UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

DEVELOPING MULTIPLE LINES OF EVIDENCE

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PAGE NO.

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

8:30 a.m.

CRAIG: Well, good morning, everyone. I'm Paul Craig, a member of the Board, Nuclear Waste Technical Review Board, and I'm very happy to welcome you all here for a meeting which is in a somewhat new format for the Board. This is a meeting which is held in response to concerns by the Department of Energy for more interactions with the Board, and we thought this was a good idea. I'm going to begin by reading our standard opening statement, and then make some introductory remarks and tell you how the day is going to work.

As you may know, the Congress enacted the Nuclear Waste Policy Act in 1982. That Act, among other things, created the Office of Civilian Radioactive Waste Management, or OCRWM, within DOE, and charged it, in part, with developing repositories for the final disposal of the nation's spent nuclear fuel and high level radioactive waste from reprocessing.

Five years later, in 1987, Congress amended the law to focus OCRWM's activities on the characterization of a single candidate site for final disposal, Yucca Mountain on the western edge of the Nevada Test Site. I'm assuming everybody here knows where that is.

In the same amendments in 1987, Congress created the Nuclear Waste Technical Review Board, this Board, as an independent federal agency for reviewing the technical and scientific validity of OCRWM's activities. The Board does not manage the Yucca Mountain project. The Board is not a part of the Department of Energy. The Board does not have approval authority. The Board does not issue licenses, as NRC does.

What impact the Board has is through its independent evaluation of the Department of Energy's work, as conveyed through reports to Congress and to the Secretary of the Department of Energy, which we issue periodically, and which we are required by the law that created us.

Those of you who have attended our meetings before know that the members of the Board do not hesitate to speak their minds. And let me emphasize that that's precisely what they're doing when they are speaking. They're speaking their minds. They are not speaking on behalf of the Board per se. They're speaking on behalf of themselves. When we are articulating a Board position, we'll let you know. We will make it very, very clear. Otherwise, we're speaking as individuals. So, please bear that in mind. We will be speaking as individuals today.

Now, the structure of the meeting for today is that we have a lot of people with a lot of ideas and a limited amount of time. The morning session, we're going to have some individual statements, and then go around the table. We're going to take a break sharply at 11:30 for one hour for lunch so that people can get to the restaurants before they fill up. We're going to reconvene sharply at 12:30.

We're going to take public comments either before lunch or after lunch, and again at the end of the day insofar as time permits. We are going to encourage any member in the audience who has a question they would like to put at any time to write it down and hand it to either one of the Lindas who are out in front, who will deliver it to me, and I will make sure that the concern of that question gets entered into the discussion.

We are not--important point--we are not going to take coffee breaks. This is a very informal meeting by design, and if you feel you need coffee, or for other reasons, please simply get up and walk out and come back, and that applies to people at this table, as well as those in the audience.

Okay, I think that handles the mechanics. We now get on to the question of what the meeting is all about. And what the meeting is all about is multiple lines of evidence. For several years, the Board has been recommending that the

Department of Energy develop multiple lines of evidence, and we've made a number of comments. All of our official pronouncements are on our web site, www.nwtrb.gov. For example, April '97 comments on provisions to 10 CFR 960, or the DOE site suitability regulations, April '99 report on the viability assessment, March 2000 comments on 10 CFR 963, the site specific suitability regulations.

At our January 2001 meeting a couple of months ago in Amargosa Valley, Nevada, Chairman Jerry Cohon laid out four scientific and technical areas that the Board as a whole believes should be given priority by the Department of Energy. One of these reads as follows: "Development of multiple lines of evidence to support the safety case of the proposed repository. These lines of evidence should be derived independently of performance assessment and, thus, not be subject to the limitations of performance assessment."

Now, notice that I quoted that directly. That is an official Board statement.

What are the intellectual challenges we are trying to address? Well, we're hoping for broad agreement on the value of developing multiple lines of evidence. We note that the international community, for example, in a report from the Nuclear Energy Agency of OECD, clearly spoke of the need to develop multiple lines of evidence. DOE took some important steps in its October 2000 revision of the repository safety strategy. But we note that developing lines of evidence can be difficult.

Several approaches have been identified: the use of natural and engineered analogues, the use of simplified calculations. We're looking for other ideas. For example, our performance assessments done by different organizations using different models, assumptions or calculational techniques, legitimately multiple lines of evidence. Or can traditional notions of defense-in-depth serve the same function as multiple lines of evidence? Well, okay, those are questions which are among those we will address today.

The meeting is informal by design, despite the fact that we've got these cards and we have Scott Ford over here recording everything and making a transcript, but we don't have time slots on the agenda. We want to explore critical issues in a collegial fashion, leaving time for give and take.

We're going to begin with a presentation by Steve Hanauer, after a few comments by Lake Barrett, and then have several consultants we've brought in by the Board, Bill Murphy from Cal State University at Chico, Rod Ewing from the University of Michigan, and Cliff Voss from USGS.

Where's Cliff Voss? Okay.

Then we're going to explore four specific questions which are shown on your agenda. The physical characteristics, what natural or man-made characteristics, such as configurations, features, processes, designs or materials--I'm not going to read these. We're going to put them on the overhead when we get to them. So there are four specific questions which we're going to go through.

And then, finally, DOE has agreed to detail its impressions of the meeting and suggest how it has and how it plans to develop multiple lines of evidence.

There are people in the audience who have thought a lot about these issues, and I noted already that we're going to give you time slots. And at this point, I think the main thing to do is to encourage the people on the Board to be brief, because we do have terrible time pressures, and I do have a clock here, so when you hear this kind of a noise, you should think seriously about turning the microphone over to someone else.

Okay, at this point, we now turn matters over to the acting head of OCRWM, Lake Barrett, for a brief introduction.

BARRETT: Very quickly, I want to thank the Board for having this meeting. I think communications and dialogue is absolutely essential to what we're doing, especially on an item such as the multiple lines of evidence, which I think we are all at the state of the art and advancing the state of the art in this very important area.

As you all know, we at DOE and the DOE family are working very hard to strengthen the technical bases for decisions that we think this country is going to make in the future. We want to have the best practical scientific basis for that as we can make, and I think this dialogue and interchange will help us do that a lot.

And I don't have anything further to say, unless there's any questions, because I would like you to get on to the dialogue and the discussion on the multiple lines of evidence.

CRAIG: Okay, thank you very much, Lake.

All right, the first comments are from Steve Hanauer. And Steve has 15 minutes. Steve is going to talk about DOE's views of multiple lines of evidence. And, Steve, I really am setting this clock.

HANAUER: First, I'd like to acknowledge the critical contribution of Bob Murray and others in developing these ideas, as well as to develop this presentation. Here, we have what Paul has already read to you, and which you have in the handouts.

I want to acknowledge that there is a spectrum of viewpoints about this. We don't subscribe to either of these extremes which I have drawn at the ends of the spectrum, but I believe it's necessary to point out that there are people who said use the TSPA. If you need multiples of evidence, put them in the TSPA. And this, of course, goes contrary to the Board statement.

At the other end of the spectrum are people who say of course we look at multiple lines of evidence. That's what science is all about, and it therefore pervades our whole existence, our whole program. This is true, of course, but it doesn't focus on the safety case and, therefore, even though it's true, it's not useful, and we are somewhere in between, and I will try to show what we've been thinking about.

Here, we have across the bottom, the body of knowledge, the principles, the models, the analogue data, the testing, the direct observations, the process models. We have the data, which is what we know, and we have the models, which is what we believe. And what I've tried to indicate here is the existence of a body of knowledge which underlies all our technical work. This body of knowledge is not static. We're learning new things as we go along, and we're changing models, as well as getting new data.

So then on the top are the products. The product of the TSPA is given in calculated dose rates, and some other things, sensitivity studies and uncertainties, and so on. And we're looking at the question of can we get multiple lines of evidence which are represented here by a surrogate, which I took from the NRC proposed regulations, and that's the existence of multiple barriers between the noxious substances we have to deal with, and the biosphere where there are people and the environment.

In the middle are three things which I have labeled TSPA, which we all know about, and also the analysis of the site and the analysis which gives us the design. Those are three of the many ways in which we can organize this body of knowledge to give us these results.

Now, the next thing I'll do is look inside those three middle boxes, and in order to show it on the viewgraph, the top products have been omitted. They're still there, but you will have to imagine them.

First, we look inside the site, and we see the various aspects of the site which influence the repository performance and, therefore, the public health and safety. And then to the right of this yellow box, we see the attributes of the site which we distill from these various technical aspects on the left, and which promote the safety of the site and the safety of the public.

The next, we look in the middle box, the TSPA, which I don't have to spend much time on. We take this body of knowledge. We abstract it, distill it, run it through something called GoldSim, calculate dose rates through sensitivity studies, calculate uncertainties, principal factors of the safety strategy, and look at the redundancy or not of the various barriers.

Please note that three of those boxes in the body of knowledge are not used directly in TSPA, but they're still there. Some of them figure indirectly. For example, when we develop our process models, we use the information in the analogues, and we also test the models against the analogue information. The simple models and calculations don't go directly into the TSPA, and the confirmatory monitoring test and evaluation is not yet available. And so we don't use it in this TSPA, but when the world goes on, and if the site is ever designated, and if we ever actually put some waste in the repository, then we will use this confirmatory information to improve our body of knowledge, and to improve the analysis of safety.

On the right-hand side, we have the design. The design, the performance assessment in TSPA, and the understanding of the site go hand in hand. Or, if you prefer, are involved in an iterative process where when we learn something in one of them, it propagates then into the others and provides a way of improving our analysis, or our design, or our actual operations when and if we get to such a point.

There are two interesting things here. One is that the properties of the site are in fact part of the input into the design process, which should surprise nobody, and the other is that the products here are the long-lived barriers and the operational flexibility, which I'll say nothing more about today, but which is a principal current technical occupation.

Now, the question arises can these be independent of TSPA, as the Board asks. There is a certain aspect of angels dancing on the head of a pin here. I'm not talking about mathematical independence, but in a more practical way. And the answer is yes, but. Yes, these other lines of evidence which we will explore don't use the abstracted model, they don't use GoldSim, they don't make horsetail curves, although the parallel analyses of some of these things like multiple barriers do use GoldSim, do use the abstracted model, and there is a duality in our approach to the more important of these lines of evidence.

We use the TSPA, but we also search out ways to investigate them, or to develop parts of the safety case which don't use GoldSim and the abstracted models.

But, of course, in a more fundamental way, they're drawn from the same base of knowledge, all those things across the bottom of the previous viewgraphs, and many times there is one or a small number of process models which are used to analyze them.

The result is a safety case, the goal at least is a safety case which is more than a TSPA, but which includes a

TSPA as our primary line of evidence.

I hope, Mr. Chairman, that we don't get involved in a long discussion of independence, which becomes more academic than is justified by the goals that we're dealing with.

Now, finally, I simply want to draw again in larger print and living color the lines of evidence that we are going to discuss. My colleagues will deal with these as we go around the table. And they are in various stages of development. I have to say that this is work in progress, that we don't have in some of these areas a long list of reports, although in some of these areas we do have a lot of previous work that we can point to. But in some other areas, for example, in the confirmatory monitoring tests and evaluation, what you will get is a plan rather than the body of data, since we don't have anything to confirm a test at this stage of development of the repository.

That's my discussion. Do you want to take questions, or do I sit down and we talk about it later?

CRAIG: Yeah, according to my clock, you have been an exemplary presenter and, therefore, we do have a couple of minutes for comments. Dan Bullen?

I haven't introduced the Board members because i was going to do that later on. But when one of them chooses to comment, Dan Bullen is a Board member from Iowa State.

BULLEN: Bullen, Board. Steve, could you put Slide 6 back up just briefly? It's the one in which you say that feeds to TSPA, do not include the analogues or the simple calculations or the test and evaluation.

HANAUER: What I said was directly.

BULLEN: Directly. I guess the question that I have with respect to simple models and calculations is could you differentiate between a simple model and an abstracted model?

HANAUER: Yes, I do.

BULLEN: No, could you for us? Yes.

HANAUER: Yes. The objective of a simple model is to strip out a bunch of complications. That's why it's simple. And at the same time, to enable a check on the results. And, in general, these simple models are unsuitable for putting into the TSPA. They leave out things, that's why they're simple, which we want to put into the TSPA, so that the results will be more realistic, or more conservative, depending on which TSPA we're talking about. So that in general, the simple models don't find their way into the TSPAs that we've been doing lately.

There are TSPAs, EPRI's model is simpler than ours, there are TSPAs which deal in simpler models, and we accept their limitations in order to exploit their advantages in ease of understanding.

BULLEN: Thank you.

CRAIG: Steve, thank you very much.

At this time, we turn to Abe Van Luik, who is appearing here wearing a different hat from the normal hat that Abe wears. Abe is a Department of Energy person, but for the purposes of this presentation. For this purpose, Abe is chairman of a nuclear energy agency, OECD, a committee developing concepts of multiple lines of evidence. And Dan tells me that you are, once you're wired up, going to talk for six minutes.

VAN LUIK: Actually, I asked for ten minutes. I was given seven.

I have a handout. I only brought 60 copies, because that's physically all I'm capable of carrying. The handout I'll go through very quickly explaining the IGSC, Integration Group for the Safety Case. It was created last year within the Nuclear Energy Agency in Paris. They had an election and I drew the short straw and got to be chairman.

If you notice on the front, there are two e-mail addresses for myself. If you have something that you want to talk to me about that's specifically for the IGSC, we have an e-mail address for that. If you want to talk to me about something related to Yucca Mountain project, it's there, too.

The federal government requires that if we chair something like this and we have to be impartial, that we file a suitable piece of paper with the Government Ethics

Committee. That was done. So I can legitimately stand here and represent the NEA in this talk. It's legal. It's proper.

The Radioactive Waste Management Committee created IGSC and gave it its mandates, and I just highlighted on page 2 on the bottom that we're supposed to identify emerging issues, review the state of the art, promote understanding, and promote exchanges with other groups inside and outside of NEA. And it's in that capacity that I'm here right now.

The next page we can skip over. It shows how the IGSC works, and the kinds of things that we do. We do have our own web site, so that we prepare on the web site for each meeting. It's a very efficient way to go.

Our core activities, the first item under core activities is anything to do with promoting an integrated safety case. And, therefore, that's the topic under which we're speaking right now. One of the core activities that we're doing is called IPAG-3, Integrated Performance Assessment Group Number 3. And the report that is being created by that subgroup of the IGSC has a section on multiple lines of reasoning. It's in draft form right now. And we are also producing this year a safety case booklet. For those people who are new to the idea of a safety case rather than just the performance assessment, which is part of a safety case, we're creating a booklet to explain what it is and why it's important and why it should be the focus rather than just a part of it.

In that safety case booklet, the IPAG-3 report will contribute material, and the booklet will describe issues connected to a safety case and associated approaches. So it's in that context, too, that we're looking at multiple lines of reasoning.

Now, one of Steve Hanauer's slides explains very well to us, you know, his big "but," the b-u-t on the slide, that slide, and the arguments behind "yes, but" is exactly why in the international community, we decided multiple lines of evidence is not the correct word. It's multiple lines of reasoning, because as Steve said, it's the same evidence, but you're using it in different ways.

For example, paleohydrology, you use it to constrain your modelling that goes into your Total System Performance Assessment. You also use specific data and examples and reasoning from it to say but here is an argument of why radionuclides are expected to move at this rate, you know. So it's the same evidence, but you are doing different types of reasoning on that evidence.

Multiple lines of reasoning, and this is where we get to the meat of what we are discussing within this group, are a set of complementary arguments that use different approaches or sources of evidence, it's possible, to build confidence in Integrated Performance Assessment analyses, which are part of, not the total of, a safety case.

Both qualitative and quantitative lines of reasoning may be used, including scoping and bounding calculations, natural analogues and a variety of safety indicators, for example, looking at the insult to the environment, or looking at the movement of non-radioactive species, if there's information available on that.

A line of reasoning does not have to address all aspects of safety. You can do things on a sub-process scale. Nor does it have to be fully independent of other lines of reasoning. We went around and around on that, and decided that full independence is really a hypothetical thing, and not part of the real world.

One particular value of the use of multiple lines of reasoning is that different arguments may be more meaningful to different audiences. There is some equivocation over that one, whether that's a legitimate purpose for pursuing multiple lines of reasoning.

Examples that we came up with by surveying the 20 organizations that are part of the IGSC for 14 countries, the IEEA is also a participant, so is the European community, potential examples, and I cut out a few from the actual list, because they don't really pertain to high-level waste disposal: but arguments for robustness and achievability of the concept itself; arguments demonstrating long-term containment. And one of the favorites in the international community is using a material that's well understood and has been around for a long time. You can see why I didn't put that in here.

Explaining reserves of safety in the Integrated Performance Assessment; showing some redundancy in the multiple barrier system; use of simple insight models. That's the same thing that Steve was talking about.

Paleohydrology arguments, looking at natural radionuclides at the site; arguments based on the use of analogues; alternative safety indicators; perspectives on hazards represented by the waste, and this is one that's somewhat controversial. Some people feel very strongly about this. Others think that it's a way of trivializing other people's concerns, because what they're talking about here is saying let's put this risk in perspective, you know, as to what the risk was of your coming to this meeting, for example.

And then comparison with other IPA studies. And what they mean here is, for example in the U.S. case, you know, we have three people that have already evaluated Yucca Mountain, NRC, EPRI and the DOE, and in other countries, they say it's very useful, for example for people working in crystalline rock or salt, to be able to compare their results with people working in the same medium. And even though the assumptions and the actual site-specific data are very different, if they come out in the same ballpark, it's a very good way to bolster confidence.

The IPAG-3 questionnaire also included regulators who noted that in the IPAs they have reviewed formally so far, they have seen very little use of independent lines of reasoning. Generally regulators encourage the use of it, but they had no way to--I mean, there was no omniscience on the part of the regulators to say and this is what it should look like and this is what it should contain. And many people felt that the longer time frames really need other lines of reasoning, because just the calculation in and of itself is not very convincing.

Is that the six or the seven minutes? Okay.

Safety case is more than an IPA. The IGSC is very adamant on that, which is why it has the name that it does. Requires multiple lines of reasoning to demonstrate safety and show a basis for confidence. Requires additional types of information or evidence not directly used in IPAs. Independence of information is not always possible. The basic idea is to provide a basis for confidence in addition to the IPA results itself.

Thank you very much.

CRAIG: Thanks, Abe. And that was good. You really speeded up there.

Dan Metlay has handed me the statement that I just read about the Board's view on these matters, and the precise term that the Board used, just to remind you, the lines of evidence should be derived independently of performance assessment. So the Board recognizes clearly that you've got to use the same database. But it's the complexity of the TSPA methodology which is problematic. So it looks like there's good consistency there.

Quick comments from anyone?

(No response.)

CRAIG: In that case, we turn to the next stage in our morning, and that is comments by our consultants, followed by comments by the Board. And we have three consultants. We're going to go alphabetically. Rod Ewing, Bill Murphy and Cliff Voss. Rod is professor in the Department of Nuclear Engineering at University of Michigan. He has his Ph.D. in geology from Stanford University. And you're on for 15 minutes, Rod.

EWING: Thank you. Well, as others were speaking, I was rearranging my presentation. So this will be a little different than I had anticipated. But let me give the disclaimer that I wanted to give for my planned presentation.

First, I haven't looked at the Yucca Mountain Performance Assessment in several years, so it's possible that things have changed in a way that addresses some of the issues that I'll raise. But in the past several years, I would say as part of a hobby of now looking into risk assessments, I have tried to pay attention to work on modelling and risk assessment associated with global warming and genetically modified food. And I think there may be something from these areas that we can apply to today's discussion.

I've titled my presentation Adding Confidence to Performance Assessment. Changing that to address directly this issue of multiple lines of evidence, what I'd like to say first is the conclusion. I don't think that we can fully develop multiple lines of evidence unless we have multiple criteria.

In the United States, we're I think burdened by a regulatory framework that eventually pushes everything through the performance assessment and an evaluation of whether we meet some quantitatively set goal.

So thus far, the discussion of multiple lines of evidence finally pulls those lines back into the same type of analysis, and puts them on the plate with, in some form, with the performance assessment. So the theme that I want to develop is we should be looking for, if not in the regulation in our discussions with the public and scientific colleagues, multiple criteria and lines of evidence that can match those criteria.

And then as part of a disclaimer, I want to say I'm about to make some critical comments about performance assessment, because that's why we need multiple lines of evidence, because there's some dissatisfaction with performance assessment, but I want to emphasize performance assessments are a powerful tool for analysis. If I had the DOE job, the first thing I would do is a performance assessment. And also my criticisms aren't directed toward the people doing the analysis. A lot of talented people have spent a lot of time developing the performance assessment at Yucca Mountain. But I think it's the deficiencies and unhappiness with the results that bring us here today.

So let me quickly say a few words about what's wrong with probabilistic performance assessment. And although this isn't on the multiple lines of evidence, it really I think is why we're here, and so I want to say perhaps the obvious.

First, the performance assessment, despite all efforts, is opaque. I had the occasion to ask for the list of AMRs, the AMR summary, so several hundred arrive on my desk. I had occasion to ask for a few of the model reports, a few of these reports to read. They're filled with information, and they're very difficult to digest. So for any review body, we have to acknowledge--or you don't have to, but I would say that the performance assessment is opaque.

Also, it's complex. It's not sophisticated in its details, but it's complex in the way the parts are connected together, and it's very difficult in the time sequence to reconstruct how those parts are connected. The fact that it's opaque and complex means that simply adding review teams to the process doesn't necessarily help very much, because it's difficult to get your hands around the work.

From the DOE point of view, I want to make the point that the performance assessment is very vulnerable, that is, the more complicated you make it, the larger you make it, the greater the number of issues you put on the table. It's also not credible, because the balance of power in terms of people and time is on the DOE side. And from the public's point of view, and even from the regulatory point of view, there's not an equal balance to look into the performance assessment and see what is actually going on.

And then, finally, the results are probably wrong. And this is not a criticism; this is just what happens when you attempt to analyze such a complicated system. Now, you can mitigate that by changing your approach a little bit, and I'm willing to discuss at length why I think we have to assume the results are wrong in their particulars, maybe not wrong finally in the conclusion, but of course we want other lines of evidence to support the conclusions that grow out of the performance assessment.

So I'll skip the viewgraph of why I think we have to expect that the answer is wrong. But it's still very useful, powerful approach in terms of analysis, but the results in its particulars are probably wrong.

So what are some alternative strategies? Well, particularly looking at risk analyses associated with climate change, you see immediately that a difference between our community and their community is a tremendous emphasis on the analysis or determination of uncertainty, and the definition of what uncertainty means. And up until very recently, that hasn't been an important part, in my opinion, or observation of what has gone on.

And we can carry this further. One of the criteria, or multiple lines of evidence, can be this measure of uncertainty. It doesn't matter what the answer is, so much as what is the uncertainty associated with this range of answers. For policy decisions based on science and technology, this is really the essential criterion, or an element of the multiple line of evidence, that is, what is the uncertainty.

Now, the whole discussion today is on multiple lines of evidence. I simply want to call your attention to the fact that in other areas, and in risk analysis in general, the idea of using multiple criteria is growing. Our university group on science and technology policy unit at the University of Sussex has really spent a lot of time in discussions of what to do or how to regulate genetically modified foods, developing a different series of criteria.

Now, when you look in detail at what they're doing, a lot of the processes that are part of what we're doing have become criteria, such as a peer review panel. It's not something lost in a report some years back before finally going for a license, but that peer review panel is one of the criteria, and public comments. So by changing the structure of what we do, we might change the list of criteria, or develop different lines of evidence.

Now, I titled this slide Useful Principles. I was thinking of something a little different when I put it together before coming, but you can think of these items as criteria. That is, the result of the analysis should depend more on the actual properties of the site than on the assumptions that go into the analysis. That could be a criteria for accepting the analysis.

The long-term safety analysis of the site should be based more on the analysis of the passive properties of the site. This could be a criteria, and an independent line of evidence. And the analysis of uncertainty--I'll say more about this in a moment--in site behavior should focus on the smaller systems, the geosphere, without including the uncertainty from the biosphere and the health effects analysis.

This is just to pull out the parts of the analysis in a way that they can be seen, and this isn't a new idea. In the Swedish program, there's a very nice report. I just brought one copy. The title is Spent Nuclear Fuel, How Dangerous Is It? A very simple question. There's some discussion of risk in the report, but it's rather a description of spent nuclear fuel, how the radiotoxicity changes as a function of time, but not in terms of the normal risk analysis as it's usually presented in this project.

So, just to say there are many different concepts of multiple criteria, and once you have multiple criteria, then multiple lines of evidence makes sense.

We'll talk a lot today about the use of natural analogues. I now don't use the word analogue. I think after twenty years of giving talks on natural analogues, it's time to move on. Okay? Analogy is not enough. But what we can do is use natural systems as a source, a real source, for data in the model, a real or an actual way to confirm, say, the process models. And, also, this hasn't really been done, but I think the real value of natural systems at this stage is to use natural systems to confirm the usefulness of the probabilistic performance assessment.

If you look around, the use of probabilistic performance assessment is not so common in risk assessments of this type, risk assessments where we look at natural systems. So a reasonable question is does it work? Well, it works because we do it. But does it give useful answers? What are the sources of uncertainty? And, in fact, there have been I think some, just in the last few years, some excellent papers where people have used natural systems, or natural analogues to address the question of in my analysis, what are the sources of uncertainty, and how will those be manifest in the Yucca Mountain analysis? It's a very different use of natural systems. And then, of course, natural systems can, in a very general way, immediately place the site into some anticipated long-term behavior.

Then in terms of alternative lines of evidence, one thing I want to argue for is do not always present the results in terms of dose or risk. Confirm the actual science and physics and chemistry of the models before you add the uncertainty of the dose and risk calculation.

The alternative strategies certainly require the use of defense-in-depth, but I would say now we have to redefine it, given the regulatory framework, which I think has downgraded the concept of defense-in-depth, and contrary I think to the present day approach. I think that the separate barriers, as much as possible, have to be analyzed independently, so that we develop some confidence to line of evidence that some series of multiple relatively independent barriers can be relied on over a set of different time scales.

And, actually, these last few comments are now new at all. Unfortunately, I've become old enough to remember where we were 20 years ago, and I would say for geologic disposal, simply remember that we have some guiding, or we had some guiding principles against which we were working, and these guiding principles immediately offer the possibility of independent lines of evidence associated with multiple barriers, analysis of parts of the system so that they can be compared to natural systems, multiple criteria that are useful in a very honest way when you speak to different audiences. And I think that's something that's very important.

We can't expect to take the most sophisticated part of the probabilistic performance assessment and go to the public and have them set for half an hour and say ah, that looks good. No reasonable person will. So there has to be multiple lines of approach that make sense in a real way to a variety of audiences. So I'll stop there. I didn't hear the buzzer.

CRAIG: You were great, Rod. Thank you very much.

We have time for comments. Dan Bullen?

BULLEN: Bullen, Board. Rod, I was very interested in your comments with respect to the analysis of the barriers performance separately, and I think you can extrapolate that to engineered barriers, natural barriers. I guess the question that I have is when you're trying to assess the risk, how do you combine those analyses to come up with like the answer, the final answer?

EWING: Well, my point is there isn't the answer. That's the fallacy. Okay? You know, the answer is that for lots of different reasons, this makes sense. And to fold all of those different reasons into some comparison to a single number, I mean, imagine--I haven't followed the discussion recently, but in the U.S., we argue as a standard 15 millirems versus 25. I don't know what's happened. I can't be--I'm not highly entertained by that discussion. But the uncertainty must be 100 millirems, must be 200 millirems. Okay? So if we behave this way in front of the public, then they can reasonably believe 10 millirems, we can see the difference, then it makes a difference. Well, we can't see the difference, and it doesn't make a difference.

BULLEN: Bullen, Board. Actually, that's exactly the point that I wanted to follow up on, is that as you bring these together, and the uncertainties overlap, I guess it's the combination of the uncertainties that the Board has been grappling with. Is that a good way to put it? And I guess how do you do that combination, or do you not? You just look at the individual uncertainties and deal with it from there?

EWING: It's my uninformed--not uninformed, it's my belief that if you in a scientific way propagate the uncertainties throughout the entire analysis, it will be so large as to make the analysis unuseful. And I know the analysis is useful if you, in a reasonable way, break it into its component parts and say, well, travel times are long. Sorption is high, and so on.

BULLEN: You've just articulated the problem that I have with the uncertainty propagation, is that it gets to be all encompassing. And I've looked at it as issues where barriers are masking the effects of other barriers when you try to put it all together.

EWING: Well, when I say I believe, what I base my point on is I can do the geochemistry by hand. Okay? So I can take an Eh-pH diagram, I can vary the energy a little bit, and I can see the exponential increase in uncertainty. Okay?

I can jump over to the hydrology. I can see the same exponential functions and imagine what happens. So I think the uncertainty, if you propagate it properly over 10,000 years, will make the analysis less useful than it actually is.

CRAIG: Dan Metlay and Debra Knopman. Debra Knopman is a Board member.

KNOPMAN: And I didn't have to say that.

Rod, I'm going to play devil's advocate a little bit here. I am not supposed to--however, we're in a real world of having to make public decisions. In the end, it's not a technical decision strictly speaking, though it's informed by technical insight and knowledge. There will be tradeoffs made. By your insistence on multiple criteria, and some of the other things that you mentioned, it seems like it could be argued that you're heading toward such asymmetry in the way this particular system is analyzed, in such a totally different way and with a kind of rigor totally out of sync with our other public decisions, including those to operate nuclear power plants, store waste on site, things of that sort, that you've created--you've actually made it impossible to make a public decision if one followed your steps. What's the argument against what I just said?

EWING: Well, the argument is that times are changing, and what I described is what I perceive to be the approach that will be used in making these types of decisions. And I didn't have time to make a viewgraph, but a very nice quote from a recent paper in nature on global change and modelling climate change and deciding what to do, reads as follows. "As a general principle, science and technology will contribute more effectively to society's needs when decision makes base their experience or expectations on a full distribution of alcons, and then make choices in the face of the resulting perhaps considerable uncertainty."

So what I'm arguing is that we need to break away in lots of environmental issues, heavy metal concentrations in groundwater, and so on, from the single point valuation of environmental impact, because all of the scientific and technical evidence is that the impact is at multiple points.

I think my time is up.

CRAIG: Your time is up, but we're certainly going to allow Board members to talk at any time. And we'll allow Dan Metlay to talk also, a staff member.

METLAY: This is a question which we probably will get into in greater degree, but this may be a good time to discuss it. In several of your viewgraphs, there was sort of a geologic centric approach that focused on the properties of a site, properties of the various geological attributes. There's another way of looking at it that says that really what counts is the system performance, and various systems may have different sort of properties. Some depend more on the geology than on others. How do you deal with that kind of philosophical issue? It's clearly not a technical issue. It's a philosophical one.

EWING: Well, no, I would say it's both philosophical and technical. And I think I can deal with it pretty easily. If you tell me it's geological disposal, and that phrase is still used, then I'll look to the geology and the geologic principles that were outlined in the beginning as appropriate.

If you tell me that the safety needs can be met mainly by engineered barriers, then I'll analyze the engineered barriers over the appropriate period of time, or argue that the time period is not appropriate.

So it's not difficulty from the side of the person who has to do the work, as long as we have the question stated very clearly. Now, if it's not geologic disposal and the site is not important, then that has other implications that probably the people of Nevada will see immediately.

CRAIG: Rod, thank you very much.

Our next speaker is Bill Murphy. Bill is from Chico State. He's a geologist with a Ph.D. from U.C. Berkeley. And, Bill, you have about 15 minutes.

MURPHY: Thank you very much.

I'm Bill Murphy, and I'm here representing myself today at the invitation of the TRB. I'm quite pleased to be here, and I want to make it clear that I'm at California State University at Chico now. I worked for a long time for the Center for Nuclear Waste Regulatory Analyses in their support for the Nuclear Regulatory Commission, and some of the information that I'll present today is based on work I did collaboratively with them. However, what I'm saying today is clearly my own viewpoint. I'm not representing anyone really except myself today.

I want to speak in particular about natural analogues. I've been involved in the study of natural analogue systems for quite a long time, and I've done work together with colleagues at the CNWRA on the Nuclear Regulatory Commission at two particular sites, and I'll show some results from our studies at those two sites specifically related to the Yucca Mountain case. There's a lot of attention that's been devoted to natural analogues. One of the themes that I'd like to make prevalent is that they can be used. Natural analogues are useful. They can provide information that helps in form decision making, ultimately builds a general safety case.

I am privileged to follow Rod Ewing's eloquent talk and I endorse many of the positions and principles that he laid out. The notion in particular caught my mind that one comes to a decision relying on multiple lines of evidence, relying on information from a vast variety of sources. And among these, I think natural analogues have an important role in evaluating geologic disposal of nuclear waste, and for good reason.

It's a very hard problem. We all recognize the challenges associated with the complexity of the system, the engineered system, the geologic system, and in particular, the vast periods of time that are necessary to come to grips with. How good are our models over those very long periods of time?

The two natural analogue sites that we've worked on, or I've worked on in the past, are the Pena Blanca site. It's a uranium deposit in northern Mexico near Chihuahua which is a remarkable system in comparison to Yucca Mountain. The rocks there are solistic volcanic tuffs. The climate is arid. The rocks are fractured. And there's one important difference between the Pena Blanca system and Yucca Mountain, and that is that there's a large natural uranium body there that was originally uranium dioxide. It's now almost entirely oxidized to a suite of secondary uranium minerals. I think it's an incredible example of a system that is very analogous to Yucca Mountain.

Now, people who have advocated studies of analogues have so long been challenged to demonstrate or to convince skeptics that they're of some value that the case has sometimes been made more strongly than is appropriate. There are big differences between Pena Blanca and Yucca Mountain, and those have to be recognized as well. It's a hard problem to draw exact analogies. It's not exactly the same. There are important differences between the Pena Blanca system and the potential repository at Yucca Mountain. But nevertheless, it gives us information on the long-term behavior of materials very similar to spent fuel. Natural uranium dioxide has essentially the same structure as spent fuel over time scales that are clearly inaccessible in the laboratories.

The uranium deposits at Yucca Mountain were probably formed originally about eight million years ago. The oxidation of that uranium deposit occurred, our best estimate is that it was occurred three million years ago, and maybe over a very long period. The oxidation is not complete. There's still reduced uranium at the site, and we've used that notion that oxidation has been occurring over geologic time to evaluate what the average long-term rate of alteration of oxidation of uranium dioxide is.

We used that rate in the Nuclear Regulatory Commission's performance assessment calculation as an alternate model. This alternate calculation is published in the two first papers listed on this slide. I'll show the results of those calculations in a moment.

The second natural analogue system in which we've done work is at the Akrotiri site. It's on an island in the Aegean Sea. It's a beautiful place to work and a fascinating place. There, an eruption of the Island of Santorini, Santorini is a volcano, and the eruption of that volcano 3,600 years ago buried a city in solistic volcanic tuffs. That city has been preserved, fortunately to anthropologists, and to some degree to nuclear waste managers, has been preserved by being buried in volcanic tuff. It's in the unsaturated zone. It's a relatively dry climate. It's not exactly like Yucca Mountain. But we used that site as a source of information to test transport modelling.

We examined the release and migration of exotic materials, in this case copper particularly. We looked at the release of copper from buried bronze artifacts at that site. We did blind modelling. We used performance assessment like tools of unsaturated flow, equilibrium distribution between aqueous phase and solid phase, and estimates of infiltration and flux. We used the same kinds of modelling that are commonly used in performance assessment to predict the extent of the plume of copper in this case that one would expect from these buried artifacts. They've been buried for 3,600 years in the unsaturated solistic tuffs at Santorini.

We then went to the site and evaluated, to the extent we could, what the nature of that plume was. We used site characterization data of the sort used in performance assessments to build up our model, and then as an analogous system, we were able to go to the site actually and do what we could to analyze the plume. And so I'll show some of those to you in a moment as well.

The bottom line is that natural analogue data can be used. They're not a final answer to the problem. They're one of a number of multiple lines of evidence.

This graph is a graph of the probability of exceeding annual doses. These curves in a CCDF were calculated using the NRC's and Center for Nuclear Waste Regulatory Analyses TPA-3 code, and they show the probability of exceeding doses as a function of what those doses are, and it shows multiple approaches to doing a performance assessment using data from quite a broad variety of sources and invoking quite different conceptual models.

There are two sets that are 50,000 year curves, and there are 10,000 year curves, and there are four in each set, and the four curves, the highest one, the highest doses are predicted using the NRC's code, assuming a rate of release that's an experimentally based rate of release, that was in fact the same as that used in the Department of Energy's performance assessment.

The second curve is somewhat lower than that, lower predicted doses. These are using a rate of release that is based on the same experimental data, somewhat different interpretation of the same experimental data. And it shows somewhat lower doses.

And then the final two curves, and these two curves are for 10,000 years, are using alternate source term models based on the Nopal Oxidation. Nopal is the name of the specific uranium deposit that we've studied at Pena Blanca. Making use of our estimate on the maximum average oxidation rate of uranium dioxide at Nopal, we've introduced that rate as an alternate source term in the performance assessment model, and get quite substantially lower doses.

I can't make the case that this is more accurate than any of the other curves. They come from completely different conceptual models, or different models for the source of the rates. They all have large uncertainties. Extrapolating experimental data for thousands or tens of thousands of years is very difficult. Doing experiments with spent fuel that's not 10,000 years old is of only partial relevance to the conditions that may eventually obtain. And obviously there are important differences between the Nopal or Pena Blanca system and Yucca Mountain, and as in all geologic systems, the information that's available to us is sketchy. We don't know completely what's gone on there. We have ideas. Fortunately we have radiometric dating to give us times.

But here we have an actual use of analogue data as an alternate model in a performance assessment. The lowest curve is based on a completely different conceptual model. The notion that at Yucca Mountain, as at Pena Blanca, the stable uranium phases will be oxidized phases, phases like Schoepite and uranifane, and this curve was based on the notion that somehow the radionuclides other than uranium are incorporated in more stable oxidized forms of uranium. There's some evidence from the Argonne studies that neptunium in fact is included in secondary uranial oxidized uranium phases to a greater extent than it occurs in the fuel itself.

That's a very intriguing set of data. We made very simple basic assumptions about the stoichiometry of incorporation of uranium, of other radionuclides in the secondary uranium phases, used that as an alternate source term model. So that gives us our lowest curve. In fact, both of our alternate models predict substantially lower doses than either the experimental rates as used by DOE or as reinterpreted in the NRC's TAP calculation.

The other site, I'll show this briefly, this is a summary slide of some of our results for the blind modelling at the Akrotiri site. Note the scale here. The scale in which we were able to do studies there is meters. It's quite a different scale than we're concerned with at Yucca Mountain. This is our model. These are our model results for copper in the solids, our model results for copper in solution. We used performance assessment type and site characterization type information to make these calculation. This was really a blind calculation of the predicted distribution of copper in the system, given that these artifacts have been buried there for 3,600 years.

We were able to go to the site, collect samples, selectively extra copper that was sorbed onto the solids, and these are some of the copper data that we collected.

So there are two ways to look at this set of data. One is that from a blind modelling point of view. The fact that we're off in absolute copper concentrations from the solid here concentrations predicted and the copper concentrations here by only a factor of two or so is really quite remarkable. It may be totally accidental, but it's a very good correspondence.

On the other hand, there seemed to be a problem in some very fundamental aspects of the transport modelling. Every model that we tested for this particular site predicted that on a time scale of 3,600 years, a steady state distribution of concentrations would obtain. The flux through the system is sufficiently fast, our source term has a constant concentration. We predicted steady state conditions to obtain within hundreds of years after the system was set in motion in all of our models.

However, looking and trying to draw as much information as possible out of the data, we had a tendency to see a transience in the distribution, evidence of a transient transport system, perhaps caused by diffusion. You can compare it to these curves. These are upward diffusion curves. And it looks sort of like this curve here.

So there seemed to be some basic problem perhaps in our conceptual model for transport in the site, that in fact there is a great deal more transience. Maybe diffusion plays a greater role. The models all predicted steady state flux of flow dominated transport. So these are some Akrotiri results.

Now, finally, I'll turn away from the specific analogue sites. I have one further slide which maybe I won't show because my time is up. It relates, however, to a point Rod made that in fact I think there can be a great number of various inducements to looking at the problem from multiple approaches, both systematically and scientifically, using analogues or other approaches. I'll stop there.

CRAIG: Okay. Questions for Bill? Richard Parizek, Board member.

PARIZEK: When you talk about uranium at the Mexico site, it's disseminated probably in small particle form or somehow versus lumps, as it might be in Yucca Mountain. So in terms of scaling, do you see any problem with that, or also the fact that you have holes in the mountain, so as you begin to modify the mountain characteristics through the engineering there, that might cause you some skewing in your analogue?

MURPHY: Absolutely. They're different systems and a number of our best comparisons had to be made. The uranium deposit at Pena Blanca is rather compact. It's a relatively concentrated--the primary uranium deposit is quite concentrated. It's very tightly delineated. It's mapped out. And for uranium deposits, it's pretty concentrated. That's an interesting idea. I haven't actually made a comparison of the concentration per square meter compared to Yucca Mountain.

However, one of the differences in the treatment, the experimental treatment versus the analogue treatment for this particular calculation was that in the experimental studies, rates are normalized to surface areas, and it's really a somewhat hopeless endeavor to try to characterize the surface area of the uranium deposit in that natural system.

We used a global oxidation rate that's pinned to the total mass of uranium that's in the system known from the exploration studies, and the known time during which it has been oxidizing based on radiometric dating.

CRAIG: Thank you very, very much, Bill.

Our last consultant is Cliff Voss from the U.S. Geological Survey, who has been for many years running a project concerning subsurface transport phenomena.

VOSS: What you're about to see as the lights go down is a slice of Swedish rock about 500 meters down below the ground, a model of fractures that are in the rock that I'll show you in a little bit, and the red lines there are drifts where it's a hypothetical repository.

The USGS has been working for quite some years with Sweden. Mainly our point is to learn from their fantastic datasets how fractured rocks work at various scales. The point of view from SKI, which is the Swedish equivalent of the NRC is to study the safety of their nuclear waste repository, which they're planning on putting about 500 meters down in their fractured rock.

Of course they have been, SKI has been looking at very sophisticated methods for evaluating the safety of such a site, both in terms of site characterization and in terms of analysis. And the hydrologic parts of that are based on a few main approaches, and here are some difficulties with the complex approaches that they use, and you'll see analogy of course to the U.S. program in everything I'm telling you about Sweden.

The deterministic approaches for these sorts of fractured rocks are impossible ever to be complete. You're always going to be missing fractures. In the models, you're going to be missing intersections of the fractures, and the properties of both of those things. The problem with these sorts of materials, these rocks, are that these missing structures probably control the behavior that you're interested in.

There are other sophisticated approaches to looking at these sorts of fractured rocks. Those are stochastic approaches. These also have some difficulties. What you get from a stochastic approach is only a tendency of the site, and the site you're at may differ from the tendency that you predict, the main tendency you predict. And if the scale of behavior is smaller than the scale of the variability you measure, then it may really be different from what you're predicting, and you aren't necessarily certain of that after you've made your measurements.

Another problem with stochastic methods is that the form of the variability, the probability distributions are only an assumption. They're really never proven for any particular site. If you were to try to prove them, you would have to make your site look like Swiss cheese with boreholes, and of course that spoils a nuclear waste site right up front. So you can't really determine the properties of your site statistically ever. That goes for most sites on earth.

And another assumption is normally that the form and the parameters of these statistical distributions are assumed constant for the site, and that's not necessarily certain. They may vary with position or distance over the site. There are problems with both of these sophisticated approaches. An even more sophisticated approach, and probably the best, is a deterministic stochastic approach where you take structures that you know are in the site, fractures, and you take the rest which you don't know, make that stochastic, and this suffers from deficiency of both of the approaches.

Okay, let me show you some of the complexities of the Swedish sites. They've been working for over 20 years in fractured rock for their nuclear waste program. It's a fantastic scientific program. These are the sites in Sweden that you see. The most recent one is here on the southeastern coast of Sweden, the Aspo hard rock laboratory site, which I'll show you data from. This is a small island near a nuclear waste power plant where they're actually in this area now considering the actual high-level waste repository.

A picture of Swedish rocks, the digital raised relief model illuminates from different directions, about 50 kilometers is what you see. You can see fracture zones. These fracture zones extend for hundreds of kilometers. This is pre-cambrian rock. It's granodiorite fractured at all scales.

We'll look now at the island, which is right here. You can see these fracture zones crossing the island in different directions. The island is about two kilometers across, and here's a map of photolinears. The green lines are fractures at this scale of two kilometers. The reds are low areas where there has either been more weathering or some movement of these blocks of rock. This is where the hard rock laboratory has been built, and it's an analogy to a nuclear waste repository in Sweden. They're doing some fantastic studies there.

Well, SKB, which is the equivalent here to DOE, for building their repository, has put in these boreholes on this island. You're looking at boreholes about a kilometer deep. This is prior to making the laboratory. You can see they're oriented, and as you can imagine, they've collected all kinds of data in these things.

One of the main kinds of data used for building models of the geologic structures from subsurface is called borehole radar. And basically this instrument sees out from the boreholes. It can see the fractures of the structures, even if they don't intersect. Here are two borehole radar indications that happen to line up. The way that we built the model of this site is by lining them up, bringing them up to the surface, seeing if they fit, if they did, accepting them into a model, which ended up after a lot of pain and terror looking like this. It's got 52 structures in it and it's a very complicated picture of this two kilometer block on the side of a one kilometer deep block. So this is a surface databased geologic model of the site.

Okay, how well does it explain where the water flows for looking at the hydrogeologic aspects of performance assessment. Well, these are boreholes that have measurements of water flowing into them. So here are the locations, all of the ones underground, and the boreholes where water flows in the rock at a rate that could be measured. How many of these are explained by that complicated model with 52 structures in it? Well, the red ones are. The black ones are not. So about half of the places underground are not yet explained by this very complicated model. So the model is not complete, may never be. There's no way to complete it I think. It's a bit frustrating after working on it for some time.

Okay, that was a surface databased model. Here's the laboratory. Now, you can see that spiral going underground. We'll zoom in on that. You can see the experimental drifts, the elevator and ventilation shafts, and some mappings that SKB did of the tunnel walls. The blue lines are fractures that have water flowing in them. The reds here are fracture zones where there are too many fractures that are flowing to map. And the blacks are fracture zones that aren't too permeable.

Well, hydrogeologists attempted to put together a model of this fantastic data. It's three dimensional subsurface data. You can actually touch the fractures that you're trying to model. And one of the corroborative kinds of data you might use to do that, or actually a target of a geologic model may be to explain where the water flows.

Well, these are the pilot boreholes that SKB makes for every four meter blasted section of the tunnel. They put out a few boreholes before they blast, and they measure inflows. And the color scale is a log of inflow, with blue being the highest. Well, drop everything but the two highest orders of magnitude of inflow. So these are the places underground in the tunnel where a lot of water flows, and I think we need to explain this first.

So one way of doing that, really to go very quickly on this, is to work in three dimensions, place structures that line things up. And here's one we're sighting down one structure right on its edge. It passes through these fracture zones that cross the tunnels in 3-D, and it also happens to cross these boreholes where a lot of water came into the boreholes. So this is a very definite structure that we should include in our model.

Well, if you play that game with the underground data, maybe you would end up with something like this. And I bet if you did it, you would come up with something different. This is what we came up with. This one has about 26 structures before we got tired. So we have two models of the site, one based on surface, and one based on subsurface data, if you will.

How do they compare is an interesting story. You can play games with that. Let me turn back to performance assessment and not talk about the hydrogeology so much. What SKI is interested in is taking a site like this and determining whether it's safe or not. They want to check performance assessment. And as I said, they applied lots of complex approaches, as well as simple approaches.

In terms of hydrogeology, they have determined that there are really two main factors that are important. Those are the flow through the site, which is given by the Darcy velocity, and one parameter that controls the retardation of radionuclides, which is the F parameter I'll show you in a moment. And their objectives for the hydrogeology are to try to get as narrow ranges as possible for these two parameters, because these are the values that they feed into their performance assessment codes.

Okay, well, here's the other side of the coin. It's a very complicated site. You're never finished characterizing it. You're never really finished modelling it in any detail stochastically or deterministically. Here's a simple way of looking at it, and it's a very powerful way of looking at it. What's a simple model? It's something that's based on simple principles of hydrogeology, simple geometry, simple parameter distribution, constant parameter distribution.

What do you do with it? Where do you want to apply it in a critical way to full ranges of parameter values and to discriminating situations, diagnostic situations. And let me show you how we did that for the Swedish site that I just showed you.

For the Darcy velocity, for the flow, well, here's the repository canister. We're looking at one dimensional flow lines up from that. How much flow goes along any possible path from the repository. So here are three paths from the repository. The most optimistic path is that we have flow going straight up from it 500 meters through good rock with just a little bit of fracturing in it, path one. Path two is sort of a design criteria, and it's what they're trying to achieve. You get ten meters away from a fracture zone, ten meters of good rock between them, that's path two. So the radionuclides go through ten meters and then up through a fracture zone.

The least optimistic, the most pessimistic one is this disturbed zone around the repository that would be caused by stress released blasting, however they finally intend to build it, connecting up fractures that never were connected with each other before, and in a sense, making a major fracture zone all around the shell of the repository, connecting that to an existing fracture zone. So you have path three, which is all fracture zone all the way up.

So three paths. If you look at the possible Darcy velocity, it's very simple using Darcy's law from that, you get a tremendous range of many orders of magnitude from what SKI considers to be good, which is low velocities, up to very poor. This does not give a narrow range of possibilities of flow at the site. We don't know if the site is good or not for flow based on this analysis.

Okay, now let's look at the transport analysis of the site, again, a very simple one dimensional path, the three paths I just showed you, but this time it's an F parameter that's important. And very quickly, the F parameter is something that controls, they found to control the maximum dose of radionuclides, of decaying radionuclides, much more important from the second most important parameter. As this parameter goes up, the dose goes down, coming out through the rock.

And what is the parameter? Basically, it's a product of the wetted surface and the travel time. So the longer the travel time, the better the radionuclides are retained, the more they have time to decay. The greater the wetted surface, the more they can sorb and diffuse into the background rock as they're moving through. So the wetted surface is a parameter that's most important. How much rock wall does the water see as it's leaving the repository? A lot or little? Well, that's something that's hard to measure in this kind of rock.

So a simple model of that might be this. Here are different possibilities for the way the rock looks internally. A simple fracture, a stack of simple fractures, step fractures in the flow direction, and across the flow direction. They have different areas that the water would see, and also different impedances to the flow. A fracture filled with crushed rock, and that's pretty optimistic, lots of surface area there.

Here's a fracture, this is a fracture, but there are only channels in it that are open to flow, and the fractures typically behave like that. Most water flows 90 per cent through a very small channel, not through the entire surface. This is called channelling.

Well, if you take these different models of the rock and calculate the F parameter, it's a little more complicated than I'm just telling you, but basically, you get a tremendous range of this parameter from very good values, very retarding rock for radionuclides would capture the radionuclides and not let them out, to very low. Okay?

Also, we have a wide range we can't determine how the rock works. Okay, how well do complex models do in comparison to that? Do they reduce that sort of uncertainty in this kind of rock? Well, they've used a lot of complex models. Here are two of the main ones that I can use for comparison. One is a stochastic continuum model. That's really hard to read. The other is a discrete fracture model of the same site. The stochastic continuum model, the permeabilities are assigned in blocks, so it's a complete full model of the entire rock, and the high permeability zones end up as streaks, sort of streaks that aren't quite random. They line up, as in the picture.

This model actually is a groundwater flow and transport model of the first of the two fracture models I showed you with 52 fractures in it, and it's connected to a regional model of the scale I first showed you, those 50 kilometer long fracture zones. So this is a very complex model that was calibrated for the site.

Okay, how well do these two models do in comparison with the simple analysis? Well, for flow, here is the range of velocities, the range of flows for the simple evaluation, and here are the ranges for different model assumptions in these two models, complicated models. And you can see that, yeah, they're narrower. They give somewhat of a narrow range. But they still don't determine whether the site is good or poor.

Okay, how about for radionuclide retardation, the F parameter? Well, the only one we could compare with, because of the calculations that were done, was the model with the discrete fractures in it. Here's the simple evaluation, and yeah, the discrete feature model for different assumptions about how the fractures connect gave a narrower range, but also does not determine whether the site is good or bad for radionuclide retention.

Okay, so what are the conclusions of that that we can draw? At least this should generate some thought. Well, the simple evaluation can't tell you whether this site is good. The complex analysis, well, it gives you narrow ranges, but it also can't tell you whether the site is good. So there was a lot more time and effort put into the complex analysis. Is it really worth doing? What's the point of it if you can get the simple analysis almost on the back of the envelop calculation. That's a question.

What is the value of a simple evaluation? Well, obviously it's quick. Back of the envelope is exaggerating. You can't do it that quickly. But it took a few days to do these analyses, and you don't really need that much field data to do it. You don't have to characterize the heck out of your site. Basically, you make assumptions about what the possibilities are at the site, and you build them into a simple analysis.

It gives you directly the basic information that you want to know about the site, no round about ways. It provides, as some others have said, a bounding check, a reality check on your more complex analyses, because I don't believe for a second that these complex analyses, no matter what I say, no matter what anyone believes, I don't think the complex analyses are not going to be done. I think they'll be done. So this provides a reality check on those.

I think they provide, these simple analyses provide as much information as complex analyses do. And also you can tell what's been done exactly. You can tell what the assumptions are, how the calculations were done, and what the results are. It's very transparent. And that's an important aspect of creating believability in these analyses.

So when we're evaluating hydrology complex environments, if we're using a simple approach, our objective is to use our knowledge and intuition to bound the possible outcomes, behaviors at the site. If we're using a complex approach, our objective is to get something that's actually practical and that's actually meaningful, despite its complexity. And then, of course, to give better results than a simple analysis would give. If you haven't done that, then there's no point to doing a complex analysis.

CRAIG: Cliff, thank you. Thank you very much.

One observation I'm inclined to make is in a certain sense, there's bad luck involved here, because if your requirements for the site were somewhat different, you could have, say over to the right, you could have the situation where there's an enormous amount of uncertainty, but it doesn't matter because the site requirements are way off on the end of that, and you're good no matter what it is, what the answer is. So this is just the bad luck of the requirements that happen to go along with what you're asking for this particular site.

VOSS: Well, they actually looked at dose from the site, and there were dose criteria.

CRAIG: If the dose criteria were, say, 10R per year, then there wouldn't be any issue at all.

VOSS: Right.

CRAIG: Richard Parizek, Board?

PARIZEK: Having compared the two, a complex one which was done, and a simple one which falls out of that, it's one thing. But if you didn't have the complex one run, say you go to Yucca Mountain, how do you draw that conclusion that the simple one is good enough, or even know how bad off your prediction would be? I mean, you're now using a place where you can compare the simple versus complex approach, but that's after you have the luxury of having the complex approach with all the time and effort that went into that.

VOSS: I think experience. If you do this enough I think experience is that the complex approach doesn't necessarily lend a lot more information to it. It looks good. It has a lot of information in it, but we're talking about hydrogeology, not about a total performance assessment.

The complex approach in hydrogeology doesn't necessarily give something, unless there are non-linearities that you can't do in a straightforward way, if there is variable density or unsaturated flow might make it more complicated, there would have to be simplifications made to those things. Where you actually, if you can't do that, you actually have to use numerical solutions to do that.

But there also is--I think the point is that there are ways of approaching these simply. The point shouldn't be to do the most complex thing first. It should be to do the simplest thing first, and only to build in complexities as needed to better understand the site. I think that should be the main takeaway point here, not that simple approaches are better. It's a way of working on things.

At other sites in Sweden, I think the sites all look very similar. They're all pretty much equally fractured. This isn't the Swedish. This is my opinion. And I think that they can do it with not very much site characterization and use the simple approach. They basically already know what the answer is going to be at most sites without putting in a lot of efforts there.

CRAIG: Don Runnells, Board member?

RUNNELLS: You talked just about the hydrology. You didn't mention the chemical environment of the proposed repository there. What can you say about the oxidizing and reducing conditions, for example?

VOSS: The Swedes are hoping that the conditions will be reducing for a long time. And I didn't mention that they're looking at the repository for time periods of about 100,000 years after there may be three glaciations, and then people come back to Scandinavia. So the initial period is the first 10 or 20,000 years, and then there's a period of glaciation where people may leave and then come back after 100,000 years.

In the initial period, probably the conditions would tend to be reducing, the way they are now. There's nothing that should disturb that. Over the long term, there are questions that have come up about whether glaciations are going to pump oxygenated water deep into the ground, and whether for periods of 10 or 20,000 years, there would be oxygen down there. There also are shield burns in these rocks, and those tend to affect the radionuclide transport properties for high salinites.

RUNNELLS: Thank you.

CRAIG: Okay, thank you. Debra Knopman, Board?

KNOPMAN: Some of this sounds familiar, Cliff. Let me ask you a question about some trade-offs here between going out and collecting more data, and use of simple models versus complex models.

I think you can make the argument that the value of additional information in some sense with a simpler model is much higher, because you're not dissipating as much of your new information on parameters when you have so many parameters in these complex models, and you just sort of lose your field data.

VOSS: I agree with that.

KNOPMAN: Do you have a sense in this case of how much you could have narrowed those bounds on the simple model estimates by intensifying some of the data collection so that it would have been more focused on just characterizing the site using a simple--

VOSS: I agree with what you said. The simple analysis points out exactly what sort of data would be needed to narrow the ranges. In this case, it's pretty obvious that that area that the water sees is very important. If you knew that for the rock over a narrow range, then you'd be able to get a narrow range of the radionuclide retardation parameter, which is the main thing going on in these rocks.

So, yeah, that would be the thing to focus on, and that's coming out, that's come out in the Swedish program, that that's something to measure.

A problem with those sorts of measurements are that you can make the measurement on a scale of 10 meters, or something, in the rock, you can find a fracture and do that measurement, but then what happens over a kilometer, or something, in the far field. Then you have to extrapolate again. But there may be ways--I think your point is that the simple analysis clearly points out the needs for data more clearly than a complex analysis, which has needs for all kinds of data and you're never really sure what you've got. There are too many parameters.

CRAIG: Okay, thank you very much. I'd like to thank all three of our consultants, and I'd like to remind everyone that the consultants are consultants to the Board, but do not speak for the Board. They're offering their own opinions, just as Board members when they speak are offering their own opinions. The only Board official opinions are the ones that we tell you are official opinions, and there's only one of those that's been mentioned so far today, and probably that's the only one that will be mentioned today.

We're now going to start the next phase of our conversation by going around the table, and we're going to begin on the left with Mike Voegele from DOE.

VOEGELE: I was fully expecting to be last.

I'm going to talk about at least one of the boxes, confirmatory monitoring test and evaluation, but I'm going to move into the box of predictive and estimation testing as well. I'd like to do that by talking about a concept called the test and evaluation plan that is an important part of our program. It's a classical systems engineering concept that is imposed on the program by a DOE order. Also, there are NRC regulatory requirements for something called a performance confirmation plan.

I'd like to tell you what performance confirmation means in the NRC's regulations. It's a program of tests, experiments and analyses which is conducted to evaluate the accuracy and adequacy of the information used to determine with reasonable assurance that the performance objectives for the period after permanent closure will be met.

In simple words, this is a program which is designed to make sure that whatever assumptions or conclusions are drawn during the licensing process are in fact verified to the extent necessary to provide the confidence to eventually close the repository, if we go that way.

The classical test and evaluation plan concept is a plan which is used to guide the development of a complex system, and so you can envision testing occurring at multiple phases along the program. There might be some very simple component and pre-operational testing at the earlier phases of the program going all the way up through post-closure in this case monitoring and testing.

We developed, in response to some Nuclear Regulatory Commission requirements, a performance confirmation plan initially with our site characterization plan. And if you go into that document, you will find a couple of tables in Chapter 8, 835-16-1 and 2, that define monitoring activities and testing activities that we started during the site characterization program that were intended to be part of our eventual performance confirmation program.

I can draw an example from those tables of something that was started with an intention to be part of our performance confirmation program, which actually functioned quite well, the way test and evaluation is supposed to function. Percolation flux was one of the things that was identified on those tables as a type of testing and analyses that we would continually look at to try to see how the site would eventually perform. And many of you probably know that over the course of our site characterization program, our values of percolation flux have changed, as we got additional information from our testing program, analyzed it and put to use in the program's performance assessments.

I'd like to talk just a moment about some of the performance, the requirements for the performance confirmation program. It is to confirm that the actual subsystem conditions are within the limits that were assumed during the licensing review. It is to confirm that the engineered and geologic systems that are assumed to operate as barriers after permanent closure are in fact functioning as it was intended that they function, or as it was anticipated that they function.

If significant deviations exist from the projections that were assumed, for example during the licensing review, we would have to go back and take corrective action. There is design testing requirements. There are monitoring testing requirements specifically laid on the waste package testing.

Generally, these are tests that would be probably factored into the specifications of the license. For example, a conclusion might be drawn during the licensing period based on baseline data which was collected during the site characterization phases of the program, and the reasonable assurance finding could be made with an intention to continue to measure the data that substantiated the arguments at the time of licensing, and there could be requirements placed on the program to monitor specific pieces of data for many years, up until the time of closure.

We're all aware that the regulations that govern the licensing of a repository involve a finding of reasonable assurance that the system will function as intended. There's also a very carefully crafted statement in the regulations that we will have to deal with uncertainties, that they're expected and that they'll have to be dealt with.

This test and evaluation, the confirmatory testing program is in fact designed to continue to provide information to make sure that the decisions that were made based on that data are in fact correct.

I think I'd like to tie this to something that Dr. Ewing said this morning, and maybe perhaps even go into another box as well. As we collect data, we tend to do simple analyses on that data. We plot data. We look for correlations. We look for one dimensional analyses that we can use to substantiate our understanding of how a particular piece of the system might be working. That information is eventually set into performance assessment, but you can look at individual components of the system through these performance confirmation and these test and evaluation programs and draw conclusions about how pieces of the system are in fact functioning.

I'd like to close just by reminding us that this monitoring program is in fact envisioned to be a very important part of our program. It will provide evidence about the predictions that were made with the models. There may be assumptions and predictions that were made based on abstracted data that were collected over relatively short periods of time. If a piece of information is specified as a target, if you will, of the performance confirmation program, we could literally have 300 years of data on a particular parameter which could continue to go back, and rather than being viewed as an assumption about the performance of a component, we actually could have measurements about how that particular component performs.

Thank you.

CRAIG: Thank you, Mike. Debra? Debra Knopman, Board.

BULLEN: Bullen, Board. Are we going to ask questions as we go around, or is everybody going to make comments.

CRAIG: I think we're, because we've got time constraints, we're better off if we go around, and if you have a comment you want to make on a previous speaker, you can do that, and then after we go around, we'll come back.

BULLEN: Okay, that's fine.

CRAIG: Because we do have to--a little over an hour now, and we want our time for--we've got to go to lunch. Sorry, Dan.

KNOPMAN: I'll be relatively brief. The main point that I wanted to make here was that we do have examples of how we have used multiple lines of evidence, both in this program and elsewhere, that I think--I was going to give three examples here just quickly to give a sense of what the range of possibilities are.

I guess when one thinks about the multiple lines of argument or evidence, whichever term you want to use, is to build credibility and confidence in our understanding of how the system works. I think of it as something like the cognitive analogue to the concept of defense-in-depth and repository safety. And, you know, the idea here is that it's not just any one thing on its own; it's the accumulation of argument and logic that takes us to the same point.

One example that the Board has right in its lap is the work that was done for the Board in reviewing the material from Jerry Szymanski and his colleagues presented to us in 1999, I guess. And that material, just for members of the audience, is on the Board's web site. You can see what some of the consultants that we hired, as well as our staff, put together.

Jerry Szymanski in fact used multiple lines of evidence, or asserted that he had multiple lines of evidence to support the hydrothermal upwelling at the Yucca Mountain site.

The Board, in its work, looked at each one of those things and really ended up refuting almost all of those points, and they relied on such things as oxygen isotope composition of modern groundwater, the lack of correlation in some of the surface morphological features that would have been associated with hydrothermal activity, the layering of the calcium carbonate deposits, lots of different physical observations, each interpreted sort of on their own terms to cumulatively present a fairly strong case that the hypothesis does not, if you will, hold water.

Now, another approach is through the use of simple calculations, as Cliff described. We also have an example in the Board's 1996 report that Leon Reiter and Victor Pacelscus on our staff then had done, just some simple bounding calculations of--I'm trying to remember now if it was on neptunium--on solubility, kind of the influence of solubility estimates on dose rates. There's a box in the report on that.

We've also done some work in Cape Cod for the Otis Air Force Base, where there was a very rich three dimensional dataset used to calculate dispersivities in 3-D, and with essentially five points of data rather than 640, came up with the same estimates of longitudinal dispersivity, which was the most important parameter there describing the plume. That was another example.

Finally, there are examples outside of Yucca Mountain per se in seismology. Leon Reiter again, on our staff, did some work on earthquake ground motion estimates, and by using theoretical arguments, regression analysis, as well as an actual earthquake, could show reasonably good agreement in the estimates. That's coming at the problem in three different ways that I think you can argue are independent.

So, with that, I will just stop and look forward to more discussion.

CRAIG: Thank you, Debra. Dan Bullen, Board.

BULLEN: Bullen, Board. Actually, I have a very nice speech that Carl Di Bella helped write for me, and I'll skip the front part of it because I've already been introduced a couple times, and most people know my background.

But I want to reiterate the fact that the models and calculations for performance assessment that we develop are being used to support the safety case. And today, we're looking at multiple lines of evidence other than performance assessment that can support a safety case for the repository.

Since I'm interested in both the natural and engineered aspects of the repository, I would like to comment on both, but I'm going to limit myself to the fact that I'm going to talk about the engineered system. We just had a great presentation by our three consultants with respect to the natural system, and so I'll diverge a little bit and talk about the engineered system. If I could have the first slide there, David, that would be great.

As most of you know, the high-level waste is going to be packaged in a very robust container, and this is the 21 PWR assembly that's been developed by the Yucca Mountain project. According to the assumptions and calculations of the latest PA by the project, the waste package alone will isolate the waste for well over 10,000 years. This is a very different mindset than a decade ago when the function of the waste packages were to solely provide a convenient means of handling the waste and helping to provide--remember this quote--"substantially complete containment" during the 300 to 1000 year thermal-pulse period. And I'll get back to a couple of my comments to Mike Voegele about the adaptability of the design and the changes that we've had in the design due to information that we've gathered about the site, specifically with respect to percolation flux. And I'd like to come back to that when we have our open discussion.

I want to point out, however, that there are uncertainties, chief of which by far is the effect of the radioactive decay heat on the temperature and behavior of the waste package and its surroundings. It seems the higher the temperature, the greater the uncertainties. This is why the Board has been urging DOE to evaluate cooler, drier, simpler repositories and to compare them with their current base case.

Now, what alternate lines of evidence might we look at for waste packages? I believe that natural and archeological analogues are actually one of the best ways that can be explored and may be fruitful. Extreme corrosion resistance of the waste package, however, is provided by the outer barrier, which consists of a nickel superalloy called Alloy 22. Alloy 22 contains mostly nickel, with some major amounts of chromium and molybdenum and smaller amounts of tungsten and iron. Hence, the perfect analogue would be a

10,000 year old Alloy 22 coin. Unfortunately, we haven't found one yet.

Alloy 22 resists corrosion because in air, it forms a very thin film of tenacious and essentially impermeable oxide that protects it from attack. In this respect, it is similar to many other metals protected by passive layers, like stainless steel, aluminum and titanium. Thus, any ancient metals we could find protected by a passive layer could assist in understanding how passive layers evolve and behave over long periods of time, and this knowledge might help build confidence in the behavior of Alloy 22 passive layers.

The sixty-four thousand dollar question here is, "Do such analogues exist?" Well, we don't know until we look, and we do acknowledge that DOE is indeed looking. Gerry Gordon briefed the Board on their work with natural nickel-iron minerals, Josephinite and Awaruite, at its January meeting in Amargosa Valley, and we saw some very interesting results in the preliminary analysis that they did at Lawrence Livermore National Laboratory.

If these minerals are indeed protected by passive layers and their age and environmental history can be determined, they would serve as an excellent source to improve our fundamental understanding about how passive layers work over long periods. Meteorites, which often contain iron and some nickel, could also serve as such functions, and I know DOE is aware of this. This is important work and the Board strongly encourages it.

Now, are there other metal analogues? And the answer is maybe. There are many anecdotes about iron artifacts surviving long periods in arid environments. This gives confidence that corrosion will be very little during the preclosure period, but will not help postclosure unless engineers can think of a foolproof way to keep the mountain dry.

The Pillar of Delhi and the Roman nails are periodically mentioned as analogue candidates. I suspect their longevity is due primarily to either arid environments or reducing conditions, neither of which would seem to apply in the bulk sense to Yucca Mountain. Thermal scale on colonial American iron nails, such as those that have been put through a house fire, may yield valuable information about how passive layers survive.

Frankly, I'm not convinced that we've learned as much about the role of patinas on ancient bronzes and their applicability to Yucca Mountain. Surviving artifacts from ancient tombs may aid our understanding of the protective nature of oxide layers.

Now I'd like to move to the second slide, which actually talks about design principles.

Each of the last four semesters at Iowa State, I've been responsible for teaching mechanical engineering students the fundamentals of engineering design. These students are mostly sophomores, and this is their first course where they get a chance to take mathematical, scientific, and engineering theories and facts that they have been digesting during their previous three semesters at Iowa State and synthesize a real world design--well, almost real world design problem. Among other things, we try to imbue in the students the overarching design principles in this design course.

Well, what does this have to do with Yucca Mountain and the theme we're talking about today? Well, if Yucca Mountain goes through the site recommendation and licensing, it will be a combination of natural and engineered barriers that provide the basis for the decision. Many of the design principles apply to the engineered system, and the confidence in the safety case will be increased to the extent that design principles are followed.

The first principle that I always teach is KISS, which is keep it simple, stupid. In fact, I hammer this into the students so much that they often refer to this principle in all of their subsequent mechanical engineering design reports throughout their academic career. Innovation is encouraged, sure. But recognize that introducing many new technologies to a single design will bog it down into a morass of complexity, not unlike the problem that we have in trying to evaluate a very complex natural system, as Dr. Voss just showed us previously.

Now, this is not to say there aren't going to be new technologies. Definitely we'll have new technologies at Yucca Mountain. But first let me define what a new technology is. A new technology is a technology that has not been applied at a commercial scale under similar conditions. In other words, any technology for which there is no close precedent for the problem at hand is a new technology, at least as far as the problem at hand is concerned.

So is there new technology at Yucca Mountain? Yes. To name a few, the technologies that are going to be new are remote emplacement, laser peening, final closure weld inspection, emplacement drift maintenance, retrieval, remote monitoring for very long periods, and maybe better, maintaining remote monitoring equipment for very long periods. What is not necessarily new technologies in the keep it simple paradigm, excavation and ventilation.

Now, flexibility being the second point, and I'm going to cut this short, Mr. Chairman, so I won't ramble on, Yucca Mountain is going to remain open for at least 50 years. And my opinion actually is that whenever the decision point is arrived at to close, the decision is almost always going to be delayed. So 50 years is probably the lower limit.

We can't predict what's going to happen. So the best course to take is to make the design as flexible as possible so that our successor generations can cope with unanticipated situations and additional data, as Mike Voegele mentioned.

Now, before I close, I do want to point out that I also try to teach Murphy's Law. The first Murphy's Law is that if something can go wrong, it will. And the second one, which is analogous to that, if something can't go wrong, it will. Okay?

Now, I don't have time to go into the rest of these self-explanatory issues on the overhead, but I would like to point out a couple of things. Common sense, which is somewhat there in the middle, and then I finally close by reiterating what I feel is a very, very important design principle, you'll see that KISS is at the top, and KISS is at the bottom, I want to emphatically implore you that if you want to have flexibility in design and you want to be able to adapt, you have to keep it simple.

CRAIG: Thanks, Dan. I want to point out that in anticipation of all of this, I have brought a C-22 sample, and then I've got an old C-22 sample and it says Q/A, so it's truly quality assured, and it says 5956 B.C. So this is just what we need.

BULLEN: We have 8,000 year data now.

CRAIG: Yeah, that's right. There's a little concern that the Q/A process wasn't watched over as closely as perhaps was needed. But these are available for your inspection.

BULLEN: Thank you, Professor Craig.

CRAIG: We're going to skip over our three consultants who have already had their say, and move to Jeff Wong, a Board member from California.

WONG: Okay, thank you.

All right, as I've said before often in meetings when I follow Dan Bullen, we should pause for a moment and allow the oxygen content in the room to rebuild up.

When we're poking fun at someone, I'm sure you've all heard the saying, "The light's on, but no one's home." Well, in California, that saying may no longer have any meaning. We will be replacing that saying with, "It looks like he's cooking without gas."

Later on this afternoon, we will bring up the question of what is the relationship between the traditional notions of defense in depth, and multiple lines of evidence when attempting to build confidence in this effort. Now, I don't come from a nuclear world, so I've been asked to give a little perspective from a non-nuclear world.

From my vantage point of having to focus on hazardous waste for ten years or more, is that we are faced with many similar problems that we're discussing today. In fact, I believe that many of the policy and regulatory decisions that we make are often made on much less information than we are demanding here.

Like nuclear waste, hazardous waste will be a danger to the public, public health and the environment far into the future. But unlike nuclear waste, facility designs and containment related to hazardous waste did not clearly take this into account.

It seems that the hazardous waste requirements seem to focus on time frames tied to permit lifetimes, which is only often in 30 year increments. Permitting decisions for hazardous waste are based upon projected risk assessments, which are much like performance assessments in the nuclear arena. Uncertainty analysis has yet to be formally incorporated into decision making. Redundant containment systems such as caps, geosynthetic liners, clay barriers between disposal cells, deep unsaturated zones and leachate collection systems are often used. But the term defense-in-depth is not often found. And things that might go wrong are soon to be detected by active monitoring. In other words, multiple barriers are used. Whether this is purposeful thinking of defense-in-depth is vague, and active institutional management is key.

I didn't work on water valley, so please don't ask me any questions about that. But my most recent experience with low-level rad waste, which is outside of the mandate of the agency that I represent, is that there is great institutional resistance to formally consider uncertainty and to go on to inform policy makers. Scientists have stated that policy makers will not understand such and, therefore, there is no need to spend time in its analysis.

For my agency, environmental protection decisions rely upon the paradigm of risk assessment again, which is much like performance assessment, and I'm not aware of any instance where an independent line of evidence outside of risk assessment was developed to support any regulatory decision.

So in many ways, much of the thinking that's going on here appears to be much more robust than that which goes into hazardous waste. So I think that the struggle here mirrors the struggle that we have in California and the struggle to expand our regulatory thinking. And it's my hope that we learn more here today by talking to each other, and that I have more information to take back home. Thank you.

CRAIG: Dennis, I was advised that you wanted to go last. Is that still true?

WILLIAMS: If you would, that would work for me.

CRAIG: Okay. We'll skip you for the moment. Don

Runnells, Board?

RUNNELLS: Thank you, Paul.

I've been trying to clarify in my own mind what multiple lines of evidence means for the proposed repository at Yucca Mountain. And it's easy I think to talk in general terms about multiple lines of evidence. But when you try to pin them down and apply it to Yucca Mountain, I think it becomes much more difficult. And the multiple lines of evidence that I've been able to scribble out on a piece of paper here this morning are as follows. Corrosion studies. The fundamental mechanisms, particularly the fundamental mechanisms involved in the corrosion of metals. Those studies are I think independent of most other aspects of the program. The more we can understand about the fundamental mechanisms of corrosion of the metals that we're talking about, the better we will be, and those can be brought in at many points into the program, including performance confirmation aspects of the program.

As part of that, I think the stuff that Dan Bullen mentioned about the understanding of passive layers on ancient metals is important. We've talked quite a bit about things like Josephinite and why that particular metal seems to be hanging around for a long time. If we could understand that, we might have an independent line of evidence for understanding, for predicting the behavior of C-22. So I would put emphasis if I were directing that aspect of the program, not just spending effort and time and money on understanding these passive layers on ancient metals, and I agree 100 per cent with Dan Bullen on that.

As an aside on that, though, you must understand the geologic environment in which those things occur, and I'm not convinced at all that we understand the geologic environment of the occurrence of, for example, Josephinite, or when were those nuggets exposed, how long have they been in the stream. This is a site in Oregon, as probably most of you know. Have they been above the water table? Are they in ancient terraces that periodically get rinsed into the creek? The geologic environment is very, very important for trying to extrapolate these ancient analogues to a modern situation.

The natural analogues, it's a favorite topic of mine, but I think I recognize the dangers in trying to use natural analogues for Yucca Mountain, and that was emphasized particularly this morning. The differences between other sites and Yucca Mountain must be recognized, but I still think there's a lot of useful information to be gained, particularly in the transport mechanisms. How are the radionuclides moving in places like Pena Blanca? Not necessarily the analogy with, let's say, the climate as much as the mechanism of movement of these materials. And there are sites in Brazil that have been studied for this same purpose, there are sites in Africa that have been studied for the same purpose of determining the rate of movement over a period of time. We must understand the controls on that rate of movement from the metal controls.

That leads me to a point that Bill Murphy mentioned several times, that is, the secondary minerals. We're putting essentially uraninite into the ground at Yucca Mountain. Uraninite is a reduced form of uranium, stable under reducing conditions. We're putting it into an oxidizing environment. Secondary minerals will form, and the secondary minerals in all likelihood will control the solubility and rate of release, and perhaps even the periodicity of release, not continuous release, but perhaps periods of release, all of the radionuclides, that is, these materials will come out of secondary minerals.

I know there's work ongoing at Argonne Lab on this. I'm glad that that's ongoing because I think it's a very important consideration, and I think natural analogues can teach us a lot about those secondary minerals, as Bill pointed out, particularly for Pena Blanca.

finally, I guess in terms of my list this morning of multiple lines of evidence would be the use of existing mine excavated cavities for understanding the movement of water in the unsaturated zone. Now, the world has millions of mines that are available for study in terms of how does the water move, how fast does it move. To be sure, only a few of those are in environments similar to Yucca Mountain. But there are--well, few is too weak--there are many mine cavities in environments similar to Yucca Mountain throughout the western U.S., and I don't think the project has made adequate use of that independent line of evidence about the movement of water in the unsaturated zone, including--and I know they've looked somewhat at the sites at the Nuclear Test Site, the excavations at the Nuclear Test Site which are available. I would consider that to be quite an independent line of evidence.

So, I guess, Mr. Chairman, that's sort of my list, going down and asking myself what are some independent lines of evidence that we could use at Yucca Mountain, quite independent of TSPA.

Thank you.

CRAIG: Thank you, Don. Bill Dudley, USGS.

DUDLEY: Well, my training, interest and experience is in the earth sciences rather than engineering, and that is, of course, a major part, but not all, of TSPA. The earth sciences rarely have examples where they can use direct observations, measurements, feeding them into direct calculations of any particular result. We rarely have the luxury of unambiguous approach to problem solving.

Rather than a direct analysis, we usually have to rely on weight of evidence to make the points that we're testing. Dr. Knopman mentioned the analysis of the Szymanski arguments relating to hydrothermal upwelling. This is a wonderful example of just the convergence of many different approaches, multiple approaches, or weight of evidence.

The multiple lines of evidence can be used also to develop input to performance assessment modelling when necessary, when there are no ways of measuring directly. The confirmatory or in some cases detracting lines of evidence, rather than necessarily being multiple, I like to refer to it as just other lines of evidence, and I'd like to use the first input to a performance assessment model, that of infiltration, as an example of a number of ways that do converge, but are certainly secondary to the direct or calculational approach to providing input to performance assessment.

This is just a fragment of the total TSPA, but it's an important fragment because it is the initial source of water that is then routed through the system in the performance assessment calculation.

The model that is presently being used to provide this input to TSPA is a combination of deterministic and stochastic consideration of basic hydrologic processes. It obviously considers precipitation as rain or snow, and it is based on measurements over a 15 to 20 year period directly in the Yucca Mountain area, and much longer than that in the region.

It includes the return of this moisture to the atmosphere as evaporation, sublimation of snow, or transpiration by plants. It involves at least estimates of surface run-on of moisture and run-off, which in many cases can be measured or determined indirectly with relative precision. And it applies a number of other factors that cannot be measured directly, such as the effects on run-off and infiltration of surface slope, the soil and rock, and the vegetation on the surface.

The model that puts all this together then is simple calculationally. It's plus signs and minus signs. Conceptually, it's reasonably complex, but it certainly is a small part of a total performance assessment calculation.

The infiltration model that is used for input to TSPA provides an estimate of an average of about 4.6 millimeters per year of infiltration over the repository area, that the range then is from something around 1 to something around 20, depending on the slope, whether there's fractured rock at the surface, all these other factors that have gone into it.

But the 4.6 does come out to be roughly 2 1/2 per cent of the estimated precipitation based on this 15 years of measuring, which provides about 190 millimeters of rainfall.

That's about seven and a half inches--or precipitation, excuse me.

Some of the ways, the other lines of evidence that we can use to test this, certainly the chloride mass balance is one that's been mentioned quite a bit, and this provides roughly 1 to 20 millimeters at various places over the repository footprint area. Calcite deposition in the fractures, the infiltration of water producing calcite there as opposed to an origin by upwelling hydrothermal fluids, provides a similar range of 2 millimeters to 20 millimeters, and as stated, the calcite that is estimated to occur in fractures in the Topopah Spring member is about 6 millimeters.

Similarly, just to maintain the perched water bodies that have been identified calls for certainly more than one, but less than about 15 millimeters.

Indirect or an independent line of evidence also is temperature, geothermal temperature. There have been analyses of the temperature profiles, attempts to match the temperature profiles by the infiltration of cool water of known heat capacity in UZ-4 and UZ-5 in kind of the northeast part of the repository area, and these have provided estimates of infiltration of roughly 5 and 15 millimeters per year.

If we look at the site as a whole, John Sass and his colleagues noted that the heat flow deficiency for the unsaturated zone over the site is between 5 and 10 milliwatts per square meter. It's much greater than that if we consider both the saturated and the unsaturated zones. And using a very simple equation again that is the, I guess the sweeping of heat downward by infiltrating water, this one dimensional analysis suggests that infiltration is 3 to 5 millimeters per year. These all seem to be converging pretty well within this range, as opposed to the earlier estimates back in the early and mid Eighties, which were based on worldwide studies of infiltration in areas having similar rainfall, and presumably similar geology, and then we were estimating the infiltration and recharge more like .1 to .5 millimeters per year.

In order to provide a sanity check, we can almost use the reasonable information around Yucca Mountain, or southern Nevada, as an analogue. In this case, we have a pretty good idea over long periods of time what the rainfall, snow fall is, its distribution with altitude, north, south, east and west, rain shadows, and so forth, and we have a reasonable measurement of discharge from this system.

Back in the last 1940s and early Fifties, the Maxi Egan method of estimating recharged based on elevation, rainfall, or precipitation, again was developed, and this has been worked and reworked, and adjusted, and so forth, until it's probably a pretty reasonable way of determining how much water gets into the system. Water coming out of the system luckily in this area of relatively closed hydrologic basins, we can measure directly. Many cases, or over much of the basins, the water table is so deep it's out of the reach of evaporation, so that we don't have to worry too much about that type of discharge from the system.

The Ash Meadows system, which is just to the east of Yucca Mountain, has been well characterized and includes some of the higher areas of precipitation, like the Spring Mountains. It's 4,500 square miles, has a discharge, depending on how much you want to allow to go under the Death Valley of 25,000 acre feet per year to 33,000 acre feet per year, and lo and behold, these work out to a range of about 3 1/2 millimeters per year, down to maybe 2.7 millimeters per year on average. Now, this includes of course the high areas, such as the Spring Mountains.

The basin, or the area to the west, the so-called Piute Mesa system, which also includes Oasis Valley and the Alkali Flat, Furnace Creek Wash system within which Yucca Mountain sits, is about half that size and has about two-thirds of the discharge based on the unit area. So that we are looking here again at 2 to perhaps 1 1/2 per cent of precipitation.

So there is a reasonable convergence, a reasonable agreement of the other lines of evidence with the calculational model, with the first part of the performance assessment calculation. I'll stop there.

CRAIG: Thank you. Bob Andrews has probably appeared before the Board as many times as anybody, and we're happy to have you back.

ANDREWS: I don't know if that's a good statement or not.

CRAIG: That's a good statement.

ANDREWS: Okay, thanks.

I want to talk about a couple of things, and build off some of the things Debra talked about about confidence building, and I'll change it from confidence building to confidence challenging, because I think we are all faced with challenging, you know, our beliefs, challenging our information, and challenging our assumptions as we go through the process. And I also want to talk about this as a process. We generally draw a line in the sand and say at some point, you know, when are you confidence enough, or when do you have enough information. But, in fact, we as scientists and analysts have a continuum here of ongoing work that started a long time ago, and probably will continue, you know, for a while in the future until decisions are made. And that process I want to talk about with a couple of examples, and being a performance assessment person, I will have to take at least one example from performance assessment, and talk about Olase--I love that one now--Olase associated with total system performance assessment, which I think is appropriate, because the total system performance assessment is the one place where you integrate many processes and process interactions that you don't necessarily integrate within any individual component.

Taking Dr. Voss' example, any one fracture observation, or even hundreds of fracture observations, is of limited value until you integrate all the fracture observations together to try to understand how the flow regimes may actually occur. Some limited observations have to be integrated into a conceptual picture of the whole. And that's what, in fact, TSPA is trying to do, is integrate a lot of little pieces of information, some of them complex, into a picture of the whole.

So let me talk about process. The process, why do we at some point in time have confidence and challenge our confidence is in fact because we've gone through many iterations, and I think scientists go through many iterations as they develop their hypotheses and test their hypotheses. Within performance assessments on the Yucca Mountain project, they started in the late Eighties to continue through I think six total iterations now of system and subsystem level performance assessment.

At each point, the performance assessment analysts are of course challenging their hypotheses and assumptions, and others who are looking at those performance assessments are challenging the assumptions and the approximations. Additional information is collected. Additional models are developed, refined, and another iteration is conducted. And, again, additional knowledge gained with respect to how the system might perform.

So those iterations, that time sequencing of knowledge, if you will, and evaluation are an important I think line of evidence that we sometimes lose track of.

Another aspect is not just how the system behaves, but understanding why it behaves the way it does. Try to peel off the layers of the onion in any particular analysis or model, and try to understand, and can you explain why the system behaves the way you are projecting that it might. And this applies to a system as well as it does to any particular subsystem.

What makes the infiltration tick? What are the important factors that drive infiltration? What are the important factors that drive the connectedness or disconnectedness in a fracture network system, you know, in a Swedish mine? Trying to understand that and peel off the layers of the onion through a lot of sensitivity analyses, a lot of barrier analyses--this is PA talk now--but I think every modeler does that to try to understand why their model is the way it is, and can they explain it the way it is. And I think that's an important part of all analyses, all models, to try to understand what it is we're observing or projecting to occur.

Another important line of evidence, and this is still in the PA realm, is comparisons. These are comparisons to other groups, other individuals that are doing similar or analogous work. In our country, we're somewhat fortunate to have two other groups who have done system performance assessments over the same time frame as the Department of Energy has sponsored them, those being EPRI over that same time period from the late Eighties until now, and NRC has conducted four or five iterations of integrated performance assessments over that same time period.

Those results are compared. You know, we on the Department side, look at the results of EPRI and NRC and try to understand why their results are the way they are, and I know EPRI and NRC, as the Board does, looks at our results and tries to figure out why they are the way they are. And generally we can explain the differences. I think the NRC has recently--they're still in the process, of course, of reviewing our document that was released last November or December, and they've pointed out in some of their public comments to ACNW areas where they believe we're conservative, and areas where we are different from their particular approximations. I think that's all good. You know, we're all testing each other's understanding of the system and understanding of how the system works.

And, finally, reviews. Review is an important part of confidence building or confidence challenging. Those reviews can occur at an individual component part, or those reviews can occur at, if you will, the integrated system level. And those have occurred within the project, and the project is undergoing another international review of the TSPA starting I think in June. Abe could give you more details. So that's the TSPA part of the Olase.

I want to also take a look at one example, one component part. I think Bill did an excellent job on infiltration. The one I'd like to pick on is another important performance driver, and that's seepage, and put it into the context of learning and knowledge gained and revisions. The performance assessments, the models for seepage in the early Nineties were very simple. They were analytical, taking literature information, first principle information, to try to come up with an estimate of a range of seepage as a function of average percolation flux and rock properties. But they were very simple. There was no direct observations at Yucca Mountain of seepage. In fact, the ESF was being drilled at that time. There had been no testing conducted, site specific testing conducted of seepage.

So you could argue it was assumption driver or you could argue that it was based on first principles of physics in flow-through unsaturated media, Phillips kind of relationships. Clearly, a lot of questions were raised. You know, when PAs were based on a very simple seepage representation, saying well, you don't have site specific information, and it's true, at that time, there was no site specific information. It was collected through a wide range of tests, underground tests at the ESF, and surface to underground tests that allowed LBL scientists to develop a first-cut model which was used in the viability assessment in '98.

People pointed out, and that was a fairly complicated model, but people pointed out correctly that it didn't include some variability aspects, didn't include heterogeneity aspects that could be important to seepage, and some other things. So ongoing testing occurred, and they modified the model. I think the Board has been presented, and others, the details of some of the model enhancements, additional science, if you will. And so the model changed, and the model changed as what we've used in last fall's report.

People raised questions about that. They raised questions of, well, what about drift degradation, what about uncertainty in drift degradation, what about uncertainty in coupled processes, which may drive changes in seepage. And the project acknowledged that some of those uncertainties, in fact several of those uncertainties were not included in that particular representation.

So additional uncertainty analyses are underway right now to better quantify maybe a fuller range of possible seepage. But it's all in the vein of increasing knowledge. In this case, it's in fact increasing complexity. But increasing knowledge based on additional observations that challenge and test and push the models, both the simple models and the complicated models, so we have or try to have as reasonable a projection of each individual component part as we can.

With that, I think I'll stop.

CRAIG: Thank you. Richard Parizek, Board member.

PARIZEK: I've always said multiple lines of evidence was important. That's sort of a belief I've had. I think serving on the Board, you see the important aspects of that. But then we think about, well, how do we test this? I mean, it's one thing to say that. It puts us in the hand of DOE and say go do it, and how convincing can they be when they do it?

Dan talked about his class and what he tells his class. Well, in terms of model development, I tell my class the same sort of thing. I say, one, in model development, which ultimately leads to something like the total system performance assessment type model, we start somewhere. We start first with a characterization of the problem we're trying to deal with, the conceptual model. We've got to create this conceptual model and do that very well. This includes climate, uncertainties about climate change or rate of change, it deals with metals and all the uncertainties with it, and so on and so on. So there's a whole series of things that go into the conceptual model development.

And as we then decide what numerical method should we apply, what model type should we pick, and then what assumptions we have, or do we have to create new models, and we go through that whole process, and here's a case where all kinds of data go into that development in order to be able to calibrate a model. We finally get a model, and we're going to calibrate it, and the calibration process, we've heard from the first talk this morning, uses up everything we've got, really. It's got all of geology, it's got all of the climates, it's got everything in it that we have available to us to create that calibration.

Having done so, another TSPA comes out and you can redo it and refine it, and so we've heard how many times this has been done. So this is good because we're getting better at it. But finally we get to the point and say, well, do we have a model now that's validated. Can we validate it? That's really where we are. Can we now use the TSPA to say everything will be safe for 10,000 years, no problem, and we're all very comfortable with it. Or to what degree can we say that?

And we've heard two elegant examples of the preponderance of evidence argument. Well, and we heard from Debra about how you add it all up and say, in terms of the hydrothermal upwelling thing, it doesn't look to credible based on all the evidence we've had, or from what Bill Dudley here talked about, all of the lines that constrain what the infiltration amounts could be. I mean, that's pretty convincing stuff. I mean, you can go off on a tangent and say I think I have some other numbers somewhere, but I don't think we can support them based on all the evidence that's been put together from that point of view.

But here we want to validate the model, and I guess one way to validate the model is to go ahead and put the waste underground and wait and see if it fails. That's kind of a dangerous approach because maybe--you're going to monitor it, and we can at least make some mid-course corrections early on in the whole process. So model validation is the hard part right here. And how comfortable do we have to be in our model validation to do that? Or did we do it? TSPA '98 was validated maybe by the site recommendation model, and maybe by the LA model. In other words, you made a forecast and now you're trying to compare how the next iteration was improved. And, again, is that validation or is that just refinements of the calibration process?

And so I'm left with this problem at the end, when did we validate the TSPA model? Going to analogues would be another approach, but the analogues have already been embedded at a number of places. We've heard this. It's been built in at all sorts of levels in the modelling process to date. What we would like is an independent one. And can we get one, or do we have a Yucca Mountain somewhere? And we've heard some suggestions that there may be natural deposits of uranium that are still there and we can use and draw from that in an important way, as Bill has pointed out. But we'll probably never find one that's exactly a Yucca Mountain, will we? So the analogue can't be perfect that way, but there are pieces of it that we can actually draw from from a field point of view.

So we're looking then for inconsistencies in all of this. I said, well, you know, there's going to be tons of colloids produced in the mountain as the wastes are released, and we're looking around for how to move colloids in the subsurface, and at various times I've pointed out I don't see the models putting colloids in, transporting them through the unsaturated zone, because nobody seems to find any colloids moving through the unsaturated zone. At least the Busted Butte experiments give us some trouble, and we said, well, look in the secondary minerals. Do we have colloids trapped there? We've got all these calcites and other things showing veins, but it seems as if no one has found colloids in there, except maybe in a gellitous form. So maybe there's inconsistencies about worrying about that part of the problem. Maybe colloids don't move in the unsaturated zone. Once we're in the saturated zone, things can attach to colloid particles and they might move. That might be possible.

One of the things that says, well, early TSPA '98, revised, had a plume in the groundwater sense travelling in a very dilute and dispersed manner, and the program took criticism, and then we see now a very pencil thin plume instead. So we have the skinny plume, whereas, before we had a good plume in the sense it was good dilution. You can look at it that way. But a skinny plume versus a broad plume is quite different.

We look at Forty Mile Wash chemistry and from the various things done by the USGS and others, it seems as if there's still a dilute mass of water coming down below Forty Mile Wash that suggests some spreading is occurring there. Now, spreading in this case could broaden and dilute out a contaminant plume. On the other hand, maybe the pencil thin plume that would come from the repository footprint would always be kept at bay and always kept to the west and wouldn't enjoy this sort of mixing. So we're looking for places where there's some inconsistency in the thought process, or in the modelling process to date. So this is sort of a challenge that I have.

So, one, then assumptions matter. No juvenile failures in 10,000 years, and why worry about all of this; right? We're sort of home free. On the other hand, we've got to challenge I guess those kind of assumptions and make sure we haven't fooled ourselves into accepting something that may be quite far from maybe the reality. And so I throw that in, and assumptions of climate change and the timing of it, and so on, these are sort of things that may be drivers that we have to also understand and make sure we feel credible about those.

So have we validated the model? If not, can we? How close can we get to it, or when are we satisfied? Actually, the preponderance of evidence approach does buy us a lot of comfort. If we go with expert opinion, that's surely important. The question is will those expert opinions go through the process that help us submit various people through, you know, to make sure the biases are reduced and all the rest of it. So from external inputs to this whole process, what comfort can be get from the expert opinion approach?

And a problem this complicated is very hard to analyze in a simple way. Rod talks about the difficulty he would have trying to take all the data and make some sort of a determination about it. So the expert opinion approach has to be very carefully looked at, because it's not a casual exercise. It's a very severe exercise.

That's probably enough.

CRAIG: Okay, thank you, Richard. Ardyth Simmons from LBNL.

SIMMONS: Some of what I had thought about addressing today you've already heard from other people. But I'd like to repeat just a few points that I think are key.

My perspective is one of trying to pull together many of the multiple lines of evidence for a project, and that can be quite a challenging task. So I've been thinking how is it that the project has used multiple lines of evidence and how should it use these, and there are a number of different ways.

First of all, it's important for every aspect of the system to try to draw on as many lines of evidence as possible. It's the preponderance or the weight of evidence that is important, and that builds a case. I don't know if we can ever completely validate a model, but to the extent that we can use as many lines of evidence to support our models and to support not only numerically, but conceptual models as well, we will have greater confidence in them.

Furthermore, because sometimes lines of evidence can be conflicting, and you've heard some examples of those already, it's important to have not only as large a number of multiple lines as you can, but also depths with which you can trace a single strand of evidence.

Now, how have we used multiple lines of evidence? Well, in some cases, we haven't been perhaps as clear or as good in bringing them out as we might have been, but much of what was done throughout the course of site characterization used multiple lines of evidence.

One area that hasn't really been touched upon today is volcanism and other disruptive events models, including seismicity, and those rely largely on analogous situations in the great basin over long periods of time. But we didn't really call those multiple lines of evidence. The paleohydrology scenarios are another example, and you've heard a little bit about those today. Seepage is the third.

So our challenge is to explain how we've used all of these lines to build a stronger robust understanding of how this site would behave over long time periods.

Now, I would say that multiple lines of evidence can be used in both a qualitative and a quantitative sense.

And one area where I think we need to do a little bit better job is in communicating how we've used the qualitative lines, particularly the analogues.

In the past, we've used a rather restrictive definition of natural analogue, because in conducting our quantitative studies we wanted to pick as close a situation to a particular process that's occurring at Yucca Mountain as we could. And so we applied rather restrictive criteria, how close the site matches geologically and hydrologically. In the case of transport analogues, what is the suite of radionuclides that's there. How well do we know the boundary conditions, and the initial conditions? And these are areas where analogues often have many uncertainties as well, but when one is selecting an analogue to study with respect to a particular process, you want to try to hit as many of those as you possibly can.

So we perhaps have given the impression that we've used analogues in a very restrictive sense, but if we look at all of the examples that have already been given today, I think we can say that that isn't necessarily true.

So how do we bring all that together? Right now, we're trying to do that through the reports that are being prepared at this time with regard to the supplementary science and performance analyses. And for each process model and subprocess model in those reports, we've tried to clearly bring out lines of evidence that hope to test our models and to confirm those.

Are they developed independently from performance assessment? Not if you use the definition of performance assessment to include all of your understanding that goes into the building of a model, including your conceptualization of it. But is it independent from the total system PA analyses? Yes. That doesn't mean that it has to be. I think Bill Murphy gave a great example of how you can use analogues or other lines of evidence in a performance assessment. But most of the ones that we've used have been independent and supporting lines.

And we now may have analogues, but we've used some simple calculations in the area of waste package performance. This is probably a good example where we don't know as much quantitatively as we would like to perhaps about the role of passive films, but we can say something about how metals have behaved in the past, and we can do some simple calculations. These have been done by other countries as well, and one notable example in which this took place was the use in the Swiss program of calculating waste package corrosion based on metals that are found from natural analogues. So we have some bounding ideas of what those rates would be.

And then I'd like to bring up sensitivity studies as a multiple line, and how those are used in our process models. And one example there, we haven't really talked too much about coupled processes yet in this meeting today. Coupled processes are something that our understanding of has been maturing in over the whole duration of the program, and it's been a challenge because of their complexity to incorporate them into both the numerical process models, and then of course into TSPA.

But what we can learn from multiple lines of evidence in the case of a thermohydrologic mechanical model, for example, is that at natural sites around the world where heat has been put into an underground facility or where it's a naturally heated facility, you will see changes in the thermohydrologic mechanical properties. When we try to put those into a model, we see that the flux rates, for example, at Yucca Mountain that we expect to occur would be so small that the effects of the mechanical changes would be much smaller than warranted to incorporate them explicitly into our model. So this is the case where a sensitivity analysis can show that the models we're using are not sensitive to the coupled effects.

And I think I'll stop there.

CRAIG: Thank you, Ardyth. Let's see, at this point, it's now 11:20. Dennis. I forgot Dennis. We're going to break at 11:30 no matter what, because otherwise the restaurants will get filled up.

The public, are there members of the public who would like to say something? Judy Treichel's hand is up. Judy, if we fail to get to you before lunch, we will get to you first thing after lunch.

TREICHEL: By 2 o'clock.

CRAIG: I promise. And if I start to fall short, yell at me. Dennis?

WILLIAMS: Thank you, Mr. Chairman, in part for allowing my people to go first. I was here as kind of a batting cleanup on Steve Hanauer's eight boxes that were presented, and as such, I will talk a little bit about expert review, direct observation, and demonstration.

Before I get into that, I wanted to mention again Steve Hanauer's presentation, and on Page 3 where he has the spectrum of color, and I think that after I'm done you will find that my remarks probably tend to be over in the far blue side. So I'll offer that in the beginning.

Often times, a line of evidence that is most compelling to the scientist/engineer, and likely based in mathematical calculation, fail to be compelling to a particular constituency. The scientist/engineer will gladly pursue the mathematical approach because it fits in the framework of their analyses and reports. They, and then I add we, because I am one, may not recognize that an equally compelling argument can be made that is not expressed mathematically. Consequently, it tends to be difficult to find the non-mathematical arguments in the standard scientific and engineering documents, such as those that we have on Yucca Mountain. And I think a perusal of the objective evidence will bear that statement out.

This does not say that we have not pursued multiple lines of evidence or perhaps--and I prefer that as the term. I didn't even talk to Abe before this meeting, but he was using reasoning; I was using inquiry. I use a little bit of a word comparison process to get to that inquiry versus review, process versus product, journey versus destination. So some of my comments will be more along the lines of the journey.

Again, inquiry being a better term in an effort to gain a better understanding of the physical processes at Yucca Mountain and how these processes will influence the engineering components of the geological disposal system contemplated, however, these lines of inquiry are often difficult, again, to find documented in our work products.

Some of the lines of inquiry I will speak to are not amenable to mathematical expression, but have been used widely to provide individual and collective confidence. I believe exposure to and understanding of many of these issues, again, the lines of inquiry, we have discussed and continue to discuss today, is part of the reason why the scientists and engineers who work on this project do show a high level of confidence in the total effort.

The lines of inquiry outside the realm of hard mathematical arguments include statements by respected practitioners of complex issues, often in the form of peer reviews, and the direct observation or demonstration of things that do in fact work and work safely.

As such, I will speak to peer reviews, observational evidence, and demonstration projects based on examples from the international arena.

Peer reviews, Bob Andrews mentioned that we are finishing up on a TSP peer review through the IANEA--no, we are starting one on the TSPA. We are finishing one up on the biosphere.

I would also like to mention waste package materials peer review, which I believe has an international flavor, in that a web based system will be used to solicit comments and observations from the international community. We feel this will leverage our information gathering ability, and will allow us to work within the framework of our quality assurance program and procurement guidelines established by our regulations.

On international interactions, we could go into a lot of that. First notable, Bob Levage advised me was for RW dates back to the Strepa Project with Sweden back in 1977.

Since that time, we have been involved with numerous countries and international agencies, to include IAEA, the NEA. There are times around the office where my managers kind of wonder if Abe, myself, Bill and Bob are really working on the international program and not on Yucca Mountain anymore.

An activity most relevant to current issues is the evaluation of coupled processes, Deco Valex, that is, development of coupled models and their validation against experiments in nuclear waste isolation. Deco Valex-3, Task 2 is modelling of thermohydrologic, thermohydrologic mechanical, and thermohydrologic chemical processing using data from the Yucca Mountain drift scale test. Research teams from France, Spain, Japan, Sweden and the NRC are involved in this modelling task.

I believe that the very process of working with other research teams on this common project increases confidence internally, and perhaps externally as well. We have to be confident, we ourselves have to be confident before we can inspire confidence in others.

Demonstrations and observations. And this first one was very compelling to me. It was an observation at the AECL's rock laboratory in Canada, the mathematical analysis of the three dimensional stress field, it's a very anisotropic stress field up there in the rock mass that resulted--it resulted in various modelling exercises that they went through of tunnel configurations, the whole analytical approach to that. It was very interesting.

However, the impressive part that really locked in my memory was actually standing in that elliptical shaped tunnel, which was accommodated--that was excavated to accommodate that stress field. And that experience, which represented to me the most compelling line of evidence, is very difficult to capture in documentation.

--and it's in a very recent document, disposal of spent fuel in bedrock, December of 2000. It's a 147 page document describing their program for research, development and technical design for their preconstruction phase. It includes a 30 page section on their safety case. Their safety case reads like the NEA definition of a safety case, in part, a collection of arguments. And one of the things that I like to do when I'm talking to folks, I put a compelling word in there, a collection of compelling arguments.

Many of their arguments are based on the KBS-3 repository concept of the Swedish program. I submit that the long involvement in Strepa and ASPO, and the many times that they went deep underground in these demonstration facilities inspired confidence in them, such that they would confidently move forward with their program of geological disposal.

Again, this is not easily captured in print, and may not be readily recognized to a reader of this particular document unless they, like myself, have actually gone underground at ASPO.

In summary, I would like to say that some of the most compelling arguments derived from various lines of inquiry are those that create a mental image of accomplishment, of safety, of being able to do the right thing. However, they are not easily articulated in the reports of a technological bureaucracy. As has been pointed out by the Board, this is the task that lies before us.

Thank you, sir.

CRAIG: Thank you, Dennis.

We're going to break for lunch. Dan is arguing for Judy. I want to get my comments in here sometime, too, but I'll do that after lunch. Judy, come on up.

TREICHEL: I thought we were doing it after lunch.

CRAIG: Do you want to do it after lunch?

TREICHEL: Yes.

CRAIG: Okay, Judy wants to do it after lunch.

Please come back here in one hour precisely.

(Whereupon, the lunch recess was taken.)

A F T E R N O O N   S E S S I O N

CRAIG: My comments are brief. I think I have about four points here. The first one had to do with the issue of confirmatory testing and its relevance to multiple lines of evidence. I guess, this is directed toward Michael. I don't think it has any; just lay it out on the table.

The country is facing a decision as to whether or not to go with Yucca Mountain. In order to make that decision, there has to be a positive decision. The has to be enough evidence to be convincing to the people who make that decision. And, that's a go/no-go decision. It is only reasonable to consider further evidence in the context of that decision if there is a credible pull-out plan if you get evidence which is really bad. If you don't have evidence it's really bad, of course, then you don't have a problem, but if you do have evidence and you don't have a credible retreat problem, then it doesn't matter whether you have the evidence or not. And, thus far, I am not aware of any statement of the kind of evidence that would cause a reversal after the decision is made. Maybe such exists, but I'm not aware of it. If there is an example of such information that would cause the Department of Energy to say we will pull out if such-and-such is found, that would be important. It would be relevant. But, of course, it would have to be combined with enough additional information to make it clear that this isn't just a promissory note, but there's actually some credibility behind it. And, that's tough, that's tough, given the instability of institutions. So, that's the first point.

The second point is I'm very much intrigued by the idea of alternate groups doing the analysis. That was brought up by somebody. I think it might have been Rod. But, it's shown up several places. For example, the work at EPRI, the work that John Kessler is doing is very, very interesting. It's using the same database clearly, but it's using it in a different way with a different set of people with different sets of motivation. And, their latest report, their Volume V, I found absolutely fascinating. It gave high marks to DOE in some areas and low marks in other areas and generally showed up as a very credible way to bring different kinds of analytic tools and different types of thinking to the problem. Now, whether that should count, I don't know, but I certainly think it should be considered in the list of things that might count. You know, I'm not going to give examples of that, but we have a bunch. And, it's well-worth reading and it's a well-written volume, too.

The third point has to do with the models as they relate to the unsaturated zone. It's interesting that there are very specific predictions made for what should happen in the unsaturated zone in tunnels, bores, specific predictions for the ECRB. And, the concept of making predictions in advance and then testing them is really powerful in science and it's different from the standard performance methodology, the TSPA methodology. So, there were predictions as to the kind of seepage that should or should not occur in the ECRB and now evidence is beginning to build up. There's an enormous amount of ambiguity about the date which is appearing, so far, and there are great problems because the instrumentation shorted out because there was more water than was anticipated. But, there's still, as far as I can make out, no agreement, whatsoever, among the people knowledgeable in the field as to where that water comes from and that seems, to me, to be a real important kind of a question which to my way of thinking does fall in the realm of a different kind of reasoning than the TSPA type of reasoning.

And then, the last point I want to make relates to the metals. The shift from the .1 millimeter per year to the 1 to 10 millimeter per year infiltration is really important and it was the main driver behind the enhanced importance of the metals and we have this terrible problem that the metals don't exist for a long time and there is a long experience with systems that are well-engineered from the point of view of the engineering community that, nevertheless, ran into trouble. The Tacoma Narrows Bridge that fell down a couple of months after it was built a half a century ago would be an example of that. The German Intercontinental Express train where the brakes failed and it killed a lot of people would be an example of that. The list goes on and on.

Don Runnells gave me a piece of copper that's a billion years old. So, I have not the slightest doubt that copper exists for very long, long times if it's in the right kind of environment and is thermodynamically stable. But, I'm not aware of anything analogous with respect to these new metals. That's a deep concern. That's a very deep concern and becomes deeper as the importance of the metals keeps increasing. And, it seems to me that that is a question that just has to be addressed in a compelling way or you've got a very deep issue for the repository. Here they are, they're back again. I wish my sample could actually pass DOE's QA tests, but I'm afraid it's not going to make it.

So, that's my abbreviated list of comments. At this point, I'd like to call--go ahead?

WILLIAMS: I'd like to offer a comment perhaps to clarify the record with regard to the cross drift and the water. I don't believe it was a water accumulation shorting out problem; I believe it was, in fact, a human error of shutting off--flipping a switch off. Now, I know that the newspapers have other stories, but the stories in the newspaper are not correct.

CRAIG: Oh, okay. Thank you very much for clarifying that, Dennis.

WILLIAMS: Okay. You're quite welcome.

CRAIG: Ed asked whether I feel better or worse which I will not respond.

Judy Treichel?

TREICHEL: Well, it's a confidence builder, Dennis.

I guess a lot of the word games with this whole discussion are very difficult and, of course, it shows that I'm not a scientist, but I don't think the words "lines of evidence" or the word "evidence" in that phrase can be interchanged with reasoning and inquiry because people have their own favorite ways of putting it. I think evidence is very different from reasoning and it's very different from inquiry and I think that's part of the reason why this whole project is so difficult for the public to understand. Then, the public sort of gets berated for not seeming to have a good understanding of the thing. It's a very circular sort of deal.

But, one of the things that comes through is it seemed to me that this is the kind of discussion and the sort of meeting that should have been held very early-on. You're sort of deciding about how things should be pulled together in order to figure out whether or not you can say that the site is suitable. We're already to the point now where if you asked the question "what could you find that would disqualify this site or knock it out, the answer now is nothing. So, trying to determine now about the lines of evidence or the sort of stuff that you have to pull together really seems quite late and it sort of fits in with the whole scenario that the program has been accused of where a political decision is being made and a whole bunch of scientific jargon is being backloaded onto it to try to justify this decision and say that we've come up with something scientifically viable, suitable, licensable, all of the kinds of words that are used.

In the presentation that was given that talked about Sweden, there's an awful lot of geology there and not a lot of the things that we hear. I think from a public standpoint, it's pretty interesting that we're hearing concern about people in 100,000 years. That's one of the things the public has been fighting for here is to have a regulation--well, just to have regulations would be interesting--to have a regulation that carried on out for the dangerous lifetime of the waste for a million years or something that is in some way related to the waste that it's supposed to be regulating.

It's almost as if from a public standpoint and from the discussions that I have with people who call in or who choose to come in and sit down and talk, it's like the geology sort of got to be so tough or this whole idea of trying to figure out how the Great Basin works, how Yucca Mountain works, how the UZ works, how anything works, just kind of got to be a little overwhelming. So, we went back to something that we could actually see, feel, and do something with which is the canister. That's kind of the way that it sort of looks to people that Yucca Mountain has kind of become a garage and you're coming up with this Lexus, you know, that could be parked in the garage and is going to be fine for all time.

It's always interesting when you sit up at the table and talk about increasing your confidence in the decisions that you're making because, I suppose, in the final analysis, it's going to come down to your confidence versus Nevada's opposition. And, they're never going to come together. So, there will be a decision made about that and I think it's--I'm not sure how, as Bob Andrews said, you know, how confident is confident enough? That's going to be a hard question and it may not really be important in the final analysis.

I guess, just to finish up that, I would have to say, as I've said many times before, you don't know enough yet and you're really not ready. You know, had it not been for the smoking memo, we would have already gone past the consideration of site recommendation and probably the public hearings would have been all done already. Here, we're sitting with a very preliminary discussion, preliminary to even beginning to figure out how to do a suitability determination--and I think it's a good discussion. I certainly wouldn't say that it shouldn't happen, but it should have happened a long time ago, or even better, the program has got to stop and wait for things to catch up. So that, as Rod was talking about, we know whether we're talking about geologic disposal or an engineering model that takes care of waste management rather than permanent disposal.

So, thank you.

CRAIG: Thank you, Judy. Are there any other members of the public who would like to say something?

(No response.)

CRAIG: Seeing none, we will now begin the afternoon session. The afternoon session consists of four chunks looking at each one of four questions. We hope there's a transparency around so we can put the questions on the board as we discuss them or on the machine. Here we go.

We've got about a half an hour for each one of these. The first question is going to be led by Don Runnells.

RUNNELLS: Thank you, Paul.

CRAIG: Don, Don?

RUNNELLS: Hello, testing, testing. Can you hear me now? Is that better?

CRAIG: Well, just to summarize what's supposed to go on. Don is supposed to summarize what's happened to date plus any observations he wishes to make. Then, we're going to have the discussion.

RUNNELLS: Is that better? Can you hear me back there? Steve, can you hear me? More, louder, please? Can we have a little more volume, please? Steve, if you'll raise your hand when you can hear and I'll keep on talking. Okay. I'll start and you yell to the technician over there if it's not loud enough. Just by way of introduction, I want to thank John Pye of the Technical Board, the Review Board staff for helping me put these things together during our very, very short lunch hour.

A general comment or question. How would we want to use analogue information? Do we want to use it for model validation, do we want to use it for model development, do we want to use it for data gathering, as Rod Ewing pointed out? So, I offer that as the starting point. In terms of analogue, natural analogue information, how would we hope to use it?

Now, what I want to do is go through selected portions from each speaker's presentation as those things apply to natural analogues. As I understand our chart, we are to sort of review and summarize what went on this morning with respect to each of the four questions. Mine is on the board up there.

First, Steve Hanauer in talking about the various three topics that he had, he had two topics in which natural analogues played some role; site attributes and a robust and flexible design. I recognized in Steve's presentation that natural analogues in the site attributes applied primarily to delay and dilution. The other aspects, there were four other aspects of site attributes, but the one that jumped out at me was delay and dilution where natural analogues could play some important role in characterizing site attributes.

With respect to the robust and flexible design, there were a number of points where natural analogues could play a role; the waste form and we heard about the waste forms in nature--not waste forms in nature, the resources in nature as being similar to the waste forms that were trying to dispose of, UO2, for example, the waste package characteristics, that is the materials, engineering barriers, and the indrift environment. So, from Steve's presentation, I take away at least five points where natural analogues could play some role in site attributes and a robust and flexible design.

Abe Van Luik had talked about multiple lines of reasoning and I'm going to pull out of his presentation something that I thought was the use of a natural analogue and that is the paleohydrology. He used that as an example of a multiple line of reasoning. I think paleohydrology offers a natural analogue, and particularly when we heard about the pumping of the system by glaciation in Sweden, the fact that people may have to leave the area three or four times due to glaciers and then return to see how the waste disposal has gone. So, paleohydrology, to me, is a natural analogue of something that may happen in repository environment.

Rod Ewing listed four areas where natural systems, natural analogues may be important. One, actually, gathering data, a source of model data, and an example of that would be a source term. What is the source term? Confirmation of process models would be number two. Number three, to confirm performance assessment methodology. And, number four, to place the site into the context of long-term behavior. Four aspects where natural analogues could play an important role.

Bill Murphy talked particularly about, you know, particularly interesting to me, two sites, the Pena Blanca Site and the Akrotiri Site and he pointed out many similarities at Pena Blanca, but also emphasized the important differences that exist and the fact that we have to be sure that we recognize the differences. I think it's important from Pena Blanca that we've had about 3,000,000 years for the source term to change from primary UO2, spent fuel, if you like, to secondary minerals which now serve as the source term and that came from a natural analogue.

Secondly, the buried Minoan city from the Santorini eruption 3600 years ago, we heard both positive and negative aspects of that; what, optimistic and pessimistic would be a better way to say it. Number one, the concentrations of copper that were modeled are similar to the ones that are found in the soils, but secondly on a pessimistic tone, evidence that the site is not at steady state, that it's a dynamic site, that it's still changing. I would make the point there, I guess, that the main thing that we might derive from natural analogues is not site-specific information as much as process related information. What are the processes that are occurring, how can they be applied to Yucca Mountain or any other potential repository?

Cliff Voss talked about the Swedish program, and from that, I derived three points from the point of view of natural analogues. Number one, the difficulty in conceptualizing a natural site. Many conceptual models are possible. Which one do you choose? Secondly, the difficulty of gathering data. You can only gather a finite amount of data and there are finite limits to our ability to gather data. Third, the uncertainty of those observations, the uncertainty of measurements in natural systems and the natural analogues. I view, for example, his description of the F factor, the retardation factor, as an integrating tool for a system that's probably too difficult to understand. But, the F factor that talks about retardation of the radionuclides speaks to the difficulty of understanding the natural system and also, again on an optimistic note, shows that we can use natural analogues as integrating tools to take into account all of the processes that are going on and still derive useful information.

If I skip any of the speakers, my apologies. It's not because I did not think you said useful things; it's because I did not glean natural analogues out of your presentation. So, that's the reason if your name is not mentioned.

In terms of Dan Bullen's comments, I've already mentioned earlier the emphasis that he put on the passive layers on metals and those of us around the table now have seen Paul Craig's washers that have survived for many thousands of years. And, Paul never tells a fib, by the way. The importance of passive layers on the metals, I would emphasize that again, but I would emphasize the importance of understanding the environment in which the passive layers formed. We can perhaps extract useful information from similar sites, but an example of a piece of metal buried at the bottom of the sea in the sediment and pulled that up and saying that applies to an oxidizing Yucca Mountain environment, we have to be careful about that. What do we ignore? What negative evidence should we also look for, if you like?

I would emphasize again the importance of secondary minerals and the rate of production of those minerals over geologic or archeological time. I would emphasize the movement of water in mine cavities. These are my own comments from earlier.

Bill Dudley, I thought, did a wonderful job of showing us multiple lines of evidence for the issue of infiltration, the fragment as he called it, the TSPA. You could hear bits and pieces of analogues in there; natural analogues with regard to the precipitation derived over a long period of time. For example, for Nevada as a function of elevation and location within the State of Nevada, a general tool. I guess that's the Maxi Egan model that's been derived for so many years. So, natural analogues that Bill pointed out for precipitation and infiltration.

Ardyth Simmons, I thought, made an interesting point in the context--or in her point, I should say, her presentation on natural analogues are implicit in the TSPA. Although they may not be pulled out explicitly, natural analogues are considered in determining sort of the reasonableness, if that's a word, of a process that has incorporated into TSPA. Ardyth, is that a fair encapsulation of what you said about natural analogues in the TSPA? They are implicit, they're there as a check on whether or not a process is reasonable, whether or not a result is reasonable.

SIMMONS: Yes, as a way to build confidence in our conceptual models.

WILLIAMS: Very good, thank you.

Okay. Finally, I think that covers my summary of things that I heard from the various speakers. Just in summarizing two more points that John Pye and I put together at lunch; number one, we have to look at the contradictory evidence in natural analogues and that's not just the supportive evidence. We have to be sure that we're not overlooking something that argues against the understanding of the process that we're trying derive from a natural analogue. As an example--and this is not real, but it could be--if we're looking at the rate or movement of radionuclides away from an ore body at Pena Blanca, has anyone looked along the fractures to make sure that those fractures are not plugged by later cements? I'm sure they have, but one has to be sure that somebody looks for that kind of evidence to show that the process is not valid, as well as is valid.

Finally, fascinated again by the history that Cliff Voss talking about in Sweden, that by the time the People have moved away and come back three or four times because of glaciation, the whole environment may have changed in the Swedish repository, the glacial pumping of oxygen down into what is now, an anoxic reducing environment. The point being that with natural analogues, we have to look at the history. We have to know what has gone on as a flow of time, not just what is there in a snapshot today. Over a period of 3,000,000 years or so of oxidation at Pena Blanca, a lot could have happened and we have to try to decipher that in using natural analogues.

That's my summary, Paul. I hope it's what you had in mind.

CRAIG: Excellent. Our guidelines now way that we have 18 minutes for conversation on this issue.

MURPHY: This is Bill Murphy and because Pena Blanca came up in the discussion here, I'd like to make a couple of additional comments about the site. I didn't speak earlier about transport studies. There have been considerable transport studies, as well as source term related studies at Pena Blanca and considerable work characterizing fracture-filling materials and in evaluating the timing of radionuclide transport from the site using uranium decay series isotopes as a kind of clock. So, to respond to the question in a very broad manner, at Pena Blanca, we've looked at source term issues and the question of the rate of oxidation. One of our general conclusions is that the oxidation rate of uranium at the site was very much faster than the migration of uranium away from the deposit. So, that bears on the significance and importance of the raw or the secondary phases, the oxidation products, for the overall performance.

With regard to transport, we've looked at the distribution of uranium series isotopes in fractures and in matrix rock. And, we haven't carried that quite to the point of introducing those data and performance assessments, but there's been a substantial characterization. And, one of the major observations in my regard is that transport is apparently episodic at the site. It has occurred--the uranium series isotopes in the fractures have gone through periods of deposition and re-dissolution and re-deposition again. Apparently, that's how we can best interpret the isotope data.

So, those are the two general areas that we've looked at.

RUNNELLS: I think this episodic thing is potentially important to Yucca Mountain, as well. We don't hear very much about it, but once the secondary minerals have formed, then the release may not be continuous, but may, in fact, be episodic as it is in geologic environments. I agree.

BULLEN: Bullen, Board. Actually, to follow up on sort of both of those points, I'm interested in the use of analogues for validation and verification purposes for the things that Bob Andrews is developing in TSPA. But, what I haven't seen or heard in a lot of the discussions about the natural systems that we've looked at is that we really do have about a 1500 year thermal pulse that may, indeed, provide for the mineralization in those fractures faster than you would have found with the analogues at Pena Blanca. So, I guess, the question I'd like to throw out here is how do we address the thermal pulse issues because that's going to be something that will affect the near-field environment, both engineered and natural. In that effort, how many natural analogues are there that actually look like Yucca Mountain. Sort of to go back to what I said this morning, it has to look like the area or the system that we're trying to model and we have a somewhat unique system by putting the heat source in there. So, I just wanted to throw those out and ask.

EWING: Thank you. Well, of course, for modeling the thermal pulse, you have the Okro (phonetic) natural reactors where you had a thermal pulse and a uranium deposit where fission was taking place. Again, that's not exactly like the situation at Yucca Mountain. But, I think, rather than always say, well, that's not exactly like Yucca Mountain, I would turn it around and say, boy, we're damned lucky that spent fuel is mainly UO2 and 95 percent of the activity at Yucca Mountain is in UO2 because we have uranium deposits all around the world in a wide variety of geochemical environments, different hydrologies. And so, if we had a different waste form, if we had a different approach, a different type of fuel to be disposed of, we might be in big trouble. But, in fact, we have lots of good examples. Now, it's unfair and not useful to say, well, I'm looking for Yucca Mountain; Pena Blanca is close, but it doesn't have the thermal pulse.

What you have to do is piece together the relevant information from each occurrence. On this point, I want to say, when you get that information and much of it's available just by going to the library, this doesn't require a new program. I mean, particularly, the UO2 work relevant to spent fuel. That's a decade old now. When you pull all this information together and look at the performance assessment, there, you find the performance assessment models are pretty crude. For spent fuel corrosion, you use a response surface. Okay? So, that would be an example of where I would say because of the amount and diversity and kinds of information that are available, the analysis has to change. That should be more sophisticated than it is simply because we know so much more about the behavior of uranium and actinides in the environment.

And then, just to follow up on that, I would say for the performance assessment, I've listed a number of difficulties, but I'm still very interested in what is the uncertainty? Well, it should be possible to go to Okro or another uranium deposit, pull out some of the performance assessment models, define the few cubic meters of the uranium deposit with the defined hydrology and geochemistry, and try it. What are the major sources of uncertainty?

BULLEN: Just a little one for Rod because I really agree with you to take a chunk of some site and try and use it as a validation purpose, but I want to go back to something that you said this morning and I've got to get this right because you talked about PA being probably wrong. Okay? And, I would agree with you that based on the ranges of things that we look at, from a scientific perspective, it probably is wrong. But, would you say the PA is wrong from-- and we have to look at it from a regulatory mindset. So, I'm kind of twisting your words here and I'll apologize for that up front. But, from a regulatory mindset, probably wrong, the question is is it good enough to adequately protect the health and safety of the public for the regulatory compliance period and, for Judy Treichel's benefit, well-beyond that?

So, I guess, looking at your spread of uncertainties and the answer being wrong, but saying that that's a valid approach to the validation and verification of the PA, can you then make the next step that said, even though it is probably wrong, is it acceptable? That was a loaded question, Rod; I apologize for that, but I'd love to hear your answer.

EWING: I'm trying to think of how to rephrase that into a question for which I have the answer.

BULLEN: Oh, nice try. Okay. I tried to make it convoluted.

EWING: All right. Let me make two observations, one to give hope. One of the characteristics in looking at other systems that are modeled of systems that are nonlinear, which this is, highly-coupled, is that they have a tendency to reside in some, let's call it, performance space. Even though the uncertainty is high, the behavior of the system is consistent within pretty wide boundaries. Once you realize that, that becomes, I think, not a way to reduce the uncertainty, but to speak with confidence about the behavior of the system. The value of natural analogues if you look at one uranium deposit after another is they show this behavior. In an oxidizing environment, uranium moves reducing its-- it's less mobile. So, I think there's something to be made out of the complexity.

Okay. Now, there was another part to your question. How do you tell if it's good enough?

BULLEN: In a regulatory time frame.

EWING: Well, in this case, you need better regulations. You need regulations that have multiple criteria that don't drive you down to a door that just says go through or not. This is not the way people live because they know life is more complicated.

BULLEN: But, that's the challenge we face as the Board because, I think, we have to live with the regulations we've got.

EWING: Right.

BULLEN: Thank you.

CRAIG: We have Ardyth and then Bill.

SIMMONS: I'm going to address the question that you asked about thermally coupled processes and where we look with respect to analogues that might tell us something about fracture sealing. In the case of thermally coupled processes, we have two general categories of types of areas where we might look. One type is inactive geothermal regions. When we look in those areas, there's obviously a scaling difference that we have to consider; that the scaling being the difference in the thermal regime that we could expect with regard to a repository is going to be much more extreme. So, we have to bear that in mind.

The other kind of system that we look at is fossil hydrothermal systems. There, the challenge is that you don't have the data. You have to infer the data about the conditions at which the fractures were sealed, let's say. And often, the way one does that is with regard to knowing the stabilities of the minerals that occur in the fracture sealing locations.

So, with that in mind, in regard to the first class of analogues, we've been looking at such things as areas of recent volcanism that are partly still active where there are fumaroles and the value of 10,000 Smokes is a good one that was active starting in 1912 with the eruption of Novarupta (phonetic). What's interesting there is that over a period of maybe a decade or so, most of the vent areas started to close up over this large area and only the central portion remained active. It was discovered that the areas that plugged up most rapidly were the ones that were the most densely welded and this is in a silicic ash flow tuff similar to Yucca Mountain.

Areas like Yellowstone can provide the same kind of information. Now, what we're trying to do at Yellowstone is actually use quantitative data that we have from cores to then test our models and see if we can reproduce the same results. So, that's taking it a step beyond the more observationally related. And, we haven't gotten to that point yet. So, I'm kind of describing work-in-progress.

In the case of the fossil systems though, probably the closest to home and the best that we have to look at--and this is another work-in-progress--is the tuffs at Paiute Ridge, Nevada where there has been intrusion of basaltic sill in dikes into nonwelded tuff like Paintbrush. There, we can look at the degree of alteration away from the intrusion. The first assumption that we have to test is whether the fracture sealings there were actually a result of contact with this intrusion or not. That's a very important point to be able to make in that they didn't occur afterwards and we can rely partly on dating to help us with that. But, what we need to be able to do is look at what happens with distance away from this sill, this heat source. Can you find different degrees of fracture sealing with distance from the heat source, a different suite of minerals, and so forth. So, that's like a cross-section of mineralogy with temperature and time that we're trying to make now.

MURPHY: This is Bill Murphy. I was going to talk about Paiute Ridge also. So, I don't need to now accept to say that there's evidence that there's quite substantial changes to the hydrolic characteristics of the rock due to the thermal pulse. There are a couple of other places in the G-2 well north of Yucca Mountain. There's evidence at depth of a hydrothermal system that occurred that altered the mineralogy that's been studied and is in the literature. At Vias Caldera, there was a study of the thermal effects of an obsidian flow adjacent to a silicic tuff looking at migration of volatiles; particularly that was an NRC study. Also, the DOE, the Livermore people, have worked at Wiraki as an analogue system considering thermal effects, in particular, to study the usefulness of geochemical modeling tools under those conditions. So, there have been a number of analogue studies devoted to thermal effects.

STUCKLESS: John Stuckless, U.S. Geological Survey. One of the better thermal analogues to Yucca Mountain occurs on the west side of the test site at a place called Yucca Mountain which was heated by the intrusion underneath the Timber Mountain caldera and stayed warm, above ambient, for about 5,000,000 years. UNLV and USGS have now completed the fluid inclusion studies. So, we've got a place that hydrologically is just like Yucca Mountain and thermally is like the repository you folks have asked for now. The temperatures we've been getting are all sub-boiling, but it has been cooling slowly. The difference between that and some of the other analogues that have been mentioned is the volume of water; a much smaller volume of water at Yucca Mountain. As a result, we don't have very many filled fractures during that thermal period.

KNOPMAN: Knopman, Board. I was just going to make an observation or two and then ask a question that actually does relate to the test site. The observation is that in some sense these sites like Pena Blanca are the closest we get to analogues not for necessarily individual subsystem components, but for more TSPA itself as an integrating tool. Nothing we could invent would do better than looking at these sites.

And, the next step from that is--and even just in this conversation today--so struck by the kind of argument that's advanced, and relatively in understandable terms, what the Board has called the Coherent Technical Narrative, to explain what has gone on, what the histories are at some of these sites and some of this stuff is not all that well-known or some of the detail is not that well-known. But, there's an analogue for the program in terms of how one explains a site and what goes on somewhere by using some of these natural analogues. So, it's an analogue at two different levels; in a scientific sense, but in a programmatic sense, as well. You've got places where something has happened where there have been some transport, some emplace--or some naturally occurring radionuclides and then transport and you're trying to say something about that. I hope that that part, that second part, will get thought through as things proceed.

Now, on a somewhat different track, I'd welcome some comments from some of the DOE folks here about the pros and cons of making more use of the Nevada Test Site itself and the many things that are there and moving and some things not moving, but the water, the end tunnels. You know, a number of us have been in some of these tunnels and seeps and one could get a lot, it seems to me, insight. It's never been given much of a priority in the program with the years that I've been on the Board. There's always been sort of reasons why it never--but it would be useful, I think, in this context now, to find out why or what is the value of the NTS analogues.

CRAIG: Thank you, Debra. Are there any other comments on the first question?

KNOPMAN: Can someone answer that, Paul?

CRAIG: Who would like to answer Debra's question?

WILLIAMS: Dennis Williams, DOE. I think your question was whether or not we were considering using the Nevada Test Site for additional analogue--

KNOPMAN: We're years into this thing. It's not a matter of whether--of why has the Nevada Test Site not been assigned priority as a source of analogue information.

WILLIAMS: Would you mind if Dennis Williams deferred to Ardyth Simmons?

SIMMONS: Well, actually, the project has considered the Test Site in terms of a number of different analogues. The one that comes to mind, of course, is radionuclide transport. I wouldn't say that the Nevada Test Site was ever given a priority in terms of where we would seek analogues, but a number of proposals had been made through the years to look at areas where tests had been done and to look at the transport of radionuclides away from these tests. In fact, Doug Duncan who is in this room was part of the collaboration effort to get some of that work going. And, as with any analogue, and particularly with transport analogues, but with all of them. it's important to try to be able to constrain the processes that you think are operating. And, in the case of the Test Site analogues, particularly, one has to be concerned about whether the transport of the radionuclides is due to what they call prompt injection with the test itself or whether the transport occurred as a result of groundwater processes.

And, there are two main reasons why we didn't get the work completed at the time, although I think we would have been able to test that hypothesis. One of them was that there was a considerable amount of additional characterization that needed to be done to be able to prove that it was a viable analogue and that's a real concern for many analogues, but at the Test Site, we found that we didn't have the body of data to work with from the beginning. So, we'd need additional characterization. That was true of the colloidally related transport situation, as well.

So, I think it's still worth pursuing, but one has to recognize that common to anthropogenic analogues, in general, you often require additional characterization data that might not be there.

CRAIG: Clark Peters want to add something.

PETERS: This is Clark Peters. One comment. Everything Ardyth said is true, but there's a pretty healthy ER program, Environmental Restoration program, on the NTS that both Los Alamos and Livermore are involved in and they use the same codes, APHN, NUFT, and similar conceptual models. So, in a sense, we are putting confidence in our codes and our models indirectly to understanding the ER program at NTS. And, I can say the same thing about the Los Alamos ER program because, as you know, that's also a TOUGH sequence.

CRAIG: Last word from Dennis.

WILLIAMS: After we've had these good technical discussions, now I will be perfectly frank with regard to the natural analogue program. For years, it languished on the program, in part, because people felt that it was one of those nice-to-have things, but was not essential to the case. Hopefully, we're in the process of turning that around.

CRAIG: Debra, we now turn to you. We now turn to Question #2 which Dan will put up there. It relates to simplified calculations.

KNOPMAN: Thank you, Paul. And, Dennis, I appreciate you saying that.

We'll we're operating, as the Board sometimes does, in a passive motif of four questions and we're now on Question #2 of our four questions. "What are the pros and cons of using simplified calculations to add confidence to the conclusions of performance assessment?"

I'd like to summarize what we heard this morning, but just as a context for that summary, I'd suggest that it's useful to think in two different categories here of when we say simplified calculations about whether we're talking about some individual component of the overall system or whether we're talking about some simplified calculations to give us some integrated view, more of a TSPA or performance assessment approach. They're different and I think there may be some different observations that we may want to make about that.

I guess, the other thing that I'd just like to say from the outset is that in answering any of these questions, particularly this one or definitely including this one, we shouldn't lose sight of why we're having this discussion in the first place. Why we're having this discussion in the first place is we're trying to see if we, the United States Government, in making some decision, social decision, about the disposition of some material, can do better than throwing darts on a board and guessing where this stuff should go and what will happen to it when it's there. And so, we're really talking about degrees of improvement over dart throwing and that kind of goes into picking up a point of Rod Ewing's which is perhaps the choice that we have here is complex and Rod said, wrong, I would soften that to probably not right, to simple.

EWING: I was going for some shock value.

KNOPMAN: I know, I know. The alternative is maybe Cliff Voss's formulation of simple and probably not right. So, with that in mind that that's really the question we're asking here, where would you rather be, let me just try to pick up on some of the points that some of our speakers made this morning.

Steve Hanauer used the terminology of simple models being a sanity check on TSPA which is interesting in the sense that it picks up on the idea that TSPA is, in fact, the only game in town that we really have to do this full integration of a very complex system. And so, we work in some sense at the edges as we can to make sure that it's not totally off the wall.

Abe in his summary discussed the notion of simple insight models and there are two that, I think, actually fits with some of what Cliff was saying. The simpler models with fewer parameters, fewer complicating features are just much easier for us ordinary humans to get our minds around and to understand what's going on as opposed to the complex TSPA kind of model.

Rod made a number of comments that, I think, are relevant to this question of simple versus complex and I thought one of the most telling comments and it's something that I've said at other occasions, as well, is the false precision of complex models. The idea and this is embedded in the regulatory process that we have now that we could actually have an argument about, 25 versus 15mg, and as if-- as if we actually had the tools to tell the difference given where we are now. Rod also made some comments about analyzing the barriers separately and in some ways I see this as kind of coming full circle. It's very interesting that we're having this discussion about subsystem behavior when there's been such an effort to move away from certainly looking at subsystem performance and look more at overall system performance. Yet, again, we're limited by our own cognitive abilities to think in such complex terms and I think are naturally drawn back to subsystem understanding. I think that's an interesting point to remember here as you're trying to explain how we think this system is going to work to the public, to members of Congress.

Bill Murphy, I thought, in his examples showed a relatively simple way of presenting data from a site. I assume those were 1-D models that you were using on the copper transport?

MURPHY: Yes.

KNOPMAN: Okay. Nothing fancy, but a lot of insight there in terms of what you can say about transport.

Cliff, I thought, made an excellent presentation and there's no question one has the capability and we have even more capabilities now than when this program started to look at complex systems and model them in what appear to be complex ways, lots of fancy graphics, again the appearance of precision when, in fact, there may be no--there in terms of added information. And, a point that Cliff and I have actually worked on a long time ago has to do with this value of information and what you can extract from a few observations that you do have and how much complex models can eat up some of that value by going toward estimating parameters, only some of which or very few of which may actually be important. It takes a huge amount of data to estimate parameters in these models. You end up having very little information per parameter in a total sense. I think that's important to remember when you're in a state of incomplete information which we always are. This is a complicated site as everyone says. How do you get the most out of what you do have? Our complex models are a very efficient way to get that information out of what you have. Anyhow, I think that was some of what could be extracted from Cliff's comments.

Mike, I think, made a good point that sometimes we don't always see even at the Board level and it doesn't come out in our public meetings which is how often you may just simply plot up the information that's coming out of the field and look at it in a fairly straightforward, unencumbered way, draw some insights, make some decision. A lot of that process is not necessarily transparent. What we see is the integration that comes much later and we lose that ability to see what you see in that sense.

I was delighted to hear Bill Dudley go through the infiltration example. It reminded me again what a fascinating process earth sciences usually follow in putting pieces of a puzzle together and making a story out of it. It's not a linear process and there is a lot of--the quality of information varies, but you accumulate this weight of evidence and you may not say--you may not be able to know precisely when you've got weight of evidence, when it weighs enough, but it seems as if people know it when they see it. We do have a few examples in this program itself where that kind of closure in a sense has been reached.

Bob Andrews, I thought, came at this in a different direction from Cliff and some of the others in that Bob talks about TSPA and understanding getting the insight from TSPA by peeling the layers of the onion off to see what happens, to understand what's driving the results. That's another way to sort of get to do the complexity versus simplicity arguments in some sense. But, it's a very different starting point. The question is whether you peel enough away to really understand what's driving the system or do you still have so much noise in it by virtue of the complexity that you lose that insight.

Ardyth gave us a number of good examples and, you know, I think in most of these natural analogues that she's been studying and trying to pull together for the program there's often a dearth of data by necessity working in relatively simple model formulations to at least explain--some explanation of what's going on.

So, with that, I will stop and see if we can get some comment or I would love to provoke a discussion debate between Cliff and Bob Andrews on which way one should move, from simple to complex or complex to simple in gaining insight.

CRAIG: Cliff is online here. I'm going to make a remark first and, Dan, keep track of others, please.

Yeah, I find the modeling really very fascinating. I recently came across--I'm writing a review of energy forecasting models which causes me to have to review modeling techniques. There is a wonderful review of things to think about in developing models that's done by a guy named Scott Armstrong from Wharton School with a long review on his website that I commend to everybody, things to think about, hundreds of things to think about and choosing the model to match the problem at hand. One of the many things that he talks about is the idea of building a complicated model in order to find out what's important and then using that as a tool to build a simple model which people can understand. Then, if somebody comes along and looks at the simple model and says but you left out such-and-such which I believe to be important, then you can always go back to the complicated model and explain why you thought it wasn't important and get into that conversation.

One of the things that DOE has not as yet done and I hope it will is to take the complicated model and construct the simple version that's comprehensible to people and that does not mean to my way of thinking a simple model which you run on a computer because there you just change parameters and see what happens. That does not provide the kind of insight that is required in order to convince people. I'm rather thinking of the kind of simple model that gives you the physical understanding for each one of the significant elements. How do you understand the water transport through the UZ? How do you understand the corrosion of metals and so forth? And, you may have the right tool there. It simply needs to be--not simply, that's the wrong term, but needs to be translated into a different idiom so that it's more accessible. I don't know whether that's true, but it seems to me that that is at least the right question to ask.

In any event, Cliff?

VOSS: Cliff Voss. Yeah, Paul, I agree with what you just said about simple models. I want to take up something that Debbie said and go a step farther with it in terms of the number of parameters we have in models. And, I'm talking about particularly sub-models, say, for the hydrology component of performance assessment.

We know in the modern state-of-the-art and its modelings that we can run inverse models. We can calibrate the models automatically using other tools, groundwater models. That calibration means that however many parameters you have in your model, whether it be the permeability of 10 different units, aquifers, the permeability of fracture zones, parameters of the unsaturated zone, if you have some field data, you can run your model in a sense backwards, force it to match the field data, and in that process calculate the values of the parameters that you need to populate the model with. That would be in the model. So that now the model apparently fits what's going on in the field? It reproduces the behavior you measure in the field.

But, when you do that, when you look at these processes, you get certain measures of how good are the parameters that you've estimated. It turns out that the more parameters that you have in your model, the more poorly you estimate them and that's what Debra was saying a little while ago. So that no matter how much field data you have and particularly the more complex the environment is that you apply your model to, no matter how much field data you have, the more parameters you have in the model, the more poorly your model performs. It looks like it's fitting the field data, but it's a very poor predictive model. The more knobs you add to your tv, the more things you can tune on your picture. It doesn't necessarily make the picture better. There's one picture underlying that that you're not necessarily really seeing. You just think you're seeing it better. Parameters are not necessarily making the model better.

Now, most of the models that we use for complex sites are complex. They have a lot of parameters and they just appear to be good. So, it turns out that when you do these fits with models with few parameters, say, three or no more than 10 parameters for any amount of field data that you might have, then you have a very powerful model to describe what's going if you've also fit that model with few parameters to the same field data. It's much better than a 100 parameter model even though that looks more interesting when you show a three-dimensional picture of it. It looks like it's real, but it's not.

So, in that sense, all of the complex models that we deal with, whether it be for nuclear waste disposal or for other toxic waste applications or for just understanding groundwater systems, they're basically wrong or not too practically useful if they have a lot of parameters. Only the simple models are the ones that are meaningful to understand the hydrology or the waste transport.

That was one point I wanted to make and the other was about the performance assessment and the sub-components of that in terms of simple models. Maybe something that should be discussed or should be thought about, that a performance assessment should not ever be based on complex models. The sub-components of it should all be simple models. Then, the performance assessment is transparent, as well as Paul was saying, each of the sub-models gives some understanding of the process that went into make a decision in the performance assessment. Complex models and complex data could be used then to check the simple models that are in the performance assessment. I think today we have it the other way around, the complex models are the ones being used for performance assessment and we're thinking maybe now we should use simple models to check them. I think that's upside down. The simple ones are the ones to use in a performance assessment; the complex ones should be used to check, and exactly as Paul was saying, to see are we missing something? Then, you can have a discussion. You can't have a discussion about a complex model. It's too complex to talk about. Nobody has any grasp of it; maybe not even the modeler who put it together.

ANDREWS: Bob Andrews. I hope I don't have to define simple or define complex because I'm not sure I could define it right now, quite honestly. But, I have to ask what the model is for and look at the function that the model or interpretive aspect is being used for. We've been talking here about one particular model which is kind of the flow model and one particular sub-element of that, the UZ flow model. One could argue, I think reasonably, that it looks pretty complex. There's a lot of images used to describe that UZ flow model in several AMRs and PMRs. It looks beautiful in color. There's a lot of grid blocks in there. There's a lot of inverse modeling that's been done, a lot of years of very hard work by some very talented people to put that thing together with a lot of data. It looks complicated. It looks complex. And, you say, well, what am I using it for? What I'm using it for is to get an understanding of spatial distribution on average and uncertainty associated with that for how much water is moving through the mountain. Clearly, it's very dependent on one particular boundary condition that Bill talked about. That is the net infiltration. But, in some ways, that's all we use them for. We'd like to have some degree of precision as where is it spatially distributed, how is it spatially distributed, how is it temporally distributed because things do change with time in this system, but the degree of precision required of that is not very high, as Bill talked about, 1 to 10 millimeters per year and maybe at the surface it's 1 to 20 millimeters per year. Does that change with time? Yeah, it changes with time because of the climate change of the time. I call that kind of simple, 1 to 10, average 4.6, plus or minus 5. So, that's pretty simple.

So, I think it's a simple representation when you actually start looking at the data, the actual underground observations, ESF observations, borehole observations, chemical observations, thermal observations, you try to put all of that information together to defend your conceptualization and to defend that ultimate use. But, that ultimate use was pretty darn simple.

So, I hate to call the UZ flow model complicated even though millions of dollars of work have gone into it. If Bo were here, he'd probably disagree with me and say, oh, it's complicated and I need, you know, additional resources to continue defending them and that's probably true. Again, if we take this thing further on with wanting to have added confidence, you probably do. But, in some ways, it's quite simple. For its use, it's quite the simple model.

BULLEN: Bullen, Board. Actually, I want to thank Rod

Ewing for framing the question that I'm going to ask right now because we were talking about the transparency or opacity this morning versus complexity. But, I guess, the real question is credibility. Which one would be the credible model if you wanted to take a look at first the scientific decision and then maybe a regulatory decision? Do you want the simple and transparent model or do you want the complex and perhaps opaque model for making the credibility case. I'll ask actually Cliff this because he led into it and then maybe ask Bob again to re-cover, and if Rod wants to jump in, he can, too. I don't want to feel like I'm picking on Rod. Which one would be the most credible in your eyes?

VOSS: That's a difficult question because it--I mean, it's the heart of the question how to go about making a decision and say the complex models are generally--they should be seen as research tools, as developing understanding in a system. I don't necessarily think that the results of them should be directed channeled into a decision. They should be interpreted, and in interpreting those, the simple models can be created or used or checked by them to make the decision with. So, I guess, I come down on the side of trying to keep the analysis as simple as possible so that you can get people to agree with what you've done. This isn't going to be a hidden thing. The opaque model will always be opaque and one will understand it. I don't think that's a good decision-making tool.

BULLEN: You worked to keep it simple just for me, right? No pun intended; my talk was keep it simple this morning. That was design, not models.

CRAIG: I'm going to jump into this one, too. However, I note that Priscilla Nelson has just arrived. Priscilla Nelson is a Board member and we hope you will come and sit here. I assume you're suffering from the usual difficulty. Your office is next door and--

NELSON: My office is next door and my mother's 75th birthday party is tonight.

CRAIG: Are we all invited? We're all invited, right?

NELSON: So, I'm very sorry. Yes.

CRAIG: Okay.

ANDREWS: Okay. I think the simple is helping explain conceptually what's going on. It's very appropriate for certain audiences and I think the more complicated, you know, based on all the observations and data, multiple lines of data that are used to support it is also important. I think I wouldn't say one or the other.

CRAIG: Now, I went up to LBL and I spent several days getting them to try and educate me on the UZ model. And, after several days of this, I understood some things, but I didn't understand all that much. For an ordinary human being who only has one lifetime to devote to Yucca Mountain, there really is a problem with a model that has over two dozen different layers with many parameters and those parameters have to be selected on the basis of a very small number of boreholes because you can't put boreholes everyplace or the mountain doesn't work for you well, as was pointed out earlier, I think by several people.

There are alternative approaches. I said some positive things about John Kessler's work at EPRI and here's an example. Kessler says what's the maximum focusing that you can get? A factor of 22. Supposing we take 4-1/2 percent of all the water and we just dump it down into 4-1/2 percent of the drifts and we see what happens? And, if the analysis survives that, then it's pretty robust. It's pretty robust. That's maximum focusing by some estimate of maximum. Well, now, one can challenge all of that. And, clearly, you've got to believe that the metals are good. If the metals are in trouble, that's in trouble. But, nevertheless, it's a line of reasoning which is readily comprehensible, readily comprehensible in a way that the computative models simply aren't.

When I read that report, I began to see--Kessler's report, I began to say, yeah, now I'm beginning to understand what's operating here in a way that I couldn't previously. And, to me, that kind of improved understanding has a deep value.

ANDREWS: Let me try something here, Paul, because it gave you confidence and yet Rod acknowledged that one other aspect of the system, in this case waste form degradation which is very simply treated, kind of left him uneasy because other observations could have been used, other models, more complicated models could have been used, and he, in fact, would have preferred--I'm not trying to put words in your mouth, Rod--but preferred those more complicated models for waste form degradation and what we ended up using was quite a conservative and bounded and very simple representation. So, I'm trying to figure out--maybe try to pull Rod in here.

EWING: Well, this may be one of those rare times when I have to immediately agree with Bob Andrews. There's some exceptions, but in general, the subsystem models are pretty simple. In the case of waste form degradation, my objection is it's so simple that it's just a look-up table fitted to a limited dataset which may or may not be relevant. That's not the argument. But, the complexity for me comes from the connection of all of these relatively simple models and that's where individual scientists lose the ability to review what's going on. Once one model becomes the input for the next and so on, then that propagates through the system and that's the real complexity. The individual subsystem models, by and large, if that's all you had to review, I think reasonable people could arrive at some consensus as to whether it's useful or not. But, connecting them makes it very difficult and that's why I continue to advocate analyzing the subsystems and not looking at the final aggregate and making judgments on that.

HANAUER: Steve Hanauer, DOE. There is a divergence in this discussion which is starting to bother me. On the one hand, we have been severely criticized by the Board and others for the degree of conservatism in some parts of our performance assessment. And, on the other hand, we are now being told and have been for a long time that our models are too complex, that they can no longer be comprehended in any reasonable way by nonexperts. In fact, this is a thruway to schizophrenia. If you really want to know what's going on, you must construct as realistic a model as possible and you must put in it whatever is important to the result. And, no one can ask it questions. You can ask it any questions about the factors which you simplified because they're not there and, therefore, the model can tell you nothing about them. And, if you want to ask to other kinds of questions, then you want simplified models and you have to give up the idea that these models are realistic. You are admiring EPRI's use of the worst focusing that you can have and to just put it in. But, don't ask this model any questions about focus. It's not there; only one number, the worst focusing you can have is there.

Saved by the bell?

CRAIG: We're going to go through the names of people we have on the list because several more--but since the bell rang, I will point out that I did advocate explicitly the idea of using the complicated model to figure out what's important and then develop the simpler model so that you can explain. Then, if somebody wants to go back and complicate things, you've got the capability of doing that.

HANAUER: Well, there was a third reason to use models. and that's to decide whether you're going to be allowed to build it or not. And, this is what I would call the licensing model and there will be simplifications bounding values, bounding models, and so on, because the object then of a licensing model is not necessarily to understand the behavior of this system, but to predict the outcome in such a way that it will always be pessimistically, or if you like the word conservatively, predictive so that the licensing authority can be convinced that you are always on the safe side.

Now, these three objectives, understanding this system in the sense of having how does the system work, understanding the system in the sense of simplifying it to where ordinary mortals can understand it, and simplifying the system in an entirely different way to get a conservative estimate for licensing, are all three different and the models that do this are three different models.

CRAIG: I agree with that. We have Richard Parizek, Don Runnells, and Michael Voegele and then we'll move onto the next area.

PARIZEK: I think the thing I got out of the total system performance assessment is the fact that I can sort of see how you get a dose. I'm going to ask you, Rod, how else would you have gotten doses that would be shared with anybody if you didn't go through the TSPA process because clearly, as you go through that, you begin seeing what things contribute or at least that are supposed to contribute based on the component parts. I wouldn't have known how I would have gotten a dose out of this whole analysis without a TSPA. I understand it serves that purpose as complicated as it might be, but we still have this problem how do you explain to anybody providing you agree with it and you find there's no errors in it. Is there another way to get to a dose that would be simple?

EWING: No. And, I want to be clear, to get to dose, you have to go through a TSPA. You have to put everything back in and calculate it. But, if you look at, say, how sensitivity analyses are done or how people look at the components, in general, those are carried all the way to the dose point. What I'm saying is, you know, analyze the credibility of your models, the usefulness of your models, as manageable components, and then once you have confidence in those, then you can begin to connect them through maybe the licensing calculations for which you'll need a dose. But, the other prudent part of the approach is to have multiple criteria. No matter what does you calculate, you should be able to argue to people that travel times are long or the amounts of material that will move is small and so on. That should be part of the discussion.

RUNNELLS: Well, I simply tried to get your attention so that Bill Dudley could say something.

CRAIG: Okay. Go, Bill. I'm sorry, I didn't see you in line.

DUDLEY: Basically, I'd like to return a little to the reason for the meeting which is developing multiple lines of evidence and how does that relate to this particular question? Certainly, there's a much greater population of alternative or other lines of evidence that can be used to evaluate the credibility of components of the more integrated TSPA if we do examine those components themselves. And, this is similar to the point that Rod just made that if we do look at the components we can find a large number of tests, some of which could perhaps prove only that the component is wrong. You can rarely prove that anything is right.

Once all the components have passed somewhat of a credibility test, then you can perform the very difficult task of trying to link them all back together in TSPA and I doubt seriously that a simplified TSPA which means using just selected parameters basically, whatever, passed muster as well as looking at the full performance of the components, and then no matter how difficult it is, what a bitter pill it is, to go ahead and put them back into a full TSPA.

VOEGELE: Two things in context. Debra said that we're always in a state of incomplete information. The comment I made this morning was whatever findings, if any, will be made to take this thing forward into a licensing phase will be based on a concept called reasonable assurance. I'd like to see if I could put the performance confirmation aspects I was talking about this morning in that context.

It will not be possible in my estimation to lay out a measurement program which will allow you to validate, if you'll let me use that term in its non-PA sense, the results of your performance assessment calculations. It's just simply nothing in there that you can measure except the dose unless you break it down into smaller component pieces. So, what I would be looking to do would be to try to determine those parts of the performance assessment calculations which could be measured most directly and simply and analyzed most simply in the field experiments of performance confirmation program. That's not to say that you would make a reasonable assurance finding and predicate then on the results of this testing program. This testing program is intended to provide additional assurance. The actual reasonable assurance finding would be made before the construction authorization starts. So, we're not saying we'll start building the repository and then we'll find out later on.

With regard to your comment about where is the backing away point, these things could end up being literally terms and conditions of your license which meant you could define the particular range of a variable that you have to operate within. And, if you found that you were not operating within that variable, you could not continue to operate because you'd be outside your license conditions. That's the real value of the performance confirmation program in the context of this question. It can help you find simple things that you can measure and analyze that can help give you better assurance that your performance assessment calculations were, in fact, correct.

CRAIG: Thank you, Mike. That helps. Abe?

VAN LUIK: Thank you. Yeah, there's a lot of discussion here about licensing, but I think the point was made by the Board rather pointedly that we have a very important decision coming up which is a society-wide decision. I'm wondering if we could remind ourselves that if we ever get final regulations, as Judy hinted we should, if 963 passes muster the way it is, it not only asks for the bottom line dose number, but it also has some 20 criteria that have to be shown that you meet. If you look at those, to me, they look suspiciously like multiple lines of evidence, reasoning, inquiry, etcetera. So, I think, you know, the idea that the regulation, if it ever becomes our regulation, does not require that, I think, is a little bit misplaced. Thank you.

METLAY: Abe, could you clarify a number of--in the preamble, there is a whole set of issues that need to be addressed, but the regulation itself to my understanding was that the regulation simply requires that a performance assessment be carried out and that the results of that performance assessment comply with the EPA standard and the NRC. So that compliance with 963 doesn't depend on these 20 odd things, but simply on the outcome of a performance assessment. Am I reading that wrong?

VAN LUIK: If you are reading it like a lawyer, you're probably reading it right. But, let me add this. We have an expert on this topic right here in Mike Voegele and he will explain that what we're actually doing is looking at all those criteria and taking them serious.

VOEGELE: Right. This is Michael Voegele. The way that proposed regulation is written requires more than simply demonstration and compliance with the proposed EPA standard. It does require that the total system performance assessment results be examined very carefully with respect to a detailed list of criteria to convince yourself that, in fact, the performance assessment calculation is defensible.

METLAY: But, in the final analysis, it's a yes or no against the standard?

VOEGELE: In the ultimate final analysis, that is correct. That cannot be made--that finding cannot be made without demonstrating that each of the individual criteria have, in fact, been addressed.

CRAIG: Okay. At this point, we move onto Question 3 which is, "Should multiple lines of evidence be derived independently from performance assessment?" Dan, if you would put that one up, please? And, this part of the conversation will be led by Richard Parizek.

PARIZEK: The answer seems to be yes from the different points that have been made. You do need multiple lines of evidence just to build your models, as was just pointed out by Mike and was also explained by Steve, if you're going to create a model. The model, as we heard this morning, requires input from all sorts of approaches; field observations, your physics of the system, anything you can do to create this understanding to get to TSPA. Once you have the TSPA, then the question is can you then create more observations from them? No, you've got to go back in the field and get more observations. As it was also pointed out, we can get them from the analogues on the one hand and get them from independent field assessments. So, the TSPA can't create the new data. It only shows you the sensitive parts of the system you're trying to analyze is what we understand.

So, as complicated as it appears to be, you've built this from the bottom up. Implication-wise, you've went from the top down, ready to go. I don't think Bob--they put the parts together in order to get the result which is a dose at the end.

And, again, I understand the complexity of it, but I said I wouldn't have known how to do it any other way, although it may be hard now to analyze it and find out where the weak points are. So, that's what we're after. How do we get confidence in something that's hard to explain to ourselves and to other people? And, maybe, you can only simplify up to a point. So, I like the component approach and, to me, it's a question of making sure we can defend it or the program can defend the component approach. Having created TSPA, then we're looking for ways to improve it. I was looking this morning for the ultimate understanding to say, well, I want a validation of this. And, you just heard from Mike that you probably can't validate it as such; you can only validate the pieces that went into it as most you can for all the physics and all the science, all the engineering you do. But, once we erect this, it's going to be very difficult to say here's my independent validation of that program.

VOEGELE: I sure hope I said this is my opinion.

PARIZEK: Well, we can go back to the record. Did anybody else want to weigh in on this? I mean, different people sort of made this statement about the multiple lines of evidence and the various place that we go in the field to get at it, with the analogues on the one hand and with the Swedish program on the other.

(No response.)

PARIZEK: I guess, maybe that one has been argued to death.

CRAIG: This is remarkable, but it seems to be true.

PARIZEK: I guess, the question is who would make the simplification determination and it's to serve one purpose. It's to bring people along to build some level of understanding or confidence in the process, right? And then, the question is how else can we do this? Rod told us that there's ways to do it. Go back to the basic principles. One, it's going to be permanent and long-term for geologic-- it's going to have geological stability, and like Yucca Mountain is not going to erode away in 50,000 years, 1,000,000 years, or 10,000 years. It will be there. The question is maybe there will be some faults in it. I think that's what you had in mind for stability. An important point was you were pushing for passive performance to the extent that the natural systems buys you something in addition to the engineered part. So, I guess that's the active part, right? You didn't exactly explain that, but passive means you're using the retardation and the slow travel time, all the other things that are part of the natural system as part of it. How can we argue against that, right? That's clearly a very useful way to strengthen our understanding.

Then, you say, well, if you go underground at WIPP, if you want to get a sense of eternity, just sit there. Turn off the lights. And, yes, it's quiet, it's dark, it's eternity in a sense, but that doesn't necessarily mean that it won't leak, right, just because you get that impression. So, we need to formalize our feelings. It's one thing to give a sense of feeling; the other thing is to try to explain to somebody; the other one is to bring along the confidence for others that they should also share in your feeling. Right? The insecurity of the airplane crashing, it's hard to get that out of people's mind. As a result, they elect not to fly and so on.

So, I'm not sure how you get the simple part of this built into it without maybe destroying the formal process you have to go through to give us these doses or the program gives us these doses. And, we have to criticize those or challenge them and find out where the weaknesses are and then we're still going to have the public who is going to be worried about buying into this for reasons that's going to be hard to explain. To make it simple, I'm not sure how you do that. Maybe, if we can get some points on how the program hopes to do that in the writeups or the presentations that are made.

CRAIG: Well, there are a number of dimensions to this. I think one dimension goes back to some of Steve's comments. I think it was also about the regulatory process. The Board, it's important to bear in mind, is not a regulatory agency. The Congress in its wisdom decided that a regulatory agency alone didn't do everything that they wanted done. So, they set up this Board to look at the science and that's the task that we have. We have interpreted that task as relating to confidence within the scientific community over the other mountain analysis. So, we are in some sense--well, we view ourselves, in some sense, charged to take some kind of consensus as to what the scientific community believes about all of this and that really is quite different from a regulatory question. So, you need to bear that in mind in thinking about it. It's that line of thinking that leads us to ask so many of these questions about simplification and transparency. If you have a large staff as the NRC has, you probably don't have to worry about that so much because you've got the technically trained people who can understand in detail. But, we don't have that capability and the public, in general, doesn't have that capability. So, it's that kind of consideration that leads us to be particularly concerned about the multiple lines of reasoning. And, the second issue on here which is the degree to which the multiple lines of evidence may be derived independently of performance assessment.

BULLEN: Bullen Board. I want to come back to what Steve Hanauer said because we actually have to take a look at it from a perspective of the technical basis for the decision. So, in looking at the simplifications and multiple lines of evidence, we really want to look at it from the scientific point of view. The project, however, has to look at it from a licensing basis, and if you want to make it transparent to the public, you have to do it from a different simplification method. I mean, maybe to the point of an animated power point presentation to show radionuclide transport over time and accelerate the time.

But, I guess, the key issue here that I was--I have heard and I'm trying to see if it's a consensus among the people sitting at the table is that multiple lines of evidence and their independence from TSPA may not have to be a divorce, a separate requirement. That the ability to use portions of TSPA to convince yourself that you understand the processes that work and then--and I have the same problem that Rod Ewing has with trying to find--or tracking from one model to the other, you know, breaking it down to simplified models and understanding the simplified models and being able to get my arms around it is one thing. But then, as you use that and fold it into something else, it is the challenge that people like Bob Andrews have to meet to store it all together to come up with a final answer that plays well in Steve Hanauer's PA that you make for the regulatory regime.

And so, I guess, what I'm learning here as we sit around the table is that there are subsets that we can simplify, but we still have to tie it all together and answer questions that are specific to the individuals; in our case the technical basis, in the case for licensing in the regulatory basis, and in the case for simplification for the general public. So, I guess, is there a disconnect in anything that I've said? I wanted to ask that of just everyone sitting at the table right now with respect to how we simplify and does it have to indeed be completely separate and divorced from TSPA?

KNOPMAN: Knopman, Board. Just a clarification. DOE won't get to licensing if they don't make a case to the public. And, just a reminder, we're all speaking for ourselves here and not for the Board. So, I'm not sure there is truly a divergence of interests here, although I take Steve's point that one can characterize these different objectives of modeling and they are not necessarily complimentary in terms of whether one goes to more complex representations or toward some different or simpler ones.

I look at this question--Board reviewing these questions before they were sent to DOE. I said, oh, that's fine. But, now, I think it's not fine the way we--with this question because it sort of has a kind of circularity about it and just to--I'm not sure how much we're going to prolong this discussion, but let me just say, I mean, again, I think there's a distinction to be made in looking at lines of evidence for physical--some small number of or a single physical process like infiltration is a good example. That is independent of performance. That has nothing to do with performance assessment because it's not being integrated with anything. It's just taken on its own terms. So, yeah, I mean, it can and should be in that--those are the kinds of things where you do develop these multiple lines independent of performance assessment because at that point PAs are relevant. Now, can they be derived independently from--well, yeah, they--I mean, we just said they can. So, I'm not sure there's anything more to say.

EWING: I think I agree with Debra in the following way. All the confusion, I would say, comes from the regulation, at least the last version I read with the wording is something to the effect that performance assessment would be the sole quantitative measure of compliance and then all of the other things would be looked at. I don't know whether that's still the same wording.

But, when it says sole quantitative measure, you lose the sense actually what the performance assessment is. The performance assessment, although it gives you a numerical answer, is very qualitative. So, if you think of it as one of a number of qualitative statements you can make about the safety of the repository, then things fall out pretty simply. You do a performance assessment and qualitatively it gives you a number. If it gives you a number that's too high, you should worry; if it's low, then that doesn't mean that's the answer, but that's useful. Then, you add to that the other thing from my last viewgraph--they've taken it away so I can't remember them exactly--but, you know, stable geologic environment, long travel time, time sorption. If you can check off most of the things on that list, then I think you do have multiple lines of evidence of which performance assessment is one. They're necessarily intertwined because you're speaking of a single site, but you should be able to make a compelling case if there's a case there to be made.

BULLEN: Bullen, Board. Mr. Chairman, never mind. I thought over what Dr. Knopman said and I tend to agree with her.

PARIZEK: I'll make a point again about Bob Andrews.

You know, what he learns, he iterates. He iterates, he reiterates, and he gets a better and better model each time. He gets TSPA-95, 98, so on. It's getting sophisticated. My concern then is when does it now serve a good predictive value? Isn't he at the point where you can make decisions based on it? And, the program really has made forecasts. When the East/West crossing was put in, there were certain predictions about where faults might be found, what rocks would be present, and the condition of the rocks, and so on. And, that was before tunneling. I guess, even before the large five mile tunnel was put in, again predictions were made, all that geological mapping, all the geophysics, and so on. In many cases, the predictions were pretty good and I guess in some places maybe this was surprises. So, to what extent can the program take credit for all the different times it's really made predictions in the presence of incomplete data at different TSPA versions? And then, as you go along finally to this point, and saying what the next predictions are going to make has to do with the next time you make a hole somewhere or go make some observations to see if you really understand it. Because part of those predictions and part of the experiments is to upgrade the process model understanding, right, and get the data for it? And, there's been a lot of work done with that. So, it's getting more and more sophisticated as you go along and still confirmation testing is going to be added to it somewhere along the lines and it's still going to get better. Somewhere along the line, you're going to have to buy into the findings of it or something has to buy into it.

So, when is good enough and when have we removed enough uncertainty that we all feel somewhat comfortable. There will always be some people who won't be comfortable, period. You know that for a fact.

ANDREWS: This is Bob Andrews. I think the project can take more advantage of the learning, if you will, the assumptions made that are verified or changed and some assumptions made that become unverified, and therefore, change other parts of the system. I don't think we necessarily document that historical, if you will, learning curve testing change aspect of the project--you know, I'm speaking for myself now--very well. I mean, we tend to talk about each point in time where we have a major product because there's--it's a particular decision point or whatever that the Department is under which is the same right now. We don't necessarily solve that progression with time as you've gone through and changed models, you've tested models, you've gained understanding. Models of XYZ have changed based on that new information. We generally haven't captured that, I don't think, historical learning curve, if you will, very will in our documents.

PARIZEK: If you put that into the simplified models, it could get worse, things are getting thicker and thicker and more confusing. I mean, if there is a predictive measure that you've been involved with all along, it seems to me, and that's just the nature of science and how you make your discoveries and then upgrade your understanding--

CRAIG: That is an interesting story and it deserves to be told.

PARIZEK: Yes, it is.

CRAIG: It definitely deserves to be told and it hasn't been told.

SPEAKER: To me, that's a good idea.

PARIZEK: That builds confidence, too. So, it's this whole question of capturing the clarity of it all, but for complicated systems, it's sort of like cancer. I got it, I don't know how I got it, but trying to make me feel good about it, you can't make me feel good about it, but maybe there's something we can do. You know, you go on from there. It's the same with this repository. This is a very complicated process, and for the average person, you can't weight into it because you work at this every day and I think you still have some things that bother you about it. Right?

ANDREWS: Both is a little strong. But, we have uncertainties that we could--

PARIZEK: --right? And, you've been at it every day, but for people who are going to come in off the street casually, this is not a casual exercise that you can analyze those things in a casual way and go away--

ANDREWS: No, that's true. And, I think, you know, there are comments made by the Board and comments made by NRC. You know, we've had a series, as the Board is aware and others are aware, a series of NRC key technical issue meetings and discussions and actions that came out of them over the last nine months that are very, very detailed questions and require very detailed responses from Department of Energy and many cases requiring additional testing, additional analyses. So, yeah, I think all those probes and questions are worthwhile and prove ultimately the final product and hopefully enough body of information so decision-makers can make reasonable and good technical based decisions.

CRAIG: I now have on deck Dennis, Tim McCartin from NRC, Priscilla, anybody else? And, Abe. Okay. Dennis?

WILLIAMS: Yes, I do not feel comfortable allowing Debra's comment to lie there without a DOE followup on that. We've realized that because of the protocols of the setting here that it is an individual observation or opinion on your part, but I wanted you and everyone else here to know that I think there are many, including others around this table, that are of the same opinion.

I also wanted to point out a bit some of the tension that we see here between the two things that I think we're trying to do; a transparency and a simplicity on one side, the traceability, the defensibility, the in-depth understanding on the other side. It almost sets up between that public, that Board over here on the simple and transparent, and our other regulator--or our regulator on the other side of it. So, there's obviously a tension there. There's a difference in what the input together to get both of those courses, both of those fronts covered, but I firmly believe that we have to cover both fronts. So, that's why we're here, that's what this is all about. Thank you.

CRAIG: Tim is next.

MCCARTIN: Tim McCartin, NRC. The little one might have might have passed, but in terms of Rod Ewing's comment about the dose from it being the sole quantitative measure for performance in the proposed regulation, that absolutely is true. However, I don't think the Commission views all the other requirements in the regulation any less or any greater than that particular requirement.

And, with regards to the performance assessment, DOE is going to have to address uncertainties in their calculations. They certainly have to address alternative conceptual models. They have to identify the barriers important for the waste isolation. I think that is very important. The Commission, when they walked away from the subsystem criteria in the old regulation, we weighed quite heavily do we want to do this? One of the problems with this subsystem requirements is NRC was sitting here with limited knowledge trying to forecast, well, what will be the most important items for the repository in a quantitative sense? That really is premature, but what we put in was the multiple barrier requirement. I think it's easier for the NRC. I think it provides greater safety for the public. It's harder for DOE. DOE has to identify all the barriers important to performance in the assessment. They have to provide a technical basis for it and explain through those items. If indeed release rate is important to the dose assessment, we get to evaluate it, the groundwater travel time. No matter what it is, all their barriers have to be described, explained, and defended rather than NRC putting a separate quantitative criteria on a bunch of different items that may or may not be the right items. We now have the flexibility. The important items have to be defended. At least when we developed Part 63, that was the rationale that we felt a need for a better regulation.

CRAIG: Thank you. Priscilla, welcome.

NELSON: Thanks. Nelson, Board. I'm very sad that I missed your presentations this morning and I'll look at the transcript to understand better, more better.

There's two things that I wanted to just identify. First, the idea of complex models like TSPA and understanding exactly what happens with propagation of uncertainties through them and understanding what you know when you finish them is really a subject of research. You've got in the next building over there Natural Science Foundation and they have tremendous investments associated with those. When these studies are done by engineering, they do not ask all the questions. Those studies have to be done in an interdisciplinary environment involving social, economic, behavioral scientists, all inputs. It's really not possible to address the issue about complexity of systems outside that full context. So, sometimes, I think we're going to try to understand what the project is doing regarding the complex models like TSPA. Sometimes, the questions are not just towards the technical side. They also--technical, but even towards the sub-technical aspects. They're also going to be important in understanding what's happening inside the model and how the uncertainties will not behave well as the model is compounded. So, just a general observation that we're looking for that kind of an input.

Another kind of piece of information that I'd be interested in hearing about is all the focus on parts of the model, whether they're sub-parts or parts or whatever level in the hierarchy they exist that can be independently assessed by an independent track of parallel thinking. I'd also be very interested in knowing what ones cannot, what parts really cannot be addressed in this kind of a context because that requires a different level of belief of satisfaction of how the model is put together. I know to a certain extent it's in some of the documentation, but as you produce an idea of what are the independent tracks that you can marshal to address a lot of the parts of the TSPA. It would be interesting to see which ones really are judged to be either cannot be or are not going to be regarding the creation of an independent track in terms of understanding that part.

CRAIG: Abe?

VAN LUIK: This is Abe Van Luik, DOE. But, aren't you going to answer her question?

CRAIG: Well, that was a very interesting--which ones can't be? That's a good question.

ANDREWS: I'm not sure I have any--

NELSON: That's a hard question.

ANDREWS: That is. I mean, the one that pops into my head and it's probably not a good one is volcanic interactions with waste and waste package. It's a somewhat--I don't want to say esoteric, but--

WILLIAMS: Slow down there, Bob. We're answering that question for the benefit of our regulator.

ANDREWS: Well, but she asked for the independent lines of evidence. We have a technical basis for the assumptions that have been made, but if I look for other lines of evidence independent from the bases that we've already used, somebody could do something probably, you know. Develop a mock package and put it in a 1200 degree C furnace or something, but I'm not sure how--most of the other systems, as Ardyth said this morning, were adding these sections on other lines of evidence component by component. You know, and as you know, there's probably 30 some components that go into the TSPA. The authors didn't have problems coming up with other lines of evidence in them. So--

NELSON: In followup, if you break it down to its reduction as component parts which is something that hard science and engineering will do, some of the building back up to the more complex models is part of getting towards that complexity. You may be looking for other kinds of independent tracks that really test something about the complexity and the uncertainty that happens when you compound models. So, we tend to go down to the reduction's base in choosing ways we can do that and we have that one for that and that for that and that for that. But, when you put it together, you haven't necessarily tested the increasing complexity of the compounded model.

I think that there are some things that could be tried, things that could be argued that do represent independent approaches to understanding more about these compounded models. It's an interesting question and maybe some additional things will come forward if you ask it.

ANDREWS: Uh-huh.

CRAIG: Is Abe back? Abe's gone. Bye, Abe.

Leon Reiter is a Board staff member.

REITER: Yes. I want to ask a question of Steve. Steve, you spent a lot of time in reactor space looking at reactors and I was wondering is there any insight from looking at that, both general and specific examples, where you used multiple lines of evidence to make arguments and react to licensing or react to considerations? Say, both general insight, and if you have some good specific examples, that would be helpful.

HANAUER: I did indeed spend a lot of time in nuclear power plant safety. In there, we have traditionally addressed this question using somewhat different terms. We talk about defense-in-depth, but in fact, the objective is similar. If we're really wrong about X, this is not a catastrophic situation because why? And, the objective is to avoid being dependent on any one thing, any one model, any one device, any one line of evidence. There are, in fact, exceptions to this in nuclear power plants. We are dependent on the primary reactor vessel whose catastrophic failure we have no answer for. If you insist on relying on one thing because you don't have any choice, this turns out to be a big deal and hundreds of millions of dollars, at least, have been spent making reactor vessels' proof against catastrophic failure in their 40 or 60 year lifetime.

Now, how do we apply this to the repository? Here, we have a very long-term period of vulnerability. And, the basic issue is the same to prevent our being dependent on any on thing, any one piece of equipment, any one model, or any one line of evidence. But, the structure of our safety case is somewhat different because of the inaccessibility and the very long time of vulnerability. In the nuclear power plant, it is continuously available for our monitoring throughout its period of vulnerability; whereas, in the repository, we are required to predict for a very long period of time.

That's twice.

CRAIG: You're our wrap-up hitter.

HANAUER: The multiple lines of evidence in nuclear power plants tend to be oriented toward pieces of equipment rather than pieces of models because it is the failures of pieces of equipment or people doing the wrong thing which are the causes of the events which have been experienced under the events which are predicted. Whereas, in our case, the system is entirely passive and failures of equipment--the waste package comes to mind--are very important. But, the thing which is really problematical for us are these models. So, they have a different emphasis, and therefore, we find ourselves talking about different lines of evidence like natural analogues for things where we see uncertainty which is not to be resolved through any practical amount of testing. And so, we don't have any direct analogues in the nuclear power plant business, at least I don't think of any, for things like the natural analogues. We do need in both cases understandable models and use simplified models where they are appropriate. But, the analogy is only approximate.

CRAIG: Thank you. We're now moving to Question 4 and actually Steve's comments began to get us into Question 4. At times like this, I like to bring up my favorite example of a time when defense-in-depth is perfectly fine if you only have one layer. If you only have one layer and it's really good, you don't need any more. That will do the job. There is in the nuclear area one example that I'm aware of that meets that requirement. What is it? Come on, guys. WIPP. WIPP. There only one barrier, but it's a really good barrier.

Okay. We're now turning to Question 4 which addressed the issue of the relationship between defense-in-depth and multiple lines of evidence. Jeff Wong has the task of summarizing what has happened, the story up to now.

WONG: Okay. Mr. Chairman, just by luck, I think that I will make the little green chairman happy by having brief comments.

The interesting thing is that I sat here and tried to listen for specific comments or specific direction or specific wisdom related to defense-in-depth and I sort of sympathize with the general public in that I really didn't hear anything clear. I didn't hear any clear statements about defense-in-depth. I sort of heard oblique comments to it. I know that Rod brought up the issue that multiple barriers or the issue of the multiple subsystems should be analyzed in-depth to increase the understanding and clarity. I know that they sort of tied various multiple lines of evidence with redundant barriers and Steve Hanauer implied the existence of multiple barriers. Maybe I'm misunderstanding, but the existence of multiple barriers was equivalent to multiple lines of evidence. So, with that, I'm a little confused.

The other part that I go on--and I just listened to Steve Hanauer about defense-in-depth and I agree with you that he took us off in that direction--was that defense-in-depth in the previously Board meetings, I sort of understood it to be multiple barriers. And, Steve now has expanded it to mean that it's to avoid being dependent on any single line of evidence or any single barrier.

Going on with multiple lines of evidence, just as an aside, Bob Andrews used multiple lines of evidence and he gave some examples; the iterative efforts, the modeling efforts, and I mean different iterative efforts by the same organization. He talked about comparisons between different groups; EPRI, NRC, and that. I would agree with that one. And, review, he said peer review to represent multiple lines of evidence, and actually I don't agree with that.

The last point that I kind of wanted to mumble through here is Tim McCartin. Tim McCartin, do you think it's fair that you don't have explicit guidelines or explicit expectations related to defense-in-depth or a subsystem performance? To me, that provides an unclear picture to the public and certainly to your regulative party. So, I don't know if it's good to be sitting so close to Dennis, but I'm glad I'm sitting far from Tim and that's it.

So, I guess I would ask for at least Abe and Steve to sort of expand on what they meant.

VAN LUIK: Abe Van Luik, DOE. It's my impression that in the European and the Japanese situation, there is a heavy reliance on the idea that you have more than one barrier. That almost, but not quite independently, no one who claims independence can assure safety. So, the Swedes, for example, to have a copper container that they claim can do a million years worth of containment and their optimistic case is 6,000,000 years or so. Then, they have a good barrier of compacted clay that swells when the water comes in and that buys them about a million years of travel time through that compacted bentonite layer. If the Norse gods are kind to them and the uplift from the next glaciation doesn't create a fast flow path through their repository, they have additional travel time. But, if the gods are evil and bring the new fractures that come in with the uplift right through the repository, then hopefully the Baltic will be there and dilute the heck out of everything. So, basically, they are looking at very simple ideas that anyone can grasp and they think that this is multiple lines of evidence.

In my meeting in Belgium a couple of weeks ago, a gentleman came up to me and said--in fact, I'm sorry that Steve and Judy are done because it sounded like Steve and Judy. He says I turned down an offer to be on your peer review this summer because, one, you don't meet any of my criteria. You don't have multiple lines of evidence, you've got one barrier, and that's it. I said, well, I beg your pardon, but he didn't change his mind and turn around and, you know, become part of the review team. But, he said, oh, well, that does make a difference.

But, anyway, my point to him was we have continuance, and then after that, the natural system takes over. He said, no, it doesn't because you have a high peak dose. Well, he remembers the VA and the DEISP doses. He said what would you think if we had it down to 120 and then it was still coming down and, if we do, what our biosphere peer review told us to do in the--ICOP-72, it probably comes down to around 50. And, he says, well, bring it down to 30 because 30 is what the ICOP says it should be. So, you know, his thing was you only have one barrier because your peak doses high. That was his simple reasoning. When I said what do you think now that our peak dose is coming down, he says, and you've got two barriers, you've got multiple barriers. And, I think that's a point that the regulation from the NRC when it's final will also make is you need to demonstrate at least two barriers.

So, I think, you know, the whole international community is on the same bandwagon. You need to be able to explain how this system works in such a way that people can understand it. At the same time, you need to explain it in such a way that you can take a scientific group like this and convince them that in their specialty, you have things covered in good enough detail that they can be convinced. And, at the same time, you also--and this sounds like Steve Hanauer now--you also have to take the regulator who has the good fortune of having enough people on staff to redo your calculations and question you on every point of it. You have to also go to that depth and be able to demonstrate to them that you have indeed a safe system.

So, I think, you know, we're on board with this, in a general sense, but there is an idea out there that we really don't have geologic disposal. In fact, that was the accusation made this morning by Rod, I think, and it was also the accusation made by the particular gentleman that came to me in Belgium and said it's not geologic disposal if you have a high peak dose. By definition, a high peak dose means the geology is not protecting the people. So, you know, after I got done with him, he says, well, you're almost there. Just get it down to 30. I'm on your side. Anecdote.

CRAIG: Okay. Dan is next. I'll request people to get a little bit closer to the microphones, please. Dan Metlay and who is after Dan? Oh, I'm sorry. Okay. Go ahead, Tim?

MCCARTIN: Well, briefly, I guess, first, I'd say in terms of defense-in-depth in a broad sense for the group of people from Nevada who are here, I think there's two aspects to it. One is that you want to minimize what can happen. That's true in the Commission for reactor and for geological disposal. That's why you bury it. Basically, you're trying to minimize what can happen. Then, in terms of if something happens, you want to mitigate whatever the consequences are and, I think, multiple barriers comes in that if something happens, then the consequences are mitigated if you have multiple barriers in a very simple sense.

Your question of how we would project this to the citizens of Nevada that we don't have other numerical criteria, I think simply state that we have a 25 millirem dose limit. If EPA comes out with a 15 millirem limit, obviously, by law, we will amend to the 15 millirem. That dose limit, the public dose limit per NRC is 100 millirem. I don't think many people realize the public dose limit for EPA is 500 millirem. It is well-below public dose limits. That's a part of the safety that we're keeping doses well-below that.

In terms of the multiple barriers, in terms of the other--you know, the release rates, container lifetime, travel times, etcetera, I believe what's easier---in my mind, what would be easiest for the public to understand, rather than NRC specified as we did in Part 60, a 300 to a 1,000 year waste package lifetime, a 10^{-5} release rate, a 1,000 year groundwater travel time. The Department has to come forward and explain all the things that are affecting that dose calculation, all the important areas, and they have to defend them. That's what I would go to the citizens of Nevada with. Here are the barriers that DOE has, here's what they're doing for performance, and here's the technical basis for those barriers. And, I'd like to think that we could do that in a simple manner. I fully support the discussion. Ultimately, I think if we go to a licensing hearing, NRC staff will be tasked to discuss this to a licensing board. I think we will have to explain performance in a very simple way that is readily understandable.

WONG: Well, how will you know when they've defended it enough?

MCCARTIN: In terms of?

WONG: You said that they would have to bring it forth, describe their barriers, describe the performance, and defend it. How will--

MCCARTIN: Well, we would be defending before the licensing board, but in terms of does DOE have enough information? We're developing a Yucca Mountain review plan to say the criteria we will use. Obviously, there's some subjectivity. There's no magic number that, gee, if you get these different lines of evidence, these particular experiments, then you're done. It is going to be somewhat subjective and I think that's dependent why we have the technical exchanges with the Department that are open to the public, the back and forth in terms of what seems sufficient lines of evidence, etcetera. But, it's going to vary. There's so much one can do in other areas; corrosion of the waste package, groundwater flow, retardation factors. There's different things you can do for different parts of the system and I think part of it, as we have indicated is that we would expect the lines of evidence. The support for DOE safety case would be commensurate with the contribution to risk.

METLAY: To someone who thinks like a lawyer, I always find these discussions of regulations interesting, but let me try to move the discussion back to questions of multiple lines of evidence and defense-in-depth. There's a lot of terminological ambiguity when you talk about multiple barriers and when you talk about defense-in-depth. Multiple barriers mean more than one barrier makes a contribution to waste isolation and containment. Defense-in-depth, at least as the NRC has used it in reactor safety, refers to not being unduly reliant on a single barrier. And, those two things are really different.

But, with respect to multiple lines of evidence, it seems to me the question that I would like to pose to this Panel, is it possible to develop arguments about multiple lines--sorry, let me rephrase that. Is it possible to develop argument about defense-in-depth, i.e. no undue reliance on a single barrier, outside of the use of performance assessment? And, if so, how would you do it?

CRAIG: I think we're going to let this good question lie there unless somebody wants to grab it and--let's let Ardyth go and then you can be our final helper.

SIMMONS: Well, Dan, your question took off on a slightly different point than I thought it was going to. But, as I heard all these comments and particularly your comment, Jeff, in the confusion that you see about the terminology that we use, it seems to me that there's an obligation of the program to be able to define how we consider multiple lines of evidence to be used, what that means. Steven did that this morning through his viewgraphs, but I'm not sure that there is a common understanding and there may not be agreement either. The lack of agreement probably is okay, but I think that we do need to clear up the idea of whether multiple lines of evidence mean the same as multiple barriers or not because that can create a great deal of confusion.

This is just my opinion now, but I think that one can look at multiple lines of evidence in both a horizontal way, if you want to think of it that way, in terms of breadth within a single process or single parameter, and one can also look at it in a vertical way. That would be lines of evidence within each component of the system. It seems to m that if you put both of those together, you have the weight of evidence and that doesn't mean that you are equating multiple lines with having multiple barriers, but it's that you can explain through more than on line of reasoning, if you will, or inquiry why a certain piece of the system is expected to perform in the way it does.

I think if there's one thing that would be good to try to come away with from this meeting is to have a way that we can explain this definition, if you will, to the public because if the technical audience still has a lot of confusion about it, then it's certainly going to be even more so with the public. And, there are two reasons which Dennis said very eloquently before why we need to have the multiple lines. It's both for the technical explanation of the underpinnings of the system and it's also to create this way of describing how it behaves in ways that people can understand. So, I would just like to plead that we try to come to some agreement.

CRAIG: Rod?

EWING: Let me repeat the question from Dan and give my answers. I hope I have the right question. But, on defense-in-depth, the question is can the multiple barriers be somehow analyzed or used outside the performance assessment? I think I have to say no. The reason is that these barriers, the multiple barrier concept for geologic repositories, these barriers fail partially and they fail over time. And, to evaluate the impact of partial failure over time, you need to do an analysis and that drives you back to the performance assessment. But, I still think you can analyze the performance of the separate barriers and build confidence in those components and that's the redundancy in your, let's say, barrier strategy.

ANDREWS: I was going to say--because I was answering the question defense-in-depth rather than the barriers, per se. The question was can it be evaluated separate from PA and I think there's a couple of examples where, in fact, it already has been analyzed separate from PA. And, I'll take an example from Department of Energy and I'll take an example from the NWTRB.

The example from the Department of Energy is the drip shields. The drip shields, if you look at any PA that we've done, don't make a lot of difference to overall system performance, but they're definitely a defense-in-depth mechanism. They're in case other aspects are much longer than anybody would ever suspect. So, they're in there as defense-in-depth in the current safety case, but if you look at any PAs, you would say why is it there?

The example from the Board is the thermal range under which this repository operates. The thermal range may make very little difference from an actual performance calculational perspective. I think the Board has acknowledged that. However, they also have stated that the uncertainty would perhaps be a little more manageable. I don't think the Board has ever said cooler is better, but I think they've said cooler is probably a little more quantifiable or a little more certain. So, were on to go cooler, it would be a defense-in-depth, if you will, mechanism, reduced uncertainty, but may make no difference, whatsoever, to performance.

So, in those two examples, the defense-in-depth is de-linked from the performance assessment itself.

CRAIG: Thanks. To be a little more precise about what the Board said on the tour. The Board laid out the hypothesis that confidence might be enhanced by going to a cooler repository without offering an opinion that it would be enhanced.

ANDREWS: Okay. I stand corrected.

CRAIG: Rod?

EWING: I have to arrange things so the bell rings while Steve is--

(Pause.)

EWING: I haven't thought it through entirely, but I think I disagree with Bob on this. The drip shields is a marvelous concept, but it's not so much defense-in-depth as a way to defend the assumption which is different than defending another barrier; the assumption that the waste package will last a long time. That assumption is more robust if you can keep the waste package dry. Right? So, I'm not sure that counts as defense-in-depth. It's not there as a barrier except to the assumptions about another barrier.

HANAUER: I have this much simpler answer to Dan's question. Of course, defense-in-depth and multiple barriers can be managed without performance assessment or without probabilistic risk assessment. They were, in fact, invented and applied long before we had probabilistic risk assessment technology available. What they can't be is applied and managed without modeling and calculations. Now, one of the reasons--this seems to obvious to me, although I must admit it's not obvious to a lot of people--is the fact that one of the contingencies that we have to deal with is suppose the TSPA we're using, not some high in the sky TSPA, but the one we have, suppose it has some serious limitation in it? Since we are human, the chances of it having limitations are, in fact, pretty good. We all know that it's imperfect. It's a great piece of work. It helps us do things which we can do in no other way. And, yet, we need an answer. Suppose you screwed up the TSPA and it really gives a false results in some important context? That's one of the reasons we're looking for multiple lines of evidence, defense-in-depth, multiple barriers, whatever manifestation of this aberration appeals most to you.

Now, what I'm arguing for is a dual approach. The TSPA enables us to do may things well including analyzing the effects of multiple barriers and defense-in-depth. But, it is not the only way to analyze multiple barriers and defense-in-depth. Now, you have to analyze them in some way, and in doing so, you have to have models of some kind of how they behave. You may, in fact, have alternate models, you may have simplified models, you may have bounding models depending on whom you're talking to and what point you're trying to make. But, the concept can indeed be implemented without TSPA, and in a certain sense, it must be. That is to say we must find ways in addition to, not instead of, TSPA to do this work and to develop these multiple lines of evidence which constitute our safety case.

METLAY: Is that thinking in the process of being implemented?

CRAIG: We're going to hear from Claudia.

HANAUER: We're going to hear from Claudia and I don't want to go out on that particular limb.

CRAIG: Okay. And, seeing no one else, we desperately need to talk--before Claudia begins or as Claudia is getting set up, let me ask if there are any members of the public who would like to speak? Yes. Wait, wait, wait. I'm trying to determine whether we needed to have a public session. So, wait until Claudia is done and then we'll hear from you.

Claudia, you're on for 15 minutes.

NEWBURY: I don't think it will take 15 minutes. I don't want to hear the bell ringing again.

Okay. When we originally talked about this presentation, we thought we'd be farther along at lunchtime and we'd have an opportunity to kind of get together and talk about what we thought we'd heard and we didn't get that far by lunchtime. So, that's why it says documentation and planning. This is kind of a short presentation on where we think that we have documented multiple lines of evidence and where we think we will document multiple lines of evidence. This is a repeat of what the Board has said which is an indication of what we think we heard the Board.

This is a bulletized version of what Steve had across the bottom of his presentation in many places as what we thought multiple lines of evidence are. What I heard today was nothing that I would add to the list. I did hear that maybe confirmatory testing in some people's minds is not a multiple line of evidence and may be independent expert review in some people's mind is not a multiple line of evidence, but we're still going to do those things.

And, where will we document them? We have a site description already which provides a synthesis of information on the natural system of Yucca Mountain. It has a lot of direct observations, measurements, and a lot of sections on natural analogues. It's very thick. So, if you're looking for a concise technical summary of that type of information, it certainly is not concise, it's thick. So, the site description is fairly lengthy. It's got a lot of information in it. It will be updated periodically, as I understand it.

Michael talked about test and evaluation and we do have a test and evaluation plan that includes work for confirmatory testing. We've had a test and evaluation plan for years. I remember in 1989 when I was first on the program, one of the first things I did in meeting Michael Voegele for the first time was work on test and evaluation plan. So, we've been doing this iteratively for a number of years and we'll continue to do it.

The Supplemental Science and Performance Analysis report, the SSPA, Volume 1 is due out in June and that does have specific sections in it on multiple lines of evidence. We hope it will be transparent. We hope that it will be relatively short. --discussed a number of the examples of the multiple lines of evidence that you'll see there. In fact, Ardyth is listed as the author on every section on multiple lines of evidence. So, she's the expert on the subject.

Here are some examples from the SSPA. Seepage, I believe that Bob talked about that. --preceded it with discussions on infiltration. You can see a lot of the things that we're looking at in terms of alternative lines of evidence that will be discussed in that particular section of that volume. I'm not going to go into all the details. Another example is the volcanic hazard. That section includes studies on late Tertiary and Quarternary igneous activity and again there's some analog sites and additional information on those particular areas. Analogous eruptive centers around the world, yeah. I'm thinking of Santorini and--

Ongoing natural analog sites for radionuclide transport at Pena Blanca, we are continuing work there. Ardyth also mentioned Paiute Ridge, as did someone else, and the Akrotiri site has been brought up, as well. Those results of analog studies are ongoing and they should be out sometime in the fall, I think, yeah, November.

Independent expert review was also brought up. We do have a peer review on the waste package that has begun. I think we have a kickoff late this month or early next month. There is an international peer review on the TSPA and we've just concluded an international peer review on the biosphere. That information is available, as well. Summary reports for these two peer reviews will be available some time in this fall. So, that will be confirmatory data that we'll have available for information.

In summary, we agree that multiple lines of evidence should be used in addition to numerical output from performance assessment to demonstrate safety. We believe that multiple lines of evidence are inherent in standard scientific practice. That's the blue end of the spectrum where Dennis is, but we're kind of--and we are planning to provide a more transparent discussion of multiple lines of evidence in our documents, such as the SSPA and our future documents. We recognize that we have not been as transparent as we should be.

That's it in a nutshell.

CRAIG: Thank you, Claudia. Are there questions for Claudia?

BULLEN: Bullen, Board. I guess, this is just a followup on a comment that Jeff Wong made. Could you explain to me why the program thinks that independent expert review represents multiple lines of evidence?

NEWBURY: Well, we put it in there because in our mind it took an alternative viewpoint of the material that we already had and looked at it. So that it's no longer merely our interpretation. It's an opportunity for another group independent of the program to take a look at the same information and come to similar conclusions or provide us with information on things that we should do that would maybe give us a different interpretation of what we already did.

BULLEN: Bullen, Board. As a followup to that, I think the NRC takes a look at peer review as a method for validating data. Is that also the approach that you're looking at that there's some sort of validation associated with the peer review of the approach taken?

NEWBURY: It's not just validation of data. We believe that--

BULLEN: It's also models?

NEWBURY: We believe that 1298 can be used for models or independent viewpoints. So, you can use it in a variety of ways. If you're only looking at a conceptual model, then you want to validate that, too, before you get too far down the line. You don't want to be out in left field.

BULLEN: Thank you.

CRAIG: Dan Metlay?

METLAY: Since Steve Hanauer referred me to you, both Steve and Bob seemed to be of the view that one can demonstrate defense-in-depth independently of performance assessment. In your repository safety strategy discussion, it was completely in terms of performance assessment, if my memory serves correctly. Could you explain what, if anything, you're going to be doing to talk about defense-in-depth independently of performance assessment and when that work might be accomplished?

NEWBURY: I wish I could. We are rewriting the repository safety strategy and probably will take into account in that rewrite that there are other lines of evidence that we should be using to develop defense-in-depth.

(Pause.)

NEWBURY: Is that okay, Dan?

METLAY: We look forward to reading that.

NEWBURY: Okay.

CRAIG: Okay. Do I see any hands? Don Runnells?

RUNNELLS: Just a question, Claudia, on what you just said. There will be a revised stand-alone document on the repository safety strategy?

NEWBURY: Yes, Bill Boyle discussed that at the January meeting that we were in the process of revising the repository safety strategy and trying to separate out the strategy from the safety case.

RUNNELLS: Okay. Lake Barrett this morning told me he didn't think there would be a stand-alone document.

NEWBURY: Well, we'll have to talk to him.

RUNNELLS: I guess so.

CRAIG: Okay. Last chance.

(No response.)

CRAIG: Hey, I think, we've come to--

SPEAKER: Steve.

CRAIG: Well, I'm not forgetting Steve. No, no, no, no. This is Claudia. We're hitting on Claudia now.

Okay. So, we've come to the end of this session and we now have time for public comments. Steve Kraft of NEI?

KRAFT: Good afternoon. Steven Kraft from the Nuclear Energy Institute. Thank you, Paul, for the opportunity.

Anyone notice how much Dan Metlay represents Ted--looks like Ted--the owner of the--

METLAY: But, not nearly as rich.

KRAFT: Well, I didn't say that. I did notice you didn't offer to buy lunch or buy us a skybox at the game on Saturday.

A couple of observations. We've been given a lot of thought obviously to how the system moves through to decisions or lack of decisions over the next six months and then on into--if the decisions are positive, on into licensing. I have one or two points to make and then an observation.

Dan's statement earlier when he was questioning Abe about the suitability of how stability rules work and it's a point decision on--it's a point number you come up with on dose because you'd run through a TSPA, and I think that's probably right if you only read the regulation. But, if you read the law which trumps the regulation, since you like to think you're aware, I'll tell you that the law says that they have to describe all their considerations of how they get to a suitability determination plus provide about 1300 pages--1300 pounds of documentation. So, now, they can choose not to give all this information and it just weakens their case. So, I suspect they will give all the information and it will all be there. When they issue the document that will ultimately be the topic of the hearings in Nevada, that is the legally required case that then gets tested. So, you have to see what they say in that document. I'm not about to suggest I know what they're going to say in that document. I mean, that's where all this case is made and what the hearings are based on and that's what's being used to make the decisions and that's where you should look, not just in Part 963.

With regard to the NRC regulations, Dr. Wong, I just want to ask you a question. When you were asking Tim about the subsystem performance standards and you were asking whether or not there ought to be some way of telling if DOE is describing it, were you suggesting that it ought to be numerical values or other kind of quantitative requirements in Part 63 like there was in Part 60 or were you going to something else?

WONG: No, my comment was--I wasn't asking whether or not. When I listened to Tim and I read that 963, it sort of --it comes across to me as we'll know it when we see it. And, since I sit in my day job as a regulator, I'm actually looking for some wisdom from Tim because that's what comes to me. Risk is acceptable when you do not have in the regulation and you don't tell us when it is acceptable. So, how do you know? And, I give the same answer that he does. I say I'll know when I see it. And, as I've done that through the years, I feel that that's actually not fair because it doesn't give the public a clear idea what my expectation is and it doesn't give the responsible party clear expectation as to what they should achieve. And so, I was asking Tim how he feels as he has to go before the public and explain his licensing decision and how he goes on to demand information from the DOE.

KRAFT: So, you weren't getting at the fact that in proposed 63, there isn't a specified groundwater travel time or a package lifetime? You weren't asking for that. You were simply asking--okay. I just didn't quite understand your question. I appreciate the clarification because NRC does have in the proposed 63 that we all read a very specific requirement which is the very end of the number, and if you look through the DOE EIS, there is evidence that says that if you try to be too specific and demand adherence to subsystem performance criteria as a long risk, some final number, you, in fact, will reduce the protectiveness of the design to the general public. So, that's why NRC in all of its areas of regulation is moving into risk informed and probabilistic type space.

Lastly, I think that there's a lot of confusion that I was sensing in the discussion. It could be just definitional--I'm not totally sure--about what you all meant about multiple lines of evidence versus defense-in-depth versus multiple barriers, etcetera. I tried sitting here to sort of sort out for myself and I couldn't quite come up with it. I think the reason for that is you're mixing regulatory criteria, defense-in-depth, with a scientific--desire among the scientific community to have multiple lines of inquiry into, well, what do you do to tell us you know that that number is true or accurate or close enough or whatever it is, whatever it is you're doing. I think that's where some of the difficulty I was sensing was coming through. What exactly is a natural analogue and how far away physically from the repository does it have to lie? Does it have to be in another continent or is it okay to be the next mountain over? I mean, I don't know. I mean, these are the sorts of definitional quandaries I thought you got yourself wrapped up.

And, having said that, I really think that in some respects, it's being made far too complicated. You spend a lot of time talking about how people are going to understand. It's far too complicated in the sense that not everything DOE does in the case they make to NRC is also a multiple line of evidence. There are certain things that are and certain things that aren't. And, I think DOE aids the understanding of their licensing case if they keep that separate. Now, it doesn't mean if there's something they learn in another location that teaches them something about something at Yucca Mountain that ought not go into the TSPA or some other analysis, but it ought to also be documented as some separate independent input. Okay? But, not everything is what it is.

Now, we could probably argue for a very long time as to whether or not what I just said was true or not. And, I suspect there are people in the room now thinking to themselves what in the world is he talking about? Because, yeah, everything you do has got this independent nature to it. And, I was only suggesting that it has to do with explaining it to somebody. That if you decide as a matter of policy in the way you're doing it that these four things, whatever they are, I don't really care, yeah, they could argue that they're multiple lines of evidence, but for purpose of priority, let's leave that out and just have that in the TSPA or whatever analysis and have the other part over here to improve the quality of the presentation. And, the reason I say that is to go to where the decision is going to be made. No one initially is making a decision about the site. We're going to do a lot of advising. You all have a specific statutory role to advise NRC we'll do licensing sometime in the future, but even the NRC folks in this room are not decision-makers. Decision-makers take in the full information. I think Debra was kind of getting at it in her questions and Dr. Ewing's presentations was touching on it. That there are, in fact, many ways decision-makers can have confidence that, in fact, what DOE is doing is writing off for them to make the decision. Some of them are not even areas that the Board is responsible for looking into.

For example, the development of alternative technologies in the future. One of the reasons we support things like advanced ATW research, accelerated research, is because sometimes somewhere in the future, 60 or 70 years from now, maybe that will work. And, a decision-maker today can take confidence in making a decision thinking, well, maybe there is something else out there and maybe other things will come along. For example, the performance confirmation which was discussed here and evaluation of uncertainties are other things. So, what I'm getting at is the decision that this all heading towards will be based on factors more than you're looking at and the question, I think, that decision-makers will want is things that will give them confidence. Natural analogues and multiple lines of evidence if explained clearly and correctly in and not mangled up in some discussion about how that also fits in to the TSPA in every single way you could possibly think of, will aid that understanding and give greater confidence than the other way around.

Thank you.

CRAIG: Thank you, Steve.

We've now come to the end of our agenda and there's just a few formalities as we close up. First of all, this has been a new format. We hope it's been useful. We'd like some feedback, particularly from the DOE, as to whether it's a useful format and whether you folks would like us to continue it in other areas. We're open to that.

We certainly want to thank our guests, our consultants, everybody around. The whole panel has been a really good group. The Board opinions, I repeat, are members only. They're not official Board opinions. If we have an official opinion, we'll let you know. Thank you for the technical support staff, Scott. Thank you, once again. And, the two Lindas who are in the back some place, not to be seen, Linda I and Linda II, thank you.

And, what have I not done? Bill Barnard, what have I not yet done that I need to do?

BARNARD: We're all set.

CRAIG: We're all set. Thank you. We're adjourned.

(Whereupon, the meeting was adjourned.)