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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

133RD MEETING

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TUESDAY,

MARCH 19, 2002

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ROCKVILLE, MARYLAND

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The meeting commenced at 10:00 a.m. in Room T2B3, Two White Flint North, Rockville, Maryland, George M. Hornberger, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

GEORGE M. HORNBERGER, Chairman

RAYMOND G. WYMER, Vice Chair

B. JOHN GARRICK, Member

MILTON N. LEVENSON, Member

STAFF PRESENT:

JOHN T. LARKINS, Executive Director, ACRS-ACNW

SHER BADAHUR, Association Director, ACRS-ACNW

HOWARD J. LARSON, Special Assistant, ACRS-ACNW

LYNN DEERING, ACNW Staff

LATIF HAMDAN, ACNW Staff

MICHAEL LEE, ACNW Staff

RICHARD K. MAJOR, ACNW Staff

WILLIAM HINZE, ACNW Staff

CAROL A. HARRIS, ACRW/ACNW Staff

RICHARD P. SANVIO, ACRS/ACNW Staff

Also Present:

CAROL HANLON, DOE

PETER SWIFT, Bechtel SAIC

WILLIAM BOYLE, DOE

I-N-D-E-X

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

(10:05 a.m.)

CHAIRMAN HORNBERGER: The meeting will come to order. This is the first day of the 133rd meeting of the Advisory Committee on Nuclear Waste. My name is George Hornberger, Chairman of the ACNW. Other Members of the Committee present are Raymond Wyner, Vice Chairman, John Garrick and Milton Levenson. And also present we have a consultant with us today, Bill Hinze.

During today's meeting, following the planning and procedures session the Committee will (1) hear an update from DOE on its performance assessment program; (2) finalize the annual research report to the Commission, and (3) discuss preparations for tomorrow's meeting with the Commissioner.

John Larkins is the designated federal official for today's initial session.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. We received no requests for time to make oral statements from members of the public regarding today's session. Should anyone wish to address the Committee, please make your wishes known to one of the Committee staff. We have received one written comment from Mr. Mel Silberberg, on the research program. His letter will be inserted into the record at this meeting.

It is requested that speakers use one of the microphones, identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

Before proceeding, I would like to cover some brief items of current interest. Items of interest, (1) Dr. Victor Ransom has been appointed as the eleventh Member of the ACRS. He is a Professor Emeritus of Nuclear Engineering, Purdue University. Prior to this, he was a Scientific and Engineering Fellow at the Idaho National Engineering and Environmental Laboratory. Mr. Timothy Cobetz and Mr. Robert Elliott have been selected the ACRS/ACNW Technical Staffs. Rob, who returns to the ACRS staff having previously served on a rotational assignment comes from NRR and will replace Noel Dudley on the ACRS staff. Tim, who joins the Staff from the Spent Fuel Project Office, will assist both Committees as the work load dictates.

Dr. Margaret Chu has been approved by the Senate as Director, Office of Civilian Radioactive Waste Management. She comes to DOE from Sandia

National Laboratories where she has been in charge of the Nuclear Waste Management Program. Prior to that she was Deputy Manager for WIP.

The attached, at least attached in our book here, February 13, 2002, a paper by Commissioner Dicus, "The Future of Environmental Protection, a U.S. Regulator's Perspective" provides a most interesting perspective on this topic and I'm sure that anyone who wants it can get a copy of this document.

Any other items? Okay, good. We are going to move to our first topic which is an update on DOE performance assessment and John Garrick will chair this section of the meeting.

MEMBER GARRICK: I'm going to waive any opening remarks for the benefit of having the time to ask questions and what have you and I think we have three people that we're going to hear from: Carol Hanlon, Peter Swift and Bill Boyle. And I would ask each of them to give us a quick statement of their assignment or their role for the benefit of the record and the Committee and those in attendance.

So Carol?

MS. HANLON: Thanks, Dr. Garrick. Is this on? Can you hear me? Good morning. I am Carol Hanlon with the Department of Energy. I'd like to introduce to you my colleagues, Dr. William Boyle and Dr. Peter Swift and ask them to join us up here. They will be giving the main presentations.

Peter is with the Sandia National Labs, Performance Assessment, and he has had a very main role in helping us with our performance assessment activities as well as the prioritization effort going forward.

Dr. Boyle, as you know, is a Technical Advisor, with Yucca Mountain and has strong underground geotechnical expertise.

So the gentlemen will be making the presentations.

You know that the Committee has been carefully following our process and are particularly concerned both with the technical aspects as well as the performance assessment. We've briefed you many times and especially last year on several of these topics, including the Supplemental Science and Performance Analysis Document, the Preliminary Site Suitability Sites and Engineering Report and I know you've been at many of the key technical issue technical exchanges. So you're very familiar with these issues.

We're also familiar with and we have

carefully considered the letters that you provided, especially the letter on performance assessment and we're hoping that you will see some of your recommendations included in our path forward.

So I'm pleased to be able to speak with you today and give you an update on some of the information that has come out, some of the reports that have come out since last summer.

I've introduced Dr. Boyle and Dr. Swift and if I may just briefly cover some of the information as an introduction.

This is our snapshot on our home page which is available at www.yuccamountain -- www.ymp.com and it pretty nicely captures the major efforts, the major accomplishments we have had during the last year or so, the release of the Yucca Mountain Site Suitability Evaluation, Rev. 1 of the Science and Engineering Report, the SR Comment Summary Document, Supplemental Comment Summary Document, those responding to and summarizing comments that we received during our comments period; the final environmental impact statement and some other information as well as the state and county impact reports.

CHAIRMAN HORNBERGER: In the spirit of

engelbrecht, you did say ynp.com and I didn't know that DOE had become a dot com.

(Laughter.)

MS. HANLON: Thank you very much for helping me. Did I say dot com? Thank you.

Everyone will correct me. YMP.gov. And I will never use an acronym again.

(Laughter.)

So the presentations that follow address these technical updates and comments on preliminary site suitability evaluation. There are two types of them. One that evaluates the evaluation, the impacts of the final regulatory standards including the Environmental Protection Agency Standard 40 CFR Part 197 as well as Nuclear Regulatory Commission's 10 CFR Part 9, excuse me, 63.

In addition, the technical updates consider the evaluations of additional information which was available since release of the supplemental science report and analysis, the science and engineering report and the preliminary site suitability evaluation report, that information that was continuing to be collected and analyzed over the summer.

Another topic that we will discuss is the treatment of uncertainty in the total system performance assessment for the license application, both the uncertainty analysis and strategy and discussion of treatment forward of uncertainty and finally, the path forward for the Yucca Mountain performance assessment focusing on uncertainty that matters and risk-informed prioritization for performance assessment.

And you have in your book and in the presentation again these major developments, on-going technical exchanges with the staff during the year and we had another technical exchange last week in San Antonio; the release in May of the Science and Engineering Report which was based on the total system performance assessment in July; in August, releasing supplemental science and performance analyses as well as a preliminary site suitability evaluation; and including the updates later to total system performance assessment, staff recommendation and the technical basis which Peter will say something about.

CHAIRMAN HORNBERGER: Carol, what was the technical exchange last week?

MS. HANLON: It was on -- what was the title again?

DR. BOYLE: Laboratory design.

MS. HANLON: And the final regulatory standards not in July, but in June, 40 CFR 197 which was finalized in November, 63 -- 10 CFR Part 63 was finalized and also in November, the Department's 10 CFR 963 was released.

In December 2002, we had -- I think that's an error -- 2001, additional information documented was presented in four Letter of Reports which we'll discuss with you today and in February, the site recommendation went forward from the President. So we're in the process of realigning our science and performance assessment activities within BSC and moving forward with a consistent direction on treatment of uncertainty as well as focusing on the risk-informed performance-based approach.

So with, unless you have any questions on that brief introduction, I'd like to turn the microphone over to Dr. Swift.

CHAIRMAN HORNBERGER: Just a quick one, Carol. What's BSC?

MS. HANLON: Bechtel.

DR. SWIFT: BSC is Bechtel SAIC Company. It's the management operating contractor and this first presentation is the four letter reports that Carol mentioned. I'll go through them, fairly quickly, but just summarize what new information there is relevant to performance assessment since the major documents of last summer.

I should credit many other people, Jerry McNish, the manager of the Total System Performance Assessment Department, in particular; and Mike DeLugo, who was the lead on one of the four letter reports, the largest, that Update Impact letter report.

And just to clarify, there was one mention made there on Carol's side on realigning science and performance assessment activities within Bechtel SAIC and what has been done is that the Post-closure Science Programs have been brought together with performance assessment into a single organization called the Performance Assessment Project. Bob Andrews is the manager of that.

And the performance assessment calculations, the TSPA, Total Systems Performance Assessment, is one department within that larger Performance Assessment Project. In fact, there are several subprojects. TSPA now actually reports to me in this group called Performance Assessment Strategy and Scope.

The science programs we're familiar with for years also now report directly to Bob Andrews within Performance Assessment.

A couple of overview slides here, just to go through quickly. What we have here first, there's a body of information that is the Total System Performance Assessment for the Site Recommendation, TSPASR, documentation and with that I'm including the Supplemental Science Performance Analyses from last summer, last spring and summer.

This, the SSPA and the other documents that are associated with that, I believe have already been presented to this group, so what I'm focusing on are things that follow that, that's this page and the next one in the handout. A Letter Report in September, completed in September, looking at the impacts of the final EPA rule and also supporting the final environmental impact statement and then a Letter Report in December on the impacts of the NRC's final rule which was, we felt, there are enough things in that to run traditional analyses.

And then this technical update impact Letter Report, known by its acronym as the TUILR. These, so you can -- a graphic showing you what the documentation is, two pages of this. First page is performance assessment documents, going all the way back to September of 2000, a document called the TSPA, SR-Rev 0 and it's updated. ICN stands for Interim Change Notice. That's basically the revision. Updated in December, that's the version which people are most familiar with. That supported the site recommendation and the upper-tier documents that were released that spring and summer, but it was also updated in the spring, the supplementary analyses were published in July in something called the Supplementary Science Performance Analyses Volume 2, SSPA Volume 2.

Then September and December, new results that you probably have not seen yet. The Part 197 update and the Part 63 update.

MEMBER GARRICK: Peter, when you get around to doing the TSPA-LA, will it integrate all of these documents into the TSPA-LA?

DR. SWIFT: The TSPA-LA will be stand alone in the sense that it will be a complete documentation of its own set of analyses. It will probably most closely resemble the models used in these ones, but does that answer your question?

We don't have to keep sending you back to a lower tier or older documents.

MEMBER GARRICK: Okay, thank you.

DR. SWIFT: This talk is about TSPA, but

it's worth keeping tracking of the non-TSPA documents also, the upper tier documents of the science documents.

I've lumped them both together on this side. Go back to 2000, you have the Process Model Reports and the Analysis Model Reports prior to the scientific basis or TSPA-SR. They fed into an upper tier DOE document released last May, the Yucca Mountain Science and Engineering Report. These were contract reports. This is a DOC document. This is a primary technical basis for site recommendation, a thing called the Science and Engineering Report, published in early May 2001.

The scientific basis was updated again in the spring of 2001 in this Supplementary Science and Performance Analyses by one which was a scientific basis. This document, published in July as a DOE document, I believe, has new science that was not in this one. And also in Volume 2 it has new TSPA analysis.

Together, these two supported the preliminary site suitability evaluation. This is the document that actually makes the site recommendation case. That was a DOE document published in August. The cover date is July, but wasn't released until August.

And this is all material you've seen or was available. This is the new part over here, the November 2001, Technical Update Impact Letter Report.

(Slide change.)

DR. SWIFT: Now the Letter Report on the Final EPA Rule and it's worth actually noting the footnote. If you try to do a search in any records, data base, looking for that document, you'll discover that the title of it says it's input to the final environmental impact statement. That's correct. Informally, we think of it as the update report on the EPA rule and it was originally planned prior to the completion of the EPA rule. It was originally planned as an EIS update.

So the TSPA was modified to meet specifications in Part 197. We went from the average member of the critical group to the reasonably maximally exposed individual. We went from 20 kilometers to 18 kilometers, both for groundwater release and for the volcanic disruption scenario and ashfall. And the EPA rules specified 3000 acre/feet per year for groundwater protection. So we ran those.

(Slide change.)

DR. SWIFT: We also, these were the ones that were aspects of the analysis that was planned for the EIS, looked at both the base case waste policy act inventory and a possible expanded inventory. And that was the main point of the EIS.

We also ran some updated igneous activity scenarios. We reran human intrusion which we had not run since December of 2000 and we looked at two different times for human intrusion.

(Slide change.)

DR. SWIFT: So far those changes were all driven by regulation or assumption. We also did make changes in the model itself since the model used in the spring of 2001. I listed the most important one here first. Waste-package corrosion calculations for the results of that show -- used a general corrosion model that was independent of temperature.

In the SSPA, the supplementary results from next spring, we had used a temperature independent corrosion model which basically showed corrosion slowing at lower temperatures. We felt there was insufficient technical basis to support that for the site recommendation. You know it was already published in the SSPA, so we took it back out and that one change there counts for most of what you're going to see in these slides.

We found an error in our in-drift

thermal-hydrology work which we omitted heat transfer. It made a whole lot of difference. So we put it back in and got it right.

We had omitted portal transport from the portal due to intrusion. We corrected that. We had an updated version of a waste package degradation model. And we modified the inventory slightly at the request of the Naval programs to treat their fuel as part of the commercial inventory, whether it's a DOE inventory. It's a small fraction anyway and would make no difference.

(Slide change.)

DR. SWIFT: Results. These are mean annual doses and millirems per year. I'm not showing the complete panel of the doses that generated that, but these are means drawn from 300 realizations. The black curve here is the mean from TSPA-SR in December of 2000. The red here is a single curve shown from SSPA June-July of 2001. And this happens to be for the high temperature operating mode that we looked. This was only the high temperature mode here. In SSPA, we looked at high and low. And then here, blue and green, you can hardly tell the difference between them, this new modified model run for both high and low temperature for the updated model.

MEMBER GARRICK: Has the red curve not reached its peak yet?

DR. SWIFT: That is correct, the red curve -- unless that is its peak. We don't know that. But in the actual highest point on the curve is here. That's due a climate spike. By inference, we believe that -- we can't rule out the possibility it might have achieved a higher peak if it ran longer, but there actually is a peak in there.

Taking out the temperature-dependent corrosion, basically moves the time of large scale package failure from here to 740,000 years and what that has done is basically it leaves the -- in the red curve corrosion rates slowed as temperature dropped later and the green and blue curves, they do not. They stay at a higher corrosion rate throughout.

CHAIRMAN HORNBERGER: What's the change to explain the differences at early times?

DR. SWIFT: It'll come to me in a minute.

CHAIRMAN HORNBERGER: Is it an assumption on juvenile failures or is it igneous activity or what is it?

DR. SWIFT: No, it's juvenile failure. For the SR, we had input from our waste package engineers, but they saw no credible mechanism for juvenile failures, so we had none. This is the earliest general corrosion failure showing up here on the black curve.

For the updates for both SSPA and the more recent work early last fall, we have the first general corrosion failures later. They're out in here. But we do now have a model for juvenile failures, early failures, due to improper heat treatment of lid wells. The number of failures, in about a quarter of our realizations, we had one or two packages out of 11,000 failing. So it's a very small failure rate, but it produces a non-zero dose. It gives you small numbers. This is a non-zero dose out to there that is largely driven by igneous iodine and Carbon 14 in groundwater transport.

MEMBER GARRICK: Are you going to later get into a little more detail about impact of the changes in this -- in the model in relation to the difference in the assumptions between the TSPA-SR and these results? I'm thinking of things like if you've introduced this corrosion model now, has that brought seepage back into the picture as an important phenomena because in the TSPA-SR it was not an important phenomena.

DR. SWIFT: It's still not particularly

important here. It matters for transport away from a package, but the corrosion model is still independent of water saturation. As long as you have humidity, you have corrosion.

MEMBER GARRICK: And you still have the same model inside the waste package of the saturated water environment, those kinds of things?

DR. SWIFT: Uh-huh. Yes. The end package transport model, I think is when --

MEMBER GARRICK: So it's still diffusive transport that's the main?

DR. SWIFT: Yes. One significant difference between and this applies for both the red and blue-green here. A significant difference between these two curves and this one is that the -- in an attempt to put a little more realism in that diffusive transport pathway of the package, we now split the transporting waste that are transported by diffusive properties when they reach the drift wall, the rock. We put the diffusive transport fraction into the matrix of the rock and we put the effective, if there is effective, transport and that would that synchrony.

We put that fraction and it fractures. Previously, we put it all into the fractures, this curve, put all the waste and the fractures and that didn't seem realistic, simply based on the surface area available for diffusive transport, most of it is going to go into the largest part of the surface area which is matrix.

So that's the only change that I think comes to mind for me anyway, between -- for the in-drift transport model, between this, these curves and that one. Probably more realistic with the splitting of the diffusive.

Ask questions as I go. The time and the fact that I'm only one person, I wasn't planning to go out for a lot of detail in this stuff, so go ahead and ask question.

VICE CHAIRMAN WYMER: I have a question. How important was the microbiological corrosion? Was it important at all?

DR. SWIFT: Bill, do you want to field that one?

DR. BOYLE: I'm sorry, I don't have the answer.

VICE CHAIRMAN WYMER: Is it a minor player?

DR. SWIFT: No, I don't think it's a player at all.

VICE CHAIRMAN WYMER: The other question

is what is meant by aging multipliers of inside out corrosion?

DR. SWIFT: Aging multipliers for inside out corrosion. That is pretty cryptic. The model does not have an explosive treatment of the behavior of alloy 22 as it ages. Instead, we apply a multiplier to the corrosion rate to account for aging, changes in the alloy aging. I don't know what the update was, but someone felt, I suspect that in the SRR model we had an aging multiplier only on outside in corrosion.

Somebody pointed out we should have it on the inside out corrosion also. But it's an uncertain parameter. It's a parameter that has a range on it to account for our uncertainty in the effects of aging and corrosion.

VICE CHAIRMAN WYMER: So the multiplier then --

DR. SWIFT: Accelerates the rate.

VICE CHAIRMAN WYMER: Accelerates the corrosion, in some arbitrarily decided way?

DR. SWIFT: Uh-huh. In some -- I hope it's more than arbitrary, but it's not physics based.

MEMBER LEVENSON: Is the uncertainty ever symmetrical? It's always in a more dangerous direction?

DR. SWIFT: We'll come to that in the next set of talks. For these analyses, I believe it is asymmetrical in many cases. I would like to see more symmetry.

MEMBER GARRICK: Generally, more of a log normal than a uniform?

DR. SWIFT: I know where you're headed with the question. Keep asking it.

(Slide change.)

DR. SWIFT: The igneous activity results. I think that's the next -- yeah. This same presentation or say similar presentation was given by Jerry McNish to the Review Board in January and this figure drew quite a lot of attention from the Board who were displeased with the lack of prominence given to the word "probability weighted" here. I want to make clear of that right now. These are probability weighted mean annual doses. This is consistent with what's in Part 63. This is not being included with obvious dose you'd expect to see, but it is -- I'll go through that in a couple of slides here, what it really is.

This is the regulatory dose of volcanic activity. The black curve here is what was shown in

TSPA-SR in December of 2000 and the blue and red and curves here were updated in September and these, by the way are essentially identical. I was also updated in the SSPA in June and July.

The red and blue were a perfect overlay here for high temperature and low temperature operating modes. The volcanos are pretty insensitive to temperature of the depository.

Changes here, recent updates, since SSPA, specifically for this analysis, we move the location from 20 kilometers to 18 kilometers and we updated the biosphere dose conversion factors. We also made all the changes I just talked about in the nominal model. That's the other feature that's here.

There are a series of other changes not described here which were updated as part of SSPA in the spring. They are what account for this vector of 25 increase from here to here, interruptive dose and the decrease over here at later times.

The smooth curve to here or all the way out to here is driven by the volcanic ashfall dose and the irregular curve here and here is from the groundwater release from damaged packages and at some point in the future the basic weight, probability weighted dose from the groundwater pathway from packages damaged by igneous activity will cross over and exceed the corruptive ones. If you put edging for the black line, you adjust the eruptive half, we have a curve that kept on going out like that where it's a groundwater curve, goes like that. So at the crossover point, you see that the curve changed from being smooth to being irregular.

The sharpness here is due to a long term climate change in the model, it's spiking here. These are glacial climates.

The other major changes here between basically we -- at suggestions from the center and the NRC staff, we looked at a different wind speed data set which led to an increase of about, a factor of 2.5 from here to here. We updated our biosphere dose conversation factors --

MEMBER GARRICK: Does that mean you will look more at a wind row than a --

DR. SWIFT: No, the wind direction is still assumed to be fixed towards the location of the REMI for these. So that would have to be a factor of 4 or 5.

MEMBER GARRICK: Yes.

DR. SWIFT: It's not a huge player, but yeah --

MEMBER GARRICK: It is quite a huge player.

DR. SWIFT: The 4 or 5 add up.

MEMBER GARRICK: Yes.

DR. SWIFT: We looked at wind from a higher altitude. They had pointed out that we had a data set that went to higher altitude than we could have used and we used that and that was part of the difference here.

As a matter of fact, we unrealistically used only the highest altitude, the 300 millibar data only went into that, whereas for this one, we used a somewhat lower altitude data set against the full column of wind speeds and got the elevation up.

This also has an increase in the number of packages involved in the eruption, due to a recalculation of how we did that. Has increased dose conversion factors due to reconsideration of the nasal ingestion pathway. That's the larger particles lodged in the nose. We ended up putting in the long --

MEMBER GARRICK: But you continued to use the assumption that all the waste packages were degraded that were in the intersect?

DR. SWIFT: Yes. All packages in the -- we were conceptualizing the volcano as a conduit, a cylinder that rises up through it. It's also got an intrusive dike, a tabular body that may cross many drifts, but the portion that erupts, we're assuming is a cylinder with a mean diameter of about 50 meters of - or medium diameter.

Yes, all packages in that cutout by the cylinder are assumed to be fully destroyed. The phrase is damaged sufficiently to provide no further protection. And the waste within them is produced to the grain size of the particles which is by-products.

MEMBER LEVENSON: Is there any significant, for this type analysis, is there any significant difference in the footprint of the high level versus the low level?

DR. SWIFT: The high-temperature/low-temperature?

MEMBER LEVENSON: Yes.

DR. SWIFT: It's a simple scaling. It affects the probability that the event will hit it at all. And if you need to have a larger footprint for a lower temperature operating mode, then the probability of the event scales -- it's not precisely linear because -- it's close enough. If you double the footprint, you're going to double the probability.

MEMBER LEVENSON: I would have expected to see a little difference between the high temperature --

DR. SWIFT: Oh, thank you. Thank you. We didn't do it. We said that and that's a caveat that is in the text and I should say that. We simply used the one footprint for this and in text we discuss how to use the weighting factor if you want to.

It's not clear that we will have different footprints. One of the options was for low temperature, was to use the same footprint and a longer and more rapid ventilation period.

So we weren't quite sure what to do with that and it was going to be a nuisance to --

MEMBER GARRICK: How about the erosion time of the 1000-year erosion time for the 15 centimeter layer? You're still using that?

DR. SWIFT: No. Let me explain what we actually did here. This is -- this is a good slide to do it with.

This is the -- what we call the conditional dose, the dose that you would get -- this is a figure that we probably should have showed them ERB in January, but didn't.

If an event happened at 100 years and these were calculated by the way it would be SR modeled, black being the curve on the previous page. If an event were to happen at 100 years, a person living after that would receive a dose as shown here. So a person alive at 2000 years might receive a dose somewhere in this bandwidth here, the mean being red, so that would be 95th shown, both shown in black.

Clearly, the dose would be worse if you were alive at the year of the volcano. There's no probability weighting shown here. The uncertainty between the lowest and the highest curves reflects uncertainty basically in the inputs to our ASHPLUME or transport model, things like windspeed and the conduit diameter. That's basically how many packages are effective. And also in our biosphere conversion factors.

The slope of the curve, how fast it drops off through time is a factor of two things. One is there's radioactive decay and the other is how quickly that contaminated ash layer erodes away. And -- all right, that's the top figure.

The bottom figure down here is just mean curves. The red here. Now it's just the mean curve shown for condition events at different times at 100, 500, 1,000, 5,000 years.

This, if you were to draw a curve,

connects the dots through the tops of them, that's the radioactive decay curve. So these curves then are our soil removal factor. That's the rate at which soil will be contaminated and ash layers being eroded away.

However, our treatment is not quite as simple, John, as the way you describe it. What we're doing is we are assuming that the top layer of top soil erodes at a rate of 6 to 8 millimeters per year. I believe that's right. However, we're assuming that soil is plowed annually, so it's constantly being remixed to 15 centimeters, so that any way -- how thick the ash layer is, the radionuclides get mixed in to a 15 millimeter soil layer every year and the top of it gets skimmed off every year. So it's an exponential decay in our soil removal rather than a simple decay. There's always some still left there.

And so if we weren't mixing, we would take off that 15 centimeters in several hundreds of years. It would be relatively rapid. We are mixing, so that we're always creating a -- we've always moving radionuclides deeper down in that soil layer with each year's plowing and erosion.

Clearly, we are fairly sensitive to the way we treat erosion. If we had zero erosion, if the soil layer stayed there forever, this curve would look like the connecting the dots from the top of there, simply go out like that. Would be a radioactive decay curve.

CHAIRMAN HORNBERGER: This is a pretty critical part of the model for the first few thousand years?

DR. SWIFT: Yes, it is.

MEMBER GARRICK: And it struck me as an extremely conservative assumption.

DR. SWIFT: The assumptions that go into that have to do with whether you think we're dealing with agricultural land or stable desert soil. If we're dealing with agricultural land that really is being plowed every year, this may not be that unrealistic. We have a fairly high, compared to what, for example, the NRC staff has recommended, we have a relatively rate at which stuff blows off, but because we're plowing and mixing, that is consistent with what you'd expect to see on crop land.

On the other hand, if we didn't have this plowing and mixing going on, we had stable desert soil, we shouldn't have such air mass loading or such rapid erosion. We have pretty high air mass loading in our BACS. It's dusty air people are breathing in, consistent with agricultural land. It's blowing around.

It's what we did anyway. I just want to show one other slide here.

(Slide change.)

DR. SWIFT: This is how you get from those conditional doses to dose mean doses because this is something that it's not intuitive and this is just a question of probability space rather than a real phenomenon. This is what the role asks for and I believe it makes sense.

Think of these as mean doses from the previous curve, the mean conventional dose. If an eruption happened, Volcano 1 happened in Time 1 and you dropped the time axis here, this is dose/time, a person alive in the future could get in the Year T-1, they would get that dose. If they were in Year T-5, but the eruption was in Year T-1, they would get this dose here off that curve there.

If, on the other hand, they were in Year T-5 and an eruption happened in Year T-6 out here, they'd get a zero dose. The eruption hasn't happened yet.

Now put it into probability weighting, the probability that a person living out here in the Year T-5 could get a dose from an eruption that already happened back in Year 1, Year 2, 3, 4 and so on, well, the probabilities of those were all the same, similar to the process that has a time constant probability and so the probability weighted mean dose we'd get out here is simply the sum of the probability of all the events in the time interval of interest, 0 to 1,000 years times the doses associated with each one of those events at the time you're interested in.

So at Time 5, this person living here could be getting a dose from this event, from this event, this event, or that event. That one is a zero. And each one of them has equal probability and you multiply them and sum them up. And what you get when you do this, this is actually what we do, but a little thought experiment suggests that at early times, although the consequences are highest, the probability that the event happens in that year or has already happened, the probability is low. As you go out in time, the probability accumulates.

So the probability if you're living out here at the Year 10,000, the probability that the event has already happened is 10,000 times the probability in the first year. So in this sum here, the doses go down at later times, but the probabilities accumulate and you'd expect to see a mean curve that starts out low and climbs to some intermediate peak and then falls off again as doses decay from radioactive decay.

And that actually is what -- that's what the blue curve is here or the black one. The peak is around 3000 years and after that radioactive decay takes over and starts to drop off.

So that's -- the point of that explanation is just to say, show how we got from things that look like this to the probability of weighted sum that the regulation asks for.

I've got to speed up here.

(Slide change.)

DR. SWIFT: Human intrusion scenarios. This is the forced assumption that a driller drills through the waste package. Part 197 says one waste package, made a pathway to the borehole pathway to the saturated zone and assume it occurs at a time when the waste package is degraded enough that the grower would not recognize it. And this then is -- we picked 30,000 years. And this is an intrusion of 30,000 years as our annual doses, a full set of 300 of them with a mean shown.

And these are -- we also reran it for the proposed NRC rules, was prior to finalization of 197.

We used the 100-year time. What you see here, for example, from 100 out to the first arrivals coming in, this is basically your minimum saturated zone transport time. The spread of arrival is out from time after that show the spread and saturated zone of transport. So some realizations showed first arrivals here. Some didn't have them arriving too well out there.

(Slide change.)

DR. SWIFT: The December Report looked at the impact of Part -- final Part 63. The main difference here was the rule now requires us to use 3000 acre/feet per year for individual protection which is something the EPA has not clarified in their rule. So now we were using a sample value previously. We also in this report, if you get a hold of the report and read it, will discover we ran a couple of cases that are now moot following clarification of the word "unlikely" and the new proposed rule.

We went ahead and ran a case with an igneous intrusion eruption for the groundwater protection standard and also for the human intrusion. And with a clarification of the rule in those cases are moot.

What happened with the 3,000 acre/feet.

The result was to scale those by approximately two thirds.

CHAIRMAN HORNBERGER: Two thirds or three halves?

DR. SWIFT: Two thirds. We're diluting them.

CHAIRMAN HORNBERGER: Oh, that's dilution.

DR. SWIFT: This was a sample value. There's the range given, with a mean of about 3,000 acre/feet in our -- this is what we found from our survey of these in the region. Well, this just pushed us to the upper portion of the range in the rule and produces a little more dilution.

These -- the numbers shown here are the nominal performance only. These are the numbers of the doses due to the juvenile failures of nominal performance. We took the volcano out to show that.

CHAIRMAN HORNBERGER: It's two-thirds because you put everything into the volume.

DR. SWIFT: The larger volume.

CHAIRMAN HORNBERGER: Rather than have a concentration?

DR. SWIFT: Yes.

(Slide change.)

DR. SWIFT: The Technical Update Impact

Letter Report, this is the fourth of them. I said a letter report. This is -- this report is underlying science rather than TSPA. I had a much smaller role in this report, but Mike DeLugo is the person who did most of the coordinating of it.

The point here was documenting additional information since completion of the underlying science for the Science and Engineering Report and the Yucca Mountain Preliminary Site Suitability Evaluation. So this updates in science since roughly the spring of 2001. In some cases it goes back a little further than that. But this was a new work that was going on last summer and early fall in experimental programs. And then the impacts of this work were evaluated on TSPA and preclosure, basically to make sure there weren't any -- wasn't any new information that would necessitate a re-evaluation of the said recommendation.

It's a thick report. It's almost 400 pages long. It includes 11 White Papers in each of the topical areas where the technical staff was sent back to just document what is their new information. Then we had a rapid series of workshops where we looked at the impacts. To do this, we got the technical experts who wrote those White Papers together with TSPA analysts and we had our workshop setting. We went through each topic and on the spot estimated elicited impacts on total systems.

A couple of pages here of examples of the sorts of things. This isn't a very complete list. Just the sorts of information that was available: fracture data, seepage data. These were things that were written up and then people were asked how would this affect the input for models and the modelers were asked would this have an effect. And so on, the high profile one here, the discovery of high concentrations of chloride in seepage waters at low temperatures.

VICE CHAIRMAN WYMER: In your technical update, you didn't include a couple of processes.

DR. SWIFT: To the extent that they're captured in the on-going work related to the -- usually the air field environment and the engineered barrier system, yes, we did. It was structured around the existing science programs. We didn't force a White Paper on a couple of processes.

You want to deal with that?

DR. BOYLE: That fluoride example is actually -- falls within the realm of a couple of processes. As it turns out the flourine came from the materials that were introduced by jackets, but it was postulated that it could have been coming from the fluoride. That's the small amount of fluoride that's present in the rocks.

VICE CHAIRMAN WYMER: So to that extent you put it in.

DR. SWIFT: Yes. There were some portions of this were entirely preclosure, for example, updated data on aircraft activity. We have a new survey of aircraft traffic in the area, know that that would change the risk of accidental airplane crash, which seems moot now.

(Slide change.)

DR. SWIFT: So what were the results of this and if you've got a copy of the technical update, the TUILR, I recommend you go straight to the very back end of it on pages 350 on where there's an appendix that discusses impacts on post closure assessment and there are a series of chapters in that appendix, a weight for each of the various measures of the components. And here's the conclusion. All impacts of all the new work are insignificant, except for these two.

First, the Transport Team believes that they're now able to show a reduction of nominal dose, igneous dose, but the nominal performance. They thing it may show a more delayed retardation in unsaturated zone perhaps lowering the 10,000 year dose up to one order of magnitude. This is lowering, but it's also just pushing it up further in time. It's slowing the transport.

VICE CHAIRMAN WYMER: What's that due to?

DR. SWIFT: Excuse me?

VICE CHAIRMAN WYMER: What slows it down?

DR. SWIFT: It's a change in the way they've been treating diffusion near the matrix and this is not my field. It's out of the realistic case AMR and so-called realistic case AMR and that it was a change -- actually, I believe a numerical treatment in the model of the -- I believe it added more cells in the matrix, so you -- instead of having the matrix represented by a single cell and diffusion occurring all the way to the center at once and way back out again in a single step, I believe in the numerical model there, so it takes longer for the diffusion to get in and out. And it's the back out part where we're seeing the benefit.

The model probably will not be permitted in the TSPA because it's numerical intensive. But anyway, and also we weren't too excited about a possible one order of magnitude reduction in a dose that's already 10^{-5}. But it's worth knowing anyway. It may be there.

Possible increases in the eruptive dose. Basically, we looked at the impacts of the Center's model and the conclusion of our staff was that no more than one order of magnitude increase there. That's based simply on looking at the total number of packages that might be involved. The largest difference is between -- from a performance point of view, the largest difference is between the Center's model and the one we're using as the Center's proposed mechanism has more packages.

MR. HINZE: Peter, if I may, Bill Hinze. You have eruptive there. What about intrusive? Is that considered in that?

DR. SWIFT: Yes. The effective is only on the eruptive side here. That's deliberate to say eruptive in the dose there.

The Center's model that basically we're concerned about here, we worry about, is the one that calls for what I call a dog-leg eruption where magma rises, hits a drift, flows down a drift and goes back up again. Ours goes straight through and they proposed, and we can't rule out the possibility it would go a dog-leg path and sweep the entire drift and of course, the eruption. And if so, more packages will be involved.

MR. HINZE: Are you considering it at all in terms of the intrusive? Is that going to be coming along?

DR. SWIFT: We are reevaluating our model for the intrusive effects. We don't see a big impact there on dose. We believe our model needs more work to be ready for the LA, but we don't think that's going to change much.

MR. HINZE: Are you eliminating the zones that you had in terms of disruption of the canisters?

DR. SWIFT: That may be modified for LA. It may not. We're working on that right now. Some version of that is likely to stay at the Zone 1 of extreme damage and Zone 2, lesser damage, but in fact, there are igneous geniuses are working on that question right now.

MR. HINZE: Thank you.

MEMBER GARRICK: If the new data that's being talked about now on igneous activity results in an increase in the likelihood term, what is that going to do to your results?

DR. SWIFT: In terms of increasing the probability of a volcanic event or an eruption, those are separate probabilities.

MEMBER GARRICK: Yes.

DR. SWIFT: Increasing the probability of either of those at the site is pretty much a direct scaler on the probability, the way it goes.

MEMBER GARRICK: Right.

DR. SWIFT: There is a question about the air magnetic data and whether or not that will change the probability.

MEMBER GARRICK: So if the cases increases that a 10^{-7} number is not even justified on the basis of the supporting evidence and it may be more like 10^{-6}, if that happens it's going to be pretty much a linear effect?

DR. SWIFT: Yes. We don't think that's going to happen. Our of our impact assessment, we don't think that probability is changing much.

And I think that does it. No, I've got the summary slide.

(Slide change.)

DR. SWIFT: Just for completeness, to note that we did look at impacts on pre-closure performance of new data. Also, we didn't see anything there.

I think that's simply a summary side. Let's me sit down.

The analyses that I've just summarized here, basically provide confidence in the adequacy, appropriateness of the SR.

MEMBER GARRICK: I know we're running a little behind and I want to give the Committee a chance to ask questions, but you'll be hanging around, will you not?

DR. SWIFT: Actually, I should have said that right off. No, I have a 2:55 flight to catch.

MEMBER GARRICK: Oh, I see. Well, then let's give the Committee the benefit of your presence and see if there are any questions.

DR. SWIFT: Bill and I are your speakers until 12:30.

MEMBER GARRICK: And you have to leave -- yes. Okay.

Ray, go ahead.

VICE CHAIRMAN WYMER: In your backup slide 24, you indicate that Carbon-14 is rated Class C.

DR. SWIFT: Yes.

VICE CHAIRMAN WYMER: Why did you put that in there?

DR. SWIFT: I don't actually know where that came from, Class C. So I guess I can't answer your question. I realize it was likely to come. I don't know.

VICE CHAIRMAN WYMER: That's the first time I really heard it talked about the Carbon-14 being rated Class C.

DR. SWIFT: I can say that we do not have a realistic model for groundwater transport. This is based on the assumption that Carbon-14, carbon, in general, is a nonreactive species for groundwater transport, our groundwater chemists just don't like that.

(Laughter.)

DR. SWIFT: So basically that's an upper bound on Carbon-14.

VICE CHAIRMAN WYMER: You don't know where that came from?

DR. SWIFT: No, I don't.

MR. BOYLE: Good morning. Thank you for this opportunity. Peter had talked about some updates, and these next two talks -- the first by me and then I'll be followed by Peter -- are going to deal with uncertainty analyses and what we're doing with uncertainties.

For those of you that were present at the NWTRB meeting at the end of January in Pahrump, I made this presentation there, and I'm pretty much going to make the same presentation. And I think Peter will as well, for the most part.

This report -- it's available at our website, if you haven't seen it already. And it represents the work of others, in particular the two people whose signatures are on the report -- Kevin Coppersmith of Coppersmith Consulting, and Jerry McNish of BSC.

And Chapter 2 of the report was prepared by Jerry and the various process model leads. Chapter 3 was prepared by Kevin, and with input from Peter and Bob Andrews, comments from them. And Chapter 4 was prepared by Karen Jenny and Tim Nieman of GeoMatrix.

Now, the overview of the next two talks -- the first is by me on the report itself, "Uncertainty Analyses and Strategy Report," and Peter is going to talk about how to -- the implementation of a consistent treatment of uncertainty in the TSPA, total system performance assessment, for license application.

This is the title of Section 1 of the report. It's "Introduction," and the three main goals of the report are listed on page 2 of the report. I've distilled them here in these three bullets. This is what is done in Section 2 of the report. Summarize and discuss what we at the project have done to evaluate, clarify, and improve the representation of uncertainty in the total system performance assessment. That's Section 2, and it also gets at comments made by other groups.

Based on this discussion, Section 3 develops a strategy for how to handle uncertainties, and it also proposes some improvements for the future. And then, Section 4 deals specifically with how to communicate uncertainties to various groups, decisionmakers, technical people, and also proposes some improvements for the future.

The next I think it's six or so -- I think it's up through page 9 -- pages 4 through 9 of the package you have are a table. And it's related to something that's in Section 2 of the report. Here is the title of Section 2 of the report, "Evaluation of Uncertainty Treatment in the TSPA and the Significance of Uncertainties."

On pages 30 and 31 of the report, there is Table 2.2 and it's called "Key Remaining Uncertainties," and it deals in the table with these first four columns. And in the report there is in that table in the report, this fifth column isn't there. The information that's in the fifth column I'm showing you here is in the report, but it's in the text of the report. But we had a request from people at headquarters to distill down those paragraphs and pages in the report and create this fifth column.

As I did at the NWTRB meeting in Pahrump -- we'll be here all day if we go through each and every item in this table. The main point that I want to get across with respect to Section 2 is the various technical investigators were asked to summarize the state of uncertainties. What I asked them to do is I asked them, how can you sleep at night knowing that there was a potential at that time that a decision was going to be made? How can you sleep at night with the remaining uncertainties? And that's what this table and those parts of the report tried to capture.

We got back two very common answers of why these people were able to sleep at night. One is the uncertainties really didn't matter. They looked at a broad range, and for some of the items it didn't really affect the dose at 18 or 20 kilometers.

MEMBER GARRICK: But, Bill, isn't that dependent upon the model?

MR. BOYLE: Sure.

MEMBER GARRICK: Because in the VA, for example, seepage was a very important phenomena.

MR. BOYLE: Right.

MEMBER GARRICK: And so you changed your corrosion model, so that in the site recommendation report it's not an important phenomena.

MR. BOYLE: Right.

MEMBER GARRICK: And I think it's those kinds of connections that are very important.

MR. BOYLE: Right. And that's the point I would say when they -- when I say that there wasn't -- it really didn't have an effect or it wasn't important, it is with respect to the insights that were being gained by an implementation of either the TSPA itself or some subsystem.

But, you know, the answer is both. Sometimes it was -- when carried all the way through to the end of the TSPA calculation, it showed that it didn't matter, which then just raises the question, what if the underlying models really aren't right?

But those were the answers I got back from the PIs. One is it really didn't matter, it seemed, over a range of uncertainty. But the second answer that came back quite frequently, and is represented in the far right column, in various words is, "Well, I was conservative." You know, I took a bound, like the one that deals with the rock -- acknowledge that the analyses were very conservative.

Whether that's a palatable approach in the end, that is what was used at this point, and that's the answer that was given.

So with that, we can't possibly spend a lot of time on all these technical items. In January in Pahrump, I jumped up to slide 10. I'm going to do it here today again. And it's -- we're jumping to a new section of the report, and this was a very important section of the report, Section 3, and that's the title of it up there, "Strategy for the Future Treatment of Uncertainties."

And Section 3.1 of the report has a compilation of words from the regulation. It quotes from the EPA's regulation on how uncertainties should be treated. It has quotes from what at that point was -- probably started with the draft of 63, and then we may have stayed with the draft or perhaps we got the final comments from 63. I think we did get the final comments from 63, but also comments from this committee, the Nuclear Waste Technical Review Board, the NEA/IAEA peer review group for the TSPA, and also the peer review group we had for the TSPA-VA.

So we synthesized all of those -- you know, provided the quotes and synthesized those comments in Section 3.1. And then, in Section 3.2, came up with a strategy for the future. And on these next two slides, slide 10 here and 11, there were eight recommended things to do. And these are the quotes from those eight things. The first four are shown here.

And if you read the report, each of the eight recommendations starts off with a section in bold, and that's what's reproduced here. And so they are develop a total system performance assessment that meets the intent of reasonable expectation. That's defined in the EPA rule and also the NRC's word-for-work exactly the same.

Quantify uncertainties in inputs to performance assessment. Identify processes that encourage the quantification of uncertainties and gain concurrence on approaches with the Nuclear Regulatory Commission. And provide the technical basis for all uncertainty treatment.

Also, the fifth recommendation was to address conceptual model uncertainty. Develop a consistent set of definitions and methods for bounds and conservative estimates. Develop and communicate information that can be used by decisionmakers. And this is dealt with more explicitly in Section 4 in the next few slides. And also, develop detailed guidance and provide for its implementation.

After the report came out, the DOE sent a technical direction letter over from our contracting officer over to Bechtel SAIC and told them to develop this detailed guidance based upon a strategy, either this strategy or one similar to it, and incorporate that strategy into the planning exercises they were doing to get us out to license application. And that's what Peter is going to talk about in the next talk.

At the meeting in Pahrump of the NWTRB, detailed implementation was being developed -- a document. I have a copy of it here somewhere. It was being developed at that time, but now it actually has been developed. And Peter will talk about that.

Now, Chapter 4 -- or Section 4 of the uncertainty analyses and strategy report -- that's the title of it -- "Communication of Uncertainties." This exact figure is not actually in the report. There's a very similar figure in the uncertainty report. I think it's -- I wrote it down. It's Figure 2-13 on page F-18 that's very similar to this. But this is the slide I showed in January.

And what I wanted to get across -- for

those of you that -- you saw Peter's slide this morning, slide 9, that showed the black, the blue, the green, and the red curves. Carol in September showed a similar such figure when she was making a presentation for somebody else at a Nuclear Waste Technical Review Board meeting. Tim Sullivan was sick, and so Carol made that presentation with a very similar figure.

And there were comments from Dr. Knoppman, a member of the NWTRB, on the fact that that figure doesn't show any uncertainty. It just shows means. And so we took that comment to heart.

And if you go back and you look at the preliminary site suitability evaluation document that was out last summer, that also had figures of that type which didn't show any uncertainty, where now if you go and look at the final site suitability evaluation documents you'll see this figure and some of the other figures that I'm going to show in this talk.

At the time of the talk in Pahrump in January, the site suitability evaluation documents weren't final yet, so I couldn't reference them. But I was pretty sure that this figure might end up in it. This figure is also -- Peter showed, I believe it was slide 10, this morning, the one that he had labeled as the probability weighted dose axis.

There was -- you could have read about the controversy about that figure even in the general press. It made the Las Vegas Review-Journal, and The Sun, and also some of the energy-related documents.

The Nuclear Waste Technical Review Board, even in their most recent letter to DOE, had concerns essentially that generated from this figure and the one that Jerry McNish had shown in the presentation before, in that -- and I'm reproducing it here exactly how it was shown in January to show -- it's interesting that it comes up in a talk about communication of uncertainties.

The concern is is that it's just labeled as total annual dose, with no recognition that it's probability weighted. And there were some concerns perhaps that things were not being communicated quite clearly. But as I said at the meeting in Pahrump, if you go to the uncertainty analyses report, you'll see an explanation down here that does describe it as probability weighted.

Or if you go to the SSE, the site suitability evaluation document, you'll see a big paragraph that explains the fact that it's probability weighted. But for a PowerPoint presentation, in order to have a nice, big figure, that was stripped out. So, you know, there were no ill intentions, but it just shows that in communicating sometimes there can be unintended consequences.

Now, all of these charts -- these next few charts deal, as Peter has already described these charts -- these have to do -- when it says "total," it takes the disruptive igneous event doses and adds them to the nominal. In a sense, they just look -- because of the magnitude of the igneous doses, they just look like the igneous doses.

I would much rather show the nominal results. But by the time I get a few slides in you'll see that in order to make meaningful graphs of some of these results we have to go with something like the igneous results, not the nominal results, because the nominal results produce too many zero doses and they don't make very meaningful graphs.

CHAIRMAN HORNBERGER: So I take it you've solved this problem, and you now know how to communicate --

MR. BOYLE: Yes.

CHAIRMAN HORNBERGER: -- clearly what a convolution integral is to the lay public.

MR. BOYLE: We try.

(Laughter.)

I will say the January meeting ended at midday. I drove right back and met with Ken DeLugo for -- he was working on the site suitability evaluation documents. We went through every figure in there to make sure that the Y-axis was correctly labeled and that we had the big paragraph explaining it.

So we did take the Board's comment to heart. We did not want to be misrepresenting anything to anyone.

MEMBER GARRICK: Did the Board want you to continue to show all the realizations, given that --

MR. BOYLE: I'll get to that. But wait until you get to the next slide. One of the recommendations in this Section 4 of the report is, with respect to communicating uncertainties, there are different audiences. Some people are much more comfortable with a lot of detail. And decisionmakers, or those that don't have a background in mathematics, or in TSPA in particular, perhaps need less.

This is full-blown. But even in the preliminary site suitability evaluation, we never showed any such thing, which led to the comment about

Carol's presentation in September. So we did want to show -- this shows all -- this shows probably a maximum amount in terms of what you would want to show in the results.

But since some people have difficulty with the horsetail diagrams, one of the recommendations of Section 4 is we'll thin it out some, if you will, you know, clear it up. So these are essentially the same results, but it's just shaded in between the 5th and 95th percentile, still showing a mean.

To remove some of the distractions of all of the horsetails, try and get it across simpler, that -- if you will, that this is an air band, if you want to think of it that way, and it was shaded in to show the possible range of results between the 5th and 95th results. And this slide also is now labeled probability.

CHAIRMAN HORNBERGER: Why the mean and not the median?

MR. BOYLE: Why the mean and not the median? Because it's the regulatory measure. That's -- just make it simpler. You know, my wife understands the difference between the mean and median. She had to take a course. But many people do not, so --

MS. HANLON: Bill, before you go on, I just want to mention, since Bill is -- has talked about the fact that Dr. Knoppman, as well as Dr. Cohen, mentioned several times that they were unhappy with the level of treatment of uncertainty, in the final site suitability evaluation we did spend a great deal of time, both in the executive summary as well as in Chapter 4, going into a discussion of uncertainty and putting more treatment in with what Bill is talking about.

MR. BOYLE: Yes. And we may have added the first -- the two figures, the full horsetail diagram, which was a change from the preliminary site suitability evaluation. I believe we added this one, and I believe we added this one. And this is the figure that gets across why I'm showing the igneous -- the combined total doses rather than the nominal results.

This represents a cumulative distribution function and a relative occurrence of PDF, if you will, of the 5,000 realizations for the igneous doses. And we get a nice, smooth cumulative distribution function based on those 5,000 realizations. It goes all the way from zero to one.

Whereas, in the nominal, within the 10,000 years, which is what the site suitability evaluation dealt with, some 70 or 80 percent of all the realizations for the nominal case are actually zero. And it makes -- you end up with a funny-looking cumulative distribution function, which I didn't want to have to go into all that explanation, so we chose a data set that gave a nice, smooth one.

And this figure is in the site suitability evaluation, and what it represents is at the time of the peak in this plot, at 312 years, right here, we looked at all 5,000 realizations and plotted them up as a cumulative distribution function and as a relative occurrence of probability density function, if you will.

And it can be seen just at first glance because of the log scale, but it's a first cut. And so they look approximately normal, so it's a log normal result that's a first cut.

Then, my last slide, I ended with a quote from Charles Darwin. I thought it was appropriate relative to TSPA and uncertainties first, and so I don't -- I haven't read this book by Darwin, but I got the quote out of a book of quotes. And I don't know in which -- what context he made this.

But it's interesting that it's by Darwin and that our TSPA has been evolving, not by natural selection, but we hope by survival of the fittest -- you know, the better models for surviving.

Also, TSPA -- it relates to TSPA in that we are looking at the future in a TSPA, and we also must make judgments based with conflicting and vague probabilities. And with that, I turned it over to Peter with one last explanation.

I think as perhaps this committee knows full well, that there apparently are perhaps two types of analysts, those that are very comfortable with bounding, conservative approximations, and others that want a fuller representation of the uncertainties involved.

And I had -- after I put these slides together I attended a National Academy of Sciences meeting, Committee on Geological and Geotechnical Engineering, where that discussion came up of the frustrations when the two groups collide.

And it had nothing to do with Yucca Mountain, but it put it in perspective for me that we're not the only project that deals with this choice of, do we just bound it and get on with it, and remove some of the information, or should we deal with the uncertainties more fully?

And I said at the January meeting in Pahrump that the two different approaches, when viewed in the extreme by the proponents of the other approach, can be viewed as an unyielding rock, if you will, one that doesn't yield any sort of information, whereas the other can be viewed as this big whirlpool that sucks in all available time and money.

And with that image of a rock and a whirlpool, between which a path has to be charted, brought to mind Odysseus sailing between Scylla and Charybdis. And for us I said, "Peter is our Odysseus, who is going to tell us how he was to chart a course and the detailed implementation of how we were to treat the uncertainties."

And at that point, the guidelines that I showed you were in the process of being prepared, have now been prepared. I think they provide a proper course on how to deal with uncertainties. I'd like to think that this committee would feel the same way, but there's always a little caution in that, you know, the answers in the implementation, you know, that the guidelines are not that prescriptive in terms of everybody would follow them exactly the same way.

So time will tell, but I'm heartened by the approach that Peter and his staff have developed.

And I think he will tell you about it now.

MEMBER GARRICK: Just to telegraph something that may be for the benefit of Peter is that the problem is not whether the situation lends itself to a bounding analysis or a probabilistic analysis.

The problem is that when you do a bounding type analysis and you try to embed it in a probabilistic analysis with language that's very confusing, an example of which is to say, "Well, I don't know what the solubility is, and I don't want to put a distribution on it. So I'm going to assume that this is what it is, and it's an upper bound." And then you later say that there's no uncertainty associated with the solubility because you assumed a point value and as an upper bound.

Now, that's where you throw the system into total turmoil, and that particular flaw is very evident in the TSPA-SR. It's one thing to use bounding analysis in a screening capacity, and what have you, but it's another thing to use bounding analysis on something about -- something that's very uncertain, and then, in the wrap up say that there is no uncertainty associated with it because you bounded it.

And that's the same as ignoring the

uncertainty, and that's something that we have real concern with.

MR. BOYLE: Right. And I think on that same issue the Nuclear Waste Technical Review Board, they used different words, but it's the same issue of how --

MEMBER GARRICK: More elegantly, I'm sure.

(Laughter.)

MR. BOYLE: We ended up, particularly in the TSPA-SR, they commented on it in a letter of March 20th, 2000. We have this mix of where we've incorporated uncertainties for some parts, did not for other parts, and we've got this mix. The guidelines that Peter is going to talk about I think will try -- will end up in a better situation.

Hopefully, at the end of the implementation of those guidelines we won't have this unknown mix of uncertainties. We may still have some, you know, approximations and bounds in it, but hopefully we'll have a better handle on it. And that's what those eight bullets were supposed to get at, and then Peter was to implement it.

MEMBER GARRICK: Okay.

MR. HINZE: John, can I ask a -- Bill, can I get to your fifth column, a detail on your fifth column, which is kind of an ominous title. On page 9, you have, "New analyses may lead to reduction of the probability of explosive, eruptive phenomena." What analyses are these? Could you explain that a bit to us?

MR. BOYLE: You know, I would have to -- I didn't --

MR. SWIFT: The question is -- it goes to the type of volcanic eruption. Some volcanic eruptions involve violent eruption and ash pushed quite a long way into the atmosphere. And those are the ones we're worried about. They're called violent strombolean eruptions.

They're relatively rare in the geologic record from Yucca Mountain, but not -- they're there. But they're not the most common type, which are normal strombolean eruptions, which produce a cinder cone directly around the point of eruption and do not produce ash blankets over a large area.

The question is: what fraction of our eruptions are actually violent? And when does the violent phase occur? Is it early in the eruption or late in the eruption? If it's early in the eruption, then that's the time we worry about. If it's late in the eruption, the waste may already have been ejected into a cinder cone close to the conduit rather than being pushed out 20 kilometers.

For the SR and for all of the work you've seen, we took the copout path of bounding it with the assumption that our eruptions were, indeed, violent -- the strombolean ones. And so if we can justify a basis for saying that some -- only, say, 10 percent, 20 percent, whatever -- we can justify a value, we'll try to use that and produce our eruptive probability that way, our probability of violent eruption.

MR. HINZE: Thanks.

MEMBER GARRICK: Any other questions for Dr. Boyle before he sits down? Okay. Thank you.

MR. SWIFT: I wasn't completely prepared for it in Pahrump when Bill introduced me as Odysseus. I wasn't prepared for that. But it did occur to me that at least one point was relevant, that Odysseus had been on the road far too long and -- 22 years, was it? And whether that was me or the project, I wasn't quite sure. Also, it didn't have a happy ending either.

(Laughter.)

So Odysseus is not the analog here. It's Scylla and Charybdis that we're worried about.

You've got to start thinking about the

treatment of uncertainty with this question of conservatism versus realism. And these are just some simple observations here that -- many reviewers of our TSPA have criticized a lack of realism. There's a list of them, and this group is right there.

Obviously, there's a common theme. People are looking for something we're not providing.

The second bullet here is my own observation that I believe in general these reviewers, when they review the TSPA and find a lack of realism, they are in many cases not distinguishing between the TSPA and the underlying process models. For them, the TSPA is a window into the process models.

So if our process modelers make the assumption that they will bound a solubility limit within a range of uncertainty, we carry that forward into the TSPA. And, yes, it's a lack of realism. It's actually, I believe, a lack of realism in the underlying process models.

This is appropriate. I think a good TSPA should be a window into the underlying science. It should be the first place you go to look to see how well we understood something. But there are differences, and it's worth keeping those in mind. There are some places where we may have a more realistic treatment at the underlying level, and for good reasons have chosen to simplify it in the TSPA.

All of the reviewers' comments and expectations with respect to realism -- there's a good -- excellent summary of them in the Coppersmith and McNish report that Bill just mentioned, Section 3.1. But to me, I'm focused on what's in the rule, what has the NRC asked for in the rule.

And this is the two clauses out of the definition of "reasonable expectation" that basically for me sum up the issue pretty well. And I think, fortunately for the reviewers listed on the previous slide, these two bullets actually do put the key thoughts directly into the rule.

Characteristics of reasonable expectation include -- do not exclude important parameters simply because they are difficult to precisely quantify. And this one focused on the full range of defensible and reasonable parameter distributions, rather than only upon extreme physical situations and parameter values. These are the words out of the rule.

I actually take some heart in the site softness of language here. It's not fully prescriptive. It doesn't say focus exclusively on the full range or only use a full range. Rather, it suggests to me that we're looking for some common sense here, but, clearly, the goal was -- the goal is a full treatment of uncertainty.

So what's in our -- the guidance that we came up with for the project? What we're looking for is some version of a realistic analysis rather than a bounding one. But what's admitted right up front, some conservatisms will remain. Our job is to be clear about where they are, what the basis is for them, and what their impact is. There are cases where the applicant, I believe, is going to end up being conservative and explaining why and what -- how it matters.

Focus on a realistic treatment of uncertainty. That's not the same as a full understanding of realistic performance. This is a sticking point within the project. Realistic treatment of uncertainty sometimes gets equated with a full deterministic understanding of reality. And the first here is achievable -- realistic treatment of uncertainty. The full understanding of realistic performance is not achievable. That would be the -- that would require 10,000 years yet.

So the bullets that go along with that for me -- simplified models are okay in the TSPA. Broad uncertainties are okay, if they're justified and explained. This is important. Scientists generally think of their job to be to reduce uncertainty. We need a shift in mind-set here. Our job for TSPA is not to reduce uncertainty; it's to make sure we've adequately included it.

So broaden the range of uncertainty rather than -- based on present knowledge, if you weren't confident with the uncertainty bounds you've put in, make them broader. If you weren't confident in them, that meant they weren't broad enough. And then see if they matter.

MEMBER GARRICK: I'm pleased to see that there. That's a very important issue.

MR. SWIFT: These are just words so far. We still have to implement these. But that thought -- the shifting of a scientist's mind away from 20 years of experimental work driven to reduce uncertainty to the simple statement "give me a broad uncertainty amount," that's a difficult shift.

Scientists and PA analysts need to work together to incorporate uncertainty in the TSPA. I'll have more to say on that. But it -- it can't be done by either the process scientist or the PA analyst independently -- and focus on a clear explanation of what we did, mathematical/conceptual descriptions. If we're talking about parameter uncertainty, you'll actually be able to see the equations in which the parameter was implemented and the traceability. That's something to strive for.

This thing called the guidelines document. This is -- Bill described it as having been required contractually by the DOE in a direction letter in December. It was delivered on March 1st. It's a rather dull document. I apologize. Guidelines for developing a document and alternative conceptual models, model abstractions, and parameter uncertainty in TSPA.

It's, I say, dull because we don't want to call it a procedure. We are not -- it's not a quality assurance procedure in that sense, but it reads like a procedure. I wish I knew how to fix that.

It describes the -- it meets the requirements of the technical direction letter by implementing the strategy outlined in the report Bill described. It also addresses some NRC KTI agreements, and the last page of this handout is the text of those agreements.

The important thing here is that it uses a team approach for both models, the alternative conceptual models and the abstractions. And for the parameters we set up a three-cornered team -- a triangular team, with a lead for models, the abstraction models. I use the same person as the lead, he or she, same person does the lead for the alternative conceptual model work. And a parameter lead, and then a subject matter expert and a TSPA analyst.

So think of it for parameters, where there is one parameter team lead, but for each uncertain parameter in the TSPA there will be a subject matter expert and a TSPA analyst who -- the three of them jointly have to agree on the distribution for that parameter and actually sign off on it. Likewise, the models.

The model abstractions -- the goal of abstraction is to capture the important processes, the processes that are important to system interactions, and to make sure that the abstraction allows an appropriate representation of uncertainty.

This is important. The abstraction is going to use simplified parameters, often lumped parameters, to capture quite a lot of things. They have to built with an eye towards, can we actually assign uncertainty -- representational uncertainty to those parameters in a meaningful way?

The sections get developed by the subject matter experts. These are for the scientists -- reviewed by the process model analysts. They're developed in the scientists' reports. These are the AMRs, the analysis and model reports.

There is no prescription on how to actually do an abstraction, recognizing that they can be everything from -- well, not listed -- you could just put the full numerical model into the PA. You could simplify it, simple functions, response services, parameters.

The implementation in the TSPA gets back-reviewed by a subject matter expert, and that implementation gets documented in the TSPA's report.

For alternative conceptual models, there's a little simple step-through process here that we're asking our model developers to walk through. For each process of interest in product alternatives, if any, with consistent available information, there's no requirement here to go out and make up alternatives. In fact, there aren't any that are consistent available information.

If only one conceptual model is consistent with all the information, that's good. That means you've -- you don't have a valid viable alternative to conceptual models. Instead, you have things that can be screened out. And you document that at that point. That basically is part of our FEP screening process. Things like, for example, seismic rises in the water table that might flood the repository is not an alternative conceptual model because it is not consistent with available information. We believe that can be ruled out.

If you have multiple viable alternative conceptual models, evaluate their impacts on subsystem and component performance. That's the process model or the specialist in that area. If there are alternatives, if the alternatives result in the same subsystem performance, i.e. the same information that you delivered to the system model, then, again, alternative conceptual model uncertainty is not a significant source of uncertainty in the total analysis. Doesn't matter which alternative we use, we're getting the same result out of it.

If two or more show different subsystem performance, develop abstractions for both and deliver them to TSPA. That takes you back into the abstraction process. Basically, have them reviewed by TSPA and implemented.

Here's a point. If the abstractions for the alternatives are not straightforward, this is a place where I think you're going to see some conservative choices come in. I don't really have an example in my head, but some -- let's suppose somebody proposes an alternative conceptual model which would show improved performance but is going to be a heck of a chore to abstract it into the TSPA. Perhaps the example I gave earlier of metrics diffusion in the unsaturated zone might be one.

This is a place where I think the project will probably take the cost effective approach and explain why they're being conservative.

TSPA evaluates --

CHAIRMAN HORNBERGER: Peter?

MR. SWIFT: Yes.

CHAIRMAN HORNBERGER: On that point, it strikes me that what you're -- if what you're saying is that you have alternative conceptual models that are consistent with information, then you don't have a clear way to choose one over the other.

MR. SWIFT: Right. Yes.

CHAIRMAN HORNBERGER: And it strikes me that all you're saying is that, fine, if they give different performance we will use the one that shows the worst performance.

MR. SWIFT: Yes. Well --

CHAIRMAN HORNBERGER: Is that right?

MR. SWIFT: That will do. The question is, at what level do they show the worst performance? If they're showing different performance at the subsystem level, that isn't -- doesn't for sure mean they're going to show different performance at the system level. But, yes, other than that I -- same as what you just said. But the idea is to actually direct people who document this process of thinking.

MEMBER GARRICK: It seems there's kind of a corollary rule here that would apply, too, and that is that if you have multiple conceptual models -- and let's say that those models provide the same results -- then you ought to use the simplest model as the basis. This is the Copenhagen rule for the great physicist.

CHAIRMAN HORNBERGER: It actually precedes Copenhagen, because it's William of Ockham in I believe it was 1674.

(Laughter.)

MEMBER GARRICK: Well, Niels Bohr picked it up and --

(Laughter.)

-- made the point very elegantly that if you have multiple theories, and they give you the same results, we're going to, by damn, take the simplest one.

MR. SWIFT: The problem is when they don't give you the same results.

MEMBER GARRICK: Yes, I understand. But there is that issue that there is a tendency sometimes for modelers to want to impress you with the complexity rather than impress you with the simplicity.

MR. SWIFT: If the two models give the same subsystem result, my conclusion, alternative conceptual model uncertainty is not significant. The under bullet not stated there is that the subject matter expert then has to document that as to, yes, I have these multiple alternatives. They all give me the same result. Therefore, I'm only going to deliver the simplest one, or the one of their choosing, forward in the TSPA.

MEMBER GARRICK: Yes.

MR. SWIFT: And that actually is in the guidance document. That step is there.

VICE CHAIRMAN WYMER: Now, this is turning out to be a lot of extra work, but -- and looking at various parameters. But what you haven't said is how the parameters should be looked at.

MR. SWIFT: Let me get to that on the next slide when I talk about parameters. Let's imagine here that the alternative conceptual models are implemented in TSPA and you actually run a full TSPA or a subset of TSPA with the different alternatives in it.

And if the options -- the impacts are significant, then the options are -- there are basically two options. One is you can carry the multiple alternatives all the way through to the regulatory dose, but then you have to weight the alternatives, and then you have to be able to defend those weightings in some way.

So that may not be the -- the first simple thing, you always give them equal weight, but if they -- and see if it makes a difference. If they don't make a difference, then you learn something. If you can't defend weights, then at that point, again, you default to the more conservative one if you've gone through this.

Parameters --

VICE CHAIRMAN WYMER: I'd say selecting a parameter value is not the same as selecting --

MR. SWIFT: Yes, I'm aware of that. The assumption here that you just caught me on is we already know which parameters matter. And the identification of we're actually -- this is the step in the process that we're in right now. It's identifying the parameters we want to treat as uncertainty parameters in the TSPA.

And we're doing that by -- the TSPA team is providing a list of the parameters that were treated as uncertain parameters in previous analyses back to the subject matter experts in each of their areas for review and updating.

There are parameters that have been treated as uncertain parameters in past analyses that actually aren't doing very much in the analysis. The analysis would be insensitive to the uncertainty in them. If that uncertainty was appropriate, you know, justifiable, defensible, and still was doing nothing, then that parameter might be a candidate for one to be switched to a fixed value.

If, on the other hand, the subject matter expert looks at that list of uncertain parameters and says, "Whoa. Here's the one that really captures the process. Better put a distribution on that and get it in there," that will happen. But the real answer to your question is that this is -- it's human judgment, and this is why an iterative analysis better, because people learn things through time as to which sources of uncertainty matter at the system level.

We've learned a lot in 10 years of TSPA and interacting with the process model teams. I actually do think that we have the right uncertain parameters, probably more of them than we need, and there isn't a unique test to make sure you've gotten them all. That's, from a judgment, an iteration and review.

But once you've got the list of uncertain parameters identified, categorized, they get mapped back to the subject matter experts for documentation in their AMRs. And the full range of defensible and reasonable distributions gets documented by the subject matter experts in their AMRs in that triangular model with the team lead and an analyst.

There are two things yet to consider in building uncertainty distribution. First is the available data. But, second, and this is the part that typically gets missed, you have to think of how the parameter is used in the model. Model scaling issues, what's the cell size in the model. These are numerical models, and it makes little sense to use porosity data collected at a sidewall core this big, use that exactly as is in a model where you might have cell blocks hundreds of meters on the side. The distribution is something different.

So think of spatial variability, which is the example I was offering there, because it affects the scaling of the parameter, how it's used in the model. This is the point where you want the modeler, the person who actually knows what the parameter is doing in the equation in the model, working with the subject matter expert most familiar with the data, working with the team lead who is -- will have -- the statistician who is supporting them in how to apply the -- how to build a defensible distribution from the available data.

And it's typically not a matter of fitting it with a normal or log normal or some specified model, because nature doesn't work in statistics like that. What you want is -- what we want is a distribution function that doesn't add any new information, honors information we have, doesn't create new knowledge.

So the simplest example of such would be a peacefulized linear distribution. If you actually thought data itself was appropriate to be used in the model as was -- as is -- a peacefulized linear fit for the data is better than trying to force fit a normal or log normal distribution.

But the distribution is -- ultimately, it's a subjective decision, and you want it made by the right experts -- the scientist, the PA modeler, and a statistician with experience in doing that. And then you want it documented, and so we'll do that, and then implement things through a controlled database.

MR. BOYLE: And also, I think in the way this system is set up, it's the consideration of alternative models frequently gets at which parameters are under consideration. For example, if your model is that the rock is elastic, well, Young's module and Poisson's ratio is sufficient. If, on the other hand, you assume that it is viscal plastic, well, then, that generates a whole new set of parameters for which you then need values.

MR. SWIFT: The last slide here, this one actually got edited a little bit in the final review, and you'll get a kick out of what came out of it here. This bullet used to say that regulators and reviewers are not asking for the impossible, and someone felt that was a little too negative.

(Laughter.)

But I actually believe it. And I'm getting back to the idea there that if we, the DOE, were to misinterpret what you're asking for as -- I hope I'm going to get a head nod here. If we were to misinterpret that as asking for a full, realistic, deterministic solution to the future, that is impossible. We're not going to do it.

But we can commit to a realistic treatment of uncertainty. Can we actually achieve it? It will be some version of it, but there will -- pragmatically, there will be conservatisms here and there. And it's our job to explain what we did.

And this is the last point -- there's no unique solutions. A lot of credibility comes from how well we can explain it.

And that's it for that presentation.

MEMBER GARRICK: Before we go to the next, any comments from members of the committee? Milt?

MEMBER LEVENSON: Well, I'd like to make one comment. It's sort of a follow-on to the comment John made earlier, and that is that it's not of major importance that we reduce uncertainty. The importance of uncertainty is only to make sure that the true extent of some risk is not obscure, and that, as in medical work, false positives are equally to be treated as false negatives. And the whole objective is to make sure that you know enough so that you can evaluate the risk. Now, reducing uncertainty per se is certainly not an objective of mine.

MR. SWIFT: Others see things differently. I agree with you completely, but the -- on the alternative conceptual model side, for example, a question I got from the ATRB was, well, where is the step where you go out and design an experimental program to go back and test those models and throw one or the other of them out?

And that isn't where I actually was thinking. I was thinking we're going to make a decision based on information that we have now. We're going to decide if the uncertainty matters. And it's a different way of thinking of things.

MEMBER GARRICK: Ray?

VICE CHAIRMAN WYMER: Well, actually, you're not going to make a decision based on information -- based on what you have between now and the time of your license application. No, I've raised my questions already.

MEMBER GARRICK: George?

CHAIRMAN HORNBERGER: Again, just a comment following on what you've just said, Peter. I think in part, as I read some of the TRB comments, it would be that the question is whether or not there is an adequate scientific base to support the models that you have.

MEMBER GARRICK: Yes. And I like the -- your remarks about -- that the distributions or the uncertainties need to be driven by the information or the data. Too often we've seen people spending a great deal of time and effort and exercise on trying to choose a distribution that will work for them in their model.

And there's enough analytical tools available now that there's no reason for doing that. We ought to be able to forget about whether it's log normal or beta or gamma or whatever, and let the information, however it comes out as a distribution, be the basis of the model.

And even if it's a histogram, because there are tools now that very effectively convolute discrete probability distributions. And that is even a more -- often a more accurate representation of what's taking place, and very often much easier to follow.

So these are encouraging signals that you're giving in the context of guidance. I think it's very important. And I think it also addresses this whole issue of the confusion that sometimes exists between good science and adequate science with respect to solving a problem, and we've talked about that a lot in this committee.

We want good science, but we don't think it's necessary to reduce an uncertainty between, say, 10^{-12} and 10^{-7}, even though it's five orders of magnitude, if the risk is of the order of 10^{-3}. So that's adequate, even with that wide amount of uncertainty. So these are steps that are very encouraging to us.

Any questions from the staff? Yes.

MR. HAMDAN: Uncertainty means different things to different people. Milt and I and perhaps everybody in this room understands uncertainty as has been described, and Milt described it very well. And on slide 11 from your first presentation --

MR. SWIFT: In my first presentation?

MR. HAMDAN: Yes. Which igneous activity -- you showed us the slide about the effect of igneous activity -- if you want to -- this was what obscured uncertainty. This is very clear. It goes to the point and evaluates the effects of igneous activity of a property of one. So this has nothing to do with uncertainty.

But to the public and people who are on the street, this is the uncertainty. You are saying in this slide to them that if -- it were -- this is the point that you are going to give to them, not to us in this room. We evaluate the risk and then we make a recommendation based on risk.

But to the people on the street, this is uncertainty. And I think this needs to be looked at and responded to and articulated to the public. So this is the comment that I make.

I have another question for Peter, and that is on your slide on the parameter uncertainty. You probably will need to pull that out. The approach is fine, and you have articulated it very well. The real questions come with the -- when you want to assign probability distribution to a certain parameter, that's where the rubber meets the road.

There sometimes you don't have enough data to select a distribution, and that's where the problem lies. There are a lot of parameters with a distribution that have validity bases, and I wonder if you could extend your answers from that -- for conceptual models to do alternative distributions for these parameters, and satisfy yourself that, really, it does not make a difference.

And that will probably be needed because there is simply a lot of parameters with uncertainty for which we do not know what the right distribution is.

MR. SWIFT: My own experience in analyses like this is that for parameters to which the results are sensitive, the form of the distribution is less important than the range. What you're worried about are the impacts of the tails.

And so the difference between a log uniform distribution and a log normal distribution will not be that great. The difference between a log uniform and uniform distribution may be very important, but I think picking distributions that span a broad enough range of uncertainty that the range itself is defensible -- just what the scientists believe is a broad enough range -- will take quite a lot of the concern off the shape of the distribution, the actual form or function used to fit it.

MEMBER GARRICK: I see this next presentation is -- yes?

MR. HINZE: Well, I guess wanted to suggest that the most important, the most critical phase of this whole thing is making the decision on when you have done sufficient sites that you have a model that you can do some calculations with. It's very hard to put uncertainties on that.

It reminds me a bit of the ore deposit at Roxby Downs in south central Australia, which is the most important mineral discovery since the Second World War. It was found as a result of a very incorrect model. The answer was just beautiful. It's correct, but it was a totally incorrect model.

Now, if you use that incorrect model, which some people have tried to use, in other parts of the world to find a similar ore deposit, you're just not going to get there. It seems to me that you can put parameters -- the uncertainty around these parameters, but it's the question of when you've done sufficient science that you understand the process well enough so that you can make a judgment, and that judgment will have uncertainties.

MEMBER GARRICK: That's correct. Okay. I see this next presentation is a big one.

MR. SWIFT: It's not as big as it looks. I'll give you the fast version.

MEMBER GARRICK: We would like to hold, as best we can, to our 12:30 recess.

MR. SWIFT: Me, too.

So I will certainly not go through all of these. I will give --

MEMBER GARRICK: I have an advantage. I can ask all the questions and extend the time, and then blame you --

(Laughter.)

-- blame you for overrunning.

MR. SWIFT: Can we have the lights here? So we can see the screen better. Good. Thank you.

Since December, the project has gone through a replanning exercise. You'll recall that in -- starting in the summer of 2001, the project went through a planning exercise for a multi-year plan prepared by the M&O contractor, BSC, and then to be approved by the DOE, for the work to be done to support a license application.

And the plan, which was submitted in September, produced a large body of work that was -- scientific work that went out to about -- an application in about 2006. The dates were somewhat flexible. But the DOE came back to BSC and said, "Perhaps you want to replan."

There was not a prescribed date. However, we felt it was prudent to replan with the idea of 2004 in mind to see if we could, in fact, identify a scope of work that would allow us to produce a docketable license application in 2004. Of course, docketable is the NRC's decision, not ours, but we wouldn't submit an application unless we felt there was a reasonably good likelihood that it would be docketed.

So with that in mind, we set out to prioritize work in the performance assessment and science activities, which we realigned the project at the same time, so that the science became part of the performance assessment project, and focused primarily on the work that was necessary for license application, identify and select an overall scope of work to balance the project management risks. This is not human dose risk. This is the management risk. What's our risk of success or failure? And document it.

And this, then, would be the basis for the replan that was delivered to DOE March 1st. Back in December, we were planning ahead. In fact, we did -- BSC did deliver a new multi-year plan to DOE on March 1st, and that is in DOE review.

And that has not been released yet. Is that correct? The plan B of -- anyway, it's a big fat thing, work plans for the outyears, starting immediately but out to 2004.

And inform these decisions with input from the TSPA analyses, technical staff working with the science program staff, line management, project management -- this will be the senior management team -- and then project planning. These are the people who ultimately -- they're the ones that have to prepare a multi-year plan.

In theory, it's -- no, more than theory, it is a resource-loaded schedule where you can actually point to a schedule and see what things cost, how long they take, and what they do. And that's the -- so the process we wanted to go through here, just for -- this is -- remember, we're only prioritizing work within performance assessment and science.

This does not include design activities. This does not include licensing activities, quality assurance activities, the various support activities. It's only a portion of BSC's budget that was developed in such studies.

The PA team identified attributes -- basically, a short story here -- we're headed for a multi-attribute utility analysis. And we've done it. That's where this clarification is headed.

The PA team defined the attributes at which the work scope was evaluated. The department managers of the science departments defined work scopes to be considered for each model, component. Think of the unsaturated zone or the saturated zone, and so on. For each model component, they defined alternative work scopes they wanted to have considered. For each of those they should have estimates of cost and time.

And the department managers and the TSPA modelers provided initial estimates of the impact of the proposed work on the attributes. Basically, an attributes questionnaire. We scored each work scope description.

There were 25 model components, and each one had about -- almost each one had three work scopes, so about 75 different work scope descriptions were scored against these attributes. And that was actually done in a workshop in January where we had the key players all together in a room for three days and went through scoring the -- first of all, we wrote final work scope descriptions, we scored them against the attributes, and I'll go through how we -- what that means here in a minute.

And then we ran them through the utility analysis tool that I'll describe in a minute, produced an initial prioritization, in mid-January had a management review of it, and provided input to our budget team at the end of January. And that, in turn, has gone on to DOE now.

This is a conceptual figure that -- it's important because this came out of a BSC management meeting in November -- the idea that, since most of us think best in only three dimensions, let's find -- think of it in three dimensions. What are the things that matter to us in making decisions about what science activities we do on the project?

And we came up with three axes -- a quantitative performance axis. What is our calculated total annual dose? And what work are we doing that moves it up or down? This is, you know, basically, are we in compliance with 63/113?

Regulatory defensibility and acceptability -- in a regulatory framework, can we defend the models and data used to calculate that dose? Have we met the qualitative requirements of Part 63? This axis is Part 63 and 197, as implemented through 63.

So things like multiple barrier requirements to qualitative, descriptive requirement, but that would live on this so-called X-axis.

Satisfying KTI agreements -- the NRC has given us a list of what needs to be done to defend the models. We, in some form or another, need to address those agreements and produce defensible models. Then -- and we need, say, for example, quality assurance requirements, our so-called X-axis attribute.

Then there are the Z -- what we call the Z-axis out this way. This was a -- Y was up in this coordinate system when it first appeared on our white board. So the Z-axis here, qualitative acceptability, internal and external defensibility, these are issues that we know we care about them, yet you can't trace them to anything that's in the rule.

So some of these are -- some of them are actually quantitative as well as qualitative. But qualitative things -- defensibility of models, beyond what's needed for a regulatory framework, the question of, can we convince people we actually understand the system well?

Many of the Technical Review Board's concerns are on this axis, not all of them. I'll argue that some of the NRC staff and center's concerns may be on this axis. They're valid. This is not to say the Z-axis is not important, but there are technical issues that don't tie directly to the rule.

For a quantitative one, an easy one to think of is peak dose. There's no regulatory limit applied to peak dose occurring several hundreds of thousands of years out. At the peak dose is a quantitative Z-axis attribute.

So it's not three-dimensional space, and those axes aren't orthogonal. And they're certainly not mutually exclusive. We decided to define it as a 16-dimensional space for the purpose of the utility analysis, and nobody can think in that, but the spreadsheet does.

There are 16 attributes here that can be coarsely lumped against those three axes, but, in fact, for the utility analysis we scored things on each one of these attributes without considering those at X-, Y-, Z-axis. That's just there as a communication tool for our own management team.

For each work scope, we went to the technical staff and said, "Will your work, if you do your Level 1, 2, or 3 scope" -- and I'll explain what those are in a second here -- "will that change 10,000-year mean annual dose?" Which, by the way, that is driven entirely by the volcano. That's the 10,000-year total. That's the Part 113 dose.

Will it change groundwater concentrations or human intrusion? And that's it for quantitative performance.

Regulatory defensibility -- have we captured all credible FEPs? Have we excluded the ones that can be excluded according to the criteria? Are we meeting our requirements to describe -- identify and describe multiple barriers? And do they link to specific KTI agreements?

The so-called Z-axis sorts of attributes impact on conference of internal reviewers, impact on conference of external reviewers, and some quantitative ones have come out of the TSPA calculations. Change in time to 15 millirem. Change in uncertainty. This would be the distribution spread from the 9th and 5th, for example. There's no regulatory driver for that. It's the mean we're regulating on, but we do care about that spread in the uncertainty and system outputs.

We looked at a forced early failure case, peak dose, and this -- I should say associated with conditional igneous intrusion. We had a question there about, will your work affect our conditional igneous dose? Will your work affect a representation of uncertainty at the parameter level? And our ability to defend the conceptual models.

The actual questions themselves are shown in the handout. I have time to show those a little bit. This goes pretty quick here.

For each model component -- there are 25 of them I'll show up here in a minute -- department managers define three levels of work. The expectation was that this would be an increase in cost and/or time. Level 1 would be the quickest and cheapest. Level 3 would be the longest and most expensive.

Level 1 -- what work would be required to complete quality assurance issues and to validate the existing models? That's not focusing on not developing new models, but meeting our own internal validation requirements for the models that we used in the analyses I showed an hour ago -- the most recent set of PA models, which are not -- in our own terminology, they are not qualified and validated yet.

Level 2 scope -- take a so-called risk-informed approach to going beyond Level 1. Risk-informed in this sense means to us look at the impact of the work before you decide if you're going to do it, and, in particular, this might involve taking PA-based -- system-level, performance-based approaches to resolving KTI agreements rather than the literal full scope of work that was anticipated when we agreed to the agreement.

In other words, if we can show it doesn't matter, is that a sufficient way to address a question?

Level 3 -- and these are both optional. We went to the work package managers and said, "If you can close everything at Level 1, don't bother to define any higher levels for us." Level 3 was essentially the same as that plan A work scope that got us to 2006. And with respect to KTI agreements, it was the full and literal completion of all activities proposed.

Managers were -- these are the science managers, department managers, provided input on how well each proposed work scope meets the defined set of attribute scores with respect to the defining set of attributes -- better way of saying that.

And the -- I'm going to skip a couple of slides here and just go to slide 10, because the -- the same word that's on the intervening slide, where you've got this helpful equation here. Ignore the figures. They're not all that helpful.

But for each one of these 16 attributes, we asked questions of the technical staff, how likely is your work at this level -- scope of work -- how likely is it to, for example, increase confidence in your treatment of parameter uncertainty? How likely is it to result in an increase in total dose? And the technical staff provided answers that ranged from very unlikely to very likely.

We then asked a management team what types of -- what value they assigned to different types of answers. That's this V thing here. Actually, it's a relatively small player in the utility analysis. But since it's up there -- if someone said their work was likely to -- or, let's say, it was likely to result in a change in dose, we then asked the TSPA modelers, "How big a change might that produce?" These are all subjective answers, but at least we're asking the right experts.

And the TSPA modeler might say, "Oh, it could increase by a factor of greater than 10, or could have a small change of less than a factor of 10." We wanted to apply -- but this now is a management decision, what value do you apply to the different answers.

In this hypothetical example here, we gave that a weight of one and a weight of zero for a neutral effect, a small change in dose, and a weight of .15 for an increase in dose. But that's how the impact value function was used.

Then this weighting -- this is a

subjective management decision. How important did management think that question was? And these were elicited from the management team in the project.

If I go back to that three-dimensional -- you know, this figure -- if our technical staff were all-knowing, for any scope of work they actually could, in theory, define a vector in this N-dimensional space that defined where their work would put us. Would their work, you know, greatly increase qualitative defensibility? Would it greatly increase regulatory defensibility? And so on.

That would be the first term of that three-term sum that went into the utility analysis. The other two are management questions of, where does the project want to be in that -- in this N-dimensional space? If the project did not care about qualitative acceptability, external reviewer type issues, we could truncate the Z-axis and live entirely in the X/Y plane. It appears to be the bare minimum needed for licensing under Part 63.

But the project is not willing to accept that risk, and, you know, the TRB certainly understands that point. I met with them -- a subgroup of them, just a couple of days ago, and we talked about this. Obviously, we're not going to live only on the X/Y plane.

So where are our decisions headed? And that's the point of the -- listing the management weight. And for each of the 16 attributes, for each of the work scopes, you can define -- we define a likelihood that the answer will be what we -- what -- that the likelihood of a specified answer will occur. That comes from the technical staff, the scientists. And these values and weights comes from managers.

And then, for each work scope you create a spreadsheet and sum them up. And the -- you get a utility. It's a dimensionless number. It's associated with each work scope, and it's a quantitative -- and fully comparable from one work scope to the next -- measure of the utility of doing that piece of work, utility defined in respect to the questions we asked, what are the attributes, and the weights management put on those attributes.

Now, caveats that come out of this -- first of all, like any decision analysis exercise, the results of the model here means the utility model, utility analysis. It's a decision-aiding tool. The project had no intention of using it as a direct decisionmaker. Other -- that's the most important caveat. It's to inform managers who still have to make subjective human judgments.

The cost assumptions that went into it were not always consistent. Results -- we deliberately did not constrain them by schedule. We didn't force people to say, "You've got to make everything end by 2004." There were some differences among department perspectives and the impacts of their work, despite the workshop discussions. Surprisingly few, actually. That workshop was as close to a consensus as I've seen when people understood what it was we were doing.

It doesn't include all work scopes. It's just a -- for those who are looking for the piece of the management budget or design or testing the interface with design or the TSPA calculations themselves. We excluded those from the exercise.

Some questions didn't capture what we were after. We wrote some bum questions. That happens.

Utility rankings -- we presented utility-only rankings, and they're in the packet here, and also utility cost ratio rankings. Utility-only rankings ignore cost. Utility cost ratios are better. They're clearly what the tool was designed for. You're doing a -- we are doing a utility analysis because cost does matter. We don't have an unlimited budget.

But when you do a utility cost ratio, you discover pretty quickly that not all work costs the same. And very large work packages may perform more poorly in the evaluation, simply because they've got a big denominator -- cost -- where, obviously, if you're a utility people defined their, aggregated it coarsely, and produced expensive packages. You get a big denominator in that fraction.

The examples here in the packet are weights, management decision weights from two people -- Bob Andrews and myself. In fact, in the back, in the backup slides, there are different rankings provided with weights listed from other groups of people. And in the report that went with this, the people are identified by name. They were the BSC management team. We also listed some DOE managers but did not include their results. That would have been inappropriate.

What we discovered -- you can see when you get to the back -- is that actually the management weights, even though we had some -- quite a broad range of management types in the exercise, we were not as sensitive to the management weights as you might think.

And this last caveat here, that the -- deliberately, we didn't want to emphasize -- didn't want to focus only on things that show a positive benefit or a negative impact either way. Both of them are important. Obviously, we need to know if our work is going to show poor performance. That's -- we must know that. But we also want to value work that shows improved performance.

MEMBER GARRICK: I think we're going to have to wrap up in about five minutes.

MR. SWIFT: Okay. I'm there.

Just an example here, a couple of examples. Thank you, Bill. I apologize for this.

These were three different levels of work defined for engineered barrier system flow and transport. The way to read this figure -- the bars here are the amount of utility associated with that activity for each one of these 16 attributes over here.

And the first thing that -- we have the big blue band here -- resolution and closure of KTI issues. So the experts in the engineered barrier system department felt that if they did more work, the Level 2 work, they had a better likelihood of closing their KTI agreements. And the band is thicker here than it is here. But they were still -- I believe this was a likely answer, and that was very likely.

Interestingly, although these two work scopes are very different, they show an absolute -- this one costs $2 million more, takes two years longer, and in the space of these questions that we asked gives you the same answer. So that's an example where the management decision was pretty much a no-brainer. We looked at the one that gives you the same answer more cheaply.

CHAIRMAN HORNBERGER: Peter, you'll have to forgive Raymond and I. We have another meeting we have to go to.

VICE CHAIRMAN WYMER: I apologize.

MEMBER GARRICK: That's all right. We'll carry on here for a while.

MR. SWIFT: The work scopes are sorted by utility at the Level 2. This was the so-called risk-informed work scope, the intermediate work scope for each of these areas. Biosphere scored highest. This is simply because -- a number of reasons, but biosphere has a high likelihood of affecting the total 10,000-year dose.

And in the management weighting used to generate this figure, Bob Andrews and I both felt that anything that was not moved -- the 10,000-year total dose was the most important thing. We gave that a very high weighting, and the biosphere igneous activity was --

MEMBER GARRICK: Peter, let me comment on that. Of all the things on here, the biosphere is probably the most prescriptive. And so -- and one of the things that prescription does is very often eliminate a lot of decisionmaking and analysis, because this -- the biosphere -- the regulations on the biosphere are pretty binding in terms of how much flexibility you have in analysis and investigation. I'm surprised that that would end up on top.

MR. SWIFT: Well, this is a -- let me go to the next -- a different slide. This one is in the backups, and it's -- I'll come back to answering that question.

MEMBER GARRICK: Yes. Okay.

MR. SWIFT: This is incremental utility. This is --

MEMBER GARRICK: Yes, I understand that. Yes.

MR. SWIFT: -- how much more you're getting when you -- how much more utility you get when you go from Level 1 to Level 2. And biosphere has dropped well down the list here.

MEMBER GARRICK: Yes.

MR. SWIFT: The difference there is that our biosphere team felt that even at their lowest level of work they were likely to show an increase in the BDCFs. And that's what drove that --

MEMBER GARRICK: I see. Okay.

MR. SWIFT: And I actually -- this is all subjective. This is judging by work that is not yet done, and I tried to argue with Tony Smith about this one in particular. I don't think shifts are going up as much as he thought they were, but he's the technical department lead on that. And that's where his work fell out.

That was the -- did I just put up the incremental utility plot? I did. Okay. I'm looking for my summaries here.

MEMBER GARRICK: Are you going to tell us how this has affected the outcome of the decisions?

MR. SWIFT: Yes, if I can find the slide. Yes, that was my mistake. They're over here now. It's actually 22 and 23.

What did we do with this? We brought the spreadsheet as an electronic tool, and the types of rankings you see. We brought them to a BSC management workshop where we had the senior project manager of the BSC, not the corporate management, but Nancy Williams, who is the project manager, and her staff -- I could name who they are. She had a -- has an oversight -- project oversight review board. No, it's just called project oversight board, POB, that is people you're familiar with.

It would be Jack Bailey, John Beckman, Gene Yonker, Nancy Williams herself chairing it. Representatives from the national laboratories would be Roland Carson, Joe Farmer, Andrew Worrell, Sal Peterman from USGS, Tom Cotton, who is on it. These people met for a fairly intensive three-day meeting to go through the results of this spreadsheet, quite a lot of detail.

And then we put the final rankings up on the screen -- not final, we put the various versions of the rankings up on the screen. They've been up off and on for a long time, with the cost utility ratios displayed and total cost. And we started drawing lines in the budget where -- what can we afford? And that would be an example that would be -- slide 20, for example.

These are -- we do Level 1 scope for each of them. This is what it costs in FY '02, and then we start adding in more work at the -- from Levels 2 or 3 at the sort of -- the highest increments of utility cost ratios first. So the most bang for the buck principle here.

What did we discover when we started drawing the line in the budget? That there was not as much money available as we had happened, and we were able to come to the conclusion fairly quickly that, in fact, the Level 1 scope was where we were looking. The emphasis was going to be, based on the money available, on validating the models that were already available. These would be the models we showed earlier today.

However, we could not afford to move up to Level 2 and 3 across the board, so we started taking those work packages apart item by item. And the management team actually, in real time, went through all of the work package descriptions and brought things forward from Level 2 to Level 3 into the budget on subject human judgment bases.

A primary selection criteria of moving work forward was to avoid canceling any tests that were ongoing. One of the lessons learned from other projects is: don't cancel a test if you've already paid for startup costs. Go ahead and collect the data.

So this is just -- this is not all PSF tests. Some have PSF tests -- some tests are brought forward, and other examples of testing activities were brought forward.

And activities that were needed had to be accelerated to support documentation activities that could be done. Activities that were at the Level 1 scope of work but were planned to be done too late to support license application in '04 were accelerated, and that required bringing extra money forward.

Basically, this was a money management tool that they exercised about how you use your money wisely to manage your work.

And then this exercise with project management took place in late January, and early February we spent detailing work package descriptions that were then delivered to DOE on March 1st.

And I apologize for running over. I have a summary slide here. This was a decision -- the multi-attribute utility analysis was a decision-aiding tool, not a decisionmaking tool. We keep saying that because that last slide included a lot of human judgment.

You want both the technical and the

management input. The management weights are important. That's where we decide what it is -- where we want to be in that X/Y/Z space.

Consideration given to regulatory requirements, technical defensibility, and money. And, yes, we will have to reevaluate it as new information becomes available.

MEMBER GARRICK: Okay. Thank you.

Milt, do you have any comments?

MEMBER LEVENSON: No.

MEMBER GARRICK: Appreciate the presentations. I think one of the real problems when you get into this business of trying to come up with utility functions that contain preference functions is dealing with the different groups as you have, and addressing the biases that might exist in those groups, because all of us think that what we're doing is the most important. So there has to be some sort of normalization process.

But you said that this seemed to be -- there seemed to be a lot of harmony in this case. I'm surprised at that.

MR. SWIFT: There was. The trick I think was to focus people on giving fair questions -- fair answers to questions as we asked them. When -- if you simply say, "Is my work important?" ask somebody to answer that question, the answer would always be yes. But if you say, "Will this specific piece of work change a dose result?" or "Will this close KTI agreements? And, if so, please name them and explain how you're going to close them." At that level, people were quite objective, and they were willing to say, "No. Actually, this doesn't do anything to dose."

We had a broad enough set of questions --

MEMBER GARRICK: Were they thinking in the context of uncertainty when they answered the question? Because --

MR. SWIFT: Not as much as I had hoped they would be.

MEMBER GARRICK: Because the risk is really the uncertainty. And so if what they're saying is that it doesn't affect the central tendency parameter, that's one thing. But if it does affect the tails of the distribution, it could be very significant.

MR. SWIFT: With respect to the dose calculations, yes, they were -- they were thinking of that sort of thing. But they were all thrown for a curve -- thrown a curve right away by the realization that 10,000-year total dose is really a question of igneous activity.

And as soon as people realize that, you know, if you want to score on the Y-axis, the quantitative axis in that, you've got to have a volcano.

MEMBER GARRICK: Yes.

MR. SWIFT: A whole lot of people came into the room thinking they were going to say, absolutely, I've got some tail up there that's going to drive dose. Actually, no, you don't. You may score on time, 215 millirem. You may score on peak dose. You may score on the conditional early failure scenario. But those all get different weights.

And because we had a broad range of questions, I think every technical staff person was able to feel like, yes, there is a question that captures my -- their personal issues. But then, they didn't know how management was going to weight those questions, and so they were -- the technical level of agreement was surprisingly high. People always found a question they could say, "Yes, that's the one I'm aiming at."

MEMBER GARRICK: Any questions from the staff?

One thing I would say is that in the TSPA-SR you had a couple of appendices that did a very nice job of delineating the key assumptions, and then going out on a limb a little bit and indicating what the impact of these assumptions might be.

I don't know if it's the way you're doing it, but I think that it would be very helpful with respect to traceability and transparency for this to be kind of a reference point and subsequent versions be measured against this reference point. I think it would make it very clear what -- which assumptions have changed and what impact they've had, and which assumptions are being driven by the decision to go to an entirely different corrosion model, for example.

By just changing the model, you can end up with a different set of importance rankings for contributors. So I found that what you did in the SR-TSPA very valuable in boiling down just exactly what the team thinks is important from an assumption set standpoint.

And I hope that something like that is carried forward. I'm not saying you should do it, because we don't advise you; we advise the Commission. But that was -- I'm just observing that that was an example of a transparency tool or a traceability tool that was very helpful. And your presentations were very helpful, and we thank you very much.

And with that, unless Carol has --

MS. HANLON: Dr. Garrick, Bill has brought copies of his uncertainty analysis and strategy, as well as Peter's guidelines. So we're going to leave these here for you. If you need additional copies, let us know.

And I'd just like to thank Bill and Peter again for working around very difficult schedules, including technical exchanges and Peter's out of the country trek to be here today.

MEMBER GARRICK: We know they are very busy men, and we know you're a very busy lady. Thank you very much.

(Whereupon, at 12:48 p.m., the proceedings in the foregoing matter went off the record.)

CERTIFICATE

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

Name of Proceeding: ACNW 133rd Meeting

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.

Matthew Needham
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March 14, 2002

Dr. George M. Hornberger, Chairman
Advisory Committee on Nuclear Waste
U. S. Nuclear Regulatory Commission
Washington, D.C. 20555-0001

Dear Dr. Hornberger:

SUBJECT: LETTER FOR THE RECORD – NRC HLW RESEARCH: ASSURING NRC HLW DECISIONS ARE INFORMED BY SOUND SCIENCE - THE UNIQUE ROLE OF THE ACNW

My February 28, 2002 letter to the ACNW (Reference 1) transmitted a copy of a letter I sent to the NRC Commissioners on February 26, 2002 (Reference 2). Reference 2 presented concerns about the NRC HLW research strategy and its impact on the credibility and public acceptance of potential regulatory decisions made by the NRC for the proposed HLW repository at Yucca Mountain, Nevada (YM). In Reference 1 I requested that Reference 2 be included in the record at the next ACNW meeting, March 19, 2002. In addition, I request that this letter also be included in the record on March 19.

The purpose of this letter is to strongly emphasize the importance of the upcoming ACNW Annual Waste-Related Research Report and the unique role that the ACNW can play in helping to correct serious flaws in the current NRC HLW program strategy, which has overemphasized a compliance-based approach at the expense of underemphasizing research. Semantics and process appear to be substituting for the science and understanding required by the NMSS staff and NRC decision makers, which can only be obtained from a well planned, supporting research program with major involvement by RES. The transcript of the recent Commission briefing by NMSS on March 4, 2002 (Reference 3), in part, supports this conclusion.

As the only independent, oversight body to review the NRC HLW regulatory program, the ACNW can play a crucial role in altering the direction of the NRC HLW program from its current course and risk of likely failure, as the proposed Y M project proceeds towards the licensing stage. **I request that the following points receive special consideration and emphasis in the preparation of the final Annual Waste-Related Research Report, and at the ACNW meeting with the Commission, March 20, 2002.** Many of these points are not new, but merit inclusion as a frame-of-reference for this discussion.

1. As a starting point for the discussion, I fully understand that the DOE is responsible for providing the necessary technical and scientific bases for demonstrating the safety of the proposed YM repository. This guideline has also been true for reactor safety licensees and other areas of the nuclear industry regulated by the NRC.

2. Nevertheless, the NRC (RES) was also given the responsibility, in the 1974 ERA, to conduct and maintain a sufficient, independent confirmatory research program of its own. This mandate has allowed RES to provide the NRC licensing staff and senior decision makers with the necessary tools, data and scientific understanding to properly judge the work of licensees, and support decisions rendered by the staff. As a result, the NRC has been able to successfully address key issue challenges over the past 25 years, such as: ECCS performance and thermal-hydraulic code validation; severe accident source term and risk reassessment in response to the accident atTMI-2; pressurized thermal shock; and reactor license renewal.

3. **Is the ACNW confident that the NRC and CNWRA technical staff have sufficient independent supporting tools and data, and resources, to allow them to gain sufficient understanding of repository science and the associated complex issues they need, to do the job being asked of them?**

4. Is the ACNW confident that NMSS has a formal strategy, which integrates: YM key technical issues (KTIs); performance assessment methodology and its associated uncertainties and their PA sensitivities; data and information from the DOE YM program needed to 'close' the 293 'open' KTI 'resolution' agreements, which taken together provide much of the bases for NRC independent research and technical assistance to support the decisions the NRC will be confronted with over the next 3-5 yr? The next 5-10 yr? Such a strategy would also be useful for prioritization of research as well as technical assistance, and hence resource allocation. It is also interesting to note that the draft NRC YM license review plan posted on the NRC web site, is silent on the role of NRC research!

5. In their 2001 Annual Research Report (Reference 4), the ACNW recommended, "The HLW program needs to be expanded to have a modest long-term, 'anticipatory' research component, perhaps through collaboration between NMSS and RES." **What was the basis for the ACNW's judgment as to the size of the anticipatory research program? How can the ACNW understand the research needs of the HLW program without information of the type noted in Item 4? How can the ACNW call for an RES research plan, which includes HLW components, and a prioritization protocol, without an integrated plan and road map of the type described in Item 4?**

6. In Reference 4 the ACNW also noted, "Another aspect of the partitioning of HLW and non-HLW issues is the potential for ignoring anticipatory research needs in the HLW area. NMSS focuses on the relatively short-term goal of analyzing what DOE is doing. RES, on the other hand is prohibited from doing any work on HLW, even if it is anticipatory and arguably focused on the long term. There is a potential gap in the NRC program because of the separation of the NMSS and RES programs."

7. The ACNW, perhaps because of the current narrow, and shortsighted NRC HLW policy restrictions imposed on the NRC staff and the Committee, continues to address the NRC HLW research program as two distinct entities, confirmatory and anticipatory. This categorization is misleading. In the real world of nuclear regulatory research, especially with a first-of-a-kind geological repository and its complex technical and scientific uncertainties and challenges, research programs are most appropriately a continuum of closely integrated studies. **I believe the Committee needs to break out of the constraints imposed upon it and send a clear and unmistakable message to the Commission on the gravity of the current deficiencies in the NRC HLW research program, sooner, rather than later. How much research does the ACNW, in their legislatively mandated, expert advisory capacity, truly believe is sufficient to support the NMSS licensing staff's review?**

8. Several presentations at the ACNW Research Needs Workshop in November 2001(Reference 5) provided perspectives and advice, which support some of the issues and concerns discussed above.

The ACNW should give high priority, proactive attention to the issues presented here, many of which the Committee has touched upon in their recent letters to the Commission. The Committee must recognize that if the proposed YM HLW repository proceeds to the licensing phase, the ACNW will be called upon to review, and either reject or endorse, the findings of NRC decisions makers on the matter before it. **At that time, the ACNW will have at its disposal the same information resources available to the NRC staff, no more, no less, to render its crucial judgment**. The ACNW will have to evaluate and judge the acceptability and quality of the NRC staff review, as well as the adequacy of the science-informed rationale and treatment of uncertainties used by the staff to measure compliance and the margins for assuring safety. **Hence, the ACNW must rest assured that the NMSS staff and the Committee have sufficient science-informed resources at its disposal to render an adequate review. The NRC advisory committees are the guardians of sound science-based regulatory decisions.**

If the ACNW has any doubts or reservations about the current direction of the NRC HLW research program now is the time to express it, in the annual research report, and in the meeting on March 20, 2002 with the Commission. If not now, when? I am sure the ACNW has a clear vision of the needs of the NRC in the HLW arena. The Committee must take the necessary steps to see that vision come to fruition. If not the ACNW, who else?

Thank you for the opportunity to comment on NRC HLW research. Please let me know if I can be of further assistance to the Committee.

Sincerely,

(signature)
Mel Silberberg

cc: NRC Chairman and Commissioners

REFERENCES

1. Letter from Mel Silberberg to Dr. George M. Hornberger, Chairman, ACNW, dated February 28, 2002.

2. Letter from Mel Silberberg to The NRC Commissioners, dated February 26, 2002.

3. USNRC, Transcript of Commission Meeting on March 4, 2002, Rockville, MD.

4. Letter from Advisory Committee on Nuclear Waste to the Honorable Richard A. Meserve, Chairman, USNRC, dated February 5, 2001.

5. Advisory Committee on Nuclear Waste, Transcript of Meeting – Workshop on Research Needs, November 27-28, 2001.

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February 26, 2002

The Honorable Richard A. Meserve
Chairman
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Nils J.Diaz
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Greta J. Dicus
Commissioner
U. S. Nuclear Regulatory Commission
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The Honorable Edward McGaffigan, Jr.
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Jeffrey S. Merrifield
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

Dear Chairman Meserve and Commissioners Diaz, Dicus, McGaffigan, and Merrifield:

SUBJECT: RESTORING NRC HLW RESEARCH: IT'S ALL ABOUT CREDIBLITY, BUILDING PUBLIC CONFIDENCE AND SCIENCE-INFORMED DECISIONS

BACKGROUND, STATUS, AND SUMMARY OF CONCERNS

1. On January 16, 2001 I sent a letter to Chairman Meserve regarding my concerns about the flawed strategy and scope of the NRC HLW research program (Ref. 1).

2. I received a response to Reference 1 in a May 7, 2001 letter (Ref. 2) from Mr. Martin Virgilio restating the staff position on the current NRC HLW program strategy, judging it

effective and efficient, so that NRC is well positioned to support an independent review of any potential license application for a potential repository. I will address this assertion later.

3. Since May 7, 2001 the results of several relevant, timely, and substantive reviews by boards and special panels have been published. I have also found additional, related information in searching various agency documents.

4. Many of the conclusions and recommendations contained in these reviews provide convincing support for the need for change expressed in Reference 1. They not only serve to confirm and reinforce the concerns I outlined in Reference 1, but also more clearly reveal the program deficiencies. **Overall, these recommendations were sufficiently compelling for the Commission to take action,** yet another year has passed, without visible changes to the program. **Despite specific recommendations from these reviews, including several relevant Commission meetings in 2001, the Commission was silent, not even issuing a Staff Requirements Memorandum dealing with this policy issue.**

5. There is a perceptible absence of a viable NRC HLW research program, regardless of the semantics used by the NRC staff to categorize the research as confirmatory or anticipatory. **Even more alarming is the continuing lack of a viable role for the fundamentally important and valuable research arm of the NRC, the Office of Research (RES), in the regulatory process for a proposed HLW repository at Yucca Mountain, Nevada.** The passing of another year has only magnified these concerns.

6. **It is difficult to imagine how a program strategy that doesn't take advantage of all of the technical and scientific staff resources available in the NRC can be deemed effective and efficient.**

7. **If one summarizes the abovementioned concerns and the discussion to follow, I do not agree with the conclusion in Reference 2, that the NRC is well positioned to support an independent review of any potential license application for a potential repository.**

Critical decisions required to implement needed change to a flawed research strategy for the NRC HLW program are of such magnitude and importance to the credibility of the NRC HLW regulatory program, they can no longer be left to the NRC staff. **Based upon the record to date, I do not believe the senior management in NMSS and the EDO can be expected to render an objective judgment in this matter. In addition, the current NRC organizational structure no longer gives the RES Director an independent voice in waste-related matters, as was the case prior to ~ 1996, therefore, it is necessary to bring this urgent need for change involving NRC HLW research, directly to the attention of the entire Commission as a vital policy matter.**

In the following sections I will discuss further the rationale supporting the concerns and conclusions reached above.

INFORMING, BUILDING PUBLIC CONFIDENCE WITH CREDIBLE DECISIONS ASSURED BY SOUND SCIENCE: A MANDATE FOR THE NRC RESEARCH OFFICE

The institutional process driving a decision on the siting, licensing and operation of a proposed, potential geological repository at Yucca Mountain, Nevada, (YM) involves technical, scientific, and political issues. At the end of the day, if you can't build a consensus base in the scientific community for repository science, you will not be able to gain the confidence of the public. For example, recent concerns about the state of DOE scientific investigations (Ref. 3 and 4) can have serious implications for building public confidence in the potential licensing process for a possible repository. Since the current divisions over YM science will continue to exist for some time into the foreseeable future, we should not expect the licensing review to be a pro-forma process of compliance. **Regardless of the outcome of the NRC review of the license for a potential repository, the agency will have to defend the scientific basis for its decisions to all stakeholders, as well as to the world court of scientific opinion.** This issue is addressed later.

Over the course of NRC history (e.g. reactor system thermal-hydraulic performance, severe accident risk and source term, nuclear plant aging and pressurized thermal shock, to name a few) the public and other stakeholders have gained confidence in NRC decisions involving controversial, complex technical and scientific issues because of the coordinated support of a robust, independent research program carried out by the NRC, within RES. The scope and depth of the NRC research programs were of sufficient magnitude that NRC was able to independently defend its decisions based upon sound science, while assuring public health and safety. NRC was in effect the last word, literally and figuratively. The need for an adequate NRC HLW research program is not exempt from the tested lessons and wisdom derived from NRC's past. **NMSS regards the NRC HLW regulatory program as risk-informed, effective and efficient, but at the end of the day, success and credibility requires a comprehensive research effort, one that has not been evident in recent years.**

NRC HLW RESEARCH: A PRIORITY NEED FOR A PROGRAM STRATEGY

The NRC HLW research program was ostensibly terminated in 1996 owing to a severe reduction in funding for the NRC HLW program. Although funding for the NRC HLW program was restored in 1998, the NRC HLW research program per se was not restored. The chronology of these program decisions was discussed previously in Reference 1. **As a member of the public I have not found on the public record, the existence of a bona fide NRC HLW research program or strategy to support and confirm technical and scientific issues over the entire regulatory process.** The NRC Performance and Strategic Plans for the Nuclear Waste Safety Arena are silent about the role of research in the HLW program or program strategy. **By comparison, the role of research to provide the technical basis to confirm the adequacy of regulations and guidance to maintain safety in areas such as decommissioning and interim spent fuel storage are evident. How can the agency justify and defend such an incongruent policy in the application of research in the interest of safety and public confidence?**

Since 1996 the NRC HLW program at the CNWRA has been referred to sometimes (e.g. ACNW) as technical assistance, a part of which is considered to be 'research.' In their 2000 ACNW Performance Plan under Second-Tier Priorities – Research, reference is made to "technical assistance performed by the CNWRA. During presentations before the ACNW, NMSS and contractor staff noted that no research was actually being conducted under the NRC HLW program (Ref. 5 and 6). Nevertheless, in Reference 2, Mr.Virgilio states " Although some of the CNWRA's technical work might be deemed 'confirmatory research' by some, I do not believe the assignment of some research responsibilities to NMSS violates any prohibition in the ERA. The reason is that, under the ERA, the Commission has wide discretion in assigning work among its statutory offices." The first point about this assertion is that I cannot find evidence in the ERA to support it. **The second point is that if NMSS staff really believes it was given such a mandate by the ERA, why haven't they come forth, on the public record, with an integrated, confirmatory research plan and strategy to support even the current phases of their pre-licensing review.**

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Since 1996 there has been little, if any, discernable participation by RES experts in the NRC HLW program. For example, one of the key technical and scientific challenges in HLW is the question of the corrosion of the Alloy 22 waste package material proposed by DOE. RES had on its staff internationally recognized expertise in materials corrosion science as one of its core research capabilities applicable to nuclear waste safety. NMSS, carrying out an ostensible NRC policy, failed to take advantage of this staff capability in RES. There are other examples of this deficiency. **The internal culture displayed in this case tends to promote a 'closed program,' indicative of a program operating 'inside the box.'**

INFORMING NRC HLW PROGRAM STRATEGY: ADVICE OF EXPERT PANELS AND REVIEW BOARDS SPEAK VOLUMES ON THE NEED FOR CHANGE

In its Annual Report on NRC Waste-Related Research (Ref. 8) the ACNW noted: ..."Another aspect of partitioning the HLW and non-HLW issues is the potential for ignoring anticipatory needs in the HLW area. NMSS focuses on the relatively short-term goal of analyzing what the DOE is doing. RES, on the other hand, is prohibited from doing any work on HLW even if it is anticipatory and arguably focused on the long term. There is a potential for a gap in the NRC

Program because of the separation of the NMSS and RES programs."

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A statement in Reference 7 sums up the concerns and advice presented above: "Several panel members felt that regardless of the work being done by NMSS in evaluating the ability to license waste management programs, special research skills are required to review that work and verify its credibility. **Decisions regarding the ultimate safety of the Yucca Mountain Project, for example, will be carefully scrutinized by stakeholders and solid research data must be available to support the decisions made by the Commission." (Emphasis added)**

REFLECTIONS ON NRC HLW CONFIRMATORY RESEARCH: GETTING OUTSIDE OF THE BOX

In a number of the reviews referenced above, many of the comments point to the need for anticipatory or long-term research in the NRC HLW program. The anticipatory, or so-called long-term, research needed has been referred to as a 'small' or 'modest' program. No basis is offered for the qualitative nature of these judgments and their meaning. The current situation regarding what HLW research is actually being done by the NRC is confusing and troubling. If the elusive scope and magnitude of the NRC research program is not clear or definitive, how can one start to define or bound the anticipatory research program? At its outset, the anticipatory program is a derivative of the confirmatory program, and for many years, both programs should be complementary. The first question that needs to be answered is: How much of the confirmatory research needed to credibly review a possible licensing application for a potential repository been completed or even identified? I believe the short answer to this question is we don't know. The NRC needs to be sure it is doing the research needed to support a credible licensing review now, before it can initiate a meaningful anticipatory program. The reasons for this answer follow.

The NRC has listed nine key technical issues (KTI) for their review of the YM project. There are many additional sub-issues related to these KTIs. Some of these issues would be influenced more by the assumption of a high-temperature repository design. The NRC is using a repository performance assessment (PA) code to project repository performance for thousands of years into the future. Large uncertainties exist in the processes and parameters for many of the models in the code, as well as the models themselves, and are closely related to many of the KTIs. Some NRC research related to these uncertainties is needed now just to understand and confirm the DOE PA code, its models and parameters, for the licensing review, and for many years to come. Such research is also essential to gain understanding and build confidence in the use of the NRC PA mode. **Risk-informed PA can be an important tool for HLW regulatory decisions, but unless the PA model is also sufficiently science-informed; it is much less useful and credible.**

Recent reports of the Nuclear Waste Technical Review Board (NWTRB) appear to be consistent with these views. In Reference 10 the NWTRB commented on the DOE performance assessment model and its related uncertainties: "The DOE uses a complex integrated performance assessment model to project repository performance. Performance assessment is a useful tool because it assesses how well the repository system as a whole, not just the site or the engineered components, might perform. However, gaps in data and basic understanding cause important uncertainties in the concepts and assumptions on which the DOE's performance estimates are now based. Because of these uncertainties, the Board has limited confidence in current performance estimates generated by the DOE's performance assessment model. . . . . . An international consensus is emerging that a fundamental understanding of the potential behavior of a proposed repository system is of importance comparable to the importance of showing compliance with regulations. The Board agrees that such fundamental understanding is important."

This evaluation by the NWTRB, albeit directed towards the DOE, also frames the technical challenge faced by the NRC, with its current HLW program strategy, which appears to be heavily weighted towards compliance-based review, as opposed to a more balanced strategy involving more fundamental understanding of repository science. This view is also consistent with recommendations by the BRWM committee in Reference 9.

CONCLUSIONS AND RECOMMENDATIONS FOR COMMISSION CONSIDERATION

Ample arguments, supported by a preponderance of testimonials by recognized experts and former regulatory decision-makers, have been presented for the Commission's policy consideration in the matter of NRC HLW program strategy and the urgent need to fill the current void in the science-informing role normally derived from NRC research. The current NRC HLW strategy is not sufficiently pro-active in the area of confirmatory research, lacks a role for traditional RES support, and appears to rely excessively on a compliance-based approach.

There is still time for the Commission to make needed changes in the HLW program strategy. The age-old adage about not 'changing horses in mid-stream' is not a sound or prudent justification. The case for change presented in this letter indicates that the 'horse may not make it across the stream.' The potential risk of delaying needed changes to the program increases with time with the consequence of a real potential for the NRC becoming a major obstacle to moving forward with HLW disposal. **This concern is not only about who manages the NRC HLW research program, expressed in Reference 1. It's also about serious concerns with the inadequacies of the research program strategy and its impact on the very credibility of the regulatory program.**

This Commission can leave an important policy legacy for the future of the NRC HLW regulatory program or it can defer the needed change to those who follow. Assuming the Congress decides to move forward with the YM Project on the current schedule, or with a likely scenario which delays the project several years (similar to the GAO finding) to obtain more data, it is incumbent upon the Commission to inform the Congress that the NRC finds it prudent to revise its HLW program strategy, and hence its budget request from the Nuclear Waste Fund, based upon the weight of considerable, expert advice from various panels and committees.

One thing is certain. The earlier needed program changes are made, the easier they are to implement, and with a lower potential risk for a regulatory impasse to HLW disposal. If the Commission decides to take no further action and if for some reason the NRC HLW strategy is called into question in the future, a response, which in effect says, "we didn't have sufficient resources to do the job" will be unacceptable.

During a speech in 2001 Commissioner Merrifield used a quotation from Nathaniel Hawthorne, which is appropriate for the current discussion: *"Destiny is not a matter chance: it's a matter of choice. It is not a thing to be waited for, it is a thing to be achieved."*

I trust the Commission will accept these comments with the same constructive and collegial spirit in which they are offered. If I can be of further assistance please do not hesitate to call on me. When appropriate, I would be pleased to appear before the Commission on the matters presented in this letter, as an informed stakeholder with extensive expertise in nuclear regulatory research for reactor safety and nuclear waste safety.

Sincerely,

Mel Silberberg

Mel Silberberg

cc:
ACNW
NWTRB
DOE/OCRWM
NWPO

REFERENCES

1. Letter from Mel Silberberg toThe Honorable Richard A. Meserve, Chairman, USNRC, dated January 16, 2001.

2. Letter from Martin Virgilio, Director, NMSS to Mel Silberberg, dated May 7, 2001.

3. GAO Report-02-191, *"Nuclear Waste: Technical, Schedule, and Cost Uncertainties of the Yucca Mountain Repository Project,"* December 2001.

4. Advisory Committee on Nuclear Waste, Transcript of Meeting, Nov. 29, 2001.

5. Advisory Committee on Nuclear Waste, Transcript of Meeting, October 22, 1997.

6. Advisory Committee on Nuclear Waste, Transcript of Meeting, Workshop on Research Needs, November 28, 2001

7. USNRC, Office of Nuclear Regulatory Research, *"Role and Direction of Nuclear Regulatory Research - Expert Panel Report,"* May 2001.

8. Letter from Advisory Committee on Nuclear Waste to The Honorable Richard A. Meserve, Chairman, USNRC, dated February 5, 2001.

9. National Research Council, Board on Radioactive Waste Management, *"Disposition of High-Level Waste and Spent Nuclear Fuel: The Continuing Societal and Technical Challenges,"* June 2001, National Academy Press.

10. Letter from the U. S. Nuclear Waste Technical Review Board to The Honorable Dennis Hastert, The Honorable Robert C. Byrd, The Honorable Spencer Abraham, dated January 24, 2002.

In the interest of time and space, selected passages were chosen from some of these References because of the importance of the comment, view, or discussion to the specific issue being emphasized in this letter. The reading of the full-text of the reference is also recommended.

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February 28, 2002

Dr. George Hornberger, Chairman
Advisory Committee on Nuclear Waste
U. S. Nuclear Regulatory Commission
Washington, D.C. 20555-0001

Dear Dr. Hornberger:

SUBJECT: STATEMENT FOR THE RECORD – NRC HLW RESEARCH

Enclosed for your information and use in your consideration of the ACNW Annual Waste-Related Research Report to the Commission is a letter I have sent to the Commissioners on NRC HLW Research. I request that it be entered into the record at the next ACNW meeting, March 19, 2002, with my November 23, 2001letter to the ACNW, which does not appear to have been placed into the record for ACNW Research Needs Workshop, November 27-29, 2001.

Thank you for the opportunity to comment on NRC programs within the scope of review of the ACNW. Please do not hesitate to call on me if I can be of any assistance to the Committee.

Sincerely,

Mel Silberberg

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February 26, 2002

The Honorable Richard A. Meserve
Chairman
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Nils J.Diaz
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Greta J. Dicus
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Edward McGaffigan, Jr.
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

The Honorable Jeffrey S. Merrifield
Commissioner
U. S. Nuclear Regulatory Commission
Washington, DC 20555-0001

Dear Chairman Meserve and Commissioners Diaz, Dicus, McGaffigan, and Merrifield:

SUBJECT: RESTORING NRC HLW RESEARCH: IT'S ALL ABOUT CREDIBLITY, BUILDING PUBLIC CONFIDENCE AND SCIENCE-INFORMED DECISIONS

BACKGROUND, STATUS, AND SUMMARY OF CONCERNS

1. On January 16, 2001 I sent a letter to Chairman Meserve regarding my concerns about the flawed strategy and scope of the NRC HLW research program (Ref. 1).

2. I received a response to Reference 1 in a May 7, 2001 letter (Ref. 2) from Mr. Martin Virgilio restating the staff position on the current NRC HLW program strategy, judging it

effective and efficient, so that NRC is well positioned to support an independent review of any potential license application for a potential repository. I will address this assertion later.

3. Since May 7, 2001 the results of several relevant, timely, and substantive reviews by boards and special panels have been published. I have also found additional, related information in searching various agency documents.

4. Many of the conclusions and recommendations contained in these reviews provide convincing support for the need for change expressed in Reference 1. They not only serve to confirm and reinforce the concerns I outlined in Reference 1, but also more clearly reveal the program deficiencies. **Overall, these recommendations were sufficiently compelling for the Commission to take action,** yet another year has passed, without visible changes to the program. **Despite specific recommendations from these reviews, including several relevant Commission meetings in 2001, the Commission was silent, not even issuing a Staff Requirements Memorandum dealing with this policy issue.**

5. There is a perceptible absence of a viable NRC HLW research program, regardless of the semantics used by the NRC staff to categorize the research as confirmatory or anticipatory. **Even more alarming is the continuing lack of a viable role for the fundamentally important and valuable research arm of the NRC, the Office of Research (RES), in the regulatory process for a proposed HLW repository at Yucca Mountain, Nevada.** The passing of another year has only magnified these concerns.

6. **It is difficult to imagine how a program strategy that doesn't take advantage of all of the technical and scientific staff resources available in the NRC can be deemed effective and efficient.**

7. **If one summarizes the abovementioned concerns and the discussion to follow, I do not agree with the conclusion in Reference 2, that the NRC is well positioned to support an independent review of any potential license application for a potential repository.**

Critical decisions required to implement needed change to a flawed research strategy for the NRC HLW program are of such magnitude and importance to the credibility of the NRC HLW regulatory program, they can no longer be left to the NRC staff. **Based upon the record to date, I do not believe the senior management in NMSS and the EDO can be expected to render an objective judgment in this matter. In addition, the current NRC organizational structure no longer gives the RES Director an independent voice in waste-related matters, as was the case prior to ~ 1996, therefore, it is necessary to bring this urgent need for change involving NRC HLW research, directly to the attention of the entire Commission as a vital policy matter.**

In the following sections I will discuss further the rationale supporting the concerns and conclusions reached above.

INFORMING, BUILDING PUBLIC CONFIDENCE WITH CREDIBLE DECISIONS ASSURED BY SOUND SCIENCE: A MANDATE FOR THE NRC RESEARCH OFFICE

The institutional process driving a decision on the siting, licensing and operation of a proposed, potential geological repository at Yucca Mountain, Nevada, (YM) involves technical, scientific, and political issues. At the end of the day, if you can't build a consensus base in the scientific community for repository science, you will not be able to gain the confidence of the public. For example, recent concerns about the state of DOE scientific investigations (Ref. 3 and 4) can have serious implications for building public confidence in the potential licensing process for a possible repository. Since the current divisions over YM science will continue to exist for some time into the foreseeable future, we should not expect the licensing review to be a pro-forma process of compliance. **Regardless of the outcome of the NRC review of the license for a potential repository, the agency will have to defend the scientific basis for its decisions to all stakeholders, as well as to the world court of scientific opinion.** This issue is addressed later.

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REFLECTIONS ON NRC HLW CONFIRMATORY RESEARCH: GETTING OUTSIDE OF THE BOX

In a number of the reviews referenced above, many of the comments point to the need for anticipatory or long-term research in the NRC HLW program. The anticipatory, or so-called long-term, research needed has been referred to as a 'small' or 'modest' program. No basis is offered for the qualitative nature of these judgments and their meaning. The current situation regarding what HLW research is actually being done by the NRC is confusing and troubling. If the elusive scope and magnitude of the NRC research program is not clear or definitive, how can one start to define or bound the anticipatory research program? At its outset, the anticipatory program is a derivative of the confirmatory program, and for many years, both programs should be complementary. The first question that needs to be answered is: How much of the confirmatory research needed to credibly review a possible licensing application for a potential repository been completed or even identified? I believe the short answer to this question is we don't know. The NRC needs to be sure it is doing the research needed to support a credible licensing review now, before it can initiate a meaningful anticipatory program. The reasons for this answer follow.

The NRC has listed nine key technical issues (KTI) for their review of the YM project. There are many additional sub-issues related to these KTIs. Some of these issues would be influenced more by the assumption of a high-temperature repository design. The NRC is using a repository performance assessment (PA) code to project repository performance for thousands of years into the future. Large uncertainties exist in the processes and parameters for many of the models in the code, as well as the models themselves, and are closely related to many of the KTIs. Some NRC research related to these uncertainties is needed now just to understand and confirm the DOE PA code, its models and parameters, for the licensing review, and for many years to come. Such research is also essential to gain understanding and build confidence in the use of the NRC PA mode. **Risk-informed PA can be an important tool for HLW regulatory decisions, but unless the PA model is also sufficiently science-informed; it is much less useful and credible.**

Recent reports of the Nuclear Waste Technical Review Board (NWTRB) appear to be consistent with these views. In Reference 10 the NWTRB commented on the DOE performance assessment model and its related uncertainties: "The DOE uses a complex integrated performance assessment model to project repository performance. Performance assessment is a useful tool because it assesses how well the repository system as a whole, not just the site or the engineered components, might perform. However, gaps in data and basic understanding cause important uncertainties in the concepts and assumptions on which the DOE's performance estimates are now based. Because of these uncertainties, the Board has limited confidence in current performance estimates generated by the DOE's performance assessment model. . . . .. An international consensus is emerging that a fundamental understanding of the potential behavior of a proposed repository system is of importance comparable to the importance of showing compliance with regulations. The Board agrees that such fundamental understanding is important."

This evaluation by the NWTRB, albeit directed towards the DOE, also frames the technical challenge faced by the NRC, with its current HLW program strategy, which appears to be heavily weighted towards compliance-based review, as opposed to a more balanced strategy involving more fundamental understanding of repository science. This view is also consistent with recommendations by the BRWM committee in Reference 9.

CONCLUSIONS AND RECOMMENDATIONS FOR COMMISSION CONSIDERATION

Ample arguments, supported by a preponderance of testimonials by recognized experts and former regulatory decision-makers, have been presented for the Commission's policy consideration in the matter of NRC HLW program strategy and the urgent need to fill the current void in the science-informing role normally derived from NRC research. The current NRC HLW strategy is not sufficiently pro-active in the area of confirmatory research, lacks a role for traditional RES support, and appears to rely excessively on a compliance-based approach.

There is still time for the Commission to make needed changes in the HLW program strategy. The age-old adage about not 'changing horses in mid-stream' is not a sound or prudent justification. The case for change presented in this letter indicates that the 'horse may not make it across the stream.' The potential risk of delaying needed changes to the program increases with time with the consequence of a real potential for the NRC becoming a major obstacle to moving forward with HLW disposal. **This concern is not only about who manages the NRC HLW research program, expressed in Reference 1. It's also about serious concerns with the inadequacies of the research program strategy and its impact on the very credibility of the regulatory program.**

This Commission can leave an important policy legacy for the future of the NRC HLW regulatory program or it can defer the needed change to those who follow. Assuming the Congress decides to move forward with the YM Project on the current schedule, or with a likely scenario which delays the project several years (similar to the GAO finding) to obtain more data, it is incumbent upon the Commission to inform the Congress that the NRC finds it prudent to revise its HLW program strategy, and hence its budget request from the Nuclear Waste Fund, based upon the weight of considerable, expert advice from various panels and committees.

One thing is certain. The earlier needed program changes are made, the easier they are to implement, and with a lower potential risk for a regulatory impasse to HLW disposal. If the Commission decides to take no further action and if for some reason the NRC HLW strategy is called into question in the future, a response, which in effect says, "we didn't have sufficient resources to do the job" will be unacceptable.

During a speech in 2001 Commissioner Merrifield used a quotation from Nathaniel Hawthorne, which is appropriate for the current discussion: *"Destiny is not a matter chance; it's a matter of choice. It is not a thing to be waited for, it is a thing to be achieved."*

I trust the Commission will accept these comments with the same constructive and collegial spirit in which they are offered. If I can be of further assistance please do not hesitate to call on me. When appropriate, I would be pleased to appear before the Commission on the matters presented in this letter, as an informed stakeholder with extensive expertise in nuclear regulatory research for reactor safety and nuclear waste safety.

Sincerely,

Mel Silberberg

cc:
ACNW
NWTRB
DOE/OCRWM
NWPO

REFERENCES

1. Letter from Mel Silberberg toThe Honorable Richard A. Meserve, Chairman, USNRC, dated January 16, 2001.

2. Letter from Martin Virgilio, Director, NMSS to Mel Silberberg, dated May 7, 2001.

3. GAO Report-02-191, *"Nuclear Waste: Technical, Schedule, and Cost Uncertainties of the Yucca Mountain Repository Project,"* December 2001.

4. Advisory Committee on Nuclear Waste, Transcript of Meeting, Nov. 29, 2001.

5. Advisory Committee on Nuclear Waste, Transcript of Meeting, October 22, 1997.

6. Advisory Committee on Nuclear Waste, Transcript of Meeting, Workshop on Research Needs, November 28, 2001

7. USNRC, Office of Nuclear Regulatory Research, *"Role and Direction of Nuclear Regulatory Research – Expert Panel Report,"* May 2001.

8. Letter from Advisory Committee on Nuclear Waste to The Honorable Richard A. Meserve, Chairman, USNRC, dated February 5, 2001.

9. National Research Council, Board on Radioactive Waste Management, *"Disposition of High-Level Waste and Spent Nuclear Fuel: The Continuing Societal and Technical Challenges,"* June 2001, National Academy Press.

10. Letter from the U. S. Nuclear Waste Technical Review Board to The Honorable Dennis Hastert, The Honorable Robert C. Byrd, The Honorable Spencer Abraham, dated January 24, 2002.

In the interest of time and space, selected passages were chosen from some of these References because of the importance of the comment, view, or discussion to the specific issue being emphasized in this letter. The reading of the full-text of the reference is also recommended.