UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON RISK AND PERFORMANCE ANALYSIS

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8:30 a.m.

NUCLEAR WASTE TECHNICAL REVIEW BOARD MEMBERS PRESENT:

Don U. Deere, Board Chairman

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PARTICIPANTS:

Ian Miller

Seth Coplan

Frank Rowsome

Stan Echols

Norman Eisenberg

Tim McCartin

Dick Codell

Robert Shaw

William O'Connell

Paul Kaplan

Max Blanchard

Jerry Boak

Carl Johnson

Jerry King

Albin Brandstetter

Mike Wilson

Ron Callan

P R O C E E D I N G S

[8:30 a.m.]

DR. DEERE: Good morning, ladies and gentlemen. Welcome to the second day of the meeting. I would like to turn it over to the Chairman of our panel.

DR. NORTH: Thank you, Don. I am pleased to welcome everybody to the second day of this meeting of the Risk and Performance Analysis Panel. Yesterday we heard from the Department of Energy, and today we are going to hear from a variety of other organizations on the subject of performance assessment.

We are going to hear from Ian Miller of Golder Associates; we are going to have some presentations by people from the NRC; and Frank Rowsome, formerly of NRC on the subject of lessons learned from the application of probabilistic risk analysis for nuclear power plants. We are going to hear this afternoon from Bob Shaw of EPRI, who cannot be with us due to a conflict. He's at a meeting in Baltimore.

Then, we are going to have a roundtable with speakers from both days, discussing the strong points and weak points associated with the application of performance assessment to a high level waste repository and strong points and weak points associated with DOE's efforts in performance assessment. Hopefully, we will be finished by the middle of the afternoon. I would like to acknowledge at this time the contribution of Leon Reiter, who is the staff for this panel who has taken care of the arrangements for this panel meeting, and I would say made a heroic contribution in this respect towards the efforts of the Board.

With that introduction, let me introduce the first speaker, Ian Miller from Golder

Associates.

DR. MILLER: I am going to give a somewhat disjointed presentation today because I am going to introduce ourselves and jump to the end, and then come back and show you what is in the middle.

[Slides.]

DR. MILLER: The work that Golder is doing, the scope is defined as an independent study to develop and recommend a detailed strategic plan of action for evaluation of the suitability of the Yucca Mountain site. This is being initiated by DOE headquarters. We started about a year ago, went through a lengthy hibernation period while contractual issues were settled, and we are now trying to get back up to speed again.

It is operated through DOE Chicago and Argonne. We do, in fact, use Russ' money and he helps us quite a lot in telling us what to do.

You don't have to read all of this because I think everybody is pretty familiar with the general concept of using performance assessment in an ongoing basis throughout the project, both from the siting stages on up through licensing stages. We are all talking about how you do it, how do you integrate it, how do you get that feedback operating. We in Golder Associates have been promoting that sort of approach for a lengthy period of time, and the approach to establishing site suitability that we are looking at is really founded on using performance assessments in an iterative way.

[Slides.]

DR. MILLER: Here is where I am going to jump to the end to sort of demonstrate what we are talking about. Most of the speakers yesterday were talking about the same thing. We did a little example calculation exercise, and I will walk through that just to point out a specific example of how you can use a performance assessment early on in a program to get quantified information about actions that you might take in terms of establishing whether a site is suitable or not.

Here's a little sketch of the sorts of processes that we expect can take place at Yucca Mountain. As Paul Kaplan said yesterday, it turns out that nobody much has been able to make it fail without getting some very rapid transit down from the repository horizon down to the water table, the sort of pathways -- these solid orange pathways here. We found the same thing with a little demonstration model that we put together.

We also observed that in people who have done integrated performance assessments -- not that there have been very many -- there was a wide variety of the ways in which they addressed this particular pathway and various flavors of fast paths were built in with various rationales for why they were there. I think in general they are there because if they are there you have problems and because there's kind of lingering suspicions that that sort of feature may, in fact, exist at the site.

As this little calculation example, we looked at and tried to focus on how one would address the uncertainty in that sort of pathway, particularly with regard to how fast material might move down the path. Here's a little blowup of the pathway. Conceptually, here's a waste package buried beneath a emplacement. Down here somewhere is the water table. What I have shown in the colors here are sort of typical flavors of pathway that a nuclide might follow.

The white material is representing fractures much bigger than they really are. What I tried to show with the three different colors here was really three different kinds of paths. The red path, as you can see, made it all the way down to the bottom and never once encountered a fracture. That is sort of the conceptual model of what happens at a low saturation is that the factors don't want any water and, in fact, there are barriers to it. So, any flow path will literally have to work its way around the fractures.

The green path is a case where the flux might be a little higher. Some of the fractures are becoming partially saturated and starting to flow more significantly, but the fractures don't make a continuous pathway. This particular nuclide path has to traverse little bridges of matrix in order to get into the fractures. Once in a fracture, presumably it moves fairly rapidly and then gets another few inches of matrix and so on.

The blue path is kind of similar, but you will see less bridges. This particular nuclide found an unusual feature which was a connected pathway, a pathway that really could go from top to bottom without having to spend any time in the matrix. To model this is in a very simple way what we said was let's view this as essentially two parallel paths. There is a path of nuclides who will stay entirely in the matrix all the way down and there's a separate parallel path with two components being matrix bridges and fracture flow.

Let's address the scientific issue of what is the distribution, what is the length of the matrix bridges versus the length of the fracture flow. In theory, one can go out and do tests and look at the same sort of thing that Paul was talking about yesterday, how long are the fractures, how well connected and so on. I sketched the form one might have given to a probability distribution for the fraction of the total path length that would be spent in the matrix bridges as opposed to being spent in the fracture flow.

The way we divided flow was we said, everything below two millimeters a year would run strictly in the matrix and everything above it would run down this pathway. Again, I comment that the only purpose of these numbers in this exercise is to demonstrate how a test works, quantify what you learn when you run a test and update things.

If you took this probability distribution and you simulated this system --

DR. DEERE: Why does your probability go to a zero at zero matrix bridges?

DR. MILLER: In fact Don, it didn't. We subsequently had to have a finite probability there in order to get it to fail.

DR. DEERE: If you had a fault --

DR. MILLER: Exactly.

DR. DEERE: -- that is what is of great concern.

DR. MILLER: Exactly. We did not use that triangular distribution. I will show you what we used a little bit later. It had a ten percent likelihood of zero time spent in the fractures. This is the CCDF that came out. It's a Monte Carlo system, so we cranked a few realizations. We had to band it quite a lot to get it to fail. It failed the way we expected it to.

In order to get it to fail we had to disable any sorption and retardation process in the saturated zone and we had to speed up a couple of other things and so on. It really wasn't very comfortable failing. We wanted it to demonstrate something. Here's a system with a tail that's violating the three-ninths criterion.

The question is, is it violating it because of our ignorance or uncertainty or is it violating it for some fundamental reason, something that you can't make go away. This is caused by that fast pathway in the likelihood that the flux is high and there is no time spent in the matrix. The bulge here is Carbon 14 release and the rest is all the more conventional processes.

[Slides.]

DR. MILLER: I am going to step back one slide for a moment. If this was what your current distribution on that fraction of the distance in the bridges was, what would happen to that distribution after you went and did some tests and evaluated the data. In one sense you can't say that's why it's a distribution. You can't say it will be sharper and it will be centered here. What you can say in principle is that it will be sharper. There is lots of ignorance right now and we are going to narrow that down. You can't say where you will be. It may be sharper and it may be bad news. It may be sharper and it may be good news.

We go whole hog when we go Monte Carlo. What we do is, we not only use Monte Carlo for the performance assessment, we actually have an outer layer where we use Monte Carlo to simulate carrying out tests, SCP type work studies. When we simulate carrying out a test we actually simulate each individual test and what the result is, what the quantum of information gained is, and randomly what the new PDF looks like.

We took this little problem and we did some simulations updating our knowledge about that particular parameter. Here is the sort of thing that we got. This is the base case in red. This is just the CCDF now. The dash lines represent some of the realizations of what our state of knowledge, the result of our state of knowledge after doing the test. What has happened is that usually what you learned was what your preconceptions were. It was a few percent up to 30 or so percent of time was spent in the matrix, in which case you really chop off that tail.

Once a while -- once in about ten times -- it turns out that it was the bad news case, the one that you feared. In that case your performance shoots way out of bounds. The purpose of this little exercise -- I have one more slide on it -- we run the full sweep. This is probably a little confusing. This is CDF's now. What we are showing here is the fraction of the path length in the matrix. That was the parameter that we played with. At a point this is the original distribution. This is physically the distance in the matrix, and here is physically the distance in the fractures. You can sort of see the ratio directly.

We had a ten percent likelihood in our original distribution that, in fact, no time at all would be spent in the matrix and it would all be in the fracture. We had an upper bound of 30 percent. We said you would never spend more than 30 percent of the time in the fracture -- in the matrix rather -- given that it was fracture flow. These were some examples of the updated distributions that came out when we simulated our tests.

That's an example of the approach that we are building around, that is the kernel of what we are trying to do.

[Slides.]

DR. MILLER: We have a fairly ambitious plan afoot to develop a model of not only performance of the Yucca Mountain preliminary performance assessment model but also to build around that, the framework which lets you simulate the process of characterization, of gaining knowledge, of looking at whether the site is suitable and complies with regulations or not.

Some comments on what this little schematic is showing. As everybody knows and as we spoke about a lot yesterday that due to complexity, the performance model has to be greatly simplified. You can't incorporate all the details down at that level. I think it was a corollary of that though, which is that it also has to be completely general.

When you are at the bottom of the pyramid and you are looking at one process in great detail you can ignore all the other processes that are going on. When you actually are around a performance model you have to be able to say when you put out a CCDF all sources of uncertainty are in there, all credible processes and events are in there. So, while it has to represent things simply it also has to represent them all, and you have to be able to demonstrate that they are all there.

The model is defined by scientists. The way our system is set up it's really a shell. It's a simulator, but it has no content until we sit and talk to the scientists and they tell us how things work. It's almost a programming language. It is designed so that you can create a very simple performance model up at the tip of the pyramid or, if you want to, you can put in some flesh underneath it and make it more complex. You can say the travel time in this zone is a PDF or you can say the travel time is represented by this equation or by this subcomponent model. You can do that arbitrarily until you run out of computer cycles.

The scientists define the model and the model defines the parameters. When they say we are going to represent corrosion in the waste package by the following three parameter Weibull distribution, that defines the database -- an entry in the database of the parameters needed to define the model. It is important to note that those parameters do not necessarily correspond to the things that you measure out in the field.

You go out in the field and measure a fracture aperture or something like that, there is a fair amount of processing and interpretation to be done before that is an input parameter to a model, particularly when you talk about a high level model. Of course, that is where the expert judgment part of the process of integrating up gets into the picture.

[Slide.]

DR. MILLER: This is a database, in fact, and I will expand a little bit more on what it looks like. As the experts define what they want to put into our model and they define the parameters required for it, we will elicit those and put them into a database here so the model can access them.

The characterization activities database relates to the site characterization plan. I will elaborate a little toward the end of my talk how we access the SCP and other project information. The purpose of the system is to say take your current level of knowledge, do a performance assessment, actually evaluate most of the regulatory criteria and see where you sit and then simulate doing some characterization activities, just like the little example that I spoke about. Update your parameters in a Monte Carlo way, rerun the performance assessment and see the likelihood that you get somewhere.

What you find, as Dave Dobson and others spoke about yesterday, is that for most of the activities you can do you can update the parameter that is affected by that all you like and it won't change the performance. There are very few parameters that are, in fact, critical parameters. From a strictly scientific technical point of view, you should be focusing priorities and resources on those things that can have some effect.

The reason I picked the little example about the fracture flow was that's clearly a particular item, there is significant scientific uncertainty, and under some circumstances that can be a critical item. That's an example of the purpose of this sort of a system.

I am going to walk and explain in more detail what is inside the components of these boxes starting with the performance model.

[Slide.]

DR. MILLER: The orange here is the performance model that actually has three subsidiary components, a waste package model, a pathways model, and a disruptive events model. The blue box is the model parameters, is that database that was in the previous slide up above the performance model. There is a purpose for all those arrows.

Essentially, the list of parameters that we elicit from the experts defines the waste package model -- that's this arrow -- it defines the pathways model, what pathways are and how do they work. It also defines disruptive events and processes. So, within the database you talk about climate and you talk about volcanos and human intrusion and so on. When a disruptive event or process occurs that in fact changes the model parameters. Things start to go faster or to go slower or processes are affected and so on. There's a feedback there. The disruptive events can directly change the waste package and they can directly change pathways. So, the little arrows flying around are not that difficult in practice.

[Slide.]

DR. MILLER: In the performance model we try and pull out most of the significance performance measures, so every time we do a Monte Carlo realization in there we pick up the dominant regulatory requirements. We pull them out -- when we have done a simulation we can look at any of these to see what the level of performance is as a function of what we simulated. I will go into a little more detail now on the waste package model, on the pathways model and disruptive events model.

[Slide.]

DR. MILLER: This is just a schematic of waste packages. You have seen lots of pictures like this before. The main purpose of showing this is to sort of exemplify the amount of variability there is amongst waste packages. It isn't one typical waste package and one typical environment that will fail at one particular time. There are lots of variations in the local environment, temperature, moisture conditions, what happens to the rock and so on. There are lots of variations in the ways in which waste packages can fail. There are different mechanisms of corrosion, there are different mechanisms of release.

A key to the approach that we are taking to the waste package is just itemizing and splitting out what are the factors. For example, we will say what are the different modes of corrosion of the container. With no trouble at all, the waste package people can tick off half a dozen or so different, really almost independent modes of corrosion. We record all of those and they go into our model.

For each mode we can start talking about how do environmental influences affect it. Can this mode occur when the system is dry, can it occur when it is submerged, can it occur when it is moist. What are the other environmental parameters that are important to this mode. We elicit this information and load it up into this data driven model. The way the model works is, it represents an outer container, an inner container -- a cladding or container for high level waste -- represents the corrosive processes on the outer container, on the inner container, represents where the inventory of nuclide is located within the system and it represents the release processes.

All of these things are defined as functions of what the environmental conditions are; how hot it is or how moist it is. We separately will talk about the variability in those conditions, the average temperature in the repository is "x", what is the 90th percentile and what is the tenth percentile.

That sounds very complicated, but you get a lot of clarity out of that because by dividing up what the variability is in environmental conditions you can very quickly establish that here's a category of containers that are susceptible to these models of corrosion but not to those modes of corrosion. They can't all fail by this mode and that mode because they are mutually contradictory.

We divide it up, we sit down with the experts and get them to give their current opinions about how each component works and we integrate it into this spread sheet. Here's a little sketch of, for example, the container failure. We will go through it and elicit a number of separate distinct modes of container failure -- as many as the experts feel are relevant for that waste package design -- understanding the rates at which those are likely to occur as a function of environmental conditions.

We can now do a system realization. We know what the flux is, we know how hot the repository got, we know the variability, and we can develop the entire postulation of container failures for that type of container and carry on in a similar way to get inside, fail the cladding and get the releases.

The second main component of the performance model is the pathways.

DR. DOMENICO: One point. That's the waste container failure. How about the release from the fuel rods that are exposed to the environment? You said nothing about that.

DR. MILLER: It works pretty well the same way. I am kind of skimming it over very lightly here. We have a model for the location of the inventory of each nuclide; is it free, is it in the gap, is it contained within the matrix. There is a representation of the transformation rate of the exposed fuel, the fraction of the fuel that is actually in contact with water. In the pathways a direct release pathway to get it out of the waste package EBS system into the larger scale pathways.

It's quite a comprehensive model which I am trying to touch on very lightly. We struggled quite a lot with pathways as everybody does at Yucca Mountain, because there are so many kinds of processes and they are complex and not terribly understood. In a top level model you are very resistant to getting tied to some specific physics model. So, this is a nice model of a particular concept of the physical processes, and we don't want a TOSPAC in a high level model because there are other things.

There are three dimensional effects and there are fractures that terminate and a variety of other things. At the same time we needed a model with a lot of flexibility that could really represent how the system will behave without being tied to some particular physics or low level model. So, we ended up with a very descriptive and phenomenological kind of a system that I will briefly run down.

What we define as a pathway is essentially a one-dimensional tube. We don't mean the nuclides are moving in straight lines. What we mean is that there is an input locus and there's an output locus and nothing can get in or out the sides. We talked about the pathway from the repository horizon down to the top of the Calico Hills, for example. It may be a mile wide, things may meander and they may go back and forth within it, but what goes in the top comes out the bottom. That's what we mean by one-dimensional.

The dominant process and the things that we specifically represent in our model in the pathway are advection, convection, diffusion and retardation processes. We don't directly represent dispersion for a good reason. Those are the things that we represent.

Then, a rather key concept is, we recognize that within a pathway there can be multiple advective modes operating. At a given time you may have matrix flow and you may have trickling flow going on. Or, you may have uniform fracture flow going on. In a gas pathway you may have different actual physical advective modes taking place.

The approach we take is kind of like we did with the waste package, kind of enumerate, divide and conquer. Let's characterize for a given pathway each advective mode separately. What is the velocity distribution of that advective mode and what's the retardation processes, how strong are they. Then let's evaluate the population of actual travel paths and let's get a distribution -- statistical distribution and not an uncertainty -- of the nuclide distances that are traveled in each mode.

If I release a billion nuclides into the top of the pathway some may find fast paths, some may spend a bit in the matrix and a bit in a fast path, and a lot may never find a fast path. Let's look at the distribution of those things and we are going to use that to characterize the pathway.

[Slide.]

DR. MILLER: Here's a slightly lopsided diagram. Conceptually what we showed here was what might happen if the experts told us that there were three modes of flow in the unsaturated zone at Yucca Mountain. There was a matrix flow, there was a uniform fracture flow; i.e., at a saturation within equilibrium with the matrix and so on, and what we call trickling flow. This is little local instabilities and narrow bands of saturation within a fracture system that as a whole is unsaturated.

On this little diagram this represents -- this point represents a pathway, a single path through our path where the nuclide spent its entire time in the matrix. This node represents a path where the nuclide spent its entire time in conventional fracture flow. This one means he trickled down that fault. A point in the middle represents that fact that of the distance he traveled it spent some fraction in the matrix and some fraction in conventional fracture flow and some fraction in trickling flow.

The concept we have is that the low level modelers can do realizations of what the system is like, how things occur -- this is always a function of flux and other environmental properties -- and can look at the distributions within the path of what happens. Not every nuclide is going to follow the same path. If they study that they can give us what I have contoured here what the population of particles passing through the path is going to look like. What we have shown here for example is something pretty similar to that earlier case study I showed you.

Most likely, most of the nuclides are going to spend about ten percent of their time in the matrix and the rest in fracture flow, but a few of them might start getting into a trickling mode which is going to be even faster.

This is a description, it's not a physical model of flow. It's our way of trying to integrate what we think the flow modelers can tell us. As the flux increases -- as the average flux increases we are going to see this density function move away from matrix flow towards trickling flow and then towards fracture flow.

DR. NORTH: Do you do this separately for different radionuclides, or do you lump all the radionuclides together?

DR. MILLER: For the advective portion, which is really what we are describing here, we lump them all together. Then each nuclide has his own retardation parameters.

DR. NORTH: Is there a provision for doing -- I will call them issues like colloidal transport with its implications for plutonium or organic complexation?

DR. MILLER: The way the model is structured -- because the parameters are in a database -- right now the retardation parameters are in that database. You can put them in as a stochastic parameter if you choose and say I am just not sure how much retardation or you can actually, instead of putting it in as a distribution you can put in a subsidiary model. You can say we may have colloid formation that is a function of PH and EH and something else and you can actually plug in, if that's what the experts feel is significant. A component model, so that the retardation process is modeled in more detail.

Our program is really a shell. We are just trying to make a very versatile way to express what the experts tell us and to integrate and see the consequences.

The third significant piece of the model that we have put together was the disruptive events and processes. Before I talk really about philosophy, here is a brief list of the major disruptive events that seem to be able to play at Yucca Mountain. We feel it covers most of what is out there. You can look for other ones, but most of the hypotheses and concepts that have been developed represent a fairly short list of things. How they occur and what the consequences are can be quite complex. The list isn't that long, so we have a database of disruptive events.

I won't go into any detail about them. The issue is events and event consequences. A useful thing happens with event consequences in terms of a model which is, there aren't a lot of flavors of event consequences. While the list of events was surprisingly short, the list of consequences was even shorter. What kind of thing can a disruptive process do? Well, it can disrupt some waste packages somehow and change them more or less instantaneously. It can change a water table, climate changes could change infiltration rates, hydraulic gradients could change.

Because all of these are represented by parameters in our model we can just change those parameters if the event occurs. You can cause direct release to the accessible environment. You can dig it up or down to the saturated zone. You may create a pathway that drops some waste down there.

Our general approach is, let's try to categorize the sorts of causative events and get some understanding of their probabilities, let's categorize their consequences, and let's see if we can span that space completely. Rather than the sort of infinite list of scenarios, what we try to do is cover the entire space of feasible scenarios and sample it within our Monte Carlo system.

Here's a little diagram just making a simple point, if this is the space of all disruptive events and processes we don't care about them all. To begin with, they are not all credible. That gave us a subset. Of the credible events and processes, only a very few have any consequence in terms of performance. What we are really interested in is this intersection set. What our responsibility is when we put out a CCDF and we say this is a real probability and we are not just playing this time, it is to convince an audience that we span that space properly. We got everything that's in there and we represented it adequately.

The way we do that in a model, for a particular type of causative event we elicit the flavors of that event. In what ways can that event manifest itself. For a volcanic event we have these three flavors; extrusive strombolian, hydro volcanic and an intrusive event. If there are more flavors you just add them into the system.

We have a finite list of the kinds of consequences of an event. It can change our parameters, it can change our waste packages and so on, the things that I talked about earlier. So now we say if this happened, this flavor of the event happened, what is the probability of each of these things happening and at what intensity. How much would happen? We elicit that as expert judgments and away we go.

The process is, sit down and talk to the different experts in the different disciplines -- human intrusion probably does not count as a discipline but we need it anyway -- and elicit what flavors could human intrusion take, run down as many as you think are credible, assign probabilities to those and then for each flavor of human intrusion what kind of consequences could it have and assign some probabilities to those.

It's a pretty direct and head on unsophisticated approach. Let's just enumerate it all and crank it through our model. The nice thing about Monte Carlo is that you know it's tedious and you have to do a few thousand iterations. It doesn't matter how complex your system is. You can have a system with a million processes in it and you still only need to do a few thousand Monte Carlo iterations. In the end, you can grind as fine as you like in getting credible detail inside your model. It's a black box. In Monte Carlo you sample it ten thousand times and there's a CCDF.

DR. NORTH: There is a problem on that approach and that is, how do you get the details of the distribution? As you get closer to some realistic applications, I would like to hear what your strategy is so that you can in fact explore that tail where we might violate the EPA regulations without having to run an inordinate number of samples. Stratified sampling is one way that one can get around this. I don't know what you plan at this point.

DR. MILLER: We recognize that, and we don't have a magic bullet. What we did put in our code is a flavor of what we call bias sampling which is, rather than doing Latin Hypercube, each realization of a parameter can be biased towards the extremes of the distribution. When we sample we sample and we record a bias weight. We can take a particular parameter and say sample the lower and the upper decile proportionately higher than the middle section and retrieve along with the sampling what the weighted value of that sample is.

When you do that, you can get about an order of magnitude improvement in convergence on the tail which is still looking at a pretty horrendous computational effort when you run that.

DR. NORTH: Another thing you might consider is, what you have on your slide there is referred to by a lot of people in decision analysis is influence diagram. You can calculate them analytically and work out the probabilities of the various pathways you have shown. You may want to drive some of your Monte Carlo with that analytically. So again, you can think of it as stratified sampling. You are running Monte Carlo in one portion to get a conditional distribution. Then, the probability of getting into that distribution you might calculate in it analytically through your influence diagrams.

DR. MILLER: I think there are places we could do that. These are actually PDF's. If this type of event occurs, whether there is a direct release and how much direct release is expressed as a PDF -- discreet sampling -- unless the experts said it was a discreet thing, it either happens or it doesn't happen.

DR. NORTH: Yes, you could make it into discreet pieces. That, of course, makes it more complicated. The point that I am raising is that I think as you evolve this you need a computational strategy along with the principle of using Monte Carlo so that you can make it work for the level of complexity that you are going to have and not spend too much of your time sampling from the part of the distribution that's not very interesting.

DR. MILLER: Absolutely. I couldn't agree more. One of the areas that I skimmed over pretty quickly was the waste package model. That can get very complex in terms of environmental parameters, and we will probably use a Latin Hypercube sampling within that component of the system to make sure that we span that space consistently.

[Slide.]

DR. MILLER: This is just a generalization of the approach to events. Generically you talk about the types of events. Here is event group A, human intrusion as asteroids or something. You talk about the types of event occurrence. If the asteroid hits it can do this and do that or do the other thing, and you assign probabilities to the type of occurrence. These are discreet. It either occurs or it doesn't occur.

Note that more than one consequence can happen. You can have a volcano go off and it can erupt, and it can create a dike. Some of these conditional probabilities can be greater than one. Now we know the type of event occurrence and we have PDF describing the consequences, and those directly affect the model. The model walks through time in each Monte Carlo simulation and if the volcano goes off at 1,700 years, that's when it changes things.

That summarizes the model. With what is there, we intend to do a fully integrated system model later this year. I will talk about our calendar in a minute. That's not the whole thing though. That's just actually the engine. You will learn interesting things from it. The characterization activities and the ways in which our predictions can change are really the purpose of this exercise. I want to talk briefly about that database of activities.

[Slide.]

DR. MILLER: We have had some trouble coming up with a nice sharp list of activities and, obviously, for a program that's being sort of stop and start and where are lot of things are still in flux and decisions are still being made as to what ESF will look like, it's a bit much to ask and give you a very crisp shopping list of the things that they intend to do.

Here are the kinds of sources of information that we have identified. The SCP -- lot's of information and study plans generated from it -- pretty well all describe and catalogue within the paratrac database. We think we are going to get a lot of value out of that. When we sit down and talk to our pathways experts and they say we want to talk to them about a particular study -- they are interested in fracture flow -- we need to say what are the activities that are planned that would affect fracture flow. We can pretty well identify those by going through the Paratrac system. It has a list of parameters and we can look stuff up.

However, that is not the entire project. There are lots of other activities that are not exactly site characterization but are very relevant. In particular, the SCP gives pretty short shifts to waste package things. Those ancillary things are defined by scientific investigation plans. We have a list of those and we also have to look through those as well as through SCP paratrac to look for hits, to look for activities that are planned that could affect our predictions of performance.

We can get into the planning and control system through the work breakdown structure number, and that basically has scheduling and budget information for everything that is planned to be done. Someone from the project correct me if I am wrong about all this.

Something that none of this really has is precedent requirements. You can't do that test until you have done this thing. You have a ramp down there and can't do the test. So, we really need to get into some of the design documents in order to be able to get those requirements. There is no sense us simulating an alternate variation of the characterization program that is impossible.

The model we have not only does the basics I showed you, it also adds up the cost and the elapsed time to do it. We can start playing some games and saying what if you removed all this, do you save any time and do you save significant money. This is, again, the same flow of information.

The PA model gives us a list of input parameters which came from those experts. Off we go. We look at the scientific investigation plans, SCP paratrac and study plans, find WBS numbers, start following down the chain of information and eventually find the activities that we need to put in our database. These are the things on the table and these are the things that can be carried out.

I have just about used my time. We are trying to make up for lost time which is luck. At the moment this operative element is about one-half done. We are projecting having it finished by the end of July. The little dots represent the reality of developing software. We are really ready to start the process of expert elicitation, and we intend to utilize both project and ex-project resources. We are not going to be doing major 50 person type elicitation but we are planning to hold a couple of workshops on each of our topics through the summer. We expect to have a handful of project people and two or three outside academics or whatever as we go over each particular topic.

We need those inputs around this timeframe in order to do sensitivity analyses to see what happens if you don't go into the Calico Hills or if you add something else and so on, and we will try and have a draft report done by the end of the fiscal year.

That's our schedule. We have some very good feelings at the moment about the feasibility of this system. It's a pretty direct and maybe simple minded approach in a sense, but it really looks like it will all fit together and run. We will get interesting and useful results out of it. Are there any questions?

DR. NORTH: Comments or questions?

[No response.]

DR. NORTH: I guess one comment that I have on the scope of your effort, I am very pessimistic that in the course of two months for expert elicitation and maybe the order of two months -- if I read your slide correctly -- for analysis, are you really going to be able to do anything more than let us say a pilot or shakedown of your total systems modeling approach. There are a lot of very interesting questions that one can ask about the risk or performance assessment for the repository, and I am not sure you can do a very good job of exploring that space in a few months.

On the other hand, I think what you are trying to do would be very useful as a top down synthesis over the whole area and getting some perspective perhaps that hasn't really been a major part of the DOE program. I think you may add something very important to the overall DOE effort. I would like to see some thought go into what follows this exercise and how can you use this exercise maybe to define some good questions for a Phase II.

DR. MILLER: I concur with everything you said. I think we feel it is incumbent upon us to complete the exercise, to produce a pretty credible product for review and establish how valuable is it, will it work, is it credible and so on before we really recommend people getting more heavily involved with it.

At the same time, even with a very simple model you can happily spend a couple of weeks learning things, studying it and seeing what the sensitivities are and how it works. Once you have a true integrated model together there's just going to be a wealth of information.

DR. NORTH: One thing I think I would do if I were in your position is, I would read very carefully the EPRI report on their exercise and what the NRC has done in their pilot performance assessment exercise, and I would try to emphasize some things that neither of those efforts covered; explore part of the space they haven't been in and think about what guidance you can provide for larger efforts to follow within DOE. And then, help them aim their efforts in a way that would be more productive.

DR. MILLER: Thank you.

DR. REITER: Just as a follow on, and this is more a question for DOE than you. What role are you playing or will this study play in the DOE hierarchy of performance assessment. Yesterday we heard about a total system analyzer, we heard about SMS, we heard about Spartan, PNL studies.

Perhaps there is somebody from DOE or headquarters or something that can give us some insight into that.

DR. DYER: As Ian said, he has been working directly for headquarters. This started as an itiative from John Bartlett. I believe that he is still receiving instructions through John. From the project office we have not much in the way of interactions with Ian except for providing resources as he needs them.

I can't address cost schedule type things for Ian, because I haven't been involved in them.

DR. NORTH: Is there anyone here from DOE headquarters that might like to add a few words on this subject?

[No response.]

DR. NORTH: I presume then the answer is no. I think this is an issue that we might want to return to in the roundtable this afternoon.

I think we are ready to proceed with NRC. We are going to hear first from Seth Coplan. Seth is leader of the performance assessment section of Geoscience and Systems Performance Branch in the Division of High Level Waste Management. You will proceed and then introduce your colleagues.

DR. COPLAN: As Warner mentioned, I am the first of several NRC speakers here today. I am going to be followed by Norman Eisenberg, Dick Codell and Tim McCartin, who over the past year have done kind of a pilot performance assessment and they are going to be talking about that.

Before they talk though, I am going to try to give something of an NRC perspective on performance assessment. We are a licensing agency, and in that context we have I guess certain ways of looking at performance assessment in the context of the decisions that we are eventually going to have to make. I would like to try to convey today some of this coloration and kind of environment that we work in.

[Slides.]

DR. COPLAN: In that regard I am going to talk about what is performance assessment from our perspective, what the NRC and DOE roles are in performance assessment for our licensing, how our licensing process works and then some thoughts about expert judgment and expert elicitation in that context.

[Slides.]

DR. COPLAN: Performance assessment from our standpoint is a quantitative analysis of repository or subsystem performance that in general addresses three questions. What can go wrong, how likely is it to occur, and what will be the outcome. It will be the primary means for showing compliance with the performance objectives of 10 CFR Part 60; that is, the EPA standards and the individual barrier objectives.

I am going to concentrate through the rest of my talk on how it would relate to the EPA standards just for purposes of simplifying the talk. Since, in a way, that's really more complex from the standpoint of the kinds of analysis it brings into play I think it will cover the issue.

[Slides.]

DR. COPLAN: Performance assessment is really a series of -- bad choice of words. It's not really a series, it's a set of closely related subanalyses. In this diagram I am trying to show the various subanalyses that are involved in it. I would like to walk through them here briefly.

First of all, you have to have a description of the system that you are going to do the assessment of; the site, the waste form, the engineered barrier system, in a way that you can do the performance assessment. The first analysis is then one of taking the data from the various technical disciplines and somehow distilling that into a description of the parts of the system so that you can do this analysis.

A second analysis is the scenario analysis. In that, one is trying to describe possible future states that the system may be in and the probabilities of those future states. In turn, you would screen out the scenarios that are either not credible or that have consequences that would be so inconsequential that they are not really affecting the performance of the system.

To address the question of what would be the outcomes, you have to get at the consequence analysis. There, you examine the way the waste package has failed, the way the waste enters the flow system and is ultimately transported to the accessible environment. This, of course again -- I guess I should have mentioned over here that the scenario analysis involves an integration of information from a variety of different disciplines.

Again, you have that happening here, several disciplines having to come together in developing the models that are used to actually calculate the consequences of these various scenarios. When you do the consequence analysis you take probability information, consequence information and using a system code as we did you can develop risk curves. Very important part of all this analysis is the sensitivity studies and uncertainty analysis in which you, through sensitivity analysis, examine what the important contributors are to the behavior of the system.

The uncertainty analysis relates the uncertainty of the inputs to the uncertainty in your final result. Finally, you take this and make the comparison to the regulatory standards. One point that I would like to make with this is that this, together, is a performance assessment from our standpoint. If you tried to nibble away at the parts of it without trying to do the whole, you don't really get the full picture to see all the assumptions you need to make, to see all of the data that you need to do the analysis, to see all of the models that you need to develop and that you have to relate back to data from site characterization. You got to do the whole thing.

[Slides.]

DR. COPLAN: Just to further cement that point, performance assessment is a systematic analysis involving related subanalyses, several technical disciplines and a variety of data and has to be integrated across the subanalysis, across the technical disciplines and with site characterization.

DR. NORTH: I can't resist throwing you a straight line. Would you like to comment on how one makes it integrated; how does one get there?

DR. COPLAN: By doing it and by iterating it. It's very, very hard to get that integration. Crossing all the different disciplines really takes some effort. Factoring it into site characterization and, in turn, factoring what you get out of site characterization into the models that you develop and the assessments that you do again. It's a difficult thing.

It's hard to just kind of sit back and conceptualize how you do it. The doing from what we found with our first cut is a lot different than the thinking about it.

DR. NORTH: Thank you.

[Slides.]

DR. COPLAN: The roles of the NRC and the DOE in all this are that DOE has to do performance assessments to show compliance with 10 CFR Part 60 which implements the EPA standards. NRC will evaluate DOE's performance assessments making its licensing decision.

We are not going to do our very own performance assessment, put down side by side with DOE's in competition. We are using performance assessment as a tool to evaluate the performance assessment that DOE does.

[Slides.]

DR. COPLAN: I am going to take a little time now to explain how the Nuclear Regulatory Commission does all this. To start with I better introduce all the participants. The agency consists of the Commission, which is a five member panel that consists of some people who have technical expertise, lawyers, and they are appointed by the President and approved by the Senate and serve fixed terms. They cannot be removed other than for various kinds of really egregious behavior.

They are fairly politically insulated, is the point.

DR. NORTH: What is the length of the term?

DR. COPLAN: I think it's five years. The Licensing Boards are employed by the Commission to make an initial licensing decision. I will get into how they do that in a few minutes. The Licensing Board consists of three members; a Chairman who is a lawyer and two technical members. Finally, in this particular scheme of things there's the NRC staff, which does the technical evaluation of the application. For the most part, ACNW and the NRC staff in those two bodies the technical expertise in the Agency resides.

DR. ALLEN: Is the Licensing Board drawn from outside of the NRC?

DR. COPLAN: No, it's within the NRC. There's actually a whole organization within the NRC that is called the Atomic Safety and Licensing Board, and it consists of a number of lawyers, technical people, some support staff. For any particular proceeding one of the lawyers is selected as a Chairman and two of the technical people are selected to fill out the rest of the Board.

The ACNW is an Advisory Committee to the Commission. The specific role in the decision making is not spelled out in the regs for the ACNW but they review staff work and they comment on it. Clearly, their views are going to be a part of the licensing decision.

DR. PRICE: Can I ask you on your overhead -- maybe I missed something. Is that Licensing Board plural?

DR. COPLAN: Probably Board would have been best. The process -- just kind of a quick overview. The Commission is the legislatively designated decision maker. The Licensing Board makes an initial licensing decision that is subject to Commission review. The NRC staff makes a recommendation to the Licensing Board based on its independent technical evaluation of the license application. Clearly, in the area of performance assessment that would be based on the performance of the DOE's performance assessment.

Staff, in doing this evaluation, tries to get a fairly broad range of information to work with. We work with the information that is in the application, our own knowledge, we have contractors and consultants, we use the technical literature and try to get in touch with anybody who may know something that may help us out.

Given that information we get into kind of the guts of the review. With the range of sources that we consider to say conflicting views arise, I think it's pretty inevitable that they arise. We evaluate the conflicting views through examination of the supporting analyses for these various views. For each such analysis the staff considers the quality and completeness of the data that were used, the reasonableness of the assumptions, and the soundness of the reasoning.

We may do an independent analysis as part of this evaluation. We may, in some instances, pick a bit of information from one source, some assumptions from another, and some reasoning that party three applied and synthesize it in our own way and use that as a way of having in this case the Department of Energy address these various points of view and various interpretations of the situation.

When all is said and done staff has to take a position to recommend, and that position is one that will be sufficiently conservative that any adverse consequences would not be likely to be underestimated. The key issue for us is whether there is reasonable assurance of no unreasonable risk to the public health and safety which is the test that is in our regs.

With that, let's talk about the Licensing Board. The Licensing Board makes its decision on the basis of the hearing record. The hearings are conducted in an adjudicatory format; that is, they involve several parties and they focus on specific issues. They involve the different parties which, in this case, would include the DOE staff and the state, and other affected parties, putting forward information to support whatever position they have on various issues through testimony by expert witnesses.

The testimony is cross-examined by lawyers and a hearing record is compiled. The burden of proof in this proceeding rests with the DOE. In general, Licensing Board's evaluate the conflicting testimony much the same way that the staff does. Actually, the relationship is not coincidental. We try to approach the problem the way we would expect things to work out in a hearing. That is, they look at the quality and completeness of data where there are conflicts, the reasonableness of assumptions, the soundness of the reasoning and, in fact, the Board at times will ask questions that will try to get from one of the experts what his thoughts are about the assumptions that another expert has put forward or the reasoning and so forth.

The Board tries to use the various experts to validate each other. The Board bases its decision on a composite of testimony in the hearing record. By that I mean that the Board may do something quite similar to what I described in the staff analysis of taking data that Witness A uses, combining it with assumptions made by Witness B and using the reasoning that Witness C applied to the problem.

DR. NORTH: As a matter of clarification, can the Board bring in other experts beside those on the NRC staff and the various parties?

DR. COPLAN: Yes, they can. In fact, the Board has subpoena power. Again, the Board's decision is going to be sufficiently conservative to assure that adverse consequences would not be likely to be underestimated. He issues reasonable assurance of no unreasonable risk to the public health and safety which is what is in the regulation as the test.

The Board's decision is an initial decision that is subject to Commission review.

DR. NORTH: Presumably, the Board is bound by the regulation and must use the regulation as a basis for its decisions; is that correct?

DR. COPLAN: That is correct. It is possible to do what's called an exception to the regulations under certain circumstances where an applicant is able to make a case where there should be such an exception. Generally, the Board tries to stay within the regs.

DR. NORTH: Supposing there is ambiguity in the regs or conflicts between parts of the regs. Have these situations occurred in the past and how have they been resolved?

DR. COPLAN: What is done is, I really kind of oversimplified the use of the hearing record. Once the part of the hearing where all the testimony is taken is completed, then each of the parties prepares kind of a proposal to the Board that's called findings of fact and conclusions of law. If, during the course of the hearing there were questions about what some part of the reg meant came up, then in these findings of fact and conclusions of law each of the parties would present their interpretation of what the reg means and what they think in the regulatory history and so forth supports that particular interpretation.

Then, the Board will make a decision about what the reg means just as they would make findings on the technical facts.

DR. NORTH: One of the concerns I have about this whole process is that I worry about something that is this complicated in terms of the regulations and technical issues being played out for the first time with stakes that are as high as the stakes in this situation. It occurs to me that there would be a lot gained by some practice, just as with performance assessment.

DR. COPLAN: I think there is some merit to what you are saying.

One of the issues that has kind of surfaced over the last couple of years is the matter of expert judgment in licensing; how does it fit in, how can it fit in, how much is tolerable and so on. With the background that I hope I have provided here, I would like to spend a few minutes speaking to that.

In support of its license application DOE has to show that the technical criteria of 10 CFR 60 will be met. That support has to undergo staff and Licensing Board review and perhaps ultimately Commission review. Keep in mind the makeup of the Boards and the Commission. They are not a full sweep of all the technical expertise that is involved in this. There are technical people that are there, there are people that are capable of technical understanding but are not experts.

They are the ones that are going to be making the decisions. They have to understand the basis for the decisions that they are making. A lot of the supporting analyses that DOE is going to do are going to rely on expert judgment. I think that's one of the givens here. In fact, when I was talking about the staff review and the way the Board works its way through testimony they are examining these judgments in terms of assumptions and in terms of the data used and the reasoning.

The use of expert judgment is not something that is new to NRC. It has been used before. What would be new is a potential use of expert elicitation as a substitute for obtaining more objective information. In other words, something that can be tied more tightly to assumptions, data and reasoning and also, as a way of kind of combining differing views rather than go through this kind of examination of assumptions, data and reasoning.

DR. ALLEN: Why is this different than the same kinds of issues that come up say in nuclear plant licensing? I don't understand the difference.

DR. COPLAN: I don't think it is different. The thing that we think we are seeing is an inclination or tendency on the part of DOE to be leaning in the direction of using expert elicitation where the data are thin, where maybe they didn't get quite enough information about something during site characterization, and we have concern about that potential.

The reason that we have raised an issue relatively early in this regard is that we prefer that the DOE in planning site characterization be thinking in advance about the needs for data, all the various needs for data and information. When they come to the review and the hearing board they have a good, tight case, that relies as much as possible on objective information. You still look puzzled.

DR. ALLEN: My only concern is that yes, that's a very real problem in anything like this measure. It is equally as much a problem in some of the nuclear power plant applications where the data simply are not available to the same extent you might like.

DR. COPLAN: I think that's true. I think that's true. There's another facet of this. The way that licensing has been done with power reactors in the past has been primarily with deterministic criteria. The kind of analysis that would be done for performance assessment really gets into a range of calculations and analyses and so forth that just push the state of the art across the board. There is probably going to be more of a tendency to get into areas where you are at the fringe, and it has to deal with the kind of facility too. With a power reactor you are trying to predict the way it is going to be behave to the extent that you do make predictions for 40 years. With a repository you are looking at spatial scales that are distances of kilometers and at least a 10,000 year timeframe.

The areas where I think you are going to need to bring judgment to bear are much broader and a potentially bigger problem, I think.

DR. NORTH: The other concern that I will reiterate is that we are going to do one hearing, one procedure on a repository, as opposed to an opportunity to do multiple power plants involving a large number of people and formalizing procedures in process and getting a large section of professional community involved in the process.

To me, one of the central issues in terms of making this process work is getting some activity mobilized in advanced so the thinking is done early and some of the issues are surfaced early.

DR. COPLAN: That's right.

DR. NORTH: A lot of people can participate on a knowledgeable basis as opposed to we get into it and something goes wrong, and then at that point it's very difficult with all the lawyers involved to fix it on a technical basis.

DR. COPLAN: I couldn't agree more.

MR. ECHOLS: Seth, I was thinking that you might go over the fact that both NRC and DOE are contemplating a series of rulemakings on some of the issues -- you generate the generic technical positions. A lot of things are going on to flesh out the technical issues and, hopefully, resolve some of them. So, when you get to the hearing the question isn't the appropriateness of a model but really just the input to the model and the analysis of the results of the model.

The model itself, hopefully, that issue will be resolved long before the hearing. That's one example. Both sides are generating their wish list, I think, on appropriate issues to be resolved in rulemaking and the activities you are doing on technical positions.

DR. COPLAN: I am not sure that I can add too much to what you said other than yes, the NRC staff does develop and issue technical positions which are statements by the staff on what would be an acceptable approach to the staff by the Department of Energy in dealing with some technical issue. Rulemaking is a more powerful tool, because a staff technical position really can be questioned in a hearing during rulemakings.

You need a technical basis to do the rulemaking or to do the technical position.

DR. PRICE: Seth, can I ask what is the role in all of this of NRC's policy statements and guidelines, documents that they might put out, such as they put out for reactor control rooms and so forth?

DR. COPLAN: Policy statements are statements by the Commission that -- let me maybe start with saying a little bit more about a regulation. A regulation is also really a policy statement by the Agency, by the Commission. They carry the force of law. A policy statement is also a policy statement by the Commission that does not carry the force of law, but it certainly gives a clear indication of what the Commission is thinking and what is the Commission's approach in a particular area would be.

Remember that the Commission is the ultimate decision maker. Any kind of policy guidance that it issues is going to be very closely listened to by the staff and by licensing boards.

DR. PRICE: You said that a policy statement from the Commission does not carry the force of law?

DR. COPLAN: That is correct.

DR. PRICE: How about guidelines documents?

DR. COPLAN: I am not sure specifically what you are referring to. It's not something I have encountered before.

DR. PRICE: For example, there are design guideline documents for control rooms, specifically human factors guidelines documents issued by NRC. They were basically criteria type documents, at least to give the utilities guidelines on how they might human factor their --

DR. COPLAN: I am not familiar with that one. Frank, can you help me?

DR. ROWSOME: I used to be in the regulatory staff of the NRC. I am a consultant today. Below the level of rules, formal regulations that are published in the Federal Register and that are controlled by the Administrative procedures Act, the NRC uses a hierarchy of two levels of guidance below that. The regulatory guide is a formal process which is subject to a formal protocol of review prior to promulgation, review and comment, prior to promulgation, which does not carry the weight of law but has a kind of prima facie, pseudo legal basis.

Technically, there is nothing in the Administrative Procedures Act that authorizes the issuance of such documents. The industry has accepted them as quasi-legal formal guidance. Do it this way unless you take upon yourself the burden of justifying an alternate way of doing things.

Below that are informal guidance documents, most of them published as NUREG reports, which have even less legal stature and are simply technical findings of the regulatory staff or the research office. Given the authority granted the Commission to issue licenses according to its own discretion, many of these have considerable weight de facto, even though they have no stature under the law.

Applicants have found it necessary to take these very seriously, and it's been a subject of some contention in reactor licensing; that the staff has had these vehicles without any -- that afford the industry really no avenue of appeal but to go along with all the guidance that the staff can rather cavalierly throw out without much quality control.

This process of generating the lower level documents has been one that has been very controversial in the reactor arena for year but has, nonetheless, helped to establish a common understanding between the regulators and the regulated of what the shape and character of what this dialogue and process will be.

DR. COPLAN: One thing I might add. When Stan Echols and I were talking a few minutes ago I mentioned technical positions. In the repository framework we have been doing what we call technical positions rather than the regulatory guides. The intent is that they would fill the same purpose; in other words, be a statement of staff position that is binding on the staff. In other words, if the Department of Energy chooses to do something the way it's described in the position it's binding on us.

They are issued at the same level in the Agency, namely the Director of the Licensing Office, as the regulatory guides but there is a little less procedure involved so that you don't get quite as gummed up internally in getting them out as the reg. Guides can.

[Slides.]

DR. COPLAN: Turning to the last chart here and picking up again on expert elicitation, some of the thoughts that we have had on it is that where more objective information is not otherwise obtainable -- and expert judgment is an important input -- expert elicitation may be a useful means for clearly defining the problem that is being addressed for establishing a documented basis, for the deliberations about it, and for sampling the state of knowledge.

However, an assertion by DOE that any specific elicitation represents the state of knowledge is something that we scrutinize critically to make sure that there really is a cross-section of opinion. Judgments about lower level, less aggregated issues, are more readily evaluated. In other words, judgments at the level of porosity information or something that we can work with a lot better than judgments about groundwater travel time.

Finally, algorithms for combining expert opinions, in other words, combining opposing views as opposed to evaluating each opinion on its own merits would be viewed with skepticism by the staff. We look for some justification as to why that's a better approach to dealing with the matter at hand than our old standby of assumptions, data and reasoning.

Thank you. Are there any comments or questions?

DR. NORTH: Are there questions or comments?

DR. BARNARD: Seth, how many reactor licenses have been denied by the NRC?

DR. COPLAN: I can think of only one off hand, and several applications that were withdrawn during the course of staff review.

DR. BARNARD: Has there been any consideration of opening up the Licensing Board to outside experts rather than just internal NRC employees?

DR. COPLAN: You mean the people that sit on the Board?

DR. BARNARD: Yes.

DR. COPLAN: The Licensing Boards, they are inherently going to be NRC employees.

They are not NRC staff employees. It's a separate piece of the Agency, and there are a whole variety of regulations that just deal with the way the staff and the members of this panel can communicate with each other. Effectively, it's an independent type of arrangement even now.

Are there other questions or comments?

[No response.]

DR. NORTH: Let's take a break, and try to be back by 10:15.

[Brief recess.]

DR. NORTH: Shall we resume? We will now continue to hear from NRC with Norm Eisenberg.

DR. EISENBERG: I am Norman Eisenberg. Three of us from NRC would like to describe a performance assessment demonstration that the staff conducted at the NRC. Two of my colleagues that worked on the study -- the three of us are not the only people that worked on the study -- will also speak today.

I would like to give an overview, and then Tim McCartin will talk about the flow and transport modeling. Then Dick Codell will pick up and talk about source term, sensitivity and uncertainty analysis. Then, I will come up one more time to provide some summary conclusions. That's the order that we would like to go in. This, hopefully, will prevent a lot of up and down on our part.

[Slides.]

DR. EISENBERG: What I would like to present in the overview are the purpose of our work, the scope and what we actually did. Then, when I return, I would like to talk about the tentative results and some needs that we see for modeling improvement at least in the work that we did.

[Slide.]

DR. EISENBERG: As I said, this was a demonstration effort. That was the primary focus of the work, was to demonstrate that the staff had a capability to conduct a performance assessment and to what extent they did have that capability. Some subsidiary purposes were to take a look at the suite of tools that we had to do performance assessments, to thereby give us some insight into what tools needed further development or what new tools were needed. Also, to give us some insight into the site characterization program being conducted by the Department of Energy.

[Slide.]

DR. EISENBERG: At the start we had in mind to do a preliminary performance assessment. There was no intent that this would be anything near what you might see towards the end of the program when the license application was being evaluated. One of the ground rules was that we were going to use the tools that were available if we could make do with them, and only do additional code development if there was no other way to avoid it or unless, as it turned out, it was more expedient to actually sit down and write codes than try to modify preexisting ones.

Of course, we were limited by the amount of site data that is available at the present time. We tried to take advantage of as much of it as we could. We had the problem of trying to fit the analysis into the available time and resources. What that meant in fact was, in some cases we did less of an analysis. We tried to go through each step of the process at least to some degree, once again, along with what Seth said earlier, with the belief that only by putting together the entire analysis would you be able to see where the problems were.

Finally, we focused on the EPA containment standard, although we did look a little bit at the subsystem requirements in the NRC regulations. That was not the primary focus of the work that we did.

[Slide.]

DR. EISENBERG: We have what we probably should have copyrighted, our list of caveats. First of all the numerical results that we got do not represent and are not intended to represent the proposed repository at Yucca Mountain. We are beset by large uncertainties in the data and the modeling. We only used a limited set of scenario classes. That's another reason why the results cannot represent the performance of a repository.

The waste package failure model was non-mechanistic and fairly simple, and some of the modeling of flow and transport in the far field was an approximate indirect approach to looking at flow and transport in the unsaturated zone.

[Slide.]

DR. EISENBERG: We have already seen this figure once before. I think Seth walked through it, and I don't need to add much to it except perhaps to point out that one of the areas that we really did much less than we would have liked to do was in the scenario analysis part. We have only began the analysis there and really didn't get into it to the degree we would like. However, given the scenario analysis that was performed, we did carry the calculation all the way through to producing CCDF's and did sensitivity and uncertainty analyses.

One other point on this chart. The system description was not a separate activity that had a large amount of effort devoted to it. First of all, a lot of the system description is present in a number of DOE documents including the SCP. What essentially was done -- this was not a separate task but that each part of the analysis that required some interaction with the system description was responsible for acquiring it and using it.

[Slide.]

DR. EISENBERG: For the CCDF that we calculated, we include explicit quantification of two kinds of uncertainty; the data and parameter uncertainty and the future states of nature in which the repository system has to operate. Although we did recognize that there are a great many model uncertainties, we did not attempt to quantify them nor did we fold the uncertainty in the model into the CCDF and, in fact, I am not sure the NRC staff believes that's something that should be done.

Once again, in line with what Seth was saying earlier, if you fold model uncertainty into the CCDF it is difficult to piece out where the different uncertainties come from and is hard for a licensing board to examine the source of the uncertainties and their magnitude.

[Slide.]

DR. EISENBERG: This is a figure that we stole from DOE and modified. The accessible environment normally would be the surface of Yucca Mountain and ten kilometers on either side which, if this overlay works, is the part in brown. To simplify the modeling we took the accessible environment as this smaller region and cut it off at the water table. This meant that we ignored transport in the saturated zone. We believe that's probably a conservative assumption.

[Slide.]

DR. EISENBERG: Walking through that flow diagram now. We looked at four scenario classes. This is a way to look at scenarios -- by no means the only way. This is Latin square approach where we looked at the presence or absence of two fundamental causes of changes in the states of nature. We looked at human intrusion by drilling which is denoted here by "D" or no drilling by the not-D. We also had either normal climate or pluvial conditions.

Then, of course, if you have these two fundamental conditions you have four fundamental classes of things that can happen, which are represented by the four areas on the Latin square. We assume that these events were independent, so the probabilities are obtained by just multiplying the probability of the individual types of events by each other. To estimate the probability of drilling we used the guidance which, of course, is only guidance, in Appendix B of the EPA standard which gives a drilling rates for non-petroleum resource areas or geologic formations.

Using that drilling rate it turns out that the expected number of penetrations of the Yucca Mountain are around 15 over 10,000 years. The probability of having drilling is very high and it's about one. The probability of having no drilling is small.

For the case of climate change, I am fond of saying that I determined the probabilities by consulting fish entrails. That's about as good as those numbers are worth. They do sum to one, however.

[Laughter.]

DR. EISENBERG: We get the resulting probabilities. You will notice that these two scenario classes have probabilities much higher than the ones without drilling because of the high probability of having drilling, and these are the ones that dominate the CCDF that was generated. If one were doing -- and we didn't get to it -- if one were doing a scenario analysis, chances are you would drop these scenario classes because their probability is -- you might screen these out because their probabilities are so small.

[Slide.]

DR. EISENBERG: Let me go from the scenario side of the flow chart to the consequence side. We looked at three pathways for release of radionuclides to the accessible environment, liquid pathways through the groundwater, gas pathways up through Yucca Mountain and direct releases by human intrusion. For the CCDF we only calculated releases by direct release. By the liquid pathways we did some estimates of release by the gas pathway but they were not incorporated into the calculated CCDF.

[Slide.]

DR. EISENBERG: Just to illustrate the kind of thing that we did for the liquid pathway -- for the no change in climate, we had one distribution of infiltration rate. For the pluvial case we went in and modified the distribution describing infiltration rate. We also raised the water table, which meant that there was less travel time through the unsaturated zone before the radionuclides reach the accessible environment, or what we were calling the accessible environment.

DR. DOMENICO: Was that new water table an outcome of the increased infiltration rate, or was it an assumption?

DR. EISENBERG: It was an assumption. The other thing I want to mention is that within each scenario class we examined variations of the parameters because of the uncertainty in the parameters, and looked at these parameters with these distributions. I should point out that some of these are multiple distributions. For the ones describing the hydrologic conditions, each hydrologic unit has a distribution. For the ones that are tied to a particular radionuclide, each radionuclide has a distribution.

We did do parameter variations in order to generate the CCDF.

[Slide.]

DR. EISENBERG: This is a kind of a result we got. Here are the four different scenario classes and a combined CCDF. An easy way to visualize things, remember the CCDF is the probability of a particular size cumulative release. The way these graphs are plotted, it's the probability of a cumulative release for a given scenario class. You can generate the ordinance on the total curve by merely summing the probabilities that you read off of each component curve at that particular level of release.

[Slide.]

DR. EISENBERG: Two more curves. This shows the composite curve a little more clearly. You could say that the curve exceeds EPA standard points in two places; one at the .01 probability level and the other in the ten to the minus three probability level. That's relatively inconsequential, since we are not taking the exact value of the numerical results seriously.

In addition to getting estimates of performance, an important part -- at least we feel is an important part -- of the work were auxiliary analyses that looked at the assumptions that were used in deriving the estimates of performance or the process data prior to putting parameters or distributions for parameters into the models that were used to generate the estimates of performance.

In particular, there was an analysis of release of Carbon 14. If it had turned out that it looked like Carbon 14 releases were unimportant, that would have indicated that maybe we could go with the curve that we generated. It didn't quite turn out that way, so our conclusion was otherwise. We looked at how many samples are needed in order to get a good estimate of the CCDF, especially in the low probability end. We looked at the hydrologic data and some of the statistical parameters that describe it. We examined the assumption that was used that you will hear about in a minute from Tim that the flow was vertical by looking at the potential for two-dimensional flow. It turned out that there was a potential for that.

Unless there are any questions, I will turn things over to Tim.

[No response.]

DR. MCCARTIN: Today I would like to give an overview of the thinking that went on in our early analysis of flow and transport at Yucca Mountain.

[Slide.]

DR. MCCARTIN: I would like to reiterate Norm's point that the goal of this analysis was to cover all the steps. There were many simplifications made. It was looked at that what this work did was provide us a framework for future improvements. Currently, I will try to allude to a few areas where we are trying to improve on what we had done previously.

[Slide.]

DR. MCCARTIN: The strategy in modeling flow and transport was broken down into four steps, if you will. I will be discussing the first three steps and later Dick Codell will discuss the sensitivity and uncertainty analysis of step four. Initially one has to determine a modeling approach. The modeling approach should make use of obviously current site concepts, pathways to be analyzed, flow and transport phenomena that you certainly view to be important. For this particular analysis we had to look at the availability of some of the models.

As I said, we were on a relatively short timeframe to do the analysis so there wasn't going to be a lot of development, there wasn't a lot of time to get models that we were waiting on to be developed, primarily at Sandia National Labs. So, we went forward with what we had with some limited development.

The second step, certainly we acquired the models and did some limited development, and then went into the calculations. As Norm pointed out, the calculations really had two aspects. One was auxiliary analyses that were done to estimate parameters and/or sometimes to justify simplifications made in the models. Finally, accumulate release was calculated according to the EPA standard.

The fourth step is probably where a lot of the thinking goes on, I believe. Certainly, at the end of step three you have numbers. It's dependent on the analyst to try to interpret these numbers, what do they mean and give some understanding of what those results actually mean. A sensitivity analysis can sometimes be very informative, where you try to look at the sensitivities that you have seen in your results and relate them back to the first step that you have done in your creation of a conceptual model.

Many times you hope to at least see some things that are intuitively obvious in terms of sensitivity, but many times you see things that weren't obvious. Certainly, the understanding goes with trying to explain and rationalize the relationship between the sensitivities and the conceptual model in terms of what was important and what wasn't important.

[Slide.]

DR. MCCARTIN: I would like to touch upon a few aspects of the site that we looked at to a limited extent. There is not a lot new here, certainly to the extent of my first bullet. Yucca Mountain is layered with high contrasts and permeability. As was shown yesterday, one would expect to see, depending on the flux rates, a diversion of flow at some of the interfaces between the units. For our analysis we did not fully couple what we did in terms of flow and transport in terms of EPA -- in meeting the EPA -- standard with any two-dimensional flow modeling.

In this next phase we certainly are going to work at incorporating the two-dimensional model into the calculation with respect to the EPA standard.

Secondly, certainly, Yucca Mountain is an unsaturated fracture site. To date it's unclear as to the most appropriate way to simulate the fracture matrix interactions. Depending on what assumptions you are willing to make you can get drastically different performance with respect to flow through the mountain. For our particular analysis that we will be showing today, we simply used an on/off switch.

When the flux rates exceeded the matrix saturated conductivity the remaining flux went into the fractures, a very simple assumption. In later analyses we hope to incorporate a more explicit treatment of that fracture matrix interface. Recently Sandia National Labs developed for us a dual continual model, where the fracture and the matrix exist as separate continuum that are connected via a transfer term. That hopefully could take into account some of the role of fracture codings which could certainly limit the movement of water from fracture into the matrix.

Third, a low flux at Yucca Mountain we would think would imply primarily vertical flow. In fact, that is the way that we modeled Yucca Mountain for this particular demonstration. We considered one-dimensional models in the vertical direction. Last, the areal extent and dip of the repository appeared to be important. I think we certainly included some of those aspects of the repository into this analysis. I can typify that with a few cartoons better than explaining it in words.

[Slide.]

DR. MCCARTIN: This dark line here, one can see in relationship to the water table and going from left to right the distance to the water table changes. It certainly is important if you are going to use one-dimensional modeling in the vertical sense to try and incorporate some of this distance to the water table. For our analysis we used four legs or four one-dimensional models and just had those four to account not only for the different distances to the water table that you see in going across this way, but also the fact that you have different units between the repository and the water table. Both needed to be accounted for.

[Slide.]

DR. MCCARTIN: My next slide follows on that, in that our four one-dimensional models are showing here that in addition to the dip of the repository if you will, the areal extent is as what was alluded to as a pork chop. We prefer to put a couple of feet on and a head and call it a duck at NRC. I guess a different view.

What had to be done was to partition the areal extent of the repository and assuming a constant areal loading of the repository, you had a different inventory of each of these four one-dimensional models. You can see obviously that as I mentioned before, the different distances as well as different units. To give you a better appreciation for some of our results, the next slide lists those four one-dimensional models listed A, B, C and D.

[Slide.]

DR. MCCARTIN: It gives the thickness of each unit within those particular one-dimensional segments and the average saturated K of the matrix. That's a very important aspect in interpreting our results. You can see that the shortest leg, leg D, has only two units. One could argue that fortunately the matrix conductivity is the lowest for the two units that are there. However, if you remember the way we model fractures, any time the infiltration rates exceeds the saturated conductivity we will go into fracture flow. For this particular leg, leg D, where we have the lowest matrix conductivity we will have the highest tendency for going into fracture flow.

If we look at the range of infiltrations as Norm had pointed out before that we used for our base case, we had a rate of 0.1 to five millimeters per year. For our pluvial case, 5.0 to ten millimeters a year. If you remember, the average saturated K for those legs were on the order of one-half to one millimeter a year. So, we would expect for many of our realizations to see some fracture flow be created in leg D.

[Slide.]

DR. MCCARTIN: In addition to the liquid pathway model we had a direct release model that was a result of a drilling activity that waste was brought up to the surface. Our drilling model, as Norm pointed out, calculates the probability of drilling and it accounts for both waste and contaminated rock. It is probably easier to demonstrate the particular model with a cartoon.

[Slide.]

DR. MCCARTIN: Very simply put, probabilities were calculated for going directly through the waste canister and bringing up waste. Also, there's the possibility of going through the repository. After a certain time waste has leached out of the waste packages. The rock down here is contaminated and contaminated rock is brought to the surface, irrespective of the fact that you have not gone through a waste canister.

[Slide.]

DR. MCCARTIN: Once again a duplicate of Norm, was the four scenarios that we did calculate in the flow and transport part. There is a typo on your handout if you will, in that not pluvial is there -- pluvial is there. That line should not be there. There's a ten percent probability of pluvial conditions. We have these four classes that we calculated.

[Slide.]

DR. MCCARTIN: If we look at the base case, and this is just a distribution of consequences, so that it's a conditional probability it did occur. So, it should sum to one. Although it doesn't quite make it to one, if we carry the line out much further it would make it to one. You can see somewhat of an unusual shape. That shape is due to the transition from matrix conditions to fracture conditions until we get our higher consequences, obviously, when we do that transitioning over into fracture flow.

[Slide.]

DR. MCCARTIN: Looking at strictly the consequence distribution for the pluvial case, you can see we really don't see much of that transitioning period because we have a -- we are dominated by fracture flow so that transition does not occur.

Next, if we add in the drilling to the base case it does what we expected it would do. That is, obviously, the high consequence end of the curve is unaffected by the drilling. What drilling does do is add a low consequence, high probability part to the curve which is what we somewhat anticipated.

[Slide.]

DR. MCCARTIN: Likewise, if we go to the consequence for the pluvial case in drilling, we see the dotted line is the drilling consequences. Once again, low consequence, high probability. The high consequences are due to the pluvial. The pluvial -- the drilling and the pluvial drilling, obviously, you can't tell the difference. The drilling adds really very little to those consequences.

With that, I would be happy to answer any questions.

DR. DOMENICO: How does the model treat fracture flow; how does it know that it is supposed to let water go in the fractures? What do you do, just get --

DR. MCCARTIN: It's an on/off switch.

DR. DOMENICO: You just get the added water and dump it on the water table when you exceed that amount?

DR. MCCARTIN: No. The infiltration rate is an input parameter. When the infiltration exceeds the matrix saturated conductivity, the excess goes into fracture flow. It's assumed that fractures will take all.

DR. DOMENICO: That fracture flow is in your model?

DR. MCCARTIN: Yes.

DR. DOMENICO: You have permeabilities for that?

DR. MCCARTIN: I am not sure probabilities for --

DR. DOMENICO: Permeabilities.

DR. MCCARTIN: Permeabilities for the fractures?

DR. DOMENICO: Yes.

DR. MCCARTIN: It takes the --

DR. DOMENICO: Added infiltration --

DR. MCCARTIN: Added infiltration --

DR. DOMENICO: And just moves it --

DR. MCCARTIN: Moves it. It takes all the flux and just divides by permeability -- divides by the porosity and you get ten to the minus four.

DR. DOMENICO: Is the normal permeability equal to your average decay SAT that you have in the table? Is that what you have a permeability distribution in the model?

DR. MCCARTIN: Yes, of the matrix.

DR. DOMENICO: When you exceed -- the infiltration exceeds that for some of those units your switch goes off and another switch goes on and it goes into a different permeability. It presumably gets it down there faster; right? Those are the physics that are hidden in the model someplace?

DR. MCCARTIN: Yes. However, in our next stage, we intend to incorporate a more explicit treatment of fractures which is the Sandia DCM model.

DR. CODELL: I will be talking about the source term model, and then after that the sensitivity and uncertainty analysis.

[Slide.]

DR. CODELL: This picture shows a typical waste emplacement at the Yucca Mountain site. I put it up to alert you to the fact that we considered many of the processes that would go into releasing waste from the canisters, and we considered them in our head and on paper but we didn't actually include all of the processes in our model because of the severe shortage of time we had to work on this. We hope that by listing them at least we will include them in later phases of study.

In particular we want to take into account explicitly the air gap and the cladding, and the canister integrity, flow diversion of infiltrating water to the canister and a number of other processes that I didn't list. [Slide.]

DR. CODELL: Here's another picture of a closeup of the fuel rod and pellet, showing the inventories of the waste where they might be located. We considered that most of the waste, particularly the actinide, is included in the uranium dioxide but there are inventories of some of the more volatile fission products and grain boundaries and the gap between the cladding and the fuel pellet. There is an inventory of Carbon 14 within the cladding and on the crud of the cladding.

[Slide.]

DR. CODELL: As I said, the model we actually used only considered the following processes and not even all these actually. Water has to get into and out of the canister, the fuel has to dissolve or disintegrate in order to release through radionuclides. Some of the radionuclides are limited by the solubility of either the radionuclides themselves or of the waste matrix of which they are embedded. In some cases we speculate there might be formation of colloids. This might be especially important for the case of plutonium which is a potentially big contributor and has a large inventory in the waste.

[Slide.]

DR. CODELL: The next slide summarizes the model that we actually included in the liquid pathway part. Before we go on, I would like to point out that in the auxiliary analysis for gaseous release of Carbon 14 we actually did build a model that included the releases from the other compartments for Carbon 14; that is, the corrosion of the zircaloy cladding, the disintegration of the fuel pellet with the release by oxidation of carbon that might be inside of the fuel matrix. That model actually was more sophisticated than what we used for the liquid pathway.

The high solubility radionuclides are released at a rate determined only by the disintegration rate of the fuel matrix, which is Lambda. Lambda contains several terms. Q is the total infiltration over the site area. F is the fraction of the water infiltrating that comes in contact with the fuel. S is the solubility of uranium. M is the initial inventory of uranium.

For low solubility radionuclides they were further limited by the solubility. It was actually a two compartment model. The radionuclides are released from the fuel compartment and went into an undissolved inventory before they were released to the transport model.

[Slide.]

DR. CODELL: The significant parameters of the source term model that we put in were pretty much arbitrary. We had a very hard time picking realistic values. In order to perform the demonstration we had to do something, and this is what we have chosen. The waste package model was non-mechanistic. Furthermore, we assumed that a normal distribution of failure times. Furthermore, the model we chose could only take a single failure time, so this implies that all of the waste package failed at precisely the same time which isn't very realistic. We admit that, and in the next phase we will take that into account.

The solubility of the fuel matrix was taken from the literature over a range of .0001 to .0008 grams per milliliter. Some further parameters of that model -- since the water must come in contact with the fuel, coming up with a model of this water/fuel contact is critical. In order to get a handle on it, if you look at the total cross-section of canisters exposed to the areal plane it is this number, .0008. That gives you a handle on what it might be.

We speculated that most of the canisters are probably dry because the water would tend to flow around them through the unsaturated matrix, but there may be some flow diversions through fractures. There might also be the effect of thermally driven evaporation of the water close to the packages and then flowing back to the canisters.

In order to complete our demonstration we just picked a range of the water contact parameter, .0002 to .0001. Radionuclide solubilities were typical of what has been reported in other performance assessments, and I don't take credit for them.

That includes the source term model. If there are any questions we can just stop here. If not, I will go on to sensitivity and uncertainty.

[No response.]

[Slide.]

DR. CODELL: The purpose of doing the sensitivity and uncertainty analysis was to determine what the sensitive parameters might be and, also, what the insensitive ones are. If you can prove at the outset that something is unequivocally unimportant, then it simplifies your job.

We demonstrated the sensitivity by several methods with what I call ceteris paribus, but I am not sure is the correct term, changing or observing the effect of one variable at a time on the CCDF, the regression of the hundreds of repetitions in the Monte Carlo or Latin Hypercube sampling and performing multiple linear Regression. The results to look at the regression coefficients to see how they are correlated.

We also demonstrated the contribution to the EPA cumulative release rate by radionuclides; that is, looking at which were the most important radionuclides in the inventory. Finally, the nexus between NRC subsystem requirements in 10 CFR 60 to the cumulative EPA release limits in 40 CFR 191.

[Slide.]

DR. CODELL: The next slide shows the CCDF -- the conditional CCDF for the base case scenario which is the non-pluvial scenario, not considering in these either. This is for 100,000 years rather than 10,000 years, but it's the same idea. There were 500 vectors; that is, random samplings or Latin Hypercube samples that went into building this CCDF. The solid line shows the total of the 500 vectors. The other two lines show what would happen if you screened out those vectors that exceeded either two millimeters a year infiltration or one millimeter a year infiltration.

You see the very strong effect of infiltrations. In fact, you would not probably violate the EPA standard at these lower infiltration rates. We see that infiltration is, indeed, a very important variable in this sensitivity analysis.

[Slide.]

DR. CODELL: The next slide shows another -- as you saw in Tim's presentation the four columns that we used to represent the flow and transport model -- column D was the one that had only ten percent of the waste and was also the shortest, and had only two hydrogeologic units. This figure shows that for the cumulative release for 10,000 years, that for the high consequence of the release, virtually all of the effect comes from column D alone.

This is because for the high consequence releases you have to have fracture flow. That particular unit is prone to fracture flow because the units contained within saturate easily.

[Slide.]

DR. CODELL: That was the base case scenario. For the pluvial scenario we saw that there was relatively less contribution from column D. The next slide isn't in your package but I included it at the last minute, just showing the effect for the pluvial scenario that shows that column D is not strictly dominating although it's still pretty important. This is because some of the other pretty high infiltration rates, some of the other columns in the model also saturate and you have fracture flow.

[Slide.]

DR. CODELL: The next slide shows the multiple linear regression analysis performed for the base case scenario for 10,000 and 100,000 years. This technique is interesting, and it shows explicitly what the most sensitive variables are. I might point out that waste package lifetime doesn't show up to be very important, although the model we used is very simple and the maximum lifetime is only 1,000 years. Obviously, if the lifetime were 10,000 years it would be very important and you wouldn't get any release at all.

The parameters that deal with the propensity for saturated and thereby fracture flow turn out to be quite important. These are the saturated hydraulic conductivity of one of the units and the correlation length which is a measure of how continuous the fracture flow pathways could be in our model.

In the average importance of radionuclides, the thing we found is that plutonium was just the overwhelming, dominating, radionuclide in the high consequence cases. There is such a large inventory of it, and if you get fracture flow with low retardation in the fractures and fast transport you get a large contribution from these radionuclides. If you don't get fracture flow, if you never have fracture flow, then the only things that turn out to be important are the ones that are unretarded and have long enough half-lives, namely Carbon 14, I-129 and technetium-99.

These, all three, would not likely lead to a release that would exceed the EPA criteria. This tells me at least that we ought to be concentrating on these actinide as being particularly important and do everything that we can to refine our models in that area. That's one of the benefits of this analysis of sensitivity, even at this very preliminary stage.

[Slides.]

DR. CODELL: The last group of slides show the relationship between the EPA compliance and the NRC subsystem requirements. This analysis was done in the following way, looking at -- in the example of base case non-pluvial scenario, we took the 500 vectors and eliminated them on the basis of whether the vector complied or did not comply with various NRC criteria.

The first thing that you notice -- this is a slide of the EPA cumulative release versus the groundwater travel time. The groundwater travel time defined very narrowly for this case is just the flux divided by the water content of the column. The first thing that you will notice is that most of the vectors have no release at all or very, very small release. There is just a handful that exceed.

This does show that if you strictly observe the NRC limit of 1,000 years that virtually all of the cases fall to the right of that line, and there are very little releases. You only get releases if you have short travel times. This is because, obviously, you have fracture flow in short times and releases of these actinide for short groundwater travel times.

DR. REITER: This is only in saturated, right?

DR. CODELL: No, this is the unsaturated column. We only looked at the unsaturated column, but the fracture flow was a result of the local saturation of the fractures because you have exceeded the saturated hydraulic conductivity of the matrix.

DR. DOMENICO: What is the EPA ratio?

DR. CODELL: The EPA ratio -- there is a table in their regulation that gives the allowed limit per thousand metric tons of heavy metal for each radionuclide. You take your release and you divide it by that allowed limit and sum them all, that's the EPA ratio.

[Slide.]

DR. CODELL: The next slide shows a similar screening but now on the basis of release rate from engineered barrier, which reads something like one part in 100,000 per year of the inventory of 1,000 years. This also shows a strong correlation of release rate from engineered barrier with EPA ratio. None of the 500 vectors in this case exceeded the NRC ten to the minus fifth per year limit. Therefore, none of them were screened out.

There was also a similar screening on the lifetime of engineered barrier but I won't show that. If you plot those results of the screenings as a CCDF you would see the following.

[Slide.]

DR. CODELL: There is a strong reduction in the release for the groundwater travel time compliance. A relatively minor reduction for the waste package lifetime here is shown for a 500 year waste package, and no reduction at all for the ten to the minus fifth because none of the vectors are screened.

I want to finish with one further slide so that we don't take ourselves too seriously in this very simple analysis. I also have a sign above my computer and it reads the following. These slides will give you a warm, fuzzy feelings, but you must remember that warm, fuzzy feelings are sometimes caused by radioactive asbestos.

[Laughter.]

DR. NORTH: Are there any questions?

[No response.]

DR. EISENBERG: I am going to try to quickly go through some conclusions. Let me repeat again, that this is subject to some caveats. I don't need to go through them. One of the things that should be obvious of course, is that the analysis represents not necessarily what is actually true but what the result of the models give you. So, you shouldn't take it too seriously. That doesn't mean that you don't get some useful information by doing these exercises. [Slide.]

DR. EISENBERG: In going through this demonstration performance assessment we feel that we demonstrated that the staff could do this type of analysis, and I think we got some insight into both the way Yucca Mountain might behave and also into what we would like to see developing in terms of a performance assessment methodology.

We used the CCDF to describe the performance of a repository for a limited set of scenario classes and with a lot of assumptions and the consequence modeling. The base case scenario for Yucca Mountain was modeled using the Neftran computer code that was adjusted to take account of the fact that we really had an unsaturated flow regime.

There were four vertical legs under the repository to account for the spatial extent of the repository and the spatial variability of the geologic units underlying the repository. There was an improved treatment of waste form dissolution and there was a non-mechanistic model of waste package failure. Then, this analysis for groundwater flow and transport and releases by that pathway was extended for pluvial conditions.

[Slide.]

DR. EISENBERG: We developed and used a total system code in order to generate the CCDF. An important issue there is that although one could look at a system code as a bookkeeping type of activity it really represents the logical structure by which you are performing the calculation, and is a manifestation of it. Therefore, it is quite important.

We developed a model for human intrusion by drilling and generated a corresponding computer code. We did some preliminary statistical analyses using Latin Hypercube sampling and regression analysis methods and, as Dick just explained, several other types of interesting and useful sensitivity methods. As I already recounted, there were four types of auxiliary analyses that were also included which we think are quite important. That's likely to be the format of the evidence presented in a licensing arena.

Some of the important tentative conclusions are that the areal extent of the repository, at least at Yucca Mountain, appears to be important; that releases of Carbon 14 could be important and probably should be modeled as part of the estimate of repository performance; that the potential for non-vertical flow at Yucca Mountain needs further investigation. Plutonium releases appear to be important, but their geochemical behavior is quite uncertain and more data are required.

The important liquid pathway parameters have been identified. Dick just spoke about that. One of the critical ones appears to be the saturated matrix conductivity. There may be a problem with the efficiencies of the consequence models, and we may have to develop simpler consequence codes in order to be able to do the kind of sampling required to generate a representative CCDF.

[Slide.]

DR. EISENBERG: There are some needs for modeling improvement. We are currently engaged in Phase II of these performance assessment exercises. We are still doing a demonstration but we would like to try to improve the capability for modeling in the following ways. In terms of the performance assessment itself, we would like to treat additional scenario classes. The one that we would most like to get after this time is volcanism.

We would like to have a mechanistic model of waste package failure. As Tim pointed out, we are going to try to use an improved treatment of flow and transport in tuff by using a code that was developed for us by Sandia. We would like to do some additional analyses in terms of auxiliary analyses. We would like to do a more forthright treatment of the geochemistry in both the near field and far field. One of the big questions in far field is how good KV's and their use in estimating transport.

One thing we didn't look at in Phase I was the thermal affects -- I'm sorry, I have to back up on that. In the gas analysis they did look at thermal affects, but we would like to look at thermal affect at early times and especially its affect on the groundwater flow system. We would like to do more regulatory evaluations for the NRC standards as well as the EPA standards.

Once again, in line with a question that was asked of Seth, we would like to try to do more integration by getting further inputs from earth scientists both for the scenario analyses and we also feel that the scenario methodology needs to be made more robust and appropriate for the repository.

That's all I have. Thank you very much.

DR. NORTH: Thank you. Any further questions or comments?

[No response.]

DR. NORTH: Do you have any further thoughts you would like to give us in terms of what you learned from the exercise and the value of it to NRC over the time you have done it? I presume that you feel very positively about it. I wonder if I could get a little more specifics in terms of what you feel you learned was surprising and what advice you might offer to DOE based on what you learned.

DR. CODELL: I think we are still in the age of innocence. I thought that by doing this demonstration we have brought people together at NRC to focus on the important issues instead of everyone going off and doing their own thing. The strongest personalities and wills prevailing, I think this focuses NRC's limited efforts on what is truly important.

DR. EISENBERG: I would tend to agree with that point of Dick's, that until you put everything together in a total system analysis I think it's very easy to convince yourself of things which may not be true in terms of what is important or what can or can't happen. I think these are very complicated, non-linear -- I think Dick would sometimes say they may even chaotic systems -- I question whether previous experiences or judgment is all that useful in trying to estimate what performance might be or what the behavior of the system might be.

There's a lot to be learned by trying to put together reasonably robust physical models and see how they behave.

DR. REITER: Can you give us an idea of how much of an effort this was in terms of money or manpower?

DR. EISENBERG: I think -- I'm not sure. I think it was somewhere between two and four FTE's. There were I think, three or four principal analysts and we got a lot of help from other people. I would estimate it was maybe a total of about four FTE's which is, given that we were all Federal employees, wasn't much money.

[Laughter.]

DR. NORTH: FTE is fulltime equivalent over a period of how long?

DR. EISENBERG: This transpired over a period of a little over a year but there was an intense effort for three or four months. Some people -- Dick was able to spread his efforts out over a longer period as well as getting involved in the very intense effort, and some of the rest of us got more focused. I think that was about the timing.

DR. DOMENICO: I think a lot of your conclusions were interesting, but a lot of them were also pretty much model or assumption dependent. Your source term, I have seen that similar type of usage for the low level sites. I have seen similar kinds of source terms for that. Your conclusions on plutonium are only correct when you let plutonium be very lightly retarded or not retarded at all. The plutonium should have the largest retardation coefficient. It's when you take that away the large inventory makes plutonium a real bad actor.

Your comments on technetium and iodine, that's always been true. They have always been questionable because they basically are not retarded. Technetium does come with a very large inventory.

A lot of them are, in many cases, dependent upon assumptions in your subcomponents. That's maybe true for all models.

DR. CODELL: That's true. However, I think I pointed out -- our study pointed out that plutonium is potentially important and potentially very important, more than everything else put together. Therefore, further analyses should be directed at giving you more confidence that these dastardly things won't happen.

I might point out that the plutonium only showed up as a contributor if you had fracture flow, in fracture flow you generally have very low retardation. You had to have fracture flow. If you could prove you never have fracture flow the problem would go away. Also, I might point out that the values we chose for plutonium solubilities were what many people would consider quite high, many people at DOE.

I didn't pick those values personally, I am not qualified. But our cracker jack geochemist did. I was carrying a report in my briefcase which did report values that were much higher than DOE reports. I am not qualified to say which is correct. Once again, I think these kinds of data can be found in the laboratory and field exploration, and I hope that their precise values will be nailed down and eventually factored into the performance assessments.

DR. EISENBERG: I think that fact that it points to various assumptions is exactly one of the things you would like to get out of an iterative performance assessment. It highlights what might be weak in the database that you need to go into licensing with. If this is an issue -- and it may be a spurious issue -- I think what Dick has said is that he used very conservative assumptions and did not want to claim more than he could fully back up.

Under those conditions these are the things that showed up as being sensitive. That means that you may want to strengthen the database so that you don't need to make those assumptions. I think that's exactly what you want to get out of this kind of analysis at this time.

DR. NORTH: Thank you very much. Next on our program we will hear from Frank Rowsome, who is President and Principal Consultant in FHR Associates, Inc. He was formerly with the Nuclear Regulatory Commission: as Deputy Director of the Division of Risk Analysis in the Office of Research; as Assistant Director for Safety Technology in the Office of Nuclear Reactor Regulation; and as acting Deputy Director, Division of Human Factor Technology, Office of Nuclear Reactor Regulation.

DR. ROWSOME: I know you all want to go to lunch, so I will try to get through this very quickly.

[Slide.]

DR. ROWSOME: Risk assessment of nuclear power plants began with the reactor safety study which was performed between 1972 and 1975. That can be called the age of innocence for the reactor risk assessment community. It was also something of a golden age too. It was an age in which most of the creativity, most of the invention, most of the vitality that has gone into reactor assessment all came together in a very creative period.

As one looks over the succeeding 15 years only a small percentage of the invention we use today has been a consequence of later developments.

[Slide.]

DR. ROWSOME: The risk assessment in the reactor safety study and in that used up to the NUREG 1150 reactor risk assessments of today, all are built on pieces that look pretty much like this. The reactor safety study group struggled with a way to catalog the accidents in a way that would make sense both qualitatively and for quantification of accident likelihood and risk, tried fault trees and didn't work, tried a number of other methods and finally lit upon event trees, an adaptation from decision trees and that method is used to this day.

They adopted system reliability analysis techniques pioneered elsewhere, primarily fault tree analysis. That is still a principal work horse. They employed simple primitive models of the releases of the process of reactor accidents and the source terms to be expected without conservative bias, attempting realism, and that term remains to this day.

We have, from time to time, succumbed to the temptation to build much more complex and mechanistic models. NUREG 1150 study is a good example of that. They have rarely borne fruit. The simpler models have been easier to use and have supported almost all of the actual applications that we have found to be useful over the years. There is a strong tendency in the research community to want to go deeper into the pyramid than, in fact, turns out to be useful for practical decision making applications -- or at least that's been the reactor experience thus far.

The team also used realistic and fairly simple models of the offsite model consequences to relate the source terms and the frequencies to risk of consequences in human casualties and property damage to be expected. They also employed a simple but profoundly significant Monte Carlo method of propagating uncertainties through these distributions. This was at a time when hardly anybody used uncertainty propagation for any complex calculation at all. It was really quite a pioneering initiative at that time.

They took a lot of flack for having done it imperfectly, but it was part of their pioneering breakthrough. They used it only, however, for uncertainties originating in probability data. They made no effort to propagate uncertainties in phenomenology or source terms.

DR. NORTH: One comment that I would like to add at this point. Perhaps you were around in these days and have your view on it. I would question your use of the word invention of the methodology. My information from some peripheral acquaintance at the time was that much of the methodology that was used in WASH-1400 previously existed and was used within the General Electric Corporation.

There are several other people from the nuclear engineering community that also claimed that they had similar methodology developed in the mid-1960's. It was rather a change within the AEC in commissioning this study and allowing this kind of probabilistic method to be used as opposed to de novo invention of that methodology at that time.

I can provide more discussion of that later. I think we are little bit away from our main subject.

DR. ROWSOME: Briefly, I think never before had anything like within a couple orders of magnitude the resources gone into integrating these pieces, all of which existed in other disciplines elsewhere. The real contribution was the assembly of these tools and the application of those tools, and the struggle with learning how to really do it in a practical application.

All of the incremental tools and techniques were known. There may well have been little pockets of people in different corners of the community who had part of the vision and may have done some back of the envelope work that resembled this. But nothing approaching this integration -- 72 people working for three years -- had ever been done and no one had really done much to attack the problems of assembling the tools and making them practical in an integral assessment before to my knowledge.

DR. NORTH: I would agree that on that scale nothing had ever been done of this kind. On the other hand, I think another very important limitation of WASH-1400 that is worth keeping in mind is that the leadership of that study decided not to address human intrusion type of problems; employee sabotage or terrorist attacks and things of that sort. They were viewed as too hard, and so as a result they were not addressed in WASH-1400. WASH-1400 was severely criticized later for not including such things.

The pilot analysis for WASH-1400 in which I was a peripheral participant when the back of the envelope calculations were done, they tended to indicate that these human scenarios were among the leading terms. The issue was, could one get a credible assessment of those probabilities. On that basis the leadership did not pursue it.

DR. ROWSOME: The next five years for the risk assessment community, particularly the one in NRC research, could be called the age of excoriation in which they were subject to wave after wave of criticism. The AEC regulatory staff itself found the methods strange and foreign and threatening and at odds with their conventional wisdom. This may relate to a couple of heuristics that I found to be remarkably powerful as-predictors of expert judgment, that is: the propensity of experts to first of all exaggerate the importance of their own discipline; second of all to exaggerate the importance of their contributions to their discipline; third, to exaggerate the confidence they place in their judgments.

The regulatory staff dismissed the reactor safety study as essentially irrelevant to the conduct of licensing with one possible notable exception. The reactor safety study indicated that probably the single most important safety system in pressurized water reactors was a system that the AEC did not regard as a safety system at all; that did not subject to the regulatory requirements or the oversight that they did to engineered safety features, and they rapidly changed that after reading the reactor safety study.

The critics of course of nuclear power saw the very low levels of risk that were projected by the study as damaging to their case, and they launched many different diverse and redundant initiatives to discredit it. The commercial industry liked the result particularly that the risks projected were very slight, but did relatively little in that period to try to defend the study or to make use of it.

DR. NORTH: You want to mention the American Physical Society Review?

DR. ROWSOME: I was going to mention the Lewis Committee. It was really a precursor to the Lewis Committee. The American Physical Society did conduct far and away the most thorough technical inquiry into the technical strengths and weaknesses of the study in the mid-1970's -- 1976, roughly 1976 period. Hal Lewis participated in that.

On the basis of that, Hal was chosen to Chair the Blue Ribbon panel that Congress and the AEC jointly agreed needed to be impaneled as a jury on what to think about the reactor safety study. The Lewis Committee came out with essentially two conclusions; that the methodology is an essentially sound method for studying the safety of nuclear power plants but that the reactor safety study in particular grossly understated the uncertainties in its results.

That triggered in the regulatory staff a purge -- an inquiry into whether licensing engineers had ever soiled themselves by the use of risk assessment or reference to the reactor safety study. The management of the Office of Nuclear Reactor Regulation could honestly say that he found 680 virgins in the Office of Nuclear Reactor Regulation.

Then came the accident at Three Mile Island. It rescued reactor risk assessment from political and regulatory oblivion. The two principal inquiries into the accident determined that the occurrence of such an accident was consistent with the risk profile as assessed in WASH-1400. Furthermore, that most of the features of the accident sequence and most of the contributory mechanisms and phenomena that happened at Three Mile Island were among those modeled or predicted in the reactor safety study.

One could have found a great many clues as to what to expect if the reactor safety study had been used as a set of clues to where the safety problems were. PRA gradually emerged thereafter from a very low budget embattled research activity sustained by Saul Levine in the corner of the Office of Nuclear Regulatory Research among whose contributions were launching the beginnings of the performance assessment research at Sandia back in the mid-1970's.

It gradually emerged from a low budget activity into a more significant activity into 1980, at a time when Bob Bernero and I had the privilege of directing that group. [Slide.]

DR. ROWSOME: Within five years of the accident at Three Mile Island, about 30 reactors had been subject to individual plant risk assessments about one-half sponsored by the NRC and about one-half sponsored by industry. Of those 30, I would venture to say that the majority were technical successes and maybe one in three or one in four was a technical failure in the sense that peer reviewers thought it failed to live up to the state of the art or failed to live up to the objectives claimed for it.

They were almost institutional failures in the sense that hardly any of them earned their keep in the sense that results drawn from them fed decisions and that the decisions either improved the conduct of operations or the design or regulation in any direct sense. The institutions hadn't learned how to accommodate this kind of information at that time.

In an indirect sense though, the growing population of plant-specific risk assessments fed some of the institutional uses that came along in this interval that were not tied to the specific studies. The Commission decided that it had to recognize the existence of severe accidents. Core melt accidents had been declared incredible in the regulatory process up until Three Mile Island. The reactor safety study had indicated that essentially all of the risk was attached to severe reactor accidents, and one negligible part attached to accidents that stopped short of core meltdown. Three Mile Island, of course, confirmed that core melt was a clear and present danger.

The Commission decided to adopt a practice in the preparation of environmental impact statements of modeling the WASH-1400 generic reactor risk profile, source term and accident frequency model at each individual site and running a site-specific consequence calculation based upon the generic frequency of accidents for frequency of source term picture emerging from WASH-1400 that has been a regulatory success. They have not been embarrassed by that policy.

PRA was used to resolve the issue of undue risk from plants at particularly high population densities. That's story too long to go into. Let me summarize it very briefly. The NRC itself noticed that the wide variation in population densities around nuclear plants was such that if all plants were equally likely to have equally severe accidents about one-half the risk would be associated with a handful of plants at the most populous sites; Indian Point, Zion and a few others.

That opened a potential concern that these plants might be responsible for disproportionate risk or undue risk in the sense of the Atomic Energy Act. Shortly after the accident at Three Mile Island the Union of Concerned Scientists petitioned to shut down Indian Point on the grounds that risks had not been assessed and that they were likely to be disproportionate by virtue of the proximity of the plant in New York City and its location in dense suburbs around New York.

That was taken very seriously by the Commission. The Commission struggled with it for several years and decided to use the Indian Point issue as a test bed to see whether reactor risk assessment would sink or swim in the hearing arena. An Atomic Safety Licensing Board was named and charged with collecting evidence subject to cross-examination and discovery on whether the comparative risks and absolute risks of the Indian Point Plant were excessive and to use risk assessment as the vehicle for organizing this evidence.

It was viewed by the Commissioners who charted this hearing as an experiment to see whether PRA would sink or swim in the hearing arena. There was some doubt in the Commission at that time whether it could ever fly in a quasi-judicial adversarial arena.

I chaired the staff task force that prepared testimony for that hearing and was the principal witness for the staff. Later on I will come back to some conclusions of what we inferred from that experience. Other things that were happening in this five year interval, the groundwork was laid for the staff's later severe accident policy. The staff began to use measures of importance to risk derived from PRA and based on some of the inventory of published risk assessments to allocate resources to generic issues and even to some extent licensing issues.

Ironically, the researchers -- the experimentalists and co-development people in research were among the last to yield to this trend to prioritize work on the basis of risk significance.

[Slide.]

DR. ROWSOME: After the pendulum of heavy backfitting of plants following Three Mile Island swung to heavy backfitting, it swung back toward very little tightening of the regulations and backfitting became a dirty word in reactor regulation. PRA was adopted as a way of determining whether a compelling case could be made for additional tightening of the regulation. PRA studies became a filter through which candidate backfits had to pass before they were promulgated as new regulations.

[Slide.]

DR. ROWSOME: In that era, one of the politically and technically troublesome concerns was the safety of the plants licensed in the late 1960's or very early 1970's before the regulations had become anywhere near so stringent as they became in the late 1970's and early 1980's. A style of attacking the problem called the integrated safety assessment program that used both PRA and deterministic safety analysis techniques of the kind normally used in compliance measures, regulatory compliance measures, became far and away the most successful way of resolving those issues.

The Commission issued a safety goal policy expressing safety goals in terms of risks, although to this day has not settled on an implementation plan for those safety goals and still is hesitant about using them.

The Commission issued a rule requiring any new applicant for a construction permit for a nuclear power plant to submit a PRA within a year of getting the construction permit, and most of the generic safety issues concerning reactors are examined for risk significance against the profile of PRA's that are in the literature today.

[Slide.]

DR. ROWSOME: In the meantime, early 1980's, most of the companies involved in designing and building nuclear power plants developed the capability to use PRA and some of them began using it in tentative ways as a design and licensing tool. Some few utilities -- the minority of those who had sponsored PRA's -- adopted PRA themselves as a management tool with varying levels of success.

Industry support groups developed centers of expertise and so forth. We can jump over all that.

[Slide.]

DR. ROWSOME: The last five years have continued that trend. The Commission has finally decided that it would require of all reactor operating license holders that they must prepare an individual plant risk assessment to find sore thumb vulnerabilities to reactor accidents that are peculiar to their design and operation and to refine severe accident management devices in design or in operating procedures, emergency procedures and personnel training and the like.

There are efforts underway to try to establish a connection between the management and institutional conduct of operations issues that have become such a big issue in inspection and enforcement of operating plants. These efforts have thus far borne little fruit, I think in part because they are too stylized and too discipline-bound. I think a lot of progress could be made that has not yet been made in this arena.

Quite interestingly, PRA literacy has become quite widespread among the NRC regulatory staff, particularly in headquarters but increasingly so in the regions as well.

[Slide.]

DR. ROWSOME: Among the strengths and weaknesses, it's very widely known and recognized that quantitative risk predictions produced by PRA's are known to suffer from very broad substantial uncertainties. Data on the likelihood of events, failure events and human errors have severe limitations. In fact, within the technical community over the last 15 years or so, far and away the most talk and most hot air and most hand wringing has been about the data uncertainties, although I think it's safe to say that they have posed less of a problem for practical decision making than any of the other classes of uncertainties. They attract the most attention but are, in fact, the least problem.

The modeling of accident processes and phenomenon source terms and so forth as a practical matter in risk assessments invariably employ many very simplifying gross approximations that introduce possible bias or error in the predictions in the course or consequence of accidents. As I mentioned, the Office of Nuclear Regulatory Research undertook a multi-year severe accident research program after Three Mile Island and has spent many hundreds of millions of dollars trying to reduce these uncertainties.

NUREG-1150 is intended to harvest the lessons learned from that process. As a practical matter, I don't think it will have any effect on the practical decisions being made in the aid of PRA.

At the jump is probably the most important source of uncertainty, and those are the biases and the omissions in which classes of accident contributors or classes of accident sequences are simply not modeled at all. Almost all of the serious potentially compromising threats to practical use of the insights to PRA have been of this kind. They are the completeness arguments.

As you pointed out, the omission of sabotage and errors of commission, the omission of accidents involving reactivity excursions, the inability to handle the kind of patterns in human error that originate from institutional breakdown of the kind that has been a source of considerable controversy with the operating plants. Those threaten to severely distort the risk profile as PRA predicts it for a plant.

If you go back and look at WASH-1400 in the light of what we know today, we conclude that at least two and maybe three of the dominant accident sequences found for those two subject plants were artifacts of the conservative success criteria for safety systems and probably aren't severe reactor accidents at all. By the same token they left out some of the human error and institutional failure kinds of contributors that caused other accident sequences to be severely underestimated in the reactor safety study.

Although the bottom line, when you average together the exaggerations of the risk that the underestimates of risk as near as we can tell, the bottom line is about right -- cancelling errors.

On the strength side of the equation the reactor risk assessments do furnish unique insights into the safety of the subject plants. No other form of safety analysis has ever approached its success rate for uncovering obscure but important vulnerabilities that warrant regulatory attention. [Slide.]

DR. ROWSOME: Part of these insights originate really just from the catalog, the qualitative catalog of severe accident sequences, severe accident vulnerabilities. Part follow from the fact that the PRA's identify the importance of contributory accident sequences and faults in mechanisms. The PRA furnishes a model of why these accidents take place, what are the contributory factors, the when, where and how, a mechanistic model of why these accidents take place and why they emerge to dominance that allow you to draw many conclusions. Among them, how effectively to target risk reduction initiatives and how to change design or operation, or surveillance or maintenance to home in on the vulnerabilities.

One that has been used almost not at all is the insights into how to better target the NRC's regulations. One thing that jumps out at you when you have studied a half dozen reactor risk assessments is that the NRC's deterministic regulations tend to make mountains out of molehills and molehills out of mountains. They are not well tuned in on the dominant contributors to risk.

That is not just a criticism of my friends on the regulatory staff who worked on developing those deterministic regulations. It's in a sense, a testament to the success of the regulations. They push down the risk contributors that they focused on and left as the tall tent poles the risk contributors that they weren't looking at.

Finally, since you do get a mechanistic picture of why the dominant contributors are dominant, you get a great many clues with which you can try to verify whether that's a correct inference or whether that is just an artifact of the uncertainties and approximations that went into the PRA. You get from the PRA itself a great many clues you can use to validate the important decision aids or to set them aside.

DR. NORTH: These points are made in the Lewis report as I recall.

DR. ROWSOME: They were. They tended not to get recognized until that five years worth of experience after TMI took place. You are right, the Lewis report -- the agency really didn't see beyond the don't believe the bottom line message in the Lewis Committee.

Let me talk briefly about PRA in adjudication. The rest of this you already know.

[Slide.]

DR. ROWSOME: There have been three contexts in which risk assessment has been brought to the ASLB or to courts of appeals. First, the use of the WASH-1400 source terms and frequencies in environmental impact statements. The Indian Point special proceeding and the licensing of Limerick and Millstone 3 which had been tagged as high population density sites and for which that issue had to be put to bed before the Commission was willing to grant a license for those plants, in each case a version of PRA was the centerpiece of the decision making vehicle.

In each case the PRA's did succeed in meeting the objectives established for the risk assessment by the Commission. It stood up, it met the test of cross-examination and discovery. The prototype for failure which has stood in the minds of the NRC people for many years was the ECCS hearings of almost two decades ago. On that occasion hostile intervenor lawyers, skilled lawyers, used an academic controversy among people in the research community over technical issues affecting the likelihood of the success of emergency core cooling systems as meat to embarrass and discredit the regulatory posture of the licensing staff.

For literally 100 days of cross-examination the senior executives and the regulatory staff were embarrassed and humiliated and made to look like fools by very skillful hostile lawyers. Ironically, the hearing had no effect on the conduct of licensing at all of the first order. It so scarred the personalities of the people who experienced it that when I joined the Commission a decade later you could still see the scars.

There was a strong fear that when we went into hearings on the Indian Point Assessment that this process would start up all over again and the whole thing would be discredited, and all the new initiatives with PRA would be discredited and all go down the tubes.

In fact, we found that -- first of all, I should say that the intervenors challenging that case were not well funded and had not had the resources to try to learn the language of risk assessment and use it against us. If they had attempted to use it to discredit the NRC's deterministic regulations instead of discrediting the Indian Point conclusion, they could have gotten us by the short hairs and embarrassed us profoundly. That was not what they were looking to do.

They wanted to discredit the license for Indian Point instead and simply tried to use hand waving arguments to say you can't believe PRA. So, they weren't much of a problem from that point of view.

We discovered that our primary problem was one that we had never really taken terribly seriously, and that was the problem of communicating such a complex and arcane decision making safety analysis technique and decision making structure to the hearing board and get them to paraphrase it and carry a coherent decision logic forward to the Commission or to the Commission's aides, and the aides to carry it forward to the Commission.

We had to communicate with them well enough that the message could go through about three passes from one group to another group to another group without our having the legal authority to go back and make mid-course corrections. The signal to noise ratio deteriorated severely in going from us to the hearing board and from the hearing board to the policy evaluation staff, and from the policy evaluation staff to the Commission.

Communicating this difficult discipline proved to be vastly more problematic than dealing with hostile witnesses. The hearing experience suggests that the credibility of PRA and its success in the regulatory arena depends upon having mature and technically expert witnesses to be sure. Another one implicit that I should have had on this list was to keep it simple. You have to be very simple and very lucid if the signal is not to degenerate completely in being passed from hand to hand up the chain of decision makers.

Forthright acknowledgement of the uncertainties involved -- it really helps to have a diverse and redundant decision logic so that perspectives are not unduly dependent upon one or a few uncertain tests of compliance. I am made very uneasy by Part 60 and 40 CFR 191 by virtue of the fact that it does not have the diversity of decision perspectives in it that I think one needs to have much confidence that one will reliably make correct regulatory decisions in the face of the many unpleasant surprises that could surface between where we are now and where we mean to get.

DR. NORTH: The concerns of this Board are already on record on that issue.

DR. ROWSOME: The credibility of the process also rests upon a manifest NRC willingness to refuse a license or impose license conditions or shut down an unsafe facility. We were willing in Indian Point to ratchet the plant and, if need be -- if the results had indicated, to shut them down. [Slide.]

DR. ROWSOME: One thing that is very important is to avoid the diversionary tendency of focusing on little controversies outside of a perspective of how that controversy really relates to the bottom line compliance question or the bottom line risk. One of the great advantages of risk assessment as a centerpiece of licensing is that it facilitates looking at the big picture. That may, in fact, more than compensate for its disadvantages.

The rest of the stuff you can read on your own if you are curious. I know that we all want to go to lunch, and I will leave it for questions or for the panel later on if you want.

DR. NORTH: Do you have any concluding messages in dropping through this last series of slides of yours that you think you ought to leave us with before we go to lunch?

DR. ALLEN: I wish you would say something about the relevance of this to the repository problem.

DR. NORTH: I think we are happy to take some more time putting off lunch a few more minutes, and really try to mine this experience for lessons learned.

DR. ROWSOME: I guess the principal message -- the principal lessons I draw from this that I bring to performance assessment is that we, as technical people, have a strong tendency to read this as primarily a scientific problem. I think it is primarily an institutional, political, legal, human relations, government relations problem that has a little bit of technical dimension to it.

We distract ourselves perhaps from long term institutional success and technical success by allowing ourselves the luxury of being scientists and addressing the technical issues.

As I look at the history of reactor risk assessment it has been so politicized and it has been occasionally misused, but more often than not its messages have simply been ignored. In order to have the luxury of making good technical decisions work in the political arena we have to concentrate on the communication problem and the institutional problem far more than I think we tend to do.

There has been an undercurrent throughout this meeting about how well we are gearing up to do performance assessment for the repository, and it is tempting to view that in technical terms, how well are we doing technically the performance assessment. The experience I come from with things like the Indian Point hearing and participating in a number of these policy and standard development initiatives at the NRC is that gearing up for the decision making process -- solving the communication problem is the bigger task before us, and the technical issues will take care of themselves. They are a subsidiary problem.

I could talk to you about methodology, if you would like. There are a great many arenas in which I could respond to questions, but I don't want to just stand here and pontificate.

DR. NORTH: I would like to hear your thoughts about one might prepare for the hearings before the Licensing Board, given the experience you had. Perhaps give us some more details on your thoughts about communication.

For example, would it be useful for the people that are making presentations to go and get some experience on what it's like to be an expert witness and practice in making those kinds of presentations and getting cross-examined and so forth?

DR. ROWSOME: I think some mock hearings would be a very good exercise. I think another important thing to do is make very sure the Commissioners and their staff and the ASLB personnel themselves have had an opportunity to become literate in at least a high level version of lingo and get beyond the most primitive heuristics about what to believe and what not to believe about performance assessment.

Because Commissioners are appointed for five year terms and most of the come from outside the regulated industry and have little experience of it before they get there, many of them are only beginning to get their feet on the ground with the technical issues when their terms are up.

We found the situation in Indian Point in which the two Commissioner's whose brain child this hearing was had left before the Board findings got back to the Commission and nobody understood that this was to be a benchmark experiment in throwing PRA into the hearing arena. Nobody was left to harvest those lessons learned for policy development.

It is important in addition to preparing ourselves as DOE or ourselves as NRC staff or ourselves as peer review committee members, we need to make sure that the key players are literate, the decision makers are literate. I find throughout my time in the NRC I find the Commissioners and office directors had a handful of simple heuristics, rules of thumb about what to believe and what not to believe about risk assessment. From one of them the data is a big source of uncertainty and comparative risk assessment is more trustworthy than absolute risk assessment and a few other heuristics.

Which nine times out of ten are probably true, but if used outside of their domain of validity give you non-sensical conclusions. In many cases they are locked onto these heuristics and it was very difficult when they would misuse them to get them to pry them loose from them. We need to try to equip the decision makers in whose hands the fate of this proceeding is ultimately going to lie to be a little more mature in their thinking about what evidence to believe, what evidence to trust.

DR. ALLEN: Isn't it true in the case of Yucca Mountain far more so than in any of the nuclear controversies that communication with the Commissioners may be the least of our problems. Communication with the public and with the press and political leaders, isn't that where our real communication problems may take place?

DR. ROWSOME: It's certainly true that beyond a certain level of political discrediting what the Commission may do could easily become irrelevant, particularly if Congress is willing to change its determination to proceed along the lines of the Waste Policy Amendments Act. Yes, indeed, there is a much broader political arena.

One of the things that I found very useful in the Indian Point case, particularly when the

Commission's own decision logic collapsed, they thought that the best way to decide the Indian Point case was on the basis of comparative risk in part because it was a disproportionate risk issue that was in question, and in part because they had been told by people like Bob Bernero and myself that comparative risk assessment was more trustworthy than absolute risk assessment.

The licensees made a breakthrough in their risk assessment and produced one for which there were no peers with which a comparison -- inter-PRA comparison could be done. We simply didn't have the evidence to say how that risk profile that included systematic consideration of seismic and fire and some events like that, that had been done in prior PRA's but not as well, we didn't have a basis for doing a comparative analysis.

The Commission's espoused decision logic collapsed in the middle of the proceeding in Indian Point and we had to invent a decision logic that would sell in this vacuum. There may be some distant analogies with the problem of trying to produce a decision logic that is marketable in the school of public opinion and in the political arena.

We need perspectives other than just compliance with Part 60, perspectives other than just compliance with 40 CFR 191, in order to satisfy public opinion. We invented some ad hoc ones for the purpose of the hearing but aimed at the hearing board. In that case you might want to assess the evidence against the kinds of concerns one hears from the public.

We as technical people have a sense of what level of risk we may be talking about with the repository but the public does not know that we have absolute confidence that the repository will not extinguish life on this planet, for example. The public does not know that it does not pose any risk at all of rendering a four state area wholly uninhabitable.

Things of that kind are criteria that would never dawn on us to put those into an NRC rule as a licensing basis because they don't discriminate between good site selection and good design. Nonetheless, some of those questions do go to the heart of what concerns the public who don't have the intuitive sense we share of the neighborhood of the risk that we are talking about.

It might be useful to bring in some of those and, in fact, embed them in the safety analysis so they are available as decision aids. If you look at the fine print in Part 60 you will find something like the fine print in the NRC's reactor regulations, that the licensing decision is ultimately a judgment call on the part of the Commission. The compliance questions are advisory on the judgment of the Commissioners.

So, it is strictly speaking neither necessary nor sufficient to comply with the technical requirements for the Commission to decide. The Commission is technically free to entertain considerations that are not explicitly enunciated in the regulations. In light of the political dimensions of this case, it might be well to enrich that body of evidence that is outside the framework of Part 60 to add additional decision making perspectives.

DR. NORTH: Let me just quote from something and ask you to comment, and see if you would agree with this characterization of the issue. This is on the subject of the criteria, 10 CFR Part 60 and 40 CFR 191. There are areas where the criterion may be ambiguous and subject to differing interpretations. Potential problems relating to the consistency of these criteria deserve investigations.

Therefore, the Board plans to work with the concerned parties to clarify the big picture issues of how to characterize and evaluate the criteria so the reasons for deciding on Yucca Mountain can be understood in lay persons language. Not doing so could result in the appearance that the decision to license Yucca Mountain hinges on detailed technical regulatory requirements, the basis of which is apparent only to a few specialists who have been involved in the high level waste issues for many years.

Such a decision process would be less understandable and therefore less acceptable to the American people.

DR. ROWSOME: I entirely agree with that, and I would take it one step further in the sense to reiterate something that I mentioned before. We, as technical people in crafting licensing criteria -- regulatory hurdles -- tend to want to divide up the population of plausible applicants into those that are acceptable and those that are not. We want to draw criterion in the middle of the population of possible commercial nuclear power plants or possible repositories to weed out the best from the mediocre.

In so doing, we are making discriminations around a little zone where the public's concern is in a much broader spectrum of possible risks and possible hazards. When we fail to mirror the spectrum of concerns that feed public anxieties we shoot ourselves in the foot, both as regulators and as agents of the public good.

I would add to that the importance of establishing a very high level of confidence for example, that the risks are not as big as those who are really frightened by the repository prospect tend to believe that they are. That ought to be a legitimate licensing criterion or at least part of the body of evidence that is brought forward.

DR. NORTH: Thank you. I will note for the record that I was reading from page 22 of the Board's first report to the Secretary of Energy and Congress. At this time, let's adjourn for lunch and try to be back in an hour, at 1:25.

[Whereupon, at 12:25 p.m., the meeting recessed, to reconvene at 1:25 p.m., this same day.]

AFTERNOON SESSION

[1:30 p.m.]

DR. NORTH: We will resume the session this afternoon with Robert Shaw of the Electric Power Research Institute giving the EPRI perspective on performance assessment.

DR. SHAW: I am Bob Shaw. I appreciate the opportunity to come and talk to you about some of the work that we have done through our EPRI projects on performance assessment methodologies for the high level waste repository.

[Slide.]

DR. SHAW: You can see the four names of the people now and then at the bottom of this slide who have been the key people who have been involved in this. I have been the project manager, Robin McGuire has been my key contractor. He's with Risk Engineering. Carl Stepp and Bob Williams have both assisted me at EPRI as we conducted this project. What I would like to do is talk a bit about what we did. I realize that many of you have heard this before, but I still want to review the highlights of the techniques we have used, some of the results we have, and then I will talk a bit about where we are going from here. I will toss in some comments at the end about performance assessment and why we think it's important and what role we think it should play.

[Slide.]

DR. SHAW: Way back when we started this a little over a year ago, the objectives that we had in front of us were to develop an integrated methodology, and we certainly emphasize and underline the word integrated, for early site performance assessment and to identify and prioritize crucial issues.

I would say that at that time things were at a much different state than they are now. At that time, roughly a year and one-half ago, there was a lot of criticism from the utility industry focused on the Department of Energy for a lack of integration and particularly in the area of performance assessment. We felt that there was really a need to catalyze and drive some of this work toward integration, and we also felt that we had some techniques that we had developed within EPRI that had been associated with the seismicity study we had done that would lend themselves very well to the development of this kind of methodology.

The second aspect was that we wanted to involve DOE in this process and its implementation. We feel we have been reasonably successful in doing both of those objectives, but I will describe to you what it is that we have done as we go along here.

[Slide.]

DR. SHAW: The first thing we did was put together a methodology development team. You will see the names of these people on the team and their respective affiliations and expertise. I will not read through that. What you can see as you observe this is that basically what we did was to select one individual for each area of expertise. We asked that individual to develop for us models in that area that would be reasonable, not conservative but reasonable. Our main efforts were really then to take these various technologies and show that they could be integrated in an overview performance assessment.

Excuse me. Let me go back one notch. Let me mention that we have added three members to this team as a result of our effort to expand the work that we are doing. Mick Apted who many of you know, formerly from Battelle Northwest and now with one of the Integras is a member of our team with his expertise in the near field environment. In addition we have a gentleman named Stuart Childs who is a soil physicist, who is assisting us in the interface between climatology and hydrology, the area that we have referred to as infiltration.

In addition to that we have Ben Ross, an expert in gaseous release because we had no gaseous release whatsoever in this first step of our methodology.

[Slide.]

DR. SHAW: I know this is a little bit challenging to read and, more important than the words, are the nature of how these things tie together. We used this influence diagram as a starter. Across the top you would read hydrological flux, earthquake occurrences, borehole stability, waste solubility, rock fracture model, canister parameters, retardation, volcanic activity, change in the water table, porosity and canister failures. These were the various inputs that we started with as we sat down around the table and analyzed what are the important features and how do they interact with each other.

The arrows and the directions were very important to us as we started this whole phenomena. Eventually what we wanted to get and derive was a source term, the mass transport of that source term, and eventually the release to the accessible environment. That was sort of encapsulated in one slide, the objectives.

What we did next was to unfold that particular influence diagram and say what we really want to do is have a logic tree. This was the logic tree that we ended up with in Phase I of our effort. It started with flux or infiltration; that is, the rainfall that actually penetrates through the soil. We then moved to node two which were earthquake caused canister failures, change in the water table from the earthquakes, volcanic activity, the change in the water table that would occur from volcanos, the borehole stability which would influence the mean canister lifetime.

From that we get waste release as generated by the waste solubility, rock fracture model, porosity and retardation. These were the three hydrology inputs to the system. This enabled us to determine the hydrology aspects of how we proceeded from our calculations.

[Slide.]

DR. SHAW: An important principle of all of this is how the logic tree diagram works. The logic tree starts from a particular sometimes input over here, the red dot, and it says something happens. In an incorporation with that we say there are alternative choices. For example, there could be very little rainfall or lots of rainfall. Those would be the two events, E-1 and E-2, and they would have a certain probability associated with each of those. Then you could move to the next event which could be an earthquake, and it does happen and doesn't happen and so on.

When you run through this at each node you get a number of options which have probabilities associated with those. If you would take a set of these, then they conduct a scenario. This scenario could be, for example, little rainfall, no earthquakes, et cetera along the chain. So from each chain as a result of the parameters you chose, there would be a scenario as a result of a product of the probabilities that would be a likelihood of this particular scenario occurring. One can march in that way through all the scenarios. The probabilities must sum to one as you end up at that pathway.

[Slide.]

DR. SHAW: You use these end branches then to construct the source and hydrologic transport calculations, and each of those lead to a release calculation as a function of time. For every one of these particular scenarios one can generate one of these colored lines. This would be the cumulative and we have the word chemical but that could just as easily be radio isotope concentration that would be released with time.

From that it's fairly straightforward to construct a CCDF in which one takes the particular selected time, starts from the top and marches down to zero until you get to the point of the first concentration, and that produces the integral that is out here furthest to the right. It would be at that concentration and the amount that you move up is, of course, the probability, P-31 that is associated with that.

Then what you get is a series of steps. As you intersect with each of these concentrations that will give you the concentration where the step occurs and the probability will give you the step change in the total curve. Using that you generate the CCDF which is to say what is the likelihood that the releases will be less than that particular value that you see along the curve.

Let me march you through the assumptions that we made. We started off from the start saying we want this to be realistic, but we also understand that with the challenge of trying to integrate all of these things it's more representative or illustrative than it is something at this stage we would say is clearly defendable. We stared off at node one. Our climatologist, Austin Long from the University of Arizona worked with us to define what do we think is going to happen in the future with regard to rainfall.

These are the probabilities that we settled on. Any of these points are arguable. They are particular to one individual who has come through with them. What we said was that we expect within the next 10,000 years it's highly likely that we will not continue to have very low infiltration at that site. The intergalactic period between the glacial movements in North America -- the interglacial period.

[Laughter.]

DR. SHAW: As soon as that slipped out I thought how many caught that.

[Laughter.]

DR. SHAW: Let me start again. We looked at the interglacial periods and said we are certain now in that period of low rainfall. There is evidence that in that area there has been periods of heavy rainfall in the past. We felt that over the next 10,000 years there is a 90 percent probability that the net flux will be on the order of one and one-half millimeters per year, and there's even a two percent probability that it will be as high as four millimeters per year. The .5 continuation we said is eight percent.

This points out one feature that we didn't have in our initial model that we do intend to have in Phase II which we have begun. That is, we had no ability to say what we really expect is we will have .5 for a number of years and then changing to 1.6 and maybe eventually going to four. We didn't have any opportunity in our model to have time dependent inputs of this nature. We are changing our model so that we can do that.

[Slide.]

DR. SHAW: The results presented in this format show you how we ended up with an output. This particular curve happens to be for neptunium 237. In our full analysis we ended up with 1,022 branches as a result of the multiplicative effect that you get from the nodes two times two, times two, et cetera. You do not see 1,022 curves on this particular diagram because many of them are less than ten to the minus three. As a matter of fact, there's a whole large collection of them that are absolutely zero over the period of 10,000 years.

What we have shown here is in color, the effects of the three branches that we had for node one, notably .5 millimeters, 1.6 and four millimeters per year. Of course, what you see is no red at all. That's because we actually had zero release for all of the scenarios that were at .5 millimeters per year. Then you do see the blue down near the bottom and the green collected on up near the top.

One of the things that this shows us rather nicely I think is that there is a functional dependence, there is a sensitivity to this particular parameter. You see the color bands in different regions. That's one of the important features we wanted to get out of our methodology, what are the areas that there is a sensitivity to. This shows the same curve without the particular diagram in the middle.

[Slide.]

DR. SHAW: This shows the CCDF that results from that. Notice that there is a single CCDF that occurs because we take weights according to the probabilities for each of those. This shows the CCDF resulted in this case for neptunium 237 normalized -- just one isotope. That's all we had in this particular case. It shows the distance, which is one and one-half orders of magnitude generally or more below the limits that were imposed by EPA.

[Slide.]

DR. SHAW: When we looked at the sensitivity with regard to node one; that is, the infiltration, this is what results. This is the curve that we just showed you, the normal curve that we get. We took the curve then where we said let's assume that it is 100 percent occurring at the four millimeters per year and you get this curve. This shows what happens if you get everything at -- that must be the .5 millimeters per year. It shows us that it's very sensitive to the infiltration that occurs as a result of the rainfall.

As we move to nodes two and three, these are the earthquake. Because we really didn't have a stronger sense at this point, we simply assigned a probability of .5 to either earthquakes that cause no rupture in the canister or earthquakes that cause ruptures in one percent of the canisters over a period of 1,000 years. Then, if we had none of these that doesn't change the water table. If we had some that disrupted the canisters then we said there is a certain probability that we have change in the water table and we took our first estimate as being right here; .9 percent we would have a 30 meter change and .1 percent that we would have a 50 meter change.

Some of these determinations are not strongly based on data that are available to us. I am sure many of you who are involved in this recognize that immediately. For example the first one has some strength in looking ahead and saying what's going to change in the climate in the future. The estimates of water table change with earthquakes are based, I think, on very limited amounts of data.

[Slide.]

DR. SHAW: The volcanic effects, you can see that we assumed there was a 0001 possibility that volcanos were going to affect hydrology. Likewise, a 0001 possibility of magmatic release; that is to say release of lava that would actually come through the site, and a 00001 probability of hydro-magmatic release which is to say the lava would interact with the water table causing a steam type explosion and the release of material that comes from that.

The water table rises that were associated with each of these assumed that only. Of course in this case with the effect of hydrology would you have a rise in the water table.

We assumed that one of the ways in which one could invalidate the integrity of the canisters is with regard to rocks in the borehole being pushed out of place or slabbed so that they made contact with the canisters. Here we assumed that there was a 99 percent probability that there would be no borehole slabbing that would cause canister failure, and a one percent probability that 38 percent of the boreholes would slab and that in all of those cases they would cause canister failure.

[Slide.]

DR. SHAW: We looked at the whole question of canister performance, and similar to a meeting that we had with Golder Associates we found that this was a very complicated process to look first at the canister and evaluate how does the water get there, when does it get there, what are the processes that caused the canister to fail. When it does fail then what happens? The water gets in the canister and how does it affect the fuel. Are there immediate releases, et cetera, et cetera.

As we looked at that whole process and said that's a neat process to model, but we are not sure in the timespan that we have given ourselves to do this that we could possibly model that. We came to the conclusion that the best way to do this was in a more statistical fashion, and we ended up using Weibull distributions. We used those because they certainly seemed to express manufactured equipment lifetimes better than any other distribution that is around right now.

They also give you an opportunity to say you have no failures for a period of time and then a distribution of failures, and then they all fail. So, you don't get the distribution that goes from infinity to infinity. We generated three of these. We said there was a 50 percent probability that we will have a canister that its mean lifetime will be 5,000 years; 25 percent that it will be 1,000 years; 25 percent that DOE and the utilities are going to be willing to spend big bucks and give us a 50,000 year canister lifetime.

[Slide.]

DR. SHAW: That's on earthquake probabilities -- I got these two slides out of sequence here. This gives you an indication of what happens with the various likelihoods of earthquakes occurring. This shows that the results here are very insensitive to the earthquake results.

Geochemical effects which was node eight, we said there are two characteristics that are important. There's really one, and that is waste solubility.

DR. ALLEN: Excuse me. Is that also mislabeled then on our sheets?

DR. SHAW: I believe so. What does it say on it?

DR. ALLEN: It says canisters on it and it was the same diagram.

DR. SHAW: Let me go back. I am mistaken. I have looked at it incorrectly. Please, erase what I have just said. Starting again, this is -- I wasn't quite tuned yet -- this is the sensitivity to canister lifetime. The clue to that is the curve that I missed which is descending right over here. There is a green dot down here, and that's the 50,000 year canister.

What it shows is that -- it's the green line here I believe -- maybe the blue line -- which is the average of all if you make the straight calculation. The other two show the difference between the two short lived canisters, but the 50,000 year canister shows an enormous benefit if you need to be in that area. It's important to point out that according to our calculations at least for neptunium you are already below the curve. It does show an enormous benefit from going to a much longer canister lifetime.

Thank you, Clarence, for pointing that out.

[Slide.]

DR. SHAW: The geochemical effects, we felt that the primary influence here was waste solubility. We looked at the whole question of influences on waste solubility. First of all, we don't know the form that the fuel is going to end up in. Do we actually -- are we dissolving UO-2, has it been transformed to U-308? Is it going to give us a dissolution of everything at the same time? What do you expect the Ph is going to be in that locality when it occurs? What do you think the temperature is going to be when that occurs?

There are a number of factors that we really didn't know, so we chose two values, two orders of magnitude different; two times ten to the minus seven MO/s per liter and two times ten to the minus fifth MO/s per liter with a 50/50 chance. What we hope to do in this case is get bounds to see what is the influence of waste solubility.

[Slide.]

DR. SHAW: I have pulled together all the last three because they were the influence of hydrology. Rock fracture gave us an indication as to whether we had high fracture or low fracture, and we also threw in a factor that we call FMULT. This was the multiplication of the flow that went by a very fast path. We have five different flow paths that we could construct that have to do with just matrix flow, matrix flow which would interact with fracture flow, and a very fast fracture flow and then each of those going through the two different media that would be dominantly below the location of the repository itself.

Then we had high and low values of porosity and high and low values of retardation. You can see that we have put 50/50 splits on each of these as an indication that we just didn't have many data to allow us to do that any better than that.

[Slide.]

DR. SHAW: The key features of what we put together here are that, first of all, it captures uncertainties. We felt right from the start that a very important feature of the whole process that we should be constructing is that it capture the uncertainties, the real uncertainties that are part of the model generation. Secondly, we really wanted to emphasize the integrative aspects of the model.

We spent a lot of time closeting people together saying we want to get you guys together, and we would like you to talk the same language. What are your inputs and what are your outputs, do they match, are they the same units, do they have the same assumptions and so on and so forth. That took quite a process to make sure that we were integrating those interfaces between the nodes.

Transfer functions are something that are becoming much more important to us as time goes along. The concept here is that we certainly -- you certainly, the people in DOE -- have very detailed models that have been developed; hydrology could be one example. The ability now to take the results of a detailed model and use those results in an overview performance assessment such as we have constructed here is an important aspect, we feel, of the next step that you go from here. That's what we call transfer functions.

Transfer functions could be just a table of data that say as you change one parameter, how does the other one change as a result of my computer calculation. It could be actually a three-dimensional surface in which you have described that aptly so it can be an input to a new system. Our continuing philosophy is that the process that you use here should be one that you can do on a PC. All of this was done on a PC, and we aim to continue that. We don't want to have a PC that's hooked up to a mainframe to get those mainframe results.

We feel you should be able to do a performance assessment on a PC that allows you to sit there and say if I change this what happens and if I change that what happens. You can look at the sensitivity of those particular features.

The fourth key feature which we just think is absolutely vital is, you have to document the assumptions, the conclusions, et cetera. We tried as best we could in our report that has been issued to do that on a section by section basis, to have everyone write as clearly as they could what were the assumptions that went into their particular aspect of the model, what were the interfaces that were important, what were the variables that had to be considered, where did the data come from and so on.

That became a very vital part of what we were trying to do here and will continue.

[Slide.]

DR. SHAW: Where do we go from here? I had a meeting with my methodology development team about three weeks ago, the first one since last summer when we last met. During the last three months we went out to all of the people who were on that list that you saw and said we are going to start the group up again. Would you like to come back? A number of these people are academics, and I was anticipating that getting back into the academic structure of doing research and all of that we would get at least a few NOES. We didn't get any.

Everybody said yes, we want to come back and we want to be involved. I think what happened is that we actually have a lot of academics who are involved in this project and who really enjoy the integrative aspects of being involved with other than just their own narrow area. I think they really consider it fun as well as being involved in something that is not esoteric but does have an application. As I mentioned to you, we added the three people to our methodology development team.

One of those is gaseous transport. At this stage we have no gaseous transport in our model. That will be a node that will be added. That is one of the efforts that we have underway right now. We will add the time dependence for the inputs. I mentioned the example of the climatology where we can do that. We can also take temperature dependence and make that also a time dependent temperature system.

We will look more at non-homogeneous hydrological properties to evaluate the changes as you go from one unit to another. Basically our system was just two units, above and below the repository. Temperature dependence with time and with position near the repository will be in this. We are looking at how to integrate human intrusion into this. Primarily we will look at WIPP and what WIPP has done, as an example of how to include human intrusion.

We are going to model infiltration better than we did before. It's clear that if rainfall increases as we expect it to at Yucca Mountain, that it will not just be a straightforward linear increase in the infiltration. When that occurs, certainly plant life increases and you get a transitional period where there is some increase. But then, you will also get a lot of uptake of this too.

So, we are going to try to model the transition over -- this will be over hundreds and thousands of years -- of what happens as you go from a desert type atmosphere into one that has heavier rainfall. We continue to look at how do you strengthen the linkages. The key throughout here is how do you link these various nodes together; what is the integrative aspect; does the whole thing make sense. We continue to try and look at it from an engineering point of view.

When I used to teach engineering every once in a while I would throw in an oddball question that had a ridiculous answer just to see if the students were looking at, does this answer make any sense. That is something that we need to do too. Many involved in this -- many of us are scientists in contrast to engineers, and there's a sense of believing numbers which just doesn't have any validity when you are doing performance assessments. We really need to look at the logic and the sensibility. You need to say does this make sense? Does it really hang together. Do I think that I could have that kind of release over the years. Strengthening the linkages, we continually want to look at does this whole thing make sense.

That completes the slides that I had, but I would like to make a few comments about where we are, where we are going, and what I see as the importance.

We have been very excited about this whole project, and I must say that the people in the utilities that I report to -- that is my source of money that I was involved with this morning in Baltimore -- looked at what we had proposed to do two years ago and said you have to be kidding. For that much money you are going to get something like that out of here? A lot of them I think are very pleased at the results that we have been able to generate for a limited amount of money, and are excited about continuing the process.

However, there is a very strong overriding message from the utilities that says we are already paying the Department of Energy to do this sort of stuff, why should we be paying EPRI a second time to do the same sorts of things. Our message is that we think you, as a utility, kind of need to be looking over the shoulders of your contractor -- that is, DOE -- to make sure things are going in the right direction. We get some sympathies for that and some not.

The general flavor at this stage as a result of meetings that I had this morning are that we will most likely continue for another year to do work on performance assessment and in some of the areas as well. I just might mention for a moment some of the other areas that are cognizant here that we are working on.

We have been working with EPA as an output of last September's EPA workshop to see if we could generate a workshop that would pull together those various and diverse opinions that were expressed at that meeting. A lot of people got up and sort of bashed away at EPA at that meeting without any resolution as to what are the key issues that EPA should be dealing with. We have been talking with them and some other people about pulling these people back together.

It looks like early fall might be a reasonable time both for the EPA and for us to do that. Our intention is to have a workshop where people would address what are the key issues that should be part of the EPA criteria as they look at their whole remanded set of issues there.

There is a second somewhat associated area that we have done some recent work on that I would like you to be aware of, the topic of transuranic burning, actinide burning and a variety of names that are associated with it. Almost a year ago now a number of people were making proclamations about this new technique that was going to solve all of our waste problems. Some people put in news letters in newspapers and elsewhere that obviously if you used the liquid metal breeder reactor and you used transmutation and partitioning you can make a 10,000 year problem just a 300 year problem.

Of course what they were saying was, you get rid of all the transuranics and everything else and just live with cesium and strontium. That is not the picture. We ran a set of three projects over the last six months to look at what is the real technology that you would use for particularly a partitioning process -- the transmutation would obviously come from the LMR -- what would be the economics of such a system, what kind of waste do you get from these systems, and would you have to have a repository.

The overriding answer to the last question is yes. Even if you had this system and even if you had a partitioning factor of 99.99 percent, you still have enough of the transuranics that come from the processing system that you still -- it's not low level waste and it's still high level waste -- you would probably but not necessarily reduce the volumes of these materials. You would still have to have a waste repository.

The concern of the utilities was that suddenly when you said I am making a 10,000 year problem a 300 year problem it takes all the focus away from Yucca Mountain. The utilities opinion and our opinion is that's the wrong focus, that's the wrong message to be giving people. It doesn't take away for the need for a repository. It continues to have a need there.

Performance assessment continues to be our overriding and our feature project in all of this. We feel that performance assessment -- as I think Russ Dyer and others said yesterday -- is a very important tool that one uses as you make decisions along this line. It is not the tool, it is certainly not the only tool, but is one of a number of tools, very useful tools in giving us guidelines as to what are the important areas, where are the uncertainties, what are the results that we need to know more about and how does Yucca Mountain look. Is there a likelihood that it is going to be successful in a licensing process when we get there.

We have to continue to do those sorts of early site suitability assessments. Performance assessment is certainly a key tool in all of that. In addition, having one outside of the DOE program gives the utilities and other similar bodies the opportunity to say maybe you ought to think about this rather than this, and so on and so forth.

I think that's a reasonable representation of what we have done and where we are going, and I would be happy to answer any questions if there are any.

DR. NORTH: Anything from the Board members?

[No response.]

DR. NORTH: I will take a couple from the audience.

MS. ALTERMAN: I would just like to know if you have a report on your transuranic or partitioning study?

DR. SHAW: No. We don't have a report, we have five. I am sorry, that wasn't very nice. If you could give me a card or something and just put transuranic burning on it, I would be happy to. We have five because we had an overview report, we had an economic report, we had a technology report, we had a waste package report, and some other reports that I can't remember right now. There are five reports that all combine together to give the sum total.

DR. DOMENICO: I have a question. Have you changed anything since the last time you presented this to us?

DR. SHAW: No.

DR. DOMENICO: Have you added -- it's the same model that we heard five months ago.

DR. SHAW: Yes. As a matter of fact, we didn't do any work on the model for about three or four months. We haven't changed it from the report that came out. We are in the throes of beginning that now.

By the way, this Phase II that I described where we are including gaseous release and time dependence and temperature dependence, our timetable is to have that finished and reported by the end of this year. We actually feel that we will have the work accomplished and done by September or October timeframe, and then writing it up and getting it published and so forth, we expect that the report will be out by the end of the year on Phase II.

DR. DOMENICO: You say you do this with PC's, right?

DR. SHAW: Yes.

DR. DOMENICO: You have everything on one program?

DR. SHAW: Everything on one program.

DR. DOMENICO: Do you have any problem with departing with that program for a while?

DR. SHAW: We can talk about that.

DR. REITER: Bob, there's a while ago that we were talking about some future workshops and the possibility of a seismic workshop.

DR. SHAW: Thank you, a point that I forgot. Another aspect of our work that is going on now was that as we looked at how you generate an overview model of this nature -- and we clearly chose one person to represent each technical area -- that's not the right way to go.

What you would really like to do is sit down in a roundtable and get about a dozen experts in a particular technical area, many of whom I think would come from DOE but certainly some of whom would come from outside DOE -- you would sit down and say okay, we got this node and call it infiltration and call it whatever you want, and we want to get a better indication of what are the branches that ought to come off that node.

We felt that the seismicity owners group that conducted work a few years ago on East Coast earthquakes was an excellent example of pulling together a group of experts and coming to some -- I will call it quantifying the uncertainty as a result of getting those people together. As a result of that we decided that the next step in this whole process really should be to run a set of workshops.

One set of workshops would be a particular technical area, and we said we can do a pilot. We are now planning to do a pilot. The topic that we have selected is seismicity, partly because we had that experience before. We are going to do a seismicity pilot work series of workshops and we are generating that now. It's not like just -- we had a workshop last December where we asked everybody who had done performance assessment to come together and talk about how you have done performance assessment. That was pretty straightforward.

This one, I think, requires a lot of pre-planning. That is what we are working on now. Questions such as, what kind of data should one use when one looks at seismicity at Yucca Mountain. Secondly, what kind of models are appropriate to evaluate those kind of data and incorporate them. That means collecting representative papers that describe the models and the data, getting some people to put together position papers where we know there are models that have not yet been published, pulling this information together and getting it to a selected group that we want to get together as the experts.

Second phase, asking questions such as Leon has been great at asking around here -- how do you effectively make use of experts. How can you get a group of people -- ten or twelve or whatever number -- together in a room, talk about a particular technical area and end up with not a consensus but a quantification of the uncertainty that surrounds this particular area. That's number two that we are working on.

Three, how would you actually conduct these workshops. What we have sort of as a vision right now is that at the first one you get a group of people together and say okay, talk about your particular area. What do you think is the best way to do the seismic analysis, and what do you think and what do you think. By the way, what do you think of the data that we gave out to you, and what do you think of the models we gave out to you and so on and so forth.

See if we can get everybody on a relatively common ground or at least a common understanding of what they feel is valid, not valid, what's up to date, what's current and so on and so forth. In some cases you may have people defending a particular position they have against others who don't agree with it and so on, but to try and get that common understanding.

Then send everybody away to do some more work for a month or two, collect them back together again and say okay let's focus on it. Which models, which data, what's the probability that model A would fit for Yucca Mountain and what's the probability model B would fit. Would we have to change those models and so on and so forth, and then just work on that.

A third get together, a third series of this workshop would be okay, let's collect together. We have produced some tentative final results. Let's examine them and come to some conclusion as to what fits. We think that's the way that one could use expert judgment in a particular technical area to take any one of those nodes and say here's what is a more in depth perspective rather than just one individual. That's a short answer to your question, Leon.

DR. NORTH: At this time we are going to have a short break for rearrangement so that we can go into a roundtable. We want to get the speakers from both yesterday and today, and then address two broad sets of questions.

One, the strong and weak points associated with the application of performance assessment to the high level waste repository; two, the strong and weak points associated with

DOE's efforts in performance assessment. In order to keep the logistics manageable, I think we want to get one individual from DOE, Russ Dyer, and one individual from NRC, Seth Coplan. All the other people from DOE and NRC in the room, we will encourage you to pitch in as well with your opinions on these subjects.

We are going to take a few minutes to rearrange the room, getting the Board Members and the speakers on the other side of the table so that we can look at each other and have more of a sense of dialogue than the present arrangements. We will take a few minutes to do that.

[Brief recess.]

DR. NORTH: We are ready to get started. What we want to do here is to have a roundtable discussion. I would like to invite the speakers to my left to contribute their thoughts first. I would see our format as being aptly paraphrased as what have we learned and where are we going with respect to performance assessment and specifically the DOE program.

Then, I would invite questions and comments from the audience as well. Let's start up here with the speakers, and to the extent that my fellow Board members would like to contribute as well. Then, to the extent that time permits and you are willing to stay, we will open it up to questions and comments from others in the audience.

I will reiterate that if Seth Coplan or Russ Dyer wishes, you can call on any of your colleagues from your agencies in the course of your comments. Why don't we start with a question of strong points and weak points associated with the application of performance assessment to the repository. Let me just start over there with Ian, and ask if you would like to say a few words for us.

DR. MILLER: I jotted down a few thoughts, and I will do the cons first. It was a little bit longer list than the pros. The first con about an integrated performance assessment is that it is clearly very hard to build an integrated model, a truly integrated model. The fact that there aren't a lot of full fledged integrated models around yet demonstrates that point.

Second con that I thought was -- you can't directly map many of the regulatory requirements into a probabilistic framework. Where you are required to show reasonable assurance or substantially complete containment and things like that, there is quite a step of judgment and interpretation required in order to translate.

The third con that I noted down was that the state of knowledge about the world and the models that we use to represent it are pretty dynamic, and it's not going to be easy to maintain an integrated performance assessment that is current in those regards.

On the pro side which I very much lean toward, there are two enormous strengths. The first is that really an integrated performance assessment is the only way that you can do an integrated safety analysis of a highly complex, highly coupled system. You just can't analyze individual pieces and then try and snap it together without doing it in an integrated way.

The other pro which really didn't come up in the discussions over the last two days that is very important, an integrated PA let's you say no to things, let's you demonstrate things that aren't terribly important. If you haven't got that capability then you can get dragged on into endless, every deepening technical debates and researches of issues simply because they are uncertain. The fact that you don't have complete knowledge about some particular aspect of your system doesn't mean that you can't demonstrate that it's safe.

With an integrated performance assessment you should be able to close off some issues and say yes, there is some uncertainty, but it's not significant to safety. Those are my points.

DR. NORTH: That last point strikes me as particularly important. If we can get this debate more focused on the issues that are really significant as opposed to those things that are scientifically interesting and that modelers like to build models on, I think that would be a major contribution.

Certainly, it is something that should have received a lot of highlighting in the last two days, and we will retain that for the record. Bob, what would you like to add?

DR. SHAW: I was hurriedly writing things down here. I will follow the same format that Ian did, and talk a little bit about weak points. It will be a little bit, because I don't think there are a lot of weak points that need to be stressed.

One not necessarily weak point but caution, is that there can be a tendency sometimes to place too much reliance on a performance assessment, for people to become too much believers of what they had calculated to forget the assumptions that have gone into it; therefore, to rely a little bit too much on performance assessment.

I see it as one tool among many that we need to use as we go through the various evaluations, be their early site suitability, license application, testing priorities, whatever they might be.

A second challenge that we are confronted with now which is more of a challenge than a weak point is, as I expressed a few moments ago, how do you make use of experts. We are certainly involved here with the process that is trying to extrapolate far into the future, and the challenge that is associated with that means that you need to use judgment. Therefore, making use of a variety of opinions and pulling them together into some quantitative statement, some degree of the determination of the uncertainties associated with models and parameters and so on, I think is a real challenge that is in front of us.

Some of the strong points. Performance assessment forces integration, and I think that's been a vital need in this program and a number of others. Many people can get, quite appropriately, involved in very great detail in their particular area and lose perspective. Performance assessment forces that integration. It gives a general perspective that is broader than just the individual technical areas.

It gives us the overview that we should have, on a continuing basis, to make iterative kinds of calculations as one moves ahead. It has flexibility, and in that sense it is interactive. One can sort of make changes on the fly as one says I don't think that is quite right, let's make changes and see how that affects things.

There is a real need to do performance assessments early, not to wait until we are well along in the process. We should be doing those all the time to get a sense of where we are and where we are going. In that fashion, it also gives us a feeling for the priorities; where are the needs, where are the uncertainties, and where are the sensitive areas that need to be strengthened.

It includes a variety of perspectives, and that's a very important aspect of it. It can bring experts together and incorporate their opinions into a single model. Maybe most importantly of all to me, it is an expression of the uncertainties that are associated with any process of this nature when we try to predict what goes on in the future. Those would be my points of emphasis.

DR. NORTH: Frank.

DR. ROWSOME: Thank you. I want to start speaking of limitations with the truism that you already know, but may be worth emphasizing again. That is, although we all recognize that one of the most extraordinarily difficult parts of a performance assessment is coping with the complete set of scenarios, we don't begin to appreciate just how difficult that's going to be really.

The reactor safety community has struggled with this with their risk assessments and have never done a good job to date. It is going to be extraordinarily high. I think the only way to get our arms around it in a way that will have the benefit of peer consensus that it is even a responsible attempt much less a success is to start right now, and that cannot be left until we have our separate analytic models better developed.

Another comment on the cusp between the advantage and the disadvantage of the central role played by performance assessment. I think we do ourselves a disservice in the arena of public opinion and larger political arena if we stop our performance assessments with the EPA ratio. I think it's highly desirable at least in the highest levels of the calculation, perhaps not as a major sophisticated analytic effort, to carry the consequence analysis out to health effects however vulnerable that may be to the ambiguities and the health effects of low level radiation which render the ultimate signal very fuzzy indeed.

Nevertheless, for the purposes of communicating in the political area, knowing what these distinctions mean in terms of health effects, how many are affected, what affect it has on the culture of humanity living at future times, proportionate comparative effects with the other forms of bio-toxicity to be expected in the environment given the scenarios that you are developing.

All useful perspectives to put these things into an intuitable context to communicate to people. I think it's worth doing whether or not it's cognizable under Part 60. With that, I will turn it over to Seth.

DR. COPLAN: I think I will start with strengths. The first one that I would mention, I think, is kind of the flip side of the first weakness that Frank mentioned. That is that I think that the reason that one gets so concerned about whether you have identified the whole universe of scenarios is that performance assessment is kind of a unique tool for analyzing the failure modes of a system and that it is so systematic, and it let's you worry about that sort of thing.

I think that for a facility like a repository which is first of a kind effort, it's a complicated system, it probably offers a kind of a unique tool for getting at the question of what kinds of things can go wrong with the repository. That said, I agree with the weakness that you point out, that it's going to be very difficult to be able to say we have all the important ones. I fully concur that I think it's something that needs to be started yesterday, really.

The main area of weakness that I see is that the models are ultimately only as good as the data they are built from; that, that's an area of weakness in the program at this point. In the long term, they are going to need to be validated - whatever that means in this program. We are talking here about validating models for a system that has a period of performance of thousands of years, and very large spatial scales. That's going to be a very difficult problem and another one that I think we need to start to come to grips with soon.

DR. DYER: Coming from DOE viewpoint, I find it hard to separate number one and two. I may transition back and forth here.

Performance assessment provides, I think, about the only way that we can take an integrated look at what must be a synergistic system. That's the tool that we have for that. It provides support, as we had said most of yesterday, for decisions. On the down side you can't oversell it. It's not a panacea like Dave said.

From our experience in use of performance assessment we find that you need to address the decision that is facing you and then figure out how performance assessment is going to assist you in making that decision. It just doesn't automatically happen. The answer is not just obviously transparent.

The weaknesses I see in performance assessment in general are ones that have been addressed by most of the prior speakers. Those are mechanistic in nature, the application and implementation of the logic behind performance assessment; how do you construct a full suite of scenarios, how do you construct a defensible CCDF. I guess a question that we are internally debating is that for use as a total system performance assessment tool does one need to take it to a CCDF. You can get an awful lot of useful information before you roll it up into a CCDF.

Perhaps in our limited experience so far, perhaps one of the strongest points that I see is that it has forced integration communications through the program. It has become a vehicle for communications. I think I will pass it on to Dr. North here.

DR. NORTH: What I would like to do at this point is open it up to other people first, taking the same order from NRC. Any of the NRC people here care to add their thoughts on this subject? We didn't have room for everybody at the head table, and I will put this on a volunteer basis.   DR. CODELL: I will give it a try. I have been interested in probabilistic assessment of performance repositories for a while, and from time to time I have dabbled in experiments. One thing that I have seen that happens is that when you apply the techniques that everybody uses of Monte Carlo or Latin Hypercube sampling to doing performance assessment of repositories, the results come out over enormously wide ranges, so wide that even if you are on the safe side -- that is, if your curves fall below the EPA limits or some other criteria -- you still calculate results that are so bad that they pick people's interest.

It's a little bit dangerous. That is, for example --to pick a simple example of groundwater travel time. You perform a calculation and you come up with a mean or median travel time of one million years but there's a .01 probability that it is less than a year, is that acceptable or not I think a lot of people would, first of all, tend to believe that number that it's possible and secondly become scared by it. Therefore, you are doing yourself and your sponsor a disservice by doing that calculation and leading people astray.

I have actually done this with synthetic experiments which I have done a couple of. One of these experiments that I did was, I sampled the results of the hydrologic parameters for calculating groundwater travel time in the way that most people had done these before for probabilistic risk assessments. I came up with answers that were over enormous wide ranges, when actually the groundwater travel time in the example problem was quite on the safe side.

That was just my point that I wanted to add on the down side, although I am generally a supporter.

DR. NORTH: Thank you, Richard Codell. I will ask that the people from the audience identify themselves so the Reporter can have an easier time of making out the transcript. Anyone else from NRC care to add comments?

[No response.]

DR. NORTH: How about the people among the Department of Energy and its contractors, the people who were on the program yesterday.

DR. O'CONNELL: I am William O'Connell from Livermore. One problem that I have noticed from working on these assessment is the enormous unknowns, unknown data and unknown models. There is a lot of uncertainty due to lack of knowledge. There is the coupled question of what are these analyses for.

If we are intended to positively prove that this repository will work beautifully for 10,000 years, then we can't prove that completely -- only with a certain degree of confidence. This raises the question of how much confidence or what are the guidelines for sufficient encouragement or confidence. That's really a caution rather than a plus or minus.

DR. NORTH: Paul, I think I saw your hand up.

DR. KAPLAN: I am actually responding to Dick's point, and it's a generic issue.

DR. NORTH: Give your name, please.

DR. KAPLAN: I'm sorry. Paul Kaplan, Sandia Labs, DOE contractor. We have to decide in performance assessment what is represented by a probability distribution when we do a probabilistic risk assessment. On the one hand we can use it to describe a state of nature and believe all those outcomes can occur. The way I used it and tried to present it yesterday is as a state of belief, given that I don't know what those outcomes are in which case, I think it's more than fair to say that there could be outcomes that are unfavorable to us until such time as we gain the information and the knowledge to preclude them.

I would argue that we are doing the public and our sponsor a disservice by not representing those outcomes, particularly at this stage of the game.

The other thing that I see as a weakness is, we have heard many strengths and there's no question that performance assessment brings more to the process of licensing a repository than -- what I want to say is that we would be a lot worse off without it. One of the weaknesses that I see in its execution is that we have forgotten the concept of analysis and we have replaced it with the concept of simulation.

I see this process that says that unless the number or the basis for the decision is generated by -- again silicon based computer -- that it has no worth or no merit. We have to get back to the concept that performance assessment is a systematic way of doing analysis with the most appropriate tools available to us at the time we do it.

DR. NORTH: Your point is that maybe we should consider carbon based computers as well as silicon based computers and use them to check each other at a very minimum.

DR. KAPLAN: The pay is the same but it still feels a lot better when at least we get some credit and the machine doesn't.

DR. NORTH: Anyone else from the roster of DOE and contractors. Max, did you have something you wanted to say?

DR. BLANCHARD: Thanks Warner.

DR. NORTH: Max Blanchard.

DR. BLANCHARD: With the Department of Energy. At the risk of being duplicative for some of the august minds in the room who have been reviewing the discussions or participating in them, there really isn't any other way to begin the process of making long term predictions except by taking the concepts embodied in performance assessment and sensitivity and uncertainty analysis and looking at how a piece of rock might behave with respect to containment and waste isolation by making predictions over long terms.

I think it is here to stay, it is essential, we will be using it and we will be learning from it. As we learn from it, we will be adapting design changes in our concepts for either the repository or waste package or both.

This is good. I hope people that are outside this area of making predictions don't overlook the value of this tool, in that it helps us understand how to make changes in a controlled way and in a meaningful way, and that they don't perceive this as the designers goofed. Look at the mistakes they have made. This is a way to learn to improve how we design to improve our ability to make predictions. I think it's very valuable.

I think there are some inherent weaknesses that go with a tool like that, and that is that we are attempting to quantify things that earth scientists don't ordinarily quantify very much. From an engineered barrier standpoint or from a facility that you can select the materials and can construct to make it perform the way you want to make it perform, it's a very valuable tool. You can pick what you want and put it where you want it, and know when you have assembled it properly it will perform in a certain manner such as the loads on a beam that is used in civil engineering.

The mountain or any other place should Yucca Mountain prove not to be suitable, the mountain is what it is. All we can do is try to understand it and we will only understand it imperfectly. There will be uncertainties that will be very large, and I think many of us may become frustrated in that quest to numericize or quantify what it is we would like to see in terms of performance.

I feel a little concerned that in our quest in this program to quantify things that are not frequently quantified in earth science, that it may drive us in terms of time, cost or numbers of people, to do things that in the end we will look back and say that turned out to be a failure. Thank you.

DR. NORTH: I think the point that you have made has an important relationship with the point that was made about the use of performance assessment for cutting off investigation, further model, further quantification on things that are not important. I think it's naive of any of us to think that as Yucca Mountain or any other repository location proceeds forward we are not going to have enormous amounts of debate and controversy. I think that's a given.

I think the issue is, how do you try to serve up the science in a such a way as to illuminate that debate and make it as productive as possible. If analysis can serve to identify areas that are clearly not important to the debate, that may go a long way. It may turn out that some of the most important issues are highly resistant to quantification. For example, issues like future climate and its effect on infiltration.

That's really not a traditional earth sciences area or at least it is atmospheric sciences rather than under the surface of the earth. Maybe it turns out that one of the most important aspects of the whole exercise it bring in some other kinds of experts into the debate and discussions that haven't traditionally been here.

In that respect, it may be very useful if those issues can be identified early on and we can get that community of people into the discussion and debate as well rather than having them brought in at the 11:30 hour by adversaries working with lawyers attempting to prevail with their particular interest in the case as opposed to in the more collegial world of trying to do an overall integrated assessment of safety in which hopefully the adversarial inclinations of the various parties are at some relatively minimal level as opposed to maximum level.

Are there any other people from DOE and its contractors who would like to add their comments regarding strengths and weaknesses of performance assessment?

DR. BOAK: I am Jerry Boak from the DOE Yucca Mountain project. As the coiner of the phrase silicon sacrament of computation, I wanted to add a few more notes about that. I think that it is true that we have to be careful about relying too much on computers to give us a certain kind of holiness.

I wanted to stress a point that Russ Dyer made in passing, that we can get a great deal of knowledge from a performance assessment even when it does not roll up into the CCDF or the highest level on the pyramid of computational tools that we have. In fact, as far as I can tell, those highest level computational tools have yet to generate a conclusion that is simultaneously credible and something that was not already obvious from much lower level models.

I may be wrong about that, but I haven't heard one yet. This does not, in any way, negate the really neat models that came out of Golder and EPRI. I think they are terrific. In fact, I think what it argues for is for their further development. It says that we have such crude models now and we need those models not only because they are going to give us the integrated picture, but because without that integrated picture we are never going to communicate the huge stack of performance assessment detail that we have.

We passed out a 20, 30 or 40 page bibliography of performance assessments. Indeed, performance assessments are done at a multitude of levels as long as they provide some decision maker with the kind of input that he wanted there are performance assessments. Those integrated ones that are going to be absolutely essential for us to communicate more broadly that we have covered the range as has been raised repeatedly here.

DR. MILLER: This is actually a question. I thought having heard Frank Rowsome this morning, that it was an opportunity to bounce something off somebody with a regulatory experience. I feel that by the time Yucca Mountain applies for a license, if it does, that performance assessments will be much further advanced than they are today. They will probably be at your fingertips in the sense of being essentially interactive.

I would think that regulators, those that have to conduct hearings and evaluate competing arguments and so on, would find that availability of performance assessment tools extremely valuable in assessing competing arguments, technical debates and so on.

Frank, I would appreciate your comments on whether you think that's credible or not.

DR. ROWSOME: I wish I did, but I am afraid I don't. One of the patterns that I observed in middle management in the reactor regulating staff was the struggles that senior management had trying to learn when to believe and then to give credence to what their technical specialists were telling them.

Their ability to filter out what they needed from the information that was percolating up from the technical specialists wasn't very good. A lot of the signal to noise ratio deteriorated severely even within one office as you went up the hierarchy. There are long list of social and psychological reasons for it, most of which I don't understand but it's a fact.

I think unless something like the EPRI conception of the desktop computer, a really friendly model that you can teach office directors and the Commissioners to use themselves works, I don't think it's going to be accessible to them. They are going to be developing their decision making heuristics based on what was passed out second hand and third hand down from the level of those who actually understand the limitations of the models and the real approximations that went into it.

We have at the moment, a tremendous advantage that we haven't celebrated yet, and that is the absence of a lot of site specific data that would allow us to get buried into the rich problem of drawing inferences from site specific data. That's an advantage that I should have mentioned earlier. It improves the signal to noise ratio vastly, that we don't have site characterization going on now.

I think I have said enough.

[Laughter.]

DR. NORTH: I am finding myself with the urge again to step out of the Chair role and tell a story. I would like to pick up the point that Frank just made and emphasize what I think is crucial, the need for educating decision makers, and I think ultimately the public on the complexity of these issues so that basically they can be informed and feel that they have some measure of control on a highly complicated, controversial situation. Otherwise, I think there is a very strong tendency to react on the basis of simple heuristics and emotions.

The story I would like to tell is the following. A couple of years ago I got Shanghaied in the EPA Science Advisory Board to chair a group on a problem that I didn't know too much about, namely global climate issues, and spent the better part of a summer wrestling with two huge reports put together by the EPA staff to Congress, reports on the potential impact of global climate change within the United States and then a report on stabilizing options for dealing with the problem.

It involved a computerized risk assessment which ran to many thousands of lines of code and a huge database, attempting to project greenhouse gases out to the year 2100 and various models of the atmosphere and ocean atmosphere interaction, and all the potential consequences of this in tremendous degrees of uncertainty.

I scratched my head and worked on it all summer, trying to come up with an overall review for my committee distilling hundreds of pages of comments down into a 25 page summary report on this subject, and finally get it out through the SAB system.

It's about six months later and I have just bought as a Christmas present for my son a new Apple Computer, because my son, age eight, is going to a school that is a test site for Apple and are learning the latest in software. I thought it would be a lot of fun to learn this along with my son. One afternoon a few months later, I am going through with him a program called Earth Watch, I believe it is. There is something in there about global climate change. I thought gee, this is interesting. Let's see what they have to say.

We got into this part of the program and are clicking along on the icons with the little mouse and getting into different frames, most of which are highly expository. You ask about something like emissions of carbon dioxide and they give you several screens full of information. I am fascinated. This material is at least as recent as our SAB reviews. In fact, I find that I am very impressed at how sophisticated this treatment is. In fact, it's almost awesome.

Here is this computer program for my eight year old that seems to be right up on the edge of the state of the art. I am wondering how did this all happen. Finally, in the last screen I find out because there is some descriptive material, that a team of 30 or so people at Apple put together this particular program just as Gorbachev was coming out Stanford, and this program was presented to Gorbachev as a way of summarizing information on the global climate issue and it references extensively the work at EPA that my Committee reviewed.

[Laughter.]

DR. NORTH: I think the moral of this story is that yes, you can do it. There are ways where you can take very, very complicated problems and break them down and package them so that hopefully our decision makers and interested members of the public can learn about these with pretty pictures, with very well scripted text and maybe with video images -- the hypercard system within Apple is going to be pulling this into my son's education very quickly.

I think one of the messages here with a stress on communication is, maybe we all ought to be thinking more in terms of performance assessment as communication and going from the 2,000 pages of reports and the great big computer programming to what that team in Apple did on global climate to boil it down to something that will play at the eight year old level that has lots of visual information and that does a good job of educating people on the basics where there is general agreement on at least the dimensions problem rather than presenting the public with the kinds of arcane debates and discussions we can have on the fine points that they are probably less interested in than in terms of getting a perspective on it and perhaps allaying some of their fears.

When we had public hearings in Nevada with one of the panels from this Board we had a gentleman who was a retired college professor get up and talk about how the repository might blow up just like the Chernobyl reactor. That is something which I don't think is possible on a physical basis but it's something that I believe a very intelligent many who went to some effort to come and talk to us thought was quite a credible scenario.

Maybe that's the level at which we should be investing some effort to try to get more people to have -- should we say the simple kinds of understanding that can be packaged in the Apple Computer program for eight year olds, and maybe this will be effective in communicating and dealing with some of the problems that Frank was describing in the Nuclear Regulatory Commission context.

DR. JOHNSON: Carl Johnson, with the State of Nevada. I am not going to comment on your Apple story per se, but I do want to follow up with your thoughts on public communication and I think public perception.

The public has a different perception of this program and the safety aspects of this program. Some of the things that we hear in the State of Nevada are concerns over possible active faults and active volcanos and this sort of thing. The public cannot see how that we can propose to put a repository site in the same area that have these other features that could go on or could be active during the repository condition.

So, to add a little bit to some of Frank Rowsome's remarks earlier that I think the program needs to go beyond the regulations and address some of these public perception points of view, some of these other factors that the public thinks are keys to health and safety but that just don't seem to be important issues in the regulations.

I think I would caution those people who are working in performance assessment to just not routinely dismiss the certain factors or certain scenarios because they do not believe that they are important because they don't specifically meet the actual points within the regulation.

DR. NORTH: Thank you, Carl. I think that point is very well taken. If you are in a situation where you have reason to be skeptical and not trust the expert, what does it take to convince that person that in fact it's a good story. It seems to me that's a question that we should all ponder a bit.

Another story I will tell from my EPA experience where I have spent a lot of time dealing with the risk from carcinogenic chemicals. I heard one senior official of the agency say about the process, how can I go before the Administrator -- my boss -- and explain that this chemical causes cancer in animals. After our investigations of it and the risk it poses, we propose to continue to allow the American public to be exposed to that chemical.

How do I give a story to the Administrator -- my boss -- that will allow him to go out and sell that as a responsible decision to the American public. This is a problem that has been faced a number of times within EPA. They have a bunch of pesticides out there that are clearly carcinogenic in animals, and they have taken decisions to allow those chemicals to continue to be used in agriculture because they have convinced themselves that the benefits are high and the risks are acceptable.

As you all know, there are organizations out there among the environmental groups that are vehemently opposed to these decisions and use all kinds of methods, enlisting movie stars and so forth, to go out and attack EPA's credibility on the basis of these decisions. I would say it's still very much an open question as to how some of those situations will evolve.

There is no question but what the core of the problem is in terms of communicating with the public. I think those of us that are in the business of doing the analysis of risks ought to think about our product as what can we give to our elected and appointed leadership so that they can go out to the public and convince the public that this is a responsible decision.

I think about a situation if I were with the State of Nevada as Carl Johnson is, how difficult it would be for an official of the state where the repository is proposed to go before the public in that state and say I have talked with the experts and I think the following analysis is persuasive; that this situation ought to be acceptable in our backyard. It is an extremely difficult thing for an elected official to be able to do, just as it is for the Administrator of EPA to say it is okay to permit carcinogens to continue to be in the environment. We have looked at the risks, and we believe they are acceptable.

It's very hard to sell that to members of the American public that tend not to trust experts and that tend not to trust representatives from their government, and are suspicious about things scientific from the point of view of feeling quite disadvantaged that their own education and background doesn't give them much opportunity to understand such things.

It strikes me that an emphasis on communication is going to be very, very critical to the performance assessment as we go forward. As long as I am talking I will add one more point of view. That is, there are many areas about science and technology where I feel myself horribly ignorant. One of them is on subjects of buying cameras or buying VCR's and things of that sort. I have friends that are interested in that kind of technology and read the magazines.

When I go in and try to look in the store between the 18 different kinds of VCR's and try to figure out what I want to buy, I feel really disadvantaged. I am ignorant. So, what I often do is try to find a qualified expert that I can get cheaply, maybe just a friend of mine who will talk about it who is not working for the store and does not seem to have an obvious bias to sell me brand "x" versus brand "y" and try to get educated so that I can make a more informed decision.

I suspect that may be what will help people. If some representatives say of the community -- let us say one of the intervener groups stands up and says our organization has looked at the nuclear waste issue and believes that it is very sensible to have a geologic repository under the following conditions. It may be that people like Dan Reicher from NRDC can have a much bigger impact as a lawyer working for that kind of an organization that somebody like Warner North can as a scientist. He will be trusted in many areas by many people where I will not be.

That says that people like Warner North have to reach out and try to see that there are more links into those kinds of communities rather than their being a good enough technical story in place and that will carry the day. I have to admit to myself, no matter how good the story is on VCR A versus VCR B, I'm too busy to understand it. So, I'm going to depend on somebody who I trust to evaluate that rather than understand it by spending the time on it myself.

Let me get off my soapbox and encourage comments from others.

DR. SHAW: I don't want Warner to feel all alone telling stories, so I have one I would like to tell myself. The story goes that a colleague of mine was interviewing for a position at another organization and had gone through all the normal interviews with the prospective employer except one. They said recently we have developed a new interview that we have as a part of this practice, that you have to talk to our company psychologist.

That's the last part of the interview. You have passed everything else and now you have come to the psychologist. It's fairly straightforward. He went in to see the psychologist and the company psychologist says I have three questions for you. They are fairly straightforward, but I would like you to take a day or two and think about them and come back with the answers.

The first question is, how many days of the week start with the letter T. The second question is, how many seconds are there in a year. The third question is, how many D's are there in Rudolph the Red Nosed Reindeer. It seems like that is fairly straightforward. The guy comes back in and the psychologist says are you ready, all set.

How many days of the week start with the letter T. He said that one bothered me a little bit, but I think it's two. He said right, what are they. Today and tomorrow. He said, I was concerned about the next day and the day after that. Let's go on to the second question. The second question is, how many seconds are there in a year. He said that one was easy. There are 12. Twelve seconds in a year? Certainly. There is January 2nd, February 2nd, March 2nd, April 2nd -- okay. Now the next question, how many D's are there in Rudolph the Red Nosed Reindeer. He said that was a little bit tougher, I got 372.

The point of the story is that I think in performance assessment as well as many other places there may well be more than one right answer. There may well be more than one answer depending on how you ask the question that is appropriate. I think that many of us as scientists and engineers think that there is a single answer to a question, a single answer toward which we are driving. Especially when you get into the probabilistic and the risk area I think not only in the work that we do ourselves we have to accept that being uncertain means that there is more than one right answer.

To be effective communicators I think we also have to come from that basis that says there is more than one right answer to these kinds of issues. That's a perspective I found it very difficult to find myself, but I think it's very clear and true of what it is we are trying to do. It's a lot like trying to deal with my children, discipline and other areas of that nature. It's a continually changing game, and I am not sure what's going to happen today, tomorrow or the next year and so on.

I like to think of performance assessment as almost having a personality so that it's something that you have to deal with on a regular basis that is going to change as time goes along. It's a perspective that I have tried to develop that I am not sure whether it fits with risk communication or not but it sure helps me as I look at it.

DR. NORTH: Thank you for adding a very fine set of stories to our collection. Who else would like to add their perspective on these issues.

DR. KING: My name is Jerry King, with SAIC, Las Vegas. I am a DOE contractor, but I won't pretend to be speaking for DOE. A couple of things that I would like to comment on. Throughout the last two days there has been a focus on the use of performance assessment for identifying failure modes which is entirely appropriate.

Dr. North, you have several times mentioned the importance of understanding what is driving the tails of the distributions for not losing the information in those tails through Monte Carlo simulations. Seth talked about - he defined performance assessment as being a very good tool for identifying what can go wrong. That's also very true.

However, the identification of failure modes, while it is an important use of performance assessment, it's not the exclusive legitimate use of performance assessment. Another use is the use as a design tool for optimizing the design for performance in the case of expected conditions. In fact, there may very well be trade offs between optimizing the repository design for performance under expected conditions versus performance under disruptive conditions.

We can't -- while we do need to focus most of our attention on things that can go wrong, we also need to remember that we have to try to understand what we expect the mountain to do in the most likely case.

The second point, there have been some concerns expressed about the use of performance assessment or about the use of expert judgment in performance assessment, possibly at the expense of data collection and the use of data to make judgments. This is my own personal opinion. I really think that this is a false dichotomy to think that there is a one preferred mode which is to use data to validate your model and make a judgment and there's another mode in which you get a bunch of experts together and use your decision analyst and put subjective probabilities on their opinions and come up with your numbers that way.

In fact, all expert interpretations in my opinion are expert judgments. In fact, they rely upon a spectrum of a lot of data being available and little data being available. Even those judgments which we might characterize as being deterministic and having a lot of data available, they are still expert judgments. Even the most accepted scientific hypotheses and history of eventually have proved themselves to be the limited or false. We have to keep that in mind.

In fact, I would even say that although the repository program probably lies near one end of the spectrum, we often think of it as being unique. I am really not sure that it is. The example that I have in my mind is nuclear power plant seismic design bases. We have a number of operating nuclear power plants right now in the Central and Eastern United States which have seismic design bases -- accepted seismic design bases which never have been and never will be validated in the classical sense of the word.

They are assumed -- most of those design bases are based upon assumptions that the period between major earthquakes is hundreds to thousands of years in the Central and Eastern United States. We don't have a database to evaluate that in the classical sense and we won't have during the operating lifetime of these plants. It's not all that much of a different situation from a repository.

We have used expert judgment in the past and we are going to have to use it in the future, and we have already dealt with the problem of having little data.

The third point is related and it's the aggregation of expert judgment. I think Seth made the statement that the staff would view algorithms to combine expert judgments with skepticism. I think that's an appropriate viewpoint. The aggregation of expert judgment should be viewed with skepticism.

However, it is not something we can avoid. We have to aggregate expert judgments. In a real sense, a licensing decision is itself an aggregation of expert judgment. A decision to not allow for example the DOE to assign a numerical weight to one expert and roll that up to come up with a cumulative CCDF, a decision to force the DOE to use that one outlying expert for example and either base site characterization or even base licensing on that one expert judgment, that itself is a decision of how to aggregate the expert judgments. That itself is a decision to give that one expert a weight of one and the other experts a weight of zero.

It just is illustrating the point that we cannot avoid the problem of either explicitly or implicitly aggregating differing expert opinions. We know we are going to have differing expert opinions. While we certainly do not want to use subjective probability elicitation as a crutch or as a means of avoiding collecting all the data that we can, the fact is that no matter how much data we collect -- whether we collect it for ten years or 20 years or 100 years -- we are still going to have differing professional opinions.

I think we need to pursue the line of work that Bob was talking about, and start dealing now with how we in fact are going to aggregate differing professional opinions because we are going to have them.

DR. NORTH: Thank you for your comments.

DR. MILLER: I would like to take Seth's side of that argument briefly.

DR. COPLAN: I was about to do that.

DR. MILLER: I think it will turn out that if in fact a repository really is safe, that under 90 percent of the conflicting expert judgments it will still prove to be safe. A benefit of performance assessment is that you can just swap one for the other. If there is no impact, you don't need to resolve the issue. You can say yes, there's a scientific uncertainty but it doesn't really matter.

There may be some real sticky ones where it makes a difference, and clearly you do have to force a resolution by some sort of process.

DR. NORTH: Seth, do you want to add to that?

DR. COPLAN: Yes, and I want to add some other things too. I think Jerry mentioned a question of validation and contrasted that with the situation for seismic design of power plants. I think that you are dealing with some just very different things there. In the case of seismic design for power plants, first off, you are looking at a relatively short timeframe -- 40 years. Also, I guess I would argue that not in the formal sense there probably has been a fair degree of validation of the concept of frequency versus magnitude curves for earthquakes and so forth.

In the case of a repository we are dealing with analytic models that in many instances are known not to be very good predictors even over fairly short periods of time; that some of the hydrologic models, transport models -- just doing relatively short term predictions with them you find that they didn't do a very good job.

Then, you line that up with the fact that in the case of a repository period of interest is many thousand of years. I think some degree of validation and some degree of assurance that the models are actually predicting in a way that you have some confidence that the regulatory criteria are being met is important.

A second area that you mentioned has to do with data versus expert judgment. I think that there's no question that data in and of themselves are useless. It's the mind of a human being using the data that gives value to it. But I think the point that I was trying to make, that is that the fewer the data and the fewer the objective lines of reasoning and bases for an argument as opposed to really purely subjective -- almost a kind of I feel it in my bones sort of sense -- the more that the balance goes towards the former as opposed to the latter, the easier it is to convince a licensing board, the Commission and I think the general public as well.

The third point that I jotted down here was aggregation of expert judgment. I wasn't sure from the way you made the comment whether we are talking about quite the same thing. What I am thinking about is a situation where you have perhaps a number of theories to the way some phenomenon can occur that may be mutually exclusive. Only one of them could occur in actuality.

Just as a kind of an artifice, one ends up assigning degrees of belief to these various theories and says okay, that represents the spread of the state of knowledge, that represents the uncertainty that we are dealing with for many kinds of decision making that may be quite appropriate.

From our standpoint, it depends on the issue at hand, the potential seriousness of things not working right, probably the kinds of spread that you see in the uncertainty. It may just be that the Nuclear Regulatory Commission would have to react in a way that the uncertainty here is simply too great and we can't license it.

DR. SHAW: There is a tendency to think that as things go into the future, naturally we have greater and greater uncertainty about them. As a result of that uncertainty, the consequences become maybe more significant. The economists have a nice way of discounting things that cost more in the future. We don't seem to do that. We seem to assume that if a package lasts for 1,000 years, releases radio isotopes, it's going to cause cancer and a lot of people are going to die. I don't know of any death causing situations now that were prevalent 100 years ago.

I would certainly assume that in another hundred years or so we are going to have cures for cancer. It is going to go away. We look at human intrusion and what do we assume, wet drill bits that are going to get down there and dig for ores. I cannot imagine us not having lasers or other kinds of equipment that do these processes differently.

We don't seem to discount that effect when we look at what is going to happen out in the future. Sure, the uncertainties increase but somehow we should have a responsible way for saying yes, the effects are also going to be discounted in a reasonable way too. I would plead with our regulatory people to think a little bit along that way, because I think it's a reasonable balance that usually isn't taken into account.

DR. NORTH: Do any of the members of the Board here have a comment they would like to make?

DR. PRICE: I am coming away from this meeting with some impressions, and one of the impressions that I have is that there is a need for a rather than a casual or conversational view of expert judgment and how it's used, a directed and determined effort to standardize principles and procedures to the extent that they can to look at the literature that is involved in the use of experts and expert judgment, to look at the ways of evaluating and performing quantification of experts and expert judgments.

Look at specific techniques that are available, of course ideas of adelphi, NGT, nominal group technique or whatever and get this into some kind of white paper or document that people can refer to and at least have as a baseline for future hall conversations and things that might take place.

A second impression that I have is in the area of scenario identification. I don't think it's incumbent upon DOE to see the -- completely foresee the future to know whether or not there will be lasers that go down or a cure for cancer, or whatever. I think what is incumbent upon DOE is to use extraordinary care to determine what is reasonably foreseeable in the future. In order to do that, we focus in on the scenarios in the development of the scenario.

If you are using extraordinary care, I don't think that you can necessarily rely on one and only one methodology in the development of scenarios. I believe you have to be able to demonstrate that you have looked at the state of the art of scenario identification and are indeed using extraordinary care to come up with the scenarios that are necessary to come up with.

I think we have both the top down, bottom up methodologies to be applied rather than going down one particular route, as seems to be the thing. Finally, I have the impression that the issue of validation of models is still very much with us. The softness of our numbers which at least one speaker said doesn't have much bearing on decisions, I think is something that is very much with us.

I believe when we look at validation and the softness of the numbers that we have to go to, that it drives us to our knees in humility. With that sense of humility about what we are trying to do, that the natural offspring of that has to be some kind of view of flexibility. Is it 50 years and shut the door one time safe and we really knew what we were doing and when we are reincarnated 10,000 years from now we look back with pride or do we have another view of it.

I think that issue of validation and flexibility is still with us as we try to look at these things like PRA.

DR. NORTH: Thank you.

DR. DOMENICO: I always have something to add. I am not a member of Warner's team here, but I thought it was important -- no, I am not an official member of this panel.

DR. NORTH: You are ad hoc.

DR. DOMENICO: I thought it was important to come to this meeting because I realize, like I think perhaps most of you realize, that really the heart of the performance assessment is really a physically based flow model. We have seen before such presentations today. For example, path was given and recognize that path had no releases associated with its outcome because they chose to keep all the flow in the matrix.

We saw Golder's representation and they had fast paths and slow paths, and presumably some releases. We saw the NRC presentation with fast paths, slow paths and whenever the infiltration exceeded some threshold value, they multiplied by a very small porosity and moved the water maybe 1,000 times faster than it's moving in the matrix. Obviously, you had releases there. I don't know about EPRI's, but you have fast paths and slow paths and you get releases.

All of these -- it has been stated about all of these that this does not represent Yucca Mountain. This represents just a model. Some day one has to be put on a table that says -- someone has to say this is Yucca Mountain. It is very important at that time that the unsaturated zone is completely characterized in terms of what basically is going on there. Whatever you put into there that will be the heart of that particular model, and that's going to dictate what is going to come out at the other end.

This was my interest in coming to this meeting. It is not very easy to characterize fracture flow in unsaturated mountain terrain. On the point of expert opinion -- if we got 50 of the world's greatest geologists in the room and asked them whether it's in the matrix or the fractures, I would suggest that we still have not -- we still do not have one expert in the room.

In order to be an expert in fracture flow and the unsaturated zone you must first visit 50 mines in the unsaturated zone, sit down and wait until it rains outside, see how long it takes to rain inside and make some sort of note on how many times it rains it rains inside and how many times basically it doesn't. In the sense that I have never done that and I don't think too many of my colleagues have, we may not have any truly experts in flow in the unsaturated zone and fractured rock and not soil.

I discount all expert witnesses when you say I think it's here and I think it's in the matrix, and I think it's not. The point is that we may very well be better off with indirect methods eventually -- the tritium has gotten down into that rock, about the chlorine 13, indirect methods of trying to figure out just what the rates of movement through that rock is. That will all be part of the characterization program.

Again, I emphasize we have heard a lot of stochastic things and statistical things, but always the heart of the performance assessment model is going to be a physically based flow model. That's where we need the information.

DR. NORTH: Thank you, Pat. Do we have any others who would like to get in one last word before we adjourn?

DR. SHAW: I will take a passing shot at what Pat just said. At least in one area I think he and I have a difference of opinion. That has to do with whether you end up with I will call it flow model or not. I don't think you will. I don't think in the time that we have alloted between now and when we get to the point of a repository there will be a flow model that is widely accepted as being the best description.

I believe we will end up with a few flow models that describe different aspects of the flow. Just as I look back at the history of reactor development we went through multi-group analysis for neutronics, and it had a variety of codes for many years that supposedly described the neutronics of the nuclear power plants. They all gave different answers. As time went on those answers converged to a certain extent and we got multi-group cross-sections and so on and so forth.

The same can be said for turbulent flow and how you describe turbulent flow and the many models we have gone through there, and we are still trying to figure out how you define the energies and so on and so forth and how you model that appropriately. It depends on the particular application as to which model you choose to represent the phenomena that you are looking for. I think that will be true here.

It will take a long time, in my mind, to evolve the flow processes we are talking about that are typical in this particular range. My plea is that as much as we have done with our nodes and branches, that one can select a variety of models that might describe that particular phenomena of flow and then put probabilities associated with the likelihood that those models do appropriately describe it and work on that basis.

I think we will be very fortunate that if, in that particular complicated arena, we get a model that is generally accepted as being properly descriptive.

DR. COPLAN: I wonder if I can comment on that.

DR. NORTH: Seth.

DR. COPLAN: That's an area where we could potentially have a problem. In other words if, for example, out of this several flow models that you have at the end of site characterization one of them leads you to conclude that you violate the standards very badly. There is no way to argue that model away. I think we would have a problem.

You wouldn't be able to just say that represents the range of uncertainty. If I am hearing you right, Bob, what I think you are suggesting is that you kind of average that in with some other models and if the average comes out okay that's okay. Is that what you were saying?

DR. SHAW: Sure. You develop this with regard to the probabilities that are associated with the belief that this particular phenomena will occur, and that on that basis you generate a

CCDF and see how that compares. One could look at volcanos as an example and say if one occurred it might have the potential for being really disruptive. The basis upon which you look at then is, what is the likelihood that that phenomena will occur and the particular scenario that develops from that.

DR. NORTH: If we can get to the point where we have identified the area of concern and maybe some ability to discriminate specific questions on the modeling or the data inputs to the models like infiltration, I think we will have at least focused the debate in a way hopefully that will contribute as this problem evolves.

I am not sure that performance assessment is going to resolve the issue of whether or not Yucca Mountain or any other location should be licensed. Hopefully, we can structure that debate in such a way that it will be more focused on science and ultimately a more useful vehicle in terms of hopefully permitting us to say after the fact that the public policy decision was a good one and the process by which it was made was a good one.

DR. BRANDSTETTER: I would like to make some comments. I am Alvin Brandstetter, SAIC. First, since we are telling stories on the history of performance assessment there was this civil service exam and three gentlemen were asked what is two plus two. One was a mathematician and he said it's four. The second was a computer scientist and he said two plus two is 3.99997. The third one was a performance assessment who has been through the works, and he answered two plus two let me think, it's somewhere between three and five.

I think it should fit in about what we are telling here. I want to comment on a number of subjects that have been brought up. Dr. North, you mentioned or suggested bringing in other people into the review process of what the project is doing. One aspect of that which is an entirely independent group has been the involvement in international programs. Related to performance assessment, you have heard -- before there was a transport model comparison and now we have INTRAVAL.

DOE has not participated very strongly in this effort because INTRAVAL, in contrast to the previous projects which also considered unsaturated flow and transport because they included people from the low level waste site the current is only high level waste, and the international community is not interested in unsaturated flow and transport.

Even with that, I believe being involved is very important. As long as we don't understand really the saturated flow very well we cannot expect to understand unsaturated flow -- we cannot expect to understand unsaturated fracture flow which is much more complicated. Of course, we have Yucca Mountain, not just unsaturated fracture flow but also saturated fracture flow if we want to give the saturated zone any credit in our assessments.

I think involvement in those activities is very important and it's not just in comparing model results but the dialogue that exists between the scientists themselves where they learn from each other and avoid pitfalls in their work -- I think that's perhaps more important than just the number crunching or modeling is similar since today we have covered total system performance assessment extensively. Also, yesterday quite a bit, as an activity that we mentioned that is call probabilistic system assessment code comparison.

I think we could learn a lot from participating in that activity because the international community has had total system performance of codes for a number of years. I grant that they are limited to saturated rocks but really the framework of the modeling, the ideas and concepts for total system performance assessment, the same whether the rock is saturated or unsaturated.

A second comment was made -- it was suggested that we go beyond the regulatory requirements in performance assessment. I would just like to mention that it has been included in the PNL program for some time, to look at health effects at radiation doses. In fact, the total system performance assessment model that they are developing -- that is in the documentation stage at this time -- includes a dose model. It's called DITTY. It's a very exciting name, which stands for dose integrated over 10,000 years.

It computes successive 70 year lifetime doses off the maximum exposed individual as a result of releases to the accessible environment. It can consider all types of environmental pathways, the ones that we have traditionally for instance included in the reactor licensing both atmospheric food digestion, et cetera.

I should also mention that there was a remark that decision analysis should be used as an overall framework for performance assessment. DOE is working on a decision analysis systems engineering based approach for the whole project right now, and as one aspect for it includes performance assessment. We don't have it yet, but the DOE is working on it.

Then I would like to mention that there has been a lot of emphasis on total system performance assessment models. It may be forgotten -- the impression may be left that the detailed process models are here just to substantiate what we do with the total -- how we formulate and use the total overall system. I would just like to mention that we are using and will be using the detailed process models extensively in the decision process as part of evaluating tests when we design the tests, when we evaluate tests as part of evaluating design alternatives.

As an example, looking at different thermal regimes in the repository for different emplacement options for the waste package or the effects on water saturation for different repository configurations in water use.

Lastly, it's just another story on the public perception and understanding. Just to show you how difficult that will be and we know already is, I just moved to Nevada last year. My wife is a teacher and had to take a college class in public issues and issue resolution to get her Nevada teaching certificate. The professor who had written the book on public issue resolution was very strongly against the Yucca Mountain project.

He also talked about the superconducting super collider, that he was against it because he didn't like breeder reactors.

DR. NORTH: That's a good one for the file. We have one more.

DR. WILSON: Mike Wilson, Sandia Labs. I just want to throw out a plea to get on with site characterization. If we build the exploratory studies facility and find that every once in a while it rains down there or we find out that it doesn't, then we will know 100 percent more than we do now about how flow really works in Yucca Mountain.

DR. NORTH: Thank you for making sure we didn't forget that point. At this point, I think I would like to move toward wrapping it up. I want to make sure that we have covered question number two, the strong points and the weak points associated with DOE's efforts in performance analysis.

A number of the comments, especially the last couple, have certainly been directed at that question as well as the more general issue of strong points and weak points with application of performance assessment to the repository overall. Do we have anymore comments we would like to make collectively on the issue of DOE's efforts in performance assessment?

DR. SHAW: I have a comment. I made this during my talk, but I would like to emphasize it again. It seems to me that Russ Dyer and many of the people associated with him have done a wonderful job in the last year; that a lot of progress has been made in integrating a variety of technologists from a variety of laboratories.

As I have viewed it from the outside, it has been quite a challenging task to do this. I think Russ would agree with me that there is considerable left to be done yet. Nonetheless, I would like to applaud them on the progress they have made and encourage to continue along the same path.

DR. DYER: Let me, since you are being positive, let me be negative here and point out some of the problems that I know we have that we have to face. We have an awful lot of things on our plate for performance assessment. Perhaps we have oversold ourselves. We are providing support to many different efforts within the project.

It has been hard to focus on some of the longer term applications of performance assessment whenever you have somebody knocking at your door right now demanding support for something that is going on tomorrow. We have to have a robust enough program to be able to handle both these long term applications that we know are going to come around sooner or later as well as the day to day brush fires that come around all the time.

That's a management problem that I face and that Max faces all the time, but it's a non-trivial task.

DR. COPLAN: I would like to make a comment each way, strength and weakness. As far as strength goes, I think it was Paul Kaplan that mentioned the importance of analysis and that we shouldn't lose sight of this in doing performance assessments. I think we recognize that the DOE program has really some first rate people doing very good analysis. Ultimately, that's what has gotten us support.

The performance assessments that show compliance with the regs, that is extremely important work and we haven't lost sight of it, and we think DOE is doing very well there.

The weakness that I guess we see is kind of the one that Warner asked me some leading questions about earlier today. When I had the chart up that showed kind of the full suite of subanalyses that go into a performance assessment. There, we think that DOE really needs to maybe move ahead a little bit more smartly in terms of trying to pull that whole set of analyses together for the reasons that I mentioned at the time.

DR. NORTH: Does anyone else have comments to add?

DR. CALLAN: I am Ron Callan, from the National Association of Regulatory Utility Commissioners. I want to pose a slightly different question to you people who are quite expert on performance analysis.

As you know, we are concerned mightily with the people that are paying the bill for all of this. One thought comes to mind. You may remember that John Bartlett has suggested that there may be three answers to Nevada. The first one is yes, the second one is no, and the third one is we are not going to find out in any reasonable length of time.

I sense here that we are making some great strides in the analytical area. What I am wondering is, since we don't have a common analytical procedure and we don't have agreements on a lot of the equations -- granted, we don't have the data --we couldn't put together my versus your set of analyses and come out with a comparison of whether it is in Arizona or wherever.

Is there a danger here that we cannot get to the end of the tunnel in sufficient time with sufficient resource. I am thinking not only for the efficiency of this with devotion of reasonable amounts of resources, but I am also thinking of the semi-rhetorical but still worrisome question that I have seen on the face of people in public meetings that I am involved in. It's either stated as you can't or more openly, can you ever find out what will happen over a 10,000 year period.

DR. NORTH: Does anybody want to comment on that? DR. SHAW: I think it begs for a response. Ron knows well that we have worked closely with Steve Kraft of EEI and others who are concerned about the expenditures and who search for when do you reach the point where you say that's enough in context with we may not know.

I think it's difficult to decide right now whether we have the models, whether we have the capability. At this stage in my mind, it is worth the effort to proceed. We look at Yucca Mountain and we say it's very complicated. In some people's minds that suggests other places are less complicated. I don't see that as being a truism.

I think as you move to other places there are different kinds of complications that arise that, in my mind, make it no less difficult to go through a performance assessment and make analysis as to how things would happen there. I don't think the Europeans in the saturated zone are having any easier job of deciding what models are appropriate to predict what is going to happen in the future.

The question of 10,000 years and a very logical skepticism that comes not only from the public but from the scientists as the extent to which you can predict that, I think has to be wrapped together with the question of regulations and performance assessment. There is an intimate tie that deservedly belongs together here. The ultimate aim certainly is to protect the public health and safety.

At the same time we talk in and out at various times about retrievability, and people have suggested that all you need to do is make this place retrievable. I always come back and say everything is retrievable. Retrievability is just a question of money, as to how much you are willing to spend to get that stuff out of there. If it turns out that you put things in there, and despite your best predictions things do not turn out, then just as we are doing elsewhere now you go back and retrieve those things that really are giving you a difficult situation.

I don't think there is any answer that we are going to come up with that says sure, we can take care of 10,000 year predictions. There will be uncertainties that are associated with those just as Jerry King's reference to the seismic issues. Those are only 40 year issues. In the State of California the buildings are decades old and were built at times when we didn't know about earthquakes and how to predict them. We're left with that.

I think that doesn't suggest that we have reached the stage yet where it's not worthwhile to proceed. I will put it in a positive fashion. We are still in a situation where we should proceed with performance assessment, with Yucca Mountain and with the appropriate analysis that says there are ways for us to take care of in a responsible fashion the nuclear waste that we are producing that we feel the public is benefitting from as a result of the nuclear power that it is generating.

DR. PRICE: Bob, would you make the distinction that there's a difference though in designing for retrievability versus designing without retrievability as a goal?

DR. SHAW: Yes, I would. I would say there's a different basis of design. If one was to decide now we are going to design for retrievability, I would expect that design to be different than the particular circumstances we have now.

DR. NORTH: It being a few minutes after four at this point, I think this is probably a good time at which to wrap it up. If I see any hands shoot in the air in the next ten seconds I will consider that maybe somebody has a burning comment. Since I haven't seen any since I first posed that, I think that I will make my concluding comment and declare that this panel meeting is completed.

My concluding comment follows very much along the lines of what Bob Shaw said a few rounds ago. I feel very positive about how much progress I think there has been in the two years since this panel had its last meeting. This is a very, very difficult problem area to figure out how one goes about assessing the safety of a high level waste repository.

My perception coming on to the TRB and going through the first several meetings including the May, 1989 meeting on performance assessment was that there was a lack of momentum in the DOE program. I think over the last two years we have seen a great deal of momentum building up and a great deal of development outside DOE with the NRC effort, the

EPRI effort and the Golder effort, which fits into not Russ' program but on a slightly different way within the DOE umbrella.

I think the fact that all of you are here, that we have as much interest as we do in the professional community and where performance assessment is headed, is all very healthy. I don't think that as this issue proceeds we are going to easily find ourselves with one model or one methodology. I think there is going to continue to be a lot of interaction between them.

Again, to pick on an analogy involving my eight year old, I think about the little project we had a while back -- polishing up some semi-precious stones by putting them in a little grinder and letting it grind away for a matter of weeks with various kinds of abrasives in there. I think the process of developing a performance assessment may be a very similar one, having the models and the various groups grinding away at each other over time asking what if questions, trying different approaches will ultimately make for a much better basis on which to proceed in this area.

I am very delighted with the results of this meeting to see all the evidence of progress and the very active impressive efforts that are presently underway. I look forward to the next meeting in the future when some of these efforts have gone to the next set of milestones and we have even more before us that we can look at in terms of results and insights.

On that point, let me thank all the speakers and thank my fellow members of the Board, and declare the meeting adjourned.

[Whereupon, at 4:07 p.m., the meeting concluded.]