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1995 FALL BOARD MEETING

STRATEGIC CONCERNS, TOTAL SYSTEM PERFORMANCE ASSESSMENT

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Arlington, Virginia 22203

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P R O C E E D I N G S

[8:30 a.m.]

LANGMUIR: Good morning. I'm Don Langmuir. I'm a member of the Nuclear Waste Technical Review Board and chair of today's session on Total System Performance Assessment, conveniently known to us all as TSPA.

TSPA is an analytical method for assessing the ability of the proposed repository to contain and isolate radioactive waste. It can serve several functions.

It will be an important measure by which the suitability of the Yucca Mountain site will be judged. And if the site is found suitable, TSPA will be the primary means by which the NRC will judge whether the proposed repository can be built and operated safely.

At the present, where site suitability and regulatory compliance are not yet being evaluated, TSPA can and should play a significant role in guiding site characterization, assessing priorities, evaluating different engineering designs, and estimating the impact of contemplated changes in standards and regulations.

At the January 1994 Board meeting we heard about three DOE supported studies aimed at assessing repository performance. Today we will hear about TSPA-95, the latest effort by the DOE and its contractors in this area.

In a series of talks that will extend through the early afternoon presentations will be made addressing the objectives of TSPA-95 and its basic assumptions in the different earth sciences and engineering, its results, conclusion and related sensitivity studies.

A special effort was made to evaluate the impact of different factors affecting waste package performance and engineered barriers.

Similarly, sensitivity studies were carried out to present the results using the different performance measures now being considered.

We will also hear an update on TSPA efforts in assessing the effects of and consequences of volcanism.

Finally, the DOE will provide us with some of the insights developed and what they can mean for the Yucca Mountain program.

We have also asked Paul Davis of Sandia National Laboratories to provide us with some lessons learned in attempts to make use of performance assessment. In addition to being a former adviser of the ACNW on TSPA at Yucca Mountain, Paul has had much experience in the WIPP and GCD projects for disposing of transuranic waste in New Mexico and at the Nevada test site.

Following the technical presentations, we will host a round-table discussion on the uses of TSPA. More about that later.

Finally, there will be a period for public comment.

Having said all of this, the first speaker is Abe Van Luik of the DOE, who will introduce the session this morning.

Abe, the floor is yours.

[Slide.]

VAN LUIK: The reason I put up the title slide, which I usually don't do, is I wanted to explain to you I've stood before this Board three or four different times in the last five years, or since you were created, each time with a different badge from a different organization. If this is a character defect, I apologize.

[Laughter.]

VAN LUIK: I have been fiercely loyal to this program, and any time an employer moved me aside from the mainstream, from the heart of performance assessment, I just shifted so I could stay with it.

I came into this program in about 1982 as part of the crystalline work that was being done at that time, because I'm an environmentalist and I see this as very useful, environmentally correct, and an opportunity really to work in something that really solves a large public problem, environmental as well as policy. So I am very loyal to this program. I'm now with DOE, and it's going to take an Act of Congress to kick me out. But yesterday I heard that that is a very good possibility.

[Laughter.]

[Slide.]

VAN LUIK: Last night at dinner, excellent food, excellent people, and good wine, someone made the statement, and I think it was very sincere and probably correct, that TSPA-95 is the best that has been done. I really like the TSPA-95 product. I'm impressed with it. But one of the reasons I wanted to give this particular introduction is to show that, yes, we have come a long way, but there were some very good analyses done very early in this program from which we have learned we are not working in a vacuum.

[Slide.]

VAN LUIK: When I first came into this program my first assignment -- well, actually, my second assignment after the crystalline work, was for headquarters, and I was assigned to follow around the EPA Science Advisory Board. I will never forget one meeting where a certain Dr. Pigford, representing the NAS, had a surprise pulled on him. He was presenting results of his work, and someone from what at that time was the Nevada nuclear waste storage investigation stood up and said, Professor Pigford, we have decided that we are going to put this repository in the unsaturated zone.

Professor Pigford stopped for a minute and said, how long does it take your water to get from where you want to put the repository to the saturated zone? He was modeling a saturated zone repository.

They said, oh, we think about 30,000 years.

Without skipping a beat, he goes right on with his presentation and says, relabel the bottom here by three orders of magnitude and the results are still correct.

[Laughter.]

VAN LUIK: One of the things that we learned from the WISP report, the 1983 performance assessment, is that in a closed basin he just observed that limited groundwater flow could lead to substantial doses. Basically you don't have the Columbia River carrying stuff to the ocean. It is something that we knew right off in 1982 when he did the draft, in 1983 when he published the report.

Siegel and Chu also suffered under the limitation of not knowing that we had changed to an unsaturated repository. We have to give them credit. They were working for the NRC in 1983 looking at can you do a calculation that shows compliance with this new type of standard, and they were the first ones to throw in thermal effects. They looked at the buoyancy of putting a repository in the saturated zone under Yucca Mountain, and the buoyancy drove flow up into the unsaturated zone and moved it out, and they said, hey, that accelerates things.

So we take credit sometimes, because we lose track of history, for being the first ones to throw in thermal effects. Not so.

But they made an interesting observation. They said for some conditions, and the viewgraph says oxidizing conditions and low aquifer flow velocities, it was only for some cases in their probabilistic analysis that U and Np violated draft standards, because they assumed a higher solubility in oxidizing conditions. So we knew that back then.

For the environmental assessment, Thompson, et al at PNL, good friends of mine, did a nice calculation. Unfortunately, the regulation changed while they were doing their work. So they showed possible compliance with the standard. They also did some 250,000 year calculations of dose.

[Slide.]

VAN LUIK: Right in the middle of their work the EPA changed the standard somewhat, and Sinnock et al came into the picture and redid the analysis for the new version of the standards and showed basically that the sites could probably comply, and that is what is in the environmental assessment if you go back and read that. A nice piece of work.

Sinnock was very inspiring, because he said let's take really what our best estimate optimistic case is. Usually we do worst estimate pessimistic cases, and in his best estimate optimistic case nothing really came out for almost a million years. It was an interesting analysis.

The problem with a lot of these analyses, of course, is that now we are a little bit more sophisticated about the conceptual model of flow in the mountain. At that time flow was extremely slow and now we are looking at other alternatives, and you will hear a little more about that later.

EPA in 1985 actually did a very nice analysis for Yucca Mountain, including faulting, drilling, and volcanic events, and showed that those might not be the big, scary things that we thought they were.

McGuire for EPRI. I really appreciate the EPRI efforts, because they consistently do what we would call best estimate or optimistic calculations where we sometimes dig ourselves into a pit of despair because we do these pessimistic calculations and on bad nights Leon wakes up at three in the morning worrying about these things.

[Laughter.]

VAN LUIK: But on bad nights we wake up, saying, gee, is this real?

We appreciate the EPRI work, because it shows us that it may not be.

[Slide.]

VAN LUIK: I also personally appreciate the fact that the NRC is doing competent performance assessments. I hope Margaret is not here.

[Laughter.]

VAN LUIK: I must say that the regulator has become much more sympathetic of our plights since they started doing their own calculations. I find that there is an appreciation for the difficulty of doing a performance assessment from these people, where before they didn't sound like they were going to give us a break at all in anything.

PNL. I personally managed this one, so I feel some ownership of it. We did something requested by headquarters in 1988 and actually published it in 1992. In response to a congressional inquiry, headquarters wanted to have a risk evaluation. They said the heck with the standard; we want to know what the risks are. We don't understand this EPA standard. Does that sound familiar?

We calculated doses for carbon-14 and said, hey, it's a no nevermind.

We did a population dose calculation. If you ever want an obscure measure of performance, go to a population dose. You get a big number and it's totally meaningless unless you know how many people are involved and over what time period. Here we have ten to the third person-sieverts, but it's over ten to the sixth years for a population that is fixed at, I think it was, 160-some people right in Amargosa Valley. We did this because that's what we thought was the right measure, but in retrospect, that's a very difficult concept to explain to people.

We added in climate change, volcanism. The final tally was 0.3 to 131 health effects, which turned out to be about 0.1 to 1 percent of background. Which sounds about right to me, except we kind of made a boo-boo there and didn't up the neptunium solubility to reflect oxidizing conditions.

Every one of these is an excellent analysis, but when you look at it in detail you find a little Achilles heel, and all of these things have brought us to the point where we have a more comprehensive, more realistic, more complete understanding of things. Each one of these has made a contribution.

[Slide.]

VAN LUIK: TSPA-91 is the first one in the series of now three TSPAs done by DOE on behalf of DOE.

These are my personal impressions. If you talk to other people, they may not feel this way. I think the most important thing that came out of TSPA-91 is that the human intrusion and the basaltic event were shown not to be as scary a thing as we had thought before we did the analyses.

In fact, we went into TSPA-93 looking at these secondary effects from volcanism because it was suggested to us that, yes, you looked at the primary effects, but the real bad, scary thing is the secondary effects. Of course, secondary, almost by definition, turned out to be less scary than primary.

These were good analyses.

EPA did one in 1993 that most people don't even know about. For the re-promulgation of 40CFR191 they used Yucca Mountain as one of their four cases, I believe, and they put in a transuranic source term and showed very low doses for 10,000 years for TRU waste at Yucca Mountain. So if WIPP doesn't work, maybe we can sell them a mountain.

[Laughter.]

[Slide.]

VAN LUIK: Duguid and company two years ago showed that over 100,000 years doses were very strongly related to the solubility of 237Np. While we were doing TSPA-93, actually, Duguid did four little TSPAs. Some people are just talented and you have to keep them down.

He also showed by assuming a one meter capillary barrier that you could significantly reduce doses and cumulative releases at very long time periods, like in the hundreds of thousands of years, and that is why all of a sudden we are very interested in EPRI's work. EPRI is supporting Conca and company in looking at this type of barrier in their feasibility, et cetera.

EPRI is independent of us. We don't work for them; they don't work for us; but we are very interested when someone leaps ahead and looks at something that we are going to be potentially interested in.

EPRI, also, as you heard John Kessler, is doing some very good work on biosphere, looking at how to model the biosphere. We are not going to slavishly copy them, but we are definitely going to look into what they are doing.

[Slide.]

VAN LUIK: Finally, TSPA-93. I've already mentioned that it kind of put the nail in the coffin for basaltic volcanism as an important player. It calculated peak doses out to a million years, and the doses were very high. This was very disconcerting to the project.

[Slide.]

VAN LUIK: I must throw in something here. These PA people are very independent and they are very honest. You can't tell them here is your target for your calculation, make it come out that way. TSPA-93 was a very good calculation both on SNL and the M&O side, but it did not come out in a very pleasant way for the DOE.

One thing that that has caused is a very fervent dialogue between the engineered side of our project the PA side, and many of the improvements in performance that you see in TSPA-95, which when I shut up and sit down you will start hearing about, are because of that dialogue and because of the improved understanding that was transferred from that side to the PA side.

I think basically what I wanted to do I've already accomplished. I wanted to show that TSPA is in a long line of distinguished products.

Another thing I wanted to say is that when you look at the recommendations at the end of the TSPA-95 report, which was done independent of the waste isolation strategy, by jiminy, most of the recommendations are exactly the same as in the waste isolation strategy, which I think is serendipitous, but it's only right. If we did the right thing, we should come out with the right answer.

In your package you have a little backup where I give a little bit more detail about each of these studies, but I think it is important for us to never lose track of the past, because then you tend to repeat it, and we can't afford that.

Thank you very much.

LANGMUIR: Thank you, Abe.

The first presentation now is Bob Andrews of INTERA, who is going to speak to us about objectives and approach. I gather that we are going to learn why there is no Achilles heel in TSPA-95.

ANDREWS: Good morning. Yesterday we heard a lot about the NAS recommendations and then followed that with Steve and Jean talking about the waste isolation and containment strategy. Both of these very important products used, I think as Abe alluded to, some insights, if you will, from total system performance assessments done by a number of organizations, including the M&O, Sandia for Yucca Mountain, EPA and NRC also and EPRI for Yucca Mountain, and a lot of international experience that had a real role in the development of both of those products.

What we are going to be talking about today is TSPA-1995, which was done concurrently with the development of the waste isolation strategy, but you might say it is essentially the implementation of both of those two activities, the implementation of the recommendations of NAS, if you will, and an implementation of the actual strategy.

I am going to go through very quickly an introduction of the approach, objectives, philosophy, if you will, that was taken over the last six months in the creation of TSPA-1995. The following presentations, of which there are about six, I guess, will get into the details of the assumptions, the results, the implications of those results.

The total system at Yucca Mountain clearly is a complex system. Both the engineered portions of the system and the natural portions of the system are complex, and trying to capture that complexity in a reasonable way is a the goal and objective of performance assessment in general and total system performance assessment that you are going to hear about in particular.

We are going to walk a little fine line in the next few hours between trying to present some of that complexity to some level of detail but not get mired in the details too much. So we will keep at some general level. When you have the detailed questions, we can get into them, but we will keep it somewhere between the very detailed and the most abstract.

[Slide.]

ANDREWS: These are the main topic items I want to hit on in this introduction.

The philosophy of almost any total system performance assessment is to focus on those components that are most important. We don't spent a lot of time worrying about those components that are less important.

Clearly when you get into a licensing sort of arena, some of these less important aspects or things that we believe or perceive from sensitivity studies are less important will be discussed and addressed.

When we are looking at trying to prioritize information needs and prioritize design and characterization activities, we generally focus on those things that are most significant to performance. But we try to be complete. So all relevant processes are included, and there are a lot of processes that you are going to be hearing about for the next few hours.

[Slide.]

ANDREWS: I put these things in quotes because sometimes these words get into legislation and they get into things that we really have to follow, things like reasonably assured of how the performance is. So we try to be reasonably representative, but in a number of cases, and you will hear about a number of those cases, we don't have things that are representative; we don't have tests of what water looks like on a waste form 100,000 years from now. So you try to be bounding in those cases, or conservative. So you are going to over predict, if you will, releases or doses when you make these bounding, conservative assumptions.

One of the important goals of all performance assessment is to acknowledge that things are uncertain and things are variable and incorporate that uncertainty explicitly, whether that uncertainty be conceptual uncertainty or whether that uncertainty be parametric uncertainty, both of which you will hear about today. They are incorporated in the analyses.

The impact of that uncertainty, whether it be conceptual uncertainty where we do some sensitivity studies or whether that be parametric uncertainty where we are doing sort of a probabilistic consequence assessment or probabilistic risk assessment, if you will, is directly incorporated.

[Slide.]

ANDREWS: So you will see the effects of those uncertainties explicitly and try to answer, if you will, Dr. Cohon's question as he left it to everybody last night: Well, what makes a difference?

We will try to address that "what makes a difference" issue from a conceptual point of view and a parametric point of view. If you define what makes a difference, that provides some input to both the characterization and design efforts.

As Abe pointed out, you start from somewhere. You don't start from scratch; you start from the fact that a lot of work has gone on funded by the project on previous TSPAs and previous process level understanding.

So you start with, well, what did you recommend at least at the end of the last TSPA and let's see if we can't address some of those things that were recommended.

These things have been presented to the Board following TSPA-1993, but one of the things was that we have in-drift emplacement and we probably should try to capture the processes going on inside the drift, at least to the extent possible. In particular the thermo-hydrologic processes going on, and capture those realistically, capture as representative as we can, anyway, what happens given that near-field environment in the drift as it impacts the waste package degradation. In particular using humid air corrosion kind of processes rather than aqueous corrosion processes. You will hear about that.

The Board has been presented a number of times alternative conceptual models and data that support alternative conceptual models of flow and to a lesser extent transport, because I think the primary focus has been on flow so far, in the unsaturated zone. So alternative models there are needed.

By the way, in addition to being recommendations coming out of TSPA-93, these were observations -- maybe "observations" isn't quite the legal word -- made by NRC. When we had some technical interchanges with them they also "noted" these same three items.

Then we also pointed out in TSPA-93 that the significance depends on the time frame that you are interested in. I think Jean had a nice little graphic yesterday of what is important over different time periods. She didn't actually put times on that access, but performance assessment did put times on that access in TSPA-93 in terms of when is package degradation important, when is the unsaturated zone hydrology important, and when are some near-field effects important.

[Slide.]

ANDREWS: A little bubble diagram or road map, if you will. All of these things will be hit on today. The ones, twos, threes, fours and fives plus-minus represent the five items of the waste isolation and containment strategy almost in that order.

Low aqueous flux, low humidities in the near-field environment, giving a good environment for the packages to sit in.

The package degradation, of course, having to occur before nuclides are mobilized, the mobilization being a function of the alteration dissolution rate and solubilities.

The release. We are going to talk a lot about EBS release this morning.

Leading to the source term, if you will, to the geosphere, and unsaturated and saturated zones.

Transport. These, of course, include dispersive dilution mixing kind of effects in the saturated zone.

Finally, we now have the biosphere, which gives us ultimately a dose.

Also shown on here, and we are only going to talk about one external event, and that external even later on this afternoon is going to be volcanism and its impacts.

[Slide.]

ANDREWS: Given our recommendations in TSPA-93, it is not too surprising these bullets that are on what are the objectives of TSPA-95 is to put more representativeness in the EBS waste package degradation areas and to acknowledge that alternative conceptual models of flow and transport exist but test their significance.

[Slide.]

ANDREWS: Of course it's not like we are in a vacuum in performance assessment. There is a lot of work going on within the design program, a lot of work going on within the site program. All of that information, revised information, if you will, impacts the TSPA-1995. You try to use the most current information or understanding that exists, starting with thermal load.

So there are two thermal loads, a low and a high. The high is the preferred thermal load, but for the sensitivity studies we wanted to look at the impacts of low versus high.

Backfill is an alternative that DOE is

investigating. There is a systems study going on in FY-96 to address backfill issues, design-related implications as well as performance-related implications of backfill or no backfill.

We looked at it in four different ways. You are going to hear more about this later. The first case is no backfill; the second case is backfill is there but only as a thermo-hydrologic impact, if you will, so it's impacting humidities and temperatures but not in there to impact any aqueous flux in the drift.

The third case is somehow an intermediate case, saying the backfill is there and it plus the package itself limits the fact that you have no dripping through the package even though you might have dripping on the package. You have advection through the drift but no advection through the package.

The last case is the capillary barrier where there is no advection through either. This will be talked about more with some schematic pictures to depict these things.

The effects of humid air corrosion and the effects of cathodic protection on the package degradation and the package failure distribution, also revised since TSPA-93.

[Slide.]

ANDREWS: Revised site information. The Board has been presented a number of maps from Allen Flint and company looking at spatial distributions or possible spatial distributions of infiltration over the mountain. How that infiltration is distributed at depth is quite uncertain, and there are various alternative models, if you will, out there for how that is distributed. We will look at two of those. Maybe there are more. We looked at two, a low range and a high range.

The possibility of there being fracture initiated flow and hence transport. So alternative conceptual models, if you will, of transport in the unsaturated zone have become recognized more, especially with the potential observations of potential young ages at depth and their potential implications. So we wanted to test the sensitivity of the results to alternative models of how transport occurs in the unsaturated zone.

[Slide.]

ANDREWS: Of course there has been a lot of work at LANL over this time period looking at radionuclide solubilities and retardation factors. Those new numbers, if you will, or new distributions coming from their laboratory program have been incorporated into TSPA-1995.

[Slide.]

ANDREWS: In addition to focusing on what we did do, it's pretty important to tell you what we didn't do so you don't have any false expectations. There are probably a lot more things I should put up here, but I'll just give the big ticket items, if you will.

First, we'll say that the primary container degradation mode assumed is going to be pitting corrosion by humid air or aqueous processes. Other degradation mechanisms which could be postulated, and there are a number of them, including microbial induced corrosion, et cetera, are not addressed in TSPA-1995.

The impacts of the near-field thermo-hydrologic environment on thermal chemistry and on thermal mechanics and the potential back circling of those onto thermo-hydrology and onto the in-drift conditions were not considered in TSPA-1995.

Carbon-14 in the geosphere has moved in aqueous phase now instead of gaseous phase. We are trying to maximize the aqueous dose, if you will, maximize the aqueous release, thinking that this has sort of prejudged what NAS was going to recommend with respect to the standard to EPA, but we thought that carbon-14 released in the gaseous phase, they were probably going to conclude, was not a big issue and that one should focus on aqueous releases and doses. The NAS report came out August 1. Our draft report came out at the end of August. I think they actually recommend looking at carbon-14 doses even if it comes off in the gaseous phase. That we did not do.

We only looked at saturated zone transport to the five kilometer old fence, if you will, the accessible environment as defined in 40CFR191. Is that still the accessible environment? Who knows. But that's where everything is going to be presented at, five kilometers.

We did not look at alternative biospheres. The biosphere you are going to look at is the peak maximally exposed individual at that five kilometer fence who pumps the water and drinks the water. It's not a probabilistic biosphere; it is not a probable biosphere even; but it is a biosphere. You might say it's Appendix D instead of Appendix C of the NAS recommendations.

[Slide.]

ANDREWS: What's the approach? Let me go through it real quick.

First, you get the data and the design-related information.

If possible, you develop or use the process level models. You are going hear this morning about three of them, the unsaturated zone, fracture flow for drift-scale thermal hydrology and for humid air corrosion.

With these process models, general multiple realizations.

You then abstract from those generally response surfaces. You will see those response surfaces and their uncertainty later on.

[Slide.]

ANDREWS: With those response surfaces and other functional relationships, trying to account for the uncertainty observed in laboratory measurements. Those are all input into a total system performance assessment piece of software that then does all the sampling for you, and a miracle occurs. The results come out.

[Laughter.]

ANDREWS: With these multiple realizations we are going to look at today four measures of performance. Clearly the one most people focused on yesterday was the million-year long-term individual dose. We will look at those results that lead up to that.

Clearly the package has to fail before you get any release from the EBS. You have to have release from the EBS before you have release to the geosphere; you have to have release from the geosphere before you have release to the biosphere and then get dose.

Maybe risk is an ultimate measure of goodness or performance or safety, but we will stop at dose for this presentation.

But I think it is important to point out that you had to get there somehow. So you had to go through a series of steps, and we are going to show you that process.

[Slide.]

ANDREWS: Here is your menu for the day. The feed of information from data, if you will, or process level understanding or synthesis of information on the left-hand side to the measures of performance on the right-hand side.

Each of the presentations will kind of key back to this and try to show you where you are on this road map.

[Slide.]

ANDREWS: The seven courses after this opening soup are listed here. We are going to start with the process level models with respect to saturated and unsaturated zone and flow and the thermal hydrology. There is no explicit performance measure being addressed there, but it is input into the total system analyses, and I think it's useful for you to get an understanding of some of the process level information that is fed in.

Secondly, we are going to look at the package degradation models and their results, i.e., failure distributions, and "failure" here is in quotes because the first pit is not failure; it's a failure distribution of a number of pits as a function of time.

Mobilization and EBS transport will be presented as well as peak EBS release rate.

Unsaturated and saturated zone flow transport.

Volcanism effects.

Finally, I will conclude with what are the implications of this work to site and design.

If there are any questions on the introduction, I will answer those now.

LANGMUIR: Thank you, Bob.

Let me start with a question. This is great and I'm delighted with the progress. I have to as a critic go to overhead 9, the things you acknowledge you haven't been able to include yet, and ask you about them a little bit.

Overhead 9, Issues Not Addressed. The ones that obviously struck me were the second bullet, which I presume incorporates or includes the refluxion issues, the question of moisture that could come around and around and around again in a refluxion mode in the mountain, and this might be unsaturated zone water moving around repeatedly; it might also include saturated water from the groundwater below coming up into the repository and being refluxed as well.

My first thought as you were talking about this this morning was that, well, maybe you could consider this in TSPA-95 or 95.1 by simply increasing the perception of an infiltration rate at the top of the mountain as a way of thinking about what that might do. You have already done this. You've got two different infiltration rates being considered.

In a sense this might just be an extension of that way of looking at what is happening. Those effects would be increased another time by the idea that you had refluxion water being added. But then you have coupled processes which you are not considering either, which, of course, is what the heater tests are all about, whether you actually increase or decrease permeabilities in the mountain because of coupled processes along with all this water.

My big worry is going to remain, I'm sure, having not thought about these things, putting them in this model. I'm going to be asking you what kind of guesses you may have as to what they might do to the model and the doses that come out of the other end of it.

ANDREWS: I don't want to take a lot of time here, but I will try to respond. You have to realize we are trying to model 10,000 packages, roughly. It's 8,000-and-something, but for nice round numbers it's 10,000. Those 10,000 packages are spread out over an area.

The permeability is clearly spatially variable already over quite a wide range, as you will see. Especially matrix permeability, and if you throw fracture permeability in there, it's probably even greater. That variability we try to capture in some of our analyses.

Do I have a change in conditions due to thermal chemistry impacting permeability that is greater than my uncertainty or my variability that exists already? That's very questionable. We kind of have this sense that probably not. In other words, we are in the range of variability anyway that nature has dealt us.

LANGMUIR: This is extremely important, because if you can persuade all of us about this, we don't need to have any thermal tests.

ANDREWS: I'm talking about thermal chemical impacts on permeability.

LANGMUIR: Okay. That's one side issue, but it is clearly very important to us today to get a sense of how you feel about the need to do the tests that are going to take us the next ten to 50 years.

ANDREWS: We will talk a lot about that.

LANGMUIR: I've been hogging the floor here. Further questions from Board members?

Jared.

COHON: I found this extremely useful and helpful. As the person newest to this, I guess I have some basic questions to help me with some of the terminology. First of all, a really basic one.

Using the words "geosphere" and "biosphere," do I infer correctly from that that basically the biosphere includes anything that is alive and the geosphere includes everything that is not, all the physical aspects of the environment, including the surface?

ANDREWS: That is true. I think we probably use biosphere a little bit more like it includes the well and a description of that person's well or group's well and where they got their water and how much water they got. There is some demography in there as well as that water with whatever dissolved nuclides might be in it and how those get to the individual. So it's not just in the individual. Clearly that is something we are getting from actual table lookups, but how that water got to the individual would be the biosphere.

COHON: Could you go to your overhead 12, which I think is great if I understand it.

ANDREWS: You understand it then.

[Laughter.]

COHON: I don't have to ask any questions then.

What do you mean by model abstraction?

ANDREWS: That's a good question. These process level models are very detailed, generally deterministic models in their very nature. It is not something that we are going to embed directly into a probabilistic assessment, which all of these are, because we are trying to address uncertainty in parameters.

When we go from this detail of the process, a 2D or 3D representation of the site and some process going on within that site, flow or transport or something like that, to this performance assessment model, we do something here. Generally we run these models for enough realizations to generate some response surface fits to try to capture the impact of the uncertainty in the parameters that occur here and the variability in those parameters to abstract the relevant results that we need for performance assessment.

For example, drift scale flux, the actual percolation of water at the scale of a drip, so at the scale of a waste package, ten square meters or something like that. That is derived from a fairly simplistic model but done external to the actual analysis. What we take forward then is a functional relationship of dripping or percolation flux to something else. In this case infiltration rate. So we make this functional relationship based on the results of the process level model.

In the best of all worlds that process level model is directly tied to some observations. So you have some confidence that this model somehow represents reality, given uncertainty and spatial variability, of course, are there.

In many cases, although there are process level models being developed by the site program and by the engineering program -- some of these things are more engineering side of the shop -- those models haven't been full tested or proved, let's say. So there is uncertainty in the process level model. When we try to account for that we generally are doing sensitivity analyses: well, if it's process model A or process model B, here's the impact.

This abstraction process is taking that process level model, running some realizations on it to get some distribution to acknowledge that it's uncertain and you have spatially variable parameters, and then generating response surfaces which are then what are actually used in the assessment.

COHON: Do these represent all of the models in a TSPA?

ANDREWS: With the exception of volcanic effects models, I think yes. I might have missed some, but it's general enough so that maybe there are some sub ones, but I think it's pretty complete.

COHON: Thank you.

LANGMUIR: I would like to keep us on schedule. We are there right now. There will be plenty of time after in the round table, I think, to address further questions.

Our next speaker is Srikanta Mishra, and his topic is ambient and thermally perturbed flow in the unsaturated zone.

MISHRA: Good morning.

[Slide.]

MISHRA: The topic of my presentation is a description of the ambient flow models and the thermally perturbed flow models and their abstractions for TSPA-1995.

[Slide.]

MISHRA: The motivation for this presentation, as Bob alluded to, is essentially the fact that the total system simulator that we use in our calculations does not explicitly include hydrologic and thermo-hydrologic process models.

In particularly, when do the calculations of EBS, the engineered barrier system performance and adjust for transport calculation one needs from some external source some information regarding the velocities and fluxes.

The second aspect of abstraction that I am interested in here is with respect to the degradation of the waste package and also of the release from engineered barrier system, which requires information regarding the temperature, the saturation, and the relative humidity in the vicinity of the waste package, and these also have to be abstracted from external models.

The talk is essentially divided into two components, the first part dealing with the flow in the unsaturated zone and in the drifts under ambient conditions, the second part dealing with the distribution or the flow and fluids in the drifts, and that's the thermally perturbed flow regime.

[Slide.]

MISHRA: Just to anchor myself to the information flow diagram that Bob presented. The first part of the talk deals with the modeling of unsaturated zone flow. Beginning with the information that comes from the site geohydrology, we develop unsaturated zone flow models at the site scale from which the information regarding the unsaturated zone flux is abstracted, feeding into the geosphere transport model. Later in the afternoon David Sevougian will be talking about the relationship between cumulative release geosphere transport and unsaturated zone flux.

The second model that we use, as Bob mentioned, is a stochastic fracture flow model from which comes information regarding drift-scale fluxes, which are then used in the EBS transport calculation to predict the peak EBS release rate. Later this morning Jerry McNeish will be talking about this.

[Slide.]

MISHRA: Moving on to a conceptual picture of the hydrology of the scale of the mountain, here I show a cross section of Yucca Mountain with the hydro-stratigraphic unit beginning with the welded Tiva Canyon, the non-welded Paintbrush, and the welded Topopah Spring, which is the repository horizon, the non-welded vitric Calico Hills and the non-welded geolithic calico Hills.

The intent of this diagram is to show essentially the distribution of water as it enters the natural system through precipitation. Water enters the mountain as infiltrating water and then by the time it gets to the repository horizon it has been redistributed spatially into some percolation flux, which we call Q perc.

Then one looks at the partitioning of this percolation flux as two different scales.

At the scale of the repository, which is, let's say, on the order of a kilometer, we are interested in microscopic partitioning of that percolation flux between a flux that goes through the fractures and a flux that goes through the matrix.

Then when one focuses more into the scale of an individual drift and not the individual waste packages, as is shown in this inset, the idea is to determine how this overall percolation flux is partitioned into some spatially variable percolation fluxes for each of the individual drifts and subsequently how that percolation flux is further partitioned into flux through a dripping fracture entering into a drift and possibly impinging on a waste package with the rest going around the drift and flowing through matrices.

Essentially the two components of the unsaturated zone hydrologic model are the first one that focuses on a larger-scale flux partitioning between fractures and the matrix and the second one that looks more closely at the scale of the individual drifts and then diverts flow into dripping fractures all around the drifts through the matrix.

[Slide.]

MISHRA: To put it in words, there are two objectives of this part of the presentation.

The begin with, the site-scale model developed by LBL-USGS to develop repository-scale abstractions, taking into account various sources of parametric and conceptual uncertainties.

The second objective is to use a stochastic fracture flow model to generate drift-scale abstractions.

The important thing to keep in mind here is that we are assuming thermal effects had been dissipated by the time the EBS geosphere transport is initiated. This, of course, is an important assumption.

The key impact of this is that in a way this is sort of a conservative assumption because we are not taking any credit for the fact that thermal effects might further affect the movement of groundwater below the repository horizon and then sort of making it flow away from the repository; there might really be moving flow into the repository horizon because of capillary effects.

[Slide.]

MISHRA: What I want to do is show a series of pictures here in which I talk about the various components of the simulations. First of all, I will be talking about the unsaturated zone, the repository-scale simulations, and then go through each of the components in a little bit of detail.

Beginning with the first bullet, I talk about how we use the LBL-USGS site-scale model to simulate the movement of fluids in the unsaturated zone, and in particular to develop correlations between infiltration and the velocity in the fractures in the matrix and the flux in the fractures of the matrix.

[Slide.]

MISHRA: Here I show a two-dimensional cross section extracted from the LBL-USGS model. Once again, you see the same sort of hydro-stratigraphic classification beginning with the welded Tiva Canyon, the non-welded Paintbrush, the Topopah Springs, the basil vitrophyre of the Topopah Springs, and the Calico Hills unit.

Just for reference, the repository is located slightly below an elevation of 1,100 meters. Or I should say the potential repository is planned to be located at an elevation of around 1,100 meters.

The flow calculations are done using this cross section, and when we do the abstractions we use a one-dimensional column located somewhere around here to develop the correlations between infiltration and velocity. That column essentially passes through the center of the proposed repository block.

[Slide.]

MISHRA: The second aspect that I want to touch upon is the alternative conceptualization of infiltration. Beginning with the infiltration that was presented by Flint & Flint in 1994, we came up with two infiltration ranges, as Bob alluded to.

The low range, which spans from 0.01 to 0.05 millimeters per year with a mean of about 0.02 millimeters per year, comes by assuming that the surficial infiltration that comes from the footprint of the repository is essentially migrated downwards in some predominantly one-dimensional vertical flow regime. That's the low end of the infiltration.

The high infiltration range, ranging from 0.5 to 2 millimeters per year with a mean of about 1.2 millimeters per year, comes by assuming that the spatially variable infiltration rate enters the unsaturated zone. Then at some position above the repository, perhaps along the base of the welded Tiva Canyon or in the non-welded Paintbrush, there is some significant lateral diversion and some mixing, and from then on it just goes downwards at some mean value which is of the order of 1 millimeter per year and the range is between 0.5 and 2 millimeters per year.

[Slide.]

MISHRA: Just sort of a clarification as to why we use the infiltration map of Flint & Flint. Throughout this series of presentations you will see information on the design or site characterization that might not be fully representative of what is known today, but this is what we knew best six months ago when we essentially froze all of the information related to design and site characterization. This is representative of a snapshot in time with respect to what was known best.

[Slide.]

MISHRA: Bob mentioned about how in these process model calculations we do try to take into account the uncertainty and the variability in the parameters, and that is what I want to talk about next.

Multiple realizations with respect to matrix flow properties are taken from the database developed by Al Schenker at Sandia National Labs. This slide shows the ranges associated with various matrix hydrologic properties for the hydro-stratigraphic units that occur below the repository horizon and which act as the predominant pathways for the transport of radionuclides from the repository to the water table.

For example, this line shows the range associated with the porosity of the welded Topopah Springs. Here I show in this blue the saturated conductivity in log scale and its variations for the non-welded vitric Calico Hills. These drop similarly. The ranges associated with the Van Genuchlen parameters are essentially parameters that describe the curvature, if you will, of the capillary pressure curve.

What we do with this information is that given this range, we do samples from it and develop multiple input files for performing the unsaturated flow calculations. That is a way of incorporating parametric uncertainty before we transfer that information over to the TSPA calculations.

[Slide.]

MISHRA: Finally, I want to talk a little bit about the conceptualization of fracture-matrix flow.

Historically, most of the unsaturated flow calculations for Yucca Mountain have been done using the equivalent continuum model in which some volume averaging is used to develop some "equivalent" properties of the medium, if you will.

One aspect of this equivalent continuum model is that it allows fracture flow only after the matrix is fully saturated, and that is what I have put down here as a fracture flow initiation rule.

The conventional assumption is that fracture flow is initiated after the matrix is fully saturated, and that corresponds to this value 1 for this parameter sigma, which we define as some satiated matrix saturation. You can think of it as a general value at which liquid flow in fractures is initiated.

We have generalized the existing equivalent continuum model to allow fracture flow to initiate at some saturation which is less than 1. We are still under some global equilibrium constraints, but this is a way of trying to approximate non-equilibrium flow effects just by allowing fracture flow to occur before the matrix is fully saturated.

For these calculations we take a value of sigma equals 0.95 to sort of provide an estimate of what would be the effects of non-equilibrium flow, and that value of 0.95 was chosen based on comparisons with some detailed work done by Sandia National Labs.

[Slide.]

MISHRA: Now to show some results of typical abstractions that were developed. Here I show abstractions for one particular hydro-stratigraphic unit. This is the welded Topopah Springs. The graph on the left, which is a log-log graph versus the matrix pore velocity and the infiltration flux, shows a band of information that takes into account the uncertainty in the matrix flow properties, and it also takes into account the fact that we have two conceptual models of fracture matrix flow.

The graph on the right shows how much of the infiltrating water is flowing through the fractures. That is the fractional fracture flux, if you will, as a function of the infiltration rate. The bottom one, the green curve, represents the results from the conventional equivalent continuum model and the one on the top, the one in blue or black, however it appears, represents the effect of the relaxed fracture flow initiation rule. So that's what happens when you use sigma equals 0.95 as the criterion for the initiating fracture flow.

It is just an example of the kinds of information that we pass on for the geosphere transport calculation, and we develop similar correlations for the other hydro-stratigraphic units. So much for the unsaturated flow in the scale of the repository or the scale of a kilometer, if you will.

[Slide.]

MISHRA: Moving on to the problem of what is happening with the scale of individual drips. Once again, let me go back to the conceptual picture.

We are looking at how infiltration is distributed into percolation, and of course for the time being we are just assuming that when I say infiltration flux I am really talking about what is happening to this percolation flux.

I have already talked about how this is partitioned into flux through the matrix and through the fractures and through the velocities through these flow media. Now I want to talk about how this percolation flux is partitioned and locally fluxes across a series of individual drips, and then furthermore how it partitions into flux through dripping fractures and flux in surrounding matrix.

[Slide.]

MISHRA: The flow at the scale of the waste package and the engineered barrier system, as Bob mentioned, is going to be influenced by the spatial variability in the percolation flux. It is also going to be influenced by the spatial variability in the saturated matrix conductivity.

We have some information about the second component of this issue, the spatial variability in saturated conductivity, particularly from the database of Al Schenker.

We do not have any information about the spatial variability in the percolation flux. So that is where the stochastic fracture flow model comes in. It takes the infiltration flux and it distributes it for each of the waste package catchment areas into a spatially variable percolation flux.

Then, depending upon whether that percolation flux is greater than the local spatially variable saturated conductivity, it is diverted into dripping fractures that enter the drift, or it is diverted into the surrounding rock quarry.

Using these rules, we developed functional relationships between the number of dripping fractures and the flux through these fractures both for the low infiltration range and also for the high infiltration range.

[Slide.]

MISHRA: That is what I am showing here as an example of the kinds of drift-scale abstractions that we developed. On the left axis you have the fraction of packages with drips as a function of the infiltration flux shown here in log scale, going from 0.01 to to 2 millimeters per year, which corresponds to the range of infiltration that is of interest to us in the TSPA calculation.

The graph on the right shows the corresponding flux through the dripping fractures.

That sort of gives you an idea, I hope, as to what are the process models that are being used to develop information regarding the ambient flow system. I have talked about how we use the LBL-USGS site-scale model to develop repository scale abstractions, how we use the stochastic fracture flow model to develop the drift-scale abstractions.

Some remarks about what are the uncertainties and limitations in these calculations.

First of all, we do acknowledge that the treatment of non-equilibrium fracture flow needs to be improved upon, needs to be calibrated to observations and/or to more robust models.

We have not included thermal effects. We need to look at mountain scale thermo-hydrologic models to see what relationship is between the time at which the system goes back to ambient as a function of thermal load.

We need some better information about percolation flux at depth.

[Slide.]

MISHRA: Moving on to the second part of the talk, which deals with thermo-hydrologic modeling. Once again, to anchor this talk to the overall information flow, this is the same diagram.

The interest here is to develop a drift-scale thermo-hydrologic model with information coming from site geohydrology, from repository design, from waste package design.

The output consists of drift-scale temperatures, drift-scale humidities and drift-scale saturations.

Drift-scale saturations, or water content, are used to calculate the diffusive release through the engineered barrier system. Jerry McNeish will be talking about that aspect of the performance assessment calculations later.

The second and important application of this information, particularly with respect to temperatures and humidities, are in the waste package degradation model applications and the subsequent prediction of waste package failures leading to an evaluation of substantially complete containment.

Joon Lee, who follows this presentation, will talk about this sub-network, if you will.

[Slide.]

MISHRA: This analysis is motivated by the fact that a good understanding of near-field thermo-hydrologic conditions are important to provide a good handle on waste package/EBS performance predictions.

In particular, the initiation/rate of corrosion of waste packages depends on temperature and relative humidity, and diffuse release of radionuclides through the waste package/engineered barrier system depends on the liquid saturation or on the water content.

The objectives are to develop a two-dimensional drift-scale model to predict temperature, liquid saturation and relative humidity as a function of various infiltration fluxes representing a low end and a high end, two thermal loads representing a low and a high value, and two backfill options. One is with a backfill and the other is without the backfill, so there is just an air gap in the drift.

[Slide.]

MISHRA: Here is a picture of the model

First, the 2-D cross section, which is extracted from the LBL-USGS model and corresponds to a location which is somewhere in the center of the proposed repository block. It once again shows the same six stratigraphic units, and for this particular column the repository is located almost halfway in between the surface and the groundwater table at 350 meters above the water table.

An expanded view of this inner region gives you this picture of what the waste package and the drift looks like. We are taking as a representative waste package 21-PWR multipurpose canister with a diameter of 1.8 meters emplaced in a drift of 5 meters and placed on top of a pedestal which lies on an invert.

When the backfill option is invoked, we are using what I call a "gravel" backfill. I will talk about this a little bit later. Primarily there has been no decision with respect to what will be the material used as the backfill, so we are using just some sort of a representative set of values for the backfill in terms of its thermo-hydrologic characteristics.

[Slide.]

MISHRA: Let me show you some results. Here are some two-dimensional color maps of the liquid saturation distribution in the vicinity of the repository. The idea here is to show that liquid saturation changes in the vicinity of the drift after somewhere on the order of between 100 and 1,000 years, and by the time we go to 10,000 years this has pretty much gone back to ambient.

[Slide.]

I will skip the next viewgraph, which shows temperatures, and come to what we are doing with respect to the uncertainty regarding backfill characteristics. As I said, not much has been decided with respect to backfill characteristics. So we have alternative thermo-hydrologic models which are based on different assumptions regarding what the backfill is composed of.

In particular, we picked one calculation that was done by Tom Buscheck at Lawrence Livermore Labs and use it as sort of an alternative representation of the backfill drift system. The idea is to use both of these sets of results in order to evaluate the performance of the backfill-based system to see what difference it makes.

This is a listing of all the differences between the two models, and particularly let me focus on this one, thermal conductivity.

A low thermal conductivity essentially makes the heat transfer less efficient, makes the temperature higher and makes the relative humidity lower, and so you get one set of predictions.

On the other hand, if you have a high thermal conductivity, it makes the heat transfer faster and the system goes back to ambient much quicker; relative humidities are higher, and so on.

[Slide.]

MISHRA: To show what it means in terms of real calculations, here is a side-by-side comparison of the results from Buscheck done at a thermal load of 80 MTUs per acre with no background infiltration, compared to what was done in this study for 83 MTUs per acre with infiltration of 0.05 millimeters per year.

The curves which are increasing with respect to time are the humidities, and as you can see, the two models give very different values or very different predictions of humidity, from about 60 percent to about 95 percent here.

I would just note that in our corrosion models humid air corrosion is assumed to be initiated at a value of about 70 percent relative humidity.

The temperatures are the ones which are decreasing with respect to time after this early increase, and as you can see, the differences are not significant at late times. At early times the differences come because the backfill thermal conductivities are somewhat different.

This curve will come back again when Jerry and

Dave talk about the performance of the different design options with respect to EBS and the geosphere calculations.

[Slide.]

MISHRA: To sum up, we are providing temperature, relative humidity and liquid saturation results as an input to waste package/engineered barrier system calculations.

Alternative conceptual and numerical models of the backfill system have been shown to yield very different temperature and relative humidity predictions.

Some of the uncertainties are with respect to backfill thermo-hydrologic properties, with respect to how it calculates to relative humidities, and also a very important assumption that I have not mentioned so far is that all of these calculations are done assuming equivalent continuum model. Fracture flow effects have not been included in these calculations.

Let me conclude with that. I see frantic glances from Bob. I have probably exceeded my time.

LANGMUIR: Thank you, Srikanta. You're right on time.

Let me start it out with a question related to the model you are using and the limitations of it, at least my perception of them. I didn't see any discussion at all of being able to incorporate lateral flows of groundwater in the unsaturated zone, perched water, lateral movement along saturated horizons. All of your models suggested everything was a vertical flow. Can you deal with perched water below the repository and lateral flows?

MISHRA: One of the limitations of the models that we are using is that each hydro-stratigraphic unit is assumed to be homogenous in its characteristics. The reason why you might have locally perched water is you have distributed heterogeneities.

In fact, some of the calculations done at Sandia National Labs for the groundwater travel time study with distributed heterogeneities do show that you can develop locally perched water conditions if you take into account spatially distributed heterogeneity.

That is not included in this model. Perched water is not explicitly included. In a way it cannot be included unless by the sampling scheme you create, for example, a parameter distribution for the basal vitrophyre of the Topopah Springs, which has such hydrologic characteristics that it becomes saturated. That is with respect to perched water.

[Slide.]

MISHRA: With respect to lateral flow, I think in a way we are sort of implicitly including it in our second infiltration scenario where we say that if you start with the Flint & Flint infiltration map, which is the basis for our infiltration scenarios, and if you look at the high infiltration zones that come from the outcropping Paintbrush units here, and perhaps here, and then you take them down and assume that there is some lateral flow within the Paintbrush, and that mixing produces an effective infiltration rate, which is sort of the spatial average of these numbers, then I think that is sort of a surrogate way of accounting for lateral flow and mixing about the repository horizon.

It's a simplistic representation, but I think it's a preliminary surrogate, if you will.

LANGMUIR: Pat Domenico.

DOMENICO: On slide 11, the diagram on our right, if you have an infiltration flux of, let's say, two or two and a half, that diagram would suggest that the fractures take all the flow?

MISHRA: No. I would not extrapolate this, because I don't think the relationship is linear.

DOMENICO: It looks like it's heading that way.

MISHRA: If you look at this value?

DOMENICO: Yes.

MISHRA: No. It's taking about 60 percent of the flow.

DOMENICO: At that point, but if you increase the infiltration by just, let's say, one millimeter per year, what effect would that have on the amount going into the fractures?

MISHRA: I think it would probably increase it, but once again, this is a very preliminary model and I would be very wary of linearly extrapolating or non-linearly extrapolating it. It would certainly increase it, but whether it would increase it to .8 or whether it would increase to .9, I don't know.

DOMENICO: The very same conclusion can come from figure 13. If we just increased infiltration by a mere millimeter per year, most of the packages will have drips on them.

MISHRA: That is also to some extent dependent on what is the assumption regarding the spatial variability of the Topopah Springs permeabilities, because that sort of controls how you are diverting the infiltration into the drifts.

DOMENICO: Going to the one showing relative humidity, your infiltration flux has taken a mere .05 millimeters per year. I presume if it goes up to 1 the relative humidity increases pretty fairly.

I think what I am saying is once we start getting fluxes greater than 1 millimeter per year some bad things start to happen. That's what these show, if I can extrapolate that.

MISHRA: Yes. I think the caveat here is that these are equilibrium models and these do not describe the non-equilibrium fracture flow effects, particularly at high infiltration rates. The question is, to what duration can you sustain these high infiltration rates? I think if we have sustained high infiltration rates of the order of 7 millimeters per year with what is known about the matrix, then I think we are in trouble.

DOMENICO: This takes an initial condition. What was your initial degree of saturation before you started this? Basically from Flint's work?

MISHRA: For these calculations, the initial saturation distribution would depend on what is the background infiltration rate. For zero infiltration rate it would essentially be the capillary -- this is with a backfill drift. When you emplace a drift you assume that it is at very low saturation, almost dry. Essentially the drift stays dry for quite a long time.

DOMENICO: Thank you.

LANGMUIR: Ed Cording.

CORDING: Related to the same line that Pat Domenico was looking at, on figure 13 you talked about the drips. It does show that drips will reach almost all the packages as you get flux above one millimeter, but water is dripping on to all the packages. Essentially, you are basically assuming an infinite number of fractures in the system such that there is always a fracture distribution over each package that is the same throughout the repository. Is that correct?

MISHRA: Not exactly. You have a distribution of drips, but you also have a distribution of fluxes. The flux through the dripping fractures is not the same. The flux can be very small or it can be quite large.

CORDING: The dripping fracture is related to flux only, not to distribution of fractures. You don't have a distribution of fractures that is concentrating flow in any way. It's strictly related to the distribution of the flux. So there is going to be some distribution of fractures in addition to distribution of flux that is going to control drips on packages.

MISHRA: Right. In a way this simplistic model doesn't really explicitly take into account what is the local fracturing. It sort of says that you look at what is the matrix saturated conductivity and then you divert the rest of it into fracture. Actually, this needs to be refined into two component models where you take the matrix properties and the fracture properties and then use the partitioning.

CORDING: For example, if you are concentrating some of the flows with local effects that are concentrating flows in major fault systems, then you have a more favorable condition than what you are describing.

LANGMUIR: John Cantlon.

CANTLON: In your figure 4 which you have up there you've describe for us the movement of water into and dripping on the containers, but you've now created a very handsome lateral fracture flow system by building a repository that slopes downhill.

MISHRA: That's only for schematic purposes.

CANTLON: The reality is it's also sloping down hill.

MISHRA: No. The new design is that it's mostly flat.

CANTLON: Totally flat?

MISHRA: It has a very small grade.

CANTLON: Water moves down very small grades.

In other words, you are going to distribute water irrespective of where it comes in over the top of that invert. The invert is not going to disappear totally. So you are going to distribute water pretty much throughout the repository wherever it comes in.

MISHRA: To some extent that might be true, but then the other thing to take into account is that you are also creating a system in which there are some capillary differences between an open drift and what the surrounding rock is. So there is some potential for the flow to be diverted around the drift as opposed to entering the drift if you do not have local fractures.

CANTLON: That postulates some very interesting differences between your engineered structure here and native rock. I guess my conceptual view would be that if you have a concrete invert and whatever your fill on that would be is going to provide a lateral transfer system.

MISHRA: When you talk about the engineered system, what we have not taken into account here is that that engineered system is going to be thermally perturbed. Because of that, I think the local water flow system is probably not going to be as simple as we have sketched out here. This is our preliminary attempt at how one would zoom in on a fine scale and try to predict the movement of water, but it does need to take into account the effects of thermal disturbances.

ANDREWS: Let me add something. One has to recognize these are very low flux values. The capillary characteristics of the rock are such and gravity is such that any possibility for lateral flow even along these kind of gradients or steepness of grade, if you will, is virtually nil. Because of the capillary characteristics of the rock, things are going to move vertically, not laterally within the drifts.

CANTLON: It would overwhelm the flow.

ANDREWS: Yes. It would overwhelm what I think you perceive as a potential horizontal flow.

LANGMUIR: Although how do you get perched water if that's the case?

We are currently three minutes into our break.

COHON: I have a question.

LANGMUIR: A very short one, please, Jerry, because we won't have much time for coffee.

COHON: Could you put up slide 15.

[Slide.]

COHON: Just two very quick and basic questions about this. Looking at the drift-scale thermo-hydrology model, why do we take inputs from the site geohydrology rather than the unsaturated zone flow model?

MISHRA: The input comes from site geohydrology in terms of the hydrologic properties.

COHON: I certainly understand why you need that input, but having done the unsaturated zone flow model to understand or predict how the site geohydrology turns into unsaturated zone flow, why not use that in the thermo-hydrology model at the drift-scale.

MISHRA: In a way these two models are sort of decoupled, because the scales are different. The unsaturated zone flow model does not treat the repository at all, whereas the drift-scale thermo-hydrologic model is essentially looking at the unit cell between adjacent drips and between adjacent waste packages.

The only way it interacts with the unsaturated system is because of the boundary conditions. The top boundary is at the ground surface. So you can think of it as a very slender column that goes from the top of the mountain to the water table, but it's essentially a column that is inside the repository.

COHON: I've made my point and I think it's one worth considering. We can talk more about that.

The other quick question. We are assuming a certain climate and rainfall regime, and that's fixed?

MISHRA: For these calculations, yes.

COHON: That does not appear anywhere in this diagram. Is that one of those external factors?

MISHRA: I guess it would be coming under here as to what the top boundary condition is at the surface in terms of the water entering into the mountain. But climate change is not explicitly included in these calculations.

LANGMUIR: I'm going to interrupt here. We only have five minutes left for the coffee break if we stay on schedule. I will give you two more and we will try to make it up. Please reconvene in about seven minutes, which is 10:07.

[Recess.]

LANGMUIR: Our next speaker is Joon Lee. His topic is waste package degradation model and results.

LEE: Good morning. In this presentation I will discuss work performed on waste package degradation modeling and abstraction for TSPA-1995.

[Slide.]

LEE: As discussed yesterday in the waste containment and isolation strategy by Jean, the waste package is one of the major components in the current waste isolation strategy.

[Slide.]

LEE: You have already seen this diagram. My presentation covers this area indicated with color. Information on waste package design and material properties are fed into this modeling process. The importance to this modeling process is drift-scale temperature and humidity profile which was discussed by Srikanta Mishra in the previous presentation. This modeling process fed into the EBS transport modeling process, which will be discussed by Jerry McNeish.

[Slide.]

LEE: The objectives.

To assimilate relevant corrosion degradation data for similar containment barrier materials in similar environments. An extensive corrosion testing program just got under way at Lawrence Livermore National Lab, and we expect we will start getting those data soon. In TSPA we collect relevant corrosion data from general literature and we will use it to develop corrosion models.

Another objective is to develop corrosion models for the containment barrier materials and implement those corrosion models and their uncertainties to develop a detailed stochastic waste package degradation simulation model.

Another objective is to develop abstractions for waste package degradation for TSPA model.

In the abstraction process, as Srikanta discussed, the drift-scale temperature and humidity profile were used.

Also in the abstraction process the distribution of initial pit penetration parameters were used stochastically, and I will repeat that one in more detail later.

[Slide.]

LEE: The last objective is to investigate the sensitivity of waste package performance to different conceptual models, including cathodic protection, alternative thermal-hydrologic models, thermal load, corrosion initiation, infiltration rate, and backfill.

This presentation will discuss the result for the first three cases, which has much greater impact than the last three cases.

[Slide.]

LEE: Currently the waste disposal container has different designs, depending on type of waste and depending on thermal load. In TSPA-1995, however, all waste containers were assumed to have the same design with 20 millimeter thick inner barrier alloy 825 and 100 millimeter thick carbon steel outer barrier.

[Slide.]

LEE: This slide gives you sort of an overview of the stochastic waste package performance simulation model.

The humid air corrosion models for the outer barrier corrosion allowance material includes a general corrosion model and a pitting corrosion model.

Aqueous corrosion models for the same outer barrier corrosion allowance material also includes a general corrosion model and a pitting corrosion model.

The stochastic simulation model has aqueous pitting corrosion model for the inner barrier corrosion resistant material.

These models are incorporated into the stochastic waste package degradation simulation model.

Also, drift-scale temperature and RH histories are fed into the stochastic simulation module as a lookup table.

This simulation module provides information on waste package failure and waste package degradation history. These are abstracted into the EBS radionuclide transport model. I will discuss that in detail in the later part of this presentation.

[Slide.]

LEE: This slide shows the approach to waste package degradation simulation.

In the post-closure repository we expect that there will be an extended period of dry-out in the near-field environment and that the near-field environment will gradually close down and the humidity will gradually increase.

We expect that the waste package initially will be exposed to a humid air corrosion environment and that the corrosion mode will gradually change to different corrosion modes with time.

This flow chart is showing the sequence of events we expect for the degradation of the waste package in the repository. I will be using this flow chart to guide you for my presentation. I will revisit this flow chart for each step and discuss it a little bit in detail.

[Slide.]

LEE: I will discuss humid air corrosion models for the outer barrier corrosion allowance material.

The humid air general corrosion model was developed as a function of time, RH, temperature and sulfur dioxide level in the near-field environment. In actual simulation we assumed that the sulfur dioxide is a negligible level.

[Slide.]

LEE: This slide shows the humid air general corrosion depth in microns in the y axis as a function of exposure time in years. This solid line is a model fit. The dashed lines are uncertainty of 2 standard deviations of the model fit.

LANGMUIR: Joon, is this for the steel or this for alloy 825?

LEE: This is the corrosion allowance material, carbon steel and cast iron. Cast iron has similar corrosion behavior, so I included all of them.

In the stochastic simulation model there was uncertainty utilized to represent those variabilities among waste packages and among pits. I will discuss them in a little bit of detail later.

[Slide.]

LEE: To develop the pitted corrosion model for the corrosion allowance material in the humid air environment we assume that the pitting factor has a normal distribution with a mean of 4 and a standard deviation of 1. This slide shows predicted pit depth distribution of corrosion allowance material in constant humid air conditions of 60 degrees celsius and 90 percent RH for different exposure times.

The y axis shows the pit depths as probable density function and the x axis is the pit depths in millimeters.

As you will notice, with time the pit distribution is spread out. If we consider 100 millimeter thick corrosion allowance material exposed to this environment continuously, we expect that pit penetration initiates at about 3,000 years.

[Slide.]

LEE: For the aqueous general corrosion model we developed a model based on literature data as a function of time and temperature.

[Slide.]

LEE: This slide shows the aqueous general corrosion depths in micron in the y axis as a function of exposure time at the x axis.

The solid line is the model prediction and the dashed lines are 2 standard deviations of the model prediction.

Again, the uncertainties of the model prediction were utilized in the stochastic waste package degradation modeling.

LANGMUIR: A little nit-picky, but several of us had questions about one of your equations. I think it's viewgraph 8, which was the original damage function. It looks like there might be errors in the way the terms are expressed. It looked as if your relative humidity should be a multiplier rather than an inverse and your temperature should be a multiplier rather than an inverse.

[Slide.]

LEE: You mean the humid air corrosion case.

LANGMUIR: The equation suggests that as humidity goes up corrosion goes down. Not this equation, but the one on overhead 8.

LEE: A1 has a negative. I didn't say that. The same as this one. A2 is a negative number. The other one has a negative number. I didn't go into detail about the parameters.

LANGMUIR: This is interesting to us.

LEE: A2 has a negative number. RH goes up and the depth is increasing exponentially.

[Slide.]

LEE: This slide shows the temperature dependency in aqueous general corrosion of corrosion allowance material. What you see here is in aqueous corrosion we have a maximum corrosion at temperatures between 60 and 70 degrees C.

[Slide.]

LEE: For the pitting corrosion modeling in aqueous conditions of corrosion allowance material we also assume that the pitting factor has a normal distribution with a mean of 4 and a standard deviation of 1. This overhead shows predicted pit depth distribution of corrosion allowance material in constant aqueous condition of 60 degrees celsius for different exposure times.

The y axis is the pit depth P.D.F. and the x axis is pit depths in millimeters. If we consider 100 millimeter thick corrosion allowance material, we expect that the pit starts penetrating the material thickness if the material is exposed continuously to this constant aqueous corrosion environment.

[Slide.]

LEE: So far I have covered the humid air corrosion allowance material and aqueous corrosion allowance material. I will discuss a little bit about aqueous corrosion resistant inner barrier material.

Because there has been no knew development or improvement of the inner barrier corrosion model which was used in TSPA-1993, we used the same corrosion model in this TSPA. Basically that inner barrier corrosion model gives a constant pit growth rate in the alloy 825 irrespective of exposure time.

[Slide.]

LEE: The inner barrier corrosion model is simply expressed as a function of temperature only, as shown on this slide. The y axis is pit growth rate; the x axis is exposure temperature in celsius.

The solid line is a mean growth rate. The dashed line is the 5th percentile growth rate and the broken line above is 95th percentile growth rate.

As you will notice, there is about six orders of magnitude difference between 100 degrees C and room temperature. This huge difference in pit growth rate has a significant impact on the waste package performance between two different thermal load cases.

Also, the uncertainty in the pit growth rate was captured in the stochastic waste package simulation module.

[Slide.]

LEE: I'm using this slide again.

Those corrosion models I discussed so far were incorporated into the stochastic waste package simulation module with drift-scale temperature and humidity profiles at the waste package surface.

[Slide.]

LEE: Now I will present actual simulation results for the waste package performance. The first result I will present is waste package failure time, which has relevant information to the substantially complete containment requirement, NRC subsystem requirement.

Currently there is no definition of substantially complete containment, but it has been tentatively defined as having less than one percent failure of the waste package in

1,000 years, and the definition of waste package failure, as indicated by Bob, as having at least one pit penetration.

The waste package failure time corresponds to initiation of waste form alteration radionuclide mobilization.

The next result I will present is a waste package degradation history, which has direct relevance to controlled release NRC subsystem requirements. This waste package degradation history has direct input to the radionuclide release rate.

[Slide.]

LEE: Before I get into the discussion of the results, I will present a few of the major assumptions in the stochastic simulation.

Humid air corrosion of corrosion allowance outer barrier initiates at relative humidity between 65 and 75 percent.

Aqueous corrosion starts at relative humidity between 85 and 95 percent.

Corrosion resistant inner barrier material is only subjected to aqueous pitting corrosion.

The uncertainties in the corrosion models were utilized to represent pit-to-pit variability and waste package-to-waste package variability.

The philosophy behind this is that in the repository we will have about 10,000 waste packages spread over a large area of the repository. So local corrosion environment at one end of the repository may be different from the other end of the repository. We tried to capture that variability among waste packages using the uncertainty in the corrosion models.

Also the waste package has such a large area, so the local corrosion environment at one end of the waste package may be different from the other end of the waste package. We utilized the uncertainties in the corrosion models to represent those variabilities.

Currently we don't have any information about the degree of variability among waste packages and pits. So we just equally split the uncertainties in the corrosion models.

[Slide.]

LEE: This slide shows the simulation result for waste package performance versus cathodic protection.

Let me discuss a little bit about cathodic protection. The inner barrier alloy 825 has a much higher corrosion potential than the outer barrier carbon steel. So if we have two metals contacting and exposed to a corrosion environment, the outer barrier carbon steel will corrode preferentially or sacrificially before the initiation of inner barrier corrosion.

In this simulation we delayed the pitting corrosion of the inner barrier until the outer barrier thickness was reduced by 75 percent. Here the y axis is the cumulative fraction of waste packages with first pit penetration and the x axis is the exposure time in years.

The solid line here is the result for the case without cathodic protection. The broken line here is the result for the case with cathodic protection.

In the case without cathodic protection, the initial pit penetration starts at about 2,000 years compared to about 8,000 years in the case with cathodic protection. So with this simple cathodic protection mechanism the waste package is buying about 6,000 years in performance.

But if you look at the 10,000 year time frame without cathodic protection, about 90 percent of the waste packages have at least one pit penetration compared to a negligible number of waste packages with pit penetration. So this simulation result shows the significant impact of cathodic protection for the waste package performance.

[Slide.]

LEE: This slide shows a similar simulation result but for the thermal case.

Again, the y axis is the cumulative fraction of waste packages with first pit penetration, and the x axis is exposure time in years.

Also shown in this slide is the result using temperature and humidity profiles from Buscheck's model, which was discussed by Srikanta Mishra.

What is shown generally on this slide is that the waste package failure rate is much lower in a low thermal case. The major factor for this is that lower temperature environment in the low thermal case gives much lower pit growth rate over the inner barrier. You will recall the temperature dependency of the inner barrier pit growth rate I showed you a few slides back.

Also, Buscheck's model, which is a different conceptual model, always gives higher temperature and lower humidity. The impact shows a much lower waste package failure rate. This difference is much pronounced in high thermal load case.

[Slide.]

LEE: This is the simulation results for waste package degradation history. This is representative pitting histories of 25 waste packages. Each line indicates the pitting history of each waste package. We have 25 lines here.

The y axis is the fraction of pits through container wall thickness and the x axis is exposure time in years.

What this slide shows is that some waste packages have a very high pitting rate compared to some packages having a lower pitting rate. This kind of a difference in pitting behavior in different waste packages was captured by utilizing variability among waste packages and among pits. Also, changing environment with time.

This input fed into the EBS transport model simulation will be presented by Jerry McNeish in the next presentation. The number of pits on the waste packages is directly proportional to the area of transport of radionuclide out of failed waste packages.

[Slide.]

LEE: Summary and conclusions.

The current waste package design appears to meet the substantially complete containment requirement within the conditions of the degradation modes, assumptions and near-field environments considered in the simulation.

It has been shown that cathodic protection of the inner barrier by the outer barrier has significant impacts on waste package performance.

In future TSPA we need to substantiate the inner barrier pitting model and cathodic protection model. We used a very simple cathodic protection model in the simulation.

Also we need to include stress corrosion cracking of the inner barrier. Numerous literature indicates that stress corrosion cracking is closely associated with the pitting process.

The last is the potential effects of microbiologically influenced corrosion needs to be considered in future TSPA. This is one corrosion mechanism not counted in this simulation.

Thank you.

LANGMUIR: Thank you, Joon.

Ellis Verink.

VERINK: There are two or three questions I would like to ask you about this.

Is there a space between the inner and outer shell? I know this depends on how it's fabricated.

LEE: Currently we assume that the waste package people will fabricate the waste package with a two layer system with 100 percent contact. So we assume that there is no space between two metals.

VERINK: There is a little extra safety built into this which may help that become true. Assuming that the exterior shell is providing cathodic protection, the corrosion products would be larger than the volume of metal consumed to provide that cathodic protection. So this would tend to seal up the space still further.

LEE: I believe the corrosion product wouldn't provide cathodic protection.

VERINK: I don't say that it will.

LEE: The conductivity would be much lower. Bare metal would provide cathodic protection but the corrosion product wouldn't, I don't believe.

VERINK: You will not get cathodic protection of the inner shell except by the part of the outer shell which is immediately adjacent to it. It won't run clear around the outside and touch. As moisture penetrates, the cathodic protection will occur. You've got to have an electrolyte.

LEE: Yes.

VERINK: At the ends where it's a sawed off end you will have cathodic protection sitting there in the open air.

What I am saying is that even if you have some crevice between them, crevice corrosion tends to be a little more aggressive than others, but the corrosion products of that crevice corrosion will occupy more space than the metal it was made from.

LEE: That's true.

VERINK: This will tend to plug and thereby seal considerably, so you will get some benefit from that.

LEE: I see.

VERINK: I think it's desirable to have them so that they are as close as possible.

I made another note here. Your pitting business going straight through, are you assuming in that calculation that a pit just goes right straight on through the steel and the coating?

LEE: Yes.

VERINK: That's contrary to the idea of cathodic protection, right, because the cathodic protection says that the outer one is going to corrode and be consumed and thereby save the inner shell from penetration until enough of the outer shell is gone that you can no longer electrochemically protect?

LEE: Right. That was captured in the simulation incorporated with cathodic protection. We are just looking at sensitivities.

VERINK: I am just questioning your curves where you talked about the time to make complete penetration and what your interpretation of that was. If you assume cathodic protection, you are not going to be penetrating the inner shell until the outer shell is physically consumed over a considerable distance. So it is more complex than that; it's more conservative as well. Okay?

LEE: Okay.

LANGMUIR: I'm going to ask another corrosion-related question here. Looking at your summary of future TSPA needs, it would seem to me you need another thing that is not mentioned. If you look back at overhead 14, I read that as a uniform corrosion rate of mild steel, and it's done in distilled water. It looks as if your model is based upon the distilled water performance.

One of the critical issues in the coupling of all the processes in this mountain is going to be the effect of high salinity created by refluxion on not only sealing off the mountain or perhaps being involved in heat pipe effects, but in enhancing corrosion. You could easily have, I would think, waters in excess of sea water salinity in contact with waste packages once you have done any kind of refluxion at all in an evaporative drift.

I would be interested if maybe you or Dr. Verink could comment on how much of an effect going from distilled water to sea water would have on your corrosion rates for the soft steel, how important that might be.

[Slide.]

LEE: This corrosion in distilled water was utilized to capture temperature dependency. This data used to capture time dependency has much higher corrosion rates. This is tropical lake water. It is relatively warm. This is data actually for polluted river water which contains a lot of chloride.

This one only captured the behavior.

LANGMUIR: But your river and lake waters are going to be 1/10 or less the salinity of sea water, unless they are extraordinary. You are talking about fresh waters basically here. It sounds as if you need to know more about the effect of ionic strengths. Maybe the work has been done somewhere.

Ellis.

VERINK: It's a mixed bag. The effect of cathodic protection will be more intense at the more conductive waters. That means you will get more and better protection as long as it's there but you will consume the anode quicker. So there is that kind of a question.

DOMENICO: You don't get something for nothing here.

VERINK: There is no free lunch.

[Laughter.]

LEE: My perception is that in a near-field environment the dripping water will be high in ions, high ion strengths, but I don't believe that dripping water can have that much high ion strength in chloride, as in sea water.

LANGMUIR: I don't think you know that for sure. I think one could easily calculate that. Calculations have been made in fact of the evaporative effect of refluxion, and some pretty high ionic strengths and some high chlorides can be generated by it.

LEE: I believe corrosion by carbonate may be more important than chloride.

LANGMUIR: Bill Murphy from the Southwest Institute has made those kinds of calculations and come up with some concentrations and solutions you might want to look at for these systems.

LEE: Okay.

LANGMUIR: John Cantlon.

CANTLON: You have in your last overhead indicated that you are going to look at some stress corrosion cracking in the next run on TSPA. As one visualizes the aging of this system and the weakening of the container as corrosion takes place, it would seem to me the stress corrosion is going to accelerate because you will have the weight of the backfill on the remaining thinning system. It would seem to me you are going to have to look at stress corrosion as an accelerating problem.

One of the questions that comes to mind is, how would the system look if you had fillers in the packages? You'd ease that container. You'd also do a lot of other good things in terms of protection in the mobility of the fuel.

It would seem to me very useful to have a filler model somewhere in your system on the next run.

LANGMUIR: Thank you, Joon.

A quick question from Carl Di Bella.

Di BELLA: I hope so.

Your work is showing, just as TSPA-93 showed, that the waste package can be a very important barrier, and corrosion of the waste package is something that needs to be understood. What are the TSPA people going to tell Lawrence Livermore National Laboratory to do now as a result of these calculations? I realize maybe Abe might want to field that question.

VAN LUIK: I think actually Joon has been in a dialogue with these people and I think he would be the right one to answer it.

LEE: We had a meeting between the M&O waste package people and DOE people about prioritizing the testing program. We discussed that this is an important factor for the waste package performance model program.

Di BELLA: I know that their research program is very heavily aqueous corrosion oriented now, and you are showing corrosion as a function of relative humidity is also important. Do you think they will be changing their program or adding to it to do experiments at these moderate relative humidity points?

LEE: That is still very important. As I said in the assumption, we are using relative humidity between 85 to 95 percent for switching from humid air corrosion to aqueous corrosion. I think we need to better define that transition of corrosion mode. I believe that humid air corrosion is important.

Di BELLA: Thank you.

VAN LUIK: Your point is well taken. In the dialogue that we are having with the Livermore folks we have had vehement discussions on where that transition point is and how they might go about giving us a better definition of it. So that dialogue is ongoing.

In the current scenario of how the program is proceeding it's difficult to predict what we will be able to do in the next few years, but we fully intend to go after these effects.

LANGMUIR: Thank you, Joon and Abe.

We are a little over time here. Let's proceed with the next presentation. The speaker is Jerry McNeish. His topic is engineered barrier system release model and results.

McNEISH: Thank you.

[Slide.]

McNEISH: Today I am going to present some of our analyses on the engineered barrier system releases. I would like to acknowledge several of my colleagues who have actually done a lot of this work that I will be presenting, Joon Lee, Joe Atkins and Dave Sassani.

[Slide.]

McNEISH: As an outline of my presentation, first I want to talk about the objectives.

Then I will briefly describe the nominal engineered barrier system which we analyzed.

I will talk a little bit about the waste form alteration/radionuclide mobilization abstractions which we developed.

Then I will spend a fair bit of time on the different EBS release conceptual models which we used.

And then move into the actual sensitivity analyses, our approach, and the results and conclusions of those sensitivity analyses of the EBS releases.

[Slide.]

McNEISH: The objectives of the analyses were to develop abstractions for radionuclide mobilization processes, including alteration and dissolution of the waste form, and also we developed abstractions for radionuclide solubilities to use in a sensitivity case.

The key was to evaluate the EBS release rate for various conceptual models, various designs, various parameters which are uncertain, and also conceptual models in the EBS which we have looked at.

From these EBS releases we will provide that information to personnel modeling the geosphere in order for them to be able to calculate the total system performance. Dave Sevougian will present the geosphere analysis in the next presentation.

[Slide.]

McNEISH: You've seen this diagram many times. Again, there are several columns: information or data; process level models; model abstractions; performance assessment models; and what are the performance measures that we looked at. Today I will be talking primarily about the waste form properties and how those feed into the EBS transport model, and then also I will spend a lot of time on the EBS transport model itself.

As you can see, there is a lot of information that comes into the EBS transport model, drift-scale fluxes, the drift-scale temperature, humidity and saturations, and also the waste package life time and degradation information.

The results that I will present are primarily peak EBS release rate. The information, as I said a minute ago, that I develop in the EBS transport model will feed into the geosphere calculations.

[Slide.]

McNEISH: This is a schematic of our nominal engineered barrier system. This is basically a slice through the drift.

Going from inside out, the waste itself was either spent fuel or high level waste glass. We track 39 radionuclides in our inventory.

The container that we looked at. As Joon described, the inner barrier was a 2 centimeter corrosion resistant material and the outer barrier was a 10 centimeter corrosion allowance material. We also had an invert of 1 meter thick.

If backfill was included in the analyses, it would fill in this space here.

[Slide.]

McNEISH: The key processes which feed into the waste package and EBS release are shown in this diagram. You've already heard about quite a few of these things.

The thermal-hydrologic results which provided temperature, relative humidities and saturations both directly to the calculations which I did and also to the waste package degradation model.

We included a simple cladding model in our calculations.

I will spend a little bit more time on the waste form and alteration rate and how we developed those abstractions.

The information from that was combined with the radionuclide solubility check within RIP in order to calculate the concentrations for the releases.

We also had, as Srikanta described, a certain percentage of the waste packages with fracture flow or dripping fractures.

Diffusion coefficients came from the Conca information. I will talk about that in a little bit.

Also we had several different EBS conceptual models for release.

This all led to our calculations of a waste package and the EBS release both from diffusive processes and advective processes.

[Slide.]

McNEISH: I am going to try this two projector thing.

Basically, I want to talk a little bit about the waste form alteration/radionuclide mobilization aspects right now.

For spent fuel dissolution we basically took information from some experiments done by Steward and Gray to develop a functional form for the dissolution rate.

In this figure the dissolution rate is on this axis and temperature is on the x axis. The data points are shown in here, and then our model prediction. The uncertainty in that prediction is shown as well in the form of a few standard deviations away from our model fit.

[Slide.]

McNEISH: Similarly for the high level waste glass dissolution. In this case we took data from Bourcier and developed a functional form for the dissolution rate as a function of pH. You can see the data points in here, and then our model fits for different temperatures, 90 degrees all the way down to 25 degrees C.

For the calculations that I am going to show you today we just looked in the pH value of 7.

[Slide.]

McNEISH: In the base analyses that we did we used the radionuclide solubilities as elicited for TSPA-1993.

For a sensitivity case three of the radionuclides were empirically fit to some of the data that is out there. I just want to show you one of the cases for the neptunium.

You can see the actual data is shown here, and then for the pH of 7, which I'm going to talk about, a model fit is shown here, and the standard deviation of 1 to show the uncertainty in that model fit.

[Slide.]

McNEISH: Finally, the diffusion coefficient was developed based on data from Conca. We developed a model to fit the data and then incorporated into our analyses the uncertainty based on plus or minus 2 standard deviations.

This shows you basically the diffusion coefficient as a function of the volumetric water content is really important in diffusion out of the waste package as well as through the invert.

[Slide.]

McNEISH: Another important topic is how the radionuclides actually release out of the EBS. I'm going to present three different conceptual model schematics to try to describe what we did for the different conceptual models.

[Slide.]

McNEISH: The first model assumes that we have both diffusive and advective release from the package as well as from the EBS. For the advective flow, as soon as we have a single pit which penetrates the package, we can have advective flow through the package if there is a dripping fracture above that package.

The diffusive release is proportional to the number of pits which have actually penetrated the package. That will change with time. You can see that we have diffusive release from the package and also advective release from the package, and then from the EBS we have both diffusive and advective release as well.

That is the first model.

[Slide.]

McNEISH: The second model, which is perhaps more realistic but will not allow probably as much release, assumes that we only have diffusive release from the package and then both advective and diffusive release from the EBS.

The waste package is assumed to develop corrosion product in the pits so that the advective or dripping water is not able to actually penetrate into the package and flows over the package so that we can have advective release from the EBS but not from the package. Then we will have both advective and diffusive release from the EBS.

That is model two.

[Slide.]

McNEISH: The third model for release from the EBS assumes that we have very good designers and builders of capillary barriers which will last for a long time. In this case we have only diffusive release from the waste package, no advective release at all from the waste package, and all dripping water is assumed to be diverted around the drift and not come into contact with the waste package at all. That obviously is going to eliminate the advective release, which will reduce your EBS releases significantly.

[Slide.]

McNEISH: The approach that we took to evaluating the EBS release was to look at a variety of radionuclides, and I'm going to present information from three of those radionuclides today, carbon-14, which is assumed to have gaseous release out of the EBS, neptunium and technetium. Technetium is dissolution rate limited and neptunium is solubility limited.

Then I will talk about the variety of alternate conceptual models that we also looked at, different designs and different parametric situations.

For the infiltration sensitivity we looked at both a high and low infiltration case.

For thermal load I looked at both 25 MTU and 83 MTU per acre cases.

For cladding we put in a switch to say either the cladding survives or it doesn't survive in a certain percentage to provide some protection to the waste and thus reduce the releases.

For cathodic protection we implemented the model which Joon described wherein we had to have 75 percent reduction of the outer barrier before we ever started any penetration of the inner barrier.

For the EBS release models, I've just presented those various conceptual models, and we'll show the sensitivity of the releases there.

Then, finally, the thermal-hydrologic models. We will show both the 25 MTU case that we developed and also one of Tom Buscheck's comparable cases.

[Slide.]

McNEISH: It's important to bring this up, this NRC peak release rate, even though I didn't make direct comparisons to those rates.

The NRC regulations are that the peak release rate from the EBS following the containment period shall be less than one part in 100,000 per year of the 1,000 year inventory. This provides a basis for looking at peak release rate even though we didn't do a direct compliance comparison with this requirement.

[Slide.]

McNEISH: I want to show some release rate histories for a particular case. This is for carbon-14 and shows a sensitivity to infiltration.

On this axis we have the total release from the EBS and then time is on the x axis. Carbon-14, as I said, provides for gaseous release from the EBS. So for low and high infiltration there is not much difference in the behavior.

The peak is a little bit higher for the high infiltration case than for the low infiltration, but basically they have similar behavior. These initial spikes are from gap fraction release as a waste package group fails.

[Slide.]

McNEISH: For technetium the predicted release rates for this 83 MTU per acre case with no backfill and low and high infiltration are shown here. Again, total release is on this axis and time down here.

The peak release from the high infiltration case is significantly higher, but the overall value at the end of the 10,000 year period is coming back together. So the influence after 10,000 years is not so great for a radionuclide like technetium, which is dissolution rate limited.

LANGMUIR: Jerry, a quick clarification. On the previous one, carbon-14, is that assumed to be CO2 gas?

McNEISH: Yes.

[Slide.]

McNEISH: For neptunium we see a significant difference in the high infiltration and the low infiltration case. In fact the peaks are about three orders of magnitude difference, indicating the importance of determining what the flux is actually going through the repository.

[Slide.]

McNEISH: If we take those previous three slides and grab off the peak release rate, we can come up with the next slide combining each of the three different radionuclides on one chart showing the sensitivity to infiltration in this manner, with the peak release on this axis. This is a log scale.

For carbon-14, as we noted, the peaks were basically the same for high and low infiltration.

There is a significant difference for neptunium in the peak release rate for this case.

And there is a little over an order of magnitude difference in the technetium peak release rate.

This is for the 83 MTU per acre case with no backfill and low and high infiltration.

[Slide.]

McNEISH: If we look at another sensitivity to thermal load, the blue is the 25 MTU per acre case and the green is the 83 MTU per acre case; the same infiltration and no backfill.

You can see that basically the 83 MTU per acre case has higher peak releases. This is primarily due to the fact that you have more failures from the 83 MTU per acre case and higher dissolution rates because when the packages fail they are at higher temperatures.

[Slide.]

McNEISH: For the cladding failure sensitivity the blue case is a case where when the package failed the cladding was assumed to fail; the green case assumes that only 10 percent of the cladding failed once the outer container failed; the red indicates the case where you have only 1 percent of the cladding failing.

You can see there is a general trend decreasing. With neptunium the peaks are similar for the top two cases, primarily because they are solubility limited for that release.

The important thing to note here is that we did not include a process level model which gradually degrades the cladding over time. We had a switch that said it was either there or not once the outer container failed.

[Slide.]

McNEISH: For cathodic protection we show this effect. Again we are looking at the 83 MTU per acre case with no backfill and high infiltration. The green bars have the cathodic protection implemented.

For cathodic protection, as Joon noted, you push out the time for that first failure. Also, what happens is you get a slight reduction in the peak primarily because of decay. When the packages fail at a later time carbon-14 has decayed away, almost an order of magnitude in half; and the others have decayed slightly as well.

The time for the peak releases for the green bars is not in the first 10,000 years. For carbon-14 it's 16,000 years and for the other two it's almost 40,000 years when that peak arrives because of the delay in the failure of the waste packages.

[Slide.]

McNEISH: Moving on to the EBS release model comparison. Again, we have peak release on this axis and the radionuclides down here. I would note that there are two log cycles between the labels on this axis.

I've talked basically about three conceptual models. They show up here. The first two have the first conceptual model, and this is the next conceptual model for release, and this is the final conceptual model.

What is different in the first two cases which say no backfill and backfill is that those have different thermal-hydrologic information from the process level modeling.

Srikanta presented the fact that they did different thermal-hydrologic analyses either including backfill or not including it, and the temperatures and relative humidities were not that different. So those two cases have very similar results. With backfill you have a slightly reduced peak release, but it's not a great difference.

Those two models use the EBS release model which has diffusive release and advective release from the waste package and also from the EBS.

The next case, which is in red, has only diffusive release from the waste package and then advective and diffusive release from the EBS. You can see that reduces the peak for neptunium and technetium a little bit. As we have seen before, the carbon-14 releases are not affected.

The final case is the case which implements capillary barrier effect. It basically says you have no advective flow on the waste packages; it's all diverted around the drift. That buys us an awful lot, from six to nine orders of magnitude drop in the peak release. There again you've got to assume that you've got a design and implementation of that capillary barrier that lasts for the duration of this analysis for 10,000 years.

[Slide.]

McNEISH: The final result I want to present shows the difference in the conceptual model for the 25 MTU per acre case with no backfill and then a capillary barrier effect in blue versus the Tom Buscheck case, which is comparable, this 24 MTU per acre case with no backfill and no infiltration.

You can see that there is not much difference in the gaseous release and there is some difference in the neptunium and technetium release, primarily due to the fact that in the Buscheck model we have fewer packages failing and less pitting in those packages, so the overall release is lower. Again, this is release from the EBS.

[Slide.]

McNEISH: Conclusions.

I have shown that the capillary barrier effect produces very large decrease in the EBS peak release, from six to nine orders of magnitude for the case that I have shown.

The second EBS conceptual release model, which has only diffusive release from the waste package and then advective and diffusive release from the EBS, produces also a decrease in the EBS release due to the fact that you have to diffuse out of the waste package before you can reach the faster pathway of the advective release.

The alternate conceptual model also produced a decrease in the EBS peak release due primarily to the difference in the humidities and temperatures which were calculated in those models. The Buscheck model has a slightly different gridding and thermal characteristics than the 25 MTU per acre case which I presented.

Finally, the advective release component is very important in determining the overall total release from the EBS.

Thank you.

LANGMUIR: Thank you, Jerry.

I am going to start it with something that probably isn't in your area of expertise, but I think it's a very important point, and maybe it will get back to the researchers who are feeding you numbers for the TSPA.

I have a longstanding objection to using solubility limits on anything but uranium as a basis for defining a conservative concentration of radionuclides at the waste package. If you look at neptunium, it's a trace constituent in the fuel. To get to saturation you are talking about ten to the minus two, ten to the minus four moles per liter. That is as high as two or three grams a liter of neptunium in the water around the waste package.

I don't buy you can ever get there. I don't think there is enough neptunium around to do that. So I think you are shooting yourself in the foot in the program by buying into solubility controls on these trace constituents in the fuel. Sure they are hot, but they are hot at extremely low levels, and the levels ought to be controlled by things like adsorption near the waste package and not by saturation with anything.

I think we would be much better off, and we could defend it, if we chose instead of solubility limits -- incidentally, Gray in some studies in the mid-1980s dissolved spent fuel and got neptunium concentrations in his wash waters from the solubility runs. I would suggest that those kinds of concentrations are more realistic than a saturation concentration. Or, knowing what you know about spent fuel, take it further down the pike in terms of time and then you can probably pretty well guesstimate the neptunium concentrations from the spent fuel you would get, and not saturation with oxides or hydroxides or anything else. They are probably many orders of magnitude less.

That was a speech instead of a question.

McNEISH: You are right. That is outside my expertise.

Dave, do you have any comment on that?

SASSANI: I agree with what you are saying. We need to do some more work in this area. The solubility limits that get imposed are only imposed after the dissolution rate limits. So they are actually lower than the calculated dissolution rate limits on the neptunium.

The reason for that right now is the Steward and Gray dissolution rate model we have for the spent fuel is far from equilibrium dissolution rates. So they are relatively conservative compared against the steady-state dissolution rates for spent fuel. Those types of values for the trace constituents like neptunium are orders of magnitude lower.

In addition, the solubility limits, the range for neptunium that we are using are from over-saturation studies, steady-state concentration studies, which appear to be metastable equilibrium phase, which is also orders of magnitude more soluble than the stable equilibrium phase. So modeling studies may allow us to make that constraint and impose the lower solubility limits, which would also help.

LANGMUIR: I would certainly encourage that between now and the next TSPA. I think it's easy to do.

Pat Domenico.

DOMENICO: The advective release component is very important. I think that is the same as saying the rate of infiltration is very, very important in the sense that you don't take advection into account until you have dripping fractures.

McNEISH: Right.

DOMENICO: My question is, at this loading, 83 metric tons, what temperature is in the engineered barrier system that you use, because most of your parameters are temperature dependent?

McNEISH: I think the highest temperatures were 120 to 130 degrees C.

DOMENICO: Some of that you don't have data for, though. I notice most of your curves go to 100.

McNEISH: Right. We don't start things until it drops below 100 degrees C.

DOMENICO: I forget, but I think 83 is a rather low thermal load compared to what people have been thinking about lately, is it not? The so-called high-high scenarios are up over 100. That's a point where you don't have any information at all in terms of the parameters that are temperature dependent, at least from the graphs I've seen. Is that going to be rectified somehow through experiments someplace, sometime, or aren't you going to start it until it kicks down to 100?

Did I make any sense?

VAN LUIK: Yes, you made perfectly good sense. If there is one of our engineering people in the room, maybe they could help me out. It's my impression that we are gravitating towards basically somewhere around 80 and somewhere around 20 to bound the problem, and that the very high and the very low will probably not be addressed because we have to focus the program.

If we have any of our engineers here, maybe they can set me straight on that.

I think the tendency is to focus pretty much where the TSPA was focused.

DOMENICO: I see.

LANGMUIR: Leon Reiter.

REITER: I have two questions. I wonder if you could take those sensitivity charts and show us to what extent they support or do not support some of the assertions in the waste isolation strategy we heard yesterday. I'm not quite sure what the match-up is. I know they don't assume capillary barrier. Perhaps you could do that.

The second question is on infiltration rate. In TSPA-93 we saw that the critical thing, which was surprising but intuitively correct -- I think Ed pointed this out before -- was as you get more confining fractures less waste packages were being wet. To what extent is the sensitivity infiltration controlled by the assumptions about how many packages are being wet? If that's the case, sort of following up what Ed said, to what extent would knowledge we gained from underground enable us to limit that effect by pointing out where the fractures are actually?

McNEISH: That's a good point.

On your second question, the number of packages which get wet in that high infiltration case is a little bit over 50 percent. If we were to find out that there were fewer fractures within the repository zone that would actually get wet, then you would reduce that and thus you would reduce the releases.

REITER: To what extent is that factor controlling the effect of infiltration?

McNEISH: It's very significant.

REITER: In the WEEPS model -- Mike can correct me if I'm wrong -- I think they assume like only a fraction of a percent of the packages got wet.

McNEISH: It's very significant. You kind of see that when you go from the initial conceptual model to the capillary barrier model. You see the reduction of the peak release caused by getting rid of the advective releases.

As far as your first questions goes, I don't know much about the waste isolation strategy. I really can't speak to that one.

ANDREWS: Let me try to answer you a little bit, Leon. I'm going to start with the second one first, because I think there are some misconceptions about the fracture frequency and the frequency of flowing fractures that one might have.

Fracture frequency, at least from borehole observations, is quite high. You have a lot of fractures per ten square meters, if I look at an areal sort of term. So saying that you are going to limit the number of fractures that flow by or you think there is one fracture per kilometer or something like that, I think that is very unlikely.

Having said that, I would make a second observation that identifying flowing fractures even in saturated media, well characterized saturated media like STRIPA, like the Grimsel test site in Switzerland is extremely difficult. So you have fractures, but a very, very small percentage of them flow.

Identifying those and saying which ones might weep and which ones might not weep is an incredible stochastic issue that I don't think people have their hands around right now even in very well characterized systems, and those are very well characterized systems at the scale we are interested in.

That is your second question.

The first question. We looked at alternative backfills and alternative conceptualizations of flow, if you will, in the drift. We did not do explicit hydrologic modeling of that flow in the drift. We did thermal-hydrologic calculations, but we didn't say, okay, we have this drip; how does that drip redistribute itself in the drift? We did not do that.

What Jerry showed, I think, is that if I have very favorable hydrologic characteristics in the drift I can divert that water and I don't have any advective release. The whole issue is advective versus diffusive releases from the EBS, and that's not a new issue either. Virtually every other international program relies on diffusive releases from their EBS to give them the performance that they get. Most with bentonite because most are in saturated systems, not unsaturated systems. Our behavior works differently in an unsaturated system, but it has the same net effect.

The other effect of in-drift backfill type materials is the thermal-hydrologic characteristics. There, as I think Srikanta pointed out, and you are going to hear some more calculations after lunch, we have two different conceptualizations of the characteristics of in-drift thermal-hydrologic properties. They have different results. Those results have different effects. They can have a very positive effect, extremely positive effect, keeping the humidities low for extremely long periods of time.

In fact, the higher thermal loads, as I think Srikanta showed you from the results from Tom Buscheck, keep you below our magical 70 percent relative humidity cutoff for tens of thousands of years and for some packages hundreds of thousands of years. When I combine that with certain corrosion degradation a lot of packages don't fail, and in fact most packages don't fail for the first 100,000 years.

So it can have a very positive effect on the in-drift thermal-hydrologic regime which then controls a lot of other processes.

Do we know which is the correct thermal-hydrologic representation? Heck no. Will we know? I think the strategy identified that as a key need and this afternoon I'm going to identify it as a key need. I don't think we have the answer right now, but it can have a very positive effect.

LANGMUIR: Jared Cohen.

COHON: I am concerned, I think, about the exclusive focus on peak release. I certainly recognize that the results that you generate are much more than just the peak release. You show a complete release history or projection. But all of the model sensitivities are geared to peak release. Yet if the thing that is going to be driving the design and study here is peak risk, one certainly will be interested in more than just peak release.

Could you address this, someone? You are certainly supporting the risk part of this with the results that you generated, but in determining the sensitivity of your models you are focused entirely on peak release, whereas if I really cared about risk and that was driving me, I would want to be very careful about sensitivities of the model in terms of the total release.

McNEISH: That's right. Dave will present results that show the doses. These were sort of intermediate results to see what's happening at the EBS interface with the geosphere.

COHON: I understand. But you get my point about focusing a model sensitivity only in terms of peak release rather than total release, that you could find yourself not very happy about having done that.

McNEISH: Yes.

ANDREWS: One of the things we looked at and we are not going to show, by the way, for brevity of time, is correlation of the different performance measures. In a way what you are asking is, is there any relationship between this peak EBS release and anything that relates to long-term post-closure total system performance like dose or risk? In fact the correlation is very poor.

LANGMUIR: But you were doing what you were asked to do basically a year or so ago before the NAS came out with some new ideas. Am I right in that?

ANDREWS: I think we always want to look at what does a subsystem performance measure, if you will, and what we heard this morning is two subsystem performance measures, one related to the package and one related to the EBS. What correlation do they have to the total system performance? That's a very important issue, of course.

LANGMUIR: We are five minutes beyond our allotted time for discussion here. I would like to adjourn for lunch and bring us back in about an hour. We are due to reconvene at 12:30.

[Whereupon, at 11:35 a.m., the meeting was recessed, to reconvene at 12:30 p.m., this same day.]

AFTERNOON SESSION

[12:35 p.m.]

LANGMUIR: Our first presentation this afternoon is geosphere transport and release/dose. The speaker is David Sevougian of INTERA.

[Slide.]

SEVOUGIAN: Good afternoon. I see I luckily have the prime time of this afternoon, right after lunch. So everybody will be asleep and won't notice any mistakes. This afternoon, I'm going to primarily talk about predicted radionuclide release and dose at the accessible environment for TSPA-1995, and this is the five kilometer fence boundary that we've been talking about.

[Slide.]

SEVOUGIAN: Before I get to the release and dose, I'm going to talk about the models, the TSPA conceptual models for unsaturated zone and saturated zone transport. And then I'll get to the releases and doses, predicted results, and I'll wrap up with a comparison of subsystem performance. So this will be EBS versus natural barrier performance.

[Slide.]

SEVOUGIAN: And where this fits in the overall scheme of things. I think this is the last time you get to see the road map here. Sorry. So I'll be talking about the transport models. Every other model feeds into the transport models. And then I'll talk about the releases at the accessible environment and briefly about the flux, the advective flux, since we have to have advection to have transport of nuclides. So given we need flux of the water, at least for advective transport.

[Slide.]

SEVOUGIAN: This is the unsaturated zone aqueous transport model and we use the RIP model by Golder. It's the stochastic model, where we do many realizations of the some 270 stochastic variables that are uncertain. The first thing I'll talk about is the geometry of the pathways and then about the dual-continua fracture/matrix representation, and then the two parts of that, how much mass is traveling in each continuum and how fast.

[Slide.]

SEVOUGIAN: So the first part is the geometry. We represent in the TSPA model the pathway geometry as a series in one-dimension. So it represents three-dimensional flow. It's a series of parallel 1-D pathways that are connected and each pathway represents one hydro-geologic unit. I have a little picture here of the geometry.

[Slide.]

SEVOUGIAN: So at the top, we have the repository horizon and depending on the thermal load, we divide it up into either six or ten columns, six for the high thermal load. So the percolation flux goes through the waste packages, then down through the unsaturated zone horizon, through the saturated zone to a hypothetical water well that penetrates the aquifer. And each unsaturated zone pathway has a dual-continua representation.

[Slide.]

SEVOUGIAN: Now, the next part has been talked about earlier this morning. This is the process level abstractions that feed the transport models. And there was two parts, the fracture flow fraction or how much was going through each continuum and then how fast. And I'll just show these briefly because Srikanta talked about them earlier.

[Slide.]

SEVOUGIAN: If you recall, there was a series of process level simulations that were done to determine, over a range of hydrologic parameters, conceptual models for fracture flow initiation, determine the minimum and maximum fracture flow fraction. And then in the actual rolled-up TSPA model, when we sample the infiltration rate off the distribution, we come in and sample uniformly between these two curves and come up with a flow fraction for each hydro-geologic unit.

Here it's the Calico Hills zeolitic unit. And there's a similar abstraction for the velocity. That was for the fracture flow fraction.

[Slide.]

SEVOUGIAN: This is for the matrix velocity field, a similar abstraction. Okay. That's the process level feeds into the transport model.

[Slide.]

SEVOUGIAN: Now, there are a couple of other pieces to it that are directly incorporated in the transport model, in the TSPA model itself. One is fracture connectivity. And there's two types, intra-unit and inter-unit. So between hydro-geologic units, fractures from one unit don't connect directly -- well, they connect directly, but only a part of the mass goes directly into the fractures of the next unit. So at a unit boundary, there is some dispersion. And intra-unit, within a unit, the model has a Markovian process that determines how much time particles spend in the fractures and the matrix.

[Slide.]

SEVOUGIAN: And a little schematic here. There's this transition parameter lambda that's the rate at which particles transition between fractures and matrix. We looked at three cases, one where the particles spend relatively more time in the matrix, so they'll be slowed down compared to where they spend, most of the time in the fractures. So this is the average path length within a fracture in a particular unit. So here there's hardly any particles whatsoever that go into the matrix.

This case is kind of a default case that I'll show for most of the runs.

[Slide.]

SEVOUGIAN: And the other piece is related to how fast. This is retardation. There's a chemical and physical retardation of the fracture/matrix velocities.

[Slide.]

SEVOUGIAN: Chemical retardation within the matrix for radionuclides such as neptunium would be the most important one. These are based on experiments at LANL on whole rock tuff samples. And because it covers a wide range of effects, such as ion exchange, sorption, hopefully not precipitation, but possibly, they're developed as stochastic distributions to feed into the TSPA model.

And for some sensitivity cases, we looked at physical retardation in the fractures in the form of equilibrium matrix diffusion of the particles from the fracture to the matrix. This effect can slow down both sorbing and non-sorbing nuclides, but has a greater effect on the sorbing radionuclides.

That was the unsaturated zone. Let me change to another part which isn't really on this diagram, but it's way up at the surface.

[Slide.]

SEVOUGIAN: The infiltration and the climate change model which will effect the percolation at the repository horizon. We've talked about the two scenarios already for infiltration flux, a high and a low scenario and the range of 0.5 to 2.0 millimeters a year and 0.01 to 0.05 millimeters a year. Now, that's the infiltration flux at closure, the initial value.

So superimposed on that out to 1 million years is the triangular wave, with a period of 100,000 years, a peak at 50,000 years. The peak infiltration is some multiples uniformly sampled between one and five of the initial infiltration. So, for example, if we sampled five, then we could go up to a maximum of 10.0 or 0.25 for these two scenarios.

All the examples I'll show later have this climate change variation of Qinf infiltration rate included in them. A few sensitivity cases, which I'm not going to show today. We ran a simultaneous water table rise with the same period and uniformly sampled between 20 and 80 meters. It tends to increase the doses somewhat.

[Slide.]

SEVOUGIAN: Next, move down to the saturated zone here. Five kilometers from the base of the repository to the water well. Here, we used a composite permeability and flux model. So we averaged the fractures and matrix flow using the distribution that was developed for TSPA-1993 of two meters per year, a mean of two meters per year. The accessible environment boundary was five kilometers from all the UZ columns. We used Kd's, slightly different than unsaturated zone Kd's.

Since it was 1-D, there was longitudinal dispersion, but no lateral dispersion, which, if we had included it, could reduce the doses by a factor of 10, 100. It depends on where you're looking at the releases, at what point you're looking at the doses.

LANGMUIR: David, can you do us a favor? Back on overhead 12, some folks don't know what Kd is and it might be constructive to just take a minute and define it, since it's critical. It's your overhead 12, which has the equation. You've got some terms in the equation. That might be the easiest way to go at it.

SEVOUGIAN: The equation doesn't really define Kd. But the Kd is the ratio of the amount of mass sorbed on the rock matrix over the amount of mass in the pore water. This is a retardation factor. So when Kd is zero, there is no mass sorbed on the rock matrix. The velocity gets divided by this factor. So when Kd is zero, this factor is one. The radionuclides are unretarded. When Kd is greater than one, then this factor is some number greater than one, and the radionuclides are retarded by this factor. Velocity is retarded or slowed down compared to non-sorbing radionuclides.

Does that give you a better idea?

LANGMUIR: For the moment, I guess, yes.

SEVOUGIAN: For the moment. Maybe I better keep it out.

LANGMUIR: No, no, no. It's all the way through your analysis. So yes, right, keep it out.

[Slide.]

SEVOUGIAN: Okay. Next, I want to talk about the biosphere model a little bit, just briefly, and put the human into the equation here. This is the maximally exposed individual who is drinking two liters a day. That's what we modeled. We used the EPA 1988 dose conversion factors for ingestion only, assuming he was drinking two liters a day at the accessible environment.

And as far as the dilution, the volumetric flow in the saturated zone was assumed to be repository width times a 50-meter well depth from which he was withdrawing the water times the saturated zone velocity, which is stochastic distribution. It's different in every realization.

[Slide.]

SEVOUGIAN: Now I want to switch gears again and go to the results. All of them are at the accessible environment.

We'll be looking at, first, 10,000-year performance, the short time frame we can predict real well, and then 1 million-year performance. And then mainly I'll be looking at sensitivity analyses, three types; one for alternative conceptual models for the geosphere, such as fracture/matrix interaction.

For the near-field environment, this will be mainly different thermo-hydrologic models for the effect of heat and then alternative repository designs, such as the capillary barrier.

[Slide.]

SEVOUGIAN: And you should keep in mind that the sensitivity analyses should look at the relative magnitudes, maybe more than the average magnitudes, and these can act as a guide to show us what the most important effects are, like John Kessler was talking about yesterday -- dilution in the saturated zone.

[Slide.]

SEVOUGIAN: So let me just mention the performance measures at 10,000 years. First, you'll see one plot to the normalized Table 1 limits, cumulative release. The other ones will be CCDF of total peak dose to the maximally exposed individual over the range of the stochastic variables. And the last one will be expected value dose histories or breakthrough curves that give the dose history at every year during the time period for 10,000 years.

Technetium, iodine and carbon are the most important ones.

[Slide.]

SEVOUGIAN: Here's the sensitivity analyses. They're pretty similar to what Jerry showed earlier. First is high versus low infiltration rate or, equivalently, percolation rate through the unsaturated zone.

Next is the thermal loading, high versus low. Then there's the alternative thermo-hydrologic model. This is the Buscheck model versus the one we had used to begin with. Fracture/matrix interaction. Waste package degradation. Here, I'll be mainly concerned about cathodic protection. And then backfill.

Now, for the 10,000 years, actually you're not going to see as many graphs on these sensitivity analyses as for 1 million years, and the reason is because there were no releases for a number of cases at 10,000 years. Nothing at all coming out to the accessible environment.

[Slide.]

SEVOUGIAN: Here are the zero release cases. At the accessible environment, there were no releases whatsoever for low infiltration, at least for the range we picked there, for any of the examples. There were no releases for the Buscheck high thermal loading case because of his low relative humidity and high temperature curves.

There were no releases if you assumed equilibrium matrix diffusion in the unsaturated zone. And there were no releases for cathodic protection of the waste packages at 10,000 years.

Now, some of these get at what Leon was asking earlier about the waste isolation strategy. For example, the first one is seepage rate. If I can remember what all the parts of the strategy were, it also gets at the waste mobilization through the interaction with the source term and the flow. The transport and the engineered barriers, this thermo-hydrologic model gets at that. Also, the backfill. I'll talk about the other ones later.

That boils us down to three cases.

[Slide.]

SEVOUGIAN: This is the cumulative release CCDF that I promised to show, the one and only one here. So here's the normalized cumulative release relative to the Table 1 values. This is the probability of exceeding any of these values. So if you look at the median case, for example, at 50 percent, there's a 50 percent chance of exceeding 10 to the minus 3 of the limits.

So the thing I wanted to point out here is that at least for this thermo-hydrologic model, there's not a whole lot of difference in the total releases for the two thermal loading cases.

[Slide.]

SEVOUGIAN: As far as CCDF of peak dose goes, the high thermal load has a slightly higher peak dose over the range of the stochastic variables. For example, the median values is about a few tenths of a millirem. And, again, the reason the high thermal load has a higher dose, slightly higher, is due to higher degradation of the packages and a higher dissolution rate.

[Slide.]

SEVOUGIAN: I just want to show a breakthrough curve briefly so you can see the most important radionuclides. For the high thermal loading -- and this is similar. The same nuclides for the low thermal loading, pretty much. Technetium and iodine have about the same dose. Then carbon-14, chlorine, and neptunium. I'm going to come back to this plot in a little bit when I talk about capillary barriers.

COHON: When you say dosage, you really mean concentration at the limited accessible environment.

SEVOUGIAN: Yes. It's concentration multiplied by the dose conversion factor.

COHON: It is converted into a --

SEVOUGIAN: Yes. This is rems per year. I guess I didn't say that. So this is a dose. It was converted from grams per year.

COHON: So it builds in the assumptions about ingestion and exposure.

SEVOUGIAN: Yes, right.

COHON: Okay.

[Slide.]

SEVOUGIAN: The next sensitivity analysis at 10,000 years was intra-unit fracture connectivity. If you remember, that was this picture here where we had, for this value of lambda, more flow in the matrix, more transport in the matrix, and, this one, much more in the fractures.

If we look at a CCDF, at least at 10,000 years, this particular conceptual model has a large effect. There's enough unsaturated zone to delay the radionuclides significantly if they spend most of the time in the matrix as they do on this blue curve. So there's about a four order magnitude difference here between this curve and the nominal case here.

LANGMUIR: Are those dilution numbers simply that concentration in a liter or in 100 liters or 0.1 liters? Is that what that means?

SEVOUGIAN: The dilution?

LANGMUIR: Yes. Is that what that is?

SEVOUGIAN: This is rem, again, dose at the accessible environment, rems per year.

LANGMUIR: No, the fractions up on the figure.

SEVOUGIAN: These fractions?

LANGMUIR: Yes.

SEVOUGIAN: This, this, this and this?

LANGMUIR: Yes.

SEVOUGIAN: This the average length that a particle travels in a fracture before it goes to the matrix. It's like a fracture connectivity. It travels a tenth of the unit thickness. It's like the thickness of, let's say, the Topopah Springs and then it goes from fracture to matrix. That's on average. It's a stochastic model.

So here, on average, a particle will go the whole length of the Topopah Springs before it goes to the matrix. So for these two cases, we have predominantly fracture flow.

[Slide.]

SEVOUGIAN: Okay. The last thing at 10,000 years is the backfill. As I said, I wanted to throw up this breakthrough curve again. I don't know if we mentioned this earlier or not, but we modeled iodine, chlorine and carbon as gas phase radionuclides in the sense that when they come out of the waste package, they come out in gaseous form and are transported across the EBS to the geosphere in gaseous form.

And the two aqueous ones on here are technetium, which is an unretarded, non-sorbing, and neptunium. So if we look at the effect of capillary barrier, this is assuming somebody can construct this, the first two curves -- this is, again, rems per year. This is dose at the accessible environment to the maximally exposed individual.

And the two curves on the right represent no backfill or a gravel backfill, and the only difference between them is the relative humidity/temperature histories that were developed from the thermo-hydrologic model in the near field. Since those histories were not very different, the curves, the results are not very different.

Now, we put a capillary barrier in which intercepts all drips, all advective flow onto the packages. That prevents technetium -- these two curves are due to technetium and iodine. It prevents technetium from getting to the accessible environment. So that the curve drops by a factor of two because these two provide the same dose.

It seems quite possible, because of the high reactivity of iodine gas and chlorine gas, that they wouldn't make it all the way across the EBS without going into the aqueous phase.

LANGMUIR: Is this flying iodine we heard about yesterday?

SEVOUGIAN: What?

LANGMUIR: Is this the flying iodine we heard about the other day?

SEVOUGIAN: Flying iodine?

LANGMUIR: Yes.

SEVOUGIAN: Yes. This is the iodine that goes its merry way across the EBS. But if we stop it from flying, we shoot it down or whatever. We don't shoot down the carbon, though. We shoot down the iodine with chlorine. We get about an order of magnitude reduction in the peak dose at the accessible environment. Actually, if we weren't worried about carbon -- see, what we did in this TSPA is we conservatively assumed that instead of the carbon going straight to the atmosphere, it went to the geosphere and was dissolved into the aqueous phase.

So if it went to the atmosphere, then we wouldn't even be worried about this and we'd have only diffusive releases out of the packages and you'd be way off here. It's about 10 to the minus 9 or so.

REITER: You'd have a lot larger release.

SEVOUGIAN: Larger release? I'm not following you there.

COHON: If carbon went directly to the atmosphere, you'd have a larger release.

SEVOUGIAN: Yes. But the NAS report saves us from carbon, right? I mean, it's diluted so much that it doesn't really matter.

[Slide.]

SEVOUGIAN: Okay. Switch to 1 million years. Look at CCDFs of total peak dose. Look at some more dose histories and, finally, I'll show some linear regression statistics. We're trying to do curve fitting to the results to see which model parameters are most important.

[Slide.]

SEVOUGIAN: Sensitivity analyses. Same as 10,000 years, but I'm going to show them all for 1 million years because we had some releases at 1 million years for all cases. The first is infiltration rate.

[Slide.]

SEVOUGIAN: Comparison of, for technetium, the breakthrough curve for high versus low infiltration. Over 1 million years, this is the dose exposure at the accessible environment. For high infiltration, you get a sharp high peak that comes out early. For very low infiltration, past the packages through the unsaturated zone, it's too low to dissolve all the source term into it. So you get it spread out much more and much lower.

[Slide.]

SEVOUGIAN: For neptunium, for sorbing radionuclides, the effect is even stronger. And if you're at 0.03 millimeters a year, then no neptunium essentially comes out.

[Slide.]

SEVOUGIAN: The next case is a combination of infiltration rate and repository loading. I'm going to show a couple of CCDFs here over the entire range of the stochastic variables. The curves on the right are for the high infiltration rate case, over the entire range of that infiltration rate case. In fact, these points out here are for the high end of the range. These are for the low end, in general.

Then for each pair, the curve on the right is for the high thermal loading. The thing to take away here is, again, at least for this thermo-hydrologic model, there wasn't a whole lot of difference in the effective thermal loading. But as with the breakthrough curves, there was a great difference for these particular ranges of percolation rate through the repository.

[Slide.]

SEVOUGIAN: Next is a combination of numbers 1 and 3, the infiltration rate and the thermo-hydrologic model. If you remember from earlier, Srikanta's talk, the relative humidity and temperature predictions, there was quite a large difference in the relative humidity between the Buscheck model and the model that we had used. Buscheck's model is much lower. The corrosion initiation starts much later.

However, over 1 million years, it ends up not having as large an effect as you'd think. Everything kind of tends to go out in the wash in 1 million years. So this is similar to the last one. Low infiltration, high infiltration or percolation. For each pair, on the left is the Buscheck model, slightly lower doses. But it's really only about a factor of 3 difference between these two thermo-hydrologic models, even though there was a large difference in the early time temperature/relative humidity histories.

[Slide.]

SEVOUGIAN: Fracture/matrix interaction, show one plot on that. As far as this connectivity parameter, when particles spend more of the time in the matrix, and as the blue curve indicates, then you get quite a bit delay, 80,000 years, in the initial release to the accessible environment. But you see that you don't end up with much of a difference in the peak dose, which is the important thing.

[Slide.]

SEVOUGIAN: Matrix diffusion, the next one. This has a similar effect to the intra-unit fracture connectivity. When you have matrix diffusion from the fractures to the matrix, you get about a 150,000-year delay in the initial release to the accessible environment. But, again, the peaks over 1 million years are about the same and that's borne out -- if you look at CCDFs for these two fracture interaction examples over the entire range, there's very little difference for either the connectivity parameter or the matrix diffusion parameter.

[Slide.]

SEVOUGIAN: Waste package degradation. As you recall, cathodic protection at 10,000 years had a very large effect. There were no releases whatsoever at 10,000 years. At 1 million years, it tends to die away. The effect isn't very important, as you can see here by the blue curve. This is the doses with cathodic protection.

[Slide.]

SEVOUGIAN: And the last one is backfill. Although cathodic protection had less of an effect at 1 million years compared to 10,000 years, it's kind of the opposite case with the capillary barrier because of neptunium. So the capillary barrier kind of looks like it has more of an effect at 1 million years.

You have no capillary barrier. Neptunium is the main contributor to the dose. And when you institute this capillary barrier, which still allows the flying iodines, you get about an order of magnitude or so reduction. If you assume iodine and chlorine dissolve in the aqueous phase in the EBS, it looks like about close to a four order of magnitude reduction.

Again, if carbon-14 were not important, then the diffusive releases even over 1 million years are way off the page here.

[Slide.]

SEVOUGIAN: Let me go to the last part of the results, which was trying to look at sensitivity of the model parameters. I'll show some scatter plots, over 100 realizations of total peak dose versus various model parameters, to look for linear trends, and, also, step-wise linear regression which looks for the most important groups of variables.

[Slide.]

SEVOUGIAN: Let me just show my picture again here. The first thing is a plot of peak dose over 100 realizations versus the saturated zone velocity. So that would be the velocity here. This will be the effect of dilution. This is at the high percolation range and this is at the low percolation range, past the packages. So there's a strong linear trend, more so at the low infiltration because dilution is stronger there. That's the most important parameter over 1 million years, and John Kessler brought that out very well yesterday.

[Slide.]

SEVOUGIAN: The next most important parameter doesn't show much of a trend, at least here. This would be the percolation flux through the unsaturated zone. It shows somewhat of a trend here at the high infiltration, none at the low. However, these are pretty narrow ranges. If you look at the whole range, then actually it becomes a more important parameter, which we have found in TSPA-1993 that it was very important.

[Slide.]

SEVOUGIAN: Here is the plot over the entire range from 0.01 to 2.0 millimeters a year for the unsaturated zone flux. You have a transition here from iodine or technetium to neptunium being the most important radionuclide.

[Slide.]

SEVOUGIAN: Now, if we look at groups of variables, that's the next couple of slides. We did a couple of curve fits. You see two columns here. There's a curve fit of -- the first one is the log of the performance measure. That's a plot of peak dose plotted against parameter. So, for example, the saturated zone flux.

[Slide.]

SEVOUGIAN: And then here is a log-log curve fit of peak dose versus saturated zone velocity. The best fit is the log-log and you see that the saturated zone velocity explains 48 percent of the variability of the CCDFs.

When combined with the infiltration rate or the percolation in the unsaturated zone, the two together explain 65 percent of the variance. This is at the high infiltration rate range.

[Slide.]

SEVOUGIAN: If we go to the low range, the saturated zone velocity explains even more because dilution is more important. It explains 89 percent of the variance.

[Slide.]

SEVOUGIAN: And if we look, finally, at the entire range, we switch from saturated zone velocity or dilution to being number one to the percolation rate through the repository horizon, through the entire unsaturated zone explains 50 percent. Then combined with saturated zone velocity, 74 percent.

So these are the parameters that, if we get a better handle on, we can get a better idea of how the repository is performing.

LANGMUIR: Could you help me out? I'm just a geochemist. So some of this hydrology is unfamiliar to me. But would you please explain how, in filtration rate and saturated zone flux -- okay. Saturated zone flux is in the saturated zone below the unsaturated.

SEVOUGIAN: Yes.

LANGMUIR: So the infiltration rate is in the unsat zone.

SEVOUGIAN: Yes. The infiltration is through here.

LANGMUIR: Okay. So one rate is the rate of infiltration down to the saturated zone and the other one is the flux in the saturated zone. Right?

SEVOUGIAN: Yes, right.

LANGMUIR: It doesn't, in fact, define, dilution, does it? It's the quantity of fluid.

SEVOUGIAN: The ratio between them defines the dilution. So it's like how much -- you've got a little stream of water going into a big stream of water.

LANGMUIR: Right.

SEVOUGIAN: So the bigger the stream is.

LANGMUIR: And that's incorporated in the calculation. This dilution effect is part of what you're talking about. What are the rough proportions of infiltration volumes to groundwater? What kind of proportions are we talking about here?

SEVOUGIAN: On the order of maybe 10 to the 4th to 10 to the 6th. Maybe 10 to the 5th, let's say.

LANGMUIR: Okay. So that's the dilution immediately when you get down from unsat to sat.

SEVOUGIAN: Right.

LANGMUIR: Okay.

SEVOUGIAN: It depends on the values you use for the flux. Right.

DOMENICO: That's 10 to the minus 8, probably.

SEVOUGIAN: Well, no.

LANGMUIR: Is that included in your model?

SEVOUGIAN: You're sampling -- I mean, you drill a well and you just -- it depends on how much of the plume you sample. If the plume is real narrow and you --

DOMENICO: At the well, it's 10 to the minus 4. Is that what you're saying? At the well, it's a dilution factor of 10 to the minus 4, more or less, 10 to the minus 5.

SEVOUGIAN: You sample the concentration. It depends on how the plume is -- if the plume extends all the way past the perforation data, you just get the concentration that's in the plume. If the plume is real narrow, the well extends beyond it and you get the dilution ratio of the thickness of the plume to the well depth.

DOMENICO: But your dilution volume is the cross-sectional area of the repository times the 50-meter well depth times the flux.

SEVOUGIAN: That's what we assume for the dilution model.

DOMENICO: That would be a dilution volume.

SEVOUGIAN: That's what we assume for the dilution model.

LANGMUIR: Do you ignore any further dilution and dispersion beyond the mixing zone under the repository or do you simply assume you've got a packet of water mixed that goes undiluted further, without any further dilution all the way to the accessible environment?

SEVOUGIAN: It's diluted a little bit by longitudinal dispersion. We don't have any lateral.

LANGMUIR: You don't include any of that.

DOMENICO: That's probably a second order effect.

SEVOUGIAN: Yes.

DOMENICO: Compared to dilution.

SEVOUGIAN: Yes.

LANGMUIR: Excuse us. This is useful, though.

[Slide.]

SEVOUGIAN: The last slide is about subsystem performance. Here we want to look at the performance of doses at the EBS, which would be right up here, versus doses at base of the vitric here below the repository here and then versus doses at the base of the unsaturated zone versus doses at the -- actually, sorry, it's releases -- at the accessible environment.

So these are cumulative releases of neptunium at the accessible environment in curies and this is at different times. So what you see is at 10,000 years, the unsaturated zone below the base of the vitric here, that is the Calico Hills unit, has a large effect just because of the length of the flow path.

However, by the time you get to a 1 million years, most of the neptunium has come out through the whole natural system and there's only a difference of about 1.5 here between what's come out of the EBS and what's come out at the accessible environment. Approximately two-thirds of the neptunium breakthrough curve has come out at the well.

Also, we didn't have much influence, the way we modeled the saturated zone, we didn't have much effect for the saturated zone being a barrier.

[Slide.]

SEVOUGIAN: Finally, the conclusions. Just to refresh your memory on the 10,000-year performance. The releases were below the Table 1 limits. There were no releases for low infiltration for the range we used at the accessible environment. For the Buscheck model, high thermal load model, no releases. Cathodic protection, no releases at 10,000 years. And assuming matrix diffusion, the geosphere, the unsaturated zone, there were no releases.

However, that fracture/matrix interaction term, the connectivity parameter did have an effect because of the travel length relative to the time that we looked at the doses. And I didn't show the last one, but the 10,000-year peak dose is most sensitive if you do a linear regression through the velocity, matrix velocity in the Calico Hills vitric unit simply because it has the lowest tendency towards fracture flow. And then the 10,000-year peak dose is sensitive to the percolation flux in the unsaturated zone.

[Slide.]

SEVOUGIAN: The 1 million performance was most sensitive to either dilution in the saturated zone or the percolation flux in the unsaturated zone. If you remember, a barrier that could intercept the drips on the packages so you have no advective releases had a large effect. However, I don't stay awake at night thinking about it. Sorry, Jean.

Fracture/matrix interaction did have an effect -- no, sorry -- it had a delay in effect, but no real effect on peak doses. Also, we saw that the thermo-hydrologic model, the corrosion initiation model, the degradation model, cathodic protection didn't have much of an effect in a million years.

That concludes my talk.

LANGMUIR: Thank you. One of your last conclusions there is, I think, important to us since we're not -- the latest episode is we're not going to the Calico Hills. I'm wondering what your thoughts are, having done the TSPA, on the importance of that comment? The 10,000-year peak dose depends on the matrix velocity in the Calico Hills.

SEVOUGIAN: That's right.

LANGMUIR: How much could that matter, I guess, is the question.

SEVOUGIAN: We found that also in the Calico Hills system study. I guess I don't have the results with me, but it explained about 50 percent of the variability, I think.

LANGMUIR: How much would it lower your peak does, I guess, is the question. How much might it lower the peak dose?

SEVOUGIAN: If most of the travel were -- let me rephrase that. It depends on the velocity through the unsaturated zone. So because it has a higher saturated conductivity in the Calico Hills vitric it takes a much higher velocity to initiate fracture flow in that unit.

As far as how much of an effect, you could quantify it with these regression statistics and it had a value of about 50 percent. It was ranked number one with a value of about 50 percent. It is important at 10,000 years because it delays it. It delays it so it never comes out. It comes out later, but it doesn't come out within 10,000 years, assuming we have a good handle on the properties of the Calico Hills.

LANGMUIR: Pat Domenico.

DOMENICO: In all the history of the total system performance assessments, eloquently spoken to by Abe, we have never ever seen this much emphasis on dilution. We've seen dispersion, we've seen retardation. Do you have a new source code here that because now we've got this poor soul out there at four kilometers with a 50-meter well and now you can define a dilution volume? Have you done some changes in the source code to incorporate this?

Because we've never ever heard how important dilution was before, except you never got enough of it.

SEVOUGIAN: A lot of people can probably answer better than me, but --

DOMENICO: Am I right or am I wrong here?

SEVOUGIAN: In TSPA-93, it had a very large effect. We had a plot in TSPA-1993 that showed a very steep curve for dose versus saturated zone velocity.

DOMENICO: Saturated zone velocity, but you didn't relate that, at that time, to us to dilution. Now I can see where it's coming from.

ANDREWS: This is Bob Andrews. We talked about dilution, by the way, several times in the saturated zone. Perhaps everybody's awareness is more acute now with the NAS recommendations. We talked about it and we had dilution before. We're, in fact, assuming the same dilution now as we assumed in TSPA-1993.

DOMENICO: I thought the way you handled dilution before was you had this plume moving along and it was being recharged by rainfall.

SEVOUGIAN: No.

DOMENICO: That's not the way you handled it before.

ANDREWS: No.

DOMENICO: This is droplets coming into a river. You have it now.

SEVOUGIAN: Well, it's not much of a river here.

DOMENICO: Very small velocity going into a rather large one.

SEVOUGIAN: Yes. Any more questions?

LANGMUIR: Jared Cohon.

COHON: Some basic questions, again. I was having trouble relating this model to some of the prior models we were hearing about. In particular, the way you handle the flow going through the unsaturated zone to the saturated zone.

Is it fair to say that what you have is like a box model and you're passing things on from box to box? And that to the extent that physical phenomena are being modeled, it's as represented by the abstractions from the prior models? That is, you're not modeling the physics of the flow exclusively.

SEVOUGIAN: I don't know if I'd say that. Within each, it's like a coarse disparitization of the problem. So within each block, you do have like a model for flow and transport.

COHON: How does that differ from the prior models, the models from which abstractions were taken?

SEVOUGIAN: What the difference is this year is that we fed in a whole range of abstractions from process level models into the unsaturated zone transport in the rolled-up TSPA model, whereas last time we just had an arbitrary distribution for unsaturated zone flux.

COHON: So the upstream models are basically upstream in a conceptual sense to give you those input terms.

SEVOUGIAN: Right.

COHON: Okay. Good. I said it was basic. Can I ask two more very quickies?

LANGMUIR: Yes.

COHON: What is total peak dose rather than peak dose?

SEVOUGIAN: Total peak is the dose from all the radionuclides.

COHON: Thank you. What's a complementary cumulative distribution?

SEVOUGIAN: That's one minus the CDF.

COHON: Okay. Do I have time for one more?

LANGMUIR: Yes.

COHON: There's sort of a philosophical problem or issue that's been implicit and I think it needs to be made explicit, and that's the issue of how do you deal with things over such long time periods out. The implicit assumption seems to be when we're looking at a particular phenomenon and we recognize that there's uncertainty associated with it, we're going to assume that the uncertainty is uniform across some particular range. We're going to define the range and then use a distribution that's uniform across it, which it certainly seems reasonable because we don't know any better.

But there may be cases where we have to think about whether that is reasonable, whether we can dream up a credible connection between phenomena.

So, for example, one of the things I'm sort of stuck on is the way climate change is handled. Who can argue with it, except to say that you've got two separate numbers that you're generating in a random way. One is the starting flux, whether it's zero to 0.5 or 0.5 to 2.0, and you're generating, I guess, uniformly within that range, and then a second parameter is the multiplier over the triangle.

Now, it may be reasonable, if you talk to people who really understand climate changes, to the extent anybody does, or the physical phenomena underlying them, that those two parameters might be strongly connected. Do you see my point? And, furthermore, having gone up a ramp for 100,000 years, would you return to the number that you had started that ramp with?

SEVOUGIAN: Maybe we could just use an average of both of those things.

COHON: No, no, no. All I'm saying is that -- well, this is the other philosophical point that's closely related to it. The real value of the models is to help people to get better insight into what's going on. Right?

SEVOUGIAN: Right.

COHON: There's no question that you have to produce a number at the end, which is the dose number, and that's a very important part of site suitability and then eventually a license. But the real value is so that you all and people like us can understand what's driving that number.

If we get so caught up in sort of the details of just generating numbers to arrive at the number, then we tend to obscure that insight. And this is one example of it. So exploring those kind of phenomena. That wasn't a question, I know.

LANGMUIR: Victor.

PALCIAUSKAS: I would like to just pick a slight bone with you on your first conclusion, if you could put it up.

SEVOUGIAN: The 10,000 year?

PALCIAUSKAS: Not the 10,000. Skip the 10,000. The 1 million year is much more interesting, I think. It's at least much clearer. Everything is sort of simplified in your horizon. The two most important parameters that you have listed are dilution, which you emphasize very strongly and John Kessler did yesterday, and the percolation flux. I'd like to just emphasize that really the percolation flux is much more important than the dilution, because the dilution -- it's a ratio of the percolation flux over the saturated velocity.

So it appears in both of those conclusions. In fact, in the correlations that you've shown, for example, it's interesting that you showed the peak dose over the 1 million-year horizon. If you extended the percolation flux horizontal axis to the same length as you had the saturated zone velocity, you'd have a much better correlation. Remember you showed the scatter plot? Well, basically, the scatter plot appears simply because the percolation flux is only shown over two orders of magnitude, while the saturated zone graph is over four.

So I think basically you're sort of implying that dilution ratio is important. It's really the percolation factor is the dominant one. I'm trying to get it properly stated. Do you agree with that or not?

SEVOUGIAN: Two orders of magnitude on the saturated zone velocity and two orders of magnitude on the unsaturated zone. It goes from 0.01 to 1.0.

PALCIAUSKAS: You showed the other one basically where it showed a scatter plot of the peak dose.

SEVOUGIAN: Yes.

PALCIAUSKAS: And four orders of magnitude there in the saturated.

SEVOUGIAN: Both of them go from here to here.

PALCIAUSKAS: But it sort of gives you a better fit to the data. If you extrapolated the percolation flux probably to the right, you would get a very -- is that right? Bob is shaking his head.

ANDREWS: I think Dave's other plot is probably more germane because the percolation flux controls which nuclide comes out as a function of time, which one is the dominant nuclide. If I change the nuclide that dominates from the left-hand part of this curve, technetium and iodine, generally, to the right-hand portion of this curve, neptunium always, I get very different doses.

So I have this transition between which nuclide controls. So, sure, if you just were plotting dilution, per se, it would be the ratio. What Dave has shown is infiltration rates, something that is hopefully quantifiable, and aqueous advective fluxes in the saturated zone, also something hopefully quantifiable. Dilution itself is probably difficult to quantify, per se, to observe, if you will, to measure.

SEVOUGIAN: You could include a larger range of dilution, too, if you assume sub-base mixing or dispersion all the way to Amargosa Valley.

LANGMUIR: I'd like to bring us back to this in the round table, but we're five minutes over at this point. So let's proceed with the next presentation. Bruce Crowe will talk to us about volcanism effects and consequences.

[Slide.]

CROWE: I'm going to switch to this viewgraph because it's a little more stable. But maybe for volcanism, that's like harmonic tremor. The volcanism task, over the years, has been the subject of lively debate, as certainly Clarence and Leon can testify to, and I've lived through it for many years.

One of the things we've been trying to do in fiscal year '95 is to try to bring it more into the performance assessment arena, try to bring a perspective to the problem. That is, to look at whether it's a really a serious issue or where it might rank among other issues as far as total system performance.

So what we've done in this year and that I'll be talking about is our efforts there. The data that I'll be presenting has trickled in over the last couple of weeks. Some of it came in to me by phone message Monday might. So I can't say I've interpreted it a lot, but what I'm going to try to do is just give you kind of the highlights of some of the things that we've done and how it might affect the PA.

[Slide.]

CROWE: If you turn to the PA perspective about volcanism, I think Abe very nicely summarized what we've known for a number of years. PA has been telling us, somewhat patiently, but persistently that volcanism is not an issue. What we've been trying to address is some areas of criticism that have been directed toward those previous studies, which really fall into two areas.

One is that the representation of the physical process of volcanism wasn't as appropriate as some people would like it to be. The second was there may not have been enough consideration of the subsurface geometry of volcanic events associated that, where you would get both an eruptive component and a subsurface component.

Although, as Abe pointed out, some of the studies did incorporate both of those factors.

[Slide.]

CROWE: So what I'll be talking about actually is things that are all feeding into the PA effects. What we did is, first, we did some simulation modeling, focusing on what we call a disruption ratio. Most of the work we've done in volcanism has been done with what we call volcanic hazards, or PVHA. That involves the occurrence rate and the probability of disruption of a given area, those areas largely being the repository and repository system. What we've done is some new simulations where we've tried to bring in the geometry that we presently know of the different layouts of the repository, as well as the volcanic events, and then feed actually that data into the simulation modeling, including both eruptive and subsurface effects.

And then using the RIP code and the YMP base case, we've basically been trying to look at just what happens. Not worrying so much about the actual numbers, but just what is the sensitivity, how significant are the releases that are produced.

At the end, I'll just say a few quick words about some dose/risk modeling that was done back in the early 1980s that Sandia did and what it tells a little bit about what it might mean for the NAS standards.

[Slide.]

CROWE: Very quickly, and I don't want to belabor this in the quick time that I have, is the simulation modeling. We basically extended the dike models work that Mike Sheridan and Peter Wallmann have done and largely tried to bring it into specific spatial and structural models that we have for volcanism, and I'll show you a little bit about what those are.

We took roughly about 32 models and condensed them down to seven and ran those seven sets of simulations. We used two time periods that are tied to volcanic cycles, roughly a 1 million years and 5 million years. We also tried to incorporate as much as we could of what we think the subsurface geometry of the salt centers are. Basically, they're fed by dikes with dimensions. We factored all three of these into seven cases, the two age periods, and then different setups for the dikes and used the FRACMAN code for our simulation modeling.

[Slide.]

CROWE: And this viewgraph shows you basically -- it's a synthesis of the seven models. I actually included each sub-model in your handouts, but I'm not going to go over each one of those. What we try to do is we look at these as what we call source zones, where we then basically assume a random distribution in events and then use FRACMAN to track where those events would go.

This is basically what a run would look like. This happens to be what we call the Yucca Mountain region. We're running 100 simulations per setup, with over 300 runs done, about 100 realizations, with 10,000 iterations per realization. This is what it typically looks like. In FRACMAN, we designed an area of interest of about two kilometers thick and we included two areas here. This internal purple area includes both the low temperature and high temperature dimensions of the repository. Surrounding it, we have a 2.5 kilometer standoff zone that was identified by other work, Greg Valentine's work, as being a minimum distance; that if you stood away from that distance, you really could not see any effects of volcanism.

In other words, if a dike penetrated outside of that area, as far as all the modeling showed, you'd virtually have no effect. That includes both coupled and primary effects. So this is what we did with FRACMAN, though, is we'd run these with differing thicknesses, dimensions, orientations, and then FRACMAN would track the penetrations of the repository system and the repository and then also track the repository areas, and then we'd feed that data into the simulations for the RIP model.

[Slide.]

CROWE: Quickly, I'll just show you a little bit of results. The disruption ratio followed pretty much what we have seen in other calculations. The only interesting things are two things. One is that of those sub-zones that I showed you, five of them do not include the repository site or Yucca Mountain proper and two of them do. And it ends up, when you run the simulations, that the models that include it give you higher values of the disruption ratio here. These are all set up for the low temperature repository. Whereas the other models showed different ranges, with the highest being the quaternary pull-apart, which is the closest structural zone to Yucca Mountain.

Then in the lower figure, all I've done is looked at the same penetrations for the repository system. The repository system was about 51 square kilometers. So it's a little bit more than an order of magnitude larger than the repository. So all the results are just downshifted by a little more than an order of magnitude.

The interesting thing that we saw is when we translated that to the probability of disruption, which is what we call E-1 given E-2, which means the recurrence rate times the likelihood of disruption, what we found is that the numbers were a bit smaller than what we had previously calculated. Most of our numbers have been around 1 to 2 times 10 to the minus 8. And what we found when we translated the simulations, where we tried to bring in the physical reality of what a dike and a dike system looks like, our larger cases were right around 1 times 10 to the minus 8, with some of the numbers shifted down even into the 10 to the minus 10 range.

Those are lower numbers than we've been seeing in all of our previous calculations. So bringing the geometry in seems to suggest that the models we've been using have been conservative. If anything, we would argue that the numbers may be smaller than we've been proposing. Again, you see the same thing on the repository system.

[Slide.]

CROWE: Then what we set up in the RIP code -- and this is work that Golder Associates did with us under contract -- was the logic of trying to feed this data into the release models. So we would take the rate of intersection for the individual seven structural and spatial models and we first asked the question of whether we intersect the repository. If we didn't, then all that would be looked at would be some minor changes in retardation in the far field.

If we did intersect the repository, we had three different options. One was to erupt through it, with a variety of effects. The other was to have no eruption, but penetrate through the repository, but not erupt, or penetrate to some depth below it. And a number of us feel like this is not a realistic model physically, but nonetheless, we wanted to run it just to look at sensitivity. It's been proposed by other people and so we included it in these models.

You end up actually making the probability of an event higher because you're adding two events. We basically just assume that the probability of occurring either above or below was equal to the probability of occurring through it. So in a sense, we doubled the probabilities when we generated these things.

[Slide.]

CROWE: Then, just really quickly, I just wanted to show you some of the parameters. With the E-1 recurrence rate, as I mentioned, we feed the disruption probability and then things that we look at are things like different dike lengths and we simulated these as uniform, triangular and log-normal distributions, because that kind of gave you a feel of the value of information. The uniform being the least amount of information, the log-normal being the largest amount.

We're trying to look at whether or not having more information has much sensitivity. One of the key things that we looked at was the dike length and the repository, since that determines how much is affected. What we assumed was for the subsurface effects, based on, again, Greg Valentine's work, is that for the dike in 60 meters, so 30 meters on each side, we would have complete corrosion of the waste package. So you have complete virtually instantaneous failure of the waste package, which is conservative. But nonetheless, we were looking at results from that.

We also added in an eruption criteria, which basically says because we had height as a factor, we wanted to have some minimum length in order to allow an eruption to occur, and we chose 500 meters. We also looked at -- if you look at how the salts erupt in the field, only a certain component of the dike actually feeds the part of the material that erupts to the surface. So we chose a minimum ratio that the dike had to be at least 0.25 of its total length in order to allow an eruption. So this really was a trigger of whether the model would go to the eruption versus subsurface effects only.

[Slide.]

CROWE: Again, we tracked the number of waste packages disrupted or corroded. We also tracked the volume of material erupted using some representative volume curves. Again, those were done as uniform, triangular and normals. We looked at dike length. Then we have some new data that's new to this work, again, from Greg Valentine's work, where he has been doing a lot of work at looking at what we call lithic fragments. Now, this would be rock that surrounds the dike that's intruded. In other words, it's material being brought up from depth by the magma ascending.

And Greg has done a lot of work in the Colorado plateau area, where he can look at how much material is brought up from depth using a stratigraphy. And what he has is a volume, a lithic volume per meter of dike. That's a new number that we've been plugging in. We treated it in the model as a truncated normal distribution with these values. It is different from the models that we've used before. In the past, what we've used is a linear erosion model where you assume that it just uniformly erodes from the repository depth to the surface.

In fact, what we see when we plug in these is you get much, much smaller volumes of material coming from depth.

[Slide.]

CROWE: Finally, we also looked a little bit at retardation effects. We did not do much in the unsaturated zone because retardation -- chemical retardation, in particular, is not a major process. We looked at it mostly in the saturated zone. And the way we set up the runs, just for convenience, we didn't allow more than one event to occur per realization. As you go out to longer time frames, that can have an effect.

[Slide.]

CROWE: Okay. So what are the results? And these are kind of hot off the press. Again, what we're trying to do with this is we're not trying to give you individual numbers. But what we took as the base case that we got from Bob Andrews' group and then Golder ran the same -- their RIP code, a slightly modified code. Then what we looked at is the base case with no volcanic events and then the lower curve is the base case with volcanic events.

What you basically see is because the occurrence probably is pretty low, you're really only affecting the tail of your distribution, the shapes. Until you get down into the lower probability ranges, you really don't see any modification at all.

This is running only from the -- this is waste package releases. So this is summing the sub-model of waste package releases.

[Slide.]

CROWE: The second one that we also tracked is releases to the accessible environment. Again, we used the same definition as you've been hearing, the five kilometer standoff. Again, the first curve here -- I have a nice little wrinkle here, which is a new curve. The first curve here is without any events and, again, with events and all you see is a little tiny shifting. In fact, you really can't see it until you get out into the low probability tail. Again, this does seem to reinforce what the PA people have been telling us and we've been trying to ignore for a fair number of years -- that you just cannot get major effects. And this includes both the subsurface effect and the eruptive effect. So that's where we're at with that.

[Slide.]

CROWE: Let me just make a few comments on some work done by Sandia in the 1982-1983 time frame. Stan Logan was a consultant.

ALLEN: Excuse me. Bruce, can I interrupt you a moment on this last slide?

CROWE: Sure. Back to the releases?

ALLEN: What's the meaning of postcaldera cycle?

CROWE: That's just a time interval. That's the 5 million-year and younger interval. It relates to the basalt episodes that we classified out. It goes back to the spatial models that I showed. I had two time intervals. The 1 million is the quaternary and the 5 million is what we call postcaldera.

ALLEN: But the cycle has no particular connotation.

CROWE: No. We don't think it's coming back or anything. Okay. What was done back in '82, and Stan Logan was a consultant at this time and did most of the calculations and I believe he used an EPA code, was they actually carried out eruptive releases, mostly eruptive. They looked at thermal effects, but didn't really carry it out into the subsurface releases.

But they did eruptive effects, where they did both cumulative releases and they did those calculations. They assumed a reference population that is identical to the present population of the Amargosa Valley and they carried out about inhalation and immersion, basically the eruptive or airborne component, and then also looked at non-airborne. What they concluded was that the airborne component was actually fairly small and came up with maximum doses of about 14 millirem per year. But the non-airborne, under some worst cases, and their worst cases were where they set up individual doses, where somebody either lived on a cinder cone that erupted, contaminated material, or built their homes out of it, and they got some releases as high as four rems per year. Again, that is just for individuals. So that, of course, was individual dose.

That report is actually fairly interesting because a lot of the parameters they use are very similar to what's been suggested in the NAS report and it's actually worth looking at for that historical perspective almost.

In terms of what the NAS standards are, I think there's a couple of things that are important for volcanism. First of all, if you go out for longer time frames, like a few hundred thousand years, roughly, the recurrence rate for volcanism is about an event every 300,000 years, the average return rate. So if you go out beyond that, really volcanism shifts in to become an expected event.

What becomes important then is what we call the standoff or the disruption ratio, and that's about a factor of 3 in a 1,000 standoff. So what it says is you would expect an event and what becomes important is where that event occurs and how it might affect the repository.

ALLEN: Pardon me. What is standoff?

CROWE: That would be just the disruption ratio. That means where the event occurs relative to the repository. So what I meant is you're protected -- the ratio of intersecting the repository is about 3 in a 1,000, and that would remain consistent no matter what the time frame was. So that really is your only reduction in the event probability is the disruption ratio.

REITER: Is the three to four, is that the number of events in the region of the repository?

CROWE: That's right.

REITER: And only 3/1,000th of those would affect the repository.

CROWE: Yes. Those are mean recurrence times and mean disruption ratios. So that would be roughly -- that's a better way of phrasing it. Standoff is kind of a different term that I made up, I think.

ALLEN: But this is some sort of an average for all of these different areas you showed us.

CROWE: Yes, exactly. Correct.

ALLEN: And if you chose a different area, you could come up with quite a different number.

CROWE: You would. In fact, for the models that don't intersect the repository, if you take the dike geometry models, your number would probably be quite a bit lower than that.

What we have to comment on where we have done these dose calculations way back in the '80s is that they were probably pretty conservative. We assumed some pretty long dike lengths. We assumed large volumes.

And if you go to longer time frames, there's two factors that could affect your calculations. One is that it does appear that volcanism is waning in the same sort of tectonic setting and we think we're seeing waning tectonics. You might want to actually consider that in your calculations. We do not now do that. We assume steady state.

The second thing is that as part of the Golder work, they have a specialist in spatial statistics who looked at the distribution data for the last 9 million years for the volcanics and he came up with what he feels is a model that you've been seeing a southwesterly drift with time. In 10,000 years, it's not important. As you go out to increments of hundreds of thousands of years, that might be something that would be an important effect on the calculations. Roughly, what it says is there appears to be some evidence that volcanic events are moving away from the Yucca Mountain site.

The second thing that I have to point out, and this is really important, is that what we have chosen is this lithic fragment analog to do our eruptive releases. That just says that these fairly low density fragments are good trackers of how waste might behave. The reality is if we had to choose a physical model, it looks like it would be very difficult for a magma moving in a narrow dike a meter or two wide to really carry waste out.

We chose to use the analog because the recurrence probability was so low to start with, we just looked at it as a bounding calculation. As you go out longer in time, what you really would want to look more at is your physical model, how realistic it is. I'd have to comment that I think our eruptive models are probably very overly conservative because we don't think that you're going to easily remove it. What we assume is that the whole waste package is fragmented and distributed with the volcanic events.

It also turns out that where it's erupted becomes important. What they found is the airborne component is not that significant. You actually get a dilution effect with large dispersal. But what becomes more important is what's called the scoria-fall sheet. This is material that doesn't form a scoria cone, but it falls out within a few kilometers of material. That rapidly reworks into the environment and there are a variety of ways that that can get into the biological food chain and various things like that.

But one thing I'd have to emphasize is you have to be a little bit careful with applying some of the past studies that have been done because we used some pretty conservative assumptions just to really bound how you would get a dose model.

[Slide.]

CROWE: So to summarize, basically, what we found is that your choice of spatial or structural models is more important than we originally thought. Particularly, the dike lengths become critically important and we've got some new data that suggests that maybe the shorter dike lengths are more appropriate, in which case some of our calculations have been a little bit conservative.

But no matter how you run this, your probability of disruption is quite low. Our maximum disruption probabilities were about 3 times 10 to the minus 8, whereas our 50 percentile values were somewhere in the range of one to two under our previous studies. So we think the probability would downshift a little bit. If you go to the shorter dike lengths than we used, it would shift even more.

So you end up with volcanism being a low probability event and probably low consequences under a 10,000-year cumulative release. It's difficult to make it into a major event.

Then, finally, what we think we see is that the secondary effects would probably dominate a dose model if you do not accept the fact that waste can be easily transported to the surface. That would be a thorny decision because it would certainly be actively debated. There would be no question that some people would propose that that's an easy thing to do.

But what we have concluded from looking at this data and looking at the sensitivity of the changes in release curves versus the uncertainty of the release curves, which you've been hearing from the PA models, is that on that basis, volcanism would not be rated as a high priority issue for Yucca Mountain.

LANGMUIR: Thank you, Bruce. Questions. Clarence Allen.

ALLEN: Bruce, you say that it's not a high priority issue. Other people apparently would disagree.

CROWE: Yes.

ALLEN: Can you tell us what the major areas of disagreement are right now that might affect these results?

CROWE: Let me caveat that, saying there's a low probability, a low priority issue. What we would say is if you take the realization of the performance of a repository for either the 10,000 or possibly in dose periods and how we've integrated the volcanic model into that performance, that way of assessing performance, it would be a low priority issue.

You can certainly quarrel with how we are integrating the parameters into that and how well RIP represents reality. So that's an important caveat.

The real main issue of contention in volcanism is a number. We've debated at length, as certainly you know, over the recurrence rate and basically the PVHA side of it. What I have to comment is while there have been a lot of active debates, the actual difference in numbers, if you translate them into the values, are pretty small, to where I don't think that the occurrence side of it is really a major issue. At least that's my view, but you will hear differences to that, obviously.

Probably the most telling comment, I think, is Chuck Connor's calculation where he would argue, based on his non-homogenous models, that Yucca Mountain is located in a probability gradient and because of that gradient, there's a lot of sensitivity in where the actual -- where you might choose that probability value, depending on whether you chose a mid point or a different position of the tail. That's probably the major sensitivity.

We really haven't entered into the release side of that much, but arguments like how explosive an event would be, that sort of thing. The real interesting thing, in our mind, is that if you get a more explosive event, you end up diluting the releases. Under a dose standard that's -- all we do under the cumulative standards, we just sum what releases come to the surface. Under a dose standard, we'd actually get a reduction in a component that's originally dispersed.

But I do have to repeat that we really have to kind of re-look at some things if we go to a pure dose/risk based model. I think the most important thing would be to look at your actual physical interactions with the dike in a repository, and those are areas of a lot of uncertainty.

ALLEN: Thank you. If we do have a volcano there, you'd rather have it look like Pinatubo.

CROWE: In some ways, that's correct. The more you dilute it, it would end up being just a very small component of background by the time you dispersed.

LANGMUIR: Pat Domenico.

DOMENICO: Can I see the last slide, Bruce?

CROWE: The conclusion slide?

DOMENICO: The conclusions, yes. The last bullet. I hate to bring this out, but it's late in day. Your secondary effects. Corrosion, of course, that's negative, but if we believe everything that we're hearing here, the reduction of retardation is a positive effect because now the radionuclide will move faster and the dilution will be better.

So that's an unfavorable characteristic, along with slowly moving groundwater. They're both unfavorable, which is a little bit contrary to everything I've ever learned. But it's late in the day. I thought I'd throw it out.

CROWE: I would just accept that comment.

LANGMUIR: Leon Reiter? No. Any further questions?

[No response.]

LANGMUIR: Okay. Let's proceed. Thank you, Bruce. The next presentation is Bob Andrews, coming back with a summary and recommendations based upon the previous.

ANDREWS: You've heard a lot of total system performance assessment in 1995. In a way, maybe I should just open it up to questions, I have kind of that desire, and let's see if you heard what we wanted you to hear. But I thought maybe I'd better bulletize that and make clear some of the major assumptions, some of the major differences, some of the things we didn't do and their potential consequences, if you will, both from a positive side and from a negative side, and how this information or in what form this information feeds into the design and site characterization program.

As an introduction, you heard all of these things today, from item one through to the biosphere. As I think Jean pointed out yesterday, this work was done in parallel, in time anyway, to the waste isolation containment strategy, but, perhaps not too surprising, you see some of the same things floating to the top in both the strategy and the hypotheses characterized in that strategy and what is embedded into a total system performance assessment.

Ultimately, of course, testing and some of the other hypotheses that are placed in that strategy would be where we would hope to go.

[Slide.]

ANDREWS: I don't want to walk through all of the assumptions, because then you'll walk away with the idea of, well, there are so many assumptions that I can't trust the numbers that you gave me and the sensitivities that you performed.

LANGMUIR: Even if it's true.

ANDREWS: Which maybe is what you walk away with, I don't know. But let's just talk about them just to make sure everybody is clear where the assumptions are, and we can talk about the importance of them. Some of them we looked at, some of them we didn't.

The first two aspects we did look at. We looked at a difference of a conceptualization of how things perform in the drift, some calculations that Lawrence Livermore has performed, Tom Buscheck in particular, and some calculations that we have performed. Some differences in parameters, some differences in conceptualizations, clearly some differences in result.

Did it make a difference? Huge difference at 10,000 years. No one questioned it. Did it make a difference in a 1 million years? No. Factors of three we don't talk about. Factors of 50, 100, we talk about. These effects are neglected. Did it make a difference? I don't know. Dr. Langmuir pointed out some things that clearly need to be addressed in future iterations. There are some ongoing data collection programs, looking at, in particular, the thermo-chemical aspects of in-drift emplaced materials.

This fourth bullet probably should always be the top bullet of all of them -- how you distribute the flux that is there. What is the flux that is there and how you distribute that flux are key to virtually everybody's total system performance assessment. I don't care if you're talking about Sweden or Canada or Finland or the U.S., that's always number one.

Now, they happen to engineer some things in their near field environments to get away from that, but it ends up being the key issue. And, finally, how you distribute that flux in the drift itself becomes pretty significant, as you saw, even over the 1 million-year time period.

[Slide.]

ANDREWS: Some major assumptions in the degradation. The effects of cathodic protection on the initial point was clear, I think, in some of the presentations. So its effect on container degradation, its effect on substantially complete containment is substantial. The effect of cathodic protection on 1 million years, not surprisingly, is somewhat minimal. Again, another factor of three.

Had we, just as an aside, made the most optimal assumptions of thermo-hydrology and cathodic protection and cladding and a few other things, there would have been no releases in 1 million years. Just to point that out. We weren't looking at the optimums. We were looking at kind of ranges based on current expectations, if you will.

An important point here, I think Joon brought it out, but it might have been lost on some people. Tom Pigford, in a number of presentations that we made both to NAS and to Tom personally, said, well, you know, that first pit through is not the package disappearing. So we said, yes, you know, you're right, that first pit through is not the package disappearing. Let's include distribution of pits on that package as a function of time and directly incorporate it. Does it make a difference? Well, probably not too much. We didn't show that, but it adds some representativeness, if you will, to the overall package degradation and, therefore, release.

[Slide.]

ANDREWS: Cladding effects. We looked at some sensitivity there. It does have an impact on the peak release from the EBS. Pretty important assumption. How you, in your mind, conceptualize water coming into contact with the cladding or holes in the cladding and, therefore, with the waste form itself. We assumed that the waste form surface was covered, once the package had failed and once the cladding had failed, with a very thin film of water. It doesn't take much of a film of water to alter the fuel. In fact, the Argonne tests, that I think the board has heard about, have fuel altering in essentially humid air environments. But you still need an aqueous phase inside the package. That's an assumption.

We looked at alternate forms of the release of the gaseous radionuclides or those nuclides that most consider in gaseous form, iodine, chlorine and carbon-14. We didn't look at any colloidal effects in this particular TSPA. Perhaps we should quote Dr. Langmuir on that one and say we don't need to consider it, but probably there's other people who might feel differently than Dr. Langmuir.

[Slide.]

ANDREWS: In the geosphere, I think Dave has pointed out the importance, even over some long time periods, of percolation flux distribution. It controls, once again, the nuclide that comes out. It controls it because some nuclides are slightly sorbing. Neptunium does slightly sorb. It sorbs more heavily on the zeolitic components of the Calico Hills than it does in other rocks, but there is some sorption there. And for low enough fluxes, you can keep the neptunium, even in a 1 million-year time frame, within the unsaturated zone.

Is that true in 2 million years or 10 million years? I don't know. Probably not. But in a 1 million years, it is. I think the question that somebody asked the NAS yesterday, the NAS panel, was a very germane question -- why stop at 1 million. Maybe they can answer that one.

ALLEN: It's not been answered yet.

ANDREWS: That's probably not there. To get attacked a little bit. We talked about fluxes in the saturated zone. I think Dave used the word Darcy velocity. He probably should have used the word Darcy flux, to not confuse you between the amount of water moving through the saturated zone versus any retardation that may be occurring in the saturated zone. There's clearly some minimal amount of retardation in the saturated zone, which doesn't buy you much because the velocities in the saturated zones are rapid enough, at least under most conceptualizations.

The important thing here, and we did talk about this a little bit, the transverse dispersive mixing in the saturated zone. We confined the releases to the width of the repository. If one had some confidence in transverse dispersivities, you would say, well, there's some transverse spreading, if you will, of the plume greater than that width, which tends to lower the peak.

If my well is in the middle of that peak, it doesn't buy much in five kilometers. If my well is 30 kilometers away, it buys a lot. Backwards of 100. When I talk a lot, I'm talking about hundreds or more.

[Slide.]

ANDREWS: Biosphere. All of these doses that were presented, the peak doses, are essentially Appendix D of the NAS recommendations; i.e., the minority view of how to do this, not the majority view, which is looking at a critical group versus defining a critical group and then trying to look at concentrations in the vicinity of that critical group and, therefore, the resultant doses associated with average individuals of that critical group.

Does that make a difference? Yes. It makes a very big difference. It depends on how one defines that critical group. I think the NAS recommendations were that EPA, in its rulemaking, should define that critical group. But if one defined that critical group currently, based on current demography, and I think John Kessler at EPRI, they've gone to a lot of effort trying to look at demography and critical groups, one might say this has a factor of 100 to 1,000 on the peak dose to an average individual. Critical group versus maximally exposed individual.

There are some assumptions there regarding -- I need to hurry up here.

[Slide.]

ANDREWS: The major differences between this iteration of TSPA and the previous iteration of TSPA are many and let me just highlight a few. The potential effect of capillary barrier being looked at, how handle corrosion of the packages, the effects of cathodic protection are all different. More representative, we believe.

[Slide.]

ANDREWS: The EBS releases. Again, the capillary barrier effect is the pretty big one there and conceptual model of transport in the EBS is different. In the geosphere, how we incorporated fracture and matrix velocities, if you will, is different for our case. To be fair here, Mike Wilson, who is also in the audience, the TSPA they performed did include fracture/matrix interaction, did look at, in fact, sensitivity to matrix diffusion in TSPA-1993. So, in fact, it's probably not a difference in toto from TSPA-93. And the dose conversion factor is slightly different.

[Slide.]

ANDREWS: Okay. Let's look at some of the -- start with the non-conservative things. What could make the performance worse than what we've already presented here? Three main things.

One, just because of the sensitivity of the total system performance to the average percolation flux, if one increased that average percolation flux, one would increase dose and, therefore, increase risk. And I think there was some alluding to some inferences and modeling done, very recent, based on some observations, to try to look at fracture-initiated surficial infiltration rates. It's not, of course, what we're totally interested in, but somehow that surficial infiltration rate, if it is correct, and I think there's a lot of verification required there, has to be redistributed at depth. How is it redistributed? Is that number correct? How is it redistributed?

The big issue here, and I want to underscore that, the dose conversion factor. There is some uncertainty on what those conversion factors use. It depends on the biosphere that you assume and how you think that water moved from the aquifer through the well to that individual, through what pathways it moves, and, in fact, the actual dose conversion factor itself is uncertain. You can find one from ICRP-X or ICRP-Y. They are, in fact, different numbers. And some of them are higher for some of the key nuclides, like neptunium.

If there were colloidal transport, it would have some impact. Some nuclides might be different and there was no retardation of those columns. That's probably not as a big a factor and, as we've already heard, not even an issue.

[Slide.]

ANDREWS: What are the conservative assumptions? So what are the things that were given better performance than what you've seen for the last six hours? Some of these things we have done sensitivity studies, we have actually seen the impact, and in other cases not. Most of the first four you have seen.

We saw that if it's low percolation flux distributions, you become driven by things like iodine, technetium and non-sorbing nuclides. Those have lower peak doses in general than do the neptunium, a factor of 30, roughly.

The gaseous releases from the EBS. If you can assure yourselves that you have no gaseous release from the EBS, then things like iodine, in particular, would only be released in aqueous phase. And if you then combine that with this one, i.e., can design it in such a way such that you assure yourselves of only having diffusive releases, then you essentially have -- as Dave pointed out, the curves are off the map. They wouldn't even be plotted anymore if you forced yourself into diffusive releases from the EBS, all of the nuclides.

Cathodic protection we did look at and the cladding we also looked at. Essentially reducing the available inventory there is for release. If you only fail a certain percentage of the packages and within those certain percentage of packages, you have a certain percentage that the cladding remains intact for a very long time, you have a very beneficial effect.

And this last one I've already talked about. Going from the fence, where I have some mixing, to further down gradient, where I have a lot more mixing and a lot more combining of groundwaters, has a big effect on concentrations and doses. Something about dilution. I guess that's how that equation goes.

CANTLON: Solution dilution.

ANDREWS: That's right. I didn't want to say it, but I'm glad somebody else did.

[Slide.]

ANDREWS: What are the limitations? So where do we have our comfort level and where do we feel somewhat lacking? I want to point out that although we tried to start with basic process models, in some cases, those process models haven't been validated, if you use that word in a very general term, in terms of their comparison to observations. In some cases, those process models are still undergoing development and revision.

One good example of that is the recently completed Los Alamos process level model on transport through the unsaturated zone. It was just done and released at the end of last fiscal year. So when we back off from that, we have some -- I don't want to say inconsistency, but there's not the strong tie you would like to have between the process level understanding and, therefore, the results in the total system analysis.

In the case of both the site program and the engineering program, I think Joon has pointed out sort of the assumptions. The data collection program for some of the materials properties is just now starting. So we have some uncertainty on the process model for corrosion and pitting corrosion, in particular, the corrosion-resistant materials.

The third bullet, as somebody said yesterday, one of the major emphases of the site program next year, in fact, is this third bullet. It's to synthesize that information that is available for the different processes that we have identified to help in substantiation of them for future total system performance assessment.

Clearly, once that process is done, the next one would be key. That is testing how representative are your fractions back to those process level models and their understandings.

But the fifth bullet always will be there, and that is residual uncertainty will remain even after that process occurs.

[Slide.]

ANDREWS: So what are our suggestions, if you will? Kind of similar to Jean's last slide from the waste isolation strategy. What are the investigations required to enhance the realism or representativeness of those predictions that we think we'll have to make?

First and foremost is probably through the ESF and ESF mapping and age dating of water observed in the ESF or in the rocks in the ESF, to have some estimate or confirmation that the percolation fluxes at repository depth are, indeed, small. That's clearly number one.

We saw the importance of the thermo-hydrology in the backfill. Clearly, both of these two relate to that. Some of it's related to the properties of the backfill material, some of it's related to the prediction of how the near field environment behaves over time.

But the cathodic protection effect is still a very positive effect and confirmation of that, with direct observation and testing, analog studies, et cetera. It's very important.

The stability or instability, I probably should say there, if you look on the positive side of colloids, should be confirmed to assure ourselves that the projections by Dr. Langmuir, in fact, are the case.

We've talked at neptunium solubility, if that is the peak dose contributor. Perhaps we are overly conservative in neptunium solubility and better defining that would be useful.

Within the saturated zone, especially as we look at doses to critical or average members of critical groups, the amount of mixing and dispersive effects likely there. Anything that spreads things out or reduces peak concentration is going to reduce peak doses. So better quantifying this particular phenomena is very important.

And, finally, of course, and I think the NAS makes this recommendation and I think DOE has made it, that some group of people, perhaps in rulemaking, I don't know what, has to define the representative biosphere, so that we're all working with that same biosphere. And the biosphere is not something that we're -- that really is a random variable over a 1 million time period.

So with that, I'll stop and entertain questions or we can save questions for the round table.

LANGMUIR: Thank you, Bob. Yes. We have time for questions. John Cantlon.

CANTLON: Cantlon, Board. A couple of questions. In your look of this last slide that you have up there, investigations required, I guess I'm surprised you don't have climatic change as one of the elements that you need to look at there.

ANDREWS: Well, maybe we -- that's probably a good idea. Maybe we feel we have a reasonable handle on expected ranges.

CANTLON: But if you're going to double or quadruple the evapo-transpiration process, then your whole number one, which you list as your most important variable --

ANDREWS: This one would change.

CANTLON: Yes.

LANGMUIR: But isn't that included in your range of infiltration fluxes that you've proposed to worry about, in effect?

ANDREWS: Well, one could argue that with significant climate changes, you're outside the range of the percolation flux that we have in there now.

LANGMUIR: Unless you're in Alex Flint's 22 millimeters per year.

CANTLON: And it also hits your bottom bullet, too, because the nature of the representative biosphere is very, very different in pluvial than it is now.

ANDREWS: That's true.

CANTLON: The other one is more in the nature of a question. That is, I guess nobody has quite explained, or maybe I wasn't listening, one or the other, the nature of the dispersion or mixing that you have in the saturated zone. You mentioned that you haven't yet looked at the width beyond the footprint, that you're looking at the depth of the well as the variable.

But is it a presumption that you have uniform mixing in that block of water and what's the basis for that assumption?

ANDREWS: It's not an assumption that there would be uniform mixing in that block of water or within that transport path. But as the well samples that transport path, that that well sees effectively uniform mixing.

CANTLON: It's going to have import.

ANDREWS: It's pulling in water --

CANTLON: And mixing it.

ANDREWS: -- over not the entire width of the repository. You know, it's a farmer, so he's not into that big of a stretch. But it is slotted only over that first 50 meters. If I have him slotted over 500 meters, it's a dilution that's ten times greater. So how I define the well, if you will, becomes somewhat important.

CANTLON: The other question. One of the people working in, I think, one of the counties measured an up-welling of water from the water table as you perforate the seal on the top of the aquifer. Is there any thinking about the mechanics of what that might do in terms of the mobility of the radionuclides down the system? If you've got an upward thing so that every fracture in contact with that system essentially bleeds out the upper layer of water and, therefore, is a potential filter.

ANDREWS: I have a couple comments. One has to be very careful about interpreting small-scale vertical hydraulic gradients from a single point, because a number of investigations have shown, in both the reality and in models, that small-scale variability in aquifer property can give you apparent conversions of flow direction, even though they're not real in nature. They're just apparent at that point. So I would take caution on over-interpreting those fluxes.

If one had upward-oriented fluxes, then you would say, well, I have an additional source of mixing, additional dilution, if you will, because I have more water in this system than what we currently assume. Clearly, the mixing -- and I agree with the general tenor of the question. The understanding of the saturated zone, and I mean regional saturated zone, not just the five-kilometer saturated zone, becomes pretty important to us with dose-based performance measures.

CANTLON: Getting back to the climate change thing. You would also postulate some kind of an increased flow from a climate change because the feed in the system is greater.

ANDREWS: The helps, too. It helps in reducing concentration.

LANGMUIR: Bob, one of the things that we've heard about recently, and we're glad to see it, is the role played by dilution in the saturated zone. I gather, at this point, it's a fairly simple model that we're looking at. What would you propose or what would anyone who is going to look at it propose to do to make it more realistic? I'm sure Pat has some opinions on how that might be done.

But if you're going to realistically address the dilution, and I presume some dispersion and all those effects in the saturated zone, how do you go about it? You're not going to be allowed any more wells. So you're looking at what you've got. Can you address that or can someone else address that?

ANDREWS: Let me try and then maybe there's somebody from the site program who wants to chime in. The USGS is generating, as we speak, regional flow models of the saturated zone and site scale flow models of the saturated zone based on the available information that they have to date. To the extent those models, flow systems now for the transport, those models are valid ones. We use them to help bound expected dilution volumes, looking at dilution now only, in the saturated zone.

With respect to dispersive effects, the C well test is either underway or soon to be underway. It's underway hydrologically. I'm not sure if it's underway transport-wise. Joon Lee maybe can answer. And at that scale, anyway, a few 100 square meters or 100 meters kind of distance between wells, we have some indication of dispersive effects at that scale.

The correlation between dispersive effects at 100-meter scale and dispersive effects are five kilometers or 30 kilometers is, as Pat would be happy to tell you, quite uncertain. But there's some testing at least at that smaller scale.

LANGMUIR: As someone whose field it's not, I'd be interested in Pat's thoughts on how much additional dilution we might expect to see in that sort of an aquifer system over five kilometers due to dispersion, not just mixing under the site.

DOMENICO: I don't think dispersion is as important as dilution.

LANGMUIR: How about dilution, further dilution?

DOMENICO: I don't think dispersion is as important. You can tell that from your sensitivity analysis. Your dilution is far more important than your dispersion. The only way you're going to get some handle on that is with the conservative tracers, I guess, but that's going to be done over -- what? What are you at, 100 meters, at best? So that's done over rather a small scale compared to five kilometers.

LANGMUIR: Can we expect to buy anything? I mean, this is a proposal for some additional work to be done. Can we expect to buy much from a more realistic assessment of the effect of the saturated zone on the doses than a guess of 10 to the 5th?

DOMENICO: It's not a guess. I guess that's a calculation.

LANGMUIR: Well, a calculation.

DOMENICO: Yes. That's a calculation with the relative velocities of what's coming in.

LANGMUIR: But apart from that, what are we likely to gain further?

DOMENICO: I don't know. That's why they pay you the big bucks, boy. Figure it out. I don't know. Like I said, most of this has been analyzed from conservative tracer studies. That's about all you can do, I think. And that's kind of a small-scale operation.

LANGMUIR: Does the GS have that data now? It simply hasn't been --

ANDREWS: On the dispersive stuff?

LANGMUIR: That would give you the insights.

ANDREWS: I think their original model is in a state of calibration right now. So I can't answer your question, to be honest with you.

DOMENICO: The point that all tracer studies of this scale that I've seen, the ones they've done at Hanford and everywhere else, never gave you that order of -- well, of course, they're over 50 meters, too. And they attribute that all to dispersion and they calculate dispersivity. But nature doesn't know whether it's dispersion or dilution. So you can calculate a fictitious dispersivity that's really dilutionisivity or whatever you want to call it.

But the ones I've seen, you don't get that kind, but that's a small scale, very small-scale project.

LANGMUIR: On your table of bullets, a couple of modifications of the verbiage would please me more, but maybe not you. We'll try them on you. Establish stability of colloids is the verbiage and I would suggest that we're talking about stability and mobility, because they may not be mobile. And, or course, the stability is highly related to whether there's backfill or not, right? And the nature of the backfill.

And then in neptunium, rather than solubility, I would suggest we ought to look at what are the maximum concentrations of neptunium we could expect, at values below solubility, probably, and why. If you get a drop of water sitting on the spent fuel, you might get to saturation of something of neptunium, but as soon as that drop leaves the waste, you no longer even have that control anymore.

Any further questions? Leon Reiter.

REITER: I have a question to you, Bob, or Jean or some of the people of the M&O team who look at the waste isolation strategy. I just have a question about a closer mapping between what you came up with and what they're suggesting. Let me give you my quick interpretation, and this may be wrong. Essentially, it seems to support at least two of the major assumptions there about the importance and significance of percolation flux, what they call seepage rates, and dilution.

However, when you look at the role of the engineered barrier, you both think it's important, but I detect a difference. I don't know if it's real or not. It seems to me that you get your largest impact from the assumption of a capillary barrier and they don't assume a capillary barrier. They assume that the presence of the backfill itself and the increased temperature and humidity are going to be important. Is that a correct thing? I don't know if that's right or not. And how important is that?

ANDREWS: I think that's a correct interpretation. And I think we show it can have a pretty significant impact. That latter point of the in-drift thermo-hydrology can have a big impact over the tens and even hundreds of thousands of years. When we ran it out to 1 million years, the impact was minimal, a factor of three.

Their strategy focuses on both time periods, if you will, the containment time period and an isolation time period. The containment time period is very strongly related to that thermo-hydrology. The isolation time period can be still affected by that thermo-hydrology. It does lower the saturations for longer and you can assure yourself of low advective flows, i.e., minimal drifts, and still have adequate performance, even over more than that, over 1 million years.

I don't think there's that much of a disconnect. Maybe Larry or someone.

REITER: Larry, why did you guys invoke the capillary barrier?

RICKERTSEN: We saw, as we mentioned yesterday, a number of effects. For example, one of the effects is that when you sprinkle water on backfill or on like a gravel, it tends to dry out quickly. It evaporates. It behaves differently than closed rock. It has big pore space and that allows evaporation.

That evaporation, according to the Conca results, seems to happen even under high humidity conditions. That's one effect. If that were true, that limits the amount of water that comes into the drifts that could contact the waste package. That's one effect.

Another effect is that it provides an insulator around the package, as we discussed, and increases the temperature. It keeps the relatively humidity down. That's another effect.

There's other effects, as well. Conca experiments also showed that it provides a diffusion barrier on its own. The diffusion barrier happens to be the fact that we showed that you have, under unsaturated conditions, at least this small amount of water, disconnected films, or whatever other effect, so that the diffusion coefficient goes way down.

Those all look like very promising things. The difference between what the strategy does and what the performance assessment does, the performance assessment is an assessment. It's a let's see what the situation is based on what we know now. The strategy is a plan. What's the most profitable direction to go to prove your case? It looked to us that those are very profitable things to explore and it seemed to be verified in the performance assessment that was done at the same time we were going on.

The other advantage is it looks like those things can be tested in a short period of time, which was a major focus of the strategy, as opposed to long-term effects. We're worried about the ability to characterize 25 kilometers away, the closest -- whether we can do that in a short period of time or not. So we focused on the things that looked most important to us, most profitable to us, and I think that what you saw in the performance assessment today is consistent with that.

REITER: Can you address the capillary barrier specifically?

RICKERTSEN: We didn't look at that. In debating that, there was a lot of discussion about whether you can maintain the dual material nature. Would the fines go down and cause that capillary barrier effect to not exist? So we wrote it out as if it weren't there. We're going to let people comment on it. We're at that stage where if somebody thinks that's very important and can justify that it will be there for a long time, maybe it comes back in. As I said, we're in the process of that review process right now.

LANGMUIR: I'm going to cut if off here. We're about 11 minutes over schedule. Thank all of you. We can come back to some important issues like backfill and its significance to TSPA in the round table.

The last presentation before -- well, Paul Davis actually is coming up after a break here in a minute. But at the moment, Eric Smistad is speaking to us with concluding remarks. We have lots of concluding remarks. We have several introductions and several concluding remarks here. So make sure we get the point.

SMISTAD: I will not be doing a wrap-up, per se, of TSPA-1995. I think Bob and company have done a good job of that. Instead, I have been asked to talk about the future a little bit. Given the state of the program, that was no small task. I was, however, able to eke out about five viewgraphs based on yesterday's discussion. I think that's probably five too many.

[Slide.]

SMISTAD: An outline for completeness sake here. I will walk through a time line of TSPA, talk a little bit about process models and their role in total system performance, a little bit about guidance, and then some concluding summary points.

[Slide.]

SMISTAD: This time line is really sort of what I call a modern era. I started it with the SCP in 1988 and went out to the big question mark of license application. As Abe Van Luik mentioned, we have done recently a series of DOE-sponsored performance assessments, two in '91, two in '93 and the one you've been hearing about today.

So, again, this is just not something that we've just started into. We've been doing these for several years now and we are indeed learning things as we go. Out here in the uncertain time frame, we are planning TSPAs now, have them on the books in 1998, and we'll see if that comes to fruition based on the budget determinations. We will obviously be doing TSPAs if and when a license application comes to be.

The reason I started with the SCP here in 1988 is because although the SCP was a good document at the time, we are not explicitly talking SCP in terms of all the studies and detail for the SCP. One reason is because we have learned things since 1988 and part of that learning experience has been a result of TSPA; not exclusively, but it has been a part of the learning experience.

So I kind of see it as a good thing that we're not necessarily following the SCP in that we have learned a substantial amount since that time frame.

[Slide.]

SMISTAD: A little bit about process level models, and I think Bob covered this pretty well. The process level models are going to be used for the bases of TSPA. It is the intent now of the program to concentrate, as you've heard on the synthesis and modeling efforts, to support TSPA. It is my belief that as these models are implemented into the TSPA, future TSPAs, the realism and confidence will increase and the instructiveness of the guidance that we provide will also increase.

[Slide.]

SMISTAD: This is just an abbreviated list of process level models. I don't have all the process level models in here that we're planning in the program. This is just an example of some of them that we're going to be producing in the next couple of years. This is not a priority list, either. So don't be alarmed if your favorite model is down at the bottom of the list.

I italicized UZ flow because this is the only model that we had available, process level model we had available from the site or design of this last iteration of TSPA. You heard Srikanta Mishra talk about the extraction and use of that model in the TSPA.

We did, as Bob mentioned, just recently receive to the project the unsaturated zone transport model. We obviously didn't have time to implement that into this TSPA and we will be reviewing this from a PA standpoint early this year.

[Slide.]

SMISTAD: In terms of guidance, each of the TSPAs, at least in the modern era that I've talked about, have contained a chapter on guidance, both to site and design. I think we found out, at least in the '91 work, that for the most part, the guidance that we were giving there was already being looked at in the site program. So there wasn't really any firm redirection out of that.

I think in '93 and in '95, we're providing a little more meaningful guidance to those programs to be looking at.

The recent project planning has utilized PA guidance to assist in prioritizing tasks. An example of this is the recent '96 planning exercise that we just went through where PA guidance was used to assist in prioritizing tasks.

We have just recently put out a document -- well, it's still in the DOE review loop right now, but put out a document on '95 PA guidance for '95 for site characterization and design. You heard Bob talk a little bit about the recommendations coming out of TSPA-95. We've got a piece of work out of Sandia prioritizing the climate studies from a PA perspective. We did an extensive review of the UZ flow model we received and provided those comments back to the site program and those will be incorporated in the next iteration of that model.

We did some seismic work, guidance there. We also produced a -- I guess I'll call it a short document, outlining what we would like to see, from a PA perspective, in the process level models that are being delivered to us in the PA program. And there is some other miscellaneous guidance that we've got contained in that document.

Just another word on guidance. In PA, we hear a lot about guidance, what kind of guidance are you providing back to the site and design programs. I'd like to say that guidance is an important part of what we do with total system performance, but it's not the primary thing. The primary thing that we're doing with the total system performance is to tell the program something about the safety of the site, a determination of the safety of the site.

I don't want that to get lost in all this provide guidance, provide guidance, provide guidance, although it is an important part of it.

[Slide.]

SMISTAD: In summary, TSPAs will continue to be produced. The process level model development and implementation is key, I think, to producing more realistic TSPAs. The TSPAs will continue to provide guidance and support major project decisions in the future.

Thank you.

LANGMUIR: Thanks, Eric. Eric is going to be sitting on the round-table panel, which will give us a chance to get at him then. What I'd like to do now is -- we have scheduled a 15-minute break. I'm assured by Paul Davis that he can get his visual aids together in about ten minutes. So let's take a break for ten minutes and come back and get closer to being on schedule.

[Recess.]

LANGMUIR: Our next speaker is Paul Davis. His topic is making use of performance assessment, lessons learned. Paul, it's yours.

DAVIS: Thank you very much for allowing me to give this presentation to the NWTRB.

[Slide.]

DAVIS: First of all, I apologize for not being able to see this in the back. I created these last night in my room and it was either these or my handwritten viewgraphs, and I know you can't read my handwriting. So it's a little bit better.

The topic of the talk was to be making use of performance assessments in the high level waste program and I was supposed to say something about lessons learned. I actually deleted it from your hard copy and then I have it here with a question mark, because I thought long and hard about what lessons I had learned doing this over some time frame. Probably the only lesson I've really truly learned was that if your boss comes to you and asks you to be the person that integrates between experimental programs and performance assessment, don't take that job.

[Slide.]

DAVIS: Presentation outline. Before I could talk about the uses of performance assessments, I actually had to define what I thought those uses are. So that's one topic I'll talk about briefly. Second, I'll briefly review what my current understanding is of the Yucca Mountain approach to using performance assessment, and that's been gleaned from past reviews for the ACNW, as well as the executive abstract, I guess, that was sent to us as part of this meeting.

Then I'll present some alternative approaches that were tried by the greater confinement disposal project at the Nevada test site and the WIPP program.

[Slide.]

DAVIS: Look briefly at what the results are here. I think this has a lot of indications on how you can actually use these results. We've seen these. I've created some of these that look exactly like this TSPA-95 result. I think we heard earlier from Abe, the PACE-90 results, the TSPA-92, 93 results, they all look like this. That's a key element. All except for one, I think, comply with the standard, and that one I think was a gas release from TSPA-93. Is that right, Abe? Something like that. And I'm assuming that because it doesn't show up in 95, that that has been discounted and successfully ruled out. I don't know if that's true, but I'm assuming that for this plot.

But they do look like this. Year after year after year, you get CCDFs that comply.

[Slide.]

DAVIS: Another point along the same lines is I actually took what you just heard summarized in the talk before last, dose results of TSPA-95. And what I've plotted here is the dark curve is the given value, given value, given CCDF of dose in the executive summary of the TSPA-95 result. Then there were statements made in the last that try to give you some insight as to what this value means relative to some state of knowledge of the program, and those were the list of conservatisms and non-conservatisms.

And statements were made in there that led you to believe, if you actually took those into account, these would be the bands you get. That is, these much larger bands of -- see if this pointer works here. Without the non-conservatisms, I increase the dose slightly, and, in fact, I think the number was given as 10 to the 2nd, 10 to the 3rd would be the kind of relative number that I would get for an increase. These were things like the possibility of higher infiltration, things like that that weren't in that base case.

Now, on the other side, the analysis was set to be extremely conservative. And so I get this kind of curve here if I took out all of the conservatisms, and that was like a factor of 10 to the 8th, I believe. It's a question to ponder now what does this mean in terms of what our state of knowledge is. Is the state of knowledge telling us that the real answer is in here or that the whole analysis doesn't have other things that people worry about in these all together? And I hope we can talk about that in the round table later.

[Slide.]

DAVIS: As far as, then, the remaining issue. First, the compliance issue, and I think Eric hit on that very good and that is the prime use of total system performance. Well, the remaining issue is why aren't we done. I think that's one of the critical issues. You have CCDF after CCDF that show compliance. You have no accurate dose assessments that show low doses. So I think this is a very fair question.

The data collection issues are several. First of all, which data should we collect? Second of all, where should we collect that data? This is a spatially variable problem. It's not that we go out and just collect a hydraulic conductivity or an infiltration value. Where should we put the data?

This is the hardest question of the program, I think. How do we know when we're done? I think those are the key issues that have to be addressed if you want to use total system performance to drive the program.

[Slide.]

DAVIS: As far as the compliance issue, why aren't we done? This I wrestled with a lot, coming up with a statement that made some sense and is somewhat defensible. The only reason I think that we're really not done is DOE is not ready to defend the results. I put other statements in here before I edited this one out this morning. I actually put the Pat Domenico statement, I think from yesterday, which is nobody believes the results, where he said these are model results and we can make the models do anything we want. And I'm a modeler. I know that's a fact, which proves I'm a good modeler.

But this is the case. We have not gone through a license hearing. We haven't gone through a process that gets feedback from the public or NRC on the results in a formal manner that's done and over with when you go through the process. It hasn't got there yet. So instead we have questions that have to be answered, and these are the statements that I've gleaned from the process.

This one is in the executive summary of TSPA-95. We're not done because we need to provide a more robust assessment. We've also heard, and actually we just heard from earlier that actually some validation work on the process models must be done before we're finished.

And, finally, of course, from the regulatory perspective, we say we haven't provided reasonable assurance.

[Slide.]

DAVIS: As far as the data collection issues, which data should be collected. The way this process works, as I understand it and it's just been added to my knowledge, it's just been added to today, that basically it was a best estimate of what you think or most probable estimate of what you thought the site would do, do that calculation with uncertainty, wherein uncertainty is really a propagation of model uncertainty, as well as stochastic parameter input in the form of probability density functions that are sampled from.

And then I did a post-audit on that. I'll call it a post-audit. The British did this extensively, post-audit after the PA results. And some people call it a sensitivity analysis. And I use that, as you've just heard, to identify what would be the key things that need to be addressed in the future. The knowledge that was added to is I didn't know a regression analysis had also been done, they just found that out, to identify what the sensitive parameters were. So that's the process. And I'm not saying there's anything critical about the process. I'm just trying to make sure I understand the process.

The key thing about the post-audit is it's testing things that aren't in the model, and that's usually the concerns that we end up with in the site characterization in the program are not the things that are in TSPA. It's the things that are not in there.

The next one is where should the data be collected. This one is complicated. It's not easily determined in this case. Bob has been so kind as to lend me his viewgraph. It's the process of tracking this back. I'm looking at answers over here and I'm looking at data collection over here. And in this case, in between, I actually have the model abstraction process. So I've really replaced the knowledge of spatial variability and those types of things with an abstract model and in this sense, as I understand it, a response service, actually, a metaphysical model. I think that's correct. And that's not a criticism in any way, shape or form.

That just says it makes it difficult now to go back from the concerns about peak dose to a location to drill a well. If not impossible, it makes it very difficult to do that.

Note that this is not universally done. WIPP actually doesn't do this. They don't have the abstraction process in this form. Their most detailed models of the site are the models they use for performance assessment in a fully probabilistic sense.

Yes, that's more expensive in terms of computing and effort, certainly it is, but that is the process they use. However, don't confuse the issue. They absolutely do model abstraction process, which as it at the first level. They absolutely do that like everyone else does.

How do we know when we're done as far as the TSPA-95? I don't know. I just proposed what I thought was going on, but I don't know and I'd certainly be willing to hear whatever the program says. I think this is it. I don't know. Expert judgment by the program staff of saying they have a degree of comfort with the results and the experiments and, ultimately, of course, that same degree of comfort by the NRC staff. I think that's the way this works.

From a program management standpoint, from defining things like critical paths, this doesn't leave you with a very comfortable feeling about where's the end and how long it will take to get to the end. It leads you think maybe it's more schedule-driven, that the end is 1998 or some magical date and whatever data we have by then we'll use. But it doesn't give you those things that a program manager would want to have.

[Slide.]

DAVIS: Now, I'll talk about another program that used a different approach and failed. So I'll tell you that first. GCD is a location that DOE has on the Nevada test site, in area five. It is actually the only site that's regulated by 40 CFR 191, that the waste is already buried. It contains large diameter bore holes, 12 foot in diameter, that go down some 150 feet, with the waste put at the bottom, put in with a net, no containers, no waste packages, simply that kind of waste, rubble. Then the backfill that was drilled out of the hole is put back on top of it.

That's the entire design.

Luckily it sits 700 feet above the water in an incredibly dry environment. And in alluvium, which is much easier to understand and predict than fractured rock, of course.

What happened in this approach, Sandia doing the performance assessment, developed a PA using conservative parameters and models and for undisturbed performance. So we're not talking about all the scenarios. We're really talking about for today's climate and today's conditions.

Then they went through an interesting process. They actually asked the experimentalists, the field geologists and hydrologists and others, to say don't tell me what's wrong about the models in terms of their representation of reality, but please tell me could it be worse. Is there anything in the parameter distributions, is there anything in the model assumptions that can be worse than what we've stuck in there?

The experimental group had all kinds of criticism, but none of it was that it was worse. In fact, all the criticism is that it was way too conservative. Didn't find a single distribution or a single assumption that they questioned from that point of view.

Well, that complied. Put that in the system. That complied for the undisturbed case and the undisturbed case, from the EPA point of view, answers the groundwater protection rule, which is meant to be done under those conditions. And the result, from a programmatic point of view, was funding continued for the experimental group to do site characterization for the undisturbed case.

[Slide.]

DAVIS: Now, on to the WIPP experience. WIPP was essentially going along with a program very similar to Yucca Mountain's, I would say, in terms of year after year producing CCDFs, and in their case, if you know WIPP, multiple and multiple CCDFs, always complying and always in the executive summary was a statement of caveats. We comply but we don't know enough. We comply, but we still have to do these experiments to confirm what's in the CCDF, basically what's in the models. And that doesn't lead you to a point that you can identify where you should go.

So we developed, myself and Walt Beyeler developed a method that DOE actually named the system prioritization method. It had several goals. The first was, as Eric pointed out, the most important one for PA is to demonstrate regulatory compliance. Then the second one was to identify the remaining activities needed to achieve compliance. This is not experiments. It is not experiments alone. It is the combination of experiments, changes to the engineered system in terms of waste package, backfill, the same sorts of issues that we're talking about here today. And in WIPP's case, they also have another key component that they can address, and that is changes to the waste acceptance criteria.

They have RCRA waste. They have mixes of junk, in a sense. A lot of their problems could go away if they had stricter requirements on that criteria for accepting waste. So it had a mix of things that if you did those, they may all lead you to compliance. One note is that here, when we say demonstrating compliance, we're not just talking about satisfying the quantitative criteria. We're trying to talk about providing confidence that you have done that, and I will talk about the process that was designed to attempt that.

[Slide.]

DAVIS: The basis for the SPM approach, which is philosophically different than the previous approach, is a stolen quote from George Box, which is "All models are wrong and some are useful." We're not at all attempting to say we're going to predict reality or that we're going to have the probable performance of the repository. That isn't the philosophy behind the SPM approach.

That goes on to lead you to say that validation gets defined as adequate, the models being adequate for purpose, contrary to that the models are an accurate representation of reality. Those are very different things.One example certainly would be that as we've seen, these codes have used a one-dimensional model of the unsaturated zone. If you could defend that that was the highest release that would occur is one-dimensional and if you go to two dimensions, you're going to get dispersion, you're going to get more mixing, and the one-dimensional model says that you comply, I think that's a valid model. I think it's adequate for purpose. I think it's absolutely invalid from a scientific point of view. So those are the two different kinds of concepts.

This is a little bit harder one. It's also saying that reasonable assurance is defined as no credible evidence that the site violates the regulatory criteria. That's closer to absolute proof than reasonable assurance. And I would entertain the discussion that says we can provide as much proof in this program as anybody does in a court of law for any other thing we do. I don't buy the argument that you provide less assurance for this program.

[Slide.]

DAVIS: What that does is change the meaning of CCDF and the meaning of the CCDF becomes we have no evidence that the answer is out here. All of the evidence indicates that the answer is somewhere here toward lower probabilities, lower EPA sums, and we don't care. That's the hard one.

Once we've got past that limit in the manner that we're proposing to get past that limit, it complies and we're not going to search the scientific process or continue the scientific process to the answer, the reality.

[Slide.]

DAVIS: As far as the actual process, the hardest step in the program, which sounds similar to what some of the bullets I've seen today were for this next year's effort for Yucca Mountain, which was to stop and assess the data. And that's what the program was asked to do, to begin with defensible model assumptions and data, as defined by, first, the experimentalists. This is not the modelers. It was a very different approach.

For years, the modelers actually set the assumptions and they got feedback and they talked to the experimentalists. But the experimentalists, in some sense or in some cases, opted out at the end by saying your models aren't realistic, your models aren't what I would model. They weren't part of that process in their day-to-day lives. So it started with the experimentalists actually defining and defending, more importantly, the assumptions and data.

It then went on to the project team. It didn't happen alone. It went them to performance assessment and the other players within the Sandia team and then within the DOE team, which included technical staff at DOE, as well as

WTAC, the technical assistance contractor, which I think at that time was SAIC, Batelle and others, who also then had feedback into this, defining and defending the assumptions. And then it went one major step further. It went to the public and it went to the regulators and it asked them. Here's what we think we can defend today, what do you think. In an open forum, in a very documented way.

And it was supposed to go to the regulators, too. That's an interesting one for lessons learned. The regulator, in some sense -- this is EPA -- opted out of the program. They liked it. They said they agreed with it. They said it was a very defensible way to plan your program. But they thought it showed the regulatory cards in their hand too soon, and that was the worry. It's a valid worry. I think there were ways around it, but it was a valid worry.

The next step then is to assess compliance based on those defensible models and assumptions. Then if you didn't comply and only if you didn't comply would you gather additional data to make the models what some people would term more realistic, but only to the degree necessary to demonstrate compliance.

[Slide.]

DAVIS: Now, what does defensible mean? This certainly became one of the key hot button issues of this process.

Well, this is what the experimentalists were told. This is their guidance, written guidance from the program. It said experimentalists are directed to define the least, not the most, the least conservative data and models that they would defend to a group of their peers, and this is the critical part that was different from the past, without relying on future work.

We had always had statements in the -- a classic one for WIPP was matrix diffusion. We believe matrix diffusion really plays a big role, but there's another experiment that has to be done to prove it. That's a classic case. So they had to either defend that, that we believe it does, and quit or not defend it and not include it in the models. That was the process.

Then the project team and the stakeholders were asked to critique these positions and not in the way that the GCD program had. The GCD program says tell me what's worse. This is not that at all. This said tell me either way. If we've proposed something too conservative, in your view, then give us the state of knowledge that would lead us to change that in this documented review process.

If, on the other hand, as some of the public did, we think that you're too optimistic and here's data from this site or here's logic or inference from this site you haven't included, that position is not defensible, then that was the new position of the program, if they agreed with that position.

Input was not just data. Input was data, information or simply logic. We didn't require the Attorney General, who was part of this, or the Assistant Attorney General of New Mexico that was part of this to come to the table with references for solubilities and say these are the new values. But we required the same thing we would have required out of the experimentalists -- logic, inference, information and/or data.

And then the review process was fully documented so that you could trace it all the way through. So that at least if the baseline changed, there would be the documented justification of why that baseline had changed.

[Slide.]

DAVIS: Another of the major hot button issues of this process was conservatism and what it means in this framework, because it's not the GCD framework and it's not the conservatisms in the sense that we heard earlier on TSPA-95. Well, what's the answer? There is none. There is none. And that's not believed at all by WIPP today, but there is no inherent conservatism in the process.

Where does it come from? Conservatism could arise, first of all, from a disconnect in your belief about safety and your knowledge about safety. I think one of my fundamental conclusions out of the process was those two things were not in the same universe. The belief of WIPP safety and the knowledge of WIPP safety lived in different worlds. The entire program believed WIPP is safe, but actually producing the knowledge that defended it was not in the same room.

So if you believe, for example, that matrix diffusion was the thing that worked as INTRAVAL had told you, as this is a real case, the INTRAVAL group, had said, well, wait, that could be channeling in fractures, what you've seen in those tracer tests may not really be matrix diffusion. The program says we can't disprove you. That's a viable option and we haven't disproved it. Well, you've got to go back to that then. So your state of knowledge is what they've said and then you have to defend that.

The next one is kind of a hard one. It's that you may have a state of knowledge that is more detailed, more complex than the models allow you to simulate. Therefore, you may have to back up to some conservative simpler model. You may be forced to do that.

What's the alternative? There really isn't one in any case. If we were doing this in Yucca Mountain, you would still model with the simpler model. It's how you build that model now, because we've said by definition, you can't build the complex one. That's why we have the problem.

[Slide.]

DAVIS: Another misleading idea that's coming up, and I still see it all the time, is that when you consider alternative conceptual models, it's somehow related to the notion of conservatism. For example, we had a classic case that was argued for years, I think, on the mechanism that control water moving into the salt. One side believed, and this is Dwight Deale of IT, believed that basically the liquid that you're seeing coming in now is a temporary response to actually the mechanical changes around the opening of the rooms.

And so as soon as that process was spent, you wouldn't see any more drips and seepage coming into the rooms. That was one side.

The other side believed, and this is Rick Bohime at Sandia, believed that, no, you're really seeing Darcy flow. And it's small, it's slight, it's hardly anything, but it's truly Darcy flow through the salt. This process said we don't care who's right or wrong. We will use both models, get a calculation and then pick the one with the highest release. That is absolutely not conservative, and the reason is either may be true.

We may identify an experiment that needs to be done that resolves the issue, and this was a hard one because I think the experiment they identified was like a seven-year experiment to be able to resolve it, but in the end, Rick's model, that happened to lead to higher releases, may be the truth. Therefore, it's not conservative in any sense of the word, and I'd like to try to continue to avoid that problem or that perception.

But the other thing that it did is put both those models in, run the calculations, and say do we care. It's a nice experiment, it's a nice difference, it's a major scientific difference of opinion, but does it matter. And if it didn't affect the compliance result, we're happy to disagree forever and go forward.

[Slide.]

DAVIS: Identification of remaining issues in the SPM process started with not a road map or not a list of every possible experiment and waste package and geology and everything that you could do on earth. It didn't start in that comprehensive of a manner. It started with the assumption that after 20 years, we better have defined fairly well what we know or what we need to do. It started with that assumption

Well, that was documented in the experimental program plan, which had some 75 experiments. We also had an engineered alternative study which had identified various engineered alternatives that were appropriate for WIPP.

Then we had a study on alternative waste acceptance criteria. So that was the starting point.

It wasn't like going to the geologists and saying put in every shopping list or put together a shopping list of every experiment you could ever do. It was not that.

This is a critical step. The experimentalists again were then asked to define the answers of the experiments that they had not yet performed. That's a critical point. And they have to say -- and they do it anyway. I would always argue this anyhow, that basically they are saying I'm going to measure this because it's important, because I expect to see this. That is the process. That's how they come to us for funding, believe me, and they have for years.

Now they have to define the likelihood of obtaining those results. That's a different process. That is meant to weed out between the idea of simple experiments that you know you'll get the answer. You may not know what the answer is, but you know you'll get an answer. The classic case is if I take a sample of groundwater and I send it to a lab and I get back how much calcium is in it. I have a high probability I'm going to get an answer back from that. I don't know what the calcium will be. That's the first step. I don't know what that will be. I have a high probability I'll get back the answer.

Kind of contrast that with the matrix diffusion experiment, which was the first one of its type in the world, with seven wells. It was trying to distinguish matrix diffusion from channeling. There's a less lower probability that I can get an answer out of that at all. So those have to be incorporated into this decision analysis.

Now the next one is actually now what. What's the relationship of that answer to performance assessment? It's not enough just to have an answer. Is that going to go into changing a model assumption? Is it going to go into affecting a scenario probability? Is it going to go into changing a distribution or a PDF for model input? That relationship has to be tied down, a priority.

And, finally, this is a program that we're trying to minimize cost and schedule. So they had to put in what was the cost of that and the schedule for completion of those results.

[Slide.]

DAVIS: Finally, what I'm referring to briefly here is, I'll try to explain a little bit more, is a performance-based decision analysis which used then to identify the set of activities that most efficiently maximized the likelihood of satisfying the quantitative criteria. What the hell does that mean? Good question.

We used performance assessment. We used explicit remuneration on performance assessment. We are talking millions of calculations of all possible combinations of activities and their outcomes to find, for the least cost and least time, which set of activities has a higher probability that will lead you to compliance. It was a achievable. That was the first iteration that we did. These calculations with these full-blown process models was achievable.

[Slide.]

DAVIS: The hardest slide. Status and results. From March 1994 to November '94, went through the entire public process of publishing those papers, reviewing the papers, sending them to the public, and then having DOE bless them as the official program technical baseline and presenting them to EPA.

In addition, we did this iteration to see -- to answer criticism that you could never do this many calculations and to work out the bugs of the decision logic and those things. So we did an iteration before then, which is based totally on an artificial baseline. It did turn out to be the baseline that the process was judged by, though.

Since then, after that time in November, the program changed the baseline that was presented to the public without public input. Second of all, they decoupled the process from compliance. And then, third, they used an alternative process for getting the elicitation. So they skipped the process of actually tying the result to performance assessment. They just guessed in the end answer for performance assessment.

Well, what's the result? They did go forward and they got an answer, but unfortunately the answer has no value. And it's the first step that kills you. It's the step of taking it out of compliance that kills you. The analogy that I could come up with is if I told Pat Domenico to go build me a groundwater model from March to November and then I said in November I'm really not sure I'm going to like the answer, so could you take out the constraint of mass balance, would you mind taking out gravity, and pulling the rug out from under it and then going forward with an answer, because really what happened was future work, future guesses got put into the baseline. So, therefore, you could not judge the value of the answer.

So the good part is it's an exercise that told you maybe what not to do next time if you wanted to follow such an exercise.

That's it. I'll leave it there and either answer questions now or at the round table.

LANGMUIR: Thank you, Paul. We have time for questions for now. Questions from the Board. One thing that kind of intrigued me was your pointing out that a valid model may be good enough to satisfy compliance, but be scientifically unacceptable. That bothers me, as a scientist, but I guess it makes sense.

I wonder if that hasn't been a problem all along with us in this program. So many scientists doing the subsystem models wanted to be satisfied scientifically and publish what they were doing in peer review journals, where other scientists would have to think they were acceptable before they're willing to hand them over to the DOE. I can see this is a major problem in a large program such as this and I guess it would require an education on the part of all of those involved in what was needed and where to stop.

And maybe there's two parts to what they're doing, but maybe the money won't take them as far as they want to go. Did you have these experiences with WIPP, these problems with the science engineer types within the program?

DAVIS: I think those problems exist today at WIPP, even with this process. There are still people that believe that the only way reasonable assurance would be achieved is if they have the confidence in their process model, regardless of the answer. I think those problems are going to go through the entire time.

LANGMUIR: More questions? Leon Reiter.

REITER: Paul, I don't know if you can answer this, but what was the rationale of the project of not pursuing this?

I don't quite understand that. Maybe you said that.

DAVIS: I can't answer it.

REITER: You can't answer it.

LANGMUIR: John Cantlon.

CANTLON: Since the decision was not to use it in the regulatory mode, where is the project relative to its licensure?

DAVIS: Did Wendell work stuff out? I think he was here. I think it's a question for him. I'll try to say it and maybe Chris, who is involved with the program, can correct me. The project went forward, because of the 1998 deadline, with a different baseline that was meant to comply. So there's two baselines. Well, actually, there's there or four, to be honest. But there was a baseline that was originally built for this process. That baseline was altered last November, beginning last November, to finish out this process. But in parallel, a different baseline has been built to demonstrate compliance.

LANGMUIR: If we don't have any further questions now, there will be a chance to participate in discussion with Paul at the round table. Why don't we proceed with getting ready for the round table then, all of those who find their names on the list there. And I think Wendell Wert, if he would -- he's not here. Okay.

So we'll adjourn for ten minutes and return for the round-table discussion.

[Recess.]

LANGMUIR: We have reached the round-table stage of our meeting on TSPA. As you're aware, the topic of the round table is the uses of performance assessment.

The Board has continually urged the DOE to make greater use of TSPA in setting priorities. In our last report, we took the DOE to task on this issue and urged that they make a management and organizational commitment to develop more systematic and effective ways of using TSPA. At the last DOE technical program review, we heard a somewhat different take on TSPA. Some individuals at that meeting provoked a lot of discussion when they argued that TSPA wasn't good enough to set priorities. We are now hearing that the DOE will be relying heavily on TSPA to make its investment decision in 1999.

In the light of these different views, we would like at the round table to address the following questions. First, can TSPA be used to set some priorities now? If so, what are its limitations? Second, how can TSPA be made more useful? Third, how valid are assessments of compliance and how can they be used?

Next, can a simplified TSPA be developed and used effectively? This has been John Garrick's view. And, finally, what challenges do the use of individual dose and performance periods up to 1 million years pose and can they be met?

In addition to some of the speakers who participated in the previous presentations, we are joined by Norm Eisenberg of the NRC. Norm, want to raise your hand and let them know you're here? Steve Frishman of the Nevada Nuclear Waste Project Office. John Kessler of EPRI, who you met yesterday.

We have also asked some wise, old and young men to provide us with their views. These include Bob Bernero.

BERNERO: Young.

LANGMUIR: Former Director of the Office of Nuclear Materials Safety and Safeguards at the NRC. Chris Whipple of ICF Kaiser Engineers, who you met yesterday. He was former Chair of the Board on Radioactive Waste Management of the National Research Council. And Ben Ross of Disposal Safety.

We have allotted time at the beginning of the round table for those participants who have not made presentations to make a few short comments, if they so desire. Please limit yourselves to several minutes each. We're going to start with some overheads with Norm Eisenberg. Norm?

[Slide.]

EISENBERG: I'll try to be very quick, something people tell me I'm not real good at. I'll try to answer the five questions that were posed very briefly. The first is can TSPA be used now to set priorities. I think so, but you have to be careful. It can't be used alone or mechanically. I would say that I presume that all TSPAs have embedded in them a method or a post-process or a way to parse the results so you can determine the major contributors to risk from each scenario, each radionuclide, important variables, et cetera, as we've seen over the past couple days or certainly today.

I would make a distinction between TSPAs that are primarily focused on variability, and I would include in these the ones done by the NRC and the DOE, versus the ones that explicitly fold in uncertainty where, in addition to the distributions to describe parameters in future states, you fold in distributions representing alternative conceptual models, for example. That second type, I would say, is like the EPRI analysis.

For the ones focused on variability, you get out these identification of contributors to risk, but you have to supplement it with auxiliary analyses to identify what happens if major assumptions change. With the EPRI type analysis, you just get the results coming out. So that's an advantage of that type.

[Slide.]

EISENBERG: The limitations of TSPAs currently are many. But some of the ones affecting their use are transparency. If the analyses are not transparent, it's very hard to convince decision-makers of their worth and to base their decisions on them. You have to have appropriate support by the scientific disciplines. Sometimes the level of aggregation is a limitation. As we've heard, quite often we have to abstract the models to such a degree that it's difficult to treat or discern even certain features. Perhaps an example would be horizontal versus vertical emplacement of a waste package.

Of course, you need sufficient resources for the level of robustness required. Large uncertainties in bounding assumptions may mask the true behavior of the system, and this is a problem because lack of knowledge may actually reduce risks and that is not a desirable perspective on the problem.

Finally, there is always embedded in an analysis a compliance strategy. You've decided to put certain things into the models and leave others out and that automatically eliminates certain issues.

[Slide.]

EISENBERG: So how to make TSPA more useful -- get rid of the limitations.

[Slide.]

EISENBERG: Finally, the question was can -- I'm sorry. How valid are the TSPAs? And everybody understands, I think, that TSPAs do not predict the behavior of the repository far into the future. What they do provide is an envelope for repository performance.

Basically, you can get as much validity as you need for your compliance structure and as your budget allows, but I have to caution, and I've said this many times before, the usual scientific or applied science method of validation of comparing predictions to results is not possible for most of what goes into a TSPA.

[Slide.]

EISENBERG: And, finally, the question is can simplified TSPAs be developed and used effectively. I believe the answer is yes. However, the costs may be great because of the need to demonstrate robustness. You always come back to can we believe it. On the other hand, there may be less need to do gross simplifications because of the greater capability and decreasing costs of computation.

Finally, I would mention, as alluded to at the beginning of my remarks, that PA tools should fit the application and you may need different tools for a compliance demonstration or assisting in program development, such as prioritizing or planning or input to design or other purposes.

Thank you very much.

LANGMUIR: Thank you, Norm. You're not going to get away that easily. You've given us some guidance on what we should be doing. I'd like to ask you what you think of the Yucca Mountain program TSPA. What could they be doing they're not doing? How might they change their approach that would make you more comfortable with what they're doing? Any thoughts on that?

EISENBERG: Well, I think, as usual, we're quite concerned with some of the embedded assumptions in the analysis and how representative they are of the real behavior of the system and whether the results encompass enough of the physics of the system to be used in a regulatory context.

LANGMUIR: Can I get you any more specific? Say we're looking at Bob Andrews' list of investigations required to enhance representativeness in long-term performance. He has a table of bullets. I don't know if you've had the opportunity to look at that or not.

EISENBERG: I don't have it in front of me.

LANGMUIR: I'll loan you one.

EISENBERG: Did you put him up to this? I would say that this is a good start.

LANGMUIR: That was a great finesse. Steve Frishman, I think, would like the opportunity to speak to us and we'd like to hear what he has to say. Steve?

FRISHMAN: Thank you. As usual, I appreciate being invited to sit in on your round table. I feel very comfortable since I've ended up I think the last four or five times in the same seat.

I want to make some comments that are a little bit outside of your questions, first, and do it very quickly, but maybe suggest a new type of real-time approach to TSPA as it's been discussed in the last day-and-a-half or so. That's that with the presentations that we've heard in the last day-and-a-half, I think people in this room and especially the Board probably have the clearest picture of anyone maybe, other than a few people with the program, of at least one approach to application of TSPA in the Yucca Mountain project.

I think it may be useful because of the unique situation that the Board is in, it may be useful for the Board to consider very carefully what they have heard here and very seriously consider making some kind of comment to EPA, as invited by Larry Weinstock yesterday, especially since the NAS recommendation relies extremely heavily on the presumption that TSPA is very, very powerful relative to understanding Yucca Mountain.

I think we've heard enough in the last day-and-a-half to maybe understand better than we ever have before and, as Abe says, maybe as well as we ever have the application of TSPA relative to what we know and don't know about Yucca Mountain.

So I would suggest, for a number of reasons, that that would be a very timely and maybe productive thing for the Board to do. You'll be in the same situation that we're in all the time, and that's that comments are due on the 26th. But I'm sure that they would be glad to accept your insights after that.

I, too, had the same page that you referred to, Don, and what I noticed from that page and actually through the last day-and-a-half is that, not surprisingly, the highest sensitivity areas and the ones that we seem to still or DOE seems to still have the least handle on are the same ones that -- and mostly the same ones that we've known about for a long time.

The more sophisticated and insightful the models become, they more they tell us what we have already known for quite some time. I think the question that needs to be considered in your comments to EPA, if you're going to do them, relative to this very heavy reliance in their recommendations on TSPA is whether, as Paul was saying, whether we could ever get to the point or DOE could to say they had enough or they knew enough.

So I think for purposes of EPA, since they're working on a site-specific rule, they've been instructed to do that, it would be very useful to try to think through suggestions about how TSPA can be built into their regulations in a way that Larry said he would like to have and in the way that DOE would like to have, too, and that's that it can actually be applied in license compliance determination, but also in a way that is not necessarily representative of what DOE thinks it can know, but one that is responsive to what should be known in order to carry out a TSPA that gets you to the level where you think you can present information with sufficient confidence to get a judgment about whether it's good enough or not.

There's another piece of it that sort of clogs the system and we've heard it spoken about and we're going to continue to hear about it, and I heard Abe say today that thermal loading range is narrowing to about 20 to 80 kilowatts per acre. Well, that's pretty close to the range that we've all been thinking about, except it doesn't go quite as high as earlier ranges that were discussed.

One of the problems in having that type of a range -- and coming from another part of the program just in the last couple weeks, I was able to figure out that for purposes of EIS analysis, the plan is to hold repository capacity at 70,000 tons and, based on thermal load, vary the size of the repository.

Now, Abe and I went through a little extraction process in one of those meetings, where it became clear that TSPA, as it's put together now and as a database exists that is in the process of being synthesized, from what we hear, TSPA for different sized repositories is going to be based on different levels of understanding and different levels of information. I think this is something that needs to be looked at very carefully and maybe would suggest that it's time to get even more definitive about looking at thermal loading alternatives and, rather than trying to preserve the world in thinking, go after one and see if you can do anything about it and if you have a feasible system when you can.

But I think that needs to be fed into the regulatory structure, as well, because I don't believe that it is legitimate to have a regulation that, depending on how much information is available for one option that you might want ultimately considered, have something considered at one level of information for that, but then also throw other options out there and sort of let the regulator take its pick. That's a real pitfall that I see coming relative to the way DOE is trying to construct its TSPA and the pressures that EPA is under to get a regulation out that is responsive to what is seen as a current need.

So I just wanted to throw in some of those points to help you consider whether you want to try to take up the challenge of maybe an alternative use of TSPA over the next few weeks. Thank you.

LANGMUIR: Thank you, Steve. Abe or Bob, do you want to comment?

VAN LUIK: It must be something in the water, but I find myself, in principal, in agreement with what Steve said and I'm speechless.

FRISHMAN: No wonder I'm at the edge of the table. I'm getting ready to be thrown back into the pond.

LANGMUIR: Anyone else at the table like to comment? Ben Ross.

ROSS: I'd like to pick up on something that Paul Davis said. He asked the question why aren't we done and the answer was, and I think everyone would agree with the answer, it's because we're not sure about all the assumptions that go in. Although I would make a caveat, which is that the carbon-14 that gives the high numbers was just ignored in this analysis by assumption. It wasn't proven not to be there.

But I think that that answer that we're not done because we're not sure of the inputs to the models gives us a guide to how we use performance assessment to guide the program. As Norm said, it's not a mechanical process. The way you use it is not so much using the sensitivities and the numbers, as sometimes you can't, if you can prove something doesn't matter, volcanism, for example, then clearly you can use it directly.

But the main way you use performance assessment in planning the program, as I see it, is it gives you something to critique and to poke out the weak points in the assumptions, to understand what assumptions you need to make and find the weak points in them. Then once you've found the weak points in those assumptions, that's what needs more analysis.

One good example of that is what just was on the slide that everyone is talking about, the need for more studies of neptunium dissolution. Clearly, when you go through the model and you see that that number has such a big influence on the results, and others are more familiar with it than me, but my understanding is that the experimental basis for that is there are some assumptions in there and there's room for work. It's not as if you were looking at calcium. So that's one example.

I'd like to just mention another example that I found that I think is something that needs more attention and was not on the list, and that has to do with the water flow. It's closely related to the thermo-hydrology. I was very pleased to see that in this morning's presentation, we got finally a name for a parameter that was called sigma.

What that means is how much of the matrix has to be full of water before water starts to flow in the fractures.

Everyone in past modeling has assumed that that number is one, that you don't get any number -- any flow in the fractures until the matrix is saturated. Here we had another value of 0.95 that was thrown out and that was justified on the basis of non-equilibrium between fracture and matrix.

Well, I think even if you have equilibrium, that number is not one, in my opinion. My opinion is that that number is probably equal to the present saturation of the Topopah Springs matrix, something like 0.7, and the reason I say that is very simple. If you look at the interface between the Topopah Spring welded unit and the non-welded unit above there, you have bigger pores on the non-welded unit than in the welded unit. So if you have a capillary barrier that diverts water sideways at the bottom of the topopah springs, it can only happen if there's water being diverted out of the fractures, because if the Topopah Springs matrix is not carrying all the water it can carry, it's going to suck water right out of the non-welded unit into the welded tuff.

So if you find a wet zone at the bottom of the non-welded unit, which, from everything I hear, is being found, there must be more water going down through that

Topopah Springs unit, through the non-welded unit, than the Topopah Springs welded unit can carry.

So by that logic, there must be a downward flow in the welded unit equal to the matrix saturated conductivity. Well, it's a little imprecise. But in any case, if you add any more water to the welded tuff there, such as condensation from gas flow, it's going to run through the fractures and not through the matrix. If that's correct, it's going to have a lot of implications. It's going to have implications for temperature because water is going to recirculate much faster and it might help provide more effective cooling of the repository. It could also have implications for the waste package because it might provide more water to drip down.

Now, what can we say about that? To test that experimentally, there's some things that are easy to do, which is check out the capillary barrier at the top of that unit. Other things might be very difficult. But one thing that is certainly easy to do is to run the models with different values of that parameter sigma. I think that that is an example of where you really have to be very carefully critical of these assumptions and that's what's going to provide the best guidance for your research program.

LANGMUIR: Any comments from the TSPA folks?

ANDREWS: Let me try to comment. Bob Andrews from the

M&O. Ben alludes to conceptual issues of flow in the mountain. I want to emphasize that we started with the project's best estimate of conceptual understanding as of March of this year, essentially. The tunnel is still being drilled. Observations are still being made. Tests are still being performed. A lot of those tests are pneumatic tests of the type that Ben alluded to. A lot of them are looking at the aqueous phase.

There have been no observations of advective drips, if you will, into the ESF within any unit yet. What does that tell us? Does that tell us the flux is low? Does that tell us the conceptual model is wrong? I don't think we know yet. We're in the process -- not we, but the LBL/USGS flow model and the LANL transport model are assessing those data and revising their models.

We did run this sigma 0.95. We also ran sigma 0.9. We didn't show those results or use those results in the TSPA abstraction for the very simple reason that the 0.95 better represented some non-equilibrium dual permeability analyses that had been done at Sandia.

Is that reality? I don't know. It's another model. I think where I would come down is that the conceptual understanding of unsaturated zone flow and the overly used word, but reasonable representation or validation of that flow is still an issue. I think we identified it as one of our number one issues and it still is.

ROSS: I would agree with that. I think my point was really to say that you can't conclude that something isn't important unless you look at a wide enough range of alternatives.

LANGMUIR: I am going to bring us back to the original outline. What I propose we do is continue the general discussion, but, although, obviously, as we get to issues that are relevant to come around the table with individual speakers, we'll do that.

The third person on my list to make a brief presentation was John Kessler.

KESSLER: I'll try to keep it brief. First of all, to respond to a statement that Norm made about EPRI's --- what did you say? We just get an answer. Well, we sure do a whole lot of work to just get an answer and I would argue that our event tree approach certainly allows us to do the same types of sensitivity analysis and we certainly have been doing that throughout the years. So I guess I don't understand your comment.

EISENBERG: What I meant was you more directly are able to treat alternative successful models than we can in the kind of analysis that we do, where we have to do a side calculation of perhaps calculating several CCDFs. That's all I meant.

KESSLER: Okay.

LANGMUIR: It was a compliment.

KESSLER: Thanks, Norm. First of all, I'd just like to say this has been an extremely informative two days. I've really learned a lot. I compliment DOE on their advances in TSPA. There is certainly a lot of new things that I've learned.

Just one comment on one of Eric's summary slides. This whole idea of iterative performance assessment reminds of an old FORTRAN expression. I hope I'm not in an infinite do-loop here and that somehow there's a way out of this.

I am also intrigued to find that some of our models are beginning to show a few of the same conclusions as we proceed through. That was becoming evident.

Now, I guess I'd like to carefully and completely put my utility hat on as I finish my opening comments here and just start by saying, well, why is EPRI here. Why am I here? Why are we doing PA when the utilities are already contributing a large chunk of money to DOE -- well, I guess to the Congress or Congress gives some of it to DOE. And, certainly, why are we doing PA?

I would guess my first comment would be the flip answer. It's certainly not for scientific enlightenment. It is because we view PA as a management tool. I think that it helps us find things that are important and certainly, as utilities, we want to be able to comment somehow on what we think DOE should be doing.

Now, obviously, if you're going to use it as a management tool, you have to be really careful, especially when we're talking about as much of a project as the Yucca Mountain effort is. I'll be the first to admit that our models are far from reality. However, I do like to comment that all models are wrong, some are useful.

In that case, I would like to say that certainly I endorse the idea of continuing with the synthesis of the basic data into fundamental process models. That's certainly an essential feature to support TSPA. Process model development needs to continue so that the conclusions of the TSPA remain valid or confidence is built in them.

However, it's becoming clear that even at the current state of affairs with TSPAs, that there are some things that we see that always seem to be important and, on the other hand, there are some things we see that always seem to be unimportant. So I'd like to address the unimportant things first.

That is, for the unimportant things, where is the end of the road for them? I would argue that if we're going to -- for the unimportant things, will new information, new models have any likelihood of converting those unimportant things into important things. If the answer is no, we really don't think they will -- I've got "if not ..." here on my phrase. Where do we go with that? I think we're reaching an end point on those issues where DOE feels that new information, new models will still not make those issues important. Certainly, for the ones that are important, it's pretty clear.

So now it's time to put the waste isolation strategy, or I like to think of it more in terms of a safety case, into action. I think that DOE needs a large portion of intestinal fortitude at this point. There are some conclusions that are being reached on things that are important and unimportant.

And I guess I'll be brave enough to attempt to answer the question that Paul Davis said he couldn't answer, and that is why did DOE choose not go ahead with the SPM process results. I think it's pretty clear that their -- well, I'll guess and say there were a lot of vested interests both within and without of the program, intended to make or want the results to come out other ways.

So, again, I exhort DOE to have the intestinal fortitude to act upon the waste isolation strategy they're developing that is based, in part, on some of the more definite results of TSPA at this point. So, again, the bottom line here is please use the intestinal fortitude that you need to make the difficult decisions in prioritizing what is now a whole lot less money that you've had before to reduce uncertainties or whatever on those important things and to make sure that the unimportant things you're finding remain unimportant.

LANGMUIR: Thank you, John. How about some comments from the young Bob Bernero at this point?

BERNERO: And out of deference to my youth, I will stay seated. I would like to start by saying an almost contradictory thing. I'm very pleased with what I heard about TSPA-95 today, and yet I would make the comment that it is obvious that DOE is not done. The status of this work is clearly not sufficient for finality.

However, the quality and what's good about what TSPA-95 has is, first and foremost, in my mind, a substantial shift in the character of the work compared to the previous performance assessments. Now, it is not so much methodology development and methodology debate. Now, I think there are substantial insights into what the site is, what are the important mechanisms at the site. And I think it couldn't happen at a better time.

I was not here yesterday to hear the gloom and doom about the budget and the program here, but I heard it across town in another forum, from some of the same people. Right now, the tunnel boring machine is, I believe, at the bend and very close to the Ghost Dance fault and when drifting starts in the Ghost Dance fault region, there will be, for the first time, some very substantial knowledge about how much usable real estate is down there in the repository horizon and it's an opportunity for the design faction of DOE to really look at this design to say is this site going to be suitable, is it capable of meaningful capacity at a defensible thermal loading and so forth. This performance assessment, TSPA-95, is an extremely important tool in that process.

Now, I heard today about a good deal of iteration between the performance assessors and the designers, at least dialogue. The term that was used was dialogue, but I think it has to be iteration. The performance assessors now know the strengths and weaknesses, to a large degree, of their performance assessment and by engaging in iterative dialogue with the designers, I think they can assist the designers to exploit the capabilities of the Yucca Mountain site to the degree of making the best, taking the most important factors.

And this design, to a large extent, just grew and now you've got some real priority-setting to give attention to. Although galvanic protection doesn't get you 1 million years in the slides we saw, it buys you an awful lot.

It actually makes the first subsystem performance criteria of NRC a trivial requirement. A 1,000-year package doesn't mean anything in this context. It doesn't provide defense-in-depth, but galvanic protection provides you a very substantial degree of certainty of long-term zero release, substantially complete containment.

And I think if the designers, at this juncture, can look at key parameters -- there's one, it's a pet idea, I've raised it before, about other media to be inserted perhaps inside the canisters. The interstices of the spent fuel are empty and if there were another medium in there, a cement-forming potential granular material, such that you would or might retard the mobilization of radionuclides when the package finally does fail, that might be a readily obtainable, low-cost option and it would simply be a way to exploit the circumstances of this site.

There are other things. One of the things when I saw galvanic protection, it buys you a very long-lived package. It's not a 1 million-year package, like the Swedes have historically pursued. But a simple, almost brute force thing. Why not put one galvanically protected package inside another? Take your carbon steel, divide it into two and put a cement medium in between the two. That is carbon steel, nobel, cement, carbon steel, nobel, cement. What would that do? Is that frivolous or is that a meaningful choice?

I think setting priorities, to answer your question, Don, using the performance assessment to set priorities not only on design, but also on the data, experimental data needed, I think it's extremely important. It would be extremely timely now.

If the design can be optimized, then I think as the project goes forward, Dan Dreyfus is in the best position to know whether he's got a suitable site as soon as possible, with minimum expenditure of resources. And he's also going to be on a path to find out sooner and more cost-effectively whether it's a licensable site. I think everybody can do that.

I'd like to emphasize one thing. It was very enjoyable listening to the TSPM and the Paul Davis thing, the bases for the approach. There's one place where I disagree with the National Academy Committee on the Yucca Mountain standard. When they came out and they said individual risk exposure should be the test, the maximally exposed individual is not chosen, but the member of the critical population group, and then they said the regulators ought to set that in rulemaking. I don't think so.

I don't think the regulators should do anymore rulemaking than to state a standard with words like "in the appropriate" -- you know, the member of the critical population group in the appropriately examined biosphere, because that's going to be specific to different sites.

And we all know what happened the last time the regulator, EPA, tried to avoid dealing with the biosphere by going to a quantitative release standard. That's the real reason that was done. And so I think if you go back to this thing and ask what is the basic purpose here, the basic purpose with this site, deep geologic disposal, with finely-tuned optimized engineering barrier systems and containers, the purpose is isolation. It's not transport. You're not trying to produce release. You're trying to prevent release.

You want to be as isolated as reasonably achievable and you want realism to be zero release. That's your goal. Your goal is containment. And you want to be able to examine that with a performance assessment not to say I'm predicting it will release exactly this much, but I have a reasonable expectation that it won't exceed this release. I want a CCDF and I want that CCDF to be well below the goals, and the goals are going to have uncertainties. Is the right goal the biosphere model that has the subsistence farmer or the biosphere model that has this feckless individual sucking out of a five-kilometer well, two liters a day of water that would taste like you know what? That's an uncertainty.

And climate change makes the biosphere a great uncertainty here. I think the real objective for the demonstration of the safety case is we have reason to believe that reality is somewhere down there near zero, it's orders of magnitude to the left of this CCDF. This CCDF fully displayed, a robust case, as far as we know, still reflecting a lot of ignorance, compared against the uncertain standard, the biosphere uncertainty, that there is margin and that margin itself, that degree -- who was it?

Someone had a slide up there where the EPA CCDF rectilinear one was a long way from the actual or projected CCDF. I think that that would be the right outcome for this, and that's a strong case, if you can accomplish that. It is useful that TSPA-95; at least in my eyes, has given very clear perspective on some things that are no longer important and has focused on these water transport effects and so forth that I think are important.

LANGMUIR: Thank you, Bob. You've raised a lot of issues and made a lot of suggestions. I wonder if Chris Whipple, right next to you there, might have responses regarding the NAS recommendations.

BERNERO: It's my old colleague.

WHIPPLE: I want to point out that this is the hair-deprived corner of the table. I don't know if the seating was done that way intentionally. Well, a few comments and I will get back to -- I'll correct Bob's points in a minute.

First, I want to compliment the presenters today. Having sat through a lot of PA presentations, I agree with Bob that the presentations in the past focused on how we did it and today they focused on what we learned from it, and that sure makes it a lot easier to listen to, I can tell you.

The insights and technical processes that matter came to the forefront and much more helpful for, I think, an advisory board like the TRB to have this predigested for them in this way.

A key take-home lesson for me from these presentations is that, boy, do you have to know what the standard is to know what's important. It couldn't have been clearer today that if it's a 10,000-year CCDF, one list of things matter, one list of processes, data and information matter. If it's a 1 million-year individual dose limit, a whole other set of processes matter.

The sooner that gets pinned down, the better the project will be able to move ahead with working on what matters. That's why I disagree with Bob on who should define the biosphere. I think if you make that a task that the applicant has, then we won't know for 15 years what the acceptable biosphere in an NRC licensing proceeding will be. And if you make it the job of the EPA and NRC, we'll know in two or three years. So I think for no other reason than the necessity of knowing what the rules you're trying to work under are, it ought to be taken off the back of the applicant. It's a heavy burden and I think it's fair for a regulator to carry it.

What was interesting to me was in listening to the discussions on the individual dose long-term case was I felt much relieved, I must say, after sweating weather, what we had recommended in that NAS report was off-track and totally unfeasible and overwhelmed by uncertainty, was, frankly, how simple things got when you were trying to do that calculation, because it became a steady-state calculation.

You have a percolation rate down through the mountain. You have a source term that's dependent upon that percolation rate. Then you have an underlying stream that carries the stuff away when it gets there. A lot of the fine structure in the unsaturated zone tends not to be terribly important for that performance measure. It is important for the containment requirement that has been in the standard. So I think that I took some comfort in that.

A key lesson is I hope somebody in the program is trying to figure out how on earth to measure the flux rate through the mountain now that you're getting a tunnel down into it. I can suspect that's not an easy thing to do. You're drying out the rock, you're blowing air so the miners have enough to breathe. It's probably got some technical difficulties, but it's clearly the one parameter above all others that I've heard that matters most to long-term safety. And having a real physical measurement that you've got some confidence in goes a long way to replace plausible models.

Paul Davis mentioned some of the similar issue at WIPP in terms of the long-term hydrology matter, whether it was Darcy flow or just releases from a disturbed rock zone and how that was -- that experiment has been run for a number of years and it's been somewhat uninformative, not clearly able to distinguish between the two cases. But I sure think WIPP was right to try to run it. There were some problems in how it got started. I think DOE needs to look quickly at the opportunities here to make those measurements. Clearly an insight that's come out.

If it turns out that the percolation flux is naturally quite low, then you have a pretty good basis for believing that over the very long term, this is going to be a safe facility. If it's not, then the question becomes whether you can do something with engineering at the waste package or in the engineered barrier arena near the waste package that provides for locally low percolation fluxes by the waste. Then, again, that's an engineering design job and I don't know whether you can do it or not, but you will know whether you need to fairly quickly.

And I certainly agree with Bob Bernero that you should make a fairly wide open set of tools. If, by putting something in the waste can, you can increase the confidence that you're in a diffusive release mode rather than an advective release mode, again, that gives you very high comfort on performance and I think there ought to be technical fixes that would do that for you.

Something that came out of the other results having to do with the long-term individual dose was the importance or the comparative importance of the behavior of the saturated zone. Just a few questions I had. One is why, in the long-term case, almost a steady-state case, would longitudinal dispersion make any difference. I mean, it's kind of riding over itself forward and back, but it's kind of all smeared out and averaged, I suspect.

Similarly, with only longitudinal dispersion, I don't know you'd get a different result at 30 kilometers than at five. Maybe I misunderstand this, but I have a picture of a tunnel going down from the groundwater to these wells.

There was discussion of what kind of measurements you could do to get a handle on those things and I will point out that EPA is involved in more places than it can count with recent experiments of this type, having to do with contaminants inadvertently dumped into groundwater, and they've got a lot of monitoring wells tracking such things. So that the general nature of plumes of contaminants for long periods of time is something we've got a lot of data on. Now, how applicable that would be to Yucca Mountain, I don't know, but it's not like we're starting from scratch here.

Back to the question that Don opened this summary session up with, which is how can TSPA be used to set priorities. Paul Davis' comments brought to light what I believe to be absolutely true in WIPP and true in Yucca Mountain, as well, which is that there will always be a tension between performance assessment and the specific technical programs. I've never met yet an investigator yet who didn't believe that his or her area of specialization was the most important of the project, even when it wasn't.

I mean, worrying about something that could be no more than two percent of the project, people nonetheless can do elegant technical work and be convinced this has to go forward for years. And someone worrying about getting the job done on a budget has to say this is fun, but it's not what we need. And PA, I think, is, by all accounts, the only real tool we have to do that. But it's hard to do in a big complicated program. Some programs develop lives of their own and PA is complicated and messy and uncertain. In any case, judging from WIPP, it's not easy, but it's worth doing, to let that be the arbiter of what science should go ahead.

And a final point, and this kind of comes out of observing a number of things in long-term DOE programs. It's very easy for a program to come to believe that there's never time to do a good three-year experiment in a 30-year program. I can point to any number of experiments at WIPP that they don't have time to do now because they'd take five years and they didn't have time to do them ten years ago because they'd take five years.

I think that as much as Congress wants to hear that Yucca Mountain is going to be decided on in six weeks for ten bucks, we all know that's not true. We know there are some long-term experimental programs that really ought to go ahead. It's critical that we pick a few ones that are the most important and not be put off with the fact that they may not produce all the answers we need in a couple of years. This is a long-term activity.

LANGMUIR: Thank you, Chris. Lots of things to talk about and perhaps some responses from DOE. Any thoughts on the tutorial we have just gotten for you?

VAN LUIK: Yes. I appreciate the tutorial and I am in amazing agreement with almost everything, even the disagreement across there. I think the biosphere is kind of a half-and-half type thing. We would like the regulator to, kind of what was done in 40 CFR 191, at least put some limits and guidelines in place and then we have some freedom to work within those, to ask them to specify.

I think someone on the NAS committee blurted out in one of the meetings just specifying the biosphere can make or break any site. I think when you look at the uncertainty in the biosphere, it really swamps some of the uncertainty in the sciences that we've been looking at today.

So I would endorse your recommendation that the EPA look seriously at at least putting some limits and guidelines in place.

I was accused a while ago of being the person who said that PA wasn't ready. If I could, at this point, respond to that, I think Bob Bernero pointed out that it's obvious that DOE isn't done and I couldn't agree more. Where I was coming from last time is that we had not yet made the direct connection between the work that was going on in the project and TSPA in '93. We deposed, the good lawyers, the principal investigators and got from them their best guess as to what was going on in the mountain and quantified that.

What you saw in TSPA-95 is the first step to building some more confidence in this program. The process models are being built by the site program and by the engineering program. We have a list that Eric showed that we expect will come in this coming year. You heard from Bob that two have already come in. One we've already used for this TSPA.

Once we are grounded to what those people themselves believe based on the interpretation of their data, including what the best conceptual model is, I think at that point, and that has to be 1998 because probably there's nothing after that, at that point, we will have something that I think we will have a lot more confidence in. So that's the plan, as far as I can at this point.

But my comment last time was not that TSPA was useless. It's just that I was fearful that it might be used to cut off work that would show us that we have the wrong conceptual model. Until that work is done by the site program and interpreting their own data and creating the process level model that they feed to us, I would say that it would be arrogant on our part to say stop this, stop that to people who have not yet had a chance to interpret their own work.

LANGMUIR: Thank you, Abe. I've been holding this question. It was one that you really left hanging during the day. Namely, you're doing an analysis which is of the maximally exposed individual, or the born losers we've called him in the program, in all of your analysis of dose. You pointed out that could be a very different answer than if you were looking at the current idea that NAS has proposed of considering the average person in the critical group.

I guess that worries me considerably because what have we learned about all the results of the TSPA if the answers may be very different with regard to what's important and what isn't important if we take this other approach with the critical group. Can you comment on that? Have you done any analyses considering the critical group? He's pointing at Bob.

ANDREWS: Let me. All the sensitivity cases are as germane for the peak individual, maximally exposed individual at the fence as they would be for a critical group that might be existing there now in the Amargosa Valley, getting a lot of their water currently from the alluvial aquifers of the Amargosa Valley.

So the sensitivity analyses, the what's important, if you will, becomes the same. What becomes different, and I'm going to answer Chris' question here, is not longitudinal dispersion, but lateral, transverse, if you will, dispersion. We have right now, when you go from the fence, if you will, to five kilometers, the effects of transverse or lateral dispersion become somewhat insignificant.

If we had included them and looked at the middle of a plume, maybe it's a factor of two, but it's not a big deal and factors of two we don't talk about, as I've said. When I go from that five-kilometer point to 30 kilometers and I mix -- well, I have dispersive effects, transverse dispersive effects, not longitudinal dispersive effects. I agree 100 percent. Longitudinal dispersion is not buying you anything in these long time frames. But transverse dispersive effects does buy you something, and I will combine with transverse dispersion lateral mixing of a range of different groundwaters that mix in those alluvial aquifers.

We did some things in the actual report where we tried to, back-of-the-envelope, estimate the additional mixing or dispersive effects associated with going from five kilometers to 30 kilometers. That additional factor was somewhere in the 30 to 100 range. So if I'm looking at the absolute value, not the -- the relative values are unchanging, but if I look at the absolute value of that peak dose now to a different set of people instead of this guy at five kilometers, it's a factor of 30 to 100, roughly, reduced from the values that you saw on every peak dose curve.

LANGMUIR: But it would not change your suggestion of priorities for DOE's work for the coming years in terms of resolving the issues that you consider important.

ANDREWS: No. I think I had on there dispersive mixing effects in the saturated zone and definition of the biosphere. Both of those things related to that same issue.

LANGMUIR: Pat Domenico.

DOMENICO: Bob, your models are forerunners of models by Pigford, I believe. He did a lot of that stuff years ago. But it's my understanding that you don't have a transport model that can handle five to seven daughter products that can incorporate transverse spreading. Is that true? To handle all the daughter products now. I don't know that there is one available yet.

ANDREWS: Well, we're not modeling three-dimensional transport of radionuclides in the saturated zone. We're putting them into a 1-D.

DOMENICO: Tube.

ANDREWS: Tube. A 1-D tube does have all the daughters in there.

DOMENICO: But there is no model for that saturated zone, to my knowledge, that will handle longitudinal and two transverse or four transverse spreading directions available yet that can handle those daughters.

ANDREWS: To handle the daughters, that's correct. But if I say my key nuclides are technetium, there's no daughter there that I'm concerned about.

DOMENICO: Then there are models.

ANDREWS: And neptunium and the daughters are immaterial if it's neptunium that's driving things. We do have transport models.

DOMENICO: You have transport models in multi-directions.

ANDREWS: Yes.

DOMENICO: But none that handle the daughters, and does the transverse spreading, is what I'm saying.

ANDREWS: As far as I know, none that handle the daughters.

DOMENICO: Well, we have one now. I wanted to let you know we have one coming out in two months, of a graduate student.

ANDREWS: Is this a sales pitch or something?

DOMENICO: That's a pitch.

LANGMUIR: John Kessler.

KESSLER: We have one now. We've got 1-D unsaturated zone, 3-D saturated zone that handles three of our chain daughters.

LANGMUIR: Thank you. I think at this point in the schedule, we're to have public comment, if there is any. Is there anyone in the audience who would like to make a comment? Please come forward and identify yourself. This is Judy Treichel.

TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force. I wasn't worried about the time that it would take me to get up here because the only other concerned citizen I've seen here is Max Blanchard, and he wasn't here today. He must be concerned about something else. And I'm not going to take the 30 minutes.

I was interested that when the program started out today, that Abe began by referring to himself as an environmentalist, and some of my best friends are environmentalists and they say very different things from a lot of what Abe said and a lot of what we've heard here. And when you started out, Abe, you were talking about solving an environmental problem, and I'm not sure that that's ever been clearly defined exactly what this environmental problem is that we're solving.

And it would seem to me that if we have a serious environmental problem with commercial nuclear waste, that there should be something very seriously going on at those reactor sites, if, in fact, there are people in 109 locations, as NEI so often tells us about, and they are suffering from a serious environmental problem right now. Something should be done about that. NRC should get on that and get something done about it.

If, in fact, Yucca Mountain is a solution, what's it a solution for? Everybody that has nuclear waste is apparently looking for this panacea out there, this wonderful solution, and they all point to Yucca Mountain. And yesterday, I don't think it was here, I think it was over at NAS, Dan Dreyfus was talking about the amount of waste, that right now his calculations were showing something like total waste that he predicts would go to Yucca Mountain are about 110,000 tons, and that didn't count things like greater than Class C, other waste, special waste. I think some of them are called cats and dogs. So there was a discussion about whether or not he was low in his calculation.

And I don't think Yucca Mountain is that solution when you start looking at things like that. And, in fact, a lot of what we've seen here today shows that Yucca Mountain is possibly a future threat. There is all the talk about how you figure out just what a threat it is. And if, in fact, it goes from a threat to being an actual danger or a problem, it's irreversible. It's one of those things that you've done that you just can't undo.

If we have a serious environmental problem with nuclear waste right today and it needs to be isolated or re-isolated, we can go out there and solve that. I'm sure we can do that. If, in fact, the stuff inside Yucca Mountain, in 1,000, 10,000, 20,000 years somehow needs to be re-isolated because we're starting to see that there was a problem and the PA was wrong and that the confirmation period, as Steve Brocoum often talks about, is showing us that we've just confirmed we've got a problem, I'm not sure what it is that we would do about that.

I liked hearing Bob Bernero use the phrase zero release. I don't know if he means it in the same way that the people I hang out with mean it. But that has been the goal and it's always been the public implication. When you see the ads in the paper that NEI runs or when you see a DOE presentation and it shows this long, high ridge mountain and people are to perceive that all of this waste is somehow underneath there, that's the picture that they get, in their head. That's what registers to them, is that the stuff is gone and absolutely no part of it ever shows up again.

They think zero release and they've been told historically things like it has to take longer than 1,000 years for the groundwater to get out. If it's 999 years, that's too short, we walk away. If we can't prove it's safe, we walk. We're out of here.

So they were putting out this message that this thing was a solution. But what it looks like now is that we're trying to determine how much is wrong with Yucca Mountain and what it takes to fix it, and that's a lot of what this discussion has been. Not that it's a perfect solution, but it's a place with undetermined problems and how are we going to engineer or design fixes for those problems.

So it just seems to me that one of the things that has to happen is for the public to be told that, yes, the stuff gets out of there. Some of decays before it actually gets out, but there's a lot of it that really gets out of there. And paint a clear picture for them about what the solution in a very open forum, one that gives them enough time, enough information, enough respect so that they can respond to it, and then find out from them if they feel that Yucca Mountain, as a solution, is less problematic than the problem. You may find out things you didn't want to hear. It may be that they don't want to buy that solution because they may see it as not being one.

And there is still a controversy raging out there about whether or not people actually want to bury spent fuel, and it would seem to me that before we get too far along in this thing, that we should probably decide that and probably a lot of other questions that will come up.

Thank you.

LANGMUIR: Thank you, Judy. Any further comments or questions from the audience?

[No response.]

LANGMUIR: If not, I want to thank today's speakers and those of you who participated in the round table and Leon Reiter for making this job easy for me and organizing the two-day session, for this very highly informative experience I think we've all shared. We're adjourned. Thank you for coming.

[Whereupon, at 5:00 p.m., the meeting was concluded.]