in an average groundwater usage of this hypothetical farming community to come up with the concentration that's then used in the dose calculations.

A couple other elements. Climate change is incorporated on the fly in the TSPA calculations by scaling the mass breakthrough curves in proportion to the changes in the saturated zone flux. So the assumption there is that climate change increases or decreases the velocity of movement of the radionuclides, but doesn't change the flow paths themselves.

That's a limiting assumption, but nonetheless, it's one that I think is valid based on some of the other uncertainties in the modeling, and one that allows us to fairly simply incorporate climate change.

Finally, there are some radionuclides which are not amenable to this entire approach, and those are the ones that undergo decayed chains where you have to track the entire chain. And so in addition to all of this approach that I described here, there's an abstracted 1-D transport model to handle the decayed chains.

I wanted to discuss how that approach differed from what we did in the viability assessment to give you a picture of where we've come from the VA.

The key difference I think is that the three dimensional SZ site-scale flow and transport model is being used directly as opposed to a more stylized one dimensional streamtube approach that was used in the TSPA/VA.

For concentration, in the VA, we assumed the concentration within that stream in situ to be the concentration of interest. Now we're using the approach of taking the mass flux at the boundary and applying this mixing within the water drawn from the aquifer by the hypothetical farming community.

Other aspects of the modeling that's different is that some of the processes, including matrix diffusion, are explicitly simulated in these calculations as opposed to simply using an effective porosity to capture all of that detail. So I think we've got additional detail warranted by the data that's been collected, say, at the C-wells to be able to include matrix diffusion as a process.

The particle tracking method, as I mentioned, is what we're using to actually carry out the calculations. That's contrasted to a finite element 1-D transport within the streamtubes that was used in the VA.

And then finally, in the area of data and differences in the parameterization of the model, there is now minor sorption of technetium and iodine in the alluvium based on data that was collected from material from one of the alluvial wells drilled by Nye County. There was no sorption of those elements in TSPA/VA.

This describes the site scale flow and transport model. I'm going to spend a couple slides telling you about that model in preparation for showing you some radionuclide transport results. It's a three dimensional model using FEHM software code, and its dimensions are 30 by 45 kilometers, and almost 3,000 meters below the water table.

It's based on a hydrogeologic framework model that's consistent with the unsaturated zone and other geologic modeling that's occurred within the area that that model exists, but then the hydrogeologic framework model for this model also extends out beyond that. So a new effort was undertaken in the last few years to come up with that geologic and hydrogeologic description.

Grid spacings of about 500 meters in the horizontal X and Y directions, and a variable resolution of from 10 meters to about 50 meters in the vertical direction is sort of the basics of the numerical grid. The model is calibrated, and I'll talk about the data that's used in that calibration in a moment. It's calibrated in automatic inversion in which a commercial software package, PEST, is used to adjust the parameters, and you zero in on a best fit, using techniques that are used in that sort of an automated inversion process.

Now, the calibration itself and the subsequent I'll call it validation, but it's really cross-checking with other types of information is what I'll describe in a couple of slides here. The basic calibration targets are water level measurements in wells, and there was also targets of simulated groundwater fluxes at the lateral boundaries. We want to be able to capture the head distribution, but in order to get travel times accurate, that's not enough. One has to also try to anchor this model based on what we think the groundwater flux through this portion of the basin is, and that's done through looking at the regional scale modeling and applying those results to our site scale model. I'll show you that in a second.

We've also got I'll call it softer data. We infer flow paths from hydrochemical data. We want to make sure that features of groundwater system that we think are important, such as a upward hydraulic gradient from the carbonate aquifer, are captured in the model. And also in the process of calibration, we set ranges for what we think the permeabilities of these various units can be based on measurements, and we make sure those are honored in the calibration process.

And then finally, estimates that have been made for the specific discharge in the volcanic aquifer, we've done a cross-check of the modeling to make sure that that specific discharge is falling within an appropriate range.

These are the well data used in the flow and transport model calibration. There's 115 water-level measurements used to calibrate the model. That includes these red dots, which are the Nye County well drilling program. That includes six water-level measurements from Nye County.

The solid red dots are completed wells, and the ones that are the open ones are planned, and these are in progress. So we're continuously updating the model, filling in an important data gap that we had, and that's sort of hampered the ability of us to really come up with a good description of the groundwater system here, and that data is really paying dividends.

Another way that it's paying dividends is that we're carrying out sorption tests and have done that in the last year or so from samples in the alluvium from three Nye County holes, and determined the sorption, though small, is, we think, non-zero for technetium and iodine.

And as I said, the ongoing work in the Nye County drilling program is continuing to add information to fee this model.

In addition to matching water levels, one needs to, as I say, anchor this model in with some estimates of what we think the flux through this region is. And we used the regional scale modeling that was carried out several years ago in the project by Frank D'Agnese and Associates. We used that as a calibration target so that we make sure that that modeling at the regional scale is consistent with the modeling that we're carrying out here.

This is a site scale model domain split up into several regions in which we use some of these as calibration targets, and other just as a cross-check, a comparison between the regional model fluxes and the site scale model fluxes.

In the site scale modeling, we're fixing heads on the outer boundaries, so we're not actually plugging in the flux from the regional modeling, and there are good reasons for that related to different model formulations of those two models, regional versus site scale, that require us to do something not quite as formal as simply taking a flux from a regional model and plugging it right into this model. But what we're doing here is comparing fluxes from the regional model with the site scale fluxes.

There are several good reasons why these numbers wouldn't agree exactly, but in a general sense, if you look at, for example, the south boundary, the amount of water passing through this boundary here in the site scale model is of the same magnitude as the regional scale model result. And this is kind of the level that we're comparing these models and making sure that they're consistent. There are very good reasons why, for example, W1 wouldn't necessarily agree exactly between the regional and site scale models. But on a gross sense, I think the fluxes computed from the site scale model agree with the regional model, and I'm saying to within the accuracy warranted by this sort of a comparison.

KNOPMAN: Excuse me. Why do you have kilograms per second for flux?

ROBINSON: Well, that is--you know, that's a flow rate of water over the entire depth in the Z direction of this line right here. So it's a three dimensional model. You've got a given depth of this model, and we take the water flow rate that's entering along the face of each of these.

KNOPMAN: I just meant as opposed to volume. Why are you using a weight per second?

ROBINSON: Well, that's kind of the fundamental--you know, mass is conserved, not volume. So, you know, when you get into, for example, density variations with temperature, it's--all codes basically at the core of a flow code, you're modeling mass fluxes, not volumetric fluxes.

Hydrochemistry information is used to constrain the flow model as well, and what we're assuming here is that we can take trends in the chemical data and use those to delineate large scale features in the groundwater flow paths. And this diagram shows some flow paths which have been discerned from not just the chloride concentration, which is what's depicted on this slide, but also species such as isotopes and other major iron chemistry to really map out where we think on a large scale, the flow is going based on chemistry.

The way this works basically is that one tries to draw a flow line based on, say, low concentrations of chloride through this region of the model domain right here versus much higher concentrations, which kind of are bracketed by this flow path out here.

The flow model results that we obtained using a calculation of particle tracking are consistent with the flow patterns that we are deducing and sort of just drawing on the map in this type of a diagram. They're in qualitative agreement in the hydrochemical data, and that's how the hydrochemical data is kind of factored into the development of the flow model.

This is a flow and transport result of the model. This is the topography of the saturated zone model, and this is the predicted head distribution, the relief, the predicted head distribution within the model. The repository sits here, and the 20 kilometer boundary out here.

These are streamlines from various location release points beneath the repository to the 20 kilometer boundary. Transport in general is south and west, and then turns south along Forty Mile Wash, as predicted in the model.

The particle tracking method not only maps out flow streamlines, but also includes radionuclide transport processes in addition to advection, dispersion and matrix diffusion and sorption as well. What you're looking at here are streamlines of only the advective component of that, just to show you the general shape of the plume that's predicted from points downgradient from the repository.

In the third dimension, the Z dimension, the flow paths in the repository occur within the upper few 100 meters of the saturated zone. This is a consequence of the upward gradient that's captured in the model. And the 20 kilometer fence in this model, the prediction is that the 20 kilometer fence, the flow paths cross about five kilometers west of the town of Armargosa Valley.

Getting to the uncertainty of the transport predictions, we've got flow and transport parameters that are variable and stochastically generated in the model. For flow, there are three discrete cases of groundwater flux that are used, and probabilities are based on expert elicitation results for that.

There's an anisotropic and an isotropic permeability in the volcanic units, which turns out doesn't matter too much to the predictions, but it's included because it was brought up as an issue of concern during the development of the model.

There is uncertainty in the alluvial, transition between the volcanic and the alluvial zone, and to capture that uncertainty, we have a variable size of that alluvial unit. I'll get to that in the next slide. But it's an important uncertainty that we've captured. It's a hydrogeologic uncertainty based on the current data.

Then you've got the pure transport parameters that basically affect the matrix diffusion model and also the sorption model in the volcanic units and also in the alluvium. And then finally, there are some colloid parameters that come out of the way that we're modeling colloids, basically as two separate entities. One where the radionuclide is irreversibly attached to colloids, and then another in which there's a reversible attachment/detachment type model for the colloids.

This is the alluvial uncertainty zone. Like I say, we don't know exactly where this zone goes from the alluvium to volcanic, and that's an important parameter because in the alluvium, we expect longer travel times and so, therefore, by varying essentially this line in the east/west direction, we capture that uncertainty.

What that boils down to is that based on the flow paths from the repository to the 20 kilometer point, the flow path length in the alluvium varies from about 1 to 9 kilometers, and that's a significant uncertainty.

This is an example result. It's Neptunium-237, which if you recall from Bob Andrews' talk yesterday, was one of the key radionuclides out to the 100,000 year time of a simulation. These are all the simulations capturing all the uncertainty in flow and transport parameters in the saturated zone, and these are breakthrough curves where zero is the time that a radionuclide reaches the water table, and the breakthrough to one means that it's all reached the compliance boundary at a given time.

The travel times are shown in a histogram form here, down here, and about half of those realizations of neptunium exhibited median travel times, the 50 per cent breakthrough time of greater than 10,000 years, and the other half, less than 10,000 years.

I'd like to show how that plays out in terms of the behavior of the saturated zone in terms of the degraded behavior versus the enhanced behavior. Some of the other presentations have looked at this.

For the degraded behavior, we're taking the 95th percentile for all of the SZ flow and transport parameters, but only a few of them really matter, as I'll show in a second. For the enhanced behavior, the 5th percentile.

This was the plot I had previously, and I think it goes a long way toward explaining the results here. This is dose rate versus time for the base, called the base case here. We were calling it the nominal case as well. The degraded SZ flow and transport barrier is almost identical to the base or nominal case, and that's because when you get into degraded behavior for, say, a neptunium, you're talking about travel times on the order of less than 1000 years. Well, that's no different in terms of performance from a median case of about several thousand to 10,000 years, because the only thing the saturated zone really is doing is displacing in time the time at which the mass arrives at the compliance boundary. And whether that's 1,000 or 10,000 years on a scale like this, really doesn't make any difference.

When you start to get into the enhanced SZ flow and transport barrier, you're talking about travel times up in the greater than 100,000 year range for something like neptunium. And so effectively what you're doing in this blue curve is you're taking neptunium out of the picture by saying that for the enhanced transport behavior, I've got travel times in excess of 100,000 years, and that's what this model is predicting for neptunium.

So when you take neptunium, one of the most important radionuclides, out of the dose rate, then you're only getting contributions from the less strongly sorbing radionuclides like iodine and technetium.

So, therefore, the enhanced behavior shows significant improvement, whereas, the degraded case was essentially the same as the nominal case.

The next slide is a summary, which I will allow you to read. And thank you very much.

CRAIG: Okay, critical questions? Don Runnells, go ahead.

RUNNELLS: Runnells, Board. What do you see as the most significant gaps in your I guess database for the model?

ROBINSON: There are several. The extent of the alluvial zone, which really controls--our knowledge of that really controls how much of the flow path occurs within the alluvium. We're on the road toward reducing that uncertainty with the drilling of new wells. But that's a key uncertainty.

The other, I think that in addition to analyses like this where you're taking an uncertain parameter and seeing how it affects the results, those are important, but I think conceptual model uncertainty is also important. And some of the testing that's going to be coming down the line, for example tracer testing in the alluvium to complement our tracer testing that occurred in the volcanic tuffs, is another area where I think the model uncertainty, and let me say the confidence that we have in these results will improve greatly when we have field evidence of transport in the alluvial system to complement what we've done at C-wells in the volcanics, as well as the areas.

CRAIG: We're going to have to move on. Thank you very much, Bruce. You've sure come a long way from TSPA/VA. Very impressive.

Our next speaker is John Schmitt, who will talk about the biosphere. John is the M&O Manager of the Biosphere Section in the Regulatory and Licensing Office of the Yucca Mountain Project. He has background in environmental health science and health physics, and some 27 years of experience in the nuclear industry, and your allotted time is 15 minutes. I'll warn you after ten.

SCHMITT: Thank you. I'm John Schmitt, and I have the privilege of presenting to you, and presenting to you the work of a very talented team who developed 15 analysis and model reports that are used to create the biosphere process model.

Finally, in this model, we hypothesized that the radioactive material escapes the system and interacts with people. Now, admit it, that's what you came here to hear about.

On this side, we see a table taken from the TSPA presentation of yesterday, which shows the biosphere component within the context of the TSPA. The biosphere provides the highlighted areas. We provide annual usage of groundwater and BDCS by radionuclide for 18 radionuclides, and then for an additional five radionuclides that support the million year calculations. And we do this for six prior irrigation periods to take a look at build-up, and that's for the nominal scenario class.

The BDCS that we provide, in biosphere, we do not provide the doses. The doses are calculated in the TSPA. In biosphere, we provide conversion factors, biosphere unique factors that allow us to convert from concentration coming from the SZ model, to calculate doses. So this is a conversion factor.

The units are millirem per year per picocurie per liter for the nominal scenario case by radionuclide. These conversion factors, biosphere dose conversion factors, are also usable for the human intrusion situation where effectively, you have down borehole contamination of the aquifer.

And for the volcanic eruptive case, biosphere provides to TSPA BDCS by radionuclide, and we provide soil removal information also. Here, the units for the biosphere dose conversion factors are millirem per year per picocurie per square meter of material deposited on the surface through the eruptive event.

And like the other process models, we perform explicit evaluation of FEPs to improve the defensibility of the TSPA to perform for the SR.

Discussion of the assumptions for the biosphere model should begin with recognition that the documents that we must comply with, DOE Guidance and the proposed EPA and

NRC regulations, provide substantial definition of the biosphere. This results in fewer assumptions in order to construct the biosphere of interest.

For example, central to modeling the biosphere are the critical receptor and their environment, and these are partially prescribed in the proposed regulations. The basis for doing this is discussed in the material for the proposed regulations, and two quotes are provided here from each of the regulatory agencies.

The premise is that one would define carefully selected applicable characteristics that can be reasonably bounded and that would otherwise be subject to unlimited speculation.

Another type of assumption used is methods to select values to represent the behaviors and characteristics of the receptor of interest. These are developed based on demographic survey information. Some of it direct from surveys that we did, and other of this information from demographic materials available that are applicable to the receptors of interest.

For the nominal scenario case, the sole contaminant considered is groundwater coming up through the water well, and this is done, and the basis for this assumption is in other process models preceding biosphere model, there were no other significant release pathways identified for licensed material entering the biosphere.

There was some discussion about what to call this scenario. In the biosphere area, we called this the groundwater contamination scenario for biosphere purposes only, and it is usable for undisturbed performance of the potential repository and for some disruptive events, such as seismic events and human intrusion.

For the volcanic eruptive scenario, we assumed that there was exposure during the volcanic event, that is, the population does not leave the area, they're exposed to the ash fall, and this is based on analogous experiences, and we also used increased air dust concentrations after the volcano. And in TSPA, we used quite conservative dust concentrations, and these are done, and the basis for this is that this is a reasonably conservative approach.

Regarding differences between the viability assessment and what we did this time in this PMR, and as it feeds the total system performance assessment for the site recommendation, these are two of the principal differences. The critical receptor is different this time. In the viability assessment, we assumed a rural residential farmer, whereas, this time, we're instructed by the regulations to use the average member of the critical group, and the reasonably maximally exposed individual.

For food ingestion, in the VA, we assumed that 50 per cent of the diet came from locally produced foods. Whereas, this time around, for the average member of the critical group in the RMEI, we are basing our food ingestion, local food ingestion, on the survey results that were obtained for people who live in Armargosa Valley. And, in fact, we found that people in the Valley who have gardens are more apt to eat additional quantities of locally produced food, and so we used the food ingestion values for that subset of the population in order to characterize the average member of the critical group in the RMEI.

Another difference, another two differences are shown here. In the VA, we did not take a look at radionuclide build-up in soil and removal of the contaminated soil. Whereas, this time around, we did model and incorporate those parameters. And for annual rainfall, in the case of the VA, we used current rainfall, and then applied a factor of two and three times more rainfall. In this case, this time, we used current rainfall. For the biosphere model only, we used current rainfall.

Okay, regarding sensitivity, in the process model report exercise, we did some sensitivity analyses and looked at quite a few things. But the principal intelligence that we were after was pathway, how much does pathway--which pathway is the most important. For the nominal scenario class, we found that ingestion accounts for essentially all of the contribution to the biosphere dose conversion factors. And, in fact, drinking water and leafy vegetables are the subgroups within that ingestion that contribute the most.

It was fairly consistent across the radionuclides that about 60 per cent of the contribution to the biosphere dose conversion factor was from drinking water, and about 35 per cent was from eating leafy vegetables. So that's a total of 95 per cent there.

The inhalation and external exposure were not significant, 1 to 3 per cent generally. So that left the remaining 2 to 4 per cent of the contribution to the biosphere dose conversion factor to be from the ingestion of other foods other than leafy vegetables. There were seven other food groups.

For the volcanic eruptive scenario, we found that soil ingestion and inhalation dominate for most radionuclides. This was less consistent across all the radionuclides, but in general terms, 20 to 75 per cent of the dose contribution to the biosphere dose conversion factor was due to soil ingestion, and 12 to 37 per cent was due to inhalation. Only in the case of Strontium 90 and Uranium 232 and 233 were the vegetables important.

In the TSPA, sensitivity analyses were done, and a degraded barrier like case was performed. The BDCFs of course are unrelated to barrier performance. But a 95th percentile situation is hypothesized, and the dose calculated to assess sensitivity, and a 5th percentile case is also run.

This figure provides insight into the sensitivity of the nominal scenario class dose rate to uncertainties in the values used for BDCFs. It compares the base case with the 95th and 5th percentile values being used. And the dose rate calculated using the 95th percentile values is approximately a factor of two higher than is the case for the mean dose rate.

This ends the prepared materials that I have. The Chairman is smiling. I'll entertain questions at the Chairman's discretion.

CRAIG: Thank you very, very much, John. That's right, we have ample time for questions. Go ahead, John Kessler.

KESSLER: The change in the receptor, are you now assuming that the critical group is 100 per cent consumption of all local produce, or are you still assuming some importation?

SCHMITT: Yes, some importation. We used an actual survey that we conducted to find out the dietary habits of the population, and we used that directly.

KESSLER: Okay.

SCHMITT: No assumptions. All directly out of the survey.

KESSLER: Okay. One thing you didn't talk about at all was dust resuspension from the volcanic ash thing. Maybe we should wait on that one, because I know that's one that's causing problems, but it's up to you.

CRAIG: That sounds like it might be a good one for this afternoon.

KESSLER: Okay.

SCHMITT: Very conservative, though, what we did.

CRAIG: Dan Bullen.

BULLEN: Bullen, Board. You say the primary pathway is leafy vegetables and drinking water?

SCHMITT: Yes.

BULLEN: When we were at Amargosa Valley, we saw a big dairy. Did you take a look at the milk pathway and its bio-accumulation, and the kind of doses you could get associated with that?

SCHMITT: Yes, we did. Iodine of course is a principal contributor to that pathway. I don't have on the tip of my tongue the values, but yes, we definitely looked at the milk pathway.

BULLEN: And it was less than 4 per cent? Because you've added all those up, so it's a small number? I guess I just find that surprising.

SCHMITT: Yes, it is a small number. Yes, here we go, milk, effectively zero values except for three radionuclides, Technetium 99, about an 8 per cent contribution, Iodine 129, about a 4 per cent contribution, and Cesium 137, about a 2 per cent contribution.

BULLEN: Okay, thank you.

CRAIG: Other questions? Debra Knopman?

KNOPMAN: Knopman, Board. Could you just clarify the assumptions about rainfall? You say now you're using current rainfall. What about your various climate scenarios that are used elsewhere?

SCHMITT: Right. As the other presentations for the other process models have indicated, they have used varying rainfall, you know, included in infiltration, and becomes important. The rainfall change, which is about four inches per year for those various scenarios that are envisioned for climate change, an additional four inches per year or so.

In the biosphere model, it would be of interest only insofar as it changes the exposure to contaminants. It's less central to the model than it is for some of the other models.

On the face of it, more rain could mean less irrigation with contaminated water, potentially contaminated groundwater, and it could mean greater leaching of contaminants out of the soil by the fresh water instead of the possibly contaminated groundwater. So we believe what we've got is a conservative scenario by assuming current rainfall.

CRAIG: Okay, thank you very much, John.

SCHMITT: Thank you.

CRAIG: Oh, I beg your pardon. Jeff Wong.

WONG: Jeff Wong, Board. Why does the soil pathway dominate for the volcanic disruptive event, soil ingestion?

SCHMITT: Right. Soil getting into the body by any mechanism, because here we've got, in that scenario, we've got contaminated ash on the ground, and at least only in the process, it's easy to envision this ash, this contaminated soil becoming airborne. And so quite a bit of that is from inadvertent soil ingestion or purposely eating soil. There are some people who do that. But also from inhaled material which eventually travels through the gut, and is contributed--or the ingestion pathway is what contributes.

So for the particles that are less than 10 microns in size, they will dose the longest, but the particles that are greater in size than that, up to about 100 microns, get caught in the passages and eventually passes through the gut.

WONG: So the irrigation or the groundwater pathway versus the volcanic atmospheric deposition pathway is just a greater source term? I mean, with time, as you have increased irrigation, still with time, the build-up in the soil will be less than that versus the volcanic pathway?

SCHMITT: It depends. Let me try to answer your question, and then help me to do it better.

In the volcanic scenario, we're looking at the pathways or the mechanisms for exposure to volcanic ash that is contaminated. We can assume or not that the groundwater is also contaminated, and then we can add what we did in the groundwater scenario to the volcanic scenario, if we want to assume that the groundwater is contaminated. But the groundwater is not contaminated at the point that the eruption occurs. The groundwater, and irrigating with the groundwater, actually has the effect of washing the contaminants that are in the ash down deeper into the soil and away from their ability to expose individuals in the environment.

Did that get the question?

WONG: I'm trying to understand, I think I do, the volcanic disruptive, that particular pathway provides a larger source term in soil than the irrigation, or from groundwater. I'm talking about soil build-up. And so, therefore, the ingestion pathway dominates in the volcanic scenario?

SCHMITT: The inhalation or soil ingestion.

WONG: Soil ingestion and inhalation.

SCHMITT: Right. Yes.

WONG: Okay.

SCHMITT: More so than eating foods that are grown in the ash. There's a much greater contribution from that inhalation pathway, which is another expression of soil ingestion, than is the case for ingesting foods that are grown in the contaminated ash.

WONG: Was there ever any consideration for the use of the manure from, like, the dairy farms, or if cattle were grown as a fertilizer for the crops, and then having the radionuclide recycled?

SCHMITT: No. No, we didn't do that, Jeff.

CRAIG: Okay, thank you, John.

SCHMITT: Thank you.

CRAIG: Our final speaker in this session on TSPA/SR components is Kathy Gaither from Sandia. She's Project Lead on the disruptive events process model report. She's a geologist by training, with over 20 years experience, including ten years at Sandia working on nuclear waste and environmental restoration projects. She'll talk about disruptive events.

GAITHER: Hello. I'm Kathy Gaither. The disruptive events PMR group of analyses is performed by quite a few people. I'll be representing their work here today.

The goals of the presentation are to describe disruptive events analysis for TSPA/SR. Our group of analyses are a little bit different than the others, in that we focused on developing conceptual models and constraining processes, and recommending groups of parameters that could help conceptualize these models. Abstraction took place more in the PA arena, so you won't see as much presentation of lists of parameter values and abstraction processes. Again, we were conceptualizing processes in this area.

We looked at two large groups of geologic processes, seismicity and structural deformation. The framework for most of our analyses was features, events and processes examination. These features, events and processes were a subset of the large FEPs database for the project. The distribution of the processes we were to look at occurred through interactions in workshops early in 1999. And I will present the lists of some of the primary FEPs so that you can see the types of things that we looked at.

The second group, large group of analyses, was in the area of volcanism. I'm going to describe the TSPA/SR treatment of volcanism and present dose results for volcanic events. I saved the sensitivity analyses for back-up slides in the interest of time, but those are in there for quite a few of the process model factors.

These are the process model factors introduced by Bob Andrews yesterday. I'm presenting the ones, of course, related to disruptive events. There are three process model factors here; seismic activity in which we look at the probability of seismicity and structural deformation.

In the volcanic release area, we look at the annual probability of igneous intrusion, atmospheric transport parameters, the probability that an intrusion will result in one or more eruptive events, or volcanoes, and the number of events that would intersect the repository.

We also recommended to PA win direction, wind speed factors. The biosphere dose conversion factors come into this analysis, but as you just saw in the presentation by Mr. Schmitt, that's in another group of analyses. And the factor to account for radionuclide removal from the soil is also in the biosphere group of analyses.

We looked at the intrusive indirect release, annual probability of igneous intrusion, this is the groundwater pathway, and the number of waste packages damaged by intrusion. You'll see sensitivity analyses for this list here in the back-up slide.

I'll start talking about the group of analyses we call seismicity and structural deformation. In the area of seismicity, the primary geologic consequence of concern is vibratory ground motion. In the area of structural deformation, we look at fault displacement effects.

We examined three primary features, events and processes in this area. Some of those will be presented on my next slide. The general topics of analysis are the areas of tectonics, seismicity, fractures, faulting, and hydrologic effects. You'll see a lot of these are overlaping, and there's some discretization of looking at these. However, we always make sure that they cross-map well to each other and that we've had consistent assumptions.

In other words, tectonics is a pretty big topic, and we've broken it down into looking at faulting and seismicity as subsets of that.

I'm going to discuss the general conclusions with the next viewgraph, but this is a summary of the conclusion in three big areas that we looked at. You should know that the basis of a lot of the information we used for these analyses came from an expert elicitation that was conducted under the same parameters as the PVHA was, which was discussed yesterday. The expert elicitation in this area was the probabilistic seismic hazard analysis.

This analysis developed hazard curves for fault displacement and ground motion. These hazard curves were expressed in the probability, the annual probability of exceedence of a given level of ground motion, peak ground acceleration, peak velocity, or spectral acceleration, and fault displacement.

In addition, by the way, there were eight AMRs in the calculation in this group of analyses. Two of our AMRs provided additional information, an expanded analysis, if you will, to support FEP screening in this area. One of the AMRs examined the effects of greatly changing fracture apertures in the intrablock area.

We present our geologic picture in this AMR for fractures, and then we make a modeler's assumption, and the UZ 3-D flow model was used to examine the effect of a ten-fold increase in fracture aperture throughout the intrablock area, and it was found that it had no significant effect on UZ flow.

Another of the AMRs looked at fault displacement effects. The design for the repository incorporates setbacks from known faults. However, one of our analyses performed looked at a what if scenario, if a normal or reverse fault or strike slip fault were to cross the drifts, looked at effects on the waste package and the drip shield, and found that there was no significant effect to performance.

This is a list of some of the primary FEPs in the seismicity and structural deformation area. You'll find a few more of these appended to the list headed Volcanic FEPs in your backup viewgraph.

Tectonic activity, large scale, the effects of plate movements. We primarily looked at the ultimate effect on UZ and SZ flow and transport. And given the slow time frame of this type of effect, we were able to exclude these based on low consequence over the period of regulatory concern.

For both fractures and faulting, included in the

TSPA was the existing influence of fractures and faults on UZ flow and transport. You've already seen that discussed by Bo and by Bruce. Excluded, based on our analyses, are changes in the characteristics of the faults and fractures, and the resulting changes in UZ flow and transport. Those were examined and found to not have a significant effect.

Fault movement shears waste container. This one was eliminated because examination of the faults in the area, we have quite a bit of data there, shows that a maximum expected movement in a single event on a large block mounting fault, such as the Solitario Canyon, is only on the order of about a meter. And when you have a 5 meter drift and a very robust waste package, this is not--we found it's not a concern.

In the area of seismic activity, you can see here that you'll have sometimes a very broadly stated FEP, like seismic activity, and we try to be careful about telling which aspects we look at under that one, and then we look at these different aspects under some of the others. So sometimes these are spread over several FEPs, but at a high level, you've seen in the past presentations, that we did include the analysis of shaking of the package from vibratory ground motion on the internal contents of the package. The package itself is robust enough not to fail the entire package from this vibratory ground motion. But we did have a cladding breakage analysis that showed some effect from vibratory ground motion.

And in the area of one of the hydrologic FEPs, hydrologic response to seismic activity, by this, we looked at potential changes in groundwater table elevations from the moderate level earthquakes that we've seen in the Yucca Mountain area. These effects have been found to be transient, and not significant to performance.

Volcanism area, we had eight primary FEPs. Those again are found in one of your backup viewgraphs. And we were able to eliminate three of them. One of those, for instance, is the release of waste in the effusive flow of lava on the surface. This flow is expected to be of a very limited extent, and isn't going to expose the critical group 20 kilometers to the south.

Another one was the effect of potential dike emplacement in the saturated zone away from the repository. This was examined during VA. We did sensitivity analysis on it and found that it would have virtually no effect.

I'm going to show a viewgraph later that shows these dikes are only a meter or meter and a half wide. So though they may be kilometers long, they're not extensively wide and wouldn't create a large perturbation in the flow system.

We used, again, for volcanism, a great deal of support from an expert elicitation which was discussed in detail yesterday. We particularly relied on the results, the probability results there. As you'll recall, there were hazard curves developed for the probability of intersection of the repository by a dike.

One of our AMRs, Frank Perry and Bob Young's work, summarized the results of the expert elicitation in order to help better focus, the key concepts that we used to underpin our conceptual model of volcanism. I thought that was very helpful considering sometimes these expert elicitations are very detailed and difficult to abstract what it is we're using as the key points. So that was done.

That same AMR updated the probability values based on the current repository layout. It's different now than it was during the time of the expert elicitation, and also in that AMR, Frank Perry examined the potential impact of some of the newer data that has come out since the expert elicitation, some things indicating possibly different strain rates, crustal strain rates, or the presence of buried anomalies. And in the AMR it presents reasons why these would have no significant impact on our current assumption.

Another AMR, Craig Valentine's work, added some consequence data that we needed to improve our consequence models over those of the VA. I think we've made some substantial improvements here, and we produced parameters for probability and consequence then for these types of processes. Again, remember we're constraining processes, helping visualize these processes, and presenting parameter lists and ranges of values that PA can use to characterize them.

For a dike intersecting the repository, conduit within the repository, the eruptive process, ash plume, and the interaction of magma with the repository. Whereas this first one was covered pretty thoroughly in the expert elicitations, the others got a much lighter treatment, but they're processes which we need to constrain in order to envision exactly what goes on during a volcanic event in the repository.

Finally, we had an AMR that brought all the volcanism analysis together. We called it the Igneous Consequence AMR. And in that work, we summarized it all, presented the conceptual model in the form of parameter lists and suggested values for the parameters for PA to use to abstract and model.

This is a useful picture because, again, when you're talking about dikes and volcanoes, it's interesting to me to keep the geometry of the system in mind. Again, the dikes are very narrow features arising from a deep magmatic source, and then responding to stresses in the shallow crust. They tend to propagate in the shallow crust perpendicular to the least principal stress, and they're very long and very narrow features. They can be kilometers long. Again, referring back to yesterday's talk by Frank Perry, we expect them to arise in the area of Crater Flat, and because of the least principal stress direction, be oriented more or less predominantly northeast/southwest.

As a dike rises to the surface, one of our other assumptions is that a dike that reaches within 300 meters of the surface will continue on up to the surface, and the eruption can then proceed several ways. Fissures may develop, as in this second segment of the picture, or the eruption may focus into what we call a volcano, and a conduit will form, which will then grow downward.

This is the PA conceptualization of the igneous intrusion groundwater release, and I'm going to put this up here for reference also as I talk about the next viewgraph. And in the igneous intrusion groundwater model, these are pertinent factors. The probability of dike intersection with the repository, again, that came from the expert elicitation and was updated by work in one of our AMRs.

Consequence parameters, we developed a more robust set of these from research from one of the AMRs. We came up with magma characteristics, temperature, pressure, chemistry, including such things as water content, viscosity, and so forth.

Dike properties, the dike width, length, and the number of dikes, you can have more than one dike in an event. Conceptualization of the magma drift and magma waste package interaction was examined under one of our other AMRs, and our initial work was for the interaction of a dike with the repository with backfill. That's the work that's been finalized so far. However, PA has been working with the newer design without backfill. We're finalizing those documents now, although the calculations and conceptualizations have been done. And that was George Barr's work. He looked at this area.

The conceptual model for TSPA/SR, we need to look at the waste package is compromised by the magmatic environment. We envision the dike coming up, intersecting the repository, and looking at how many waste packages would be impacted, and to what extent, on either side of the dike.

After that happens, we envision again the groundwater release is a long-term effect. The magma cools over time. Magma becomes highly fractured, and as it cools, groundwater infiltrates, contacts the exposed waste, and it results in an increased source term that is coming out of the repository. So you're imagining now that the volcano ceased long ago and you now have these compromised waste packages which produce an increased source term, radionuclide source term. Then from then on, the modeling follows the same as the nominal for UZ and SZ.

This is a conceptualization of eruptive release, and this is one of Greg Valentine's conceptualizations. Again, we developed conceptual models of the geologic process, and the type of volcanism we expect in this area, as you've heard already a couple of times, is basaltic volcanic activity. And Strombolian eruption is another characterization, could have several phases to it. It can have an effusive phase where the lava is just flowing out relatively gently. It can have a moderate phase represented in the upper right-hand corner here where you have the features listed, or a violent Strombolian phase. And, again, our conceptual model is all of these can occur, however, for PA, only the violent Strombolian phase was modelled. This is a conservative assumption.

This is the same viewgraph I have up here, which I'll leave up while I discuss the parameters. To model the volcanic eruption release, we look at the probability of the eruption through the repository which starts with the probability of dike intersection. And this next probability is not a conditional probability; it's just the probability of one or more eruptive centers.

So we don't assume that just because a dike intersects the repository, that there's an eruptive center in the repository. We do assume there are eruptive centers somewhere along the dike.

For all packages, we do assume that for all packages within a conduit that may form in the repository, that those packages are completely compromised, and that the waste is then available for transport at the surface in the eruptive cloud.

The disruptive events consequence AMR presents the parameters that characterize the process. This is the work of Michael Sauer and Peter Swift, and again, they present parameters for characterizing the eruptive characteristics, conduit diameter, magma characteristics, eruption duration and volume, bulk grain size and shape. These are all factors that are used in the ash plume dispersion modeling code.

They also handled the atmospheric transport parameters, wind direction, wind speed, waste particle size. These are factors in how far the contamination might go.

As you saw in the last presentation, in order to get from a volcanic release to dose, you have to go through the biosphere calculations, and Mr. Schmitt has already explained these. They had special BDCFs, disruptive events BDCFs for the atmospheric release, and used the nominal BDCFs for the groundwater pathway.

This is the TSPA dose curve for dose from both eruptive and intrusive release, and the mean is the red line. 5th and 95th are presented. You'll see in the first, say, 1200, 1300 years, the dose is dominated by the eruptive release. However, the groundwater pathway release begins to dominate later on.

COHON: I'm sorry, can I just interrupt for one second? This is Cohon, Board.

Just for clarity, and recalling what we heard yesterday, the axis shows dose rate multiplied by the probability of a volcano occurring; is that right?

GAITHER: Yes.

COHON: Okay.

GAITHER: This is the sensitivity analysis on a given probability. You'll see the base case. This, again, is the same mean that you saw on the last viewgraph. This isn't really peak eruptive dose; it's a maximum eruptive dose. The peaks are represented by the highest bumps on the horse tail plot you just saw. But it compares the doses, given the full range in the base case that was sampled, and a run that's set at 1 times 10 to the minus 7 probability.

So in conclusion, disruptive events are included as process model factors for TSPA/SR. Sensitivity analyses have been performed on these factors. Those are in your backup viewgraph. For TSPA/SR modeling of seismicity and faulting, seismicity, groundmotion, effects are included in the nominal case in looking at the effects of seismic vibration on cladding and drip shield. FEPs analysis shows the remaining

FEPs can be excluded based on low consequence or low probability.

We're currently re-examining the FEPs with the no backfill design. And TSPA/SR includes volcanism as the only contributor to dose within the regulatory period. So I certainly have gotten myself an exciting job here. It could be why Rollie Bernard is no longer doing this and has taken a job at Sandia where he's working on Russian nuclear waste problems, and part of the job description is inoculations for frightful diseases and travelling to the fringes of Siberia. So maybe I should have paid attention to his career choice instead of Bob Andrews when he told me what a great opportunity this was going to be.

CRAIG: Thank you.

GAITHER: That's the end of my talk.

CRAIG: Okay, thank you very much. Questions from the Board?

PARIZEK: A clarification question. Parizek, Board.

I think you said 10 times increase in, what, permeability or porosity had no effect on flow in the unsaturated zone, or saturated zone?

GAITHER: Fracture aperture opening.

PARIZEK: Yeah, that's a power law in terms of the permeability effects of a slight increase in aperture.

GAITHER: Right. It decreases the saturation. I know that was one of the factors. But I'm sorry, I'm not a hydrologist.

PARIZEK: We want to make sure we understand. You said fracture aperture?

GAITHER: Right. That's what Jim Houseworth did. He cranked this through the UZ 3-D flow model, increased the fracture apertures ten-fold, and did not see a significant effect on flow and transport. And I'm sorry, I'm not--

PARIZEK: We'll have to look into that. Another question about the dike formation. If you have dikes that are maybe several kilometers long, they could be rather impermeable barriers to water flow. So in terms of groundwater flow effect, it may not be no effect. There may be some measurable effect in perturbing the flow system.

GAITHER: I know that they did a sensitivity analysis on this during the VA, and placed these barriers in the SZ system, either increased permeability or decreased permeability, and they found no significant effects on the flow. Is that not correct, Bob? I'm pretty sure they did.

PARIZEK: We think of it as affecting a full field pattern somehow.

GAITHER: It may divert the flow somewhat, but it doesn't have an effect on dose?

PARIZEK: Now, the dike intersection knocks the hats off all the waste packages and releases everything because that's being conservative, because you don't know that all the lids are going to blow? I think I understood you to say once a dike hits it, you release what's in all packages.

GAITHER: No, once in a conduit. Look at your backup viewgraph. Greg, did you want to address some of this?

VALENTINE: Yeah, just to clarify the issue of the effects of a dike on the saturated zone. The predominant orientations of the dikes are going to be sub-parallel to the flow in the saturated zone. So I think that's the reason why there's no a major effect. I mean, it's not oblique enough to really be a barrier.

PARIZEK: Does it shift it, though, into the alluvium, or away from the alluvium? It's northeast/southwest? If it's northeast/southwest, it could divert flow into the--out of the alluvium, which then shortens the path length in alluvium. So I can visualize a west/southwest direction not being helpful.

GAITHER: Regarding the package damage, this is your backup viewgraph Number 27. For an eruptive event, we assume all packages in the conduit, 50 meter mean diameter, are completely destroyed. But for the intrusive event, which we look at separately, we have zones. We have the area right on either side of the dike. I believe they assume one package is destroyed where the dike is, and three on either side. And these packages are completely destroyed. Whereas, in the rest of the drift away from where the dike actually has its greatest impact, this is the type of failure that is assumed. Failures of the end cap welds, anywhere from a square centimeter to the maximum of a whole end cap. So it is a different type of damage that's assumed.

CRAIG: Priscilla next, and then Dan.

NELSON: My question was I think partially covered by Richard, but let me just say again the question that I had in mind was about dike, or any sort of an igneous activity that doesn't necessarily engage the repository, that really can change the flow field, whether it occurs north or south of the repository, and can actually focus flow and cause significant changes in the flow path. Is that not analyzed because it's an extremely low consequence event, or what is the status of thinking about such impacts that aren't constrained to intersect the repository?

GAITHER: Those were examined under FEPs analysis. Bob, do you want to say more about it? They examined them and did sensitivity analyses. I don't know if Bob can tell you more.

ANDREWS: This is Bob Andrews. The screening argument for that, you know, was a low consequence argument, that even if a dike intrudes the saturated zone, for example, or the unsaturated zone, but not the rest of the repository, that the effect on transport, on flow and transport, was within the bounds of the range of uncertainty that was already incorporated in the abstractions, and included in the TSPA/SR.

We did not go to a dose based consequence screening argument because at that time, they didn't have the dose basis to make that consequence screening argument. Now we do, and the argument would even be stronger, you know, to exclude it, because any effect, any consequence effect of those indirect volcanic events would be multiplied by the 10 to the minus 8 probability per year. So the net effect would be zero so, therefore, screened out.

BULLEN: Bullen, Board. Yesterday, we heard from Bob, who just is not sitting down, that the wind always blows south. But you have data, you have wind rows or joint frequency distribution functions or something that you can plug into the Jenny-S code that will tell you what the real wind velocity might be? And you also have data on what the plume might look like for an eruption. And that's what gives you the doses, and it's not a dose, it's a risk; right? If it's a dose times a probability, that question that Jerry asked? So you have the information that's necessary, and this is actual? Does it always have to blow south? I mean, you actually know the direction. This is an over-conservatism; right?

GAITHER: I'm going to let Michael Sauer explain this. I like to let the technical team talk about their work.

SAUER: Michael Sauer from Sandia. What we've done is we've actually developed the distribution for wind direction. But then we decided to conservatively let the wind always blow south. The reasoning behind this is that by doing it this way, we're not accounting for redistribution of ash that might fall on the side of Yucca Mountain that would later be washed down Forty Mile Wash. And the argument we make is we're really, we've captured this similar argument that Bob just made for a different issue, that we've captured the range of uncertainty by having it always blow south, essentially a bounding analysis.

BULLEN: Bullen, Board, again. The follow-on here is that you also have the particle size distribution that optimally falls 20 kilometers away?

SAUER: Actually, we don't. What we've done with the particle size distribution is, actually, Greg Valentine developed that based on analogs that are observed in nature, and we've just utilized those directly. Okay?

BULLEN: You mentioned nature, so I have one final follow-on question. How much radioactivity is released in a volcano that doesn't hit Yucca Mountain in this region? What kind of radionuclide inventory increase do you get on the surface from the ash from natural radionuclides?

SAUER: That I'm not sure of.

GAITHER: I don't know that either.

CRAIG: I have one question. This famous Figure 14, which we've now seen several times, you dealt with a difficult problem of combining a high probability low consequence events with low probability high consequence events, and it makes it a rather complicated diagram to understand. There is a lot of interest in what the worst case could be. Do you have a graph that shows how many--what the dose rates would be if the event were to occur?

GAITHER: I'm not sure I understand that question.

CRAIG: Supposing one of these events actually occurs.

GAITHER: You mean one like this one?

CRAIG: No, no, an eruptive event. You've multiplied, over on the left-hand side, you've multiplied by the probability of the events. And you've done it in a way which is rather complicated to disentangle because of the nature of the way you've done the calculation. What I'd like to ask you to do is to disentangle and tell us what kind of a dose you might actually get.

GAITHER: Okay, I will have the tangler disentangle it for you.

ANDREWS: This is Bob Andrews again. We didn't tangle this on purpose.

CRAIG: No, it's a complicated presentational problem. I don't fault what you've done, but I do think it is reasonable to ask for the actual dose that the most exposed individual or set of people might receive should the event occur.

ANDREWS: I think that's a reasonable question, too, Paul. And we can pull that number off of this plot in fact. For the eruptive scenario, which has an annual probability of occurring of about 10 to the minus 8 per year, that means in the first 100 years, and I'll start right there at that 100 year line rather than complicate it with other time frames, at 100 years, the probability of it occurring within that first 100 years is just 100 times 10 to the minus 8, assuming this was linear. So that's about 10 to the minus 6 probability. So that 10 to the minus 6 is being multiplied more or less by the dose to get this risk, or dose rate that we have on here.

So if we take that mean curve, and the mean there is about--well, the 95th percentile is 10 to the minus 2. It looks like the mean is about 3 times 10 to the minus 3 millirems per year, and multiply it by 1 over 10 to the minus 6, or 10 to the sixth, you see that's about 3 rems per year from that unlikely low probability event.

Now, we do not show that plot, but that's what it would be. The NRC in their IRSR on igneous activity does show those doses attributed to, you know, the conditional dose, if you will, and their range, I think there's people here who can probably better give the exact range, in their igneous activity IRSR is in the order of a few rems. I think it was like from 1 to 10 rems. It was a range of values.

And that kind of indicates, you know, the amount of mass, the radioactivity, the biosphere pathways that John alluded to, that all contribute to that dose. But the probability of it occurring is 10 to the minus 8 per year, or close to that.

CRAIG: Other questions from the Board?

SAGÜÉS: Quickly. So then the multiplier, it varies with time?

ANDREWS: Yes.

SAGÜÉS: The multiplier, to get the actual probability of the event, you will have a very high multiplier on the left, and the multiplier becomes smaller as you go to longer times. Thank you.

CRAIG: Okay, last question?

MELSON: Bill Melson. One of your figures showed there would be over 6,000 casks are being damaged. What percentage of the contents are released in this kind of worst case scenario?

GAITHER: I'm not sure I can provide that information, because that gets into what happens with the waste package and waste form calculations. I'm sorry, I don't know what the percentage is. I'm not sure if anyone here does.

Well, in this area, the release then would be, again this is for the intrusive release, which would be the groundwater pathway, I don't really know the percentage of the waste that would be released. You mean of what is there, or the percentage of what would be in these packages overall? I'm sorry, I don't know that. Bob, do you know that?

ANDREWS: This is Bob Andrews again. But it's nuclide specific. You know, for things like iodine and technetium where the fuel is altering rapidly and they're very high solubility, it's virtually 100 per cent. You know, for neptunium, which is still solubility limited, you know, based on what Christine just showed you, that fractional release, effective release rate is a function of the solubility and the seepage and how much can be mobilized. For the even less mobile nuclides, most of it's staying there still. So it depends on the nuclide.

CRAIG: Okay, last, John Kessler.

KESSLER: you mentioned for the eruptive events, that you were picking only the violent Strombolian type of eruption.

GAITHER: Right.

KESSLER: Is that consistent with the probabilities? I mean, these are certain kinds of eruptions that PVHA has based their probabilities on. My understanding, and correct me if I'm wrong, is that they're not violent Strombolian type of events. So I'm concerned that there's a mismatch between probability side of this risk equation and the consequence side, that it's not based on the same kind of volcanism, at least for the eruptive.

GAITHER: Well, the probabilities that we look at are the probability of a dike intrusion, and the probability that event will form in the repository. Those are the probabilities, which seems to me disconnected from what the kind of eruption is that happens after that. In other words, those probabilities are set, whether the eruption becomes to be mostly violent or mostly moderate. I'm not sure that there's a real disconnect there. And the reason that we modelled the violent Strombolian is because that's what ash plume is designed to model, and that's the dispersion code we used. And it's also considered a conservatism by the PA group.

So I'm not sure, maybe I'm just missing something, but I'm not sure there is a disconnect. Am I correct? I'm not sure, but I don't think there is.

CRAIG: Okay.

GAITHER: The probabilities don't say what kind of eruption.

CRAIG: We'll let you chew on that one for this afternoon, and at this point, we need to take a break, and we will resume promptly at 11 o'clock, which is in 13 minutes.

(Whereupon, a brief recess was taken.)

CRAIG: Our next speaker is Abe Van Luik, from whom we've heard previously, and Abe is going to talk to us about uncertainty.

VAN LUIK: Thank you very much.

Let me start my talk on the fourth page of your handouts, because the second and third pages I actually wanted to use at 4:30. This will also help make up some of the time schedule.

The focus of this presentation, if you look at the whole viewgraph, you'll see that this is one that you also saw in January. But the focus of the presentation, and what the Board has been talking about so far, in our opinion, is the technical analysis of how quantified uncertainties are treated, both in the process models and the TSPA.

What we also told you in January is that we also need to look at all uncertainties, both the quantified and the unquantified, which we typically have dealt with in various fashions. And then also we routinely do policy and technical assessments to manage the uncertainties, and we are really focusing now also on explaining our uncertainties to various audiences.

So this is what we told you in January that was our strategy for dealing with uncertainties, and what I'm going to do now is show you how we are implementing that strategy in what I think is a rational fashion.

We told you that we would identify sources of uncertainty, treat them quantitatively or qualitatives with conservative bounds; that we would manage uncertainties, considering their impact and importance. Of course, if there is no impact or importance, then the uncertainty doesn't matter. We just need to disclose it.

We need to reduce or mitigate critical uncertainties, I mean, that's why you evaluate uncertainties in the first place, and assess the effects of the residual uncertainties, because there will be uncertainties that are not manageable by any of the other means.

So to keep the promise that we made in January to the Board, we have a task force of DOE members, MTS members and M&O members, and many of them are here in this room. We are looking at the implementation and effectiveness of this approach. We are an internal review committee, so to speak. We are trying to identify where the uncertainties and variability have been included in overall performance assessment, and you saw from Bob Andrews' talk that TSPA is on the mark as far as considering uncertainties in its analyses.

We want to look at how all uncertainties have been treated in the process model and abstraction level, and we hope to be able to have an internal report by September, and we want to evaluate the uncertainty treatment and develop recommendations by November of this year to improve the entire way that we're dealing with uncertainties.

This task force is doing a bottoms-up look. We are starting at the bottom, at the process level, reviewing all the AMRs and PMRs and interviewing the principal investigators responsible for each of these to not only read the documents, but find out from them what the documents mean in terms of what has been terms of uncertainty.

We are looking at things like alternative conceptual models, parameters, distributions, spatial extrapolation and time-scale issues, the partitioning of variability and uncertainty, temporal and Spatial boundary conditions, the assumptions and judgments made. You've heard a lot from the last five or six presenters on that topic. The use of data bounds and conservative estimates, and then we're also looking at the uncertainty that's embedded in the FEPs process, looking at features, events and processes, and the screening, as you've heard from the last talk, of low probability, low consequence scenarios.

We are looking at both quantified and unquantified uncertainties, and this presentation, and I'm trying to lower your expectations here, is a status report which will just focus on two detailed examples of the treatment of uncertainty. In other words, we have done about 23 of these cases. I'm showing you two because of their inherent interest to us and to the Board.

The first one is if we look at the waste package degradation process model, the purpose of the model is to evaluate waste package integrity. We know that there are processes that can influence the degradation of the waste package. We know that there are environments on the waste package and in the drift that are features considered subject to uncertainty and variability.

There are other features, and that's what this means right here. These are processes. These are features. Other features, events and processes were considered, but in the FEPs screening process, which is actually a great integrator from science and engineering, right up until performance assessment, these were screened out due to low consequence or probability.

Selection of specific process models is subject to conceptual model uncertainty. And I think we can go to the next one to show the stress corrosion cracking model. When we look at the degradation processes for the waste package, this is the model that I'm going to focus on, although I could have selected this, I could have selected that, but this is the one that we're going to focus on, just to give you an example of the level of detail that we're going into in this uncertainty evaluation.

Stress corrosion cracking has three overlapping influences on it; material susceptibility, tensile stress and environmental conditions. And if we're in a critical region of those three, then stress corrosion cracking can occur. The most important of these we find is the, as you saw in Bob's presentation on TSPA, is the degree to which stress is mitigated in the welds.

If we look at the conceptual model for stress corrosion crack growth, we looked at two conceptual models and received external expert advice that this is the one to go with because it's more defensible for the very long-term use that we want to make of it. It's a more complex model, but it's more defensible, they thought.

The significance of the model itself, whether we choose this one or this one, is dependent on the degree of stress mitigation. If we mitigate the stress to the extent that we think we can, the two models give absolutely the same outcome.

The process model, as has been explained before, is then abstracted into a TSPA abstracted model, but we will stay with the process model discussion for now.

If we look, and I don't want to go through all of this table, but this is an illustration of the type of evaluation that we're doing. We're looking at the uncertainty. We're looking at the variability. And we're looking at what the range of it is and what the basis of it is to see if we have a complete picture of what is being treated in each model.

And I think rather than read through these in some detail, which would involve questions that I am not meant to be answering, this is just an example of the type of thing that my technical team, it's actually Bill Boyle's technical team, but he couldn't make it, so I replaced him, our technical team is looking at in some detail.

The abstraction--that was at the process level--and then as I mentioned, we do an abstraction. In this particular case, the abstraction introduces what some of us consider an additional conservatism. We just disregard the orientation of flaws, even though only 1 per cent of the initial flaws in a weld, in a sample that was examined, 1 per cent of the flaws have a radial orientation, and that's the only orientation that could actually be subject to stress corrosion cracking. And we considered in the TSP all surface breaking flaws and all embedded flaws in the outer 25 per cent of the depth of the weld, so that some of the uncertainty in the previous page is kind of stepped above for the TSPA analysis. Nevertheless, we want to be accounting for all that uncertainty.

If we look at the results of this particular model, we see that the first waste package failures on the upper bound, the most optimistic case, using the upper bound of all the uncertainties--that would be the lower bound of the uncertainties, I guess, but the most optimistic case, you have failures right after 10,000 years. If you look at the mean, however, it's, you know, more like 20,000 years until your first failure, and then you have a cross-over of the mean and the median here, illustrating again that the mean is really torn by the larger numbers. Whether you're on the upper scale or on the lower scale, if the numbers are very large, the mean is more influenced than the median. The median is a very nice measure of central tendency.

But this is just an example of the type of uncertainty evaluation that has gone into one process model. And the treatment of uncertainty in these models varies from model to model, and one of the tasks that we are coming up with is making recommendations on how to even it out so that the treatment is more uniform.

If we go to the next viewgraph, we're going to talk now about the thermal-hydrologic models for TSPA. And this nice little viewgraph shows that the input data is run through the UZ property model, and that property model then defines the properties for all of these models. And, of course, the outputs on the right-hand side are things that are output directly into TSPA.

We're going to follow this path through here and talk about the multi-scale model. The properties model is used to define parameter uncertainties. It's a very nice piece of work that includes the property set that is most consistent with measurements, and evaluates their uncertainties.

The matrix and fracture parameters used in the flow and transport, drift seepage, drift-scale and mountain-scale process models come out of that one model, so that you don't have the problem of using this model here with a different property set than the other one.

The calibration process uses data inversion to compare and adjust the model parameters and the data. And ITOUGH2 is the computer code that's used, and it considers uncertainties in the input data, in the analysis, and the output parameters and their sensitivities, and can pass them on to the next model down the chain.

The data inverted is matrix saturation and matrix potential, pneumatic pressure, and the parameters estimated, and they are estimated for high, mean and low infiltration cases for three climate states. So for each climate state, there's a high, mean and a low.

The parameters estimated are fracture and matrix permeability, fracture and matrix van Genuchten parameters, that's supposed to be an alpha and m, fracture activity parameter. And the uncertainties are evaluated for 31 model layers, assumed to have uniform properties, however, within each layer.

Spatial variability in infiltration is incorporated using 200 meter radius average around boreholes, so that, you know, there is extrapolation of data within the model that we have quantified and know about.

Now, when we move to use these property sets in thermal-hydrology calculations, the question has been should we use properties, generic properties such as used in TSPA/VA? Should we go to the drift scale property sets, which is the TSPR base case property set? Or should we get real close to the actual location and use the single heater test property set? And there was a test done using two forms of the dual permeability model, and the bottom line is that the predicted temperatures seen in single heater test, and we did this also for the large scale heater test, but that would be a separate presentation, predicted temperatures, evaluated the differences statistically. This was not a calibration; this was no adjustment of parameter values. We were looking at which of these property sets best evaluated the temperatures in that heater test, and the conclusion was that the differences were small between predicted and measured for all the property sets, but the ambient drift scale property set and the active fracture dual permeability model are suitable for use in thermal-hydrologic models for SR.

So I don't want to, you know, make this declaration and have you ask questions on it. I'm illustrating the type of things that we're investigating in this internal review of how uncertainties are being evaluated and how that evaluation goes down into what model is selected for determining heat, for example, in the mountain.

If we look at the multi-scale thermal-hydrologic model, the treatment of uncertainty there is the uncertainty that goes into the model comes from selection of the high, mean and low rates of infiltration for the three climate states.

The model is very rich in variability, but that's the only uncertainty that comes out of it. And, of course, this shows us that there is a difference in the way that these different models are treating uncertainties. So we have a job on our hands, and that's our task, is to make recommendations on how to fold more uncertainty rather than just variability into the rest of this model.

Now, if we go to the next page, you see the outcome, that if we look at the low, medium and high infiltration cases for the present climate, you get differences in the drift wall temperatures, waste package temperatures, the time of the drift to return to boiling temperatures, relative humidity at the waste package, the boiling zone in the host rock, et cetera. So there is the uncertainty that is put into the model comes out in the output.

In summary, our approach to uncertainties recognizes the need to assess, quantify, manage and communicate uncertainties. This is a first step in that process. The uncertainties, variabilities and conservatisms are being identified. That's a work in progress and it's going very well in all process models, providing input to the TSPA and TSPA is taking care of itself pretty well, as you heard from Bob's presentation.

We're in the process of examining the current implementation. Our focus to date has been on understanding the details of what has been done and how adequately it is documented. We have found several instances where work was done and it was, you know, just not put into the documentation, and of course we'll put that on the list of recommendations.

And, of course, this is a work in progress. What are we planning to do to finish this work? We want to complete the detailed review of the uncertainty treatment and how uncertainties are reflected in the TSPA/SR. That's our goal for later this fall. We want to assess where we need to improve the characterization and/or documentation of uncertainty. In some cases, there needs to be more characterization, and other places work was done that's not properly reflected in the documents.

We want to develop recommendations to be used in future uncertainty treatment. We're looking forward, you know, for the next couple of years into the license application. We want to assure consistent definitions, and to the extent that it's appropriate, methods for treating quantified uncertainties.

We want to improve the importance analyses of quantified uncertainties, and you're going to see some importance analyses in the next presentation, too. You'll see actual results of importance analyses.

We want to suggest approaches for evaluating key unquantified uncertainties in terms of their implications for TSPA dose uncertainties.

And I think it is certain that I have made up some time.

CRAIG: Abe, that was masterful. We are not only on schedule, we are ahead of schedule, and I now turn to Dr. Cohon to ask you, because we're going to have discussion here.

VAN LUIK: You just set me up for a long discussion, is what you did.

CRAIG: I hope so. Discussions are the best part of the Board meetings.

VAN LUIK: Yes, they are.

CRAIG: How much time should we spend on discussion?

COHON: We can go till 11:45.

CRAIG: 11:45. So we have 25 minutes for discussion.

COHON: 23.

CRAIG: 23 minutes for discussion. Jerry, Alberto, Dan, others.

COHON: This is Cohon, Board. I have a big topic, and it's properly a topic for this afternoon's panel. But since we have extra time and we've got you standing up here--actually, Abe, you're exactly the person to start with it, and then maybe we can pick it up later if we all feel it's worth pursuing further.

I have sort of a fundamental philosophical concern, modeling concern, with where we're going with TSPA, and that this concern would come up now is completely understandable. It's not a criticism of what has been done. In fact, let me say here I'm very impressed by everything that we've heard. Your comment yesterday, or maybe it was Bob's, about your pride in how much integration has occurred I think is very well placed, and it shows. It's very good and really very exciting. But you've got a very tough problem, and we know that.

Here is my issue. Let me put it this way. Using the design--I have to take another step back. We know that specifying the design is essential in order to do TSPA, and that's just the nature of the integration that you and Bob were so pleased about. It's also the case that performance will be a function of both the design and the natural system, and as we've seen, we now have a very robust package with a titanium drip shield, and they have major implications for performance. And in a way, in a very significant way, you're using the design to compensate for natural system uncertainty, and that's okay. Here's my philosophical problem.

It's not okay, I think, to use the design to limit the treatment of uncertainty or its representation on individual parameters within TSPA itself. Am I getting through? Let me give you an example. Here, actually you just gave an example. If we assume we're going to treat welds in a certain way so as to relieve stress, and that means that we represent the uncertainty associated with the welds in TSPA in a different way than we would if we were not treating the welds, making that assumption about the welds would be treated, I think that's wrong, or I think that can create a problem later on. Maybe that's not such a great example. I think I've got a better one.

Here's one. If we assume that ranges in pH are what they are within the drift environment, because of assumptions we're making about the lack of seepage because of the titanium shield, let's say, then that can be a problem. So my point is in terms of overall performance, engineered system, natural system trade-offs are completely appropriate within limits, of course. But if the engineered system is used to limit or change the way we represent parameter distributions in TSPA, I think we've got a problem, and I'm going to try to tease out some more examples to find out and explore this afternoon whether or not we've gotten ourselves into that situation.

Have I made the point clear, the overall point?

VAN LUIK: I think I understood the point better the first example than the second example.

COHON: Okay.

VAN LUIK: But I think, you know, would it be satisfactory if we showed the effects of stress mitigation on the welds by doing a calculation with and without mitigation? Would that satisfy you that we know what we're about? I'm trying to figure out just what the crux of the problem is.

COHON: I have no doubts that you know what you're about. The concern is that there's so many pieces to this and there's so many people that know what they're about about their piece of it, that things might get lost in the process of pulling it all together.

VAN LUIK: Yes.

COHON: And so I'll try to come up with better examples.

VAN LUIK: I think, you know, that is one good example, where we actually know from analyses already why it is so necessary to mitigate the stress, because as Bob showed, the first two points on his five points of light of what determines performance after 40,000 years is the stress on those welds. And so, you know, you make a good point. We need to evaluate as time goes on if there is uncertainty in the degree of mitigation and other things. But we're not there yet. You know, we are not to the point where we can do that.

COHON: Just to nail this down. It goes right to the FEPs screening process. I worry about excluding some phenomena or artificially limiting the range that we're going to look at only on the basis of TSPA performance sensitivity. Using arguments about basic physical phenomenon is a good one, and we heard a lot of that in the screening. But if we base it mostly, or even worse, entirely on TSPA results, then I get worried. I'll try to come up with more examples.

VAN LUIK: I understand that one perfectly. In fact, we agree with you. That's the reason that we carried calculations out, you know, for the SR purposes, SR/CR purposes, to 100,000 years. If we stuck with 10,000 years, we would exclude everything.

COHON: Right.

VAN LUIK: Because the waste packages haven't failed yet, but because of that exact reason, seepage is very important. It doesn't become important until after the regulatory period, but it is very important, and we agree exactly on that particular issue. And I think, you know, the idea of the drip shield making seepage less important to performance during the regulatory compliance period is very true. However, seepage is in the model to allow us to look beyond the regulatory compliance period, and we have a suspicion that when we walk into licensing, that the NRC will say change this assumption, change that value, change this, and we had better have all of those mechanisms in the model to take care of that contingency.

COHON: That's exactly the bottom line point. Still, I'm going to try to come up with more specifics to kind of see if we can track them down this afternoon.

VAN LUIK: Okay. Good.

COHON: Thanks, Abe.

CRAIG: Alberto?

SAGÜÉS: Okay, I was just trying to figure out how you rule out this uncertainty on mechanisms that have been ruled out relatively early in the process. If we go, for example, to your Figure 13, just to have a quick indication, which this is the fraction of waste packages as a function of time.

VAN LUIK: Yes.

SAGÜÉS: Okay, now--of course you're looking at first crack; that's the only thing that you're looking at. But suppose that the name of that would be first penetration, it would still be pretty much the same curve; is that correct?

VAN LUIK: I think it would be pretty much the same starting point on the curve, yes. But it's a combination of stress corrosion cracking with--you know, if we have a situation where there is no surface breaking, or if there were no initial defects, you would still, you know, by general corrosion, go through that weld until you hit the first defect.

SAGÜÉS: All right.

VAN LUIK: So some of that I think shows up later.

SAGÜÉS: Right. Okay. Now, effectively right now, localized corrosion is declared in something that's not going to happen?

VAN LUIK: That's correct.

SAGÜÉS: Now, suppose that there is localized corrosion that could result on the packages showing failures at 1,000 years, you know, really way, way before that, now there's a certain amount of uncertainty about that. I mean, you're not certain that localized corrosion is not going to happen?

VAN LUIK: We are certain to the extent documented in the FEPs screening documents.

Now, as the NRC pointed out to us, the only thing that's interesting about the FEPs screening documentation is what we have ruled out. And so that will receive a very good scrubbing from them, and there may be cases where we will have to do more work to make the case that something should be screened out. But I believe, and other people in this room know this better than I do, that the work we have done so far on Alloy-22 shows that the pitting, the localized corrosion is not likely to be something that would lead to failure before these other two mechanisms.

SAGÜÉS: Now, would you say that, for example, you're 90 per cent sure of that? I mean, you realize what I'm asking about?

VAN LUIK: I trust the people that have told me that this is the conclusion that they draw from their work, yes. As a DOE person, I have to do that, and 98 per cent sounds good to me.

SAGÜÉS: Well, I said 90. But anyway--

VAN LUIK: You said 90?

SAGÜÉS: Yes. Suppose you say 90, and if you're in the 10 per cent probability you're wrong, that would result in massive failures at age 1,000.

VAN LUIK: Yes.

SAGÜÉS: Then that would cost, of course--dramatically. And so where is that assessment? Where is the quantification of--what if I'm wrong about this assumption? What if I'm wrong about the assumption? All those things are going to be moving, maybe not--maybe the dose, they're going to be moving them to a lift. Right now, they have zero multipliers.

VAN LUIK: The analysis shown by Bob Andrews yesterday that showed the 95th percentile pessimism in the seven operating processes on the waste package showed failures before 10,000 years. That's one case.

The talk that you're going to see after me, the safety strategy will show another case where we assumed that there is waste package failure with the drip shield intact. And then did you also do one without the drip shield? Yes.

SAGÜÉS: But that's only with the mechanisms that have been declared to be possible.

VAN LUIK: Yes.

SAGÜÉS: The ones that are declared to be effectively impossible, like for example localized corrosion, those ones are not going to show up.

VAN LUIK: They are not going to show up.

SAGÜÉS: Okay. I would think that that's something I think we are going to have to talk about in the future a little bit more, because I think that right now, we're rolling out entire classes of mechanisms and assuming that there is zero probability of that ever happening.

VAN LUIK: Yes. And if in the future we learn that that is not as correct as it sounds today, we will of course make a correction.

SAGÜÉS: Thank you.

BULLEN: Bullen, Board. This may even be a more philosophical bent than our Chairman took a couple of minutes ago, and is probably a good follow-on to Alberto's question, and you may rue the fact that we actually transcribe these meetings, because I can actually quote you from previous meetings here. But in previous meetings about VA, about TSPA for VA, you made comments like what VA can and cannot be used for--excuse me--PA can and cannot be used for.

And so I guess I'll go back and quote a couple of things that you said. "It probably shouldn't be used to assess compliance with regulations. It shouldn't be used to show defense in depth. It shouldn't be used to assess small changes in design, or even to determine the suitability of an overall repository design." Those are kind of--they may be taken out of context, but those are quotes that you said about TSPA/VA.

And could you comment now on TSPA/SR, or the data that we have seen and the results that we have seen, and maybe amend your comments, or at least identify where you think the improvements have been made that would soften the tone of those comments?

VAN LUIK: I would respond in this way. This is a nice question, actually, because this is kind of how I was going to start off my 4:30 talk, so I don't have to do that now.

BULLEN: If you want to wait till then, that's fine.

VAN LUIK: No, no, no. What I was going to say is that as you have seen from the presentation, as Chairman Cohon has pointed out, the TSPA that you see now is the best integrated product we've ever produced.

When its results are done with checking, and the final approval comes in, I think that it will be material that will be useful in making the regulatory assumptions necessary to have DOE go forward to site recommendation. I think it's at that point.

Now, if it turns out that there are errors, you know, that's the reason that after this decision is made, we go into the actual licensing process, which is a very rigorous process, if it's anything like has been done for other nuclear installations. But I feel that we have made so much progress since TSPA-95, TSPA/VA and this one, that this one the Department of Energy, when it is all done and checked and finally approved, will stand behind it and say this is the basis, not Rev 00 that you see for the SRCR, but Rev 01 that you'll see next year, as I pointed out in my talk, this is the basis for going forward and recommending to the Secretary that he recommend to the President that we approve this site.

If we were not of that mindset, we would be wasting your time and ours.

BULLEN: Bullen, Board, again. I've got a follow-on to that one. One of the other problems that I had with yesterday's presentation was sort of the non-specificity of the operating procedures and the design. And the problem that I run into there is that as you go into licensing and as you take this path forward with TSPA, what you have to do is you have to have a finalized design and you have to have a finalized set of criteria that you're going to evaluate against, and you have to have regulations, which by the way, we don't have either, but you'll have to take a look at those, too.

And I guess what I'd like to know is in the efforts to reduce the uncertainty, and keeping that flexibility in design, for example, we heard in May in the Rich Craun presentation, that a more robust design may allow staging and aging, and ventilation of fuel, and not hit the temperatures that would cause some of the problems that we've seen associated with cladding degradation or waste package degradation, or the like. How are you going to incorporate or encompass those in a regulatory regime and in an evaluation that you're going to make to, well, the Board and also to the NRC with respect to the I guess finalization of the design? And when will that occur, and how do you see that happening?

VAN LUIK: I was glad that Dr. Itkin answered this question yesterday. We will have one design going into the license application. It will still be flexible, however, so that we can manage it one way or the other. And I think Dr. Itkin was exactly right. As soon as you start gaining experience in the manufacturing and in the filling, sealing and emplacing of waste packages, you will redesign as you go and learn from experience, and there will be changes.

Any major changes will have to go to the NRC for an amendment to the license. So I think we will go into LA with one design, but it will still be operationally flexible so that we can adjust things, even from drift to drift if we want to, if we see the need to. I don't think we're going to lock ourselves in to where the NRC is going to take a measuring tape and say this package is, you know, one-tenth of a centimeter off where you said it would be.

BULLEN: Bullen, Board, again. Just to follow that up, that also includes an operational concept?

VAN LUIK: Yes.

BULLEN: And so you're going to come in with an operational concept that is hot, is cold, is manageable so that I can keep it cool until I close it, and then let it get hot; all of those are going to be evaluated prior to the license application?

VAN LUIK: We will come in with a preferred operational concept for the license application, yes. But we will also talk about contingencies and flexibility, and if anyone of the design group wants to step forward, be my guest. But I think I'm correct basically. We will come in with a vertical stripe that says this is what we want to license, and these are the degrees of deviation off that line that we want to keep for operational flexibility.

CRAIG: Debra?

KNOPMAN: Knopman, Board. I have two questions, Abe. The first one has to do with scientific priorities at this point. Based on what you know and your experience with TSPA, including both natural and engineered barriers, how would you--what are your priorities over the next year in terms of the science that you feel you need to have under your belt?

VAN LUIK: Actually, I'm looking at Dennis Richardson, the repository safety strategy that you're going to hear about next. Actually, that is the purpose of that work, is to define what needs to be done next. My just being a PA type person and looking at Bob's results, I would say that the highest priority is to solidify the case for the way that the waste package works. I think there is reasonable doubt in the minds of some experts as to whether we can sustain that case through licensing. So I would say that is a very high priority.

I have a personal feeling that we should also look very closely at the seepage model, because the indications that we have of preliminary measurements in the TRB drift, the east/west drift, is that the 70 per cent of the repository will be in rock that will be one to two orders of magnitude less likely to see seepage than the rock that we have tested so far. And so from my perspective, this is a great opportunity to adjust the modeling and lower that curve beyond 10,000 years. And so those are two items that I would put on my list, and then also I have several favorites, extensions of John Stuckless' work in natural analogs I'd like to pursue to show that the modeling that we're doing of seepage is probably conservative, to put it mildly.

KNOPMAN: Let me just ask one other question somewhat related to this. And that is that as long as the assumptions about waste package behavior hold and you're not really looking at failures until 40,000 years out, then it seems to me it's largely irrelevant what happens during the thermal pulse.

VAN LUIK: That has been my position for some time, and you put the words right in my mouth.

KNOPMAN: I mean, I don't believe that, but I'm just-- that is the logical extension of what you've been saying.

VAN LUIK: That is the logical extension of what I'm saying, yes. If we can sustain that case, then what happens in the first thousands of years is irrelevant to the, you know, 10 to 40,000 year performance.

One more item that I forgot to mention on the list.

There seems to be an opportunity for dropping the concentration of radionuclides travelling from the waste package into the unsaturated zone by looking at the secondary mineral formation and the likelihood that radionuclides would be trapped in them. This is kind of the phenomenon that you see at Pena Blanca, for example, where after millions of years, the oxides of uranium actually contain a lot of the radioactivity that could have gone away but didn't. Of course, a lot of it has gone, too. But that's the kind of thing where we need some insights from systems that have been around a little while to match with laboratory observations.

So there's basically three areas; waste package, waste form behavior, and seepage to me are the three highest priority items, and I don't know what the RSS results are because I haven't read the latest version. But I bet they're among that list that we'll be showing you in a few minutes somewhere.

CRAIG: Okay. Seeing no other questions, thank you very, very much, Abe.

EWING: More comment I guess than a question, but you might respond. In your list of your approaches to dealing with uncertainty, one thing that's missing from the list is an analysis of how the uncertainty propagates through the analysis. That's a very simple example for water/rock interaction. Say you wanted to know the pH, then there's some uncertainty in terms of the mineral phases present, the amount of water present, the temperature, the temperature dependence of reactions, and so on. And all of those factors come from other models. They have an uncertainty, and so the calculated pH will have an uncertainty band with it, even before you do the probabilistic analysis. Do you have any plans to look at how the uncertainty propagates through your analysis?

VAN LUIK: I think Bob showed in his table and in his examples that to the extent that the process model and the abstraction pass through the uncertainties, they're fully incorporated into the TSPA model.

EWING: Now, I'm saying something very different.

VAN LUIK: Okay. Then I misunderstood you.

EWING: I'm saying that all of your 400 parameters, your input parameters, half of them sampled over a range. Each of those parameters has a certain uncertainty.

VAN LUIK: Yes.

EWING: And in a normal scientific analysis of very simple systems, we routinely track the uncertainty as it propagates through the analysis, and it grows very quickly. The mean values may not change very much, may be useful, but as you extrapolate over space and time, you expect that uncertainty to grow. And the, you know, what has been presented to us where you look at the range of the 5th to

95th percentile, that's not at all the measure of the uncertainty of your models. If you stand 20 kilometers away and sample the water in a well and calculate a dose, you're not capturing at all the uncertainty of the models used in the performance assessment.

VAN LUIK: I think I understand what you're saying, and I think that's one of the reasons that we have this test for, is looking right at the 121 AMRs and the abstraction AMRs to see, one, how was uncertainty treated in those AMRs, two, how is it propagated out, and do we need to change or add to the way that uncertainty is treated at that very low level that you're talking about. And that's what this whole task force is about. I just showed two examples where we evaluated two models, which are actually parts of clusters of models addressing larger issues. So I think we hope to be getting at exactly what you're talking about.

CRAIG: Okay. Abe, thank you very much. We will now call this session to an end.

COHON: Thank you, Paul, for your fine job of chairing this morning's session.

Though more than two people have signed up on the public comment sign-in sheet, my understanding is there are only two who have to leave early today, and they're Judy Treichel and John Hadder.

Is there anybody else who wanted to make a comment today and will not be able to stay until the 5 o'clock or so comment period?

(No response.)

COHON: Seeing none, then I'll call first on Judy Treichel. Judy?

TREICHEL: Thank you very much, and especially thank you for changing the schedule after everything sort of got imposed on us at the same time.

It strikes me as I sit here and listen to this, and I've been doing it for a very long time, that the Yucca Mountain project is a terrific one for doing field work, for doing lab work, for doing all sorts of important, interesting science. But when you start showing viewgraphs and talking about receptors, that's where it all changes, because you can do a whole lot of guesswork and you can do a lot of possibilities, probabilities, TSPA, all of that sort of thing, but if it's with the intent of then putting it onto an unwilling receptor, or a person who you've actually met, I think it's wonderful that you've gone to Amargosa Valley to have meetings, you know Michael Lee, you know the McKrakens, you know a lot of those people, those are the receptors, as well as their children and their grandchildren and people who come on, and I think this is a dreadful thing when you look at it that way.

When Ivan Itkin was standing up here, he talked about how they're working to finalize the regulatory framework. There was a regulatory framework when we all started on this thing, and of course we were assured for years and years and years that that was in stone. Yucca Mountain had to crash up against that and survive. And, of course, you know that that's not the case.

Also, when Dr. Itkin was asked about what is the design, and he should certainly be able to tell all of us what the design is, and my next statement isn't necessarily all mine, I've been discussing this with other people, but what comes down is he made the statement that right now, we're talking about the Wright Brothers airplane. And what he's expecting us to swallow is that when this thing gets done and gets built, he will have somehow magically built the space shuttle that we can all be absolutely confident in.

And even if it turned out to be the space shuttle, and I don't have any confidence that it will, you shouldn't be marching people at gunpoint into that thing against their will, and then flying it over their kids against their will. This whole thing is crazy in what we're seeing, what we're talking about, and the fact that people are going to be forced to accept it as being true.

When the presentation was given by Drs. Barkatt and Gorman, they talked about problems that had already happened with some fairly fancy metals, and it happened in nuclear reactors, and the big difference is that you can afford some trial and error when you're doing a nuclear reactor. You can shut if off. You can fix it up, and you can turn it back on. That's not the case with Yucca Mountain.

The questions come up here many times, well, what do you, with various presenters, what do you think you need? What kind of work do you think should be done? And each one has answered you, and yet we're screaming toward this site recommendation. There's a lot that's still needed. There's a lot of work still to be done, and there probably always will be.

I think it's dreadful the way that those charts were diddled with so that when you were looking at doses, if you didn't know and if you didn't ask the right questions, and thank God the right questions were asked here, that you had doses going from a picture that you could look at from 100th of a millirem to 3 rems. And that's part of this risk performance based stuff that we're supposed to fall in love with, and we're not. And the old guidelines that I mentioned earlier would not have allowed that.

I don't think that I've seen anything having to do with defense in depth. First, we were told the mountain was perfect. You could toss the stuff bare naked inside of it and it would be just fine. Then we were told that C-22 would last forever. And as we've heard, there's serious questions about that, in fact, outright failures. Now it's all hinging on titanium and the 40,000 years seems to be a given. There is no given 40,000 years. If somebody looked hard enough at titanium, it's probably not going to stand up either.

And just finally, the evaluation of uncertainty, as Abe was just talking, is supposed to be coming in in November of this year. That coincides--well, maybe it will be in the same package with the SR/CR. I think these things are really piling on. I think it's unfair. I'm not sure as a public advocate, I'm still talking to other public advocates, what we're going to do about the SR/CR, but I doubt we're going to do very much.

And just as a final statement, none of this has to happen. It doesn't matter that Yucca Mountain is the only site. We're just not ready to do it yet, and we aren't solving the problem. We're clearing space for new waste.

So thank you.

COHON: Thank you, Judy. Now I call on John Hadder. If you would state your name again and your affiliation, if you like, so we have it for the record?

HADDER: My name is John Hadder, and I'm on staff with Citizen Alert out of the Reno office. I appreciate this opportunity to speak, and it's been quite interesting, all the information that's been presented. I agree it's impressive. It's also very confusing, and I should point out that the same kind of information was presented in a similar manner at lot of times at the hearing with the public, and they're not often of technical background. So that problem needs to be seriously addressed in the area of public confidence around this entire program, because there is almost none, and certainly almost none in Nevada.

I want to state for the record that Citizen Alert is very concerned about the public process around this considerations report. What we do support is public hearings around a site recommendation report that contains all the information that the President would see, so that the public's comments that would go to the President are meaningful, and that the time is not wasted.

One thing that has happened a lot in Nevada is--and I'm sure it's true in other places as well--is the public has felt frustrated by coming to public hearings and making comments and feeling like they haven't been adhered to or they haven't been listened to or their time has been wasted. This again addresses the problem of trust.

We all know this is a political solution to the problem, but the public should be involved on the radioactive level. And it should be meaningful.

So we do not trust basically the process around the considerations report, but we would very much welcome, and by law, a hearing around the site recommendation report, period.

Also, the final EIS won't be available until next year either, so the public will not have a chance to look at how the DOE responded to its comments around that. That is also very unfair. It's very disrespectful to where the public is at in this whole process.

And in regards to the total system performance assessment, again, this is another one that the public neither understands nor trusts. I think that the big elephant in the room are the guidelines, the guidelines that still exist to this point, which do have actual conditions based on the physical characteristics of the site itself. This is something we can kind of understand. And also Citizen Alert recognizes that a TSPA is a valuable tool and could be very useful, and we don't disregard that its work is important to the Yucca Mountain project. However, we don't see that it should be used exclusively in determining the suitability or the regulatory procedure around Yucca Mountain.

Our recommendation is why don't you use the subsystem performance criteria in tandem with the TSPA. Wouldn't that better protect the public? Wouldn't we have a better sense? Wouldn't we be better, more confident in what we're doing? We've never really gotten a good answer to that.

I want to also state that be careful in all this science that we don't dive into the Oppenheimer Syndrome, as I call it, where we lose track of what we're dealing are real people that will be affected by this. I think Judy spoke to that briefly. Science can be very interesting, but remember there are people behind all implications of this, and I appreciate that the Board will take that very seriously.

We certainly do in Nevada appreciate the Board as an ear for concerns, and to really evaluate what's going on objectively. We haven't seen a lot of objective evaluation in other areas.

There are a couple--I have a few comments around the discussion of--technical comments around the discussion of C-22. There was the idea that there was certain information that was not understood by the nuclear industry and their realistic range of material conditions and stresses. I'd like to point out the possibility that maybe more was understood than we think, and that the nuclear industry is possibly driven by profit. I know it's an ugly word, and I know that we don't want to admit to that, but these thing happen. So let's be aware of possible uncertainties in the process that are based on maybe less than honorable intentions. It does happen and we have to face up to that fact.

Also, too, I wanted to point out something that was brought up regarding the assumptions and results around the components of the waste form degradation model. At one point in the discussion, there was a plot shown, which is the neptunium solubility versus pH, and they used three points to validate a model. This was used for thermal-dynamic data as a reference. Three points? I hope this is not common in the project that only three data points are used to validate an actual model. To me, that's scantily short information. Certainly when I was going to school, I would have been laughed out of the classroom for that.

And, again, I also agree that dose rates, and so forth, should be represented in a realistic manner so the public can understand them.

I appreciate the time. Thank you very much.

CRAIG: Thank you, Mr. Hadder. We will now adjourn for lunch, and reconvene at 1:15 for the afternoon session.

My thanks to all the speakers for their contributions this morning.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

CHRISTENSEN: Good afternoon. I hope you've had a good lunch and are well fed. My name is Norm Christensen, and I have the honor of chairing this final session of the Board's summer meeting.

Before you, are all of the speakers from our previous sessions on TSPA/SR. For the most part, and against our core instincts to do otherwise, we have limited our questions to these folks to issues of clarification. I emphasize for the most part.

We will now submit to our core instincts and I know that many on the Board, as well as our advisors, have important questions and comments for this panel.

You might recall that Rod Ewing and John Kessler are here as advisors to the Board on TSPA-related issues, and that Bill Melson is here to help out on questions related to volcanism and its effects. And John and Bill, I hope you'll feel free to chime in on these questions, and for that matter, on any other issues that have come up over the last day and a half.

I'll come back to the panel in a moment, but I want to point out that following the panel, Dennis Richardson will discuss the latest version of the repository safety strategy, or the RSS. This strategy is the set of structured arguments that the Department of Energy will use to convince us, the Board, the administration, the Congress and the public, that the repository is, indeed, safe. And as such, it's obviously very important.

The Board is especially interested in the non-TSPA elements of the repository safety strategy, in particular, issues related to natural analogs and their actual use, defense in depth, and issues of safety margin, and the Department's views on principal factors, that is, those technical factors most important in determining post-closure safety.

General plans will be presented by Dennis on work that the Department feels is important before it proceeds to licensing, if indeed Yucca Mountain is recommended as the site for a permanent radioactive waste repository.

Abe Van Luik will close this technical session with a wrap-up from the Department of Energy on the performance assessment.

I will then hand the meeting back to Chairman Cohon for our public comment period, and would like to point out that if you would like to, that is, members of the audience and public, would like to ask questions or make comments during that session, please sign up with either Linda Hyatt or Linda Coultry at the table on my left and your right.

You may also provide them with questions during this session, written questions that we will try to, if we can fit them in, address to the panel and presenters.

Okay, let me come back to the panel. Our rules for this session will be relatively simple and relatively open. I'll try to keep close tab on the sort of queue of questioners among the Board and the panel. Board members and our advisors will get first shot, and then followed by the staff, and if there's time, we may be able to take questions from the public.

I will try to be careful on the order of questioning so we can keep everyone in the queue, but I will want to, as you're asking questions, if there are particular questions directly related to a particular question, that we try to deal with those sort of in one set so that we have a more coherent conversation. So I would ask the Board members as they're posing initial questions in an area, to keep them relatively broad, and then if individuals want to chime in on something very specific to that question, that that would be appropriate.

Ordinarily being the shiest member of this Board, I will exercise actually chairman's prerogative, and I would like to ask the first question to open this up, and then I'll take my seat and act more like a chair.

This is probably a question directed most specifically at Dr. Pasupathi, and relates directly to issues of waste package performance. Until recently, nearly ever performance--or every presentation of performance that I've seen has showed some radionuclide release prior to 10,000 years. That is particularly true in the TSPA/VA.

Not withstanding issues related to volcanism and seismic activity, we now see no release under any scenario until after that time. As near as I can tell, there have not been really major changes in the waste package itself, and so one might ask in a sort of cynical vein whether this is simply a matter of knob twisting of the models, which moves the degradation of the waste package out to a later time.

More positively, what I would ask is specifically, and this may be to clarify things that you covered yesterday, what have we learned since VA that makes us now more confident that we really won't see any so-called juvenile failures, or failures in the first ten millennium of the operation of the repository?

PASUPATHI: Let me try to answer the question as broadly as I can, and hopefully I can get some help from several of my colleagues who are seated in the audience.

First, we do have quite a bit of a different design in waste package compared to the VA design. And going back to the juvenile failure, we did not really have a model, so to speak, for juvenile failure in the VA. As I mentioned in my presentation, some of the assumptions and the choice of how many failed, when they failed were somewhat arbitrary and based on data that aren't particular relevant to the fabrication of the waste package and the process that we're going to use. So that's one reason we do not have early failures at the time, same kind of time frame that we had in VA.

In the current model, we do have a basis, we believe we have a technical defensible basis for the early failure scenario. And looking at all of the probabilities of different aspects of fabrication, human factors, and all, we believe that the manufactured flaws in the weld is the only aspect of waste package design that could contribute to early failure. That, too, it says that when you have defects, just the defects by themselves are not going to go and cause a failure on day one. You need to have an additional mechanism, such as localized corrosion or stress corrosion cracking, to have a defect propagate into a true wall failure. So that's what we have built into our stress corrosion cracking model, and the results of that model show that the--our of the 100 realizations, or so, you get the earliest failure starting around 11,000 years.

CHRISTENSEN: Let me be clear then that the main thing, it sounds to me like, that has changed then is the extent to which human error in fabrication plays a role, or the fabrication process. Is that where the main assumptions are?

PASUPATHI: No, they have been taken into account in the current early failure model. There was an analysis done in AMR on that subject, looking at all aspects of human factors, all aspects of manufacturing the waste package, and it turns out the closure weld flaws happen to be the only ones that could lead to early failures.

CHRISTENSEN: We'll go with Paul, and then with Dan Bullen.

CRAIG: Yeah, this exchange reminds me of a section in Richard Feinman's book on the Challenger inquiry where he asks several engineers what the probability is of failure, and one of them writes down zero, and some of the others give some numbers which are different from zero, not very big, but nevertheless different. And from this, Feinman goes on to talk about a certain management mentality.

When the probability of failure is zero, one really does have a reason to worry. It would be very useful to, and I'm now asking you if you would either say that you really do believe the probability of failure is zero, or else give me a number.

PASUPATHI: No, we're not saying the probability of failure is zero. When it occurs is the time frame we are calculating on the basis of what we have. In other words, the failure does occur at 11,000 years, for example.

CRAIG: No, no, I mean specifically failure prior to 10,000 years, and you seemed to be stating very clearly that the probability of that is zero. Am I wrong?

PASUPATHI: No, it does occur at 11,000 years, and no failure occurred below 10,000 years.

CRAIG: Let me repeat it. I'm asking about failure before 10,000 years, between zero and 10,000, and the statement that you appear to me to be making is that the probability of that failure is exactly zero. Is that correct?

PASUPATHI: No.

CRAIG: If it's not correct, then what is the proper number?

PASUPATHI: I'm sorry, let me have Bob Andrews answer that.

ANDREWS: It's not zero. It's a very low number, and what drives that very low number, because we can push, you know, with the distributions on flaw sizes and flaw uncertainty, defect size, defect uncertainty, the rates that we have, the stresses and the uncertainty in the stresses, it's clearly possible with a very low probability to have pre-10,000 year failure. So it's not zero. However, it's a very small number. It's maybe 10 to the minus 5, 10 to the minus 6, something in that order. If we look at the flow and defect distributions, I don't think Pasu showed the actual curve of them, but it's in the supporting AMR. He summarized it in his table. The probability of having a flow of sufficient size to be through wall at the weld from those observations is less than 10 to the minus 8. So, yes, it's possible, it's greater than zero, but below the kind of 10 to the minus 4 regulatory concern. But it's not zero.

BULLEN: Bullen, Board. Actually, I have some followup for Pasu here. When you're evaluating stress corrosion cracking, you are emphasizing the stress relief at the final closure welds of the inner and outer lid. Do you have any mechanism to take a look at residual stresses that may be endemic from just the manufacturing and processing, grinding, handling, bumps, dings, whatever happens? And how do you handle that as another driving force for the initiation of a surface flaw?

PASUPATHI: As the cylinders are being made, we plan to anneal all of the cylinders. The only ones that would not be annealed would be the final closure welds, and that's where we are doing the mitigation steps on those. As far as handling and other things of concern, those were addressed as part of the early failure mechanism using human factors values.

BULLEN: So you've incorporated that part using the human factors evaluation.

PASUPATHI: Yes.

BULLEN: Actually, maybe you could be a little bit more specific. When we looked at the VA design, there were other mechanisms to allow the canister to fail, and they were contained in the Waste Package Degradation Model, WAPDEG, and I assume that those failure mechanisms are still there, localized corrosion, general corrosion, crevice corrosion. You mentioned that the ones that you're having operational now, or that are operational, are stress corrosion cracking, aging and phase stability, MIC effects you listed in your Number 6 viewgraph, and then potential effects, radiolysis and then the bounding conditions on the environment on the waste package and drip shield. You use the FEPs process, the features, events and processes to toss out, because they were low probability of occurrence events; is that how you screened out not having localized corrosion, crevice corrosion, general corrosion in this?

PASUPATHI: No, sir. The general corrosion model is in the WAPDEG, and so is the localized corrosion model. And there we are looking at the critical potential for corrosion, localized corrosion, and the threshold potential for localized corrosion. There is a model in WAPDEG. It compares the pH and the potentials required to cause localized corrosion. If the potential is not exceeded or the delta is not there in the positive range, it doesn't turn the localized corrosion on. So the model does exist.

BULLEN: Okay. And then do you also have a model for radiolysis?

PASUPATHI: No, there is no model for radiolysis.

BULLEN: And that was screened out by FEPs?

PASUPATHI: That was screened out by FEPs, yes.

BULLEN: I'll just express my concern. And you always note it. But I think you might want to take a look at that, particularly in light of the fact that you're loading packages that have a pretty high surface dose rate in a potentially moist air environment. It's going to be humid in there.

PASUPATHI: As far as the radiation dose rate of the surface, or the dose levels of the surface, the highest number I've seen for 21 PWR case with the fairly hottest fuel, I would say, five year cooled fuel, 70,000 megawatt burn-up, is about 1200 rem per hour. That is as loaded.

BULLEN: Okay, 1200. So that's down from about 3700, which is the last number I had in my head.

PASUPATHI: Right, it is down, and also after 25 years or so, it goes down to in the hundreds rather than thousands.

BULLEN: Right. Any chance that you're going to have a shield plug in the top of that so you can rework that weld?

PASUPATHI: Don't know.

CHRISTENSEN: Dr. Cohon?

COHON: Thank you. I wanted to follow up on the point I started to make during Abe's presentation before the lunch break, and I promised that I would try to come up with some additional specific examples to try to demonstrate this point, the point being that there's danger in artificially, my word, artificially, bounding or limiting the range of uncertainty with regard to certain parameters by using TSPA performance results.

Let me try out two. One, in Kathy Gaither's presentation, you made the statement that--and it was brought up again in questioning--that though you would see or predict a ten-fold increase in fault aperture, that would have no impact.

Now, the question is when we say--when you say, when you conclude that there's no impact, does that mean no impact on dose, or no impact on water flow?

GAITHER: It's both, in my opinion. I'm going to let Bob discuss that in detail.

ANDREWS: Yeah, I mean, the answer is--I think Kathy is right. It is both. If there's no effect on flow, which is the process that changed in this case, we've changed flow properties, in this case, permeabilities or apertures or porosities, and that change, albeit may be large and may be local, did not change the flow, because the flow in this system is driven more by the boundary conditions, in particular the infiltration rates, the climate state, not by the properties of the rock per se. It's how much water is moving through the system that affects the system performance, and if it doesn't change the flow, then it won't change performance.

COHON: Yeah, please, save me the trouble and you the time. You don't have to explain that to me. The question was are we talking about no impact on flow or no impact on performance? And you've answered it; no impact on flow.

The second example comes from Christine Stockman's presentation. This is the problem of not yielding to our base instincts during the presentations, because now we don't have the slides up. The diagram you showed of--it's Number 5, the pH over time, does that depend on assumptions made about seepage flux?

STOCKMAN: Yes. The reason I was saying before that there was a larger uncertainty in the process model runs was because there's a wide range of seepage in the process model runs. In these runs, there's almost--there is no seepage before 40,000 years, and then after that, it's very minor. So all the uncertainty from seepage is not showing up in these TSPA runs.

COHON: But does that have implications then for how uncertainty is represented within the I want to say base case, but that's not what you call it. You call it nominal case, I guess. The way you represent possible ranges of pH values, is that then influenced by what you just said about seepage flux?

STOCKMAN: Correct. If the seepage in the nominal case was a lot higher, you would be sampling much more neutral pHs, and you'd see the broader range of uncertainty in the outcome.

COHON: Then the question is isn't this seepage lower as seen by Christine's model because of the waste package?

STOCKMAN: Yes.

COHON: So here's an example where the design--yeah, because of the drip shield. So you see this is an example. This is exactly an example of my point. And it's a little bit troubling, especially in light of the presentation we received about the work from our visitors from Catholic University and elsewhere--go ahead, Dan.

BULLEN: At the risk of really putting my career in jeopardy, I'm going to disagree with you.

COHON: Yeah, that's true. When was the last time I fired a Board member? Hey, Bill, can I fire Board members?

BULLEN: Have to wait till the election is over and get the new President.

COHON: Yeah; right. Go ahead.

BULLEN: Why can't you take credit for the design? I know you're talking about reducing uncertainty.

COHON: Here's the point. It's a subtle point, but it's a crucial one. Taking credit for the design should mean that you get this performance because of the design. It should not mean it changes the way you represent physical processes in TSPA. That's my point.

BULLEN: But I have a question. Don't you just turn that physical process off with the design?

COHON: What if the design changes? What if we don't know as much as we thought we did? What if titanium drip shields in fact could be misplaced so that water can get through them?

BULLEN: I agree with that uncertainty.

COHON: That's my point.

BULLEN: But I guess I don't see it wrong to turn off a mechanism if the design mitigates or adapts for it. Otherwise, you wouldn't be able to take credit for any design.

COHON: You know, I'd be more comfortable if you actually turned off the mechanism rather than changed the way you represent it in the model. You limit the range of uncertainty.

BULLEN: As would I. If they turned it off, I would agree with you then.

ANDREWS: This is Bob Andrews again. We have to be careful that when you're in a process model and they're developing a response surface, which is what Christine is talking about, a response surface that says the chemistry is a function of seepage, which is what they've done, and the seepage they say, well, I don't know what the seepage is, but I know it's a function of seepage, so let me run this process model over a very wide range of possible seepages, and that's what they did, and I think did it appropriately and correctly.

Now you come to the integration tool. You come to the performance assessment, and you say, well, that's nice that you ran this over a wide range of seepages, and in fact we asked you to do that, because we didn't know what seepage we were going to get, but when we implement it, we know what seepage we're going to get, and it's within, you know, it's still a band, but it's a narrower band than Christine ran her process model over. Thank goodness. I mean, thank goodness our band of actual seepage uncertainty is well constrained within her total band that she did her process model on.

COHON: Let me interject a specific question to help me with nomenclature. What you just described, is that the abstraction process from a process model?

ANDREWS: It's the abstraction and the integration in the TSPA.

COHON: Yes, I understand. But the process model then you're saying has a wider range of uncertainty. But in abstracting from that for the TSPA run itself, you may narrow the range of uncertainty.

STOCKMAN: We actually didn't narrow the range in the abstraction. If we put in a high seepage rate, we would have gotten a much different pH range. So the uncertainty is in there. It just was not sampled in this TSPA.

COHON: Now, is that the same thing you just said before, though?

STOCKMAN: I think I may have, after talking to some people, I think that maybe the way I spoke about it was a little confusing. There is only some loss of information in the abstraction process, but the full range is there. If the full seepage had been sampled in the PA, we would have seen the full range of uncertainty in the pH output.

COHON: So if you suddenly got a call from Bob saying we've decided to take out the drip shields, your model-- everything you've done up to now would still be applicable to the next runs?

STOCKMAN: Still work, yes. And you'd see a much wider range in the co-disposal pH.

COHON: Okay. It sounds like I still haven't come up with an example where this is really a concern. I have a whole line of questioning about heat, but I'll wait because that has another--

RICHARDSON: Dennis Richardson. I'd like to just add a comment onto your question, if I understood it right. If we have parts of the design, say the engineering design, that we take credit for in terms of perhaps mitigating water or whatever, that would have to be clearly identified in the licensing application, and the basis for that would have to be identified. If later on we found that we made a mistake or we had to change that, we would then have to identify that change by law to the Commission, and we might even have a reportability to look at, because anything that would be against the design basis, or violate the design basis, immediately has to be reported and have to be re-analyzed.

So there is protection for the Commission. The applicant must do this, and any of the bases for either the natural or the engineered design that we credit has to be clearly identified, and we have to show that we're always within the bounds of that basis. So from your point, I think there is--we certainly should credit what we want to credit. But then the applicant again always has to show that that basis is sound.

COHON: My point really has nothing to do with that. I understand that, and I'm sure that you will document fully any credit of that sort that you take.

My question is purely a modeling issue. It goes back to TSPA and the way it works. But I'll defer to someone else for now.

CHRISTENSEN: Dr. Sagüés, and Dr. Wong is on deck. And maybe we could just ask everyone if you do come to the mike, to just say your name before you speak so that when we do the official transcription, we'll know who was speaking. It's a very confusing and large group.

SAGÜÉS: Alberto Sagüés, and I have the feeling that they could identify me without the need of saying the name. But anyway, this is a question to Pasu, but then again, we may hear answers from some of the other members of the panel.

Specifically, from Dr. Bullen's question, I understand that localized corrosion is indeed set up as a module of the waste package degradation program. But do I understand correctly that that particular path does not get activated because the conditions are never presented to trigger localized corrosion? Is that the way this is set up?

PASUPATHI: Yes, that's correct.

SAGÜÉS: Okay. So then my question has to do with the reasons that you provided here in your presentation as to why localized corrosion is not included, and one of them is that specimens with geometry in the long-term test facility, the tanks, right, at LLNL showed no evidence of localized corrosion. Now, first of all, those tests that showed no evidence of localized corrosion have been going on for, what, two years, three years?

PASUPATHI: At least two years.

SAGÜÉS: At least two years. And needless to say, we're talking about extrapolating that kind of information, if that is the information that we use to make the decision, we're using that for an extrapolation into the 10,000 to 100,000 years regime, and I think that that--I would say that unless there is a lot of additional explanation to it, I don't see the technical justification for such an extraordinary extrapolation of results if it is based simply on observation.

One thing that is not being collected in the long-term test facility is the open circuit potential information for those specimens, which is, as you know very well, a crucial piece of information. If for some reason those specimens are developing a field negative potential, you're not going to initiate localized corrosion. They're going to be protected. So before I continue, I have two other points, what would be your observations on that?

PASUPATHI: I'll try to answer, and I may need some help from Dr. Gordon and the audience also. The localized corrosion model is not just based on the two year corrosion data or the specimens, crevice specimens looked at from the two year data. It also is based on the cyclic polarization test done with those three media. In addition, we have added the saturate solution as a media also. This is approximately

15,000 J-13 in terms of chloride concentration.

And looking at that data, we find that the threshold for the localized corrosion is not exceeded under these conditions with these environments. Okay, the tests were also done up to 120 degrees C. with the saturate media. So that is the basis for the model, and the two year data is only a corroborative evidence. And in addition to that, Dr. Farmer had done a crevice corrosion test using multiple crevice forms with the basic water solution, as well as lithium chloride that we looked at, and he has not found any crevice corrosion in any of these samples.

SAGÜÉS: You are aware, of course, that the cyclic polarization test, and that was my second observation, the tests are conducted--in which you get a specimen in a very small surface area. You take it to a condition which is quite unnatural. First of all, you strip out the oxides from it, and the like, or maybe you start from the open circuit potential, and then you run a scan up and down. The test is finished in a few hours. And then maybe you can do a dozen of these tests, maybe a couple dozen of these tests. But that by itself is again a very limited base of information to make a decision on what the performance of the material will be over, again, this extraordinary long period of time.

So basically--well, in addition to that, the cyclic polarization tests have to be taken together with some kind of an assumption as to what will be the open circuit potential of the material, again over the long-term. And again, as you know, the open circuit potential of stainless steels and alloys of this type tends to creep up with time, and we don't know at this moment what will be the long-term evolution of open circuit potential. It could be creeping up and creeping up, maybe aided by things such as radiolysis on the surface of the material, and then it could conceivably get into regimes where localized corrosion could perhaps be triggered.

PASUPATHI: I believe Dr. Farmer took into account the effect of potential changes due to radiolysis, in addition to what he was doing with the cyclic polarization. I don't know if Dr. Gordon can add any more to it in terms of using the cyclic polarization test results.

GORDON: Jerry Gordon, M&O. In addition to just doing the cyclic polarization tests, the margin between the breakdown potential and the open circuit potential was several hundred millivolts in these range of environments. So even if the potential drifts up, for example with the hydrogen peroxide, it went up as high as 200 millivolts above open circuit, that still left a lot of margin in terms of the breakdown potential for the passive film. We are doing more testing and longer term testing to confirm the results.

SAGÜÉS: Okay, thank you. That's part of what I wanted to aim at, that is, that maybe the amount of information that we have available right now is still quite limited. A 200 millivolt swing in the open circuit potential, although fairly large, is not something that could be completely ruled out on the basis of available information.

The main issue that I wanted to bring up, and I'm going to finish with this, is shouldn't these models include some kind of allowance for the chance that these assumptions, implemented or not, could be wrong, that building it mathematically in some fashion, you could establish sort of a probability, quantitatively, that this switching, for example, of corrosion may not be right, and then building that eventually into an adjustment to the expected dose rate?

PASUPATHI: I can answer it this way. The localized corrosion model currently relates the corrosion potential to pH, expected pH of the solution, and that is taken directly from the EBS chemistry model that comes into contact with the waste package. So the uncertainty in the pH is built into that model, and that's what's imported into WAPDEG.

SAGÜÉS: Just one way to do it, of course.

PASUPATHI: Right.

SAGÜÉS: And there may be many other things that may affect the value. But then again, I didn't want to exceed my portion of the time here, and maybe I can leave it at that.

CHRISTENSEN: Dr. Wong, and if I don't have anybody else, I'm going to return to Dr. Cohon. Jeff, Debra, Rod and then Jerry.

WONG: Okay, I have four questions, and they're all unrelated, but I want to ask all four questions, and then you can answer that. And I want to do that before Dr. Bullen starts arguing with Dr. Cohon again.

Number one is, the first question is related to the biosphere. Again, it's the issue of why the soil ingestion pathway becomes dominant in the disruptive event scenario. I can see that a larger contribution to a soil concentration in the case of the disruptive event is obvious to me, and I can speculate as to why the soil ingestion pathway would become dominant, but I don't want to guess. So I'd like an explanation of that. That's my first question. I'll go to the next question.

The next question is related to the saturated zone presentation, and I saw this list of data used for model calibration and validation, and as I listened to the presentation, for a person like me who's not a modeler, it seemed like all of the studies that were presented were related to calibration. So what part was related to validation? That's my second question.

The third question for the group is we saw each one of the key attributes of the repository, and we saw the analysis of enhanced barrier and degraded barrier for each one of those attributes. Are you going to present the whole enchilada with all of the total system integrated with Goldstem so we can see a final dose output for the entire system?

And the fourth question I have is related to peer review. You had peer review in the VA, and the peer review group pointed out a number of deficiencies or issues related to the VA. Are you going to do a peer review of the SR? It seems like that that would be logical because it's a really important document, and you would want to make sure that none of those issues that were originally pointed out in the VA persist in the documents such as the SR. So those are my four questions.

SCHMITT: I'll take one of them. This is John Schmitt.

The question regarding biosphere and the concern about why is it that soil ingestion and inhalation are so dominant for the volcanic eruptive scenario? I've been digging to be able to answer this, and I've got a multi-part answer. Let me say that in my slide, my Slide 11, I talked about the sensitivity results for the volcanic eruptive scenario, and indicated that what we found is that soil ingestion and inhalation dominate for most radionuclides. And, indeed, that's true.

Perhaps I should have gone on further from there and say that for a lot of radionuclides, the third most dominant contributor to the biosphere dose conversion factor is leafy vegetables. And we saw leafy vegetables be very important for the nominal case, too. And, in fact, for seven out of twelve of the radionuclides that I've got in this table, I'm in the PMR on Page 3-66, Table 324, for seven out of twelve of these radionuclides, this third parameter, this third in the priority of parameters, comes in in the range of 10 to 15 per cent contribution to the BDCF. So it's not negligible. So I probably should have gone on and talked about that some, and not just stopped with soil ingestion and inhalation. So that's kind of an answer that goes to extent of the statement I made.

But looking at what goes on, the mechanisms that go on, soil ingestion is not as important in the nominal case because you've got the source of contamination is from the soil that is contaminated by potentially contaminated groundwater on the irrigated land, on the farmed land only. And so you've got a less distributed source term. In the case of the volcano, you've got the contaminants all over, on all the land, not just the farmed land. And in the case of the nominal scenario, you've got this contaminant on wooded land also. So there's less chance for the soil to get into the air, although as it dries, it would.

In addition, as the people recreate, they might recreate on land that has been contaminated by the volcano, but they probably would not recreate out in the alfalfa field, you know, in the irrigated and farmed lands. So those are some of the mechanisms that go on that cause it to look this way. But, again, that needs to be combined with the fact that I probably somewhat overstated what was going on, Jeff. Does that take care of it?

WONG: thank you.

CHRISTENSEN: Before we move to your other three questions, Jeff, Dr. Parizek has a couple of questions directly related to this topic.

PARIZEK: Parizek, Board. On biosphere issues, there were two things that were of concern to me. One, you had the present climate only as part of the assumptions in the biosphere modeling. And that may have something to do with the flow field dynamics on the one hand, plus also I guess crop uses and so on. The other was whether the soil variations are considered. Surely the uptake by various soil types that might be present in the farmed area around Amargosa farm region could be quite variable.

As a result, a build up of radionuclides wouldn't be uniform, sort of like the Chernobyl example. There's quite a variation in terms of where radionuclides are, what plants take out of the soil. And so do you have a uniform homogeneous soil for the whole place, or do you have variable soil? And should you have variable soil if you didn't include that?

SCHMITT: Right. For the PMR and the analysis model reports, as they are constructed at this point in time, we do not have a variability in the soils as far as plant uptake. So we did not go into that level of detail. We know from sensitivity studies that the transfer from soil to plant is not very important as far as varying the BDCF. But we don't have--we did not do growing in ash, you know, the transfer coefficients for growing in ash.

On the broader part of your question, if I got it right, the amount of rainfall that would accompany possible changes in the climate, as the climate evolves, as documented in the AMR on climate change, Bo Bodvarsson's Slide 7 showed the values and the periods when they might occur. But for the modern period, it's 190.6 millimeters per year, and then for the monsoon and the glacial, he gives values, the highest of which is 317.8 millimeters per year. So you're adding 130 or so millimeters per year, five inches perhaps.

So for the biosphere, what we'd need to look at is how important is it to exposure of people, the mechanisms by which people are exposed, how important, how different might it be if there were an additional four or five inches of rain per year. And on the face of it, there is not very much difference. You would need to irrigate less if you had more rainfall, irrigate less with potentially contaminated water, although it may not be as much less as you might think, because if the seasonal distribution of the rainfall remained as it is today, most of the rain as happens today would happen when crops are not in the field.

Additionally, that rainfall would have the function or have the effect of rinsing out some of the radionuclides that otherwise are collecting, banking within the soil, and leaching them out to a lower level in the soil, where they were not available to uptake by roots of plants.

So those are some of the types of mechanisms that would occur if we did hypothesize increase in current rainfall. We think we have a model in the biosphere that is conservative in that regard. As we saw in the other presentations, of course, the other models did include changes in rainfall. They have a significantly different effect on those models.

PARIZEK: One clarification question. Parizek, Board.

Are children in or out of the dose calculation?

SCHMITT: Children are out by the regulation. The regulation tells us, among many other things, that the receptor of interest shall be an adult.

PARIZEK: Thank you.

CHRISTENSEN: Is it directly related, Bill, to this?

MELSON: Yes.

CHRISTENSEN: Okay. And then back to Jeff.

MELSON: Bill Melson. In volcanology, air fall is what we see all the time coming down on people, and of course they evacuate almost immediately. Now, in the future, if you had some of the scenarios that have been presented, people are going to know the dose they're getting immediately, if there is any dose. Is that factored in? We can't pretend as if people are going to stay there, given any sort of significant dose.

SCHMITT: Okay, what we assumed was that the people would remain. In the TSPA, we assumed people would remain in the area. Earlier on, we were looking at self-evacuation. But we did away with that based on some discussions with NRC, among other reasons. Some of the logic for that is that if you have a volcano that its mode of eruption is really endangering people's lives, they will probably leave the area. If there's a lava flow coming in their direction, they'll get out of there. But when you have the case where it's only ash fall, which is typically what we're looking at here, or what we did in the biosphere, where you only have ash fall, people go about their business as long as they can continue to do that.

One analog is Mt. St. Helena. People more remote from the mountain where there was only ash fall went about their business and did not evacuate, and lived with the discomfort for a period of time of the increased ash fall.

So it did create a biosphere dose conversion factor for that period of time.

Now, it turns out that when you run the numbers in TSPA, that period of ash fall which the mean or the median, I forget which, value or length of time is 8.6 days. It's not a large period of time, compared with the year, a year for which you're doing the calculations. That dose is essentially lost in the noise compared with the rest of the exposure and dose then that they get for the remainder of the year.

CHRISTENSEN: Abe wants to chime in here.

VAN LUIK: Just a point of clarification. I would recommend that you read the Environmental Protection Agency's reason for choosing the adults, because it's a very well reasoned argument, with a good background that shows that if you look at a critical group or an exposed population, the average member or the RMEI, by definition, you know, the statistics of the group would be an adult. But they also look at the uptake factors for fetuses, infants, children and adults, and if you're looking at a committed dose for a lifetime, it is the adult dose that by far outweighs anything that at these early stages of life when you are a little bit more susceptible to it, but they don't last long. It's really a well reasoned argument for why the RMEI that they want us to use should be an adult. And I would recommend that you read that. It's not just oh, the EPA told us to do it so we blindly did it. They have a very good statement of why they chose that approach.

CHRISTENSEN: Did you have an additional--

MELSON: I think it's important to distinguish a cinder cone eruption, what's likely to happen in Mt. St. Helena. I mean, Mt. St. Helena was a really large eruption, which we have no records of in the Yucca Mountain area, and it's an important distinction, because I hear these little diddly cinder cones equated to things like Mt. St. Helena. That's a mistake. If it happened, they would see the cinder cone upon the slope most likely, and they would have sensations of what's happening and they wouldn't continue to run around. Certainly there would be an alarm, and I wouldn't ever portray that situation of people just continuing about their average life. That's not what they do, especially when they've never been exposed to volcanic ash.

SCHMITT: Okay. We as a conservative assumption in TSPA assumed that they would remain there. We also took no benefit for institutional controls. So anything that people did, they would do out of natural instinct and not directed by some governmental agency, or such.

CHRISTENSEN: Returning to Dr. Wong's questions, and let me just say that the next in order is Dr. Knopman, Dr. Ewing, Dr. Nelson, Dr. Cohon, and then Dr. Bullen.

You're probably going to need to go back and repeat your question.

WONG: I already forgot my questions. Saturated zone. Again, the presentation, it was Number 7, talked about using data for a calibration and validation and, again, it all sounded like calibration to me, so I wanted to know what was done to validate the model.

ROBINSON: Bruce Robinson. Let me define better the term calibration and the way I'm using it. When I'm talking about calibration, I'm referring only to an automated or semi-automated process in which one takes observations and adjusts model parameters to obtain a minimization of the least squares fit to the data. With that terminology for calibration, the datasets that we are calibrating to are the water levels and some of the fluxes from the regional modeling effort at the boundaries of the regional and site scale models. Those are the true calibration targets.

The other elements of the modeling, which I wrapped up in a term that I call validation, really gets at softer data, data that we want to make sure the model is consistent with, but isn't a true calibration activity in the sense that you're looking for a more qualitative consistency with the data rather than, you know, minimizing some function. And that one included the hydrochemistry, which remember only allows us to qualitatively map out the pathways. Another one is making sure that the model handles the upward gradient from the carbonate aquifer.

The reason that was important, and I'm not sure I covered it in my talk, is that radionuclides, if that gradient persists, that upward gradient persists throughout the entire model domain, that would mean radionuclides are kept in the upper few hundred meters below the water table. And so we felt it was important for the model to reflect that, even though the data are sparse on whether that upward gradient occurs throughout the entire model area.

So does that help you draw a distinction?

WONG: I understand the distinction. The issue that I was trying to get at was it sounded like you calibrated a model and you have hard data for the calibration, and you have soft data for the validation. So, in essence, you're not absolutely sure that you've calibrated the right model?

ROBINSON: Well, absolute, you know--

WONG: I'm just saying that you've calibrated a model, but your data that you used to validate the model as being the appropriate model is weaker.

ROBINSON: Right. I would say that there's various elements of the efforts at validating the model. So far, I've spoken mainly of large scale flow issues and getting the right flow directions and velocities. There's also validation efforts in terms of measurements at inter-well hydrologic and tracer testing at the C-wells, for example, which gets at the issue of whether or not we ought to be using a matrix diffusion model. That's a validation, that's a more pure validation exercise, in my estimation. You're demonstrating that a conceptual model agrees with the data and is well explained by the data.

WONG: Okay, again, the next question--well, maybe actually three and four could be played off of that issue. But, you know, are we going to get to see all of the calculations wrapped up? And then the issue of peer review, you used peer review in the VA. Are you going to use peer review again? Maybe that would help with this issue of whether or not the SZ model is valid or not.

ANDREWS: Let me hit the sensitivity and when are you going to see the total results. You kind of have seen the total results, albeit preliminary and still, I think as Abe pointed out, being reviewed and checked right now. This is Bob Andrews again.

What we have in the total results is the sampling off of all of the uncertainties that are included in the models that people have talked to. I summarized. I think the individual presenters hit on the ones that related to their particular aspect included in that model. And so you have that 300 realizations or 500 realizations of possible outcomes, each one of those being equally likely and each one of those being appropriately weighted by its probability of occurrence.

We then looked at the statistics associated with that total distribution of possible outcomes, and plotted the means and 95th percentiles, et cetera.

When we've done these exploratory studies, whether it's a sensitivity analysis or a barrier importance analysis, we're trying to gain understanding on which aspects of the system are moving the mean curve the most, which ones are moving the 95th percentiles the most. But the total system results are that first set of curves that I showed, both for the nominal scenario and for the disruptive scenario. These other ones, you know, as we've pointed out several times, have a very low probability of occurrence. You know, they're in the possible set of outcomes, but their probability of occurrence is very, very low, in fact, probably never sampled in some realizations. I'll let Abe answer whether we're going to do another peer review.

VAN LUIK: The peer review that we did for TSPA/VA was designed to carry us with recommendation for further work right into the license application. So we don't see a peer review of that scale and magnitude for the SR. We are still working to look at NRC, TRB, and peer review issues that have been raised, and I think that the SR documentation will identify many of those and how they have been dealt with.

The TRB and the NRC and the State and many levels of internal review are expected on the SR. Once the process has taken place and we give the SR, the secretary gives the SR to the President and the President makes a decision, we are thinking of asking the IAEA and the NEA to do a peer review, as they did for WIPP at one time just before their licensing work was submitted to the EPA.

So we would look for them to give us guidance on what to add to this product in order to make it even better for licensing. That's the thing that is under consideration. That is not a firm plan at this time. But if the answer is a yes or no answer, are you going to have a peer review on this product, maybe later is the right answer.

CHRISTENSEN: Dr. Knopman, and Dr. Ewing is on deck.

KNOPMAN: Knopman, Board. There are two areas that I'd like to explore. One is the cross-over from the process level UZ model, the seepage in particular, into TSPA, because I still don't understand what happens. And the second point really relates to the introduction of conservatisms throughout the modeling process all along the way so that--versus introducing conservatism at the end of the line so that you actually know how conservative you really are, because you're controlling it at the end process rather than embedding it separately.

Let me just start with the seepage questions I have. It began, Bo put in his Slide 16, and specifically it had to do with the thermal period. At this point, I'm not so concerned about the thermal issues as what is assumed--where this assumption about percolation flux 5 meters above the crown of the drift then comes into play. You make that assumption at the point where you're starting to abstract your flow field for TSPA? I still don't understand why that assumption has to be made, because to me, it adds in an incoherence to the larger story that you understand what's going on in the system.

To me, you've just undermined your modeling and insights that are coming from experimental data, and I can't figure out what you get from this except it is this somewhat poorly quantified conservatism that you're introducing. But I'd just like to kind of walk through what you do to get from your detailed process level model into the TSPA.

BODVARSSON: I'll take a crack at it. Bo Bodvarsson. The answer as I recall it, and I was involved in some of this, is as follows. The seepage model, both the calibration and the seepage model for PA, are ambient models at this time. They don't consider heat effects. There have been concerns by various overseeing bodies, as well as within the project, that the stochastic heterogeneous fracture fields may generate some feedback of mobilized water, condensate water, back to the drifts.

There is a technical paper by one in my shop, Karsten Pruess, a few years ago that also concluded that it's possible for water fingers to move through the heated region towards the drifts. Based on these considerations, and one meeting at least I was at at Berkeley, it was decided to be conservative, quote, and try to get some idea about the maximum type of seepage that may occur during this thermal period.

And the way that was done was to look a location which would lend itself to significant percolation flux driven by capillarities going into the heated zone. And as we knew, the boiling zone and dryout zone would be on the order of 5 to 10 meters, 5 meter zone above the drive was selected as would probably give a very conservative percolation flux, then could be carried to the drift to calculate seepage.

This was all done in lieu of a rigorous process model that includes the proper heterogeneous fields to quantify it better, or eliminate this as a concern. But this is what the project is trying to do now, though.

KNOPMAN: All right. So do I understand it correctly then that if you make that assumption, then you do get seepage into the drift at the point in which you used to say you were going to have dryout? Okay, so you've got--that's true; right?

BODVARSSON: Yes, that's true.

KNOPMAN: I haven't misunderstood that?

BODVARSSON: That's true.

KNOPMAN: Okay. Can we just keep going in just the steps so that I understand what happens with the flow field that you've generated? How does that get into TSPA? It's almost like a lookup table that's there for every other model to pick off of, so if it needs a seepage term, it knows for each time period and each place in space, you know what seepage is; you've just sort of--

ANDREWS: Let's just stay on seepage rather than the overall mountain flow. Is that okay?

KNOPMAN: Yes.

ANDREWS: So on the seepage, we've discretized as we did in the VA, we've discretized the repository into varying spatial locations. Those spatial locations are driven a little bit by the thermal-hydrologic response, i.e. edges are a little cooler and the center is a little warmer. So that was one level of discretization.

Another level of discretization was the degree of infiltration/percolation. So that's spatially variable in Bo's model and in the surface infiltration, and so we tried to capture it discretely in areas of repository that we expect to have a little higher percolation, or a little lower percolation. And in the end, I think we end up with 30 discrete areas of the repository block with slightly different thermal responses in those 30 areas, and slightly different infiltration/percolation rates in those 30 areas.

Each of those 30 areas has a certain number of packages associated with it. It's a variable number of packages, you know, from a few hundred to--well, it's probably a few hundreds, each of them, something like that. Total number of packages is 11,000, so divide that by 30, so it's about 400 per, but they're not equal size areas.

Within those then, we use the seepage model. So we take the percolation flux within that area, within those 30 areas, which is now time varying, you know, because of the thermal response, and go into the seepage model and say okay, what is the probability of seepage for the 400 packages sitting in that particular area, and what is the amount of seepage for the packages in that area. And it's then that probability, which is now area dependent, and that amount of seepage that's used as the direct input, if you will, to everything then downstream from that, which includes drip shields and waste packages and chemistry, et cetera. But it's that seepage fraction and that seepage amount that's being used, which is not spatially dependent.

COHON: This is Cohon, Board. This is an opportunity for me to clarify something that's confusing me as well. Just to nail this down, Bo's model, the UZ flow model, does consider the effects of heat. But the seepage model, as we heard from Ernie Hardin, does not. Right?

HARDIN: The ambient seepage model that Bob just talked about, and Bo did, is just that, it's an ambient temperature seepage model calibrated to ambient temperature tests in the ESF. We use that model with inputs developed from thermal models.

COHON: Yeah. but to develop the flow model, you do treat heat, and that gives you a seepage at 5 meters above the drift. But getting it from there into the drift, you ignore heat; is that correct?

HARDIN: That's correct.

COHON: Okay. and that's why we can have two presentations like this with statements that directly contradict each other, and now I understand why. Well, Ernie says approach does not incorporate dry within 5 meters, and you have one that says liquid flux towards the drifts, 4 millimeters per year, but is all vaporized by repository heat. Now I understand how I can reconcile this.

HARDIN: Just one point that I'd like to add to this discussion is that--this is Ernie Hardin, by the way--that, you know, any particular location in the repository, the extent of dryout will evolve with time. So you could have a location, for example, where dryout exceeded 5 meters at the maximum, but later, 5 meters might be a perfectly reasonable representation of the maximum flux that could occur because of thermal reflux. So it's a regime that varies with space and with time, and we have approximated it using a single point.

KNOPMAN: But TSPA doesn't have dryout, so it doesn't matter.

HARDIN: Well, in the case of a very hot drift, dryout can exceed 5 meters.

KNOPMAN: But it's not in the TSPA.

HARDIN: In which case, the flux calculated by this process that we talked about for TSPA--

KNOPMAN: Oh, I see what you're saying. Okay.

BULLEN: This is Bullen, Board. This is one little quick question that actually may follow onto this, and it's to resolve the issue between Bo's Figure 16, which everybody has seen and has the 5 meter percolation flux, and Ernie's Figure 7, which has these thermal pulses, actually it's a waste package surface distribution over time. And I guess the question harkens back to the last Board meeting where we had Rick Craun make a presentation that says if you ventilate or age or stage long enough, that you could make these pulses go away. So is it possible in your models to take a look at making the pulses that we showed in these two figures go away, and does that simplify the task of PA, reducing uncertainties, or whatever method you want to have? And the two of you can grab that, or you can turn to your left and ask Abe or Bob. But if indeed you can, by a simple operating parameter of the repository, make it go away, does that make your job easier?

HARDIN: This is Ernie Hardin. I would speculate that closure will change the boundary conditions on the heat transfer such that there will always be a pulse of temperature. If you ventilate for some period of time, then you go and close, you change the system. There will be a transition. There will be a pulse.

BULLEN: Bullen, Board. But if the pulse doesn't mobilize a bunch of water, does that help you?

HARDIN: I think that would reduce uncertainty.

BULLEN: Thank you.

KNOPMAN: If I can just finish up here?

BULLEN: Thanks for the interruption.

KNOPMAN: That's all right. Let me again make sure I understand what you said, Bob. How is it, you talk about probabilities there with the seepage model, and I somehow missed where those probabilities come from. Where does uncertainty from the seepage model, this is this cross-over that I'm puzzling with, where does the uncertainty of the seepage model get itself into TSPA? Because you have at each of these 30 areas, you have a distribution; is that what-- you've ended up generating a distribution from Bo's model by having sampled from probability distributions of all the various parameters? Is that the way it's done?

ANDREWS: And there one--Bob Andrews again. As Bo had one beautiful figure in there, nice colors, too, of the K over alpha, which are the two driving fracture parameters affecting the likelihood of seepage and the amount of seepage, the fracture permeability and suction are both uncertain. They're both variable. The project is gaining more information, you know, at the repository block that might reduce that uncertainty significantly. But at this present time, it's still a fairly large uncertainty on fracture permeability and fracture alpha suction.

That uncertainty is incorporated at each of those 30 regions that we talked about. So each of those 30 regions, areas, has a different probability of seepage driven by the sampled K over alpha, and there's a couple other factors in there, the flow focusing factors and others. So for each realization, so we go through 300 realizations, for each realization, we have a different fracture permeability and fracture alpha for each of those 30 areas and, therefore, a different probability of seepage and a different probability of seepage occurring and probability of seepage amount.

KNOPMAN: Okay. And finally one more question on the seepage that came up in Christine's presentation, and that was on her Slide 9, and there's way out in the 80,000 range, 80,000 year range, she's comparing where localized corrosion may occur, and it shows up as being higher, slightly higher with intermittent dripping versus always dripping. And your answer on that, Christine, before was, well, there's more water coming in through the intermittent dripping than through constant dripping, and I just wanted to make sure I understood why that was the case.

STOCKMAN: That's what I've been told. Somebody else has to answer why.

ANDREWS: I think we'd have to, you know, go into the model and actually look, but I have a feeling that the volumetric flow rate, you know, the number of liters per year, is greater for that intermittent flow case than it is for the, if you will, the steady constant flow case. And Christine's results are driven by the volume of water coming in, not by the probability of water coming in. So you have to kind of break out the amount from the likelihood.

KNOPMAN: So it's just the way you set up the scenario for dripping, that you have higher volume through the intermittent dripping. It's not a physical--it's not a consequence of your physical understanding?

ANDREWS: I'm not sure which one it is. There's uncertainty and we're trying to factor that uncertainty, whether it's intermittent or steady seepage, is being factored into the analyses, and there's different cases, different packages are seeing different sets of conditions.

KNOPMAN: I don't understand. If I can just end on this last philosophical question that perhaps will come up in other questions from other Board members, and that has to do with the theory of introducing conservatism all along the stream, let's say, rather than doing it downstream in your analysis, so that you actually have some handle on the extent to which you have introduced conservatism. This is what the Board has been--one of the things the Board has been struggling with that's part of the discussion about uncertainty. We don't know how conservative you are. It looks in lots of areas, it seems like you're being conservative, but we don't have a way of evaluating that at the end of the line there with your results, because it's come in in so many different places and so many different ways, and not clear what the orders of magnitude are that are being adjusted in parameter values. So we don't know what you have at the end. What was the judgment there? Could you explain what your options really were there?

ANDREWS: Well, this is Bob Andrews again, I mean each of the individual--it depends on the individual component part, whether, you know, the conservatism was added in at the process level because of tremendous complexity and uncertainty that that individual, originator and the others supporting it felt that was the most defensible way to go in the face of that large uncertainty. And in some cases, you know, the conservatism was added in towards the end. But I think there is a way to parse out the significance of that for each of the component parts, because each of those conservatisms, generally there is a parameter or sets of parameters or conceptualization embedded in the model where that conservatism resides. And it is possible to change that particular parameter or conceptualization and see what effect it does have.

You know, the example that we just had here of the seepage flux being driven by percolation 5 meters above the drift put in there as somewhat conservative, we could change that to be a half meter or 1 meter or 10 meters, and see what the effect of that particular aspect of it is on seepage and on package degradation and on total system results.

The same is true with virtually every one of the other conservatisms. You can evaluate their potential contribution to subsystem or system performance. Some of those have been done. Some of those we've alluded to. Many others have not been done yet, quite honestly. I mean, these are preliminary results and I think we'd welcome your comments on which conservatisms you might want explored as far as their significance.

CHRISTENSEN: Dr. Cohon has a very, very brief question.

COHON: Yes. That was a good answer, Bob. One of the problems you have, you're going to have, is that you're going to have to--you will have a story that you have to tell. That's the model, not just a result, but a story, and it's all got to hang together. So how is it that the mountain dries out around drifts, but then you assume it doesn't? Where is the consistency? You have to start thinking about the story.

BODVARSSON: One quick comment, too? I just wanted to mention that, Debra, I think you're right to some extent, and I think DOE is doing something about it. There is this effort that we are doing now which is called more the expected case for some of the models, and I don't know if you have heard that or not. Some of us have developed our models perhaps conservatively because we work very closely with performance assessment and we like to blame them on a lot of things, and I'll give you a good example.

For example, we have always had some--we started a few years ago with flow in the PTN, assuming considerable fracture flow in the PTN and considerable fracture flow in the vitric Calico Hills, and that was just because we didn't have sufficient data and we wanted to be conservative, because of PA issues and all of that stuff. That kind of thinking has been retained in the model to some degree. So there is significant conservatism in many aspects of these models, as you have pointed out.

There's now significant effort with some of these models to do, quote, the expected case, to do exactly what you're talking about, to look at what is realistic with these models to represent it and perhaps use it for some purpose.

So I just wanted to mention that.

CHRISTENSEN: Thank you. Dr. Ewing, and Dr. Nelson is on deck.

EWING: I'd like to change gears a little bit and discuss colloids. And I'll need Christine to help me develop a line of reasoning.

In Christine's presentation, it's Page 30, there's a very nice diagram of the model to be used for the colloids, and I must say it's entirely reasonable. It describes the availability of colloids, the stability as a function of ionic strength of pH. It considers reversible and non-reversible, or irreversible sorption. Presumably as you go down the line, there would be the question of whether the colloids are mobile or immobile, and so on. So this looks fine.

But if you think about the data that are required to support the model as it's constructed, my impression is the data are pretty thin, and so my first question is to Christine, can you characterize the extent or substance of the data available to support the model that's been developed?

STOCKMAN: In some areas, we have quite a bit of data. In other areas, you're correct, we don't have as much as we would like. In those areas where we had less data than we would like, we went to analogy and we went to conservatism.

EWING: And for my information, what area do you feel like you have a lot of data?

STOCKMAN: We do have all the Argonne data on plutonium and americium coming off of high-level waste glass. And we have quite a range of stability and ionic strength. So we have that pretty well.

EWING: And those are experimental values?

STOCKMAN: Those are experimental values. For groundwater, we have some experiments that show how stable the rust type colloids are versus pH. But we didn't have any good experiments that said this is what the actual mass per liter of colloid would be, and so we use analogy with groundwater colloids for that one.

EWING: But, you know, just to pursue that, I'm a little bit familiar with the Argonne data, and I might argue that it's not clear that the material being generated is colloid in the sense of material that will transport actinides. There's fine grain material that has a high actinide content. When will you call that a colloid in using those data?

STOCKMAN: Well, the colloids are characterized by dynamic light scattering and by sequential filtration. So there was a range I believe from greater than 10 nanometers to about a micron.

EWING: But there's no evidence that this fine grained material, say where you transported a few meters, would actually be a colloid for the transport of actinides. It's just that it's a size range definition; right?

STOCKMAN: Correct.

EWING: Okay. And in terms of further field, and I come to Bruce with that because you had some colloid factors in your saturated zone discussion, the point I would make, or it's my view looking at the literature, it's really very difficult to say what proportion of the actinides might be sorbed irreversibly versus reversible sorption. I mean, am I wrong on that? I mean, there aren't many experiments?

ROBINSON: Bruce Robinson. No, I agree with that, and I would extend it to colloid transport, and the difficulty of really pinning down parameters for colloid transport.

EWING: So where did you get your parameters? You had them listed, but you didn't comment on them.

ROBINSON: Let me speak to the transport parameters themselves in the saturated zone. The transport of colloids in the fracture volcanic tuffs were obtained based on microsphere experiments carried out in the C-wells. And that was used as a way to get at the filtration of colloids in the fractured tuffs.

In the alluvium, we had less to go on. We went to some literature studies. The references escape me, but I could tell you which ones those are. But the bottom line for the alluvial transport, our range of parameter values for filtration of colloids is extremely wide. The uncertainty range is extremely wide, ranging from essentially little or no filtration to complete filtration. So it's an extremely wide uncertainty range, and that's I believe just the nature of the business of colloid transport.

EWING: It may finally be very--well, it may finally be an intractable problem. But I guess the point I want to come to is that, Christine, in your presentation, you arrived at a point and you said, well, based on these model results, I think we can put this to rest, that colloids really aren't very important, and I just want to question that conclusion, let's say, given my impression of the data available.

STOCKMAN: Well, that conclusion is a preliminary conclusion, and it is based on the fact that whenever we had a problem with not enough data, we went to what we believed was conservative values, and we still, when you use those conservative models and conservative values, colloids were only 10 per cent of the plutonium release. Now, certainly more data might surprise us, and we may find that we were unconservative. But we believe we were conservative.

EWING: Well, of course this is leading up to a surprise point I want to make. The model incorporates the role of iron oxides in actinide transport by colloids, which is entirely reasonable. But whenever I travel, I grab a pile of paper that I wouldn't read otherwise, and in my briefcase, there's a very nice paper recently published on mineral associations and sorption of plutonium in volcanic tuff from Yucca Mountain, and the work seems to be done very well, and the surprising result is that the sorption isn't on the iron oxides, but it's on the manganese oxides.

So that's very different than the conceptual model you've presented, and I think the point I want to make, it's not a criticism because I would have done it exactly the way you've done it, is that there's a very real, and in some cases, potentially very large conceptual uncertainty in these models. I mean, the difference between the presence and abundance of the iron oxide versus the manganese oxide may be good or bad for the final result, but it's very different than the approach that's been taken. So I think the moral I'd like to leave everyone with, it's very difficult in these elaborate analyses to discount any possibility.

STOCKMAN: I agree.

CHRISTENSEN: Dr. Nelson, and then Dr. Cohon, Bullen and Parizek are on deck. I want to comment just briefly that we have about 30 minutes, and so think about that in your questions and answers. We do need to be pretty much on time because of plane schedules, and so forth, this afternoon.

NELSON: Nelson, Board. I must admit I still do not understand these two figures, Bo. And so very quickly, can you tell me on the left-hand side, C-flow rate defined as water entering drift; correct? Why from ten, or before ten, up through 50 years, you have no seep rate. Why is that? Is that because of ventilation?

BODVARSSON: Well, there are two reasons for it. One, it's correct that the ventilation takes away a lot of the heat, so there's less rapid heating of the drift area around and, therefore, less boiling potential and stuff like that. And then the other effect also, though, is that with time, the boiling front moves away from the drift. So even if you didn't have a ventilation, there wouldn't be a large seepage flux coming 5 meters above the drift, because remember, just take this one location of 5 meters above the drift, you would only get this high flux there--right at that zone, that 5 meters, so that you have a huge percolation flux going through that region.

NELSON: So you're thinking percolation flux 5 meters above the drift and turning it into an assumed seep flow rate?

BODVARSSON: Yes.

NELSON: Which is entry into the drift?

BODVARSSON: Right.

NELSON: And it does or it does not include evaporation?

BODVARSSON: No, it does not. What we do is this rate is taken as a percolation flux rate. It's then moved mysteriously right to the drift wall, where we then employ a seepage model, the ambient seepage model, and determine from that how much of that total amount of water will actually seep.

NELSON: But in reality, in the reality that you have, in fact it will not seep, because there is a thermal pulse and it is hot?

BODVARSSON: And in reality, in my view, and based on some of the studies, you see on the right-hand side there is that for most all of the fracture stochastic heterogeneous variability in parameters that we see at Yucca Mountain, with exception of high permeability faults, you are very unlikely to get any seepage during the thermal period. That would be my conclusion.

NELSON: Okay. Well, then I guess I don't understand what this figure is trying to tell me.

BODVARSSON: This figure is telling you that in order for PA to be very conservative, because we haven't demonstrated conclusively using rigorous analysis that takes into account the uncertainty in all of these parameters, that dryer land, having an optimistic--was conservative, and allowed for seepage, even though it's likely that none would occur.

NELSON: Okay. Well, I'm going to have to think about this. Maybe Dick can explain it to me later. But I have a second question, which is I don't expect an immediate answer on this, but it comes from a gnawing suspicion that I myself am not particularly a chemist, I appreciate the chemistry is a science where different things can cause sudden changes in the system in terms of what's happening, what reactions go, where precipitates occur, so it's interesting particular from the standpoint of turning off and turning on things. And things can get very complex in a system like this.

We heard yesterday about the EBS chemistry model from Bill Glassley, which really gave me the feeling that there's a lot of possibilities in terms of what can be happening, what can be dissolved and what can be precipitating and, in fact, what could happen to the chemistry of the water. And then we heard from Dr. Barkatt and Gorman about the importance of water chemistry on Alloy-22, and we think about the thermal pulse with water cycling through, precipitating, re-dissolving, forming caps, not forming caps, dissolving, moving. And I'm just struck by the importance of chemistry in exactly what's going to be happening, what's setting the stage for the processes that are going to cause drip shield problems, waste package problems, or waste form changes, or transport.

And I'm looking for some feeling that, yes, there's an overall understanding that those thresholds, those places where the chemistry changes are causing the precipitation and solution, where things are happening, are well understood and are well encompassed in the overall flux model through the mountain, including the waste form and the transport, and I don't get a strong feeling that that kind of a thinking has happened, that we very often, in terms of our data, we think about flow through the mountain, we start with J-13 water, and many of the tests are on J-13 water, and when in doubt, assume J-13 water. And we're not going to have J-13 water, i suggest, and we're going to have some sort of ground support is going to be around the tunnel, some other things are going to be there as well.

So what can you say to me as people who have worked with the chemistry to feel that there's been a consistent overall look at what's happening to the importance of chemistry on how this mountain and this waste package, or EBS, perform overall?

HARDIN: This is Ernie Hardin. I'm going to take a crack at that. I think there are some other experts up here who might also have something to contribute.

We have a great many samples of water from Yucca Mountain and from the thermal tests. And so we can profile for you the composition of those waters, and we can show you that as those waters evolve, we can show you in the laboratory that as we evaporate those waters, that they follow certain trends, and they take us to certain end points which might be important for the EBS performance during the peak of the thermal period. So what I'm suggesting is that we understand the range of aqueous chemical conditions that will be encountered by the engineered barriers.

There are a finite number of chemical components involved. The rock is dominated chemically by a set of elements for which the dissolution aqueous chemistry of those components is within our understanding, calcium, sodium, potassium, magnesium, sulfate, chloride. So we have a lot of experience with those components, and we have laboratory data. We'd like more laboratory data on the thermal evolution of these solutions. The tests are not that difficult, and we have some in process. We found laboratory data to be very, very useful in describing the evolution of the system.

So I guess to summarize, there are a couple of--we have identified some end member water compositions. Okay? We've identified that we could have a bicarbonate dominated water. That's your J-13 water, to a simplification. Or you could have a chloride sulfate water. We've looked at those both numerically and in the laboratory. More work will be done. Given either one, our models now predict what happens when those waters approach dryness. So we know approximately what chemical conditions will be imposed on the drip shield, possibly on the waste package, during the thermal period.

Now, long-term, say after 5,000 years, and certainly after 10,000 years, things cool off and so we begin to revert to pre-heating water compositions. Our current database of waters from Yucca Mountain becomes more and more relevant. I can offer that to you as well.

ANDREWS: Let me add something. That was an excellent question, and I think part of it is based on how we've discretized our presentations to you, going back to something Dr. Cohon mentioned. Part of this is in the presentation, and when you pick a topic, in this case chemistry, or colloids, that cuts across a lot of people across this panel, because it cuts across space and cuts across time, then when you discretize it by space, which is more or less the way the presentations have been structured, you miss some of that integration, I think.

But let me try to pull it back together a little bit. Bo presented chemistry in the rock and changes in chemistry of the rock. That is in what's called the THC model from some of his co-workers. That is used as an input to Ernie, who then talks about chemistry in the drift, and chemistry on the drip shield, and chemistry on the package.

Pasu then also talks about chemistry, because he's now concerned about a more detailed chemistry look you know, on the package surface. So he's taking stuff from Ernie and from the EBS environments. They then all are passing off to Christine, who looks at the changes in chemistry inside the package.

Now, if we had one completely integrated chemistry model, you know, from ground surface into the package and back out again, perhaps it would be a little clearer. But I don't think the complexity of the analyses would change or the uncertainty that we have in the chemistry would change. Bo has uncertainty of the chemistry coming into the drift. Ernie has uncertainty in chemistry in the drift. Pasu has uncertainty in chemistry on the package. And Christine has uncertainty inside the package. All of which are tied to a range of possible interactions, you know, including interactions with the structural materials that are there for safety of the drifts themselves.

And then, you know, on through the rest of the system. Ernie picks it up again with the invert, and Bo picks it up again with transport. So, you know, when you pick a process and cut across spatial and temporal domains, perhaps we need to do a little better job of integrating it back up again for you, because right now, it's spread in probably eight or ten AMRs, I would guess.

NELSON: I think it is very much, and actually it could actually be a wonderful exercise to--because the water is the essence of what's doing it, and to see how the water is evolving and what's important for Bo, in terms of reactions, would be quite different from what's important to Christine. And, therefore, the tendency to decide conservatism by Bo will be completely different from what Christine would feel would be conservative for her application.

So the sense of building that understanding of what I don't even know--or making the case for selective and conservatism decisions and how it fits together, various mechanisms of looking at the water may help. It would help me to understand and to trust the overall picture more than I do right now I know.

STOCKMAN: This is Chris Stockman again. We started to address this very issue with a weekly phone call where we have Eric Sonnenthal, and basically all the people that we just discussed are now talking once a week about common issues, and we're trying to make the presentation better in the future.

CHRISTENSEN: Before I ask Dr. Cohon, I just want to note that in an act of genuine but typical generosity, Dr. Bullen has yielded his place in the queue. Dr. Cohon?

COHON: Are you sick, Dan?

BULLEN: You guys just ask very good questions.

CHRISTENSEN: Dr. Parizek is on deck.

COHON: At the end of the colloquy involving Dr. Sagüés and Dr. Bullen and Dr. Pasupathi and Dr. Stockman, I thought I heard you say, Dr. Pasupathi, that the pHs you have to look at are bounded, which is the information you get out of Dr. Stockman's model. Dr. Stockman feels like she can bound those pHs because you're telling her the drip shield will never fail. Therefore, the seepage is very low.

Do we have to worry about some circularity here? did I get that right, and is there a problem? Is there an issue, I should say?

PASUPATHI: No, I don't think I ever said that. This is Pasupathi. I don't think I ever said anything about what I feed Christine necessarily.

COHON: No, but did I get the thing right about pHs, though?

PASUPATHI: Yes, the pH that we use for our localized corrosion model comes out of Ernie Hardin's model.

COHON: Oh, Ernie Hardin's model. And does your pHs that you produce for him depend on the integrity of the drip shield?

HARDIN: No, they don't. This is Hardin.

COHON: Good, I'm glad I misunderstood. John Kessler asked Kathy Gaither a very good question at the very end of her presentation about the importance of consistency in the assumptions one makes about the probability of the occurrence of a volcano and the probability of the kind of eruption you would get, because their occurrences and types are linked. That kind of consistency is an important thing, and it's come up before. We just talked about it in the case of how heat was handled.

And, Ernie, in that regard, I was wondering, you talked about diffusion through the invert becoming an important process, potentially an important process at very low water volumes. But do you need more water than that to mobilize the wastes from the package in the first place? Can it get to the invert without more water than you can tolerate from your molecular diffusion case?

HARDIN: Okay, in the current conceptualization of the process, we have a release mechanism that relies on molecular diffusion in traces of water originating from the waste form and finding its way across the surfaces of the waste package, both inside and out, and then entering the invert. And that can happen with an intact drip shield, that is possible. If the drip shield eventually develops a hole, then you go to an advective dominated flow mode.

COHON: Yes. So there's an assumption about a consistent estimate of water availability, both at the package and at the invert? That's what I was getting at.

HARDIN: I believe the approach is consistent, but highly conservative.

STOCKMAN: Right.

COHON: Okay. How do you--I'm sure you worry about, but what are we going to do about the question, how do you know you don't have coding errors in here, that your code is wrong, or the data was input improperly? I mean, some member of Congress is going to point out to you that there is a certain famous Mars Lander that didn't make it. It is a very real issue. I mean, you can pooh pooh it or not, but you're going to be asked it and you're going to have to have an answer to it. What is the answer to that?

ANDREWS: I'll start, and then maybe Dennis wants to add. I mean, every input, and it's not just the PA input, it's all the inputs of each of the process models you've heard about and each of the abstractions goes through a checking process. You know, the software is qualified or is going through a qualification process. The inputs are checked, not only by the originator, but by a checker and a reviewer to check. That's absolutely what we're talking about.

Am I sure, you know, right now that everything has been checked? No, that's why we had on those viewgraphs these are unchecked results from the PA perspective. All the inputs have been checked and gone through that process, but the TSPA is the last thing on the list, and the checking is going on. But that's a process that we have to go through.

Dennis, do you want to add to that?

CHRISTENSEN: Dennis, do you want to comment?

RICHARDSON: Yes, Dennis Richardson. Yeah, there's no-- you can't ever give a solid answer to this. Last year, I worked at AED, and after 40 years of evolving the same code for Westinghouse, we found a small error in it, amazingly enough. But there's processes and procedures in place for when this happens, and it will happen. We get new data. We'll find errors in codes, and that's why for one thing, you know, we try to ensure that starting off, we have ample amount of margin, defense in depth in case this happens. And if you can't live with the error that you find, if it exceeds something, or if you have to change methodology, then you have to go back for re-review and approval to the Commission. And if during our performance confirmation time frame, or after licensing, we find something like that, if we don't have the margin to handle it, if we have to change methodology, we obviously would have to do the same thing. But we try to get some assurance of safety built in initially, and I'll talk to it a little bit later on, with ample other elements of the safety case, which include margin and defense in depth.

CHRISTENSEN: Dr. Parizek?

PARIZEK: Parizek, Board. Five minutes?

Well, we had dye experiments that you reported out where the dye apparently went from small openings into a larger opening, a lithophysal cavity floor, and we just want to understand the physics of that, or explanation of it, because God's little creatures who live underground in burrows ought to pay attention to whether they're going to get wetted by this new process that you're going to describe for us. But how does this work? Is this a wicking effect up the sides of the lithophysal cavity?

BODVARSSON: What happens is what Abe was talking about, the different characteristics of the lower lithophysal rock mass. It has big holes with the lithophysal cavities, as you know, but it has a bunch of small fractures that Mark has been talking about for years and years, and maybe some ignorant people like myself didn't think that they were so important, but he was absolutely right. The capillary suction of these little suckers, if I may call them that, is such that water doesn't go down by gravity like in the middle lithophysal. It goes around things. And what happens is when we put water into the boreholes, and we put a lot of water in, then it goes up as well as down and around cavities. But it showed up, the dye, at the bottom of the cavity. That doesn't mean that the water necessarily ended at the cavity, so it's not in any conflict with our capillary barrier assumptions, but it might be one mechanism to have evaporation or water below a cavity that may give you chemical signatures and deposition within cavities.

PARIZEK: So there was a staining of the bottom rather than actual water sitting there?

BODVARSSON: That's exactly right.

PARIZEK: Now, many people mentioned the colloid process of transport. This is Christine's document, and Bo, you did, and Bruce Robinson and others. Colloid migration in the unsaturated zone could be important as a way to bring radionuclides to the saturated zone; correct?

BODVARSSON: Yes.

PARIZEK: The question is what data exists to support any evidence for colloid migration in the unsaturated zone at this point that anybody might have used? It was in the various models. Various people talked about their models for that. So I don't know where the data comes from, and we only know of experiments going on, and the Busted Butte, that's still up in the air as to what the results will be, and we know you are putting water in to boreholes and picking up water out of other locations in these drillhole experiments. Do you do colloid sampling in those experiments as well to get some numbers on this?

BODVARSSON: Bo Bodvarsson again. I just have to echo what Rod said before and what Bruce said and what others have said. We have very limited data on colloids, so I could blab here for another minute or two, but the bottom line would still be we have very limited data on colloids.

PARIZEK: So that part of the modeling will be pretty weak for the time being?

BODVARSSON: And it depends on two main things in the unsaturated zone. One is the filtration process, and the other one, of course, is the size of the colloids with respect to matrix diffusion and other effects, too. But, again, you know, I can blab another two minutes, but it doesn't matter.

PARIZEK: A follow up on that. As far as Bruce Robinson's presentations--

KESSLER: Can I interject something? This is John Kessler at EPRI. We funded some work looking at colloid migration in the unsaturated zone with tuffs, and there's a little bit there that we found, you know, that it is a function of the saturation and the particle size and a few other things that we looked at. But you're right, there's precious little.

BODVARSSON: But that comes from the NTS. We're using some of that data.

PARIZEK: That's in the saturated zone. That's a saturated zone problem. And I'm on record as having said look in the fracture fillings and lithophysal cavities for any evidence in the mineral phases to see whether any colloids have been trapped there through geological times since the mountain was built in order to see if there's any evidence of it, and various people probably are--

BODVARSSON: Right after you said that, Dick, I went straight to Zell Peterman and told him that you said that, and I asked Zell to look into it. So we are looking into that possibility.

PARIZEK: Now, you said faults are important in the saturated zone modeling that you were doing, Bo. And the question is, Bruce, do you have faults in the site scale model, and if so, what data sources do you use to characterize the faults and, you know, how did you put them in your model?

ROBINSON: Bruce Robinson. Yeah, there are faults basically to control the large scale drops in the potentiometric surface that are to the west and the north of the repository, as well as--and those are low permeability, low permeability to flow across the fault. That's the conceptual model that says why you have a large drop in the potentiometric surface as you go north into the region around Yucca Mountain and the repository. And then there are a series of features, additional features put in the model that are used in which the permeabilities are used as calibration features to capture the head distribution, the measurements.

PARIZEK: Okay. You don't support Linda Lehman's conceptual model of flow south. You have flow southeastward still, and then south more or less along Forty Mile Wash, you still have that? Figure 11 shows that as the pattern of flow for your plume.

ROBINSON: Yes, that's right. But as an alternate conceptual model, one of the alternate conceptual models that's built into the TSPA is the use of anisotropy to give rise to a more southerly transport pathway than occurs on what I'll call the base.

PARIZEK: You have a five to one ratio. Is that the basis of Figure 11? Is Figure 11 isotropic or is that anisotropic?

ROBINSON: Could you show me Figure 11?

PARIZEK: Figure 11 is the little plume, little red plume.

ROBINSON: That was the isotropic one.

PARIZEK: Isotropic.

ROBINSON: You have transport to the east, southeast, and then turning south.

PARIZEK: Right. So the question would be how do they differ, the results differ for the anisotropic case versus isotropic case, and that's perhaps a detail that will be in your analysis, that will be discussed somewhere in the analysis?

ROBINSON: Yes, that is discussed. But basically, there is somewhat more southerly, direct southerly route taken by the radionuclides in the anisotropic case.

PARIZEK: And the porosity data in the alluvium is mentioned as having some heterogeneous variability to it, which makes sense. But for the moment, what data did you use for the alluvium part of the model? The only C-well that's been drilled, that's been tested recently, is a single well that I'm aware of. That's part of the testing complex that's planned for the future? Where do you get your alluvium data to put into the model?

ROBINSON: I'm going to have to look up the detail on that. But basically, there was a distribution in which the mean was .18 plus or minus one standard deviation of .05, and that was based on a literature study in similar types of kind of Valley fill type systems like this.

PARIZEK: So it's the best you have available until new test data become available?

ROBINSON: That's right, and that's why I think that that test data is an important hole to fill.

PARIZEK: The flux boundaries you used came from the USGS regional model, and was the old model of several years ago, or runs that are being made currently to bound your model domain?

ROBINSON: I believe that it was the older model. And if somebody has reason to correct me on that, older meaning about 1997.

PARIZEK: The three layer model versus the current 17 layer model, which had its limitations, so that could affect your results in terms of bounding your problem area, your problem domain?

ROBINSON: Yes, I think so. And I think that would be, you know, a continued revision and improvement of the models, in my opinion, should include a look at the regional scale as well as the models such as the site scale model, which really, you know, on the one hand the radionuclides are being calculated in the site scale model, but if there's a significant boundary condition, if you will, that could be refined in another model like the regional model, I think that that would be a wise thing.

PARIZEK: I think, frankly, these promises around a steady state run by SR I think--or is that by LA, I don't remember now the date of his promised delivery of a new run for a 17 layer steady state model.

ROBINSON: It won't be for--I mean, it wasn't for this version of the TSPA. So it must be LA.

PARIZEK: I hope you get the latest runs when you finally go to LA, if it comes to that point. How about the technetium and the iodine, those experiments are important, were they steady state values or were they early-on data? It seems like the alluvial testing on Kds for technetium and iodine was underway, and what you used was a steady state number, or sort of a preliminary number?

ROBINSON: If you're referring to the batch sorption testing, those were carried out with the same sort of procedure. They were not transport tests. Those were batch sorption tests. And so it's essentially a steady state measurement after having carried out the tests long enough to obtain a value which we're confident is not exhibiting kinetic effects in the sorption measurement.

PARIZEK: And then on Figure 11 again with the plume, that sort of must depend in part on the regional model in terms of the role of, say, Funeral Mountains and part of the regional flow system of how regional ground water moves to the south of your site scale model. And I guess I would say that the hydrogeological characterization of the Funeral Mountains is still pretty loose, or not too well constrained. I understand some drillholes are someday planned there. I hope that becomes available to sort of see whether your plume shifts another direction.

And I raise the question about climate states. You say change in climate states probably won't change the flow characteristics of the flow field. But I would, again, think that you'd have Forty Mile Wash recharge that may cause spreading of the flow field, and could be beneficial to the program if that transit was considered in your models.

ROBINSON: Right, that was--what I meant to say there, what I meant to convey there is that that was the assumption that was taken, and we believe that there won't be significantly worse performance than the assumption that we took, which was that the flow patterns remained the same.

PARIZEK: And were they with pumping from Amargosa farms area; was that pumping effect at flow field?

ROBINSON: The flow field is a steady state flow field in which the current day had measurements, are what is used in the calibration. And so you have the decline in the water table due to the pumping effects.

PARIZEK: One last question, and that is a lot has been said in two days and it's hard to digest all of it, but does the natural system matter in hindsight, just to anybody on the panel, and do we get any credit at all for the rocks, or is it strictly drip shield and C-22?

RICHARDSON: Dennis Richardson. Yes, it does matter, and I'll address this in the next presentation.

CHRISTENSEN: The final word will come from Dr. Runnells, who says he has one quick question.

RUNNELLS: Runnells, Board. It isn't even a question. It's a statement that could be very long, but I'll try not to make it that. In listening to the questions that have been asked and two days worth of presentations, the issue of integration just keeps coming up over and over again. How do you tie all of these complex things together? Nature has already done that for us, and I am worried and I guess a little disappointed at how seldom the natural analogs are mentioned.

I know there is a program about, you know, to investigate natural analogs. But sitting here during the question and answer period, I filled one sheet of paper with issues that could be addressed by natural analogs, and none of those were mentioned in any of the presentations.

For example, the THC modeling, there is a wealth of information, a hundred years of studies in hydro-thermal lower deposits, which are available for us to look at, diffusion away from veins, temperatures tied to those fluids through fluid inclusions. There is a wealth of information in the literature on the shape and variation, and so on, of contaminant plumes in alluvial aquifers, in bedrock aquifers, and that literature incorporates the heterogeneities that are so difficult to model. The empirical data are there, thanks to Superfund and a few more things.

We've often talked about Josephenite as a metallic mineral, an alloy that is apparently inert to oxidation processes, and to the best of my knowledge, the program has just barely started to look at that. And why? It's apparently inert.

The more obvious things like the diffusion of radionuclides away from uranium ore deposits, there's been quite a bit done on that, and I know the project is aware of that, but I don't hear it coming into the integration and the validation of these very complex numerical models we've been talking about for the last couple of days.

So my statement is that I wish, I hope that as we go further along this path of trying to bring all of these very complex models together, that more and more emphasis will be placed upon natural analogs that will help us tremendously, I know they will, in terms of tying these things together. The geothermal fields that Bo mentioned previously in other meetings, those are analogs waiting to be tested with the models that the project now has, with a wealth of data sitting there waiting to be used.

I know time is short, resources are short, people can't do everything, but I do want to put in a plug for natural analogs in many, many, many aspects, not just diffusion or migration away from uranium ore deposits.

CHRISTENSEN: Thank you. Two comments here. First of all, I want to say that we do have a question from the public. I'm going to give it to Dr. Cohon, who I hope will pose it during the public comment period, and I want to thank this group for I think wonderful responses over a two hour period. This is the closest thing to a group doctoral exam that I've ever taken part in.

And we'll break for a little less than ten minutes. Be back here at 25 till the hour for our last presentations.

Thank you.

(Whereupon, a brief recess was taken.)

CHRISTENSEN: We welcome you back to this final portion of our meeting. We have two presentations. Dennis Richardson will give the next presentation. Dennis' background is in mathematics and mechanical and aerospace engineering. He's the manager of the M&O Repository Safety Strategy Department.

Of particular interest in his 30 years experience in nuclear electric power--pardon me--is his 30 years experience in nuclear electric power, much of it related to licensing and safety issues at nuclear power plants and defense facilities.

Dennis, it's good to have you.

RICHARDSON: Thank you very much.

It's a pleasure to have an opportunity again to talk on the repository safety strategy. You've heard in the past from both myself and Jack Bailey, and so this is a chance to give a status update on what we're about. We're right in the midst right now of writing it and getting technical checking on it, and so some of the things that I would like to share we you we don't quite have ready yet, but I'll share as much as I can at this point.

A couple of differences, a couple of things to recognize on Rev 04, the safety strategy, is this will cover both preclosure strategy and the postclosure safety strategy. Now, this presentation and discussion today will just be on the postclosure. Certainly if you have interest, in the future, we'd be happy to share with you the preclosure side of things. But today, really we're focusing just on the postclosure ends of things, and this is a fairly large effort that we've been going through for the last six months involving all of the national labs and the DOE and all the people you've seen here, the PMR lead, all the technical people, bringing their insight and issues for consideration as part of the strategy.

The chief and the technical lead and the writer for the postclosure side of things is Larry Rickertson, who most of you know in the audience there, and also I'd like to recognize our DOE, Department of Energy lead who's helping us out and keeping us on the straight and narrow, Mark Tynan, who I believe is in the audience somewhere. There he is in the back. And obviously on PA, we have Dave Serukian, who you've probably met in the past, has the tremendous task of trying to take all the demands from Larry and myself on things we want to see and do, and providing that type of information. So just to recognize a few of those folks that are helping us.

What is the repository safety strategy? Well, really, we're trying to identify what is really important on the postclosure safety case. What are we going to base our safety case on? What are the what you would consider the rocks of Gibraltar, defensible factors, and how do we show the assurance of safety for meeting the regulations, the proposed regulations in Part 63? And for those that have glanced at that, you'll notice that the assurance of safety plays an important part of that, understanding the multiple barriers, not just the output of PA, and I will discuss this and the other elements that we want to bring into focus to help support the total safety case hopefully as we move on to licensing.

And one thing that we wanted to bring up, and we'll discuss this also in the strategy, the safety strategy, is the importance of the geological setting. Often as you develop a system, as we look at the system, the repository for Yucca Mountain and the natural elements and the engineered design, it's really important to understand that we have a very good geological setting, and it really allows us a platform for understanding the system, for having a design, and sometimes that's missed when you look at the sensitivities and look at the very importance analysis, sometimes that gets left in the background. But we do recognize that we have a great setting, really, for the system, the barrier, and for the design that we're doing.

The postclosure safety case also obviously incorporates the PA, and as I mentioned, the additional elements that we'll talk about a little bit later to increase the confidence in that case.

And very importantly, we identify what we believe are the principal factors, and this helps us to prioritize what we need to do, the work, how we qualify data, all kinds of things. And as a part of this, the Rev 04 of the strategy will be a QA document. It will go through the full process, the QA procedures, and have transparency and traceability to everything that we have in there. And this was not the case in the previous versions of the strategy.

I mentioned the geological framework, and I have listed here just some bullets. I won't read them to you, but some of the things that we feel are important. And, again, sometimes some of these things get lost when you start looking at the bottom line curves and sensitivity, to realize that some of these attributes are very significant in terms of our confidence in our ability to come up with the design and a system that works for waste disposal. And some of these will come up a little bit later, but I did want to give a reference to the mountain and the framework that we have existing here for the Yucca Mountain.

Likewise, you recall that Bob Andrews talked about the attributes of the system. Well, when you look at the entire system itself, these are the types of attributes that the system allows us to have, and you've seen these before. There might be some slight evolving of the definitions as we move the strategy forward, but again, these are the types of things we want to do, you know, limit the water coming into the emplacement drifts, and hopefully have very long-lived engineered barriers, drip shield and waste package. And when they do degrade, or so, to the delay and dilute the radionuclide concentrations through the natural barriers, and then obviously, the last one, a new one for Rev 04, the consideration of the disruptive events and the low expected dose rate, even considering these.

And so you've seen we have the natural setting. We have the attributes that the system allows us to have. And then from this, we try to develop and understand what are the principal factors that we're going to make our safety case on. And so we evolved into that. And the principal factors, when you start thinking about these, you have a large set of factors considered obviously for the siting criteria and taken into account in the TSPA/SR, many, many factors. And, again, Abe and Bob discussed and showed a lot of these in the earlier presentations.

However, only the principal factors would be explicitly credited in the final safety case, and what I mean by that, on some of these factors, DOE has a decision to make in terms of how to credit, how much to credit, everything that is credited obviously has to be fully defendable with the Commission. It has to have a strong basis of defensibility. And so we want to be wise with how we choose what we're going to base the safety case on, and make sure that it's something that we can live with, we can defend, and we have great understanding of, and we understand the importance of the certainties around those, and that's what we're trying to get at.

We also identify them to obviously understand and increase the transparency of the analysis itself, understand what's gone on in the analysis, and as we discussed before, part of the essence of the strategy is the understanding and the treatment of uncertainty, mitigation of uncertainty on these principal factors.

And to do this, we have a large variety of, as you saw some of it, sensitivity analysis and very importance analysis. In the Rev 04 strategy, we'll have a few dozen different types of neutralization analysis. We'll also look at non-mechanistic infant value analysis and sensitivities in order to get a large amount of insight as to actually what's going on, try to unmask the entire system to really understand how it works.

Part of this is, we discussed it must have been a couple years ago, got into quite a bit of discussion on this, but use of neutralization analysis. And one thing I wanted to do is just try to gain that we have a common understanding of what we mean here. You've seen a lot of the sensitivity analysis and the degraded barrier analysis. Those analyses of course are within the bounds of the considerations of the PMRs and AMR studies. That's the best knowledge of this information, our understanding of the uncertainties.

The neutralization analysis steps outside those bounds, non-mechanistic, it's really to unmask what's going on in the TSPA to understand how the barriers, the different barriers contribute, to understand the system and multiple barriers, and that's what we're doing with the neutralization analysis.

And I'll show just some examples of this to go through, and this is just a simple schematic, nothing real here, this could be almost any type of a system. But on the very top there, you see somewhere you have, if you have no barriers, no systems in here, you have a certain amount of release, very high, it could be in the 10 to the 11, 10 to the 12, something like that. As you start including sets of barriers on here, you start obviously bringing that potential mean annual dose down and down and down. As you include all the barriers finally, as in the base case, nominal case, you have that result over there.

So to understand how the various sets of barriers or individual barriers contribute to bringing that down, and how you look at them, what order do you look at them, things like that, that's what the neutralization allows you to gain insight on, and it really helps to start unmasking. Sometimes you look at sets of these to understand the contribution of some of the barriers.

Likewise, on assessing the defense in depth, which is one of the key elements of the safety case, this is one of the elements that I believe is as important probably as the PA results itself. Basically, it means, as written there, failure of any one barrier does not mean failure of the system. You know, we try to have a system work so that we don't have any what you would call silver bullets in it. If there's one little element somewhere, if we're wrong about that, it's catastrophic. We don't want that. And so we try to analyze and unmask and understand the system to see how we have and what we have to do to build in defense in depth. And we would want to have--you know, the system failures require multiple independent low probability failures, and of course the probability of that is reduced through installing defense in depth into the overall system.

And you can't understand this only by looking at single barriers or single factors. You have to look at combinations and one offs, and things like that, and that's why we do so much analysis in order to unmask what's going on to understand what we have in here.

And so the complete assessment says the system requires neutralization of combinations of barriers or factors as well as individual neutralizations. I was trying to think of something to bring this to real life a little bit, and you know, if you look into one of these brand new buildings of the hotel in Las Vegas and you want to understand the superstructure of it, you know, you have to tear away all the decorative facade and all the wallpaper and the paint and everything else to see how is the structure supported, and all the different things. And that's likewise on the TSPA. You really have to tear the guts apart to get the insight of how the various barriers are helping everything.

I was trying to think of a real life example of where people do--that you can understand defense in depth and then to neutralize the barriers, and for those that grew up in Pennsylvania in the coal mine region 50 years ago, the way the operations were, my family ran coal mines and we would go in to try to design to figure out how many pillars of coal we would have to leave to support the roof, and so, you know, to have defense in depth to have enough pillars in there so if one fell down, the roof still wouldn't collapse. And so you'd go through and mine all the coal like that, and then when you close a mine, there's other people who would come in and try to get the easy coal, because they had the fillers of coal. So they would do the neutralization, and they would start pulling down pillars and understand, well, I think we can pull this one down because that one would still support the roof. And sometimes they were right; sometimes they were wrong. But that was a real life example of defense in depth and neutralization. So that's what we're trying to do here.

And as we do all this analysis, this gives us the insight at what's gone on, the understanding of the principal factors of the system. And to get into that, I have a couple--one more schematic showing the defense in depth analysis, and the two blue lines here just show a couple different barriers that may be neutralized, and you might get some small shift from, say, the base case. So each one individually maybe doesn't look like it does much to the bottom line dose, and that may be because each one of these may be acting as a backup to the other. An example of this may be if you neutralize the UZ and the UZ transport.

But if you do them together, you find you may get a tremendous shift, impact on the dose, because then perhaps there's not much backup left to those individual barriers. So you start getting a sense of the defense in depth and how even though in the plain sensitivity, you may not see much sensitivity to the particular barrier, but if you understand

and unmask it and see that oh, it's acting as a backup to another barrier, it could become very, very important and give you that additional assurance of safety.

So to identify the principal factors, as I said, we have this large set of neutralization analysis that we do. We have all the sensitivity analysis, all the degraded barrier analysis to try to understand how the barriers are impacting or the potential impact and function for the overall bottom line dose calculation.

The analyses are used to determine contribution of a factor. It really is not to explore what might happen. It's just to unmask and understand the analysis itself. And as the bottom bullet shows there, the neutralizations provide insight into the TSPA analysis. They don't indicate performance possibilities. Those are addressed in the horsetail diagrams that you saw in the earlier presentations.

So now we're looking at just a couple examples of some of the preliminary neutralization analysis that we have. As I said, we'll have dozens of these in the report. We were working on these last week and over the weekend. I just brought a few examples here that are preliminary. This one happens to show if you totally neutralize the waste package and the drip shield, and show the result against the base case here. And as you can see, the results really aren't that bad. It's a little above 100 there, and this really means that even with that totally, the waste package and the drip shield in there functioning, the rest of the system is still giving you somewhere along the 10 to the 9 reduction in terms of the potential dose.

So you start to get a sense of how the system is functioning, the type of backup we have to these particular engineered barriers and what's gone on here. The next example shows neutralization of the cladding, and here we just totally knock the cladding out at the beginning, early in life, and you can see you get a--here, a fairly small shift, about a factor of 5 to 7, or so, and this is complete neutralization now, and as you recall earlier when you looked at the degraded cladding results, you got close to about the same shift, and we found that one of the major factors here is really the impact on the chemistry when you remove the cladding here.

But you can see, looking at this, you can start getting a sense of what the barrier, how the barrier is performing, what it's adding or not adding to the overall performance, is it backed up or not backed up, what's it doing for other things, and you start going through a series of these and different combinations, you start gaining good insight as to what are really the principal things you have to be concerned with in terms of the bottom line dose, the health and safety of the public.

So then using these, we went through this. As I say, we've been working on this about the past half year. We've had a series, we started with a series of workshops. We went through all the FEPs. We went through all the AMRs and PMRs, and we brought in all the experts on everything to try to get their insight with what they thought was important.

We had preliminary sensitive analysis from TSPA. We now have a host of results from degraded and neutralization analysis. Out of all that, okay, this would be our preliminary list of principal factors for the nominal scenario now, not including the disruptive event. And you can see here's our geologic framework that I talked about, the principal attributes, and then the line-up of the principal factors or rocks of Gibraltar, if you will, for the safety case. And you can see we have seepage into the emplacement drifts. We've had that before.

Performance of the drip shield and drift invert system, and I'll talk a little bit later about this as I show the evolution from Rev 03 to Rev 04. Of course the waste package gets in there. Radionuclide concentrations, and colloid associated concentration. Now, this came in from the workshops. You heard a lot of discussion today on that, whether that is something that's important or not. We're still--that's still under review and analysis. And of course we have the UZ and the SZ radionuclide delay as principal factors.

The next slide shows for the disruptive event, and here, this is really looking at the indirect release of the igneous activity. The probability of igneous activity is a principal factor, directly related to that. The repository response to the intrusion. That means how much damage the waste package, how many waste packages, things like that, drip shield, engineered barriers. And then many of the other factors obviously were also on the nominal.

So if we look at all this together and compare it to where we were in Rev 03, that's the next slide, and if you look at this, a couple things probably come to mind. One is that the work where we are so far with Rev 04, does I believe a pretty good job of validating our earlier conclusions in Rev 03. First of all, you should recognize that in Rev 03, we didn't have consideration of a disruptive events. We didn't have that analysis. So these are new, but we recognize that.

The dilution at the wellhead, we have taken that off as a principal factor. That doesn't mean it isn't important. But we thought since that has such--is somewhat prescribed by the regulations, that that doesn't fall into the same category as the principal factors. So we've taken that off the list.

And you can see the others are pretty much the same, except for the site redefinitions. Again, we've added a drift invert system, and I'll show later on how that comes in with the drip shield, because that kind of acts as a system for both advective and diffusive release. And likewise on this, we've evolved that definition somewhat to include the colloid associated radionuclide concentrations at the source. But other than that, there's not a lot of change there, so I believe we do have a pretty good validation and, again, the Rev 04 will be--have full transparency and traceability of all the results and conclusions in the document since it will be a key document.

So that kind of shows where we are with principal factors. And now I'd like to move on to really discussing, maybe taking almost a step backward and talking about all the elements of the safety case. As you recognized, of course, PA is just one of those elements, a very important element obviously. But in terms of making the full assurance of safety case, we aren't just dependent on a bottom line result of the computer code for the PA, as the PA result is.

We also have, obviously, margin, defense in depth, consideration of the disruptive processes and events, insights from natural analogs, and performance confirmation. So all these elements together are what we call the safety case per se, make up the safety case and make up the assurance of safety. And I thought I'd just leave this up here a little bit so you can see that as we now go quickly through these one at a time.

TSPA, of course you've heard all about that. I don't need to say much more about that. You know it's all traceable. You know what's done there, the models. The bottom there, obviously the barrier importance assessments from that helps us to understand and gain insight as to what's gone on. We have to do an identification of the barriers important to waste isolation for regulations, and the description of the capability of these barriers and the basis for that description. And that's part of what we do.

Next slide is on the margin and the defense in depth. There's been kind of a standard approach to these in the nuclear industry for the last 40, 50 years. Safety margin, you saw from the base case results we are in fairly good shape with respect to safety margin. And we like to think of it almost like a two dimensional safety margin here. One in terms of absolute dose margin to whatever the regulations will finally come out to be in the first 10,000 years, and also a time margin as you look out, say, to 100,000 years.

We like to see margin in both directions, and as our base case results in the TSPA/SR right now are showing, we have an excellent margin in both directions there.

And this is good because I forget who brought it up earlier, but you always are getting little surprises here and there in terms of data, maybe a little here in the model or stuff like that, and you always want to have margin already built in there that you can easily live and account for these types of changes and stuff.

And you also want to use that margin wisely in terms of areas where you might be able to simplify parts of the code, or things like that, where if it's not very important, then you can take some of the complexity out when you go to meet the regulations.

So that's a little bit on the margin. And on defense in depth, again, this is one that I think is really critical. We hope we want to show no undue reliance on any single element in terms of the safety case, TSPA. And here, preliminary results indicate neutralization of any individual barrier does not exceed 100 millirems per year. That's pretty good results. And I'll show some information, some results on this a little bit later, but we're in pretty good position right now on defense in depth, and I think we can even get a little bit better, and we'll show some of the recommendations we have on that.

On disruptive events, you've heard a lot of information on that over the last couple days. This first slide shows kind of handling of almost everything except for the igneous activity, and how it's handled, you know, the seismic and the future climate changes, a lot of that is built right into the TSPA model.

And water table rise, that was shown to be not being credible in the FEPs AMR, so that's not part of the model. Postclosure nuclear criticality, that is excluded in the FEPs AMR, partly because of the long-lived waste package. And all these would have bases that will be described and documented in the AMRs. And, of course, inadvertent human intrusion is addressed as a separate scenario, as dictated by the regulation.

On the next slide, we show information on the disruptive events, and as you've seen already, the direct eruptive release scenario has a mean probability that is occurrence in 10,000 years that is less than one chance in 10,000. So we are going to evaluate this scenario, but do have a consideration of not including it in the licensing case. Per the regulation, we could exclude that, if we have a firm and valid basis for the mean probability.

On the indirect release scenario, that is, as you've seen, sufficiently probable that warrants consideration and is explicitly treated in the TSPA and with the groundwater release scenario, and will be combined with the base case, the nominal results for the overall TSPA results.

On the natural analogs, currently the analog information that we have is somewhat limited. I know we had a discussion on the importance of this near the end of the panel discussion. Here are three areas where we do have natural analog information that is being utilized in the PMRs, and certainly, you know, where you have a good natural analog that you have confidence and you can show a basis for, you know, being part of the Yucca Mountain, defending the model, you want to make use of, so we are certainly evaluating other studies to possibly provide additional confidence building information.

And I know we heard a few suggestions today from the Board that I'm sure we'll look into. This can be a very important element of the safety case. We do have to be careful we don't overstate our usage of it to possibly lose credibility where we can. It obviously can be very important to help defend the type of models that we have and reduce the uncertainty on those models.

On performance confirmation, this is one that we've had a lot of discussion on. Part of our thinking on this is that the principal elements, where we can infer or where we can show through testing, through the preclosure period that would support the assumptions or the bounds of those principal elements, obviously that's types of performance confirmation that should be dealt with.

Performance confirmation I believe would become a formal part of the license, kind of like surveillance requirements for preclosure. Testing we believe is dictated by three considerations that we have listed there. Certainly there are some that would be requirements of the regulation. Those that we can use to address the principal fractures, such as perhaps further testing on the materials for the engineered barriers is an example. And also any decision-making associated we say with permanent closure or possible need to exercise the retrieval option, and this will also be addressed somewhat in the safety strategy.

And so these are the areas. Now, there's obviously a lot of testing that you can think of during the preclosure period, and I think our way of thinking is that obviously a large part of this testing would be to support these considerations and be part of the formal performance confirmation, formal part of the license, and other testing would be that testing that the applicant would deem important to them, but perhaps not part of the license per se. So that's the performance confirmation. And, again, some of these five elements together help make the overall safety case, help bring your assurance of safety for this.

Next, I'd like to talk a little bit about where we are, what we see happening in terms of as we proceed hopefully to the licensing application. And in the event the

Yucca Mountain site is found suitable for the repository, obviously a licensing application would have to be prepared. And in this event, we would have certain issues that perhaps would have to be addressed to ensure defendability and credibility of our safety case for that postclosure safety case LA.

And as part of our workshops that we went through the last half year, we tried to identify each and every issue that the experts, the labs, the PMR leads, that anybody felt perhaps was important in terms of their case and everything, and I wanted to identify a few here, not all of them, but a few of them that have come up, and perhaps what we could do about it.

First, as you might recognize, the issue, the waste package performance, obviously very important, critical to the defendability of our safety case. And the technical basis obviously for the models must be sufficient to justify probability of the waste package failure before 10,000 years is very low. We believe that. We have to be able to show that.

And part of our approach here is obviously to continue to increase the database for waste package degradation, conduct modeling to evaluate the consequence of the low probability modes, and third, perhaps very important, hopefully to show defense in depth to address the residual uncertainty that we have with the waste package, to show that it has been properly mitigated, in other words, to show that the waste package uncertainties are not overly important, and to do that through defense in depth.

And speaking of defense in depth, I believe an essential element to the safety case and first of all, to prevent undue reliance on the waste package, for example, and we've talked a little bit about this, I'll show some information on this shortly, but right now, we believe we do have a conservative representation of the drift invert diffusive transport model, and it does not completely support what I would consider full, very robust defense in depth.

And the approach here is to do additional studies of drift invert diffusive transport model to help verify Conka's conclusions in its paper. We'll show some results here using 10 to the minus 11. Part of Conka's conclusions were that the arch really broke down for the very low moisture content, and that the diffusive coefficient really went very low, even much less than 10 to the minus 11, and if we can do some independent testing to either verify or not verify, or see what conclusion we can come up with with respect that, that would certainly be a great help in terms of enhancing that defense in depth story. And also to look at other conservatisms in the flow and transport model that could impact diffusive release.

And the next slide shows kind of a story. There's a lot of information on this slide, and this is one of our defense in depth slides. The top line here is what happens if I totally neutralize all the waste packages early on with a big 100 centimeter squared patch right off, time zero. So all the waste packages are caput. And you can see the results here are really pretty good, 100 millirems per year.

SAGÜÉS: You said 100 centimeters square?

ROBINSON: Yeah. A patch on every waste package.

SAGÜÉS: 100 centimeters squared is big.

ROBINSON: Yes. So that's what's done there. And, again, this--just looking at the red curve, it does represent pretty good defense in depth. The other, the natural systems, the other barriers and everything, are doing a reasonable job at backing up that waste package, even in situations like this.

Now, all that release up through here is totally diffusive release, because the drip shield is still functioning. There's no advective release at all. And so to think of what can I do to enhance that defense in depth, I have to do something that would impact my diffusive release. And, of course, the first thing, one of the first things you might think of is looking at the assumptions in the modeling for the invert diffusion coefficient.

The base case is shown here, and both the base case and this case have the same diffusive model, same understanding. This slide here, I hope you can see that, it's in blue there, that is the neutralized waste package with a 10 to the minus 11 diffusive coefficient. And what that shows you is that when I have that, all of a sudden, my drip shield and my invert are really functioning together to really knock off both advective release and diffusive release, and it is really a robust defense in depth. I mean, this totally backs up all the waste until you get out here, this is the first drip shield failure, and then all of a sudden, of course you get the full advective and you lose your diffusive release.

So there's a lot of information that comes out of a picture like this. So you can kind of gain an understanding of how when you start looking at these and you look at one offs on the neutralization and everything, you really start unmasking what's gone on and gaining an understanding of how various barriers come into the picture, whether it be seepage or anything else, and you get a picture of the type of releases that are coming out, and it kind of gives you insight as to what you may do to help improve your assurance of safety case.

And so this is, again, the types of information that we use to try to come up with first of all, how things become principal factors, second of all, to recommend areas that we may look in to enhance the safety case. And so to me, a picture like this really has a lot of stories, a lot of information on it when you start analyzing it and tearing it apart.

CRAIG: Could you explain how the diffusion coefficient comes in? Where in the model does diffusion--

RICHARDSON: That's the invert.

CRAIG: All of the invert?

RICHARDSON: Yes, just the--this is just with the invert right underneath the waste package.

CRAIG: The neutralized waste package assumes no invert also?

RICHARDSON: The base case and this both have an invert model in it. It's the normal one that's in it, but we believe it's fairly conservative. Okay? It uses arches law and everything else. This is the identical waste package neutralization, these two cases, the only difference is the invert diffusion coefficient now for this is reduced to 10 to the minus 11, and that's Conka's conclusion says that it's less than that.

So I wanted to get with the one off of the waste package neutralization, get an understanding of how the invert is impacting my defense in depth conclusions on this. So that's what this is for. Does that help? Okay.

NELSON: Can you explain what exactly do you mean by mean dose rate? Is this for a nominal case?

RICHARDSON: Yes, this would be the same basis as your base case. Okay? Except I've neutralized the waste package. I've taken the waste package barrier to water out of the picture.

COHON: I'm sorry to keep interrupting, but you haven't taken the waste package away. You've put holes in it; right?

RICHARDSON: Well, yes.

COHON: Okay.

RICHARDSON: Times zero.

COHON: I understand. But you have not taken it away. You've put a hole in it.

RICHARDSON: But that's all you need now to get the diffusive release on it, full release.

Another issue is a little bit related to the last one, but the issue of possible over conservatism. And in general now, where appropriate, this lends confidence to the case, allows you to simplify, allows you to get maybe rid of some complexities in the modeling. However, it also, you can see just from the last slide, it can limit detailed understanding of the overall system. And it could be inconsistent with the overall risk-informed, performance based approach.

Part of the approach here again is to assess over conservatism in some of the key models, especially ones that may impact some of the elements of the safety case, like defense in depth, and we mentioned a few there, the in-package transport model, that could be including thermal effects that could also give a natural barrier in case of waste package degradation.

We've already mentioned the drift invert diffusive transport model. The UZ and SZ transport models also help, could help to limit the diffusion release coefficient.

And then model stability. It's not good to keep changing the models for the safety case. Normally, you always enhance, that's desirable. But the prospects for significant changes affect confidence in the current models, and especially with the Commission that has to finally end up reviewing all this.

And the approach here is really to focus on models in areas associated with the principal factors, and except for significant changes, you know, changes that would be non-conservative, or new data that comes into that shows that perhaps the assumptions were wrong that you had, except for those, really to maintain the models from the SR to the LA, and use the new information or enhancements to really help bolster the defensibility of the margin type of arguments. And there's precedence for doing this in industry, too, on the commercial side. There's always model enhancements gone on with the codes, but rarely do you step in and use that new model, but you have it as a backup to show and to help the assurance of safety and to show margin, and things like that. So this would be the approach that would be recommended as we hopefully transfer to the licensing application.

So a summary of all this, the repository safety strategy does focus on increasing the confidence in the safety case, including, as you saw, the TSPA analysis. It will provide transparency, identify key uncertainty treatment. It works with all the elements of the safety case. A key element, one of the key elements certainly is the margin and defense in depth to address those unquantified uncertainties and to hopefully show that no uncertainties are overly important. We've got to show that they're properly mitigated through defense in depth.

And of course important to the strategy is the scientific soundness of the TSPA sensitivity and barrier importance analysis.

So part of the heart, part of the essence of the strategy, one, is to formulate all the elements used to make the safety case, not just dependency on TSPA. Part of the heart of it is to address uncertainties to make sure that uncertainties, if they're not reduced, are properly mitigated, and to have a defensibility of those principal factors when we do get to the licensing stage.

So that's the presentation.

CHRISTENSEN: Dennis, thank you. We do have time for a few questions, and I'd like to ask really a question of clarification that comes from the audience.

Just to be clear, on your graphs where you plot doses, those are doses at 20 kilometers? They're comparable to the charts that we saw throughout the TSPA?

RICHARDSON: That's right, yes.

CHRISTENSEN: Board members? Dr. Cohon?

COHON: Could we go back to Slide 12? Does neutralization in this case of the waste package mean the same thing it did in the later graphs?

RICHARDSON: Yes.

COHON: So there's a hole in it?

RICHARDSON: Yes.

COHON: What about the drip shield?

RICHARDSON: Oh, the drip shield means that it doesn't divert any water. The water coming into the drift drips directly on the waste package, no diversion of water by the drip shield.

COHON: So the drip shield is basically removed?

RICHARDSON: Yes.

COHON: And the only question occurs to me why? I mean why did you do the waste package--why does neutralization mean this now, when I believe when we saw the barrier neutralization studies in the past, they represented complete removal of whatever it was, in this case, the waste package?

RICHARDSON: Oh, boy, Larry I think has insight on that.

RICKERTSON: This is Larry Rickertson from the M&O. Let me just make one point about 100 square centimeter hole. Most of the radionuclides that come off are solubility limited, so it doesn't depend on how much is exposed, just whether they're exposed. So in the sensitivity analyses that people have done about the size of that patch, whether it's 100 square meters or 200 square meters--square centimeters, you get the same answer. And so in a sense, it's completely neutralized. This is, in fact, the same approach that was used last year. We had a certain size patch. Now, that patch isn't just a patch on top; it's a patch on the bottom, too. So it's two patches, if you like. So that it's complete exposure of effectively as much as you can get.

Now, that's a funny answer. That's a funny kind of answer, but it's an artificial calculation to reveal what's going on. So it was enough to reveal what would happen when you take the waste package away, and that's the purpose of it.

COHON: So the word neutralization means the same now as it did a year ago?

RICKERTSON: Yes. It means an artificial calculation.

COHON: I understand that. And does this curve look more or less the same as it did the last time we saw this?

RICKERTSON: Other aspects of the model have changed, and so what you saw was the peaks were more pronounced. Iodine and technetium were coming out early, and that was a peak, and then neptunium came out later. In the updated models, neptunium was moved forward in time, comes out sooner, so that peak, that first peak you see is a combination of neptunium and iodine technetium. So it's a little bit different, but roughly the same. It's down a little bit in magnitude. It used to be up in the order of about 10 to the 3rd, that first peak, and now it's down a little bit. But that's also due to refinements of the model. So it's effectively the same, I think.

RICHARDSON: Yeah, part of that reduction of the peak I believe is due to the evolution of the model for the high-level waste for the glass test dissolution rate. During the VA days, I think we had a very, very conservative very early dissolution rate, a few hundred years on the glass, and now we have a much more robust defendable model that's longer than that.

COHON: Thank you.

CHRISTENSEN: I've got a line-up of questioners here, and we have a limited amount of time. I've got Dr. Craig, Bullen, Knopman, Sagüés, Dr. Melson, and then several staff members as well, Dr. Metlay, Dr. DiBella and Dr. Reiter. We don't want to be here all evening, so if we can keep the questions relatively short and not overlapping, that would help.

CRAIG: Craig, Board. I'm glad I got my hand up early.

That certainly is one of the most interesting curves I've seen in the whole meeting, and I'm glad you did it.

RICHARDSON: Which one?

CRAIG: The one that's on the board right now. And in terms of thinking about that, could we go back to Number 11, the one that just preceded that? Because there on the second bullet, you've advised us that we're to determine contribution, not to explore what might possibly happen. I'd like to understand what you mean by that.

There are those around who consider that passivated films might fail, and that two years of data in dip tanks is not enough for C-22. For the people who have that kind of concern, it seems to me that this is a discussion as to what might possibly happen, and it's going to be used that way regardless of your attempts to argue that it's something different.

So I'd like to understand what you've just--talk to me about that second bullet, what it means to you.

RICHARDSON: That's a good question. Partly what it means is we have, as you're aware, obviously been working very hard on the AMRs and the PMRs, which is really the documentation of our belief in terms of the models, in terms of the waste package, and everything else. And so I have gone outside that box, totally non-mechanistically in our thinking, to do the neutralization analysis.

So from that viewpoint, it isn't something that we would expect. It's really done to gain the insight of what this barrier is doing, is there backup for the barrier, understanding the overall total barrier contribution. But in a sense, it's totally outside our belief in terms of what we believe through the AMRs and PMRs and everything, as Bob Andrews discussed earlier, this is not what we would expect. We're really doing this to unmask what's going on within the confines of the dose calculation, and how the barriers are working. So that's what I meant from that statement.

CRAIG: But that kind of an analysis can do a lot to help your public and folks like us understand the strengths.

RICHARDSON: Sure. Again, as I said, these analyses really unmask the TSPA, helps you gain understanding of the multiple barriers, what type of backup we may have for barriers, helps you look at, you know, removes certain barrier functions and see the impact of that. You really get a lot of insight on that.

CHRISTENSEN: Dr. Bullen?

BULLEN: Bullen, Board. Actually, can you go first to Figure 12? And in this case, what fraction of the waste packages never see drips?

RICHARDSON: The same--that has not changed. That's the same as in the base case.

BULLEN: So 30 per cent of the waste packages see drips and 70 per cent don't?

RICHARDSON: I'm not sure of the exact number, but whatever the base case is, that would be the same here.

BULLEN: Okay. So essentially that 10 to the 9th reduction is just in the area where they would have gotten wet anyway?

RICHARDSON: Yes.

BULLEN: Okay. I guess I have a question, since you brought up clad credit, I might as well as you a couple things now, because you mentioned that none of the models are going to change between--or not change significantly between SR and LA, and so the question would be then what additional data might you need to take clad credit as you go to the NRC? Right now, we had people talk about pellet/clad interaction and creep rupture from the inside as being a problem. We also don't know much about the exact thermal history or the power history of each of the assemblies. And if you look at burnup credit as an example with the NRC, burnup credit might not be allowed unless you do a survey of every individual assembly to verify in some measure and form how you're going to do that.

So the question I want to ask you is in a cost/benefit analysis of clad credit, if you're only getting a factor of, I don't know, three, four, five, how much money are you willing to spend to go after that little bit of credit that you claim to be getting based on your neutralizations?

RICHARDSON: Dr. Bullen, I think you're reading my notes on this. No, that's an excellent question, and what I meant by models not changing, if I could make a comparison in the commercial nuclear industry? A lot of the safety analysis codes are very, very robust with everything in the kitchen sink included in them. Okay, control systems, all kind of stuff. But when we run the case for the license, a lot of that stuff, 40 per cent of the code is turned off. You don't credit it in the licensing case to take those issues off the table.

Likewise with cladding, DOE will have an opportunity to do--look at that cost benefit and, hey, if I credit the cladding, this is what I get in the benefit. This is the cost associated with meeting Appendix B and everything else to credit that.

If I were going to go out and make a recommendation right now, I'd probably say I don't think I want to credit cladding for my LA. But these are the type of discussions and decisions that DOE will make shortly, and by not changing the model, what I meant was turning off part of the model I don't consider that as change in the model. It's just, you know, how you credit parts of the model and don't credit part.

BULLEN: Bullen, Board. I understand that, and let me just get my last question in and then I'll not take up too much time. If you'd go to Figure 23?

In your performance confirmation, one of the things that you want to be able to test for is that the barriers important to waste isolation are performing as expected. But if you have the current repository design where you don't see the thermal pulse until after you close the repository, how are you going to know anything? You won't see the response in the mountain. You won't see any of the issues associated with the response in the confirmatory testing stage, so you won't have the data.

Now, the converse of that is if you kept the repository cool, then during the course of the confirmatory testing stage, you might have a lot of data about how the rock dries out and how much water there is and the movements under ambient conditions, or conditions that aren't going to be above boiling, thus, reducing the uncertainty, if I could quote Ernie Hardin. He did say that if it was cooler, it was less uncertain, so I'll remember that. But I just wondered what you might see for barriers important to waste isolation.

Prior to, you know, closure, you're not going to have much data unless you do something. And what might you do?

RICHARDSON: Yeah, that's a--well, that's a tough question. I might have to pull in a friend to get that answered. You know, just off the top of my head, and then I'll let the audience chime in here, we will have to show that any native considerations like thermal effects, like anything else, are appropriately either considered or bounded in terms of the negative impact on dose calculation. We will have to be able to demonstrate that in defensibility of the licensing case.

I'm hopeful that the TSPA will be able to uncouple itself a little bit from some of those types of issues by appropriately bounding the native considerations, or doing something else to reduce those uncertainties. And I'm not sure if we know exactly what that will be yet, but Abe will help in this matter.

VAN LUIK: Yeah, can I be your friend?

RICHARDSON: Yes.

VAN LUIK: This is Abe Van Luik, DOE. One of the things that we have under active consideration is actually sealing off a test drift without ventilation to look at exactly those type of impacts before the permanent closure. But this is under active consideration at this point.

BULLEN: But keeping it cool would be another way of reducing that uncertainty. thank you.

CHRISTENSEN: Dr. Knopman? There's seven minutes or so, so please--

KNOPMAN: Two quick questions. One, back to 12. Is there a reason why you didn't put the time from zero to 1,000 years on there?

RICHARDSON: It's just the way--we got the results plotted from TSPA. I guess it just was easier to show it this way.

KNOPMAN: It would just be interesting to see what it looks like, because that would say something about your other assumptions and how that comes into TSPA in terms of travel times.

Second question, I just wanted to clarify. You said the red line there where your neutralized waste package drip shield represents a 10 to the 9 reduction from--

RICHARDSON: Approximately.

KNOPMAN: From what? From having all the waste sitting in Amargosa Valley?

RICHARDSON: Dissolved and, you know--

KNOPMAN: Just sitting there?

RICHARDSON: And no barriers, you know, just--so it gives you some indication. We have a system here of the natural barriers and engineered barriers, and even without these two things, we have a reduction of about 10 to the 9 in terms of magnitude of the expected dose.

CHRISTENSEN: Sagüés, and then Dr. Parizek.

SAGÜÉS: In looking at that figure, I was saying to myself how amazing it is that when you neutralize the waste package, you end up to within an order of magnitude of expected regulatory limits. Is that a coincidence?

RICHARDSON: I'm not sure I quite understood the question.

SAGÜÉS: Well, the regulatory limit would be, what, like about--

RICHARDSON: 15 to 25.

SAGÜÉS: And internationally, maybe you're talking about maybe a hundred. You take off a little bit. So anyway, we're awfully close, I mean, considering this, is it a coincidence?

RICHARDSON: I always like to say we don't make this stuff up. But, I mean, this is how the results came out with the present TSPA/SR model.

SAGÜÉS: I must say that this is the kind of thing that to an external reviewer, it sounds noteworthy.

CHRISTENSEN: Dr. Parizek?

PARIZEK: Parizek, Board. Is the difference between the red line and the black line in Figure 12 the answer to my question to the panel? That's the roll of geology?

RICHARDSON: Except for cladding credit, dissolution rates, yes. All the other barriers are there. All the other systems. It's the system without those two barrier functions.

PARIZEK: But that's cladding plus dissolution rate of the waste form?

RICHARDSON: Sure, UZ, everything.

PARIZEK: Whatever happening to climate? The TSPA-98, we had all these little kinks every time it went super pluvial, and they've vanished in all the runs we've seen in the last two days.

RICHARDSON: I'm sure somebody--I know almost anybody in the audience can answer this better than me. But part of it, you're talking about on the base case here now?

PARIZEK: Well, in any of the runs.

RICHARDSON: Part of this--the reason I think part of this is from diffusion, and it doesn't--you know, whether you have a lot of flux or very, very little flux, it's not going to impact your diffusion release very much. Is that close? So in that viewpoint, the amount of infiltration, precipitation, isn't going to, especially early on, maybe much later on it will, and we have, what, two or three--we must have three climate changes in through here in the 10,000 years. I think one goes about 700 or 800 years, another takes off to about 2,000, and then the glacier comes in through the rest of the time.

COHON: Wait a minute. This one is without the drip shield. So it's not just diffusion; right?

RICHARDSON: Right.

COHON: There's advection, too.

RICHARDSON: There will be advection, sure.

COHON: So why wouldn't that be sensitive to climate changes?

RICKERTSON: This is Larry Rickertson. You know, stay tuned for the RSS. You'll see curves where that ringing comes in. That has been stripped away and you see the ringing, so you'll see some effects. This is effectively that curve up there, even though the drip shield and the waste package are taken away, that invert hasn't, and so it is still controlling, it's still a diffusive release. It's still largely dominated by diffusion. So it's damping out that--the advective part that has that ringing in it, that little bit of oscillation, is much lower in magnitude, so you don't see it. You'll see this in the updated curves, you'll see traces of this effect.

RICHARDSON: You also have that cladding in there, too, that helps.

RICKERTSON: If I can, can I just make another comment to what Debra said? She mentioned that she would have liked to have seen it at 100 years. This illustrates the point that was made that this unmasking strips away what's in the model, what's in the calculation. It doesn't get at what the physics is that wasn't included in the calculation. So if you don't see effects that you would have expected to see due to heat effects and those kinds of things early on, this would reveal them.

So the very question that she asked is the question that should be asked every time. That's the point of these unmasking kinds of calculations.

CHRISTENSEN: Dr. Melson?

MELSON: Yes, please, Bill Melson, consultant. Would you go back to 21, please? If you allow for intrusion into the repository and its effects, the probability that that intrusion, that the dike releases surface is judged pretty high by most of us.

RICHARDSON: You're talking about the direct eruptive release?

MELSON: Right. So I think to release that certainly isn't kind of what most of us are thinking about, that that really ought to be considered and evaluated.

RICHARDSON: It's kind of a--this is a call that DOE will make. It depends on how defensible we believe our basis is for the probability calculation. But according to draft Part 63, strictly you can exclude an event if it's less than 10 to the minus 4 over 10,000 years. And right now, our mean calculation meets that criterion. However, I believe even if we pursue that path, we would still want to have a back pocket calculation showing the consequences anyway. But strictly according to the regulation, and in fact I asked this at--we had an NRC tech exchange a few months back, but you can exclude this event. But you have to have a defendable basis, obviously, for that probability excursion.

I don't know if anybody wants to add to that.

CHRISTENSEN: Given the importance of this and the fact that we've got several staff members, we've given a little bit more time, and I want to invite Dr. Metlay and then Dr. DiBella and Dr. Reiter to pose their questions.

METLAY: Dan Metlay.

RICHARDSON: I know this question. It's too hard. Go ahead.

METLAY: We talked a little bit about this. But I think it's important to get it onto the record as well. You've talked about the RSS in terms of building confidence for a license application. Of course, there's another decision point that's coming up perhaps within a year. And so the question is how useful is this strategy for building confidence for a site recommendation? And so I guess what I would like to do is give you my assessment of where they are in terms of the strategy, and then have a real quick followup in terms of the implications of that.

And I guess the first thing I'd ask you is your assessment is substantially different than mine. I guess I would argue you really have six pillars. I would separate out safety margin from defense in depth. I think they're conceptually different, and I think thinking about them is more useful if they're separate out than put into a single bucket. So if we take that as a starting assumption, there's probably six pillars of wisdom here, six pillars of confidence. It seems to me that three of them are not independent, that is, they all rely fundamentally on TSPA, and those three are obviously TSPA, discussion of disruptive events, and safety margin.

So the degree to which you believe TSPA, then you will also believe your discussion of safety margin and also disruptive events.

So that leaves three additional pillars left. I think the discussion that you made and Dr. Runnells made would lead me to conclude that the availability of information for natural analogs is not likely to be significantly different in a year than it is today. Is that a fair assessment?

RICHARDSON: That's, I would say, probably yes. Obviously, we want to take whatever credible credit we can for natural analog.

METLAY: I do understand, but as you indicated on your slide, that data is now limited, I don't know money the program has allocated for the next fiscal year. But realistically speaking, if we're talking about an SR and a year from now, we're not going to have much more natural analog data.

RICHARDSON: I would concur.

METLAY: Okay, that leaves two more pillars in your strategy. The next pillar is performance confirmation. That's a set of promises for the future, and we've had the first draft of the performance confirmation plan that hit the street to give us some indication of what those promises are.

As I read it at least, of your six principal factors for your nominal scenario, three are totally absent in your performance confirmation plan, and it's certainly arguable that you're not going to get a lot of good information on some of the other three in the 50 year period that the plan talks about. So that leaves defense in depth, and I think the Board on a number of occasions has pointed out the importance of defense in depth, and the importance of developing an independent and multiple lines of arguments, and I think we can begin to see some of that being developed in this presentation.

So I guess now I'll throw it over to you, and ask is your assessment of where the strategy is today and a year from now significantly different than mine? And then a trickier question, which if I were you, I wouldn't answer, but maybe someone else might want to, is it appropriate to make an SR decision at a lower level of confidence than a licensing decision?

RICHARDSON: As I said, very good question. Yeah, just to comment on a few viewpoints, yeah, I also think in my mind of margin and defense in depth are kind of two different animals. I think they're used to gain confidence in two different ways. Even though margin obviously comes right off of your, you know, the base TSPA, I feel a little bit better like if I have three or four orders of magnitude below whatever my final regulatory limit than if I'm about up against that limit, because that gives me, margin is margin, and it gives a little wiggle room for things to go bump in the night, both on that and also on the time.

Defense in depth, I agree, I think that is as critical an element as the TSPA. I've always felt that way. I think we can do a lot to enhance and to develop the basis for how we feel about the defense in depth, and I think we're trying to identify a few areas that can help that. I think we have some pretty good defense in depth right now. I believe we can show it better.

On natural analogs, I concur with what you're saying. On performance confirmation, we'll see what we can do there. I think there probably are a few things that we can do to try to infer, as Abe said, not only for heat or some of the other things, but also to help infer that some of the bases, some of the assumptions that we have based the principal factors on are indeed sound. Some might be very difficult. There might be no real good way. In commercial nuclear, there's a lot of things you have to infer from some indirect measurements, and you do the best you can do there, and then you put in the appropriate margin for uncertainties on that inference to ensure that you haven't violated the basis of any assumptions.

We will continue to try to enhance and involve the elements of the safety case. And, again, this is somewhat-- well, not somewhat, it is preliminary because we've only had just a few days really to try to digest all the data that we have asked for and have gotten, and then to figure out, okay, what does it mean, what do we do, what should we do in the future. We may not be able to do a whole lot of new stuff for the SR, but I think we can certainly do some enhancement to make those elements stronger for the LA.

And, again, I believe in the SR, you know, if you look at draft Part 9-63 and some of the stuff, we really need to show that we have a good belief that we'll be able to meet the requirements of draft Part 63. And, of course, as we go to LA, we have to meet them in a defendable manner.

So that's how we'll proceed forward, and we'll just work as hard as we can to ensure that we are doing things in a credible, defensible manner, and I think the real start to that will be the Rev 04, which will be a QA document, and at least show the basis for where we are at this point in time, and what we believe we further need to do as we march down that road.

METLAY: I notice you took my advice and didn't answer the followup question. Maybe there's someone from DOE here who would be interested in responding.

BROCOUM: Steve Brocoum with DOE. The SR decision is a major decision. It's probably the most important decision DOE makes in this whole process, whether we decide to go forward, and it's really the Secretary's decision, and he will take into account all the information in the SR, the comments he gets from the State and other interested parties, the information he gets from the NRC on the sufficiency, and any other information he deems that he needs to have.

So I can't tell you what that decision is, how he's going to make it exactly. We are going to give him the SR/CR and presumably the SR, for him to make that decision. But it's the single most important decision the DOE makes. It's a recommendation. It's not even a decision. It's a recommendation to the President. Then that's a positive decision accepted by the President, then we go into the very detailed licensing proceedings, which will be at least three years, with the NRC. And this will be dissected, a whole safety case will be dissected as the NRC can expect in many different ways, and it will be all looked at very carefully I expect in that whole proceeding.

So I can't give you a clearer answer than that. But this--the DOE decision is fundamentally a policy decision, it's a policy to the country to go forward, that the decision is coming up.

CHRISTENSEN: Dr. DiBella?

DI BELLA: Thank you. My question was already asked and so I'll pass the mike down to the next person.

CHRISTENSEN: Dr. Reiter?

REITER: It's just a quick comment, and then a question. In response to Dan's question, the implication is defense in depth is independent of performance assessment, and it seems that a lot of the calculations showing that you have defense in depth, at least now, are based in large part upon performance assessment, and in many ways are subject to some of the problems, particularly different levels of conservatism, may mar the contributions of different components. So you may not get an accurate description of what defense in depth is. That's a comment.

The question is Dr. Parizek asked you a question earlier on and you said yes, well, what level does a natural barrier contribute, and you say it adds a lot. And I'm just wondering, what we haven't seen here is anything about the contribution of the saturated zone or the unsaturated zone, or retardation or anything like that. So what is the basis for your answer that it adds a lot?

RICHARDSON: Thank you for that question. I meant to add additional information on that. We have--I haven't brought, obviously I haven't brought all the analyses that we have, and we are doing neutralizations and looking at different natural barriers, and I tried to give some indication of some of the results, and some again is somewhat masked by the invert, if you understand what I'm saying, because a barrier that impacts advective release early on with the invert model we have right now, is not going to show much, just like the drip shield.

So you have to do a number of different one offs to gain the insight as to, boy, given this condition, how is that barrier doing, and is it acting as a backup for something else. Right now, if I would look at the UZ or the SZ transport and take that function away just by itself, I'm not going to see a whole lot of change because of the backup of one to the other. If I would take them both off, it shows they're acting as a defense in depth, and I would get a pretty major change.

So those are the type of viewpoints that we're getting that show that the natural barriers do play a very important role and come in, but you have to look at them in special ways to understand how they, as a whole system, act in terms of helping defense in depth, backing up other barriers, considerations like that.

And also again, as you saw, removing some of the main key engineered barriers, it's the natural barriers that, you know, are knocking that dose down eight and nine orders of magnitude. And also, I tried to infer at the beginning that the geological setting itself, which is the mountain, really provides a terrific platform for the repository system. And often you won't see credit per se for that in the sensitivity or defense in depth calculations because it's kind of designed for. But if it were thought that, you'd have a hard time.

I hope that helps a little bit. I'm sorry I don't have other analyses and stuff here to show you. But we will have all these analyses and stuff in the Rev 04.

CHRISTENSEN: Dennis, thank you. I think we probably need to bring this part of the session to a close. And, Abe, I'd like to invite you to put a wrap on our discussion on TSPA, if you would.

VAN LUIK: This won't take very long. As I was trying to figure out just what to say in this meeting, it occurred to me when I gave my talk this afternoon that what I really wanted to convey to the Board and to the assembled public here is what's on the first two slides, which I skipped over, in this presentation.

And if we can go to the first one, if we look at a document written by Nuclear Energy Agency people, in fact, I was part of the group that wrote this, so it's a little bit prejudice, but it's 14 nations and the IAEA and the European community all agreed on this language. "It is appreciated that decision making requires that the technical arguments, including performance assessment and arguments that give confidence in its findings, are adequate to support the decision at hand, and that an efficient strategy exists to deal at future stages with uncertainties that may compromise feasibility and long-term safety."

You know, I would suggest you read the whole document because there's a couple of other clarifying paragraphs on this. But the point is that you have to look at the stage at which your repository program is. Are you receiving wastes and incurring radiological risks? Are you contemplating a decision that commits the nation to spending a lot of money? Those types of considerations have to go into whether or not the level of confidence that you have in the calculations at this point support that decision making.

And that's why I said earlier VA, I felt we were not there. SR, I feel that once we get through with the process that we have outlined internally of checking and making sure that everything is correct, I think we're ready to make that societal decision as Steve described it, exactly as Steve described it, and then comes the decision which weighs more heavily on are you willing to go forward and anticipate spending so much money to construct this thing and spending so much money--not so much money--but also a few years later, five years at least, beginning to incur the radiological risk of actually transporting and moving waste into the underground. So, to me, there is an escalating need for confidence in the modeling.

Now, if we go to the next page, I think that we are following this exact logic in the construction of the SR. We are estimating system performance, and as we have discussed here roundly, there are uncertainties in the modeling. There is a credibility problem with some of the modeling from some of the external experts, and, you know, it's an indication that we have not nailed this thing down to the point where everyone that looks at it will say oh, yeah, we believe this.

But we are looking at quantifying uncertainties and we are, you know, because of the recommendations by the Board, we are seriously trying to improve that aspect of things. And you heard a lot of things today from the process model people that show that they are busily evaluating uncertainties and trying to bring up the confidence level that you can have in each one of the models.

And then also, we have a safety strategy that discusses confidence, and also discusses steps forward. Now, the reason that we're still doing steps forward is because we do believe that there's a difference in the degree of assurance that's needed between SR and LA, and we will continue to do that afterwards also.

If you look at the performance confirmation plan, you see that it is focused both on regulatory requirements and on larger scale issues like not losing an opportunity for collection of data that, you know, is a once upon a time opportunity, keeping the seismic network in place, for example, just in case there's an earthquake and you want to learn from it. And there's a lot of other considerations in the plan that we have for performance confirmation.

So I think when you look at the stage that we're in, I think that the SR and the TSPA that feeds the SR is at an appropriate level. If we, the DOE management above me, especially did not think so, we would say we're not ready to make this decision.

So I think that's a good setting for the whole discussion that you've heard today. Yes, there are uncertainties. Yes, we are looking forward to the opportunity to do some natural analog work, and we do have some plan for next year in the field. But it will be two years before that pays off in terms of new insights and modeling improvements. And, yes, we do have plans to look at the lithophysal zone more carefully, and probably reduce some of the uncertainty in that modeling, and we do have plans to continue the work in the saturated zone, especially, and then I have a few pet things that I would like to do also. But we are continually looking at improving the basis for decision making as decision making gets closer and closer to taking on the actual radiological risk.

So I think, you know, that's all I wanted to say in a wrap-up sense, is that this discussion today has been very good for us. I don't know how it was for you. But I think it's been very good for us because we've heard some strong comments, especially on one of our key, if not the number one feature, in the repository, some comments saying that you're not quite done creating a case that I can believe in. And I think we need to hear that and we need to react to it positively.

And with that, I will of course not take questions because there is no time.

CHRISTENSEN: Really quick.

BULLEN: Bullen, Board. I know I don't want to eat into public comment period, and I apologize. But you mentioned steps forward, and I guess the one thing that--you go back to the IAEA comment or the NEA comment on the previous slide, if you'd do that for me? It talks about sufficient strategy exists to deal at future stages with uncertainty. Does that strategy also include an exit strategy, what if we find out that the dikes are actually going to intersect the mountain and volcanism with a higher probability than we expected, and so we really might have to exit the site? Is this part of the repository safety strategy, that you're going to provide to the Secretary of Energy that there would be an exit strategy?

VAN LUIK: I think, well, maybe it should be said, but I thought it would go without saying that if it looked like the system had a reasonable chance of being unsafe, we would not go forward. I mean, perhaps it should be stated in the strategy. We don't want to go back to the SCP days where we made tables and tables.

BULLEN: Bullen, Board, again. I guess it's just that if you do find some surprise, and I guess the thing that harkens to memory is the Swedish experience where they're taking a look at a phased licensing approach, which is the wrong words to say here, but they've got a we'll put 10 per cent in and we'll see what happens, and then we'll put the rest in, and there is a complete exit strategy associated with that which allows for retrieval, and I know that's an expense and I know that's something that you don't want to deal with associated with here, but it adds credibility to the fact that if you really do find something, that you know, this is not just a big bureaucratic inertia that's going to get this thing in the ground no matter what, so when you look at that strategy, a few words that address an exit strategy might be prudent.

VAN LUIK: It might be prudent. We already have that in the DEIS, and it will be in the FEIS. We have the 50 year retrieval period with performance confirmation testing, which may be extended to 100, 200, 300 years.

The thing that I don't like about the idea of, you know, doing an impartial emplacement of waste and watching it is that we expect nothing to happen. So, to me, this is a subterfuge. You really don't expect to learn anything from that kind of thing. You have to aggravate the conditions.

BULLEN: Bullen, Board, finally and lastly. I didn't think that you were going to learn anything, and I mentioned that in fact that the confirmation testing wasn't going to show anything. I was thinking of something you found as a surprise, like the dike example, which is what's fresh in my memory. And that's the only thing that comes to mind now.

NELSON: Dan, I thought you were going to bring up self-shielding again.

BULLEN: Later.

CHRISTENSEN: Abe, I want to thank you and your colleagues for a really excellent, very clear and high quality set of presentations. I, for one, have learned a great deal and I appreciate also your willingness to meet with us in a much less formal setting in the panel discussion. And with that, I'll turn the meeting back over to Chairman Cohon.

COHON: Thank you, Norm, and thank you for your fine job of chairing the afternoon session.

We have one person signed up for public comment, and then one written question, which I will ask after our commenter. And that's Bob Williams.

WILLIAMS: Thank you, Dr. Cohon.

I'm Bob Williams. I retired from EPRI six years ago. During the first six years of the TRB meetings, I attended essentially every meeting. In the past six years, I've attended only three meetings. It's probably a measure either of my ego or my hubris that I'm bold enough to stand up here and after a five year hiatus, presume to give you advice.

But I spent enough of my life at this that I see--I am concerned that you're headed for some major pitfalls, and I want to bolster the courage of the TRB, I want to bolster the courage of the M&O, I want to bolster the courage of DOE to take some time to restate your safety case. I think that's what it comes down to.

As I've agonized over what to say here today, let me first offer a perspective. I think WIPP is a perfect example of how tenacity will pay off. If you hang in there, after 20 years, you can probably get a license. But now let me hasten to add that they have roughly 5 per cent of the radionuclide inventory that you have, and a much simpler, much easier to license geology. If anybody wants to debate that, I'll buy you a beer in the bar and we can go into that.

Now, the problem I see is I would not have the temerity of Mr. Richardson to stand up and say that the safety margin is adequate in both magnitude and in time, having had Bob Andrews show this chart the previous day. It's adequate if you are talking strictly of the 10,000 year licensing period, and it's adequate in time in the sense that nothing starts to happen until 20,000 years. But if this is the mindset that we go forward with, then I think we will lose all credibility and will play right into the hands of the people in Nevada who are fighting this repository.

So I've agonized and I conclude do I think Yucca Mountain is safe, and the answer is yes, it can be made a safe repository. But I conclude that the analysis that you have done has not made the margins of conservatism at all visible.

Now, the last speaker today tempered my remarks a little bit by showing the--I can't think of the jargon, this analysis--neutralization analysis. This goes partway, and my simplistic advice would be go beat on Mr. Bodvarsson and go beat on the lady who does waste packages, and take back some of the margin that each of the individual analysts has in their pocket.

I still argue that you have let individual investigators keep too much margin, and it's not an unethical thing to do to ask them to make that margin visible so that you can have an expected case that doesn't look like an accident scenario. You shouldn't be bouncing along in the undisturbed scenario showing doses that at the 95 per cent confidence level are up above 1000 millirems.

Now, I won't argue whether the confidence intervals should be 95 per cent or the mean or 80 per cent, but I don't think it can be the mean value and I don't think it can be the median. It's going to have to be a little bit on the conservative side of the mean or the median. And in this game we're playing, that gets rapidly up to the 95 per cent value.

So I think there are some management techniques that have been used in the past and could be used again. Back in the 1990 to '92 time frame, then Program Director John Bartlett put Golder and Associates to work, and he put EPRI to work, and together I think we came up with the framework that is in large part captured in the EPRI model and shows up in all these angel hair diagrams.

So it might be time to get a small team of creative individuals to come in and figure out how working with the existing staff to recast the safety analysis. I reiterate I would not go forward if this is the basis for your analysis. You're going to have to figure out how to take back and make visible Mr. Bodvarsson's conservatism, and some of the waste package conservatisms.

Just as one very quick example, my first meeting at EPRI had Mr. Roger Staehle talking about steam generator tube cracking. And the same issues that he mentioned at that time, he mentioned--his people mentioned earlier this week. You are not going to resolve those stress corrosion cracking issues in all honesty well enough to project to 10,000 years. So the quicker you put in some type of ceramic barrier or some type of barrier in the waste package, the more this analysis will look robust, and it will not--you know, I think I heard one board member characterize this as, well, what do we have, a waste package in a mountain. And I have to say sitting in the audience, that the impact of these presentations does come across that way.

So I believe there are a lot of things that can be done. One of them might be a subterfuge, but I think it's a legal subterfuge. I think you need to move the engineered barrier system five or ten meters into the geology. Just as one example, we talk about the drip shield. If we were to put multi-levels of tunnels in there and put capillary barriers in the tunnel, arguably at least, this would be as foolproof a way of building a drip proof repository as your titanium drip shields.

Now, if I had the answer to this all sketched out, I would volunteer it to you. These are just brainstorming suggestions. But I think some brainstorming has to be done to illustrate the areas in which you have conservatism in the Yucca Mountain site. You have conservatism both in its ability to drain, in the ability to go in and, you know, the buzz word would be a drip proof repository.

You know, Larry Rickertson, Abe Van Luik, come back in two months and show me as the reference case, the drip proof repository. It might have no release for 50,000 years and be a credible base case.

Now, one of the early studies I did at EPRI was to show how thermal expansion blocks off the fractures. You know, if you took into account the thermal pulse, its clamping off of the matrix, the apertures in the fractured matrix, these and other factors could go aways toward giving you that extra one or two orders of magnitude that I think would be a credible case.

Let me reiterate, and I'll sit down, I think you will just play into the hands of our critics and you'll probably bring down the program if the reference licensing case, the nominal scenario case, has out-year results that are up above 500 millirem, more like 1000 or 2000 millirem.

So I appreciate your taking a few minutes to hear these comments. They're offered strictly to be constructive. I think that you can perfect the explanation of this analysis, but I think it's going to take, my experience, probably another year. It's going to require a major effort to recast your analysis and make visible the conservatisms that now are buried in this complex model.

Thank you.

COHON: Thank you very much, Bob. It's a pleasure to see you back here at our meeting.

We have a question, written question from the audience that was intended for Kathy Gaither. I'm not sure she's still here. But in any event, I think Abe was going to answer it anyhow. Let me read it into the record, and then Abe will answer it.

"Among the 13 FEPs on Slide 4 of Kathy Gaither's presentation, you state, 'Hydrologic response to seismicity/faulting; exclude low significance.' Assuming the University of Nevada Committee investigation headed by Jean Cline shows a deep seated hydrothermal origin for the calcite silica deposits in the ESF, how will this affect the disruptive events PMR for seismicity and faulting? Giving the foregoing assuming, assume further that some of the ages of the deposits are less than 1 million years old."

You're on, Abe. Do you need this to refer to? Or you've got it. Got it?

VAN LUIK: Some of the speculative answers that the question is looking for I can't give you just right off the cuff. It's true that water fluctuates. Water levels in the water tables fluctuate when there's an earthquake. This has been measured. It's even been measured at Yucca Mountain.

The typical water table rises are centimeters to a few meters. They are transient rises. They don't last very long. Water tables after these events return to previous levels, or very close to them.

Now, since in our modeling, a climate change induces a change closer to 100 meters, changes that last a long time, the possibility of a temporary rise in the water table of a few meters would have no effect. Therefore, it was screened out in the FEP screening process. There would be no significant consequence from this particular effect within the bounds that we have felt were reasonable.

The idea that seismic activity could propel water into and flood the repository has been reviewed by a committee of the National Academy of Sciences, and of course it's been reviewed by our own scientists. It is considered incredible, meaning it has such an extremely low probability that that probability is close to zero. And so it is screened out on the basis of lacking credibility scientifically.

The work being done by Jean Cline at UNLV with her collaborators is independent. They are looking at two phased fluid inclusions in Yucca Mountain. That work is not yet completed. Inclusions found thus far are associated with the older fracture fillings, meaning they the fillings closest to the rock. Work continues, but the warning has already been sounded that the results may never be definitive.

Unless proven otherwise, the scenario of a hydrothermal event pushing water into the repository is screened out. It may be that the fluid inclusions seen to date were created during the cooling phases that are extremely old, with the higher tuff layers being overlayed over deeper ones. But that is just a hypothesis at this point.

We have looked at the secondary effects of volcanism, introducing aggressive hot fluids. We evaluated that in the TSPA/VA, and saw that it has a very minor effect on a limited number of waste packages in terms of their lifetime, compared to the direct effects of a magmatic intrusion or eruption.

So that is my answer to this question. As to speculating what if what we feel is incredible turns out to be credible, we will face that if that actually is the outcome of that research.

COHON: Thank you, Abe.

Jerry Szymanski is here and he asked to comment on this issue as well. Jerry, state your name again just for the record. Thanks.

SZYMANSKI: Jerry Szymanski. I wasn't intending to speak. But I heard this, and it is incredible to me. Number one, we are not speaking of the effect of vibratory ground motion. The transitory effect, which we know what it is, it's small, what we are concerned is a--induced changes to the system, which contains a hydrothermal system. In other words upsetting the balance of the rating numbers.

It is so misleading what I have heard, that I just couldn't resist.

There's another issue. Where is this inclusion occur? We do know that three years ago, they were not there at all. A year ago, they occurred at the base. But we do know now, and anyone probably knows better than I do, they occur at the base, in the middle, and in the top. Where do you stop it? We already know that the oldest dated mineral which contains this inclusion is about 9 million years old. The young one, about 20,000, and everything in between.

How then can we, with a straight face, state what I just have heard? The main point here is that indeed, the nation is facing a decision like never before. We'll go to the president and we'll ask him to sign this thing. There was a very appropriate question, how much confidence do we have to have? But if we derive this confidence from misleading and erroneous information, how good is it?

Thank you.

COHON: Thank you, Jerry. Are there any other comments from the public?

(No response.)

COHON: Seeing none, let me close the meeting with a few very brief comments. I subscribe entirely to what Abe said in his summary of the last day and a half. I think it was as good for the Board as it was for DOE and its contractors. There was a tremendous amount of information. It showed a degree of integration and connection that I don't think we've ever seen before at our meetings.

Many of the results that we saw were very recent, very fresh, and we know that, and we recognize that it takes a certain amount of bravery on the part of DOE and trust and respect for the Board for you to do that, and we thank you for your willingness to present those results, and to expose yourselves, open yourselves up to the kind of panel discussion and free-for-all that we had.

I think everybody affiliated with the program included themselves very well, Abe, and you should be proud of them. And on behalf of the Board, thank you very much for all that you did and all that your colleagues did over the last two days.

In closing, I want to thank my colleagues for their support in this excellent meeting. Linda Hiatt and Linda Coultry for their wonderful organizational and logistic support. Leon Reiter who basically was the brains behind this entire thing, and miraculously pulled this off in terms of getting as much and as many people into the program over such a short period of time. Thank you, Leon.

And, finally, to the only person who actually knows everything that everybody said, Scott Ford. He's with us once again and we're delighted to have him here.

With that, we stand adjourned. Thank you very much.

(Whereupon, at 5:30 p.m., the meeting was adjourned.)