

NUCLEAR REGULATORY COMMISSION

ORIGINAL

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the Yucca Mountain Site Recommendation
Considerations Report

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1 UNITED STATES OF AMERICA
2 NUCLEAR REGULATORY COMMITTEE

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4 ACNW AUDIT REVIEW OF CHEMISTRY ISSUES
5 FOR THE YUCCA MOUNTAIN SITE RECOMMENDATION
6 CONSIDERATIONS REPORT

7 (ACNW)

8 + + + + +

9 THURSDAY

10 FEBRUARY 22, 2001

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12 ROCKVILLE, MARYLAND

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

18 COMMITTEE MEMBERS:

19 DR. MARTIN STEINDLER, Acting Chairman

20 DR. JAMES CLARKE, Member

21 DR. PAUL SHEWMON, Member

1 ACRS STAFF PRESENT:

2 DR. ANDREW C. CAMPBELL

3 DR. TAE AHN

4 DR. JOHN BRADBURY

5 DR. RICHARD CODELL

6 DR. GUSTAVO CRAGNOLINO, CNWRA

7 DR. BILL DAM

8 DR. CARL DIBELLA, NWTRB

9 DR. DAVID ESCHE

10 DR. BRET LESLIE

11 DR. TIM MCCARTIN

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A-G-E-N-D-A

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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

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1 will be no introductory remarks by Ray Wymer.

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

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

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

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

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1 how are they doing it.

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

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

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

25 The issue will be, one, is that doable,

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

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

13 DR. ESCHE: Yes.

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

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

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

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1 credit for cladding.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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1 of different chemistries.

2 But in performance assessment -- and I
3 have heard you guys talk about this, but we always
4 want our most realistic number, and then our range of
5 uncertainty. We don't like the conservative bounding
6 effects, because when you propagate it through 10, or
7 15, or 20 perimeter distributions, you start ending up
8 with a ridiculous result, which has been said.

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

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

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

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

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

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

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

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

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

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

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1 shield, we will evaluate the chemistry at location
2 two.

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

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

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

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

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

25 Or if our mechanical folks said that you

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

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

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

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

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

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

24 ACTING CHAIRMAN STEINDLER: Yes, DOE's
25 TSPA.

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

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

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

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

22 And whether that is -- and I don't know
23 about the process model, but in DOE's TSPA they used
24 500 year time steps. Well, if you use the 500 year
25 time step and you were trying to look at chemistry

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

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

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

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

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

16 ACTING CHAIRMAN STEINDLER: Yes.

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

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

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

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

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

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

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

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

3 ACTING CHAIRMAN STEINDLER: The surface
4 area of what?

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

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

14 DR. ESCHE: Well, certainly --

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

17 DR. ESCHE: How did we pick what?

18 ACTING CHAIRMAN STEINDLER: Why did you
19 pick that?

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

25 ACTING CHAIRMAN STEINDLER: So you must

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

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

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

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

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

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

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

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

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

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

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

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1 defending that 10,000 year lifetime.

2 ACTING CHAIRMAN STEINDLER: Okay.

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

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

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

20 ACTING CHAIRMAN STEINDLER: Are you
21 talking about washing machines?

22 DR. CAMPBELL: Right.

23 (Laughter.)

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

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

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

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

16 DR. CAMPBELL: Right.

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

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

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

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

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

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

3 DR. SHEWMON: So that is penetration.

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

5 DR. SHEWMON: Thank you.

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

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

10 DR. CAMPBELL: Based on general corrosion
11 rates?

12 DR. ESCHE: Yes.

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

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

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

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1 than they may be in reality.

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

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

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

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

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

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

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

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

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

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

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

24 DR. ESCHE: Sure.

25 DR. CAMPBELL: Otherwise, things would

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

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

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

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

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

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

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

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

11 DR. CAMPBELL: Are they using pure water
12 diffusion?

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

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

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

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

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

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

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

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

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

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

25 But DOE has an experiment or two out there

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

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

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

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

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

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

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

4 ACTING CHAIRMAN STEINDLER: Okay.
5 Gustavo.

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

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

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

25 It changed the emphasis, and the emphasis

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

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

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

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

20 ACTING CHAIRMAN STEINDLER: Why would you
21 expect that?

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

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1 enough to dilute the chemistry back to ambient pretty
2 quickly.

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

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

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

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

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

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

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

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

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

16 ACTING CHAIRMAN STEINDLER: So two orders
17 of magnitude.

18 DR. MCCARTIN: Two orders of magnitude.

19 ACTING CHAIRMAN STEINDLER: That's still
20 not bad.

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

25 ACTING CHAIRMAN STEINDLER: All right.

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

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

6 (Laughter.)

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

9 DR. CAMPBELL: John.

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

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

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

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

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

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

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

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

25 DR. BRADBURY: What I want to make sure is

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

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

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

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

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

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

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

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

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

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

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

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

24 (Whereupon, the meeting was concluded at
25 9:27 a.m.)

CERTIFICATE

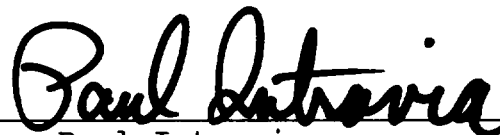
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