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NUCLEAR REGULATORY COMMITTEE

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ACNW AUDIT REVIEW OF CHEMISTRY ISSUES

FOR THE YUCCA MOUNTAIN SITE RECOMMENDATION

CONSIDERATIONS REPORT

(ACNW)

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THURSDAY

FEBRUARY 22, 2001

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ROCKVILLE, MARYLAND

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The ACNW Audit Review Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B1, 11545 Rockville Pike, at 8:30 a.m., Dr. Martin Steindler, Acting Chairman, presiding.

COMMITTEE MEMBERS:

DR. MARTIN STEINDLER, Acting Chairman

DR. JAMES CLARKE, Member

DR. PAUL SHEWMON, Member

ACRS STAFF PRESENT:

DR. ANDREW C. CAMPBELL

DR. TAE AHN

DR. JOHN BRADBURY

DR. RICHARD CODELL

DR. GUSTAVO CRAGNOLINO, CNWRA

DR. BILL DAM

DR. CARL DIBELLA, NWTRB

DR. DAVID ESCHE

DR. BRET LESLIE

DR. TIM MCCARTIN

A-G-E-N-D-A

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

(8:30 a.m.)

DR. CAMPBELL: All right. The situation is that Dr. Steindler here is going to be the Chair for this morning's session, such as it is. All of the group has gotten early flights out to avoid getting caught in the storm that is eminent.

And Ray actually had to catch a flight very early. So he had already had to go. Otherwise, he was going to get stuck here. So, Dr. Steindler will chair the meeting, and what we are going to do is we are going to have to adjourn before 10 o'clock, is that right, Gentlemen?

ACTING CHAIRMAN STEINDLER: Yes, I believe so.

DR. CAMPBELL: And we are going to go ahead and have Dave Esche from the NRC staff discuss some of the aspects of the TPA code. We will be adjourning about 10 o'clock this morning because of the situation with the snow coming and people having to catch flights. So with that, Marty, the floor is yours.

ACTING CHAIRMAN STEINDLER: Thank you very much. Unaccustomed as I am to public speaking, if I can find my agenda, I would be in good shape. There

will be no introductory remarks by Ray Wymer.

And there will be a little bit of a discussion as to what we learned yesterday, or what transpired yesterday. The focus of the meeting was or is chemistry, and specifically chemistry as it relates to the waste package, and its role as a source term for using and transport.

We did hear some of the issues, but certainly not all of them, dealing with the chemical background built into the models. We heard considerable discussion on corrosion and were told in no uncertain terms that the excessive use of conservative assumptions could easily lead to a nonsense output.

However, we do I think have to change the role of our normal protocols, and this is not a scientific discussion. This is a practical analysis of what needs to be done to satisfy the Commission through the staff, and provide them with reasonable assurance that the models that DOE is using that are checked by the staff are appropriate for in our case ultimately the license application.

I think that is the focus, and our focus is to determine whether or not the staff, the NRC staff, is able to satisfy that role and specifically

how are they doing it.

We heard a considerable amount of information and handed a considerable amount of information on the methodology, the process that the staff is using to resolve what they call issues, which are really questions to the Department of Energy on the source, the nature, and the implications of some of the assumptions and activities in the models and their abstractions.

It isn't very clear at this point whether the staff believes that they are in satisfactory condition, considering that they don't have a whole lot more time between now and the time that, one, somebody is going to ask them about the TSPA site recommendation.

And not a whole lot of time when the TSPA licensing application is going to come bouncing on somebody's desk. With that, I would suggest that one of the important issues that we are going to try and at least address today is whether or not the staff can produce estimates on an independent basis of the behavior of the Yucca Mountain system as it is described in gross terms by the Department, but as it is described in detail models by the staff themselves.

The issue will be, one, is that doable,

and has that been done, and two, and perhaps most important, how independent is that exercise done by the staff. It is at the moment to us here, but obviously not to other people, somewhat immaterial what the answers are, unless the answers get to be drastically different than what the corresponding Department of Energy results are.

And then somebody needs to at least look at the reasons for that difference, and try to unravel those. With that, my suggestion is let's hear what we can about the staff's efforts to model abstract and calculate. Can we do that?

DR. ESCHE: Yes.

ACTING CHAIRMAN STEINDLER: All right. State your name, rank, and serial number, and sit down.

DR. MCCARTIN: Can I just add one thought? You talk about the differences, and I think whatever calculation the staff does, we will have to know why we got our numbers, and why DOE got their numbers, and whatever comparisons are appropriate.

And I will give you the best example I have. To date, generally our calculations show similar doses and similar release rates. DOE has a much higher release rate, but takes a very large

credit for cladding.

We take no credit for cladding and have a lower release rate. We get a similar answer, but for drastically different assumptions, and I think that is the healthy part of the process, is that we will understand not only our own results, but DOE's.

And whatever assumptions affects ours, versus theirs, I think -- and whether we have to walk through them all in the licensing area is another issue, but I think we have to understand all those. And we get the same results, but like I said, for very different reasons.

ACTING CHAIRMAN STEINDLER: That's quite right. It isn't just focused on the number at the end.

DR. ESCHE: All right. I am Dave Esche, and I am in the Performance Assessment Section, and I am in charge of the TPA5.0 code development. I was in charge of the TPA4.0 code development, because Tim was responsible for that in the past, but he got overwhelmed with rule makings. So the activity was shifted to me.

One of the things that we are looking at for TPA5.0, and working pretty hard on developing, is a revision to the gas and seepage models. And I think

this picture up on the screen is a pretty good representation of the methodology to use to try to evaluate that chemistry.

It is the connections and flow paths that the DOE is using, and I think it is a pretty good picture of how things will be moving, and in what locations you may need to evaluate the conditions.

And our focus right now is on location one and location two for the time being. What is the gas and seepage chemistry coming in, and then what happens to that once it gets on these engineered systems.

So we have a team made up of Gustavo, and Tae Ahn, and the CLST folks, the corrosion people, working with the near-field environment people, and the TEF, thermal effects on flow people, and all of those are put together to try and evaluate this problem.

The hard thing is that you have uncertainties coming in each area, and so what does that mean. What we are trying to define is what is the window of environmental conditions that we think you may have there.

The answer may be that the engineered system is still robust after you have defined that window, but for right now that window is pretty ill-

defined, and so we are working to better define what that window is.

And at location two, we have a group at the Center that is using a code called OLE, I believe, which is -- well, it is to predict the concentrations of salts under very extreme conditions.

So EQ36 breaks down at a certain ionic strength, and it is no longer applicable, but this OLE code, I guess, is used or has been used at Hanford to evaluate what is the chemistry of these extreme conditions, and that's what we are trying to use at location two to evaluate what happens to the chemistry.

So our idea is that we are going to propagate the differences in the chemistry and water coming in from location one, and if we have high temperatures, and a hot repository, then that water coming in boils and leaves something there.

So we try to evaluate what is there, and then based on what is there, then that determines what happens to the chemistry from that point. There is some key uncertainties, like what happens when you go from completely dry to you first start wetting.

What is the chemistry that that surface is seeing, and is it aggressive to those metallic

materials. Well, it is hopeful that the corrosion people can get data.

Once we define what the window is, then maybe they will be able to do some tests and evaluate it to see if there are any corrosion problems in that window o susceptibility.

ACTING CHAIRMAN STEINDLER: When you say wetting, you mean gross wetting?

DR. ESCHE: Well, we have both. Like we have considered that some locations may have seepage, but other locations may just have an increase in relative humidity.

So the relative humidity may be low where it is effectively dry, but then the relative humidity increases, and depending on the compositions of the salts that are there on the surface, that will define when you start having moisture present on that surface, and at what temperature effectively.

But the key problem is propagating the uncertainties, because you have so many permutations that you can only do a subset of those permutations, and how do you choose what the right subset is.

So we are doing the best we can in trying to calculate the edges of that window, but we are not going to be able to do every point within that window

of different chemistries.

But in performance assessment -- and I have heard you guys talk about this, but we always want our most realistic number, and then our range of uncertainty. We don't like the conservative bounding effects, because when you propagate it through 10, or 15, or 20 perimeter distributions, you start ending up with a ridiculous result, which has been said.

So we try our best at, okay, this is what we expect to happen, and this is our uncertainty, rather than setting things towards the ends of distributions.

Lauren Browning would be good to talk about this, but she is on an audit out at Las Vegas. She is the one who is coordinating the work down at the Center.

And it is likely that for TPA5.0 that we will have at least some revision to the chemistry model, but we might not get the whole way to where we want to be, because it is resource consuming, and it requires cooperation between five different disciplines basically. It is a difficult problem, a really difficult problem.

But I think this picture that is put on the board is a pretty good framework. You run into

all sorts of complexities though, like what happens when you go from dry to wet, and you don't really have data.

What happens if the salt on the surface has some porosity to it that causes vapor pressure lowering, or if you form an aggressive condition on the top of the drip shield, and the drip shield fails, what is the mass transfer of that material then to the waste package surface.

And if you don't have dripping is there any mass transfer, and if you have intermittent dripping, and so we are going to try and model it with basically equilibrium calculations at each step or at each point in time.

And maybe that is a bad assumption, but considering that we probably won't have much data about the kinetics of some of these mineral phased dissolutions, et cetera, that is probably the best that we can do to get a rough answer at what is happening.

DR. SHEWMON: You sort of dropped your voice as if you are finished. Are you going to get to location three, or is that somebody else?

DR. ESCHE: Well, as to location three, what we imagine is that when we have an intact drip

shield, we will evaluate the chemistry at location two.

And then if we open a gap in the drip shield due to either general corrosion, et cetera, we will propagate the environment at location two with some mast transfer mechanism to location three. When the drip shield is intact, we don't imagine that we will have progressive chemistries at location three.

DOE has collected information on dust, and it is going to evaluate what the chemical composition of that dust is. So maybe the dust interacting with relative humidity gives you some sort of environment at location three.

But it certainly is not going to be as aggressive as the test conditions that we have seen so far. So maybe it is of minor concern. And maybe that is --

ACTING CHAIRMAN STEINDLER: It looks like your temperature is going to be higher.

DR. ESCHE: Yes, your temperature will be higher. So at location three, it is conditional on what happens to that barrier above it. We won't have an aggressive chemistry at location three until we have a hole at location two.

Or if our mechanical folks said that you

can form gaps in the drip shield due to seismicity, then that would provide the opportunity for the environment, either the seepage water from location one going directly to location three, or seepage water that lands on location two.

DR. SHEWMON: And currently the seismicity people have decided that the seismic won't break up the drip shield. Isn't that what I read someplace?

DR. ESCHE: Yes, DOE has calculated both that it won't fail the drip shield and that it won't open gaps in the drip shield, because they designed the drip shield with, I think, a lip on each side that kind of locks over the top of it.

So the seismic forces just aren't large enough to pop those off and make a space. But I don't know what we have done what the NRC feels or the Center feels about those calculations, but that is DOE's results.

ACTING CHAIRMAN STEINDLER: Okay. Other than the attention to conservative estimates, how does this differ from the TSPA?

DR. ESCHE: How does it differ from DOE's TSPA?

ACTING CHAIRMAN STEINDLER: Yes, DOE's TSPA.

DR. ESCHE: I think they aren't using -- they aren't evaluating -- they are modeling what happens to the chemical conditions at location two when you go from completely dry to start wetting.

So we are trying to better get a handle on what is happening in that time regime, and what is happening to the chemical condition at location two when you go from dry to wet.

If you look at the ionic strengths that they have in their model, the ionic strengths that happened at location two are much lower than what happens when you have a salt or precipitate layer on the top of the drip shield, and you start adding a little bit of water.

So the ionic strengths are much lower than that. I don't know exactly what the assumptions were in that regime, but I think we are evaluating location two differently, and with a different code, and with a different -- well, maybe the same geometric framework, but maybe a little bit better consideration of the time scale to the processes.

And whether that is -- and I don't know about the process model, but in DOE's TSPA they used 500 year time steps. Well, if you use the 500 year time step and you were trying to look at chemistry

that is happening at location two, you skip right over all those processes that happen when you go from dry to wet.

How important are those? Well, maybe they are not important at all, and maybe some simple testing could identify that, and I think that it is testing that DOE is doing.

They have samples that are halfway submerged, and sickled wet-dry, and we can model it to death, but the data is really the good way to put to rest some of these things.

ACTING CHAIRMAN STEINDLER: Why did you think that was an important thing to do?

DR. ESCHE: To look at the wet-dry conditions?

ACTING CHAIRMAN STEINDLER: Yes.

DR. ESCHE: Just because the -- well, whether you have, say, a localized corrosion phenomena, for our model, it is dependent on the temperature and the ionic strength. You need high temperatures and high ionic strength.

So what is happening when you go from dry to wet is that you should have the maximum -- at that point in time, it should be the maximum of both ionic strength and temperature.

So if you have certain deliquescent mineral phases on that surface, they will start taking water from the atmosphere before, say, if you had sodium chloride there.

So depending on the mineral phases that you form will define the temperature and ionic strength that you have. And it might be a short period of time, and maybe it is not important, but if the corrosion rates are fast, that period of time may be important to try to characterize, and so that is what we are working towards.

ACTING CHAIRMAN STEINDLER: It still isn't clear to me whether or not in the grand scheme of things if it is worth the effort if it looks like what is going to take to get a better handle on what is going on.

DR. ESCHE: I think in the grand scheme of things that it may not change the result at all, but at least that we asked and answered the question would provide confidence that the -- well, right now a lot of people have uncertainty, yourselves or the NWTRB, that you have this package that lasts 50,000 years or a hundred-thousand years, a million years.

And actually at a hundred-thousand years, only .2 percent of the surface area has failed. So

you have a few hole, but generally the thing is pretty intact.

ACTING CHAIRMAN STEINDLER: The surface area of what?

DR. ESCHE: The surface area of the waste package. Only .2 percent has actually failed at a hundred-thousand years.

ACTING CHAIRMAN STEINDLER: So that is what I am getting at. If in fact that is true, I guess I can argue that the amount of resources that you are going to spend, and the folks that you are going to try and pull into this team that you have got, may not be worth the effort.

DR. ESCHE: Well, certainly --

ACTING CHAIRMAN STEINDLER: In other words, how did you pick it?

DR. ESCHE: How did we pick what?

ACTING CHAIRMAN STEINDLER: Why did you pick that?

DR. ESCHE: Because the waste package is the most risk significant thing in the whole system. If the waste package isn't working, then that is the only way you start getting close to doses that would violate the standard in the regulatory time period.

ACTING CHAIRMAN STEINDLER: So you must

have had some assumption that the approach that DOE is using is unsatisfactory or uncertain.

DR. ESCHE: Well, it is uncertain, yes. Unsatisfactory, I would say no. They have made leaps and bounds in that area, and they continue to work further. But in a lot of these areas, we find -- we don't know the right questions to ask until we look at the problem ourselves.

And when we look at the problem ourselves, then we say, oh, we should have been asking about this. So that's why we do a lot of the independent analysis that we do, because when you are reading a document and you look at it, and you go through it, and you may find questions.

But when you try to solve the problem yourself, you identify a lot of different things than when you identify it by just looking at a document.

So that's what I see the utility of it is, and the engineered barrier system is the most risk significant system. And even if you take out the engineered barrier system though, the natural system still does the time.

You can see just by some of the neutralization analyses that DOE Did that a lot of these things are working in combination to take that

hazard down. So it is not just the engineered system, but in the regulatory time period, or in the 10,000 year time period, the engineered system is buying you compliance.

DR. MCCARTIN: If I could add one point. I think the regulatory question that we have been pushing DOE on -- and I will say maybe about a year-and-a-half ago that we raised the question, and they said we will have no waste package failures in 10,000 years.

They have made that statement, and we can defend it, which is fine. The question we asked is have you considered an appropriate range of conditions that would affect the waste package in a deleterious way, and they said we believe we have.

And that is where the staff is now, okay, are we certain, and we are just pushing to make certain they consider an appropriate range of conditions for that 10,000 life time.

And then not too long afterwards, you guys are aware of the State coming in with the trace metals. And I think it is just the ZLST people at the center and NRC, and the near-field people, let's get together and make sure how we found the conditions that we think would capture an appropriate range for

defending that 10,000 year lifetime.

ACTING CHAIRMAN STEINDLER: Okay.

DR. CAMPBELL: Just for the record, that was Tim McCartin from the NRC staff, and Dave Esche is the speaker at the table of the NRC staff.

Given, Dave, what you said about the 0.2 percent failure at a hundred-thousand years, and the doses associated with that, I think that emphasizes the importance of establishing with some high degree of certainty that in terms of reasonable assurance that the 10,000 plus lifetime for the waste packages is defendable in a hearing type of process.

And not only on every technical aspect of it, because 0.2 percent is a pretty small segment of -- I mean, there are a lot of waste packages in there, and that is still a lot of surface area. But when you start thinking about failure rates of normal stuff that people have in their every day lives, you are looking at 5 percent being an acceptable failure rate.

ACTING CHAIRMAN STEINDLER: Are you talking about washing machines?

DR. CAMPBELL: Right.

(Laughter.)

DR. CAMPBELL: But you see what I am saying, is that is what most people are dealing with

in their normal lives; is that 5 percent of the products that I get are a lemon, and they are worried about that.

And then you say, okay, now 2/10s of a percent at a hundred-thousand years, and we are getting doses that far exceed the limit, even though we are way out in time, a lot of people are gong to say, hey, it doesn't take much failure earlier on to get you into trouble in terms of compliance.

DR. ESCHE: That was -- the .2 percent is surface area failed, and the actual percent failed, the ones that get very small holes, or cracks, I think it is like 50 percent have cracks in them, and 26 percent have patches. But it is only .2 percent of the area.

DR. CAMPBELL: Right.

DR. ESCHE: It is kind of number smithing or something, but in general the amount of surface area failed is very small at a hundred-thousand years.

DR. SHEWMON: Failure here is significant corrosion or penetration?

DR. ESCHE: Well, the larger numbers are penetration.

DR. SHEWMON: No, the .2 you said was --

DR. ESCHE: The .2 is a 6-inch-by-6-inch

patch, I think, is roughly the size if a hole failed that big.

DR. SHEWMON: So that is penetration.

DR. ESCHE: So it is an opening, yes.

DR. SHEWMON: Thank you.

DR. CAMPBELL: But isn't that an assumption of the size of that? It assumes --

DR. ESCHE: It is just the size selected to model the problem.

DR. CAMPBELL: Based on general corrosion rates?

DR. ESCHE: Yes.

DR. MCCARTIN: But DOE gets releases from cracks, no matter how small, or early, or how much water.

DR. ESCHE: And the other thing you have to remember is that if they have a patch that fails and let's say a 6-inch-by-6-inch patch, they have diffusive releases over that whole area. So that whole 36 square inches or whatever.

In reality, you are going to have a water film around the outside of that hole, which is the diffusive area. So they might be greatly overestimating diffusive releases which are making those numbers a hundred-thousand years much larger

than they may be in reality.

So some of those conservatisms you have to keep in mind when you are looking at what the curves are telling you at the later times, and I think they are working on evaluating that, and you may see a revision to that in the future.

DR. CODELL: Dick Codell, NRC. And in fact almost across the board the DOE puts this diffusion model in, and it seems wherever they have that that they have exaggerated it.

For example, they assume that the waste package is sitting directly on the inverts, and so there would be a direct pathway, and without taking any credit for the fact that it is sitting on a metal stand right above the invert.

And that like the example that I gave yesterday where the waste package just failed and that it would lead to the maximum diffusion. I mean, it is not our job to tell them to be less conservative, but it just struck us all that way. That the diffusion models in their TSPA are exaggerated.

DR. ESCHE: I guess the only thing we were worried about was the conservatism, as if it is masking what we should be worrying about. So like maybe the amount of advective flow is really what we

should be concerned about, and the uncertainties associated with that.

But if we have a conservative diffusive model clouding that, then we might not be focusing as much attention on what are the uncertainties in the seepage model, you know.

So that's where we -- at least in PA space, we try to communicate what we think and that you may need to look at your conservatism here.

I mean, when uncertainties are large, in some instances you have to be conservative. But you have to be careful in risk assessment that you are not doing that all over the place, and that you aren't generating some goofy number, I guess.

DR. CAMPBELL: Well, we did talk about this issue of what appeared to be -- and I will use the phrase "ultraconservatism" in the way that they are implementing diffusion in their models.

And not only setting a boundary condition at zero concentration, and never altering that, even though obviously if a species is diffusing along that gradient, then the gradient is going to be attenuated with time.

DR. ESCHE: Sure.

DR. CAMPBELL: Otherwise, things would

diffuse away from everything, and we would all be in some sort of equilibrium state that doesn't exist in the real world.

But that even raises even more questions about the value of these neutralization analyses, because the way that they implement neutralization of the waste package is to impose a 300 centimeter square patch open.

And if you are saying that then have 300 square centimeters of diffusion in an area, and you multiple that by the 8,000 or so waste packages in there, you can see where these high doses come out on the calculation.

But it has no relationship to a real world type of situation of failure, and that creates a lot of issues, I think. That maybe it isn't the place of the NRC to comment about the conservatism built into that, but it sure can mask a lot of other -- you know, as you say, potentially important issues, because that just dominates all the release.

DR. MCCARTIN: And you don't need water. You assume there are monolayers of water all the time and that's the fusional --

DR. CAMPBELL: Right. Well, you need water, but all you need is a few monolayers of it.

DR. MCCARTIN: Well, they make no calculation with respect to water, in terms of how much is there, et cetera, especially for the neutralization that it has failed at T zero. That is pretty hot.

And in terms of perimeters, for the diffusion coefficient for source term in the waste package is 10 orders of magnitude larger than the diffusion coefficient they use in the unsaturated zone; 10 orders of magnitude larger.

DR. CAMPBELL: Are they using pure water diffusion?

DR. MCCARTIN: Oh, yes, for the source. But in the unsaturated zone and the matrix diffusion, they have a number that is 10 orders of magnitude less. And that's fine, as it certainly can't be any higher.

DR. CAMPBELL: No, it's not fine.

DR. MCCARTIN: Well, it is their calculation, and they need to put forward what they think they can defend and support. And as long as we know what they are doing, we can evaluate. And it is not our calculation.

DR. ESCHE: Usually when you are doing something conservative, there was a reason for it.

But in some instances maybe it is a little too far, you know.

DR. CAMPBELL: Well, I guess what disturbs me is that diffusional ungradience is not a new phenomena, and it is 19th Century science. This is not a new phenomena, and for DOE to take the position that this stuff is too uncertain to deal with in a more realistic fashion I find incredible.

DR. MCCARTIN: Sure. And the only thing -- and Dick may remember this better than I -- and Tae -- but there are some experiments where DOE has some information that the experiments were flawed, but I thought they suspended the spent fuel particle, and they got some concentration above a pool of water, and they got some concentrations in the water.

And you back out some numbers, and we have measured this, and we can't say that something isn't going on here. And this is about 4 or 5 years ago.

DR. CODELL: Those were the experiments at Oregon on spent fuel.

DR. MCCARTIN: And Dave is absolutely right. We look at this and we want to understand this assumption of what it does to the calculation, because it can cloud some of your results.

But DOE has an experiment or two out there

that tends to support some larger releases, and it could be their concern with that experimental evidence that we will not be able to refute it.

DR. CAMPBELL: Is this documented in an AMR somewhere?

DR. AHN: Well, originally in the early '80s, Northwest Laboratory, supported by DOE, spent a long time on spent fuel testing. In other words, in emerged air or simulated ground water, and periodically measured the dissolution rate over a long period of time, from 5 to 7 years.

Then all of a sudden a new testing was initiated in Livermore, and they attempted to study a fundamental using only a single carbonated solution, excluding all complicated species. The test method was different and it was a so-called flaw testing to determine the dissolution rate.

That rate was very, very high compared with the emerging test results, which came from real well water conditions. We hoped that they could incorporate that in other species later on, but they never did it.

They kept going on with the single carbonate solution, because they wanted to understand the very basic mechanism. Later on we questioned them

on why do you need to do that, and they said, well, this testing is conservative. That was the only reason.

ACTING CHAIRMAN STEINDLER: Okay. Gustavo.

DR. CRAGNOLINO: Well, I know that it is important about what Dave Esche mentioned and understanding the chemistry and the drip shield, and the waste package and the corrosion, and that the drip shield was conceived by the DOE as a way to control the flow of water, and this was a very strong statement in number two.

And from the point of the engineered barrier system, the two principal factors were the performance of the waste package and the performance of the drip shield. However, if you look in the recent recital, and condition three, it is not the drip shield alone.

Now it is the drip shield drift invert system. Why? Because apparently the drip shield has not played the significant role that was expected by using the dose after 60,000 years. And I think this is an important modification, and is something that we have to explore in more detail.

It changed the emphasis, and the emphasis

is now more in the relief and leaving aside performance of the waste package. This is where we really have to refine our approach to the knowledge in the same way that David has mentioned.

DR. ESCHE: Well, the way I look at the drip shield is that it may not show up in the current analyses as being extremely important because of the large diffusional releases, and it is mainly preventing water flow.

But it also minimizes rock fall and aggressive chemistries if you have it. So like say the titanium is only susceptible to fluoride, for instance.

Well, the time that the drip shield lasts, which is on the order of 10,000 years in our model, and 20 to 40,000 years in DOE's model, I think, you would expect that most of that aggressive chemistry has been rinsed out of the system so to speak by the time of the drip shield failure.

ACTING CHAIRMAN STEINDLER: Why would you expect that?

DR. ESCHE: Well, in DOE's model, what happens is when you have seepage, it is just as a mixing cell. So the seepage comes in and mixes with the salt and carries some of it out, and that is

enough to dilute the chemistry back to ambient pretty quickly.

ACTING CHAIRMAN STEINDLER: But ambient fluoride, which is the issue.

DR. ESCHE: Well, the ambient fluoride, I think we heard, was 4 milligrams per liter. And from what our people told me, the corrosion of the drip shield requires a threshold fluoride concentration, but the fluoride is consumed in the reaction.

So you need to get a higher fluoride concentration, but then you need a certain mass flow of fluoride into the system for the corrosion, because it is consumed in the reaction.

But if the drip shield is preventing all these chloride and other salts that are formed there initially, those salts are probably rinsed out either due to seepage water in, or relative humidity becoming high again.

And by the time the drip shield fails the aggressive chemistry is prevented on the waste package. So we forget that whenever we are doing the analysis, too, because right now the data suggests that even if you have those aggressive chemistries, the waste package only in DOE's model has general corrosion and stress corrosion cracking.

Localized corrosion never happens for any of the conditions that are generated, and so that is the issue. If you have a different window and those conditions were generated, would you have localized corrosion, and that's what we are trying to find out.

And it would build credence to, oh, yeah, the drip shield doesn't show up in your model as doing much now, but there is maybe a good reason for why it is there.

ACTING CHAIRMAN STEINDLER: Okay. Any other comments or questions?

DR. MCCARTIN: One quick correction. It is not a 10 order of magnitude. It is only a 2 order of magnitude. I did the unit conversion in my head wrong.

ACTING CHAIRMAN STEINDLER: So two orders of magnitude.

DR. MCCARTIN: Two orders of magnitude.

ACTING CHAIRMAN STEINDLER: That's still not bad.

DR. ESCHE: And my comments about what DOE is doing is my understanding based on reading the SR. So I may not be accurate in everything that I said, but I hope that I am.

ACTING CHAIRMAN STEINDLER: All right.

Well, this chair is going to turn into a pumpkin not too long from now because you folks have such unfriendly weather.

DR. CAMPBELL: As opposed to the friendly weather in Chicago.

(Laughter.)

ACTING CHAIRMAN STEINDLER: Well, we don't shut the place down.

DR. CAMPBELL: John.

DR. BRADBURY: John Bradbury. We have been talking about the ultraconservative and the problems with that approach. I would like to pose a possibility that there is a situation where DOE has been nonconservative, and that relates to what was raised yesterday concerning the use of hydrochemistry in the saturated zone to delineate flow paths.

Again, to remind you that the line from Yucca Mountain down to the critical group is at constant chloride chemistry, chloride being a non-conservative tracer similar to technetium and iodine.

Essentially this is saying that there is no dilution along the saturated zone flow path. Also, one should consider the evidence that perched water in the UZ is similar to perched water in the saturated zone.

So one could also think that there is no dilution on the whole flow path for these conservative species or elements. Now, why I am saying this evidence hasn't been used that way is because one of the key attributes is delay and dilution of radionuclide concentrations provided by natural barriers.

DOE's modeling assumes or part of the model involves movement through the UZ and then there is a separate leg along the SZ, and there is a technique, a convolution technique, to essentially allow source material from the UZ to be added to the SZ.

So it is like throwing sticks on to flowing streams. Now, the flowing streams flow at different rates. So the faster flowing stream, if you throw it in at the same rate, dilutes the radionuclide. And that is what DOE models.

That is not what this type of chemical evidence would suggest. So here is evidence that may suggest that DOE's model is non-conservative.

DR. CLARKE: If I understand you, John, you are saying that you can't have it both ways. Do you have an opinion which way it should be?

DR. BRADBURY: What I want to make sure is

that all evidence is used to the point where you have got alternative models being considered.

DR. CLARKE: Well, I guess my question may be better phrased by saying do you have a problem with the flow paths as they have been delineated?

DR. BRADBURY: Well, the flow paths, the hydrologists say that those flow paths -- and hydrochemistry aside, those flow paths are reasonable.

I take that evidence and say that seems reasonable, and then when you couple that with the hydrochemical evidence, you come up with a situation where you say, well, it looks like hydrochemical evidence, which is evidence that is not just short term, but evidence that has been developed over thousands of years, it is really kind of very good evidence to say, yes, no dilution occurs.

And therefore I am seeing this discrepancy here with regard to the evidence.

ACTING CHAIRMAN STEINDLER: Okay. Any additional comments? If not, then my intention at the moment is to suggest we all go home. What else do we need to do here?

DR. CAMPBELL: I think the path forward for this group is that all of us agreed in our discussions yesterday among ourselves that we are

going to look harder at the IRSR and do a cross-walk with some of the individual issues that we have identified in our own look-see at the various DOE documents and models of TSPA.

And that basically the group will be writing a report to the main committee for its consideration as a possible letter to the Commission. So that is basically the path forward for us.

This meeting is not the end of our work, but rather a halfway point, in terms of getting a better handle on some of the issues and interacting with people here. But we have some more work ahead of us.

DR. MCCARTIN: I was going to offer that Dick Codell will provide you with the paper that talks about this experimental evidence that I think at least at one time DOE was looking at and suggesting a high release rate.

ACTING CHAIRMAN STEINDLER: All right. Well, thank you very much for all of your work, and I hope you all get home for those of you who have someplace to go. And we will call the meeting adjourned.

(Whereupon, the meeting was concluded at 9:27 a.m.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: ACNW Audit Review

Docket Number: (not applicable)

Location: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Paul Intravia
Official Reporter
Neal R. Gross & Co., Inc.