UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

FULL BOARD MEETING

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Plaza Suite Hotel
Las Vegas, Nevada

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Dr. Patrick Domenico, Co-Chair, Morning Session
Dr. Clarence Allen, Co-Chair, Afternoon Session
Dr. Ellis Verink, Member
Dr. Donald Langmuir, Member
Dr. John J. McKetta, Member
Dr. Edward Cording, Member
Dr. Dennis Price, Member
Dr. Garry Brewer, Member
Dr. Warner North, Member

ALSO PRESENT

Dr. William Barnard, Executive Director, NWTRB
Dennis Condie, Deputy Executive Director
Dr. Leon Reiter, Senior Professional Staff
Dr. Sherwood Chu, Senior Professional Staff
Dr. Robert Luce, Senior Professional Staff
Dr. Carl DiBella, Senior Professional Staff
Russell K. McFarland, Senior Professional Staff
Dr. Nava Garisto, Consultant
Dr. Michael Apted, Consultant
Ms. Paula Alford, Director, External Affairs
Frank Randall, Assistant, External Affairs
Ms. Vicki Reich, Librarian
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant
Ms. Nancy Derr, Director, Publications

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P R O C E E D I N G S

DR. CANTLON: We'll convene the second day's session of the Nuclear Waste Technical Review Board. My name is John Cantlon. I chair the Board.

This morning's session will be a continuation of yesterday's session on the source term. The chair for this morning's session will be Dr. Domenico, and after lunch, where we switch to the Ghost Dance Fault, the session will be chaired by Dr. Clarence Allen.

So, Pat, it's all yours.

DR. DOMENICO: Good morning. I'm Pat Domenico. I'm co-chair of the Hydrogeology and Geochemistry Panel, and I've first a few announcements. There's a break scheduled for eleven-fifteen, which is three hours from now. We're going to change that. The break will be at 9:55.

The second announcement is again, today, they are having a buffet lunch for $5.95 like they did yesterday.

A few years ago, or maybe not so long ago, maybe a year or so ago, the Performance Assessment Panel heard some presentations on transport modeling. Imbedded someplace in those discussions was a source term that we did not hear too much about at that time. Yesterday, we heard perhaps all we wanted to know, and perhaps even more about the details of a source term. Today, we will hear about which of those details are actually imbedded in the source term that will be used in transport modeling.

So for lack of a better title, we might say that we're looking at today, in the practical sense, the source term in transport modeling for performance assessment.

So with that, we'll get started right away; first, by hearing from Bill O'Connell from triple L, dealing with combining processes, an engineered barrier system source term. Following Bill's presentation, we will have four presentations on the actual use of the source term by different groups.

Bill?

DR. O'CONNELL: Good morning. I'm Bill O'Connell from Lawrence Livermore National Laboratory. This morning we'll be looking at the source term from a different perspective, starting from the use of the source term and working backwards, or the input information that's needed for the source term, and the first three talks this morning are by DOE contractors, and these talks are correlated with each other.

Now, the first talk, my talk, will be how we pulled all these different pieces of information together to make a source term; the oxidation and dissolution of the spent fuel as you heard yesterday, and many of the other elements we heard yesterday; and also, the local hydrology and the container breach process, and the processes inside the waste package, the hydrology and the transport. So pulling these together into a source term model will be the subject of my talk, and then the two following talks by DOE contractors will also talk about the source term model and its use in systems applications.

Now, expanding on the introduction, there have been two trains of calculations carried out in the Total Systems Performance Assessment-91 and this was presented to the Board in April of this year. So today, the source term, rather than the total system, will be the main focus of those, but there were two trains of calculations. They were coordinated so they were working with the same input space, but for slightly different purposes to cover the whole space or to do several detailed calculations within that input space.

At Livermore, we developed a simplified source term model based on our experience with our other source term models, and transferred the model to Sandia, and they used it in the Total Systems Performance Assessment.

Now, a few view graphs of the general survey of the field. First, a source term integrates over the performance of a large number of waste packages spread out over a large area, which may be a portion of the repository or the whole repository, so we have to look at the local conditions of all these different waste packages, what are the responses of the individual waste packages to those conditions, and then add up those responses to get an area-wide release rate, which is the source term going into a total system transport analysis.

And particularly for Sandia's total system analyzer, which samples many times over a broad input space, we want a source term which is simple so that it can be iterated many times, and of course, we want it to have the major features of the process results, of the processes that are part of the engineered barrier system, and as a side feature, we want to avoid the minor features so that we don't get bogged down in details, and because of its application it should be good over a broad range of parameters, and it should use the total system parameters as inputs where appropriate; in particular, in the hydrology, the percolation flux downward through the mountain, and the saturated hydraulic conductivity of the matrix.

Now, this is using a conceptual model of Sinnock and others. It was published in the site characterization plan, whereby the downward percolation is carried by the matrix, and so if there is too much percolation, then the excess goes over into fracture flow and seepage flow. That's one possible conceptual model, and that's the one we'll be using in this source term.

Still looking at the big picture, the near-field environment is an influence on the engineered barrier system and on the transport through the near-field zone, and then these different zones are also transport pathways, resistive transport pathways, and in the first application we didn't specifically consider the near field as a transport zone, so we sort of avoided the question of where to do the hand-over of the release rate, you know, from the engineered barrier system to the far field. It could be here or here, but we sort of avoided that problem, but we did include the near-field environment as an influence on the source term.

Now, doing the source term, we have to keep in mind that we have the full repository as a system, and at the heart of the repository we have individual waste packages, and the averaged hydrology goes into the local hydrology at the different waste packages, and there is a spatial variability to be expected, and one of the new features of this source term model is an explicit treatment of this spatial variability and hydrology.

Then we look at the individual waste package responses. The concepts in the individual waste package model would date back about two years to the Working Group 2 joint effort in 1990, which was presented to a panel of the Board in 1991, but using those concepts, we get the releases from the individual waste packages, and then just sum them up to get the area-wide averaged release rate.

Now, the issues we have to keep in mind are, first, linking the processes on a broad scale and on an individual waste package scale, and including localized variations about an area-wide average, and one thing we should keep in mind but did not in the first application was the correlations among the inputs and correlations of the outputs with the inputs. For example, the release rate might be higher in a local area if the hydraulic flow there is higher, and so the release rate and the transport capability are both correlated through the hydrology.

Now, looking at the individual waste packages at the heart of this EBS system, we started from the outputs, which is the release rate as a function of time from the waste packages, and worked backwards as to what do we have to know in order to calculate that release rate, and as in the 1990 joint effort with U.C.-Berkeley and Pacific Northwest Laboratories, we decided to focus on these later processes in the sequence of events.

So the mobilization of the waste form from a solid to a soluble form, and the transport from the edge of the waste form into the host rock, of course, these processes depend on inputs from numerous fields; for example, the mobilization depends on waste form properties, the geochemistry and the hydrology, and the whole thing depends on having breach of the containers.

Now, these other processes we're taking as inputs right now, but in the future evolution of the model, we could put process models instead of input structures. Now, the reason why we're being so simple in this approach is because even at the simple level, there are many alternatives which have to be considered.

The radionuclides, they are high solubility and low solubility. This affects the mechanism by which they're mobilized and released, and there are a few gases, and some of these are in two or three different locations within the spent fuel, and particularly important are the cladding surface and the fuel cladding gap where soluble elements could be mobilized quickly, and those in the spent fuel matrix, which can only be mobilized slowly, as the matrix is altered or dissolved.

In the initial model, we'll be looking at the high solubility elements in both locations, and the low solubility elements, which are mainly in the matrix, and the gas is not included in the initial model, but Sandia added a treatment of the gas before they went into their total system model.

Now, this concept of taking the broad average, individualizing it to the individual waste packages, and then adding it up again is shown in a data flow diagram format here. So the first sub-model is the local environment model, where we take the broad averages and find out what the local distribution consistent with those averages is, and then apply this local distribution to the waste packages, and it turns out to be convenient to subdivide these waste packages into sets according to the type of water contact they have. Then calculate the release rates and sum them up, and finally get a release rate time history for any radionuclide over all the waste packages in an area.

Now, for the next segment I'll be talking about the local environment model that went into this. This is primarily a hydrology model, but I'll say a few words about the geochemistry at the end, in view of all the presentations that were made yesterday about geochemistry.

So we did treat the local variability of the hydrology by a few concepts and equations which I will get to. We treated variability in the rock mechanics in the sense that there is initially a gap--at least in the conceptual design--there is an air gap, and this gap may get filled over time through weathering or spalling from the borehole rock wall, and whether it does or not, that is an input parameter in this model, but at least it is considered, and the container breach times are considered as an input in the model.

Now, within a single waste package as well there is variability at different locations within the waste package. Only a fraction of the spent fuel is likely to be wet, given these low water flows and the partially-saturated conditions and high drainage possibilities.

The simplified model assumed that a fraction of the fuel was wet, but this fraction was consistent from one package to another. We didn't consider further variability.

Some specific features of the base case hydrology that were posed for this set of calculations by the total systems people and hydrologists; first, they had a distribution of percolation flux. This was based on alternate possible futures. It's one flux for the whole mountain, but it could depend on future climate, so there's a distribution. The average was about one or two millimeters per year, which is somewhat higher than we were considering as anticipated conditions, and that will have an influence on the waste package's response to this much of a groundwater flux.

Now, as the average flux increases, the local flux at the different packages will all increase, although there will still be differences from one package to another. More packages get wet, and they get more water.

Now, we wanted to have a response which varied smoothly, actually, as a function of the average infiltration flux. Different packages may have a sudden large transition from fairly dry conditions to fairly wet conditions, but different packages might see this response at different average infiltration levels, so we didn't want to treat all the waste packages the same and have an artificial big jump in their response.

Now, how this works out for the local environments, for the waste package to have any water contact and, therefore, an aqueous release of radionuclides, it has to have some water contact. Either rubble could provide a pathway for diffusion, or seeping water could provide a mechanism for advective transport of radionuclides, and we assumed these two things were independent, and we assumed that the local percolation flux was lognormally distributed.

This concept was picked up from Duane Chestnut of Livermore, who published a paper in the International High-Level Waste Conference proceedings last April. He looked at Stripa data and some saturated hydrology conditions data, and found lognormal distributions of spatial variations in the flow under various conditions, so we picked up that concept to give some specific way of addressing what the variabilities are, and we assumed that, as I said earlier, if the total flow downward gets too high, then the excess goes into seepage through fractures, and that these are independent.

And the diffusion coefficient in the rubble zone depends on whether it's only moist conditions, or whether there is seepage over the surface of the rubble.

Now, this shows four different possible futures for the average percolation flux downward through the mountain, ranging from small to high, and as you go toward the higher averages, we're assuming that the standard deviation is proportional to the mean, so the distribution gets broader, as well as going to higher values. We're talking about values on the order of 0.5 or 1 mm/yr of Darcy flux downward.

Now, the response at individual waste packages to such a flux is assumed to be what's shown here. As the total flux gets above this threshold for seepage, then the excess goes into seepage. So the seepage flow which can provide advective transport goes up linearly here, and we're assuming that the diffusion coefficient has a step change, just from, you know, it's higher when there is some seepage flow as well. This was a step change of one or one and a half orders of magnitude, actually.

So the output of the local hydrology model, we can calculate the fraction of waste package boreholes that have seepage flow as a function of the average flow, and an input that's passed through is the fraction where the walls have provided some rubble, and each of the waste packages has one of these four modes of water contact; either one or the other, or both.

Now, if there are both types of flow, then there are two parallel pathways away from the waste package, and we have to take that into account in the transport. Now, as far as geochemistry in the near field and impinging upon the waste packages, we did not treat variability in this, but I have to say a few words.

Particularly within a waste package, the geochemistry will be dependent on the hydrology for the following reasons: Incoming with the groundwater are a certain amount of groundwater chemicals which control the chemistry, and then at the same time, part of the uranium surface is wet, and that is producing uranium into a chemically-reactive system, and if the uranium overpowers the incoming chemicals, then it's a uranium-controlled chemistry and we just have to keep track of its ratio; take the water influx per year times the concentration of calcium, and you take the fraction of surface of spent fuel which is wet, and an additional factor depending on how many grains down from the surface are wet, and the surface reaction rate.

Now, this particular interest in calcium arises from a calculation of Bruton and Shaw in 1987 that showed that for low amounts of uranium, the solubility is controlled by the solid phase of a calcium-uranium silicate, which precipitates out, and the recent work by Gray yesterday, the surface reaction rate of uranium in a flow-through system that separates this reaction rate from other things that could be going on in a low-flow system, so now we have at least some access to all the needed information and we can calculate this ratio.

And it does appear that the uranium does control the chemistry within the waste packages, so we have a low flow system in most of the waste packages; you know, low water to uranium ratio.

Now, getting back to the higher level of calculating the release rate from the various waste packages, we've organized the release rate according to the type of radionuclides and their locations, and then by these water contact modes. So there are many different cases, each of which has to be considered.

Now, for one radionuclide type and location, this data flow diagram shows the little expansion. The several arrows indicate the different water contact modes, and the release rate and time history is calculated for each water contact mode, and then the sum over a group of waste packages with similar properties is done in this part of the calculation, and the total sum is done here.

Now, expanding on this release rate calculation at the waste packages for a group of waste packages, the data flow diagram looks like this. Now we're getting down to a single waste package, or to a group of waste packages concerning breaches at different times, and we have to look at the waste form alteration. If a waste gets wet, then its alteration is spread out over considerable time, and if some waste is mobilized in a soluble form and it's release from a single waste package is spread out over time, and these various time processes are coupled by a convolution over time, and the net result is a time history of the output.

Now we decided to treat this by looking at the parameters of a time history in a schematic way, and earlier work from 1990 and '91 show, that for the high solubility radionuclides, the release rate curves looked generally like this. They went up and they went down within a predictable time; sometimes in less than 10,000 years, or in the case where there's diffusion through a rubble zone with a large retardation, then this could last considerably over 10,000 years. But at least we could see the end; we knew when it was going down.

Now, the low solubility radionuclides rise and reach a steady plateau, with a constant diffusion conditions or with a constant advective flow, then this release rate would be constant over a very long time period. So these are the characteristic curves. I showed two examples that are in the handouts from previous work which was presented in 1991 to the panel on performance assessment.

Now here's releases from the highly soluble radionuclide, and for different water transport mechanisms, you still see releases go up and they go down, so we've decided to fit all of those shapes with this single standardized release rate shape that has a rise time and a decline time, as well as a delay in starting.

Now, the most important parameter is the decline time, which is the longest time constant in the curve, and which pretty much determines what the peak release rate is. And in case you're interested in the breakthrough in the early parts of the curve, then the rise time and delay time are secondary parameters of this curve, and we did a similar standardized shape for the solubility limited radionuclides.

But getting back to the high solubility radionuclides, and the three processes that were involved in time, there was the container breaches, which depend on the time spread in re-wetting of the containers after a hot period, and the time spread in breaches after becoming wet, and we used the one parameter model for the breach rate of the waste packages given that they had become wet.

In the waste form alteration, we assumed that the alteration was proportional to the amount remaining. In previous work, we assumed a constant value until it was all gone, but with the first order, any shape is about equivalent as long as it has the right properties over a reasonable period of time.

And transport. In the flow-through case, the transport goes rather quickly, and in diffusion it would be a much longer spread over time. And we can calculate these curves by taking the longest time constant in each and doing a square root of the sum of the squares to get a time constant of the output. So that's an approximate treatment, but it does capture the primary features.

Now, the main parameters which are important--and, of course, all of them are uncertain at this time, but the important uncertain parameters, I just listed a large number of them in the--and look at the handout for the details, but in total system hydrology, there are several parameters. In the waste package hydrology, there's the fraction of the local flux which actually gets into the waste package, and the fraction of the fuel surface wetted.

In rock mechanics, the influence is the fraction of boreholes with rubble and how much of the waste package is contacted by that rubble, and there are some parameters for diffusion.

And in geochemistry, waste form interaction, that's, of course, very important and we only handled that by input parameters, but the fuel matrix alteration rate, we were taking a maximum rate over a range of temperatures and conditions, and element solubilities, a maximum over a range of conditions. So this really sums up the things which were presented yesterday. We were taking maximum values, and this does not work very well. We will have to have a reiteration and get more detail into the model, because by taking maximums and simple bounding values on the chemistry and on the hydrology, this is over-predicting the releases; and the container parameters as well are important.

Now, looking ahead to the interplay of the source term model with the total system model, for the next iteration, we would want to consider whether there are trends in these downward percolation fluxes across the repository, or trends in the chemistry, because there is a correlation of the water flux on an area-wide basis, and the source term on that same area-wide basis, and because the repository is fit into one bed of tuff--which is at about a 10 degree slope--there are differences in the distance to the water table and in the number of rock layers, so, therefore, in the transport time. So there's some correlation through the hydrology in both source term and total system performance.

But summarizing what we learned from the first simplified model, with the assumptions that I described before and the results that are at low, say at a low distribution of downward percolation flux, you might have just a few per cent of the waste packages with a seepage flux, but those few per cent will contribute most of the source term because of the higher diffusion when there's seepage present, and because of the advective flux with the seepage. So a few per cent can contribute most of the source term.

However, in the total system problem that was posed in the last exercise, the average percolation flux was fairly high, so I think more than 50 per cent of the waste packages had percolation flux, and there was a fairly high release rate calculated because of those assumptions.

We can see qualitatively that there are correlations induced by the hydrology among the container performance, the release rate, and the total system transport performance; and similarly, if we go to a fracture-flow model such as Buscheck and Nitao's, temporally transient and spatially distributed flux down through fractures, non-equilibrium flow, then we could have a set of flows in fractures and this would again be distributed over space, and most of the fractures would not be carrying any flow, because you are limited by the total influx constraint, and would have similar results as we found even in the matrix fracture interplay model, that a few waste packages would have a substantially high release. So a small fraction of the waste packages would control the whole release.

So in summary, this model does consider the spatial distribution in an explicit form, and it's based on concrete ideas backed up by data which was published within the last year, and as far as chemistry, we would like to include that in the next iteration and we're looking, at least, for some handle, some concrete ideas to make it more than just a "what if" approach. So as ideas come above the threshold level from these various detailed studies, we can incorporate them into the total system model.

Thank you.

DR. DOMENICO: Thank you, Bill.

Any questions from the Panel; Warner?

DR. NORTH: Warner North.

Could you describe to us the state to which this has been implemented; in other words, how far have you gone from essentially a conceptual description of this, to implementing it with numbers and carrying out sensitivity analysis? You gave us a set of qualitative insights, and I'm not really sure what backs those up. And then, what's the state of the documentation of it?

DR. O'CONNELL: Well, this model, you know, I was saying this year we did this and we did not do this, and within those limitations, that conceptual model and specification were transferred to Sandia. They did implement it in their total system analyzer code, so it has been used. You'll see some results when Mike Wilson makes his presentation.

DR. NORTH: Okay.

DR. O'CONNELL: We did not do any sensitivity study using the full computer code. The sensitivity considerations I alluded to were just derived from the equations, or looking at the conceptual model, but it's easy to do a sensitivity analysis on part of the problem. When adding up all the different components of the problem, then we really would need to use the computer code and do a formal sensitivity analysis that has not been done yet.

This model is documented in Sandia's report on their total system calculation. I have the report which documents the rationale for it that's in progress. It's limited by budget and priorities.

DR. NORTH: So the short summary is the documentation that exists is in the Sandia report?

DR. O'CONNELL: Yes. It does describe the equations and assumptions.

DR. NORTH: What strikes me as interesting is I don't recall quite as much focus on the spatial variability issues in that report as in what you've presented, and I would be interested in seeing a little bit more in that area.

DR. DOMENICO: Any other questions from the Panel members?

DR. LANGMUIR: Don Langmuir.

Overhead 17, I was encouraged by the wording at the top, which said, "Geochemistry variation is a fertile field." What I was interested in, in particular, was your finding that calcium-uranium silicates, where the basis for selecting a source term for uranium was to use that mineral set as the source term rather than the oxide field itself, and it occurred to me--pardon me?

DR. O'CONNELL: No, the oxide field was used as the basis for the source term. The calcium-uranium silicate would be used as the basis for determining the solubility of the uranium only, but the--in principle, the source term could be--I mean, the matrix could be altering and then re-precipitating in this other form, so you have a churning going on, which gives you a non-uniform release of some other elements.

DR. LANGMUIR: Yeah. The only point I wanted to make was that this is a very tricky business in the sense that whether those solids are there at all will be a function of the water content that you assume present at the time of a breach. So at very low water contents, you're going to get highly-soluble phases produced, which then could be secondary source terms. At very high water contents, you're going to have less soluble phases perhaps controlling the releases, and so you have to be flexible on what you pick for your phases that control the releases away from the waste and make it a function of the water content you assume. It's a very tricky moving target.

DR. O'CONNELL: You mean, as a function of the water chemistry or of the--

DR. LANGMUIR: Well, the water content itself will define whether a highly soluble phase even exists.

DR. O'CONNELL: Yeah. I agree it's a--there is a lot of details that have to be pinned down before you really know the geochemistry. The calculation I referred to was a scoping calculation, using J-13 water, or concentrated J-13 in water as the assumed incoming water, and then gradually titrating more uranium into it over time, and the time axis was reaction progress rather than actual calendar time. And now with the surface reaction rate, we can convert that into a calendar time. But there are many factors that would have to be checked.

DR. DOMENICO: Any more questions?

DR. APTED: Bill, we've seen yesterday some, oh, some proposed alternate designs. I don't how the terminology goes within the project. I don't want to misname them, but obviously there is some consideration of different emplacements from the SCP-type emplacement.

Could you comment on the applicability of these models--and maybe the subsequent speakers can address the same issue--to handle these, you know, if an alternate design is proposed for the tunnel emplacement, are we still in good shape with applying these same models?

DR. O'CONNELL: Yes. We have a suite of models, and recently we have been doing some work in collaboration with Babcock and Wilcox on looking at the performance considerations for alternate designs, such as some of these in drift designs. So we have been doing sensitivity analysis there in relation to design rather than total systems, and we have a diffusion model which can handle the emplacements with a backfill rather than an air gap, and we have an advective model which can handle the seepage flow, and whether the air gap is an effective barrier. So we have a suite of models and we hope to combine them into one grand model in the future.

DR. DOMENICO: Any other questions from the Board?

I have one easy one, I think. The advective flux is merely a carrier here. What controls, in your source term, what controls the rate at which these materials enter into that carrier? Is it strictly solubility controlled, or what are the details?

DR. O'CONNELL: For low solubility elements, it is their solubility which controls their rate of entering this advective flux. For high solubility elements, it is the matrix alteration rate, the uranium oxide fuel pellet matrix which controls. Now, we're assuming that this matrix alters and part of it can re-precipitate either as a secondary phase with calcium, or a secondary phase with a different oxidation state of uranium, so that there is a forward progress of the uranium oxide surface alteration even under these low flow conditions.

Now, there may be some limiting factors which would bring this progress to a halt, but we are not including that in the model. I think that's an interpretive approach.

DR. DOMENICO: So this is merely--it's rate controlled. It's reaction to actual surface. Where did that information come from on that?

DR. O'CONNELL: We took our rate from Chuck Wilson's experiments, where he was looking at the release of cesium, iodine, and so forth as an indicator of the overall reaction progress. Now, this may combine grain boundary release as well as matrix release, but we took that as a bulk indicator of what fraction of the uranium oxide fuel pellet mass in its cracked and fragmented form could be released per year when it's contacted; in fact, when it's immersed in water.

Now, the other experiments--for instance, by Walt Gray--on flow-through tests would give other surface reaction rates, and I think he said they may be a factor of ten lower, and if you consider a low flow system with solubility or secondary precipitants, you know, you may go up or down on the order of magnitude, so that becomes more complicated.

DR. DOMENICO: Are those complications imbedded in your term?

DR. O'CONNELL: No. We took our results from, as I said, this one series of experiments, sort of a static reaction progress experiments dissolving various elements, and those static tests do incorporate a number of processes going on, but other conditions could give you other processes and other net rates.

DR. DOMENICO: And then the last thing, do I understand that this is the source term used in the Sandia model? This is what you developed here?

DR. O'CONNELL: Yes.

DR. DOMENICO: Any questions from the staff?

(No audible response.)

DR. DOMENICO: Perhaps we have time for one from you people out there.

MR. McGUIRE: Robin McGuire with Risk Engineering.

In this uncertainty characterization of the infiltration rate which is, as you say, a lognormal distribution assumed, are you assuming, effectively, that that distribution represents the range of flows, of continuous flows from just under the subsurface to the saturated zone?

DR. O'CONNELL: We're considering the percolation flux which gets, say, below the root zone and percolates all the way down toward the water table, and we're assuming that in the repository horizon, in that type of rock, that the spatial distribution is as I have described. Now, it's lognormal. We have no idea what the variance should be, so that's an open information issue. It could be different at different horizons, but it's the same total flux. It could be a different variability.

MR. McGUIRE: But you're assuming that if at some point in the repository it is at the mean +2 ▯ level, you're assuming that that mean +2 ▯ flux is continuous down to the saturated zone; is that right?

DR. O'CONNELL: No, there could be mixing, say, if you have more concentrated flows in certain local areas, these could mix together at the next layer below the repository horizon. So we're not making any assumptions on whether that happens or not.

MR. McGUIRE: You're not making any assumptions? Okay. All right.

DR. DOMENICO: We're going to have to go forward now. Thank you, Bill.

We're now going to hear about the Sandia model source term. Actually, we have two presenters here and the first one will be Mike Wilson, giving us some information on the source term for SNL total performance assessment.

DR. WILSON: All right. I'm going to follow up Bill's discussion with some of the application of the source model in our total system performance assessment, and discuss the results a little bit and some of the things that we want to do in the next iteration of the total system performance assessment, and then Rallie is going to follow--Rallie Barnard is going to follow with some other aspects of the source term.

One thing I want to emphasize is that we're talking about a source term for total system performance assessment, which has rather different needs than the source term that would be used for the more specific EBS performance assessment, and Bill has already alluded to some of this; the fact that we need to couple the source term into flow and transport calculation going onto the accessible environment, and the fact that the EPA requirements force us to do probabilistic calculations, whereby we need to calculate a lot of realizations, make us want to keep our source term relatively simple compared to the kinds of source terms that they used in the EBS performance assessment.

So the trick there is to make it simple so that it doesn't take too much computer time and that we can still understand it, and yet, somehow retain what's important.

I'm going to just talk about the areas where we expanded or added a little bit to what Bill has already talked about. First of all, let me mention that we didn't include all the possible radionuclides in our calculations. There is a set of ten that we used in the calculations where the source term was coupled to transport calculations. In some of the other calculations, the human intrusion and volcanism calculations, where the releases were direct to the surface without any transport calculation, then we included a much larger group of nuclides.

But for the transport calculations, we included the ones that are highly mobile that we expect most of the releases to be from and, in addition, we included a few of the high inventory actinides, so we think we have a representative set of nuclides that should encompass most of what we expect to be released.

As Bill talked about just a minute ago, the basic releases in the source model are divided into two classes; the alteration-limited and the solubility-limited, and they have different shapes for the release rate curves, and we picked five of each for our calculations, and the model is strictly for spent fuel at this point. We didn't do any modeling of glass waste, and in coming up with the inventory for it, we simply took a mix of 60 per cent PWR and 40 per cent BWR fuel, with some specified typical burnup.

Now, as you may recall from the presentations we made to the Board last April on our total system performance assessment, we included two different models of how the water flow takes place in Yucca Mountain in the unsaturated zones; one, the composite porosity model assumes kind of a widespread flow pattern with the matrix and fracture flows tightly coupled, and that is the model that Bill was assuming when he developed his source term. And so the source term applied to the composite porosity flow calculations is exactly what Bill already described.

The other water flow model was what we call the Weeps model, which is an episodic fracture flow dominated model, and we had to make a few alterations to the source model to fit it into this framework. First of all, Bill talked about how he calculated the number of wet containers and the number of containers that were only moist based on the hydrology inputs. Now, the whole basis of the Weeps model is a calculation of how many of the waste containers are contacted by water from flowing fractures, so we replaced that part of the source model with a different algorithm for calculating how many are wet.

And then these other two are simple approximations that we thought kind of fitted into the philosophy of our Weeps model. We only had releases from the wet containers, so that the moist ones were not contacted by any dripping water, and so the assumption was that they simply did not fail, at least within 10,000 years. And we included only advective releases from the flowing water in the fractures, and not diffusive releases for this. These two would be easy to modify, but I don't think they would affect the results significantly. This change in the algorithm for calculating how many are moist and wet does make a big difference.

We also wanted to calculate gaseous release and transport, and the source model that Bill developed was intended for aqueous releases, so we had to kind of make do with it as best we could given the amount of time that was available.

First of all, this I don't think is really an approximation. Carbon-14 is the only nuclide that we think is going to have significant gaseous transport. I don't know that anyone's done an exhaustive study on that, but that seems to be a fairly general consensus, and the primary form of it should be in carbon dioxide.

Now, the source model that we used didn't have any provision for cladding failure or anything like that. The interior of the containers is basically just one black box, and so the quick release part of the Carbon-14 from the surface of the cladding was lumped together with the quick release part from within the fuel rods, the gap and grain boundary parts, as one prompt release fraction, and that's a pretty typical thing for people to do anyway.

The releases, or possible releases from fuel assembly hardware and from the inside of the cladding--I mean, the matrix or the cladding, if you will--was not included in the source model. My feeling is that the releases from the fuel assembly hardware might be significant, but I don't know. So that's one place where we're not really completely conservative, and the 40 per cent there is the--what we included was the Carbon-14 in the fuel pellets and in the little layer that's on the outside of the cladding, and that amounts to a little over .6 of a Curie per metric ton, and that is 40 per cent of the old Carbon-14 inventory of one and a half Curies per metric ton.

Now, as Rich Van Konynenburg talked about yesterday, he's done some work on revising the estimates of the Carbon-14 inventory, and he revised it downward to one Curie per metric ton, and if you go by that, then we are including 60 per cent of the inventory.

We didn't do any kind of calculation of how the Carbon-14 gets out of the waste container. We assumed that as soon as it's mobilized, that it's released.

Okay, so let me talk about the results a little bit, and this is going to take a little bit of explaining. This bottom one is something that I think people are used to seeing. This shows our final results of the CCDF of the releases to the--cumulative releases to the accessible environment over 10,000 years compared to the EPA limits, and the two curves here are for the two different water flow models.

The top one is something that is probably a little foreign to people. It is a probability distribution of the peak release rate from the EBS compared to the NRC release rate criterion of 10^{-5} per year, and what this is showing us, then, is that for the Weeps calculation, something like 7 or 8 per cent of the time, the NRC criterion was exceeded, and for the composite porosity calculation, 100 per cent of the time it was exceeded.

Now, I'm not presenting this to tell you that I think that the release rates from the EBS are high or anything like that. This is intended to put our source model into perspective and show you that the releases from the source model, as we applied it, and with the input parameters that we used, are very high. And so that's something important to keep in mind.

However, it turned out that the travel times to the accessible environment were long enough that it compensated for those high release rates, and the final values are still well below the EPA limits.

In the gaseous releases, the story is similar, is very similar for the release rates compared to the NRC limit. These curves look pretty much just like the curves you saw, and that's because we didn't have, as I already said, we didn't have a lot of special things in the source model for handling Carbon-14. The source model for Carbon-14 is just about the same as the source model for technetium, with the different inventory and different prompt fraction, but the story is pretty much the same.

Now, the story for the releases to the accessible environment is rather different because, in this case, the gaseous travel times are not long enough to compensate for the high release rates, and so there's two things to keep in mind about this:

Number one, I strongly believe that this is a very conservative source model, and if we--as we hope to do--go into some of the parts of the model that are the most conservative and try to come up with more realistic versions of them, to move these curves over, then it's going to move these curves over as well; and secondly, the way we did the Carbon-14 transport calculation is also probably pretty conservative, and we're going to try to refine that somewhat, also.

All right. Now let me go on to some of the things that we hope to do in our next performance assessment iteration. As you know, we've been committed to iterating these total system performance assessments every one and a half to two years, and so these are some of the ideas we have for directions we want to go for the next iteration, and chances are, we're not going to be able to do all the things on this list. It's going to depend on how many people are available and how much time is available and how much money is available and all those kinds of things, but these are some of the things that we know are important and need to be looked at, if not in this iteration, then in the next one.

The first two are things that seem to be on everybody's mind these days. We really want to look at the performance of some of the new container and emplacement designs and thermal loadings that everyone is interested in right now. We think that people are really, really interested in seeing how these things stack up as far as their total system performance.

We want to look at, take another look at the radionuclides that we included in the calculations and see if we can think of other ones that may be important and need to be included, and that's something that is somewhat scenario-dependent, you know. Depending on the kinds of processes and events that you have going on in any given calculation, you may need to revise the list of nuclides you include in the calculation.

We need to include models for releases of Carbon-14 from the cladding and from the fuel assembly hardware, or else show that they're not significant.

We need to come up with a model for releases from the waste glass. We would like to look at the performance implications of colloid formation and transport. One really key aspect of the source model we have is the matrix alteration assumption or hypothesis, whatever you want to call it, and based on the kinds of things that Bob Einziger and Walter Gray presented yesterday, one thing that's likely is that that matrix alteration is highly temperature-dependent, and so this fits in with what I said before about looking at the variations in thermal loading, and in order to be able to look at variations in thermal loading, we're going to have to put in some of these temperature dependencies like that, and I probably should have listed this one next. Other temperature dependencies may be important as well. We may need to put in something about how the solubility varies with temperature and how container failure varies with temperature, for example, but we're limited in how much information we have on some of those things.

Another thing that is important is the coupling between the source term and what's going on in the far field. In the current source model, there are three of the source model parameters that are coupled with the far-field hydrology, and that's the number of containers that are in wet conditions, and the amount of flux that those containers see, and the affected diffusion coefficient depending on whether it's moist or wet. We would like to include additional variables, you know, strengthen the coupling.

One obvious example is container failure. You would expect that containers in wet conditions probably fail quite a bit quicker than containers that are in only moist conditions.

If possible, I would like to see us include the container and cladding as barriers to the transport out of the waste package. That's a difficult thing to do and hard to justify, and so I don't know if we will get very far with that.

Many of the source parameters in our calculation for this TSPA were just taken as constants for convenience, and we need to develop distributions for them. One simple example is the fraction of the containers that have rubble infill in their boreholes. We just said that to be 50 per cent for these calculations, but it would be a simple matter to use a distribution. It's not as simple a matter to come up with some sort of justification for the distribution that you use, which is why we didn't do it the first time, and there's many other kinds of cross-correlations that you can imagine that could be important, and we would like to look into that.

To sum up, the source model that we used for these calculations, I think, is a real good start, but it's probably too conservative, and in the future calculations we'd like to work on relaxing some of the conservatism, and also, to look at some of the new design options that people are interested in these days.

DR. DOMENICO: Thank you much, Mike.

Any questions from the Panel; Don?

DR. LANGMUIR: Don Langmuir; Board.

Mike, in light of the new energy bill, C-14 may be a moot point, but does your model consider retardation or retention of C-14 as CO_2 in the carbonate systems? It should do that.

DR. WILSON: Yes.

DR. LANGMUIR: As it goes up through the far field, that is part of your model?

DR. WILSON: Yes.

DR. LANGMUIR: How have you quantified it?

DR. WILSON: Whether we've included as much retardation as there should be is uncertain. We have a coupling between the carbon dioxide and the carbonate, or bicarbonate in the water. We don't have any coupling with the solid, and that's something that could increase that retardation that we have not included yet, and it's also something that's temperature-dependent, and we haven't got the full temperature-dependence in the transport calculation, either.

DR. DOMENICO: Yes, John?

DR. CANTLON: Cantlon; Board.

You indicate that in your next--some of your future iterations you're going to look at the glass wastes, and also look at colloid formation in transport. Are there any data that would suggest colloid from the glass wastes might enhance mobility of the spent fuel?

DR. WILSON: I don't know enough about it to be able to answer that. I think that there certainly is evidence that the glass forms colloids as it breaks down, but I don't know enough to really address the subject.

DR. DOMENICO: Any other questions from--yes, Nava?

DR. GARISTO: Nava Garisto.

I didn't understand your boundary conditions. For example, at the source, you were saying that the radionuclides--the technetium, selenium and others--were released based only--limited by alteration of the matrix?

DR. WILSON: Um-hum.

DR. GARISTO: I don't understand why this should be valid.

DR. WILSON: Well, I should probably let Bill answer that since it's his source model, but I'll try. The idea is, as I understand it, is that as the UO_2 oxidizes to the U_3O_8 state, that the technetium and iodine and carbon and those things are freed up for transport.

DR. GARISTO: This seems to be inconsistent with the presentations yesterday that showed that these kinds of radionuclides are released from gap and grain boundary and their releases are not really dependent on the dissolution of the matrix.

DR. WILSON: We're including the gap and grain boundary part as a separate thing, but the question is whether the releases from the fuel matrix are important. If we only have releases from the gap and grain boundaries, then the releases are comfortably low and we're safe, but we feel like, to be conservative, we need to consider the case of what if there is a significant amount of release from the matrix or the fuel pellets. Without that, we have very little trouble in meeting the standards.

DR. GARISTO: And what boundary condition did you use at the exit?

DR. WILSON: For the advective ones, there is no really boundary condition. You just have some amount of water flowing through and it is saturated with the nuclides--or it's probably not saturated in the case of the technetium because it's rate-limited, and then it carries it out.

For the diffusion calculation, they set the concentration to zero right at the borehole walls, and calculated the diffusion across a 3 cm gap.

DR. DOMENICO: We're going to hold further questions and let the other part of the Sandia team speak, and then maybe we can open it up to the first three presenters.

Rallie Barnard?

DR. BARNARD: I'm going to talk about an aspect of the total system performance assessment analysis that was not discussed at all in the April meeting for the Board, nor is it contained in very much detail in the TSPA document.

And what that is, is to look at one sensitivity study done on the source term; and specifically, it was done on the human intrusion analysis which used both the standard source term, as you've heard about previously, and what I call a detailed one, which contains more information. All the remaining analyses of the TSPA-91 use a standard source term.

Well, what is the detailed source term? I think we can all agree that there are a great number of uncertainties about the source term, but one of them that we can look at fairly easily is to ask whether the standard source term glosses over some of the differences in the radionuclide inventory which could arise because of a difference in the reactor types of the fuel that we're looking at, the degree of burnup of the fuel, and the decay of the fuel since its discharge from the reactor.

Well, the standard source term that was used was taken from the SCP, but abstracted--to use our favorite word for TSPA-91--to make it more computationally easy to use, I guess. The detailed source term was taken from the characteristics data base. That's a document and a project done primarily by the folks at Oak Ridge, which is to characterize the exact nature of all the spent fuel being discharged from commercial reactors since the start of time.

The standard source term, as we used it--and as you have heard before--is 60 per cent spent PWR fuel, 40 per cent BWR, where the burnups used for the PWR and BWR are as follows here. Furthermore, ten years was used as the decay time for the fuel. If you stop and think about that, based on the current schedule of the repository, that's a fairly short amount of decay which has occurred.

Now for the detailed source term, what I did was to look specifically at both PWR and BWR spent fuel inventories as a function of burnup and decay, and develop the detailed source term. You've seen this picture before in a number of incarnations, but this is my 3-D color version of it, and what it shows it the discharge year starting in 1970--this is for PWR. This is the first date for which there are data in the characteristics data base--going out to the year 2040, which is the projected end of the data base. There are no further projections for discharges of fuel.

It plots, also, the burnup given here in megawatt days per metric ton of uranium, and so you can see that there is a great range of burnups and decays, peaking at roughly 40,000 megawatt days in about 1990-some odd.

The detailed source term was not a general purpose source term. It was somewhat specific in its application, because it was designed to only be used for looking at the consequence of disruptive events; and specifically, I used it for the human intrusion drilling scenario. Some of the assumptions that I made were that the repository was going to be active until roughly 2040, which happened to coincide with the end date of the characteristics data base, and that the repository would be loaded with the oldest fuel first, and the reason that this is for disruptive events only is because it was assumed that the repository would be closed and everybody would have gone home before the disruptive events would start. It's quite unlikely that people would be on top of the mountain drilling while other folks were down inside working on the mountain.

So the point of this is that the most sensitive measure of the source term would be to actually look at differences in inventory, and a disruptive event such as human intrusion drilling which directly brings material to the surface without any filtering process in the way of aqueous or other transport will be a sensitive measure of the differences in the two inventories.

So in order to be able to make this computationally usable, I grouped the inventories as shown on the previous slides for the PWR inventory as a function of burnup and decay into ten-year increments, and then I calculated the weighted average burnup for each of these decay groups, and figured out if people started loading the repository from oldest fuel to newest fuel, what proportion of it would be PWR and what proportion would be BWR, what would be the burnups for those, and what would be the proportion of the entire repository for those groups.

Well, the results are that there is very little difference between the base case, which is the blue line here. This is the CCDF showing the cumulative release to the accessible environment, similar to what Mike showed previously. There is very little difference between the results for the base case in blue and the detailed source term in green.

The reason for this is that the repository inventory for the detailed source term breaks down into roughly 25 per cent apiece for fuel which is 30-years-old, 40-years-old, and 50-years-old, and with burnups of about somewhat higher than any value in the standard source term, but in order to get a significant difference--which is the most significant is seen down at the lowest probability--you have to have a confluence of unlikely events, such as hitting a very low or a very high burnup source term--or inventory--and doing it early in the game or late in the drilling--in the repository life cycle, and those are unlikely occurring events. Otherwise, it appears that the results are substantially the same.

Well, that's for the overall picture for all the releases. If we look at the three or four radionuclides which contribute the most to direct releases, we see plutonium, americium--^{240}Pu and ^{239}Pu, and ^{241}Am roughly contributing in the 30 per cent range, and cesium next.

If we look at some of those individually, here's what you see if you look at the actual inventory of Plutonium-239 as a function of burnup and decay. Here I show a log, on the log scale, the decay years, and you can see from two years all the way out until you get very close to 10,000 years, which is pointed out here, the inventory is roughly level. This is not a surprise, based on the half life of plutonium.

Furthermore, if you pick any single year and go across the burnup, you find that there is roughly a difference of a factor of three.

Well, the implication there is that if you randomly picked a time--as we do in our simulation--and then you look at the variation in burnup which you could get, there would only be roughly a factor of three for this component of the release occurring. Well, a factor of three is not very much in the scale and the precision to which the initial TSPA has been done, and so this is a contributor to the lack of difference in the overall.

Americium-241's kind of a pretty picture. It starts low, builds up, but its short half life means that by 10,000 years, it dies off. However, you can see that there is a much greater variation in both time and burnup, and so you would expect to get greater variation from the releases of that, and Plutonium-240 is quite similar.

Well, I don't think that for the initial TSPA's that we're doing it would be necessary to vary from the standard source term that we're using. It does not appear to contribute any more to the precision of the CCDF's than-- there isn't any greater precision by using the detailed source term than the standard source term. However, it is clear that for individual radionuclides you can get a wide variation in the releases, and so if those radionuclides turned out to be the significant ones for releases such as due to aqueous transport or something, it might be necessary to be a little more specific about the nature of what the source term looks like.

For the TSPA-93, we may decide to include the inventories of individual radioisotopes for the aqueous releases from a detailed source term, and the last thing I want to point out is that what we used was the characteristics data base, based on the ORIGEN program from Oak Ridge, and this is what I'll call the old characteristics data base. It has a discontinuity in it based on the fact that this segment of the curve, the burnups calculated were done with a single enrichment for one of the points along here, and on this segment of the curve another enrichment was used. And as you can see, is produces a discontinuity in the inventory.

The new characteristics data base, which is literally being published as I talk, takes care of this and uses the proper enrichment for each one of the burnup values, and so in the future, any detailed data base, and for that matter, the standard data inventory that we use, any detailed inventory or the standard one, we'll use the new quantities data base, which will eliminate this.

I should also point out that in your handout, the scale on that is correct, and on my beautiful color view graph, it's wrong. I picked up the wrong data.

Are there any questions that I can answer?

DR. DOMENICO: Well, thank you; thank you, Rallie.

Any questions from Board members? Any questions for any of the first three people? Yes, go ahead, Mike.

DR. APTED: Rallie, take this the right way, but it seems what you've--on the first day Dave Stahl defined source term as release from the EBS into the host rock. It seems to me what you've shown us, albeit important, has nothing to do with source term, and to keep calling it source term does a disservice, I think, both to the importance of your work and to the quite separate sort of work that other people are doing on the true source term. I mean, the definition Dave put up on the first day really is, I think, the one internationally people operate to. Human intrusion has nothing to do with source term, just as it has nothing to do with far-field modeling. It's something quite different.

DR. BARNARD: I agree completely, and in the paper that

I'm writing, I make a specific point in the second paragraph of saying: "Any further reference to source term which I use in this paper is my poetic license for talking about strictly the inventory," and I recognize that, and it's--maybe it's a shorthand that I shouldn't be using, but I do it anyway, so there.

DR. DOMENICO: Any other questions from Board members?

DR. APTED: I have one more for Mike.

On your slides where you were sort of normalizing to the NRC release rate for release from the near field, I didn't quite understand. The NRC release rate is on a nuclide-by-nuclide basis, so how can one--and normalized to the thousand-year inventory, so how can Carbon-14 have a normalized NRC release rate greater than one ever?

DR. WILSON: Mike Wilson.

I'm not sure I understand your question. Carbon-14 has a limit which is something like 10^{-5} per year, and if its release rate is higher than that, then it exceeds it. I calculated--for doing those, it was calculated for each--

DR. APTED: How can you reach more than 100 per cent?

DR. WILSON: It was calculated for each nuclide individually. Well, that just means that the release rate is great than 10^{-5} per year. How--I don't see how that's difficult.

DR. APTED: Oh, okay. All right. I see, so a given waste package--

DR. WILSON: Well, it's for all the waste packages together, not for a single waste package.

DR. APTED: Okay. We'll talk about it later, then.

DR. WILSON: Maybe the normalization is confusing you. I've divided by the 10^{-5} in doing that.

DR. APTED: Okay.

DR. WILSON: When it said, "normalized release rate from EBS," I had divided by the 10^{-5} per year to make a dimensionless quantity.

DR. APTED: All right.

DR. DOMENICO: Any questions from staff?

DR. REITER: Leon Reiter of the staff.

Mike, I wanted to ask a question about the EBS, the release rate, and I'm not sure if it's to you or to somebody in DOE. Is this the first quantitative calculation of a release rate? I think I've seen some qualitative estimates. Is this the first time it's been done quantitatively?

DR. WILSON: I'm not sure if I understand the question. There have been many quantitative calculations of source terms in the past.

DR. REITER: No, of the release rate.

DR. WILSON: Of release rates, certainly. I mean, for example, in the PACE-90, they had a whole report on calculations of release rates.

DR. REITER: And did they show, also, this exceedence (sic), as you indicate?

DR. WILSON: I can't remember if they did or not. One of the things that goes into that is differences in what you use for parameter values, and we used a much higher water flux than they used in PACE-90. I think Bill wanted to say something about that.

DR. O'CONNELL: Yeah. The PACE-90 calculations were published in a Lawrence-Berkeley lab report, as well as in a conference paper, and the release rates did appear to be higher than the NRC limits, and the report was qualified by the statement that these are hypothetical input values and bounding assumptions that were being used for the calculation, so the fact that the answer comes out higher does not indicate that the real case would be higher than the NRC's limit.

In other words, we started with a simplified model and bounding values for the input parameters, and the results appeared to be quite high. Now, that is just a starting point of an iterative number of calculations to refine the assumptions and data values.

DR. REITER: I understand. So you're saying the bounding case could be high. Is there any basis for DOE to assume that once they have a realistic estimate, it will be less than, and what's the basis for that?

DR. O'CONNELL: Yes. Now, taking this second generation simplified source term model as an example, we assumed that 20 per cent of the fuel rods would be wet in a flow-through case, but yet with a influx of one liter per year, that's one drop of water every 30 minutes, and it most likely would just wet the pathway of one drop of water trickling down through a bundle of fuel rods. So perhaps one fuel rod or one-tenth of 1 per cent, rather than 20 per cent. So there's a factor of a hundred or so that we were giving away by assumption because we did not have any real calculations of this internal hydrology yet.

And similarly, in the geochemistry, we were taking bounding values which may be a factor of ten or more too high, and in the diffusion release, we were just considering diffusion across a rock/rubble zone. We were not taking into account the resistance to diffusion of the materials inside the waste package.

So there are many areas where, with more data and more models, we could do much better, but we have just assumed that those good features are not present to make a simplified analysis as a first cut.

DR. DOMENICO: We're going to have to move along. Thanks, Bill, Mike, and Rollie.

We'll now hear from the PNL representatives. Dave Engel will give us some information on the source term views in their performance assessment.

MR. ENGEL: Just to follow up a little on the question over here, the analysis that I will present is very similar to what we did in the PACE-90 work a year before, and so the results are very similar. And so, that'll be the same results pretty much that I show here, and so we can look at that, too.

Similar to what Sandia did, at PNL we did a parallel analysis on the total systems, and in particular, I did the source term analysis starting from the same, pretty much, scenarios and data base as what Sandia did, and so that's what I want to describe today.

What I want to talk about is briefly describe what we did in our total systems performance assessment, just briefly, also, describe the models, and look at some results of the analysis. And then we were asked, what happens to our release rates if we look at different thermal effects, and so we did a simple analysis to see what would happen if we changed the thermal loading on the analysis, and then just some little conclusions at the end.

For the TSPA, we looked at several different scenarios. In particular, we looked at the base case or the nominal cases of the repository. We looked at the effects of tectonics and the volcanic activity, and we also looked at human intrusion scenarios. Source terms were specifically calculated for the base case and the human intrusion analysis, and that's what I'll describe.

At PNL, for doing this TSPA, the source term was calculated using the AREST code, which is a code that we developed at PNL. The transport was then calculated either using SUMO or using MSTS, and MSTS was used for the transported Carbon-14 in the gaseous phase, and then we calculated doses using either GENII or SUMO.

Specifically, again, the source terms were for the human intrusion scenario--and I'll briefly describe that in a few slides. The base case scenario or analysis was done on spent fuel and glass. We looked at different infiltration rates on the system; .01, .05, and .5 mm/yr, and then we also looked at diffusion-controlled releases. And again, we looked at gaseous releases where, one scenario, we looked at no infiltration and just the release of the gas. We looked at analysis where we had a low or .01 mm infiltration, and we also looked at the effects of container failures, where we looked at early failures whether we assumed that they failed as a uniform distribution between 300 and 2,000 years where the containment is all gone; and late failures, where we looked at 2,000 to 5,000 years.

For the human intrusion analysis, what we did was we assumed that someone drilled through the container and displaced a container down to a lower aquifer, either the tuff aquifer or the carbonate, the Paleozoic aquifer, and then we used either an advective flow-through model, or we assumed a diffusive wet-continuous release model to transport to the host rock.

Uncertainties in this analysis were the different drilling times at which they would drill through the repository, and we also simulated the groundwater velocity in the aquifer.

And this is just a little picture to show the scenarios that we did look at and the source terms for the human intrusion scenarios where, again, we drilled through the repository and in one case, we would pull the entire container up to the surface and calculate exposure, looking at the entire contaminant. And in another analysis, we would drill through the repository, miss a container, and exhume the rock and the soil which is contaminated by the release; or we would drill through the container and displace a waste package down to the lower aquifers, and then transport it away.

I apologize for my cold, so--good timing. The base case analysis, again we used gaseous release of Carbon-14, where we looked at as soon as a container has failed, we would have an instant release of the gaseous parts of Carbon-14 where we'd assume it's all gone in a single year out of the EBS, and we coupled that with a slower release when there's a water environment, and we coupled those together and in MSTS we calculated the gaseous release.

And then we looked at the waterborne releases, when there's water after container failure, looking at our flow-through release models and what continuous release models, and in this analysis, since it was for this total systems analysis, we kept it quite simple, and we assumed that the cladding was no barrier to the transport. We simulated containment failure and we assumed once the container was failed, that we lost the entire containment, and release for the waterborne release then would start releasing as soon as there's water in the environment, as soon as the temperatures dropped below some re-saturation level or re-wetting level.

And in this analysis we looked at the uncertainties included; the simulation of a containment failure as a uniform between 2,000 and 5,000 years for the waste packages. We simulate temperature for each single waste package, and then we looked at temperature-dependent boundary conditions at the waste form surface, where we did calculations using EQ 3/6, which was described in its entirety yesterday--well, not entirety--but we calculated solubilities as a function of temperature and used those values in our analysis, or when we used glass, we developed a glass-reactive controlled model, which calculates a concentration of the glass at the waste-borne surface as a function of temperature and such, and I'll describe the briefly.

This picture shows basically the design that we used, and the same design that was used earlier in the Sandia analysis, where again, we place a container in this borehole where there's a 3 cm air gap to the host rock, and in this analysis, we're looking at a wet drip water environment, where we assumed that there's water going to be dripping onto the container at some infiltration, and we assume that the water can drip onto the container, flow through the container, and drip out the bottom after it has dissolved the fuel.

In our wet-continuous or moist-continuous environment, we assume that there's a rubble-filled zone surrounding the waste container, allowing a diffusive pathway of water to flow into the waste container, and then also flow out of the waste container, and use diffusive release models in this analysis.

In the AREST code, we've developed--or Pete McGrail at PNL has developed a glass dissolution model, similar to what Bill Bourcier talked about yesterday. We used EQ 3/6 to estimate concentrations of different elements as a function of reaction progress. From that analysis, using EQ 3/6 as a function of temperature, in the AREST code we use a math balance analysis to estimate or calculate reaction progress as a function of the glass dissolving, and also transport away of the waste as it dissolves. And so we calculate a reaction progress and we go to the analysis using EQ 3/6, and at a given reaction progress, we can estimate a concentration at the waste form surface for each element, and use that--or each nuclide, and use that concentration to transport to the host rock and out of the EBS.

Next I'd like to show some specific cases, just some examples of typical analysis or results that we got for this TSPA. Here's a human intrusion analysis where we displaced a container down to the tuff aquifer, and then used our release models for transport into a host rock, which was one meter away from the waste container. And in this analysis, we see that the nuclides that are controlled again by the alteration rate of the waste form, the fission products, are the higher released nuclides, where again, we have the--as shown earlier--the slower releases of the solubility-limited models that dissolve--or are limited by the solubility. So that's just a typical result from the human intrusion, and that's about all I want to say about the human intrusion, just to show a little results, and so I'll skip the next slide.

The base case analysis, where we look at--in this case, we're looking at an infiltration rate of .5 mm/yr. These results are very similar to what we would get with the different infiltration rates. Again, we used .01 mm/yr and

.1 mm/yr, and in our models, in our analytical models, the flow rate is a scaler on the equation. That's if we have lower infiltration rates, the release rates will be lower, and such if it's higher. And so again, we see that our alteration, dissolution-controlled nuclides are the release rates that are much higher.

And we see the effect here of the simulation of the different waste containers, where we assumed that they failed between 2,000 and 5,000 years. But again, we don't get any releases until we have water actually in the system, and in our analysis, it was somewhere around 1500 years or so in most waste packages until the temperatures actually dropped below some saturation value. And these results are similar to what we got in the PACE-90 work earlier analysis.

And here I'm just showing the fractional release rates, where we just normalize the release rates by the thousand-year inventories of each of the nuclides, and we see that the highly-soluble nuclides are up above 10^{-4} parts per year in this analysis, where here we see the solubility-limited models are much lower in release rates.

Then we looked at glass as a waste form, and used our glass dissolution or our coupled reactive model for calculating the concentrations at the waste-borne surface and the transport, and in this analysis, the alteration-limited nuclides have a much lower release rate due to the dissolution of the glass and such. And so we see that the alteration-limited nuclides are not mainly the dominant ones in all cases. Here we see that uranium is much higher in this analysis with the glass. That's due to a couple things, the dissolution and the kinetics of the waste of the glass, and also due to shared solubilities with Uranium-235 and 238, and there's more 234 in the glass analysis in the waste form than for the spent fuel. Thus, the shared solubility is going to be higher for the 234.

Just looking at the same analysis, normalized by the thousand-year inventory, the fraction of release rates in this analysis, we see that the release rates are much lower than earlier. In fact, none of them exceed 10^{-5} parts per year.

Looking at an analysis on Carbon-14, where we're looking at .05 mm/yr and the release of Carbon-14, where this early release of Carbon-14 is entirely due to the gaseous release. We assumed here that the containers failed between 300 and 2,000 years, and there's no water in the environment at that time due to the higher temperatures, and we assumed a uniform failure of the containers and the instant release of the Carbon-14, so we have a uniform release of the Carbon-14 in gaseous phase. And then the later release is when the water reenters the system and transports the Carbon-14 away.

And then just the same analysis again, normalized by the thousand-year inventory, and just looking at the magnitude of around 10^{-4}, due, again, to some assumptions about how we failed the container, how fast the release rate, and also, the alteration of the waste form producing--in this analysis, we used for the waterborne releases, we used an alteration rate of 10^{-3} parts per year, so then we assumed that the waste is going to be all dissolved after a thousand years, and this gives us high release rates. So that was basically the analysis that we did a year ago for the TSPA, the parallel analysis to Sandia's results and such.

And then we were asked to look, you know, at the new thermal scenarios and the new waste package and such, so what we did was a simple analysis here and to see what higher thermal loadings. I didn't do a--we didn't do a full thermal analysis on this. What we simply did, we'd just see what higher temperatures and see how they're going to affect release rates.

We know looking at our release models, that the release is going to affect the re-saturation times, time at which releases are going to be able to start due to the later times at which it's going to drop below the re-saturation time. In our analysis, we used temperature-dependent solubility, so that will affect our release rates, and we also have our glass dissolution model, which is temperature-dependent.

So what we did, we just simply increased the temperature profiles in our analysis. We looked at spent fuel, the same nuclides that we're looking at, the solubility-limited and the alteration rate-limited models, and then we also looked at the effect on glass with a groundwater-coupled reactive model to see how it would affect the release rates.

And this just simply shows a very simple analysis that I did. We just increased the thermal temperatures, and just to get a brief idea of how it's going to affect the release rates, instead of doing a full, detailed analysis.

Looking at some of our solubility-limited nuclides, we see here that our delay due to the re-saturation because of higher temperatures, we see this in all of our release profiles here, that it's going to be released at a later time. And we also see here, due to the effect of the solubility in our calculated--or temperature-dependent solubilities as we calculated with EQ 3/6, that the solubility is lower in the higher temperatures, because we estimated the solubilities, using EQ 3/6, and we got lower solubilities with the higher temperatures and thus, in this model, the release rates are lower.

With our alteration rate-limited nuclides, which are entirely limited by the dissolution of the waste form, temperature does not affect that analysis since we assume that it's a constant release into the water, and so it has no effect on the magnitudes. It does delay it, but no affect on the magnitudes.

Then with our glass dissolution or coupled reactive model, we see that there are different effects. For uranium, the concentrations at the waste form surface are the same with higher temperatures and lower temperatures. It didn't change it at all. For neptunium, it actually increased at higher temperatures the concentrations, and then for plutonium, it lowered the concentrations at the waste form surface for higher temperatures.

And for a couple other nuclides, we see that it actually increased the concentrations at the waste form surface, using our glass model. So that just briefly describes just a simple analysis that--just a look at how it would affect our release calculations.

The last slide is just some conclusions from the analysis that I just presented. In the TSPA source term analysis, we had some of the nuclides that did exceed the NRC regulatory criteria, same as what Sandia had. Several different assumptions could explain this; the dissolution rate of the waste form, because we saw that the alteration rate limited nuclides were the ones that exceeded the regulatory criteria, and we used a constant dissolution rate for all of the simulated waste packages and such.

What this does mean to our modeling is that we do need some more detailed modeling; in particular, the more coupled reactive transport models, similar to our glass dissolution model that we have in the AREST code. We want to develop a spent fuel dissolution model using the analysis by Walt Gray and Bob Einziger. We need to develop a model that will incorporate their work, similar to our glass modeling.

And then, just briefly, we looked at the effects of higher thermal loading, and we saw that in all of our analyses, it delayed the transport due to the re-saturation in the higher temperatures, but we also saw that in some cases it lowered release rates, some cases it had no effect, and in some cases it actually increased the release rates. And so this points to--there needs to be more modeling in this looking at the thermal effects and the new designs, which we haven't looked at, and a lot more analysis.

DR. DOMENICO: Thank you, Dave.

Are there any questions from the Board?

DR. GARISTO: Dave, how did you calculate the re-saturation time as a function of temperature?

MR. ENGEL: The saturation as a--

DR. GARISTO: Re-saturation time as a function of temperature.

MR. ENGEL: All we did was we simulate temperature profiles, and we just pick a temperature at which we assume that it's going to be below that temperature, it's going to start re-wetting. And then looking at our temperature profiles, we just pick that value off. I mean, we simulate temperature distribution for each waste package, and we just have a constant value for at this re-saturation temperature, and at that rate--at that temperature and below, there's going to be re-saturation.

DR. DOMENICO: I have a few questions, Dave. When you say coupled, do you mean that the advection may impede the kinetics like might be expressed by a DOM (phonetic) colon number? What do you mean by your coupled reactive model? What is coupled there?

MR. ENGEL: Well, okay, I just want to paraphrase. This isn't my expertise, but I can talk a little about it.

It was developed by Pete McGrail. He's a geochemist at PNL, and I know Bill Bourcier talked about it yesterday, but what we do is we couple the dissolution of the waste form with the groundwater, and in our model, we also look at the effects of a backfill region. We can couple in the backfill region, and also, iron content, if the iron corrosion of, say, the waste container is corroding away. In EQ 3/6 they have the capability of modeling at the waste form or the concentration.

DR. DOMENICO: Also, have you modified the AREST code to take into account temperature dependence? Is that what we were seeing here?

MR. ENGEL: Right.

DR. DOMENICO: What--just basically, quickly--what are the physics imbedded in that AREST code? I mean, it sounds like you have a robust source term model. It appears that way anyway. What's involved in there?

MR. ENGEL: Well, our models, our analytical models in the AREST code were developed mainly by Tom Pigford's group at University of California-Berkeley.

DR. DOMENICO: Okay.

MR. ENGEL: And for instance, most of the models that they developed were a constant concentration at the waste form surface.

DR. DOMENICO: Yeah. They're pretty heavy into diffusion control, also.

MR. ENGEL: Correct, but they do have models, later models for advection for the Yucca Mountain site, and we've discussed with them, and such that varying, say, concentrations at the waste form surface, and if that would affect the models and such, and it possibly could. And that's why we feel that maybe the analytical solutions that we have in the AREST code currently are limiting in that sense, because the models were developed for constant boundary conditions, and we've basically stretched that a little.

DR. DOMENICO: Any further questions? Don?

DR. LANGMUIR: Langmuir; Board.

I've got a more general question, Dave, which maybe someone else needs to answer, but I guess since you happen to be here, I'll start with you.

On your plots, you showed obviously different radionuclides coming off, depending on the scenarios you chose, and that piqued my question, my general question, which has to do with the relative release rates from glass, defense waste glass versus spent fuel.

Presumably, the glass will comprise 10 per cent or so of the total inventory in the repository, but something we haven't--I haven't learned about or haven't assimilated here is the relative rates of release of radionuclides from glass or fuel, and if the rates are much higher from fuel, its impact, if there's a breach, could be much greater than the 10 per cent.

So I guess if someone could address for me the relative rates of release--and that's going to affect the individual radionuclides that get out there as well, since they differ in the fuel from the glass. I guess that's my question for anybody who feels they could comment on that, I'd be interested; relative release rates and that impact on which radionuclides are the issue.

DR. BATES: John Bates. I made a presentation on glass yesterday.

In my presentation, I described that, you tell me the conditions, and I can tell you the release. Some of the release rates for glass are, as I showed in the final release, quite rapid. In addition, if the glass is aged and it contacts water, then that would also be quite a rapid--it'll be a pulse and it'll affect the radionuclide distribution and release, and it'll affect the subsequent reaction of the glass.

So depending upon what the scenario is, you have wide range of release for glass, and I suspect that in some of those cases it could be greater than it would be for spent fuel.

DR. LANGMUIR: Sounds like something someone ought to worry about.

DR. DOMENICO: Any further questions from Board members?

(No audible response.)

DR. DOMENICO: Staff questions? We have a few minutes. We can entertain a few questions.

MR. CURTIS: I have a question about--oh, Dave Curtis from Los Alamos.

I have a question about your slides, I guess, 22 when you show the relative release rates of cesium and technetium. I wondered why those are different. Is the assumption that those are both infinitely soluble? Shouldn't they have the same form?

MR. ENGEL: Which?

MR. CURTIS: Well, 22 is--there's a couple of them, but--

MR. ENGEL: These ones?

MR. CURTIS: Yeah.

MR. ENGEL: That's due to the retardation in the transport.

MR. CURTIS: Oh, so this has transport built into it?

MR. ENGEL: Right. And the cesium has a much higher retardation.

DR. DOMENICO: Transport where; within the barrier?

MR. ENGEL: Just the transport to the host rock, across either the rubble that--

DR. DOMENICO: Okay. Anything further from the audience?

(No audible response.)

DR. DOMENICO: Well, we're right on--

DR. REITER: I have a question. You indicated early on, and we know that PNL did individual dose estimates along with cumulative release. If the regulations are, indeed, changed from cumulative release to individual dose, can you give us any insight as to how one might look at the source term differently and how this might affect investigations?

MR. ENGEL: That's a good question, and I'm not an expert to answer it, but obviously, if we're just looking at mainly doses and we don't need to look at the NRC criteria as such, as long as we can meet the dose criteria with a source term, then maybe we don't need as detailed an analysis. But we need to look and make sure that the repository--there are releases from the source term, and we need to know what they are.

Anyone can jump in and help on that one.

(Laughter.)

DR. O'CONNELL: Bill O'Connell from Livermore.

When you take transport and host into account, then different radionuclides would have a different weighting factor than they have in either the NRC's release rate limit now, or in the EPA's cumulative release rate, where there are weighting factors. So perhaps some other radionuclides would rise to larger relative importance because of their environmental transport and uptake, but that would just change the mix of radionuclides which are at the top of the list. It wouldn't be a major change as far as the source term goes.

DR. DOMENICO: Well, we're right on schedule. Why don't we take that fifteen-minute break that we moved up, and then we'll hear from EPRI.

(Whereupon, a brief recess was taken.)

DR. DOMENICO: Our next presenter, Bob Shaw from EPRI, will give us some information on the source term used in their performance assessment.

Bob?

MR. SHAW: Thank you, Pat; appreciate the opportunity and the invitation from TRB to come and talk a bit about the work that we've done in performance assessment, focusing on the source term.

Our performance assessment model does calculate radionuclide releases to the accessible environment. It uses a probabilistic-based approach, using logic diagrams to calculate CCDF's, even though CCDF's may soon be outmoded if the President does, in fact, sign the National Energy Strategy Bill and we change to a dose-based environment. It uses individual experts to develop the nodes on the logic tree, and that's a very important and vital part of the analysis that we do, so we have individual experts, not group expert judgment at this stage, but each one going out and looking and assessing the current state of the technology and, through that, developing the logic tree node that's the responsibility of that particular individual.

Let me also state that the approach is meant to be a realistic, not a conservative kind of a calculation, but a realistic calculation of what we expect the release to the accessible environment would be under what we presently understand to be the best possible estimate of what will actually occur at Yucca Mountain.

It relies very heavily on more detailed models and analysis. It is meant to be a top overview kind of analysis, so we look at that for the controlling mechanisms and the parametric values. We do not attempt to include each and every aspect of what might happen under every possible scenario, but rather, to filter through and get the most important aspects to include in our model and, as such, we rely very heavily on the work of others that you've heard here today.

The overview of my presentation is it will be in two portions. The first portion will be to review with you the source term results from our previously published work. This is our most recent report issued in May of 1992, which was the Phase 2 demonstration of risk-based approach. Those of you who remember Phase 1, it was meant to be illustrative, show how the method would work. Phase 2 is, as I mentioned before, meant to present a realistic estimate of what is actually the release of these radioisotopes to the environment.

Subsequent, and actually, somewhat previous to this, we began some initiative that says how can we refine the results that we have, and a lot of the refinement that we have been doing over the last six months or so has focused on the source term, so I will review in the second portion of the presentation some recent refinement that we have in the source term analysis. The refinement will describe, actually, input because we do not have results yet from that refinement as it's taking place.

To take you from the end results of the first phase of this, the Phase 2 reports, the results are displayed in the following form. This is a CCDF, with the EPA's current limits expressed as the staircase on the upper right. You can see the various radioisotopes and the relative contributions that they make to the total CCDF. If you were actually to sum these together--I don't have a summed curve on here because it further complicates things, but because it's a long scale, the sum scale is not much different than the outside envelope that you would construct by going through the outside here. There are some additions, some places where they are fairly close, and there are some additions. Because it's a log scale, though, you don't see too much of that.

An important feature of this is that you do see an order of magnitude difference between the limits and the calculations that we come up with.

Considering the source term itself, and adhering as best I can to Mick Apted's rules about what the source terms are, this is meant to be the release of radionuclides from the near field, and as such, we have seen this has three aspects. The first is the waste package degradation; the second is the dissolution of the waste once that degradation has taken place; and the third is the transport of the released radionuclides. That transport, in this case, is meant to be over a very limited basis, but it's the transport that takes the radioisotopes from the spent fuel to the point where the hydrology is considered far field, so that's what we consider the transport processes to be taking place here.

Reviewing the logic tree that we have that makes up the core of our calculational procedure, it is one which is an ordered calculation, starting with infiltration, a change in water table from that infiltration, earthquake occurrence, the change in water table from earthquakes, volcanic dikes, the repository temperature, borehole fractures, the engineered barrier system, solubility and dissolution, diversion of infiltration, fracture and matrix coupling, matrix sorption, saturated flow velocities, and finally, human intrusion.

The ones that I've circled in blue is the particular area that I consider source term. It is somewhat, but rather mildly affected by the ones that are in front of it, and I'm not going to consider the influence that they have on it. In addition, the borehole fractures, although an important part of this, will not be a prime consideration. So I'll be talking about repository temperature, EBS, and solubility and dissolution as the primary ingredients of the source term here.

So starting off with waste package degradation, as many of you are aware, we use Weibull statistical distributions to describe the degradation and the loss of integrity of the various features. We do include both container and cladding failures. Our experience in the utility industry these days is that zircaloy, after it comes out of the rather aggressive environment within the reactor, is found to be on the order of one/hundredth of one per cent of the fuel rods that actually failed. That's a remarkable figure, I think, under those particular considerations, and it's quite a statement to the fact that fuel vendors have done a lot to understand what causes those kinds of failures.

So we do include the zircaloy in addition to the outer container, two different Weibull distributions, and in each case we will define--we do define threshold failure times, mean lifetimes, and failure rate at mean lifetimes. In other words, we use the three-parameter, Weibull-type distribution.

Some have asked why we use Weibull; because it seems to express in the best fashion that we can see the statistical distribution of failures that come from manufacturing-type of processes.

In order to determine in some reasonable fashion the values for these three lifetimes, we use the degradation modes and failure mechanisms. Now, that means the environment, the materials, the closure, and the thermo-mechanical history. Here, in particular, this makes reference to the container. The environment, of course, is the chemical environment that one will experience at the outer surface of these containers, the particular material that has been chosen for that closure because, in many cases, we expected that this will be a welded system and, as a result of that, heat-affected zones may be the primary area in which you do eventually get failure; and then the thermo-mechanical history, meaning the manufacturing history of the particular system.

We use these, then, to deterministically come to the Weibull parameters that are used in our particular distributions. In a similar fashion for the cladding, as you heard yesterday, the primary sources of loss of integrity will undoubtedly be creep rupture and hydride reorientation, so we use those again to estimate what we think are reasonable failure times, mean lifetimes, and failure rate at the mean lifetime.

Considering the temperature profiles that one might anticipate within the system, we have selected three. The particular order may be awkward, not going alpha, beta, gamma, but three temperature profiles have been considered.

One, of course, is the hot profile that says we get significant temperatures increase and, in this case, over a period of about a thousand years, in which the temperature of the system is above 100☜ C; a second profile, gamma, in which we consider that heat pipes and other effects take place that allow the system to go to 100, but not above 100☜, so that the boiling point is retained; and thirdly, a cold system, where decay has taken place because we've stored the fuel for a longer time or spread it out within our repository, meaning that we have a temperature pattern that is always below 100☜ C.

When we look at the entire repository, however, we come to the conclusion that there will be a fraction of the fuel that will have different curves representing that. For example, if we start out with what we term the hot repository, and we consider there's a 60 per cent probability at this stage that we will end up with a hot repository. That, in our estimation, would have 90 per cent of the containers represented by Curve A, and 10 per cent represented by the curve gamma. So we will have 90 per cent of those being hot, above 100☜ C, and 10 per cent being represented as going to the boiling point, but not above.

And then, similarly, you can see the 30 per cent for the warm, and 10 per cent for the cold. In the cold system, we consider that everything, all the containers would be following a pattern described by beta. That's the curve that's always below 100☜ C.

In addition, when we look at the engineered barrier system and decide what it's going to look like, we first of all decided there could be three different types of containers. Type 1 we determined would be 304L stainless steel; secondly is an alloy 825; and the third is a multi-barrier, and you can see that what we've decided is that depending on whether you have a hot, warm, or cold repository, the design decisions would be altered, depending on which of these you were going to have.

So that we expect that if you have a hot repository, there's a 70 per cent chance that Alloy 825 will be chosen, 10 per cent it would be multi-barrier, and so on and so forth. So this means that when you begin to talk about thermal effects, it's wrapped up, in our model at least, with a design decision as well. So it's not completely independent in that sense, but we think that does represent the process that would take place in the design decisions.

When we look at the sensitivity of our results to the selection of the engineered barrier system, it looks somewhat like this. Let me go back just a second here and describe to you what I mean by what we do here for sensitivities.

When we make our calculation here, every one of these scenarios is included in our calculation, and they're weighted according to the probability, and of course, there're probabilities associated with each of these as well, as I showed you in the previous diagram. So when you make a calculation, you include all of these.

When we want to look at the sensitivity to a particular parameter, we then choose that parameter and change the probabilities; for example, looking at an engineered barrier system, and we want to know, what is the sensitivity to this selection process. We will, in turn, say 100 per cent, or 1.0 is stainless steel in all these cases. So we put zero to the others, et cetera, and we go through that process for each of the three, and that's the way that we investigate the sensitivity of the selection of the engineered barrier system to our particular calculation of the CCDF.

And you can see the results here, which show that the integrated case, which appropriately weights all of these, is very similar to the Alloy 825. Stainless steel is slightly less effective, giving a slightly higher concentration of radioisotopes, and the multi-barrier system gives you a significant additional protection, on the order of one and a half orders of magnitude better as you go through the multi-barrier system.

When we look at the influence or sensitivity regarding the choice of whether it is a hot, a cool, or a warm repository, we see less sensitivity. We do see some benefits to going to cool, but you must again remember that wrapped up in this choice is the decision that's made, the design decision with regard to the material.

Considering, then, the next step, which says once you've lost the integrity of the container and the zircaloy, what happens, then, to the radioisotopes? So the dissolution and the transport, then, first of all encompasses the release modes, and the first is, of course, dry, in which case we have no release pathway other than the gaseous transport. Second is the wet drip. The container fills to the penetration. We do not consider that there is a hole in the top and also a hole in the bottom. We consider that somewhere on the system there's a hole, and the water goes in and it fills until it gets that hole, and then release takes place of those radioisotopes that are contained within that water. And then, third, is the moist, where you have a diffusive and advective pathways as a result of moisture carrying over the surface and holes being through the container.

We have also have chemical constraints on release that are similar to those that have already been discussed here. First is dissolution, which, of course, will be affected by the alteration rate for certain isotopes, or the reaction rates for certain other isotopes, or it may be the solubility limit that limits the transport of the radioisotopes themselves. So we do that isotope-by-isotope to determine which is the appropriate constraining factor there.

The picture that we use does take into account that the various radioisotopes are contained in different locations, and starting at the outside with the cladding, you do have Carbon-14 on the cladding. Actually, further outside than that you have a crud layer which has Carbon-14 within it. Then there is a gap that contains this inventory of radioisotopes, the grain boundaries which we consider to have this inventory of isotopes, and then the UO_2 matrix itself, with the actinides and about 98 per cent of the fission products. And so we do relegate the inventory of radioisotopes into these various categories, and on the next table it gives you in a little more detail, in numerical detail, just where we consider these various radioisotopes to be.

And you can see that you have the outer surface layer and the gap that has about 2 per cent of the Carbon-14, and the other 98 is contained within materials which is much more slow to release. Most of the other radioisotopes are contained within the UO_2 matrix, with some small amount or small fraction being contained in the gap and the grain boundary. So this is our current model for, numerically, the distribution of those radioisotopes.

The other feature that's important here is the one of solubility, and one of the questions we looked as we went to a temperature-dependent system was, what is the effect of temperature on solubility? But to start off there, we looked at what's the range of solubilities for these various radioisotopes, and if you look a little closely at these, these ranges are atrocious.

To take an example, neptunium goes from 10^{-4} to 10^{+2}, Americium, 10^{-7} to 10^{-1}, and you wonder how anyone can possibly come up with a range that's quite so widespread. The manner in which we did this was to go back and look at the data that's available on these various radioisotopes.

The first crucial question, of course, is, what is the chemistry that they are experiencing? What is the pH, et cetera? The other, what is the temperature, pH, and oxygen concentrations? Well, those are very big unknowns, and as we look at the range of chemistries than can be present in the aqueous conditions and we look at the solubilities, that leads us to these kinds of ranges. So this is a result of the uncertainty of the chemistry that leads us to such a wide range in the solubility estimates that we have for some of these radioisotopes.

As a result of that, we say, putting temperature effects in here is nonsense. The temperature effects are significantly less than the ranges that you see here, so we don't bother with them at this stage. We feel that's not a sensitive factor, considering the unknown of the chemical condition itself that would exist at the point of dissolution.

I should make one other point here. What we've called the moderate is meant to be our best estimate of what we think would be in there. It's in no way some statistical difference between these two. It's meant to be our expert's best judgment of what we think the chemistry and the resulting solubility most likely will be in that particular environment, and then we use each of these three cases in our logic diagram when we consider solubility, and we do consider that there's a 90 per cent chance that the moderate solubilities on that table will be appropriate, and 5 per cent chance that the high and the low will be the actual solubilities that will exist at the time that the dissolution takes place.

When you look at the sensitivity of the results to the solubility, you find that it's a bit more dramatic than the other sensitivities, and as a matter of fact, I believe this is the largest sensitivity we have of all of those that we investigated as we went through our nodal analysis. So you can see that in going from the integrated--of course, the moderate and the integrated are about the same since this is 90 per cent of the integrated case, but you can if you go to very low, it's a very significant decline, and if you go to high, you get to some points where it's pretty close to the limit that the EPA has established.

Now I want to go into the second portion of this presentation, which talks a little bit about the refinements that we have been undergoing recently. We had some meetings with Lawrence-Berkeley Lab, U.C.-Berkeley, Lawrence Livermore Nuclear Lab, and Sandia National Lab in the early parts of last summer to discuss with them what are the particular items in our performance assessment that would benefit from some upgrading, and four items are listed here that were the prominent outputs of that.

Thermal loadings and the waste containers. There are some changes. We had these in there before, but there are some changes that I'll show you in just a moment. These, of course, are design parameters, and it's important that as we think through some of these models, we recognize that things like solubility are uncertainties in the physics, but designs are just as much uncertainties, but they're, of course, of a very different nature. Three heat-transfer mechanisms: conduction, convection, and heat pipe, and four time-temperature curves.

And just to further confuse those of you who know Greek, we have placed now another curve out of order with regard to the Greek alphabet, but what you see is the alpha, gamma, and the beta are the same curves as I've put up before, but we now have a delta, which says we have high temperature for a very much longer period of time; that over a period of 10,000 years, we are keeping the waste canisters above the boiling point. So that's the addition to our time-temperature curves, which gives us the refinement.

Now, the manner in which we have handled the variety of aspects we were considering here is illustrated in this particular chart. To start off with, this is the fractions of the repository in different environments for the average power distribution of 57 kW/acre. This is illustrative in the sense that there are others which have different heat loadings, so this is for the intermediate heat loading.

Taking that, then we can say, well, we think there's a 50 per cent chance in this case that we'll have conduction-dominated heat transfer; 20 per cent that we'd have high permeability; and 30 per cent that water would be mobile in the fractures. And then you can see that for each of these cases, we have said, "what's the probability that you would have dry?" and "if you have dry, what are the temperature curves that would represent what fraction of the packages, of the waste containers that are there?"

So this is the whole matrix, then, adding up to one, that says what fraction of the waste packages are likely to be at that particular, either temperature or water, condition for each of these cases. And so this is the manner in which we have developed the probability matrix that's associated with each of these conditions as you pass on through that.

Now, we're at the stage now of having completed the collection of relevant data from the experts that can be used in our performance assessment. We have not changed our performance assessment code yet, and as a result, we don't have any results to display what's the effect of these refinements, but I wanted you to be aware of the refinements that have taken place.

Now, pulling all this together, I want to talk about what I think are some of the key issues that we really need to deal with, and the first and very highest on my list is the EPA High-Level Waste Criteria, the fact that it has very, very strong regulatory and licensing implications that are associated with the legislation which is yet to be signed by the President, if I'm up to date. It has a lot of implications, and I think it's very important for those of us who are involved in the process to be involved in the process.

If the National Academy is given the responsibility to carry out some dose-based recommendations to the EPA, I think it will be vitally important for us to be supplying appropriate technical input that says what implications does this have.

One of the steps we will be proceeding, then, on is to take this release of radioisotopes to the accessible environment and say, how do you now translate that into some dose calculations?

The second key issue here is the engineered barrier system design; borehole or drift emplacement. I think almost all of us continue to do modeling based on borehole. We will be changing that in the next few months to do drift emplacement kinds of calculations, since they seem to be the implied, although not the official design that's currently within DOE.

The questions of thermal load continue to be vital ones that everyone is considering. I'm not as convinced that it has a very strong implication with regard to performance assessment, but it does have a fairly strong implication with regard to unknowns.

The selection of a particular waste container--and I've put here with question marks, UCS, representing the universal container system--that, I think, has very important implications for how the waste container might be put together. The universal container, for those of you who are not aware, is something that has been pushed rather strongly by the utilities as a thin-walled container that might contain up to 24 PWR fuel elements, which could be sealed by welding at the plant location at the time it is loaded, never to be unloaded again. So it would give you a one shot, let's put the stuff in and then let's keep it there forever. It has some strong economic and radiological dose implications with regard to operations that are very positive, as well as developing some sort of a standard.

It is recognized by the utilities there is not a single universal container that would suffice for this, because of various differences in sometimes size, but most often crane capacities. It's clear that not just one universal container system would suffice for all utilities. Nonetheless, the idea of standardization is important, and we've had some very significant early discussions with DOE which seem to be going very well regarding some standardization.

If one put the universal container system in there, that gives an additional barrier. One concept could be that what, of course, you have on the inside is the zircaloy barrier. Now you have a thin-walled universal container system, and presumably, you'd have an overpack which, for the example of a corrosion-resistant or a corrosion-barrier type one, maybe of ductile cast iron, you'd now have a heavy material on the outside. So it gives you actually a triple layer of protection involved at the waste containers.

The concept of backfill materials is one that we have not included yet in our concepts. That design aspect is one that is being considered by the M&O, and it is one more modification that we will want to consider in our performance assessment.

Moving down one layer, some key source term issues, as I see them: The first and obvious one at the top of my list is solubility data. Actually, in many cases, the data is there, but the particular conditions under which it would take place, the solubilization would take place, is not very well known. That means that the water conditions that could exist at the time that solubilization takes place is probably a very important feature.

The spent fuel inventories, especially Carbon-14, continue to nag us, and I think when we ask detailed questions about especially Carbon-14 and how we know where it is, we find there's extremely limited data, and sometimes even extrapolations that have taken place in order to get us to the point where we seem to consider that 2 per cent is on the outside of the fuel, et cetera, et cetera. I'm not very confident that that particular set of data is a remarkably good one.

I had alteration rates on this list. I think we heard some significant input of data yesterday regarding both alteration rates and solubilization. They also give me some pause, and a lot of the pause has to do with the effective way in which we integrate these results. On the one hand, I heard Bob Einziger talk about the alteration rates that take place in spent fuel and how important it is to look at actual spent fuel, not just uranium dioxide. And then we heard Walter Gray talk about the release that comes from his solubilization tests.

Now I, as a modelist, have a lot of difficulty taking the weight per unit area per unit time and somehow--for solubilization--and somehow translating that into a solubility term, because it was clear from yesterday's presentation that the unit area is how much grain boundaries there are, and yet, from the previous presentation, the grain boundaries are a function of time, and I don't know how to go over the whole repository and make some estimate as to how much grain boundary I have there, and whether it's a function of burnup, thermal history, et cetera.

So translating that basic data--which I consider to be valuable--into a meaningful source term for these overview models is still very much a challenge. It's not clear in my head how those steps are going to take place.

Retardation is certainly a very significant question here, and when people talk about colloidal aspects, the only difference I see is that you have a significant change in retardation if you have colloids. And so it's a simple change in our model to account for colloids. Just tell me which isotope you want to make as a colloid, and I'll change the retardation of it and that, in effect, takes care of it. Of course, the difficulty is identifying which material could be in colloidal, and that goes back to the first question here, which is not solubility data, but which is water chemistry, since the water chemistry will control the extent to which we have colloids that might be formed and that, of course, is the last item here, which is not meant to be last in importance.

There are a number of site uncertainties. Infiltration, what is it at the repository elevation? We have various estimates, but we do not find in our model that it's an extremely sensitive area. The degree of fracturing, spacing, sizes, and I think we'll probably hear more about that as the Ghost Dance Fault gets described, although not maybe in the detail that I'm talking about, but also the degree of fracturing is a function of temperature, and if we change the repository temperature here, that can influence the time dependency of spacing and sizes of fractures.

The coupling between the fracture and the matrix flow is a very vital part of the site uncertainties; permeability of fractures and the extent to which they're connected; lateral flow distribution, the extent to which we have impermeable layers, which seems to be one of the positions that's being taken recently, that there are some significant impermeable layers which will influence significantly the amount of water that actually gets to the site elevation.

Continuing on performance assessment modeling issues, how can performance assessment models be validated? We can look at subsections with some models, and we can look at integrated systems. And to a very great extent, the best technique we have right now for validating these overview models is simply to compare one with another and see if there are inconsistencies. But I think that there would need to be stronger techniques that we develop for model validation as we proceed towards the licensing application.

Bounding or conservative calculations versus realistic or best measure calculations, I've already stated my preference, which is the latter, but we certainly do have bounding or conservative calculations. I'm always troubled by the fact that sometimes these bounding or conservative calculations can almost, in a multiplicative effect, have an undesired effect on the results.

There needs to be self-consistency. We need to continually go back and look and make sure that we haven't violated any of the basic heat, mass, and water transport balances that need to be present in the system, and that they need to be appropriately coupled. It's very easy to couple things together and lose sense of, in a chemical engineering sense, the fact that there has to be a balance on each of these features.

Cell versus homogeneous modeling is a question at this stage. We are moving more and more, I think, towards homogeneous modeling.

The use of expert judgment is, in my mind, absolutely vital, and as a result of that, we have to explore the various ways in which expert judgment can be used effectively and defensively, so that we can use it effectively in getting the right results, but also use it in a defensible fashion so that when you come to licensing, you can show that expert judgment was used in an appropriate fashion for that particular purpose.

The issue of how you go from detailed models to overview performance assessment models is continually an important consideration. We've talked about transfer functions or, in some cases, just data tables that enable you to take detailed calculations, compress them down, and make use of those then in the overview performance assessment model. The level of details in the overview models is one that we try and restrict as much as we can. The more detail, the less opportunity you have to really look at these sensitivities. So I'm in favor of less detail at this level.

Incorporating probabilistic and deterministic aspects is an important consideration, and these things have cost and schedule implications. Further, they can be used as a tool to look at cost and schedule implications that have to do with various sensitivities, and that's another area that we will be exploring in the near future.

So that completes the presentation I wanted to give to you, and I'm open for any questions.

DR. DOMENICO: Thank you very much, Bob.

Any questions from the Board or the consultants?

DR. GARISTO: Nava Garisto.

I wonder what your opinion is about the current limitations of analytical models in these kind of calculations now that computer resources become more available and much cheaper. Do you think that numerical calculations will provide more flexibility, especially if the design of the repository keeps changing?

MR. SHAW: I see numerical calculations as simply a way of doing the calculations that our theories say will be the important mechanisms and processes. I don't presently see that we're going to be moving, especially in overview performance assessments, to much in the way of numerical calculations. In the more detailed calculations, I can see that that can be very useful.

DR. DOMENICO: Any other questions? Yes, John.

DR. CANTLON: Bob, your comment about looking at the universal cask concept and having the utilities seal it at the point of entry, in a sense, that would preclude looking more seriously at some of the buffer-filler options that might cut down oxidation on cladding and speeding up of solubility of the waste pellets. Has any of that gotten into your model thinking?

MR. SHAW: No, it hasn't, and what you say is true. It would mean that the filler, buffer material that would go inside such a container, that you don't have the option to do that. It does give you the option to put that between the overpack and the universal container, as well as on the outside of the overpack, but it would limit that option, that is true, I agree, and we haven't looked at that. Matter of fact, we don't even have in our model at this stage the effect of any filler material. I noted backfill, but filler material also is another aspect of that, too.

DR. CANTLON: Well, it would seem that looking at a buffer-filler in the rods and in the assemblies would stabilize the system and slow up, if that were a major oxygen cell, could behave very much the same way that you have a bentonite sump for water, preventing it getting into the pack.

MR. SHAW: I agree. I could see, for example, iron filings being used as an oxygen absorber, yes.

DR. DOMENICO: Any questions from the staff?

DR. NORTH: I'd like to follow on with a question that gets into the issue of engineered barrier design and repository design. One of the things that I have learned from the meeting today and yesterday was the potential importance of what I'll call dependencies or inhomogeneities. That, for example, it may be that a fuel element, that is on the tail of the distribution in terms of some kinds of radioisotopes, may be more likely to fail through a mechanism such as gas pressure. Or another example is, we may have inhomogeneities with respect to the amount of flow in the wet drip mode and, as a result, I think we can see a number of scenarios for why failures in some areas of the repository or some canisters might be a good deal more likely than others.

Some of these we might be able to identify in advance, and some of these we probably can't. They will remain uncertain. But getting at this variability issue, set of variabilities, versus uncertainty, those things we can't learn about, certainly will be important in the modeling and to the extent that we can identify where failure is more likely, we might want to take that into account in terms of the design decisions.

As one example, if there are known ruptured fuel rods, it might be that in a situation where that is known to be the case, there, for sure, you want to use a filler material and you might decide for economic reasons that where there is no indication of damage to the fuel rod, you choose not to do that.

So this seems to me to open up another class of sensitivity questions, and another area in which one might want to look very carefully at the aggregate performance assessment models, as opposed to the detailed models of mechanism, and I wonder to what extent you've thought about that or would like to comment further.

MR. SHAW: Well, to go back to your original premise, that there can be special circumstances where you get increased possibility of rupture, first of all, there are inspections, of course, that are carried out at the plant sites to determine the extent of spent fuel degradation and, in particular, to look at where there have been fuel failures. And in most cases, either those assemblies--or in some cases, those rods--are removed, and we've gone from the point of just sampling an assembly in order to identify whether there's any radioisotopes being released, to the point of actually identifying which rod that is being released from, and that technique that's used is to identify where water has actually been absorbed inside the rod, and using that as the technique to identify the rods.

So there will be cases, certainly, now, where rods are being removed from fuel assemblies where they are able to disassemble such fuel assemblies, and you will have a collection of rods, or maybe an assembly that has a collection of rods that have failed. So there is reason to consider that that might occur.

Secondly, the question of internal pressure and maybe an excess of radioisotopes that would be present as a result of operation would certainly have to do with burnup, and the more burnup you have, the more you have both more heat generation and more fission product inventory. And certainly, all of the utilities have the capability of calculating burnup on a rod-by-rod basis, and so therefore, that information should be available once one knows what's in a particular canister.

Now, it might be possible to then begin thinking about how would one use a filler material in maybe selected kinds of assemblies, but it's also important to remember that if you have 24 PWR bundles, roughly 250 rods per bundle in a particular canister, we're talking on the order of 6,000 rods that will be in a particular assembly, and even with .01 per cent, you're talking about at least a handful of rods that will have failed within a particular collection like that.

So one could look for anomalies where there were significantly more failures than that, or situations where the burnup was decidedly higher in a particular container than others, and that might give you reason to say, well, let's consider how we might design that system so that is more effectively buffered with the loss of integrity.

I can see that, you know, process that we might well follow along on.

DR. DOMENICO: Any further Board questions? Staff? Bill?

DR. BARNARD: Bill Barnard, Board Staff.

Bob, one of your recent refinements that you discussed was a new scenario, temperature scenario delta, which involves high temperatures for over--above boiling temperatures for greater than a thousand years, or up to a thousand--10,000, I'm sorry.

In a subsequent slide, you stated that you didn't think thermal load would have much effect on the CCDF. Now, was that based on the runs that you did for alpha, beta, and gamma?

MR. SHAW: Yes, it is.

DR. BARNARD: Do you have any feeling for what sort of results will come out of the delta?

MR. SHAW: I'd prefer not to try and pre-judge that. The calculations get fairly complicated at times, and I think it's pretty difficult to estimate what's going to happen as a result of that, so I'd like to await that for the next presentation when we're ready to discuss those results.

DR. BARNARD: But you still don't think that thermal load has much effect probably relative to solubilities, is that it?

MR. SHAW: Well, first of all, of course, my judgment comes from CCDF's, and it precludes--it may be precluded, you know, based on the fact that we may be looking at dose, and if dose turns out to be a very important feature, it may be that keeping container integrity for a longer period of time becomes a much more vital issue.

I think the whole scenario is in a transitional stage right now, and we're going to have a different set of limits that we have to look at, and so some of those--all those conclusions are based on CCDF's and the EPA limit, and so I think we can't throw out any of these things right now on the surmise that we're going to have a whole different set of standards to compare with.

DR. DOMENICO: Anything further from staff? We have time, possibly, for one question from the audience.

MR. CURTIS: Dave Curtis from Los Alamos.

I only bring this up because you did, but the concept of validation is really important, I think, and I don't mean validation in the sense that your computer code works the way you think it works, I mean validation in the sense that you're going to have to convince somebody that this has some connection with reality.

Could you comment on how you think that case might effectively be made?

MR. SHAW: What I can comment on is that we've been doing a lot of thinking about how you do that. How do you take the physical processes, which is what you're emphasizing, and show that the computer model that you have actually verifies the physical process?

I think it's much easier to do on a sub-model basis, where you look at, for example, the hydrology, and you try and say: Is the hydrology model that we've developed for unsaturated environment, does it have anything to do with what's going on at Yucca Mountain?

And I think as we get into Yucca Mountain and get more data, as we do large block tests and some other features of that nature, we begin to get to the point where we can validate a little better whether our models are approaching the reality, at least for those sub-groups of tests. But I agree with your first point, I think it's a very difficult and challenging process for us to be able to validate this, because there is inevitably very limited data over very long times of predictions, and it is a challenge. I don't have any nice answers for it.

MR. CURTIS: And the reason I bring this up, it seems to me that's a subject which seems to get lost in most of these presentations. We feel comfortable dealing with the technical issues, but the "validation" issues just seem to get lost and I'm not sure whether they just get lost in these meetings, or if they're getting lost in the whole process.

MR. SHAW: Well, I think partly it got lost--I would hesitate to use the word "lost." That's why I keep it up there. But I think for the last year or two we've been at the stages of putting together the first performance assessments and coming up with these results, and then looking at these results and saying, do they make any sense?

And when we begin to see different results--I'm sorry--different assessments, models that have different bases or at least different calculational techniques, albeit that they go back to very much the same physical processes and estimates, when we come out with those and say they're not too much different, it gives us a first level of confidence that now says, what's the next step to go to in terms of validation?

MR. CURTIS: I don't agree with that because basically these things--

DR. DOMENICO: Gentlemen, we must go forward here. We have the questions, but let's not have a debate on the floor, please.

Thank you much, Bob.

The fourth presentation on source term comes from the Nuclear Regulatory Commission. My agenda says Dick Codell and Tae Ahn. I don't know if we're going to have one or two presenters here, but I guess we'll find out; right, Dick?

DR. CODELL: I'm Dick Codell. In the interest of time, I'll be making the presentation, but Tae Ahn is sitting at the table here. John Walton is in the audience, and Ginny Colten-Bradley, who is another person here from NRC. I may be deflecting some of the questions because I'm not sure I'll be doing justice to the entire source term matter.

Let me state first that I appreciate being last on the program because it gives me a chance to make some editorial comments on the other presenters.

(Laughter.)

DR. CODELL: Let me say a little bit about NRC's role. First of all, we can't hope to duplicate the vast talent that DOE has assembled for this project, but we hope we can keep them honest. Also, it gives us an opportunity to participate in this very interesting project of coming up with source term models, because I think we'll have to have these skills honed in order to do a good job as regulators.

In my presentation today, I'll be going through the following: First, the temperature model, looking at the temperature in the near field of the source term engineered barrier; waste package failure model; liquid radionuclide release; a Carbon-14 gaseous model which is different from most of the others I've seen; some discussion of kinetic effects, which I feel have been not treated properly in most of the models; and a short discussion of disruptive releases from the source term. Finally, I'll finish up with support work going on at NRC and at the Center for Nuclear Waste Regulatory Analyses supporting the source term.

The canister temperature model is simply a semi-analytical model that treats only conduction in a uniform medium, but it looks at each of the individual--some 30,000 canisters--in the background of the geothermal gradient and the impact of the earth's surface, and heat from the other waste panels. The heat load can also vary with time and space, so you can take into account different fuel loadings and sequence of loadings. At the iterative performance assessment, Phase 2, which we're currently involved in, it's mainly used for looking at canister failure, and also in the Carbon-14 model, which is temperature dependent, but not in the other releases.

The canisters in our representation of Yucca Mountain are arranged in about 30,000 spent fuel canisters-- we're only looking at fuel at the present time--in 17 waste panels arranged in drifts in the mountain, and each one of these can have a different heat loading, and does in our model.

Now, in our performance assessment, we're somewhat limited to how fine a resolution we can have in terms of the flow and transport model, so actually, it's arranged into seven sub-areas, A, B, C, D, E, F and G, which are whole numbers of panels, and these, in turn, go as the source term inputs to the transport models in our overall performance assessment. So these are seven sub-areas or cells, as we call them.

Now, we go through with the temperature calculations and come up with curves that are similar to these, which show at different points in time--each line being a different time, 500 through 3,000 years--the number of canisters whose temperature is above or below this temperature of 96☜, which we take as a representative boiling point. And when a canister falls below that temperature, we assume that it instantaneously is wetted, not taking into account the time that it probably would take to re-wet the canister after it starts to cool down, and at this point we start invoking the canister corrosion models for looking at corrosion failures.

Now, that temperature I had up there was the temperature of the skin of the canisters. The temperature inside the canisters, which we need for the Carbon-14 source term model, is an empirical correlation of measured fuel temperatures versus the skin temperature and time, and this temperature is added to the skin temperature to get the fuel temperature.

The canister corrosion models. We consider general corrosion, crevice corrosion, and pitting corrosion. John Walton may have to answer some of these questions if I don't make the presentation right, but the model considers the corrosion potential similar to this curve for the crevice and pitting corrosion; that is, with time, the corrosion potential--which is this heavy line--increases up to a point until it passes a point at which pit initiation begins, and from that point on you would have pit corrosion or crevice corrosion until it drops below a repassivation potential, and then corrosion in the pit stops, but you still have general corrosion of the background material. The models in the source term model act this way, but they are empirical. They're not too mechanistic.

There are other modes of canister corrosion--I'm sorry--canister failure considered in the source term model. There is buckling, where we're looking at the SCP design with 304L stainless. It's a long cylinder approximation for buckling, and the thickness is allowed to decrease generally by corrosion. We don't consider an air gap, nor is there stiffening.

Furthermore, in addition to buckling failures, we consider initial defective waste canisters which are presumed to fail shortly after they're emplaced, for no good reason; and finally, there is disruptive scenarios, where we look at seismic failure, also coupled with the buckling model; volcanism, where we're looking at intrusive or extrusive volcanism of the dikes or cones through the middle of the repository; and finally, human intrusion through drilling.

Once the canister is assumed to fail, we start considering release rate from a canister, and the source term model, SOTEC--which it's called--we look at dissolved and colloidal releases by advection; that is, flowing water goes through the canister; diffusion through the rock; and we also, in considering the releases of dissolved or waterborne radionuclides, have some consideration of kinetic effects as in the disintegration of the uranium matrix.

We also have a Carbon-14 gaseous release model, which looks at the inventories in the metal, cladding, grain and gap, and the fuel itself, and I'll be spending some time on that.

Our dissolved model borrows heavily from former DOE-sponsored efforts on source term. We consider a vertical canister in this cartoon, but where we have a fuel element--fuel rod which is considered to have no protection from cladding. We do not consider that in Phase 2. The water will flow in through a hole in the top by dripping, will either fill up the canister to a certain level and then flow out again, or run down the side of the fuel rods, dissolving the fuel as it goes.

By varying the volume of the canister that we're allowed to fill up here, and the fraction of the fuel that can be wetted by the dripping, and the flow rate, we can get various combinations that DOE's explored in the past for dissolution. We also consider that if the radionuclide is solubility-limited, that it may first be released from the disintegrating fuel and then go into a released, but undissolved inventory, which would be released, perhaps, later as the source term from the fuel itself diminishes and if the water continues to flow through and out of the canister. That's the advective part.

I might point out here that the amount of water entering the canister is a great unknown to everyone in this room, I'm sure. How we're treating it is not very satisfactory, either, but we're getting it from a coupled unsaturated flow model, where we're looking at separate flows through fractures and matrix coupled together, and the amount of water that flows through the fractures, we're taking a portion of that and diverting it into the canisters. So that's where our flow rate through the canisters comes from.

Now, to the advective model, we add the potential diffusion, and this is our liquid diffusion model. We now consider the canister as a sphere, for mathematical simplicity, and we assume that at the surface of the canister, the concentration is set by the concentration in the total canister. This differs from some other DOE approaches where they looked at this concentration as a fixed concentration, also for mathematical simplicity, as being set at some sort of a solubility limit. This is an improvement in that it allows the concentration to vary with time, but this had to be solved with a numerical model rather than an analytical model.

We look at diffusion through three rock zones. First is the air gap, which we assume to be partially filled with tuff rubble, and then there's a damaged rock zone, and then the intact rock zone. The boundary condition set on this is that at some arbitrary distance, say, ten meters, concentration is zero. The concentration gradient then drops between the canister concentration and zero, which we feel is a conservative assumption, of which there are many in this model. And that flux that comes from the diffusion model is simply added to the advection model.

Now, this is a point where I want to start editorializing a little. We're taking into account releases of plutonium and americium and other actinides, probably much more so than the other participants here today, although I'm glad to see that Bob Shaw did recognize wide variations in these concentration solubility limits that I think capture some of our concerns.

We feel that some of the other contributors have, in the past, given short shrift to the potential of plutonium and americium. If you look at the dose potential as some sort of a measure based on what EPA allows you to be released, the quantity present, the half life, these two elements add up to over 99 per cent of the potential dose, and so we're reluctant to ignore them.

Even though in some ways it's possible to calculate very low solubilities for these things, and also measure experimentally very low solubilities, there are other factors that we ought to maybe address. One of these is the kinetic effects, where the way in which these elements are released from the uranium fuel would cause various complexes or colloids, other strange things to form that could be much greater than we're allowing in some of the modeling studies.

Now, we don't, at this Phase 2 study, have any sophisticated modeling built into our performance assessment, but we're taking a wide range of plutonium solubilities to represent these great unknowns, and between 10^{-5} and 10^{-9} M, which I think is appreciably larger than others have used.

We've done some speciation calculations at 25☜

that indicate that this is a reasonable range, but haven't been so successful at 85☜. Ginny Colten-Bradley may be able to answer some more on that.

Now, getting further on, getting further over my head, I should say, in this topic, I'd like to talk about Tae Ahn's subject, looking at what possibly could lead to fast releases. First of all, we suspect if there were fast releases, they could be colloids, either real colloids or pseudo colloids; that is, the radio elements attached to naturally-occurring silicate or other colloids in the water, which have been observed in Yucca Mountain water.

One of the mechanisms for the growth of colloids is supersaturation of, say, the plutonium as the uranium oxidizes and releases. It releases at a rate much faster than the water could dissolve these other things, and so they would be supersaturated and you could start growing colloids from this supersaturation.

Furthermore, you could get speciation into various states of the plutonium with different charges, some of which may be organic species that would be formed from naturally-occurring chemicals or even organics that ended up in the water from human means.

The surface area of the spent fuel is an important factor in these. As the fuel dissolves, the surface area is likely to increase, both from the dissolution and exposure of grain boundaries, or the spallation of the fuel. Increasing surface area according to Dr. Ahn's model, would lead to higher rates of the release of colloids.

Furthermore, the effects of radiation and stress would lead to formation of colloids. Stress could come about because of the change in volume as the uranium oxidizes. In certain states, it either oxidizes or shrinks, depending on its oxidation state, and this would lead to stress and spallation of the fuel. Dr. Ahn also describes microbial attack as a possibility in the oxidizing, warm environment of Yucca Mountain. This might be a consideration in both corrosion of the canisters and of the fuel.

There is much evidence for kinetic factors being important in uranium. There is natural experiments in the field with uranium mines. There is a lot of evidence for multi-phase formation as the uranium in these mines oxidizes. There are 160-some species of uranyl compounds, probably many more. You can see 50 to 150 of these in the paragenesis of secondary phases in some of these mines.

Some of these phases are unstable. One of the reasons that it could be unstable is from the radiation in the spent fuel itself; also, microbial attack. These don't generally form protective layers, so they likely lead to further paragenesis to other phases.

Finally, there is the environmental changes which we observed in nature at uranium mines, wetting and drying cycles, temperature changes, and we're certainly going to have environmental changes over the 10,000 years at Yucca Mountain, both in terms of wetness and temperature.

I'd next like to move on to the Carbon-14 source term model, which I feel is one of the contributions NRC's made to the source term issue. We look at the inventories of Carbon-14 in cladding oxide and crud--this is the outer boundary of the cladding--the grain boundary and cladding gap, the Carbon-14 that's inside of the zircaloy that is not readily accessible, and then the Carbon-14 in the fuel itself that can get out as the fuel oxidizes. The model includes those four mechanisms.

I'll put up this familiar picture of a fuel rod, just to refresh your memories of where these inventories are; the crud on the outside and the metal itself, the gap, the fractures in the inner grain boundaries, and the fuel matrix itself.

The next slide shows the inventories which are similar to what Rich Van Konynenburg had put up, and we're assuming that, on the average, there's a total of 1.24 Curies per metric ton of carbon distributed between the various compartments of the fuel.

We assume that the carbon is in a reduced state initially and has to be oxidized before it can get out. The

Carbon-14 in the fuel gets out as the fuel oxidizes. I'll get into that more later. The carbon dioxide diffuses out through the fuel as it oxidizes. We assume that it's released quickly from grain boundaries, cladding/fuel gap, and initial zirconium oxide. We have some reasonable data to confirm this quick release fraction of about a total of 2 per cent, and there are minor releases from oxidation of the cladding, and there are also reasonable data on that based on many years of experience of zirconium in reactor fuel.

The model for the diffusion of Carbon-14 out of the outside zirconium oxide is based on some experimental data collected at PNL, and what this table shows is that for any reasonable range of temperatures, the amount of time that it would take to diffuse virtually all the Carbon-14 out of the oxide is short, even compared to 10,000 years. All of these times over this range and two different assumptions of activation energy show that we'd expect it virtually all to get out in 10,000 years. That was data collected by Smith and Baldwin.

Now, the fuel oxidation model, since most of the Carbon-14 we're worried about is in the fuel itself, we assume that there is no protection of the cladding and, of course, no oxidation can occur until the canister fails. Oxygen diffuses through two layers in our model. The outer layer represents diffusion through the grain boundaries of the fuel. The inner layer represents diffusion through the oxidized fuel layer; that is, the layer of the fuel grain that is starting to oxidize. We assume that that oxide is U_3O_7 stochiometrically, because we need to know how much oxygen it takes to convert the fuel. So it takes, for each half mole of oxygen, you convert three moles of uranium dioxide fuel.

We've assumed that the oxygen concentration is zero at the point that the fuel is oxidizing, and that the oxygen profiles in the fuel are at steady state. This can be illustrated on the next figure. I think I'll skip one of these here. This figure here.

We have the oxygen available at the outer boundary of the grain boundary layer, and then the oxygen's diffusing through the grain boundary layer, which has a rather high diffusion coefficient, and then further diffusing for the oxide layer, the U_3O_7 layer, to the UO_2, unoxidized UO_2 fuel.

As the front passes, we assume that the Carbon-14 in the fuel oxidizes, and then diffuses out through these two layers, the same two layers that the oxygen diffuses in. To make this model viable, we have to assume a certain size for the grain and the outer layer, and pick coefficients that represent the diffusion.

This assumption, incidentally, about the carbon oxidizing is difficult to substantiate, but thermodynamically, the carbon is less stable than the uranium, and it does appear that it would oxidize the carbon as soon as it comes into contact with the oxygen; that is, at the time that the front reaches the fuel.

So in order to identify the model, the parameters, we used data collected almost exclusively at PNL by Bob Einziger & company, that looked at the weight gain of the fuel that's exposed to air between--and also in dry bath experiments that Bob described yesterday--between 110☜ and 250☜ C.

Even though we expect a wide range in sizes of the particles, we took the grain diameter as 20 microns, and the outer layer diameter as 2 mm, representing the approximate size of a cracked up fuel grain inside a fuel rod. We also, in addition to the weight gain data from these two experiments, looked at ceramigraphic data, where they sliced the pieces together and physically observed the size of the oxide layer growing on the fuel grains.

We picked activation energies and diffusion coefficients based on the best fit from eight temperature ranges between 110☜ and 250☜, but there are little direct data on Carbon-14 releases. This is all inferred from the oxidation of the uranium fuel only.

So this next slide is not in the package, but shows that we came up with a single set of values for this simple model that adequately fit the data, and the next slide, which is in your package, shows the fit we got for four of the temperature ranges that shows us pretty reasonable fit, I would say, starting here at the lowest temperature, 109☜, 130☜. This, incidentally, is conversion versus time and hours, conversion of the fuel from the unoxidized to the fully oxidized. This is 225☜ and this is 250☜, so we're reasonably happy with that model, and that is the model that goes into looking at the release rates of Carbon-14.

Just as an illustrative example, we looked at two cases here. We assumed 66 randomly-spaced canisters that fail when their temperature drops below 96☜, and then has a failure--I should say that start to fail when the temperature drops below 96☜, and then has a lifetime beyond that of 1,000 years ✂ 300 years. We see that the release rates from the engineered barrier only exceed the EPA limit, and that most of it is due to the fuel, a little bit from the prompt release fraction, virtually none of the Carbon-14 from the oxidation of the zirconium.

Now, if you have a failure that instead of 1,000 years ✂ 300, 200 years ✂ 100, the canisters would fail when they're much hotter, and the rate expressions are temperature-dependent, so we'd see a much greater release rate mainly from the fuel. So temperature is dependent, and the canister lifetime is dependent in the Carbon-14 gaseous release model.

I might also point out an important point, that you can have the Carbon-14 release irrespective of any water. So this is one of the radionuclides that could be released before you get any water into the canisters.

I think I will skip the next two slides, other than to say that we're improving the model to take into account transient oxidation effects. Some of the coefficients may be off because we're assuming that the model's steady state, and in some of the data you see some effects that are transient with the oxidation weight gain, and this might lead to some errors in the application of the model. Also, we hope to look at the increase in surface area of the fuel with time at some later stage.

Briefly, the disruptive release cases, we look at the intrusive volcanism with a dike between 1,000 to 4,000 meters long, one to ten meters wide; also extrusive volcanism, a cinder cone between 25 and 100 meters radius; and drilling that either brings up a portion of the contents of a waste package, or if it doesn't hit the waste package but the canister has started to leak already, brings up contaminated rock.

In the intrusive volcanism case, we look either at bringing the contents of a waste package to the surface--which we consider unlikely--or the volatilization of some of the radionuclides in the waste. For those volatile radionuclides, we considered that iodine, oxides with selenium and technetium and cesium could be volatile, though there's some question about this--and also Carbon-14, of course. This is the ratio of what is in the waste as compared to what you're allowed to release under current EPA standards, so there's a great deal of cesium and not too much of anything else. Technetium is the next largest.

So according to some of the data I've laid my hands on, some of these things are, indeed, volatile. Whether they will get out into the environment is another thing, because they might very well be trapped in the liquid water in the rock and not actually get out to the atmosphere, but could be presented as liquid source terms to be transported down into the groundwater.

But the upshot of the calculations is that there is not very much alarm from the volatilization, even in about the worst case of a 4,000 meter long dike that's 10 meters wide and has a effective heating width of 100 meters of the rock, you would not volatilize more than 10 per cent of what you'd be allowed to be released of those things, according to the EPA standard. And also, the probabilities would be quite low for those things happening in the first place.

To begin to wrap up here, I think that our biggest information needs are on this slide. First of all, the integrity of the canister; are our corrosion models and our buckling models valid? I think we have got a long way to go. Furthermore, we don't even have a final design.

How does water actually get into the canister? It's hard to visualize how that would actually happen. I think we need modeling and physical experiments to get a handle on that. I think some of the modeling experiments of Tom Buscheck show that we're likely to have large areas of dry rock and long periods of saturation and re-wetting, but also the possibility of water being driven off one canister onto a cooler one, and I think we'd dearly like to get some good numbers to plug into the performance assessment modeling.

How does the water interact with the fuel? Will it drip onto the fuel? What will actually happen? I think we have only a few experiments. I was very glad to see Dr. Bates' results on the glass dissolution, where they were dripping water on the glass, and a very recent paper where they were dripping it onto uranium dioxide at 90°C to try to get a handle on what actually happens when water drips on the fuel.

I'm a little concerned that what I've seen with all of the uranium species that are out there, whether we'll ever be able to use modeling calculations to come up with syntheses of release rates from the canisters, and we may have to rely on empirical observations like that.

Does cladding offer protection? We're not taking any credit for it, but it's, as we all know, very corrosion-resistant and maybe we should take credit for it.

Are kinetic effects important? Will colloids form? Colloids, potentially, of things like plutonium could cause a lot of havoc. They could also be transported long distances in the rock and fractures, or they could be filtered out. I think there's a lot less known about those.

In conclusion, the NRC's model, SOTEC, includes waste package failure, releases of dissolved radionuclides. We treat colloids as if they're actually soluble, but extended the range of solubility of some of the radio elements to take that into account. We have a gaseous release model for Carbon-14, and we include codes for other gaseous radionuclides, volcanic intrusions, and drilling.

Finally, I'll put up a short list of some of the work going on at NRC and the Center for Nuclear Waste Regulatory Analyses, which support the source term. There's a detailed source term model that's being developed, called EPSPAC, of which SOTEC is a derivative. There is quite a bit of work going on in looking at natural analogs; Alligator Rivers, Pena Blanca, Santorini, Cigar Lakes, and Oklo, and a number of NRC and CNWRA scientists take part in those.

Tae Ahn and others are looking at kinetic effects of fuel dissolution. There is some work going on at the Center on thermodynamic properties of actinides in high-temperature solutions, and finally, there is some work going on in metallic phases in spent fuel, which I'm talking about the so-called five metal alloys, which may include technetium. If technetium's important, these things are very insoluble and may ultimately lead to reduced rates of technetium release in our performance assessment models.

Thanks. I'll like to answer any question.

DR. DOMENICO: Questions from the Board? Staff?

At this time I'd like to go forward. We're running a little behind, but before we--I'd like to depart from the agenda for a minute, and maybe John Bartlett would like to introduce the Under-Secretary to us.

MR. BARTLETT: Thank you very much, Pat.

I'm grateful for the opportunity here today to introduce to all of you, to the Board and those others in attendance, Dr. Hugo Pomrehn, the new Under-Secretary of Energy.

(Applause.)

MR. BARTLETT: Dr. Pomrehn brings to his responsibilities a wealth of great technical and management experience. He has over a 25-year career experience with

Bechtel Corporation in designing and running nuclear power reactors. He has solid professional experience in his thesis work in probabilistic risk assessment, and I think he's a very valuable addition to the Department in his oversight of this program, and I look forward to it and I hope you do, too. Thank you for the opportunity.

DR. DOMENICO: Thank you very much, John.

Returning to the agenda, it's time now to--well, as we sit here, we're going to have some overviews by our two consultants, and then followed by an open discussion, which should be at least a half an hour, and then some concluding remarks on the source term sessions by Dr. North.

So Mick Apted from Intera Information Technologies will give us a fifteen-minute presentation on his views of what he's heard here in the last few days.

DR. APTED: For those who don't know me, I'm Mick Apted with the other Intera, which is different than the Intera that's currently involved with the M&O, and if you want to know more about that story, I'm available for a few beers over lunch.

It's difficult in a sort of overview and review role, not to end up sounding an awful lot like a scold, and you shouldn't do this, you shouldn't do that, and I will get to that in a little bit, I think, but first I guess I'd rather be a gossip a little bit, and we're going to gossip a little bit about what's going on internationally in the area of source term modeling of near-field performance assessment, because I think the Board should probably be aware of this. The DOE and NRC are aware of what's been going on, so for the first two view graphs here, just sort of a simple summary of some recent events.

When one looks at, internationally, what's going on in the area of performance assessment for geologic disposal of waste, performance assessment is often resolved into--boy, that slide is awful, but anyway, into far-field and near-field performance assessment, and we also sometimes see biosphere being an important component in performance assessment.

We've done a preliminary review, and by "we," I mean myself and Dr. Kjell Andersson, who was formerly of the SKI regulatory group in Sweden, and we started looking at the various total system performance assessment reports that have been done by a variety of international organizations; the SKI, SKB are the two groups in Sweden, TVO in Finland, here we have the PACE-90, the WISP report from the National Academy of Sciences back in '83, the PHASE 1 report from the NRC, PAR report done by PNC in Japan, the Kristallin work done by Nagra in Switzerland, and this environmental impact work which is in draft from by the AECL.

So we looked at them in a very cursory, sort of overview way to evaluate what was being done in terms of near-field performance assessment, what was the role of the near field, and there is a considerable bit of writing behind this, so this is a summary, but in our summary, the role of the near field is very high in most organizations and in most reports. There are a variety of reasons for this that we really can't go into today fully, but a few years ago I made a presentation about the near-field PA being the little brother of performance assessment. We're often told to go over and stand in a corner because it's not really a very dominant part of the system.

What we're finding is more and more the idea of the near-field performance assessment is dominating the international scene, partly from an ability to achieve a higher degree of predictive reliability from these, what the Scots call the "robust, massive redundant barriers of the near field," and so I commend, I think at least some of the drum beats behind the scenes I hear within the U.S. program, that some of this message is, I think, coming in to them.

The argument here is not to eliminate the far field. I'm not saying that the far field is flawed or we have to compensate for the far field, but it's very much into this concept of multiply redundant barriers, that we're de-coupling the performance of an engineered part of the system, which may have an adequate performance from itself, and maybe have a certain degree of high reliability, from a far-field part of the system, which may have a tremendous isolation capacity, but may have a certain higher degree of uncertainty about that performance.

And so, it's not one or the other, but what we're seeing is that certainly in most countries, there's a considerable emphasis on evaluating, taking credit, and understanding the near-field performance. It looks to me like DOE's possibly getting their feet on that road, also, and that's, I think, to be commended. It is somewhat disheartening, sometimes, to see still the justification for near-field budgets, and so on, based on site suitability and exploratory shaft facility. Those are important, but the near-field performance is important in its own right, and probably that ought to be first and foremost in terms of trying to prioritize the work that's going to be done, in at least my opinion.

What I wanted to say as a follow-on from this review that Kjell Andersson and I have done, the Nuclear Energy Agency in Paris has endorsed and is coordinating with us a review of all the current work that's being done on source term modeling everywhere; everywhere from Korea, Japan, U.S., Canada, all of Europe. We're all putting this together for a large review. That is a presentation in itself. I'm not going to make that today. These are just some of the things in terms of the who, what, and where and why aspects of what we're doing.

The NEA is endorsing it. The proceedings will be put out, our review will be put out as an NEA document. The review has started already. Our draft discussion document will be available in January for review. We've been meeting with various DOE, NRC groups, academic groups here in the States. There'll be a workshop, not in early spring, but in late spring in the south of France. There's a sign-up sheet in the back of the--no, I'm kidding. But there will be a workshop that the CEA in France has graciously decided to host for us, and we will review some of the issues and try to find if there are common topics internationally by which all programs could benefit by consulting and comparing notes on approaches, what's worked, what's not worked for them. As I say, that's a whole other talk and I'll be glad to talk to people about exactly what's happening in this area.

Coming back to the last two days of talks, this is a view graph, actually, I got from, I think, Mike Cloninger awhile back from the DOE, and I like it not because it shows any sort of circular arguments--although we've heard a number of those--but it tries to put waste package performance, I think, into a proper context of its connectiveness to a number of important functions within our program.

I think yesterday we heard an awful lot in the area of waste package testing and modeling, and that's to be commended, and there's obvious interplay between the performance assessment model and this effort, and I'll come back to that in a second in terms of, perhaps, what that should be, what I perceive it to be in terms of what we have heard.

Today, my sense was that we've heard a lot more in terms of this coupling of the performance assessment, to addressing the regulatory issues and compliances; again, a very central and important role for performance assessment, but not the only role. And the trouble is, we sometimes confuse all this different activity that we need for performance assessment. It's very central to many activities. It's important to the design process. We heard, in passing, that some people are now gearing up to look at, possibly, some of these more robust waste package designs, and again, I think that's to be commended and we need to see more of it.

Hopefully, the design people are open to the idea that there could be something useful coming back from performance analysis. Maybe there's some way to possibly optimize this a little bit and it won't all be based on good engineering practices, but actually on something that relates to performance of the system.

I think one of the difficulties in addressing and sort of rolling up the role of performance assessment in testing is that--this is from Dave Stahl's slide on the first day, and he had a, you know, another diagram which had a whole other tier of different test activities below this--is that when we come up with this kind of flow diagram, what's passed upwards is a large degree of uncertainty by data collected, but with certain uncertainty about them, and what we also heard a lot of in almost everybody's view graph is the word "need." We need this, you need this, and that is really the chorus that comes up from below; we need this, we need this.

The real problem, the real difficulty, real need in the program is not a specific data point, but the need to sort among these apples, oranges, pumpkins, watermelons, and try to really, with limited time, limited resources, decide what is important. That's the role of performance assessment towards modeling, is providing some sort of guidance and sensitivity, allowing some sort of equitable comparison of very different activities.

I think we even, on the Nuclear Waste Board, we see sort of the same approach, is that all of you have certain important disciplines that you want to see reflected, but--and that's to the good. What's necessary, though, is how do we balance all these different importants (sic). Somebody says, well, advection is important, or heat transfer, or solubility is the most important. We need some integrative tool--and that's performance assessment--to put these together.

There's a fellow, Dr. Charles McCombie, a few years ago--he's in charge, or the Technical Manager of the Nagra program, and for those of you who may not know, Nagra is one of these programs that when everybody from the U.S. goes over there and comes back and says, why can't our program be more like Nagra's? They really seem to be getting things done.

Well, a few years ago, Charles McCombie made a presentation, really a challenging presentation to the Materials Research Society, which basically I have summarized here in a sieve, and a sieve is a series of questions. He says we should ask ourselves, as program managers, about work that is proposed to us.

The first thing he says: Does the study have scientific merit? If not, well, get lost. I mean, this is easily done through the standard channel of peer review, generally. But if the answer is yes, we go to his next question, which is: Does the study have significant relevance to waste disposal? Here, if the answer is no, we should say, seek funding elsewhere. We're not saying it's bad work, just probably some other group, National Science Foundation or somebody else; should be funding this work for you. But how do we do this? How do we rate among the studies that come to us whether it has significant relevance? This is the role, again, of integrated PA. There's no other tool for this.

If we get together a peer review group of a chemist, a hydrologist, and a health physicist, and we get a proposal in one or more areas, you tend to see this skewing of one person feels great and the other people feel, well, I don't know where to place this. What you need is people that are thinking laterally in a number of areas, and have the tools and integrated PA tool to make a decision on the significant relevance.

Once we have an answer of yes, I think the other activity we need to focus on is sensitivity analysis, handed down, if you will, from the modelers or from the modelers to the testing people to the design people, trying to help them to prioritize all the work that they have in mind in terms of barrier performances, processes that are important, site characteristic data that we need more information on, or maybe we have enough information on. How do we begin to turn off some of these spigots of need, need, need more, more data. How do we reach an end point on this?

So that being said, I think my feeling is in the last few days, we've heard a lot of good information down here, and sort of looking upwards towards saying, you know, here are our needs from modeling. We've heard a lot of good information about waste package and the role of regulatory interpretation. I haven't seen very much emphasis on how PA is being used to prioritize testing, and say we don't have an awful lot of money, we don't have an unlimited source of money. We're going to have to pick and choose, or in the same way, a sort of a feedback into the design process. So in that way, I was a bit, I think there's more yet to be heard from this group on these topics.

Now, how am I doing? Where am I, Pat, about ten minutes? Okay.

The last thing I want to say is that I was disappointed that we didn't hear about, and maybe I'm taking the Board to task, or I don't know who, but this is similar to design information that was presented on Monday by Dave Stahl, about some of these possible alternative designs, and, I mean, it wasn't in that particular view graph. What struck me, looking at this version of it, is we're talking about here distances of this package of almost two meters, or up to maybe four meters of backfill--or in this case, some sort of tuff gravel or something like that--between this waste package and this emplacement drift.

Now, the opening talk by Dave Stahl made the important point that source term modeling is the release from this engineered barrier system into the host rock. It's not released from the waste form. That's an important component, but that's not enough. What we need to focus on in addition is not just release, but transport in the near field, especially when we're going to start getting into these very, what are rather long pathways for partially-saturated media, and I'll show you why I think this is particularly important in partially-saturated media.

This is work done by Jim Conca, reported a couple years ago. He sort of dropped off the face of the earth after he left the national lab system, so it's sort of that if you want to keep current, I guess, you have to stick with the lab system. But anyway, this is some important work I think he's done on a variety of material, including gravels, tuff gravels here, looking at the effect of the diffusion coefficient as a function of volumetric water, and the classic models that we're familiar with--at least that I'm familiar with from these groups with saturated sites using bentonite, are all up here with a value, basically, of about 10^{-5} cm^2/second.

Now, on the other hand, for gravels, if the reason is going to gravels, they have very different transport characteristics. Their type of diffusion coefficients are going to be orders of magnitude lower than transport in compacted bentonite; orders of magnitude.

Well, why is that such a big deal? Well, we've seen a lot of the models, and this is a very--since Tom

Pickford's not here, I guess I feel somewhat obligated to flog some of the work that they have been doing for years and years now. This is a model developed by Chambre'. It's a simple analytic model, but what I want to use it for is to illustrate the relative sensitivity that emerges from just simple models like this. This happens to be for solubility-limited nuclides, so we see a direct scaling between the solubility term and the release rate out of the engineered barrier system.

But we also see a direct scaling from the diffusion coefficient; i.e., if we can demonstrate that these gravel barriers have a low transport rate, three orders of magnitude lower than perhaps what these other groups are using in saturated repository systems, maybe three orders of magnitude is rather significant. Maybe we ought to study a little more about the transport, because the other factor that comes out of transport is that--well, there are two other factors.

One is that the retardation term, which is sort of embedded here in the Kb term, is also a function of volumetric water content. As that volumetric water content goes down, reciprocally, the retardation in the gravel goes up, goes up considerably. So in two ways, we're getting a lot more performance if we gain a little more understanding about transport in the near field. And again, I think that was a missing technical element from this meeting.

We heard a lot about the chemistry. We heard a lot about the materials. Those are very important topics, but what was missing, I think, or what we need to hear more of and the Board needs to hear more of and to consider, is what is the role of transport in these type of systems.

Just to show you the effect, this is some work that was done, actually, for bentonite for Nagra, some work we did for Nagra awhile back. But here's the bentonite; b is bentonite thickness. This is basically a term--this D term that was in the other equation--bentonite thickness, decay constant, retardation, and diffusion coefficient. And if you can get this term, so a thicker backfill or a higher retardation coefficient, or a lower diffusion coefficient, all of which is likely to emerge from a better understanding of transport in gravels, we're going to be able to limit the release rate out of this system, or maybe decrease release rates by orders of magnitude.

The other thing that at least I like about transport is that, for example, if we're interested in neptunium or plutonium and Americium, we've got to go out and we've got to get separate information on each and every one of those radio elements. I'm not knocking that. I think we do need to do that, but notice that the release rate of all those radionuclides, all, every one of them, are going to be a function of the diffusion coefficient in this system.

So with one measurement, we're gaining a lot of understanding, and perhaps a more favorable performance for, essentially, the periodic table.

DR. DOMENICO: Mick, can you close?

DR. APTED: This is my last one.

DR. DOMENICO: All right. I don't want to cut into our discussion period too deeply.

DR. APTED: Yeah, I know; the last one, also work by Conca, and this relates to some of the other issues that emerged from this when we started considering transport, and that is some of the questions about whether we really need to know surface area, to what degree, whether colloids are going to transport, and that is in partially saturated systems as we expect at Yucca Mountain, through gravel barriers, there is essentially no--the available evidence indicates there is no colloid transport, usually because--or conceptually because what we have is very thin, basically monolayers of water that are dominating the actual diffusive transport, or very inefficient sort of diffusional pathways through these grains.

Because these diffusion coefficients are so low, again, it's this mass transfer resistance, not changes in surface area, that are going to dominate the release even of the alteration rate-limited elements, and so a lot of the attention that we paid yesterday to things like surface area models and colloids, and so on, may--I'm not saying are, but may all be knocked out if we start considering the transport factors.

Thank you.

DR. DOMENICO: I'm going to hold discussion. If Mick said something that doesn't set too well, I'm sure you can get him during the discussion period.

And our last formal presentation here, up to the discussion period, will be from Nava Garisto from Beak Consultants, Limited.

DR. GARISTO: I'm going to give a Canadian perspective on source term models, and although the Canadian program is not perfect, I think that some of its successes are due to the fact that we have been consistent with the recommendations that Mick has been doing today in terms of both the leadership role of performance assessment, and the integral part of transport modeling in the source term development.

In this context, source terms, again, are the flux of radionuclides at the exit from the engineered barriers, and it's the flux at the interface between the engineered barriers and the surrounding geological medium, which in our case is plutonic rock. Our engineered barriers include metallic containers, clay-based buffer, and backfill layers, and in this sense, it's actually quite a similar system.

So in this presentation, I will very briefly go over the source term for the Canadian concept, which includes container failure, release from the fuel, and includes mass transport as an integral part, but I won't go into details. I'll just try to point out those points that may be of interest to the U.S. program. In this sense, I'll go over just assumptions, improvements, and why we carry out these improvements, what are the practical limitations.

I'll try to focus on issues that are not very specific to a particular site or a particular design, so that the conclusions will be of interest to you, and as an example, I'll cover some issues that have to do with the probabilistic nature of some source terms that are related to risk assessments. And finally, I'll just make a few recommendations regarding ways to enhance the credibility and acceptability of source term models.

All right. So the first barriers that we are looking at is the container. In Canada, we are looking at titanium and copper, and I understand that titanium is not of much interest in the American program, but I want to say just a few words about it, because first of all, those of you who are looking at zircaloy may, instead of starting from scratch in terms of developing a zircaloy filler model, you can steal a lot of the methodology from this model, because the two metals behave very similarly. So that's one reason why I mention it.

The other reason is in the development of the titanium, a container failure function has been guided by PA. We had assumptions in the model; for example, we have the natural defects, we have corrosion along the containers. We assumed that there is sufficient oxygen to allow for a unlimited propagation. We have hydrogen-induced cracking below a certain temperature. We don't take credit for the mass transport properties of a container, and we don't take, explicitly, microbial activities into effect.

We know that we can improve the models, but we don't go into these improvements if the performance assessment, for example, show us that the containers, on one hand--that the models, on one hand, are conservative enough; that is, the improvement will just lower the dose, especially in those cases where we don't have to lower the dose, where we know that we meet the criteria.

On the other hand, if we know from performance assessment and sensitivity analyses that a certain feature maybe hasn't been done in detail enough, or maybe it's not conservative enough, then we know that we should spend more effort on this particular point.

Regarding the copper container, what we have here is a model that combines mass transport with corrosion. The corrosion rate is controlled by mass transport of corrosion products away, and we will improve it by also including mass transport of oxidants towards the container.

And again, the points that I would like to make for the American program is that the only way to get realistic corrosion models is by combined effort of corrosion scientists with mass transport specialists and hydrogeologists. In our case, the corrosion behavior of copper depends on the hydrogeology of the site, and I haven't seen that much of this joint team effort in the American program.

Realistic models require this kind of dialogue, and if you have only corrosion people defending the model, or only the performance assessment people defending the model, the credibility's going down, because it's only through the joint effort that people are ready to support this kind of model.

All right. So then the next barrier that we are looking at is spent fuel, and we have again short-term and long-term release models, similar to your program. The short-term release is instant release for gap and grain boundaries. In our case, the zircaloy is not a barrier, and if you compare the two programs, the rationale for it, I think, is that in your groundwater, the chloride concentration is very low, and in our groundwater, the chloride concentration is high, and that's why, you know, the environment interaction there is very severe, so they say that if the zircaloy can withstand the environment interaction, why should it fail once, you know, once it's outside the radiation field? The thing is, the zircaloy is very sensitive to high concentrations of chlorides, and in our environment, we can't--we assume that the zircaloy would fail within a year, and that's not, you know, if it fails within a year, it's not worthwhile to take credit for it. But we may revisit it, especially to look at maybe better models for the release of radionuclides that are trapped in the zircaloy.

In terms of the long-term release, we have congruent release of radionuclides, solubility-limited, from dissolving the fuel matrix, because we assume that the redox conditions are below U_3O_7. There is some effort now in the program by Frank Garisto and by Dave Shoesmith to include the radiolysis effects, which again are probably of less importance in the American program, but the methodology used can be--is applicable, also, to oxidizing conditions. So in this sense, again, there is some similarity, and I think the two programs can probably learn from each other.

The thing that we have included and, I guess, was referred to often was organics, and we did an analysis of the effect of organics on the source term. And in our case, the effect wasn't large.

The other effect that we have is precipitation, both of secondary phases, and of the uranium itself, so we have included diffusion and precipitation coupling in the model. I don't think that this is as important in the American program as in the Canadian program because you have more oxidizing conditions, but this is something that is missing from the glass model. The glass model that you have is affinity-limited, and with a precipitation downstream, for example, in the presence of iron particles, you have to include this kind of coupling.

You know we have applied it for fuel, but the methodology in the model is the same, and I think that for the people who are doing the glass dissolution modeling, it may be interesting for them to see how we coupled diffusion and precipitation processes.

The other thing that we have done and can be of interest in the American program is we have looked at the effects a calcite precipitation, on how it slows down the movement of CO_2 and carbonate. This was very preliminary work. We developed a couple of reactive transport models for this. We've showed how the cell can be, maybe, projected into an equivalent Kd kind of a model in case--for those people who can't handle the coupled reactive transport code, and again, this can be a starting point that can be of interest to the U.S. program.

In all of this derivation, the fuel behavior defines a boundary condition for mass transport equations, for transport of dissolution products away from the fuel, and the flux that is calculated from these equations is the source term. And again, I think that the strengths of the Canadian program in this was the fact that the source term was developed not by mathematicians, and not by geochemists. It was developed in a joint effort of electrochemists, material scientists, and theoreticians with a mass transport and hydrogeological background.

Mass transport, in our case, is an integral part of the source term. It's coupled to the source term and it affects it. In the EIS, which is the concept assessment, we have an analytical model with sectors, with mass transport coefficient boundary conditions and other simplifying assumptions, but we are working on ways to improve the models. For example, we do have a mass transport model now, and it has recently been incorporated into the assessment.

And again, whether we carry out these improvements or not depends on performance assessment, so the cycle that Mick has alluded to is actually being practiced in the Canadian program, and I think it's working relatively well.

So the bottom line on these sub-components are that it's important for experimentalists and theoreticians to work together in a project team, and I think that it's even important that they will be under the same roof. We've seen some nice collaboration, for example, in the glass program between people in Argonne and somewhere in California. I feel that most progress that I have done, for example, in source term development has been over coffee breaks, over daily contact with the experimentalists, because it's very difficult for a modeler like myself to gain the trust of an experimentalist, an "oh, you are just playing with numbers," and you need this daily contact to gain a respect for each other and to work together. So you need a model that is developed and defended both by the experimentalists and by the mathematicians.

I have just a few points to make on the statistical nature of the model. We have seen that people are using different distribution functions. Sometimes they use probability, sometimes they use uncertainty. The first thing that I would like to point out is that it's quite dangerous to mix all these things and to lump them into one delta, or one sigma. Variability is not the same as uncertainty, and it's not the same as probability, and we don't have time to get into details here, but even in our program, we mix all of them and it's just not valid.

Also, people tend to replace time dependence by a wide distribution function, and again, sometimes it's valid, sometimes it's not. It has to be checked. All along the model, there should be a consistent treatment of uncertainty in the first components. You can't just have uncertainty in one part, and then forget about it in the other one.

I have done, you know, I have seen here, too, you know, sometimes when you take a very simple model, it's very difficult to assign an uncertainty to it, you know. It's much easier to assign uncertainty to data, you know, the data is ✂ such and such, but if you use a simplified model instead of a more realistic one, how will you know what kind of delta to put on it? And all of these things have to be taken into account because the final result with the ✂ has to reflect the uncertainty in the sub-components.

Specifically, the recommendation that I would make, especially for the short-term release, is again to look at some of the work that we have done in Canada regarding the probabilistic nature of the instant release inventories for container. We've done it for gap and grain boundary release. It was a very preliminary work. It's not something that I am too proud of in terms of inconsistencies and things like that, but it's a starting point and I haven't seen a similar kind of approach in the U.S. program.

All right. That's the last slide. There are the usual ways to enhance the credibility of source term models, like we all talk about sub-component validation, and there were some references this morning to natural analogs. Some of these are good. I would like to just mention three which I personally like.

One is benchmarking. I was quite encouraged by a few comparisons that were mentioned today. They weren't mentioned in detail, but I think that there is some effort towards this, and that's encouraging. And to all this, I've done some comparisons with Mick, and I think that that was of benefit to both programs. I learned a lot from these comparisons, and I would very much recommend it to continue. It's not validation, but it's a step in terms of credibility.

The other thing is the role of performance assessment, and I guess I would just reiterate what Mick was saying earlier. In substance, we need a wide scope program, but it has to be focused, and otherwise, you know, you ride off in all directions, and the focus has to be provided both by data sensitivity analysis and by model sensitivity analysis, and that's the role of a performance assessment.

The last point comes mostly from the fact that I've recently moved from ACL to the private sector, and I have started to have experience in defending what you are doing in court, and when you are talking to the public or when you are talking to specialists from outside your field, and there are beautiful animation programs available now, and sometimes what we present to other scientists is too complicated, it's lists of numbers. We are talking here about a complicated 3-D design with hydrogeology and with source terms. There are gaps in the communication between us even in this room. Once we want to go out and sell these kind of concepts to people outside the program, I think that we will have to improve our presentation methods.

Thank you.

DR. DOMENICO: Thank you very much, Nava.

Well, we've come to the point in the program that everybody's been anxiously waiting for. We have approximately twenty minutes for an open discussion, and by open, I think we really mean open. We would like very much to hear from the silent audience out there.

Comments, questions to any of the participants?

DR. YOUNKER: Hi, I'm Jean Younker with the M&O, and Carl Gertz did ask me to respond to a comment that Mick Apted made when he was talking earlier, and that was to Mick, you might not be familiar with this, but we have given a number of presentations to the Board of tasks that we have done in the past, and we have one that we're just kind of completing, where we've taken every bit of performance assessment intelligence that we can possibly apply in an attempt to use it to focus the site program to ask the question: What kind of data is going to be most useful in assessing whether we have a suitable site, and also in assessing whether we can meet the regulatory standards that are out there?

So I think, you know, we've tried our best. You know, I agree with you that making that link is a tough one to do, but I think we have really had a pretty concerted effort to do that, and the Board has been informed on that.

DR. APTED: Yeah, no problem with that, just all I can comment on is what we saw in the last two days, and we certainly didn't see anything like you just suggested, so...

MR. BOAK: I'm Jerry Boak. I'm the Technical Analysis Branch Chief for the Yucca Mountain Project, and I guess that makes me the current bad parent in Mick's scenario about the orphan child of engineered barrier performance assessment. We haven't really been abusing them, although they may be a little shoeless these days.

I wanted to mention a couple of things; that Bill Halsey reminded me of a presentation that was done at one of our internal meetings, and Mick turns up in some strange places, but he didn't happen to turn up in that one, in which Joe Wang presented an analysis of borehole versus drift emplacement, and the effect of the backfill on that, and we do see a substantial improvement. There are some sensitivity studies that we haven't gotten ready. To some extent, our agenda here was constrained by some specific requests, and our attempt to try and get the right information, but we haven't gotten everything that could go on it.

With respect to Nava's comment about housing experimentalists and theoreticians under one roof, I think we've gone one better by getting them all out from under the roof and dragging them out into the field, the theoreticians kind of kicking and screaming every step of the way. But some of them came back with some interesting insights from actually having a chance to talk to Alan Flint and see what rocks look like outside of the computer.

And I think she also raised one of the points that we've been addressing a great deal, about the question of abstraction of models: How do we go from something that we can run thousands of iterations? How can we get answers to the vast array of scenarios and potential ranges of variables? We don't think at present we're able to do that using full, completely implemented and completely coupled process models. We are driven to the kind of simplified, higher-level models that we've shown in our diagrams of the pyramid that we showed.

Thank you.

DR. DOMENICO: Was that a comment, question? Mick, do you want to respond to it; or Nava?

DR. APTED: Well, I'll just add I don't mean to make the assertion on that, that I know there are activities that connect the design to the PA and the testing, and so on. It was just, again, perhaps at this meeting we haven't heard as many things as I sort of knew about and was sort of actually fishing for some more amplification from DOE.

DR. DOMENICO: Well, I'll make a comment.

Yeah, I see a gap between the detail presented yesterday, and the source terms presented today. Obviously, all of the information that we heard yesterday has not yet been captured in the essence of these models, and perhaps some of it is not important and perhaps it never will be captured in the essence of these source terms.

So when we talk about transport modeling, the source term is the most important thing that we have to deal with, the concentration that rock sees is going to determine basically what's going to end up at where you make your dose calculations, and if we're several orders of magnitude off there for some reason or another, we're several orders of magnitude off in the far-field calculations.

That said, I'm impressed with the progress that has been done because at least I now feel I know something about what was in the models that we heard a few years ago in a performance assessment meeting. At that time, all we got was far-field information, output, output, output, CCDF's, or what have you, and never any details of what went on inside the models; no physics, and just no details, and I, for one, feel that this morning has been quite successful, at least in trying to ferret out what processes, chemical and physical, are being incorporated in the source term model, so I feel pretty good about it.

Does anybody on the staff have a question or comment? Board? Dennis?

DR. PRICE: Dennis Price; Board.

Just a quick comment. We heard a conservative model presented in which it didn't satisfy the criteria in some ways, and so the answer, since it didn't satisfy the criteria, was it's too conservative, and so we need to go back and change the model.

We also heard a "realistic" model presented, and it was based on expert judgment. It satisfied the criteria. You might ask, if it didn't satisfy the criteria, what is the response? Do you get other experts, or do you go back to the experts and say, it didn't satisfy the criteria, and it sounds like in this sort of fuzzy business of model building, it's easy to get circular, and I think the issue that was mentioned from the floor about validation, not verification, but validation, is still at the crux of it, but with such a long term, it's an extremely difficult problem. That's just a comment.

DR. DOMENICO: Anything further from anybody? You mean we're going to--yes?

DR. O'CONNELL: Bill O'Connell from Livermore.

I feel that we could do a lot more in expansion of the simplified models. The progress that we have made over the past year or two has been a modest amount of progress, limited by funding. I realize most of the other parts of the project are also limited by funding, but we could make a lot more progress in incorporating some of the information that is becoming available from the detailed studies.

And second, we could do, you know, much more thorough and consistent treatment of uncertainty. The models really cannot be used to cut off some experimental studies, because the models are not robust enough and the input data are not robust enough. But perhaps by using uncertainties consistently, we could provide the robustness and show whether things matter or don't matter.

Now, the Golder Associates has a total system model which is including uncertainty in this way, and the EPRI model, by running through the CCDF's, they are effectively covering uncertainty, but there is a lot more that can be done, you know, starting from the simplified models and expanding them at the same time that detailed models and detailed experimental studies are underway and producing new information.

DR. DOMENICO: That's a point well taken. The thing that I'm still curious about is that if I had your four codes and I fed exactly the same information into all of them, assumed everything the same, the source concentrations that I predict, I presume, are going to vary all over the map, and I would be curious to find out the results of such an experiment like that, and I think that becomes important when NRC is developing their own to, in the words of Dick, keep them honest. And so there is variation there. I think that the processes covered have been covered in different detail by different groups, but your point is well taken.

With that, I'm going to turn the final discussion over to Warner North from the Waste Board.

DR. NORTH: Okay. I think I'll go up to the podium so I don't have to have my back to everybody.

I'd like to start off by thanking everybody. I think we've had a series of excellent presentations, a lot of good comments, and I'd especially like to thank our two consultants, Nava Garisto and Mick Apted, who have, I think, done an excellent job of making the points that I had intended to make, among other things, which simplifies my job considerably.

Nonetheless, I think it's useful to reiterate some of those points, just to stress their importance and set forth a clear agenda for some future meetings.

We have had, I think, in the last two days, a lot of very impressive and interesting material regarding processes and mechanisms related to the source term. I will certainly speak for myself to say that I've learned a great deal and have found it all extremely useful.

On the other hand, I've been quite disappointed. We haven't seen the kind of iteration and priority setting based on interaction between the scientific researchers and the performance assessment people that I would have liked to have seen, and I think Nava's right-hand column, what is the impact of the information, is just exactly what we were missing. That was relatively simple, and relatively quantitative, but in most cases, you had an indication of direction. Which way might this issue take us; higher numbers, or lower numbers? And what kind of thing might we need to do in order to resolve the issue and get better numbers?

Then we can turn that into questions of priority for a particular experiment or a set of tests, or further refinement in the analytical models, or perhaps more elaborate uncertainty analysis, and I hope we're going to see a lot of that. I'm encouraged by Jean and Jerry getting up and saying, well, it's coming. We've been doing it over on the site. We've been carrying out sensitivity analysis that is not yet presented, but then I go back to the point Mick was making, of how much of this presentation that we heard was directed at what I'll call the big issues of the regulatory criteria as opposed to the smaller scale issue of repository and engineered barrier system design.

It would have helped a lot, I think, if many of the presentations had been developed around several alternative designs for engineered barriers, maybe even some of the questions having to do with the repository design that are being considered in other meetings. That way, I think we do a lot better job of getting some insights as to what is important.

I'm concerned that the style of much of the presentation is, here is an overview of the processes and mechanisms and what we know about the science, and then we come down to some calculations of numbers--hopefully, we'll see a little bit more sensitivity analysis, rather than just one set of results--but what we're not doing at this point is going back and getting an iteration. Now that we have some numbers calculated, what are the driving issues? Which of these mechanisms and processes appear to be the most important, and why? What have we learned from the modeling exercises that we've just been through?

Now, I think there's a lot of experience in the nuclear industry in carrying out very complicated analyses on safety issues concerned with reactors, and a lot of these issues of how do we manage the process, I think, have been learned in that context.

We started out with exercises like WASH-1400, the very elaborate attempt to calculate a bottom line; how safe were nuclear reactors? Then I think we got some very insightful criticism from people like Hal Lewis and Bob Budnitz in a review they did of that whole exercise, and the way I remember their major theme is that you have to recognize the assumptions and the uncertainties that go into those calculations, such that if you try to get an absolute bottom line result out--"How safe is this? What is the performance of the reactor or the repository?"--it's very hard to defend the strength of that number. It's a soft number rather than a hard number.

On the other hand, if you use the analysis as a way of looking, what difference does some change make? What if we add one more backup system, or we make a pipe or a brace a little thicker, what difference does that make on the risk? Is it a big number or is it a little number? That can help a lot.

I think Mick was just explaining to us one example on the transport, that if you start doing some detailed analysis of the effect of crushed tuff or gravel, we may find that you can pick up orders of magnitude in the performance of the repository for a very, very low cost, and I submit that's where we ought to be focusing.

We ought to be using the analysis as the engine of evolution, and we ought to be iterating around this loop, and we ought to be using it to inform the top management what are the most critical issues, and provide a basis for planning, where, as we go through this era of very tight budgets, we can direct the funding and the scientific resources in the most productive way, with full attention to the time scales so that we can start the long lead time information gathering and analytical activities in an appropriate time to have the results where they can make a difference in design decisions, as well as the questions of overall site suitability.

Now, I'd like to conclude by making a few remarks about the teamwork issue, again, picking up from some of the remarks that were made by Nava and Mick. I'm very impressed with how much international commonality there is on these issues. The Board has had the opportunity to travel to a number of different countries and see what their programs are doing, and there certainly is a lot that can be learned about source term by taking advantage of the international community, and I think our two consultants have done a marvelous job of providing us with a brief summary of that.

The issue of having the performance assessment people and the scientific investigators from a multitude of disciplines all work together seems to me very critical. It's hard to do that under one roof, given the institutional structure of the U.S. program. It's certainly a great idea to get people to go together out in the field and trade information out there, but it seems to me there is a tremendous value to coffee break communication, to use Nava's phrase, of having people see each other frequently and communicate in an informal way, as opposed to just formal meetings, so that they can understand each other's point of view and find ways of working together that may be very hard for the management to be able to accomplish, other than essentially enabling the informal communication to take place, and letting people find effective ways of working together because there is a clear incentive to do that.

So as I think about where we're going on source term and the performance assessment area more broadly, it seems to me these issues of teamwork are really quite critical. We have a lot of pressure in terms of the time and the schedule, and we have some pressure in terms of how difficult it is to provide continuity of effort. It's really hard to keep a team working effectively when we get some new players coming in and we get some other players going out. We had an example that Mick presented of somebody who's apparently done some very valuable research, who's no longer available in the national lab system.

So it seems to me that in thinking through where this ought to go, the questions of how to build, maintain, and enhance the team are really very important, but what I'd like to conclude on, as overall, I think we've had an extremely useful and valuable meeting, and, really, the issue is how can we continue from here and make a great deal of progress, putting these ideas into effect to help the Department of Energy have a more effective program.

DR. DOMENICO: Thank you, Warner.

My agenda says we have just listened to the concluding remarks on the source session, so I suspect we are near conclusion.

I want to thank all the speakers we had this morning for their deliberations, and DOE for its organization, and the audience for its participation.

We're done until two-fifteen.

(Whereupon, a lunch recess was taken.)

AFTERNOON SESSION

DR. ALLEN: May we reconvene, please?

I'm Clarence Allen, Chairman of the Board's Panel on Structural Geology and Geoengineering, and it's my privilege to introduce the next speaker.

Last month, a number of us were on a field trip to Yucca Mountain and we were exposed for the first time to some of the detailed field mapping that's been done over the past few months by the U.S. Geological Survey on the Ghost Dance Fault, and some of us thought this was of significance enough to the program that we ought to be briefed on some of the recent findings there today.

So the person in charge of that particular project is Rick Spengler. I'll introduce Rick Spengler now.

MR. SPENGLER: Thank you, Clarence.

Let me just briefly mention that this particular study, detailed mapping of the Ghost Dance Fault, was initiated the beginning in FY92, and we are in the process now of field-checking our maps, quality checks of the maps, writing our open file report on our findings to date, and we intend to submit this for technical review within the next few weeks.

With that in mind, it's a bit unique in that under a normal process, review process, we wouldn't want to present the data until we have an opportunity to do our analysis of the data, do our complete field-checking and complete technical review, but obviously, because of the importance of new information regarding the potential repository area, we certainly welcome the opportunity to present some of these early findings.

Just as a brief overview, the rock characteristics section, the rules and responsibilities are the collection , analysis, and interpretation of geologic and geophysical, as well as geochemical data to be incorporated in a three-dimensional geologic model of the site area, and this particular three-dimensional model in turn feeds information to a number of other models, a number of other assessments, as well as other concerns.

To name some of those, we've got this information that we collect, geologic information provides data to the site structural, tectonic models, seismicity models; the site-scale unsaturated zone model; transport pathways or the saturated zone concerns the fast transport pathways and the steep gradient to the north of Yucca Mountain; geochemical models; resource assessment; and then, finally, supplies information to design and performance assessment of the potential repository.

The way that we have our studies within the rock characteristics section organized, the three major components all feeding into the 3-D geologic model; those being geologic mapping, stratigraphic studies. Our mapping until FY92 was basically the mapping by Scott and others at a scale of 1:12,000, or one inch equaled 1,000 feet. We had a number of stratigraphic studies ongoing to measure sections in and around the vicinity of Yucca Mountain. Those, in turn, other components including any of the subsurface drilling, supplying subsurface information, as well as borehole geophysics and surface-based geophysics, both feeding into the geologic model. We also recognized the need for the underground geologic mapping to feed into the overall model to complete the correlation of surface and subsurface.

As kind of a brief summary here, there was interest in the Ghost Dance Fault on the site tour, the NWTRB site tour held June 28th, 1989, and at that time, at the stop--one of the stops being at the Ghost Dance Fault--there was a summary given of some of these components. The summary includes that the Ghost Dance Fault is a high-angle, west-side-down normal fault. It offsets the 12.7 million-year-old Tiva Canyon member or the Paintbrush Tuff. Down to the south, the southern end of the potential repository, it offsets strata about 100 or so feet. As it extends northward, it dies out into a fractured zone. This is well-documented in the Scott and Bonk map, 1984.

The work by Swadley and others, 1984, indicates that no quaternary offsets were found along that fault. The fault is expressed by the offset of strata, the presence of breccia zones, slickensides, and the positive relief on the upthrown side of the block. The fault dips 79☜ to 90☜ at the surface, and I also inferred at that time that the character at depth is pretty well unknown. It maybe a single fracture or it may be a set of fractures, may be a listric fault, and that other faults or fracture zones may be present. We didn't anticipate any major structures; however, at that time, I also pointed out that there were minor faults and fractures are probably numerous throughout the area.

In the Ghost Dance Fault study, our objectives for this particular, for FY92, was to combine some of our fracture mapping with some detailed geologic mapping to better characterize this particular fault. The objective here in our discussions and integration meetings with the unsaturated zone modelers, one of the components that was mentioned was the need for structural data within their site scale model area, and so the primary objective was to provide data for the unsaturated zone model.

We wanted to initiate a study within the confines of one fiscal year to test out our technique. We wanted to also establish some type of grid system to be used here so that if the study proves to be worthwhile, that we can later expand on our network.

This is a map of Yucca Mountain. This is taken from Scott and Bonk, 1984, showing the potential repository area and some of the structures that are from Scott and Bonk. The major throughgoing feature within the potential repository area, obviously, is the Ghost Dance Fault, shown here, and along the Ghost Dance Fault there, Scott had mapped a number of areas showing breccia. I might also point out within the potential repository area, Scott had also mapped several other structures, one being the breccia zone along this particular north-trending feature, a number of breccias found throughout. I also point out that he did see these linears as far as a northwesterly fracture trend in other areas of Yucca Mountain, some discontinuous faults to the south here.

I'd also point out he did map at least two other faults to the east, until he hit what we are now referring to as the broken zone on this side of Yucca Mountain. This particular scale here, this grid system, this grid here on the side is 4,000 feet. This is our study area for FY92. We decided to cover an area--straddle the Ghost Dance Fault to the south here--it's a width of about 800 feet--as we extend northward, our width of our study area decreased to 600 feet.

We established a grid system based on the Nevada state coordinates. This attempt to do this is so that we can go back and expand on our grid system. We have mapped 61 areas along the Ghost Dance Fault. Each area is labeled. Each area is 200 x 200 foot square, and it includes everything from the southern part up to about the middle part of the Ghost Dance Fault.

Our blocks here are labeled based on the coordinate system. We just kind of drop the first and the last three digits of the northing and the easting to come up with a designation for the block, and then we go ahead and subdivide the block into 200 x 200 foot squares.

I would also point out that our study area includes the south face of Antler Ridge. It includes both faces of Whale Back Ridge, and to the south here, includes the northern face of what we call Broken Limb Ridge.

Just for an explanation here--I'll go through this quickly--we had two types of mapping here in that we wanted to map where we had alluvium, colluvium, and then also we tried to map out where our area partially covered with colluvium. Now, our definition here is that we can still see large-scale features showing through the cover, but it still is partially covered. Our lithologic section that we've measured out at Yucca Mountain includes everything from the Cap Rock down to the Hackly Unit. The Columnar Unit occurs directly beneath the washes.

This is an example of one of our areas, our 200 x 200 foot areas, 6262B; 200 x 200 on the side, and then when we're out in the field, we go ahead and make up a temporary grid and divide up this area into 40 foot increments. In this particular case, this is along one of the areas along Antler Ridge, indicating where we've mapped some of the breccia zones. This is the main trace of the Ghost Dance Fault here, and we found breccia zones in other areas. We've mapped fractures, we labeled the fractures, and what also is present on this is the breakdown or contacts of the units. In this case, this is the upper lithophysae unit here, this being CKS, the Clinkstone Unit.

So our fracture mapping mapped at a scale of one inch equalled 20 feet. We mapped fractures or sets of fractures that measured six feet in length or longer. Our fracture attributes included the location, the length of the fracture, the elevation, lithology, attitude, spacing, roughness coefficient, and fracture mineralogy.

Likewise, in our fault mapping, mapping at the 1:240. We were mapping the location, nature, and continuity of some of the breccia zones, offsets that we see in the Tiva Canyon Member, and any changes in attitude that we see from the contacts of the subunits of the Tiva Canyon Member.

These subunits are zonal variations, and these zonal variations include a wide range of attributes. These include differences in the groundmass devitrification. They include welding, changes in welding, the shape of the eroded surfaces, the texture of the subunits or the weathered surfaces making up the subunits, the abundance of lithophysae cavities and lithic fragments, as well as phenocryst ratios. Now, any of these attributes that are used in the mapping are basically the same attributes that were used in the 1:12,000 mapping done by Scott and Bonk in 1984. However, we've refined it a little more in that we are able to see slight variations in welding and in many of these attributes that can be used to detect offsets that may be on the order of a couple of feet.

We then have taken all of these 61 areas that we've mapped, compiled them on a base map of one inch equals 50 feet, or 1:600.

Back to our fracture mapping, we've managed in this area to map 745 fractures distributed throughout the subunits of the Tiva Canyon, the lengths varying from 6 to 85 feet.

This is an example of some of our slope that we're mapping here. This is the south-facing slope of Antler Ridge. Some of you have had the opportunity to visit this slope recently, and what we see here, looking to the north, we can from Scott and Bonk's mapping at 1:12,000, you can pretty much pick out the main trace of the Ghost Dance Fault coming through here, offsetting the cliff-forming or Upper Cliff Unit here from the Upper Cliff Unit here. So the main trace is right through here. As you can see from this photograph, the area is pretty much partially covered, but when you get down on your hands and knees, you can see through part of the cover and identify other features.

For example, here, these areas here are what we would map as covered, and these peculiar features are rock slides of the Upper Cliff, presumably coming from this area, sliding down the slope. Other than these covered areas, most of the area is partially covered.

Well, in our detailed mapping, we have discovered more than just the main trace of the Ghost Dance Fault. What we've mapped out are several other north-trending traces; again, the main trace of the Ghost Dance Fault here, but in mapping the subunits and breccia zones, we've discovered that on either side of the main trace, there are several other faults that can be identified by offsets of strata, as well as breccia.

This particular outcrop here is roughly 250 feet of relief, and in our grid system, it would come out to about here. So it's about 600 feet this way. So in this, you'd note that there's about 50 feet of displacement along the main trace of the Ghost Dance Fault. There are these other features where we identified offsets on the order of five to twenty feet.

Some of our preliminary findings, the fracture mapping indicates that there's a dominance of northwest-trending high-angle fractures. The trend is roughly north to north 10 west, and this particular pattern occurs throughout all the subunits of the Tiva Canyon within our study area.

The Ghost Dance Fault, as indicated by the previous slide, appears to be anastomosing a subparallel network of several north-trending faults, and these faults show minor displacement on the order of five to twenty feet, and if you take the width, or if you include this as some type of zone, then the lateral extent of the zone may be around 500 or 700 feet.

With these preliminary findings, and in reviewing our maps, we have proposed several things for continuing this effort in FY93. The items include extending this type, using this type of mapping technique to extend northward to cover the entire length of the Ghost Dance Fault; to use this grid-attribute technique to also map a much broader area outside or farther away from the Ghost Dance Fault to compare with what we're seeing in straddling the Ghost Dance Fault. We would also like to attempt to apply this technique to selected areas along the north and south ramp, and in addition to that, we have proposed that we augment this mapping technique perhaps by exposing the lowermost flanks of some of these ridges, such as Antler Ridge, to see what the character of some of these rock units look like, either clearing pavements or constructing some type of road cuts.

These proposals include these particular areas. This, in yellow, shows again our area completed in '92. We'd like to extend that and include the entire length of the Ghost Dance Fault, shown in green, to expand in both directions, east and west, shown in blue, and on also selected areas in the north and south ramp areas.

And just as some qualifying statements here, again, I reiterate that this data is very preliminary and incomplete, and at least at the present time, we haven't had the opportunity to do a complete analysis of our fracture and fault data, nor interpretation of that data; and also, that this structural information or this structural study was not focused in on the age determinations of the Ghost Dance Fault. Swadley and others had had at least one trench along the fault, Ghost Dance Fault, and three trenches along the Abandoned Wash segment, and as I indicated earlier, there does not seem to be any quaternary movement shown by those trenches; however, that does not negate the possibility that a trenching program also be initiated along segments of the Ghost Dance Fault.

Finally, I'd also like to reiterate that as far as our geologic mapping and development of the 3-D geologic model, that we certainly recognize that there's a need for both components, both the surface component as well as the subsurface component, and to get a high degree of confidence in the geologic model, you certainly need both to make the correlations of what you find at the surface, or what you find at the subsurface with what you see at the surface.

Thank you.

DR. ALLEN: Thank you, Rick.

One point of clarification, please; a couple. You state that Swadley, Hoover & Rosholt found no evidence of quaternary faulting, but as I understood it, they found no evidence to preclude quaternary, either. Is that correct or not?

MR. SPENGLER: Yeah, that is correct, in that the units that were actually sampled in those trenches--in my understanding, the units that they sampled in those trenches--however, I'm not an expert in quaternary fault mapping--that the units weren't there to isolate the age of a fault.

DR. ALLEN: Okay. Although I don't mean to sound like a lawyer, but am I correct in saying that the recent work on the Solitario Canyon Fault, which is the next fault to the west--about a mile away, bordering the west side of the repository block, where there are quaternary units of young age--we now have documentation of Holocene displacement on the Solitario Canyon Fault. As I understood it, that was true. Is that right?

MR. SPENGLER: Yes, that's correct.

DR. ALLEN: Okay. That's why I'm so concerned about--or why we're concerned about this fault, because if we cannot find evidence precluding late quaternary displacement, Holocene displacement on the Ghost Dance Faults and the branches, then that may put us in the rather difficult position of perhaps having to assume that there is, to be conservative.

Are there questions from the Board? Yes, Don?

DR. LANGMUIR: Langmuir; Board.

Another obvious concern we have is the potential for fluid movement along the faults. I wondered if Spengler or you people identified any kind of bleaching or coloration changes which would indicate fluid flow had occurred preferentially along the Ghost Dance or any of the faults that you've been able to see from the surface?

MR. SPENGLER: Yes, we have. In our mapping, we have also attempted to map any areas of alteration, or relative differences in alteration, and we do see some type of alteration. We attribute it, at least in the preliminary sense, to be vapor-phase alteration, or related to early cooling of the ash-flow tuffs, but the mapping out the distribution of that alteration and doing any petrographic analysis basically remains to be done. All of this has been megascopic mapping.

DR. LANGMUIR: So you can't tell whether the fluid was a vapor rising or water going down to the system by infiltration?

MR. SPENGLER: No.

DR. LANGMUIR: That's something you'll be looking at.

MR. SPENGLER: All we've done at this point is map out relative areas of alteration.

DR. ALLEN: Ed Cording?

DR. CORDING: I'm pleased to see the program for looking not only along the Ghost Dance Fault this next year, but also areas away from it to get some real details in typical areas that perhaps are away from the Ghost Dance. I think that will be very valuable information, and then, also, the surface information along the ramps. When that's combined with, ultimately, underground information, I think it'll be an important picture to see both the surface and the underground information.

MR. SPENGLER: I think this detailed mapping 1:240, which is nothing new to the mining industry. That's something they routinely do as far as the mapping mining claims, as well as open pit mining, so our attempt was get to a scale that would at least be similar to a scale that could be used to the underground mapping, and use that as a correlation tool.

DR. CORDING: Exactly; yes.

DR. ALLEN: I would also urge that we make another attempt to define localities, particularly in the southern branches of the fault where it goes out into those alluvial valleys, to see if there isn't some possibility somewhere of telling something about the age of movement, because this may turn out to be quite critical in terms of, you know, a fair segment of the repository block.

Staff?

(No audible response.)

DR. ALLEN: We still have a couple more minutes. Any questions from the audience?

(No audible response.)

DR. ALLEN: Okay. Thank you very much, Rick. We appreciate being brought up to date.

Let me turn the meeting back over to our Chairman, John Cantlon.

DR. CANTLON: I thank you.

Our next speaker is John Bartlett, who's going to give us some introductory remarks.

John?

MR. BARTLETT: It seems rather odd to have the third item from the end of a two-day agenda labeled, "Introductory

Remarks." Consequently, what I'd like to start with are some summary remarks, if I might. I would like to make some comments relative to what I heard earlier today, and since I have the benefit of ignorance, not having been here yesterday, I feel free to address only the limited area that I did have an opportunity to listen to, and what I'd like to do is give you something of a, needless to say, a management or director's perspective on some of the issues that were addressed by the presentations, and by Dr. North's summary, and the critiques from the consultants and the like, because I think they're vitally important to the program.

I think it's obvious, probably, to you--not probably, I'm sure it's obvious to you--from the presentations you've heard that the technical issues associated with near-field matters associated with the repository are, indeed, a very rich and fertile load of opportunities for technical activity, and this raises, in the Director's mind, a very important issue, and that is to distinguish the possible from the necessary.

The array of possible, technically possible, is just enormous. The key issue for management of the program, defense of the program in the external arenas is what's necessary among the possible, and the key question there is, how do you determine what's necessary, and how do you defend that determination?

And basically, the strategy we would use--I used to do that--is to look ahead, of course, as to what information is necessary for the regulatory arena, regulatory compliance. And then that gets into the second order question, which is basically, how do you convince the regulatory arena that what you have defined as necessary is, indeed, the necessary; that you have made the proper selection of the array of the possible; that is, the information that's appropriate for the regulatory arena? Because, as you heard today, the array of possibilities that can be identified by the regulatory body is of a comparable magnitude to that that can be identified within the program itself.

So what we try to do is look ahead to the regulatory arena, make our estimate of what is necessary, and then the question becomes: Can you demonstrate that what you have defined as necessary is, in fact, that, without doing it all to prove that that was what was necessary? Very challenging question.

My answer to that is the way you do it is through such things as use of expert judgment and performance assessment, and I was delighted to hear Dr. North use my phrase, "engine of evolution," because that's exactly what it's all about. We're evolving that, and I'll talk about that in a couple of minutes in my introductory remarks. But fundamentally, that's where that comes into play, is in the process of exercising performance assessment expert judgment and the like for purposes of selecting and defending the necessary work for the program from among the range of possible activities, and that's one of the things we will be focusing on in the future.

I'd like to comment very briefly on a couple of other things I took notes on. Bob Shaw made the comment that he thought maybe the CCDF would go away if the regs are changed. I don't think it'll ever go away, because it will pop up inevitably, even if you have a dose standard rather than a probabilistic standard. It'll pop up in the regulatory compliance demonstration inevitably, because the compliance demonstration process will inevitably be probabilistic in itself, and the CCDF is a tool for that no matter what the standard is. You might have different things on the grid, but you're still going to be using the same method of analysis for demonstration of compliance.

There's another thing. I'd like to offer you an opportunity to win. There was commentary about the universal canister, and there was an oblique comment that that's not the right phrase. It sure isn't. There's a history of use of that where the concept is a single canister that does everything, and it's used to pick up at the reactor, to transport, to store, and then to dispose. That's not the concept I want to emphasize. What we're talking about is a relatively lean and mean beer can or something of that nature that the spent fuel would be put in at the reactor, and then that has sleeves that would come and go, depending on what the function is. You put that into a transport cask, for example; remove it from the transport cask, put it into, perhaps, something else for intermediate storage, perhaps something else for disposal. That's the concept.

The trick is, the opportunity to win is, name that concept. Get away from the phrase "universal canister." We need to get away from that, because it's almost pejorative, and it certainly has a history that we don't want to associate with that.

Let me turn now to the introductory remarks. I think you're certainly all aware of the fact that we inside the beltway are just coming off a very intense period of activity, wherein the Congress passed, in relatively short time frame, the Energy Bill, the WIPP Bill, and the Energy and Water Appropriation Bill, and let me make first a couple of remarks about the Energy Bill.

Within that, of course, the most significant aspect of it for our program is Title 801, which dealt with the disposal standards. It called for the National Academy of Sciences to do a scientific study as a basis for promulgation of standards through the EPA and the NRC. It set a schedule for that, and basically, the most important thing about it, it will convert under law now the type of standards used for the program from performance-based release from the repository to dose-based, which is the approach to regulation that's used throughout the rest of the world.

The thing I want to comment on here for emphasis is there's been a lot of press comment and the like that this will relax the standards. There is no implication whatsoever in the legislation of an impact on the stringency or whatever of the standards. What that will turn out to be depends on what comes out of the process that's been set up by the Act. They could be more stringent. They could be more relaxed, although that's a very hard equation to make, because to get a relationship between the performance of a repository, the release and the dose standard is, in fact, a very difficult thing. So I think it will be very difficult to say it's going to be more relaxed or it's going to be more stringent or whatever. Frankly, I think that's an irrelevant issue.

What does need to come out of it is appropriate standards to assure the public health and safety for disposal, and I think with the process that's been set up, the opportunity is there with a scientific base from the Academy, and the public processes, that the EPA and the NRC have to use to promulgate the standards, will be that we will get out of the process with what I certainly hope will be appropriate standards for disposal safety.

Let me turn now to the budget aspects. Very simply, again, I think you're all aware that the Congress appropriated for Fiscal Year 1993 for our program $375 million. That's a $100 million increase from last year, and basically, Carl took it all.

(Laughter.)

MR. BARTLETT: He's going to tell you how he's going to spend it.

I would inform you today that I cannot say anything whatsoever about funding beyond the current year. As a matter of fact, the Department has not even submitted its own proposal for Fiscal 94 funding to the OMB yet, and of course, the internal budget process within the Administration goes on without essentially public process until the budget is submitted by the President to the Congress next January. So, frankly, I have no idea what the budget will be beyond this year, but you will hear from Carl the details of how the expenditures are expected to be used with respect to the Yucca Mountain Project this year.

I'd like also to comment briefly on three things that we have underway within the program, basically in support of the work activities for Yucca Mountain. These are not new to you, but I would like to underline them and emphasize them.

First of all, we have underway an assessment of opportunities to revise the work activities associated with the Yucca Mountain Project. We have been working since 1988 to the specifics of the site characterization plan. As a result of data acquisition and interpretation since then, we now have a better idea of what might be done in the future, and we are in process of using that information we've acquired to determine if there are revisions to the original work plan that might be beneficial.

The tools of the doing of that are the early site suitability evaluation report, which was issued to the Department by our contractors, and to the public for review back in February; the total systems performance assessment report that was issued in July; and an ongoing effort which we call integrated technical evaluation, which is looking at the interactive aspects and opportunities for prioritization in the various technologies working within the Yucca Mountain Project. All of this will be brought together for an assessment of potential revisions to the work activities, and we expect to complete that early next year.

We are also developing, as part of our program plan activities, and a public exposition of our activities, a series of program milestones associated with evaluation of site suitability. I'm sure you're all aware that we have been historically focusing on the 2001 date for submission of license application to the NRC if the site is found suitable.

There are several reasons of history for that focus, not the least of which is that that's the date set by the Secretary's plan, and it also is the target date for the overall planning of the Yucca Mountain Project activities, and so what we are doing now is building into the time space between now and that point in time, specific performance milestones for communication, with the Board and with others, with regard to progress in evaluation of the suitability or unsuitability of the site against the criteria for site evaluation.

So we will be presenting again in the near future what amounts to a more detailed program plan which shows you the basis for, and includes these milestones, essentially which will be related to topical reports of findings relating to the technical issues associated with suitability.

One other thing that we're doing is continuing our effort to assure effective management of the project. Engine of evolution started it. You may recall in a previous presentation to the Board, you heard from the M&O about convergence, and you'll recall the sideways ice cream cone diagram where we had the various activities converging on the products of site suitability findings and environmental impact statement license application if the site is suitable. We are converting that into a management action plan, and no surprise, under the title, internally, of "Convergence." And we have that actively underway to assure that we have the right things underway and active to implement the engine of evolution, and that we have the right performance assessment tools for the engine, et cetera. And so that's the framework under which Carl was working, and I'll ask Carl now to talk about, specifically, the activities for the coming year.

MR. GERTZ: John, thank you very much. It's always a pleasure to follow your boss, and then be assured that I'm pretty consistent with what he said. So I'll enhance on some of the things John said, including the particular budget, and I will tell you a little bit about what we think it takes to do the job in the out year. Whether that is part of the OMB submission, is another question, but it's a part of our 2001.

So I'm pleased to be here. I intend to talk maybe for 40 minutes or so, and then take any questions you might have as long as you might have. I notice we're last on the agenda, so I'm here as long as you all want to talk about it.

I'm going to start off with "focus of the program," where we've been focusing in the last year. I'm going to talk about some '92 accomplishments and the '92 budget so you can see where we're going in '93, and I'm going to spend some time on the '93 activities, and then address challenges and issues, some of which John already addressed.

This last year, in '92, we've tried to conduct the maximum amount of site geological investigations, tried to find out physically, gather data about suitability or disqualifiers, and our focus was to design the initial ESF activities; field work represented by activities like this, soil pits, drill paths, et cetera. It's underway. You'll see lots of that tomorrow.

Design work represented by a concept for the ESF where we've designed this type of facility and the first 200 feet of trench. Our goal in '93 is to build that design, to build that design and be ready to order a TBM and be ready to put a TBM in at the end of the starter tunnel. In '93, we want to continue gathering field data, address our environmental program and our outreach program, and monitoring and other site surface-based testing.

In '93, our goal is, in a simple cross-section, be a couple of hundred feet into the mountain. That drill pad you saw on the last view graph will be taken down. We'll have a new pad for the ESF, launch chamber, starting tunnel, call it what you want. Hopefully, a year from now or so, that's what it'll look like. The TBM won't be there, we don't think--I'll show you a schedule a little bit later--but we hope it'll be on the way at that time. The first 200 feet will be drill and blast.

I'll give you some of the bottom lines first. I need to just give you a little summary. There are some people in the audience who may not be familiar with the program, so I won't bore you to death with it, but certainly, everything we're doing was created by Congress. They told DOE to do the job. They set up a extensive suite of regulations, including standards by the EPA--and John addressed the update of that--regulations that we need to meet by the NRC, Department of Transportation for transportation, and they created a regulatory role for the State of Nevada, not only through current EPA and other flow-down areas, but also, they can veto the site after the study. That veto can be overturned by both houses of Congress.

They also created an extensive suite of oversights. I'll expand on that a little bit. This is one of the oversight activities. I've said it publicly before, and I'll say it again. I think when Congress said, "Study only Yucca Mountain," it was a stroke of genius in creating the Nuclear Waste Technical Review Board. It provided independent oversight; allows us to go to the public and say, "Not only are we licensed independently, but we have an independent Board chosen by the President to oversee our program."

Excuse me, one more thing. One more thing that is now coming to light a little bit in the state is that there are benefits available not only specifically in the law--a lump sum or impact assistance--but the state could work with a negotiator, to negotiate whatever role in the management, in the operation, in the safety, in the overview, along with financial benefits if they so choose. The state, at this time, has chosen not to pick up on that option. They've remained adamantly opposed at the political levels.

Talk about oversight. Here it is, and this is no small part of a cost of doing business on the project. We have the Congress, we have the NRC, we have you all, we have the EPA, we have OMB. I spent seven hours with Steve Goldberg and my staff going over the project on oversight with OMB on Monday. We have the utilities who are represented here by EEI, EPRI is involved, NARUC spent an extensive amount of time out here a month ago. The State of Nevada has three entities. Many of those--some of those people are here. They have the Waste Project Office, headed by Bob Loux; an agency of the Governor, the Commission on Nuclear Projects, headed by former Governor Grant Sawyer; and a legislative committee, and I think three of those members are in the audience today.

We have other affected units of local government. They're here today. The National Academy is involved with the permanent Board and some ad hoc boards, the most prominent of which has been the coupling effects one relatively to Jerry Szymanski's theory.

The GAO, they spend a lot of time with me. They write a lot of reports about the program. They're coming in next week for another week with us. The IG looks at different aspects of the program. We've had the Secretary of Energy's Advisory Board on Trust and Confidence out here for hearings and in other parts of the country, and Department of Interior, through the Parks Service, Fish and Wildlife, and BLM, and I can go on and on, but these are the major ones, and I thought it was important to point out to you that that's a lot of our customers. That's a lot of points of views that we need to meet and talk to about.

With all that oversight, and with everything else, we do have momentum, though. Field work is underway. Our QA program is in place and it's working and the scientists and QA professionals are working. Our technical baseline is in place and accepted, as John points out. It is the SCP. Can it be changed? Absolutely, it can be changed. With proper rationale, we can add or subtract tests from that.

Cost and schedule control system. I'll talk more about that, but we have a sound cost and schedule control system that withstood 16 months of an IG investigation, along with the almost monthly, GAO looks at it.

We have drilling equipment in place, as you're well aware. We have a construction management team on site in the form of the M&O, and the M&O, of course, has moved into transition with other activities here, too. We have major permits in place, and the state continues to process applications in a timely manner. The state is not holding us up at this time, and we have critical milestones that have been met.

Required funding? Well, we're somewhere in the check. We got more than we got last year, but we don't have what we think is enough to keep on the schedule in the out years, and I'll talk more about that in the future. But we do have momentum, and you'll see some of that tomorrow when you're out there.

Let me talk now about the accomplishments, and I'm going to go through these very briefly because I only want to set the stage for the other ones, and we'll do this with two things.

You're well aware we have the LM-300. We've built the pad, and we're on site drilling. Some you may not be as much aware of as Alan Flint's activities. They have completed 17 drillholes, seven more and we'll be finished with that program then we'll do our analyzation. Dr. Flint will talk to you a little bit about that tomorrow. Alan's using two different-type rigs.

We have an extensive amount of soil pits. You won't see all these tomorrow, because they're filled up. We got the data, and have filled them up, but that's out in Midway Valley. Right in there will be our entrance into Exile Hill, almost right into the picture, a little bit off to the right, for the tunnel.

In Midway Valley, of course, we're doing trenching, seeing if we have any hidden faults in the area. More of the soil pits, other trench-type investigations, that is an engineering soil pit, and this was our drilling on NRG-1. Now, although it's part of site characterization in our classification, it really is ESF work, because we're trying to find out the engineering properties below that drill path so we can design the tunnel.

We did some environmental drilling. We called it JF-3. That, of course, is complete. Our monitoring plan is in place and, in fact, as I'll point out later, we had a supplemental water permit from the state after our initial one for a 300 per cent increase in water. So, in effect, we have all our permits.

Other work that's going on, many of you were on the volcanism field trip of 40 excavations. Forty-five excavations were completed by Bruce Crowe. A long trench was completed in the Lathrop Wells area for scientific investigation.

So I want to point out, you'll see some of it tomorrow, you may not see all of it, but lots of work's going on.

Some of the things you don't see by the physical work, though, is our interactions with the Regulatory Commission. You all see the interactions with you all.

They're extensive. It's been an excellent three days today, or two days, and tomorrow will be the third one. We've had increased interactions with the Advisory Committee on Nuclear Waste. They're here next year--next week, excuse me--and they're going to go through some of the same stuff that we've gone through, including this kind of presentation, and we have been working towards closure by preparing an annotated outline. We've transmitted one for the NRC. The second revision to that is on its way.

What is driving the program? Boy, that's tough for a project manager to figure out what the priorities are. We are using performance assessment to identify some of those things. I think it was an excellent product, our first total systems performance assessment. We looked at many things. We did happen to publish some older performance assessments this year, too. We documented a number of performance assessment codes, and the other major product this year was the early site suitability. That helps focus the program. Where do we need to gather more data?

Certainly, these are both available. I hope you all have your own copies of these. They're available to the public, and this one, of course, evaluated DOE siting guidelines, came up with the 13 to 17 disqualifying conditions are not present, and probably additional data would not change that conclusion. Four of them are not likely to be present, but we need some more data.

As to the rest of that site suitability, we talked about the qualifying conditions, and at this time we believe 13 were present, and additional data is unlikely to change that, and 19 are likely to be present, but we need further information.

The bottom line of this report: How is it being used by DOE? Well, one, it's going to help us set priorities, and the ongoing activity of this, the test evaluation, integrated test evaluation activity results from this. But the bottom line is, it supports continuing site characterization. There's no reason to not continue site characterization based on a re-review of the data, and performance assessment was used in that, and it was peer reviewed, as you're aware.

Just to give you a little check, in '92, I think a year or so ago I talked to you about what we were going to do in '92, what was my 12 or 13 priorities. We had hoped to do the early site suitability evaluation report. We did do that. We did do this kind of work, and I showed you that on the view graphs. We did continue surface-based monitoring of 52 seismic stations, monitoring of rainfall, monitoring of groundwater.

We did begin and complete the Title II design for this one package that I'll talk to you about. Our quality assurance program remained in place with hundreds of audits; be it by the participants by Don Horton's oversight organization. As I pointed out, though, I think we really have a cooperative environment established between the quality assurance people and the professionals; scientists and engineers, so that we're doing what is necessary, and that's about it.

Maintained a sound environmental program. In this day and age, when you have a big project, you are burdened with RCRA and CIRCRA, and other Acts and other laws, be it cultural resources. We live in a fishbowl in this project, and we have to make sure we're complying with all those activities. As a matter of fact, right now Russ Dyer was telling me that one of our activities is bigger than most of the excavations you saw. It's 20 feet deep and 60 feet long and 40 feet wide, and it's digging up an old oil spill and hauling that soil off site, and that costs money, not only to haul it off site, but to dig it up. But that's a spill we created in '78 or the eighties, but we're responsible for cleaning it up as part of RCRA.

And we're implementing a sound cost schedule control system. It pleases me that the scientists and engineers know about what the costs are, what about the schedule, whether they're behind or ahead of schedule, are they meeting their goals, are they meeting their milestones.

On the other hand, we only had a very minimal waste package EBS near-field environment waste form characterization program. We didn't do much last year. You were able to see some results. Some of the results were done by EM's program the last couple days, but we didn't have much money there. We did maintain our roads, buildings, and record centers. We conducted an institutional and outreach program that we think appropriate for the activity going on, and we brought on the M&O and they transitioned to the major project activities, the roles of integration, the roles of project direction.

I'll talk to you about permits. As I pointed out, in order to make progress in the field, we needed the state's cooperation. We received permits fairly regularly throughout the year. The most recent one was additional groundwater appropriation. Although we had a nine-day hearing back here in January for our first water one, we received 300 per cent more water without any hearing. It was based on the previous hearing. So we hope this will continue and we're pleased with the professionalism of the state at this point.

John alluded to this, and he pointed out some of the things that are on my chart. Now, I don't know if he read my chart before I put it up here, or it's stuff we've been discussing all the time, but Congress is concerned about reducing overall program costs. They don't like the idea of spending so much money to study a site. So what actions have we completed to address this?

Well, we have completed an independent cost estimate of the work in place. You've got to know your baseline and what we've agreed to do and what that costs before you can change it. That's not "we," that's other elements of the Department. It was done by independent architect engineers within other organizations of the Department.

We undertook the Mission 2001. It was headed by Dale Foust's M&O team. It was to look at: what does it take, let's do a scrub of what it takes to do the work; and can we get it done by 2001? We wanted to validate the estimate. We wanted to make sure we could meet the schedule under our current funding, what we got last year and the year before.

We did do the early site suitability report. That helps us focus. Scientific tests are being prioritized, and we hope, through issue closure, to eliminate some tests from our suite of tests in the SCP. So that's some actions that are completed.

In the planned-what I'm trying to remind everybody, cost consciousness is everybody's responsibility, much like safety, and much like QA. We have to step up, we have to be professional, and say, "Is this necessary?" Just the things John pointed out. Not, "Is it nice to do" and "Would it really be great to do a paper on it," but is it necessary for program success.

We have established our 2001 baseline. We've had some recommended cuts to that. I'll talk to you about it. I've asked my technical project officers to meet with me here in October and see if we can brainstorm and can up with some other top down approach to this plan. Then we'll formalize our baseline with the '94 Passback, we'll probably incorporate some top down reduction, and as John pointed out, we'll start to develop a list of candidate activities, specifics for cost reduction. We want to lay out specific activities, get with our Regulator, and say, "Can we take these out of the plan?" And, really, that's one of the challenges that we have.

We have a mechanism for doing that; either the semiannual progress report, a topical report, issue closure activities. That's where we're heading with this particular activity. As I said, there is a mechanism.

Let me now step back a bit to put things in perspective. We've talked about the $6.3 billion baseline. It's approved by the Energy Systems Advisory Acquisition Board. It includes 20 years of activity, from '83 to 2002. It included total costs for site suitability and license application, if suitable. It had actual costs in it of about this much, $1.3 billion. It had some unescalated state payments to the counties. We anticipated there might be a benefits agreement, so we put in that estimate, about $800 million for oversight and benefits, and PETT payments, payment equal to taxes. We also put in that estimate escalation, which left about $3.6 billion of unescalated direct project work to be done.

So when you say $6.3 billion, well, that's one number. Here's the amount, unescalated, to do the work from now to 2001. These are very broad. They're only meant to be put in context for you, but site investigations, trenching, drilling, ESF testing, the scientific investigation, almost a billion; build an ESF and operate it. The testing's up here, the building's down here of an ESF, about a billion.

Waste package and repository design, you have to have designs to support license application; $500 million. The systems engineering, the technical data base, the performance assessment, we happened, in this category, to put in environmental and institutional support, $600 million; and then the project management, training, records management we put here. That's how that $3.6 billion comes up.

Now, we've got a well-detailed, according to our work breakdown structure in what we call our Mission 2001, and you'll see how that comes up a little bit later, but I'd like to point out about the independent ICE results.

As I said, they've issued their report just a month ago. It's very current. They said our approved baseline, which was in the $6.3 billion range, represents a reasonable value for the currently-planned work. It's an adequate baseline for the project. They looked at cost rates, they looked at labor rates, they looked at everything they could. They believe that license application in 2001 is achievable, providing adequate funding is received. They found no technical obstacles. They believe proposed staffing and capital equipment ramp-ups can be accomplished in the out years to meet that.

They also made a comment that we could be more success-oriented if taken off budget. Now, I have a little star--Steve Goldberg said off budget is not the right word; try "improve budgeting process." This happens to be the word they used, whether it's a revolving account, whatever the right word is, but they said, you're going to need something like that if you're going to assure availability of adequate funding. They said the transition to the M&O should be reviewed to make sure there's no duplication. We're sure trying to do that. The M&O is working with us on that. And they said we, the federal staff and the M&O, need to define the scope and requirements for this waste package and repository design, because we're going to be producing a lot of paper at a lot of money, and is that what's really necessary for a license application. That's just paper, license application, designs for waste package and repository. So that was their results.

This was how we spread our money in '92. That's how we spread it and, in effect, spent it. That's just there for your reference. That's how it was.

I'll talk about the Mission 2001, because this is our baseline plan. It's in the book. The numbers get small, but I wanted to put it all on one page. This line across the bottom is what we predicted our spending would be, $6.1 billion, in order to meet a license application in 2001. The line below that is what the ICE predicted our funding should be to get to a license application; fairly close on the number, up and down at different years, up and down in different categories, but overall, we spent a lot of time with them reviewing this estimate, but you can see right off the bat--and I'll tell you in a minute--that's not what I'm going to spend in '93. John has not allocated that much of the $375 million.

We still think we can meet 2001, even with what we have in '93, but we're going to take some big jumps in '94 if we want to do the current program under the current schedule. There's footnotes to that, but you can look at it.

Now, this is not a chart I use to manage anything by, but it's a chart I've discussed with you all, at your request, about how the funds are broken out, so it's not my detailed accounting system, but I want to put it up here to make some points.

If you recall, we discussed, in your definition, real work, per se, was called drilling or gathering data, and designing things. Everything else, performance assessment or whatever, we considered a foundation to do the other work.

Last year, we had 33 per cent above the line and 67 per cent in the foundation. This year, with this kind of a budget, $245 million, we're 56 per cent below the line. We reduced our percentage, and increased our percentage above the line, and the big swinger, of course, is exploratory shaft. So I think we're heading in the right direction. We did have an estimate that was almost 50-50, but that was of a $321 million approach.

So that's some broad categories. As I said, don't try to track this. It's not our WBS. It's just a broad picture for you all to show what we agree what we think is above the line in this breakdown, and what's below the line.

We also provided for you a detailed list of what was below the line and where was that on that chart, and this is in here for you. This is how we spread it last year, and let me tell you, the GAO comes in and they debate with me about what should be above the line and below the line. EEI comes in, everybody debates about this distribution. You can make any distribution you want. It all depends upon definitions, because we don't manage it this way. That's just a listing of those costs.

And in '93, that's a listing of the costs, which leaves about $107 million above the line to do scientific and technical activities. I say preliminary, because I have not finalized this spread to my division directors and my participants. We've had a lot of meetings. We've had three full days of back and forth on how much. Let me go ahead.

Although we're at 244.7, when I told everybody what the priorities are and what the target was, the first input of numbers I got was over 300 million from everybody, and that was not to do what they'd like to do, but what they thought was necessary for '93.

This is our work breakdown structure. It's in the book. As you well know, we have lots of accounts. We now have 15 elements across the top. We're going to emphasize ESF and site investigations next year. I just wanted to show you that's up there. Put up the other part of it; I'm sorry. That's the other 15 elements. You can read them. That's how we manage the work, and let me point out what that consists of:

Below that, for each participant, for everybody, we have 816 planning and scheduling accounts, summary accounts below that, that's eight participants, 44 minor participants, this many activities. They're all scheduled out. They're all resource-loaded for next year. They report against milestones in each of these accounts. I'll show you how that works.

We just happen to have Rick Spengler here, and it's one of his accounts, and this was last year's, but as you go down the account--go back to the big one first before I get to it. I'm just going to expand this one little box. That's geology, at the fourth level under site. As you expand that, geology has a person responsible for it. Some of these names have been changed. It goes down to different people; Raytheon doing some things, USGS doing some things, REECo doing some things.

In Rick's area at USGS, he then has some summary accounts, and in case you're wondering, inside the summary accounts there's ten activities, total up to $500,000; 14 activities here, seven activities here, seven activities here. So we manage, plan, and report at a fairly low level. That's why we can survive audits. That's why I can stand up honestly and tell everyone I know where the money's going, and we do this with national labs, we do this with the USGS, we do this with all the contractors. That's across the program.

Does it cost money to implement this kind of a cost-control system? Yes, it does. Is it worth it? Absolutely, in my mind.

Here's where we come down to the real '93 now. Now is where the fun kind of starts, because nobody likes these numbers. Everybody thinks they don't have enough to do what is necessary for next year. We have 245 million. Here's how I've spread it through the 15 work breakdown structure items. Vince Iori and the M&O team that supports him have worked hard with my division directors trying to get the best split. I still have some ongoing meetings, because people have said, "I can't do what's absolutely necessary," and so I still may have to adjust this a little bit. I hope not too much, but that's how we've spread it.

I'm going to go through each of these 15 items so you get an idea where we're going, but I need to do one other thing, because, as I said, we had three days of meetings. I asked everyone to come in and tell me what they could do. I gave them a bogey number, so to speak, a planning number, and I thought it'd come out around 250 and I could handle that, and it came out around 300. And some of you know, I have an avocation that involves sports officiating. Well, it's pretty chaotic during a football game sometimes, and it was pretty chaotic during these budget meetings, and I'm not sure if Russ Dyer, Larry Hayes, Ardyth Simmons, if anybody's happy with the kind of money they get.

Ace, you know, is going to run some tours, but he said, "I can't do all the tours that you want me to do." The schools are calling up. They want to come on tours, and I may not have enough money to take the kids out to the site, so it's a challenge, splitting out this money, because that's not enough to do what we think is necessary, but we'll do what we can do this year and put the rest off to next year.

Let me tell you how we're going to use that money, and I'm going to go through each WBS and talk about what we're going to do for the money. I tried to limit most of them just to one page. There is backup detail for you in the book if you want. We're going to have reams of data. Our PACS system is about this thick. I think Russ McFarland asked for some of it for estimating. I think he can attribute it to you. We went through in some detail with OMB on Monday about the system.

But we're going to ESF site preparation, and we're going to construct the first 200 feet of the north portal and ramp. That's our goal. I'll show you a schedule for that in a second. We're going to continue the rest of the design. We have to design some more of the ESF, the next stop, the next part of the ramp. We're fast-tracking. There's no doubt about it. We don't have an ESF design complete. We're designing it in segments because of our funding restraints. We've got to prepare facilities for ESF testing. The first 200 feet is going to have an alcove in it for testing.

We need to award a subcontract for underground construction. We wanted to find the world's best underground constructor. We went out for bids, we got proposals. There were some modifications that needed to be made to those proposals, so we didn't want to lose the bid process, that's the bottom line. So we had to go out for proposals again. It probably won't be until January until we award this underground construction. That doesn't affect the first 200 feet, because REECo's going to do that with force account by drill and blast. This is for the 14 miles or so of tunnels.

We hope to issue the RFP, receive proposals, and award a contract for first large TBM and support equipment. We want to do that next year. We need to upgrade our power so a TBM can operate, and I'm going to show you some pictures and some schedules about the ESF. I'm going to concentrate on the ESF this afternoon. It's in the book, but here's our milestones:

Issue the TBM RFP, start site prep. November 30th is our date. That's the date we've been planning on for about two years. We're going to make it, it looks like. We're going to receive TBM proposals, award a TBM contract, and deliver TBM. That's one line.

But here is our sequence for doing the work at the portal; prepare for access and drainage, construct the north portal and a slot. We're going to start with a slot. We're going to then construct rock storage and pads simultaneously with that, in parallel, construct the first 50 feet of starter tunnel, cut and cover the tunnel entry, and then do the 200 feet of starter tunnel.

I caution you it is preliminary. I have not given this to John, other than the November 30th date as a milestone, an absolute milestone. We're still working out the details. We don't know if we can make it. It involves around-the-clock operation out here in this area, three shifts, but I think that's the efficient way to operate anyway. So by the time we get this done, we should have a TBM on the way.

Someone may wonder, well, why will you finish this 200 feet and not be there? Some of the scientists and engineers say we may have to drill and blast more than 200 feet, it may be advantageous in the first part of the excavation because of the hardness of the rock. We won't know.

I hope when you come out a year from now, that's what you'll see; 200 feet into the mountain right here, a very austere drill pad. This is the only thing we're going to have here is probably some trailers and this is a temporary power area; very austere so we can get started. That's our goal, is to get started.

To put it in perspective, of course, put it on this map of our north ramp, we're right up here, and we'll be in about this far. That's the start of the ramp. Eventually, depending how fast the TBM's move--and that depends upon the power of them, and we don't know if we're going to get a new one or a used, modified one, that's what we'll go out to look at--whether you move 50 feet, 100 feet a day, that's about 6,000 feet of ramp, so you can come to your own calculations as to how long it will take us to get to the bottom. That will provide a great opportunity for the scientists to look at the faults and to examine the strata in the area.

That's what we're talking about, for those of you who aren't aware of what the TBM's are, about that size.

Here's a plan view. Being a civil engineer, I have to throw some of these kind of things in. Here is the pad. That's the little electrical building you saw. Here's our slot for starting it. Many of you have been to Exile Hill before. Trench 14 is right here. Jerry Szymanski's theory is represented in that trench, so you know where this is. This is Midway Valley. We'll have, of course, some--this is the existing road. We'll have access roads in here. We'll have a water line, water tank storage, and we'll start our muck pile.

Now, believe it or not in this desert, we're going to have to put a membrane under that muck pile, and that membrane's going to cost us $2 million. That's the EPA requirements. That's the way you've got to do business, because we can't prove, and we're not going to sample every piece of rock we get out of here, that we won't have contaminants in that rock; oil, whatever. So that's how we view the regulations, and how most people do.

I'll pass it on that I was over at Sellafield in Europe, and on their drill pad, they had to put a membrane under their entire drill pad. So it's not just United States regulations, it's the environmental regulations that many countries in this world are going to. So it's not unique to our environmental activities.

Here's some more engineering drawings. This is a section of the entry into Exile Hill, the first part being a concrete portal face and a multi-plate steel arch, and then we'd start our starter tunnel in here. That's just to show you what it looks like in section, and this design is still being finalized. As some of you who did participate in the design review, this is in plan, of course, and this is the steel arch over the top, and that's the slot and then we start in. Then, of course, there's our first test alcove. So we start testing not only mapping as we go in, but we'll start testing as soon as we get in there.

That's ESF. That's our focus next year. I'm not going to go into as much detail on some of these other things, but let's talk about what other things are we going to do, because when we say ESF, many other elements of the work breakdown structure support ESF.

Certainly, we're going to do UZ-16 and complete that. We're not going to work around the clock right now, to the best of my knowledge--although that's the most efficient way to use a big drill rig, but we don't have money to do that. We are going to complete these boreholes along the ramp alignment so we can design the rest of the ramp and start at the south ramp and do a borehole there. It provides design data for the engineers. Although it's in this budget, it's really ESF work, too. It just happens to be the way we account.

We are going to complete our drilling and data collection in support of Alan Flint's unsaturated infiltration studies. There's only seven more holes to go there. We need to revise any study plans and job packages for the tests in the ESF. When we get in there, we want to be able to do the tests. The only reason we're building it is to do the tests, so we have to do that. We have to complete the trenching program in Midway Valley, and most of the trenching program for Quaternary faults. John has given me a priority to try to close the seismic issue a little bit, so we're going to be focusing on seismic activities.

We going to continue the collection of data that would otherwise be lost. I think you're aware, the seismic network, as of the first of the year now, is being run by the

University of Nevada, Jim Brune. In fact, he's speaking next week at the ACNW activity. He's running that for us. We're going to do some pump tests in a C-well complex, and when we finish UZ-16, we'll be moving on to UZ-14.

Now, for 50 million, that's just a few bullets. There's a lot more, and we can go on and on. In fact, Russ Dyer gave me a package that thick when we went over it in our budget presentations, but I'm just highlighting some of the things for you.

In 1.2.5--you notice I didn't go numerically, 1, 2, 3. I went to kind of what I think is some of the important things first; ESF, site, and now the regulatory activity, 23 million. In this category, we have, of course, monthly interactions. We have to prepare and issue documents. We're committed to provide comment responses to the early site suitability. We have been working on the 191 reviews. I don't know if we'll be doing some more work with the National Academy on the new approach. Who knows if we'll support them or not? If they ask, we will.

Two semi-annual progress reports for the site characterization program, that's a legislative requirement. That's required by law. We're probably going to do one revision to the annotated outline next year, and we'd like to close some issues. One is erosion, the other is seismic hazard, and work on volcanism--which isn't in here--too.

We're also going to revise our regulatory compliance plan. We now, at Yucca Mountain, have clear responsibility for three things. As the Associate Director for Geologic Disposal, I'm responsible for the license application, for the EIS that goes with that, and for the site suitability determination. John has provided me that direction. I'm developing some strategies with my staff for that. But now, if someone asks, "Who's responsible for the license application in OCRWM?", only one guy's going to raise his hand, and that's going to be my organization. Other people will review, provide regulatory oversight, but it's clearly our responsibility.

We have to add these initiatives to that plan, and we're going to revise some study plans, as needed. Why do we have study plans in both categories, you ask? The writing of the study plan is done by the scientists under 1.2.3; the review and regulatory pushing forward of them is done under 1.2.5. That's the way we account for it.

We have a lot of technical data to manage; parameter dictionaries, revise our technical data book. If you don't have our technical dictionaries, let us know. We put out our data report. We'd be glad to share them with you, but that's a big package in itself. That includes the GIS, geographic information system, and performance assessment happens to fall in this category; I think, appropriately so. It's a regulatory base requirement, but it also helps us set priorities.

We will prepare for our second total systems performance. We'll use it for thermal loading analysis, support surface-based testing, next generation EBS model, and whatever study plans are needed to support site characterization activity, whatever PA is needed for them.

Now we move on to 1.2.1, systems, about $6 million. We need requirements documents for traceability so we know we're meeting regulatory requirements and other environmental requirements, as well as project management requirements. This, in essence, continues our document hierarchy work, it develops and issues some mined geologic disposal system documents. It is our change board activity.

We have set documents. We've been doing a lot of work. Win Wilson, out at the site, has, I think, the last count was he had 57 change board actions. Even for the limited amount of work we do. We can change. It's not a problem. You just have to document it, that's all.

This also conducts conformance reviews that are designed, will meet the new requirements. I think you remember an initiative that was established by John when he first came in, called "Management Systems Improvement Strategy." That has developed a new suite of documents, performance-oriented documents, and we're working on an old suite. We don't think there's any holes in it, but we're going to have to make that conformation. We're pretty sure there isn't, but we're going to have to make that cross-check.

Develop specialty engineering plan, value engineering cost savings activities go on here, special studies, tradeoff analyses between ESF, repository engineered barriers, design activities, what helps, what hurts, and we support what we call a total system life cycle cost analysis.

Waste package. You know, we all talked the last day and a half about this category. Well, there's 8.3 million in the category for next year, but I asked Dave Stahl to summarize all this activity that went on in the last day and a half. What was the cost of that, and what would be the cost next year; about one and a half to two million dollars, in a quick thing. So what you saw was only a very small part of this activity. It was this, and probably that is what you mostly saw.

But next year, what are we going to do for 8.3? Well, we're going to start our advanced conceptual design, do some calculations, develop concepts, conduct some thermal loading options, start some large block tests. I'm convinced at this time that that's important. We're not going to have enough money to go into Busted Butte with a testing facility, but the scientists tell me we can get some information from these large block tests.

We'll develop some plans for testing in the ESF, because we're going to get down there maybe a little sooner now with one TBM. We continue long-term testing of spent fuel, survey and testing of metal barriers, and continue the release modeling, of course, and that was going on the last two days, too, so that's kind of the modeling that was going on.

Repository. We have an element called repository. We're not building a repository, but the ESF has to be integrated, by regulation, into the repository. Some of the analysis for the ESF, because a repository will be hot, we have to make sure our openings at the ESF are compatible to repository design, so some of that analysis is being done in this category.

We are going to start our advanced conceptual design. We need to get that started. We'll do our basis for design documents. We'll continue some laboratory and rock mechanics tests, complete our drawings of a proposed repository, because in order to show that an ESF is compatible with a repository, you have to have some repository drawings, some repository design.

We'll talk about waste emplacement equipment, just so we can do appropriate tradeoff studies. People are continually interested in our borehole sealing requirements.

It happens to come under here, and we'll investigate grouts for borehole sealing.

Test facilities. This is kind of a misnomer, but it's really facilities that support tests. Win Wilson supports the people out in the field, be it with bulldozers or drills or whatever. We also have a lot of tours and outreach activity out there now. We're going to develop a conceptual design for the area. As we start to increase our activities, we need warehouses, we need all different things for build-up. We need fire protection in our own site office. We don't have appropriate fire protection to the codes out there now. We have a waiver, because it's not a personal hazard, but it is a property hazard.

Construct hazardous materials storage area, as part of RCRA, we're now going to be using oils and grease and everything. We have to document exactly how we use them and what we do with them, not due to NRC, this is due to RCRA-type things, and we begin an area complex near J13, where we're probably going to have some more office buildings for the people close to do their work, for medical facilities, what we need.

And in order to supply that, we're taking some facilities from Tonopah. That's the Area 51, the Stealth bomber. That's been shut down. They have some great mobile offices, some great mobile facilities; only been up there a relatively short time, and we're bringing those down and we'll put them in place probably at this complex when the time comes.

Program management is this category. It includes several things; writing procedures, not change board impact, but change board procedures are done here, compliance reviews for procurement. The PACS system is run out of this, software so our division directors--they now get a printout on paper monthly to tell how their activities are doing. We want to work software so they can come right up on their computer to see how they're doing, so that can do it even without paper.

Financial assistance. This is money directed to the State of Nevada for oversight, to the affected counties, cooperative agreements with universities, and payments equal to taxes. A little bit of debate, this may be 4.2 instead of 3.7, depending how you read the law and, John, you're shaking your head. The new reading is whatever--4.6, or whatever it is. Now I've lost 600,000 already out of the rest of my project amount. And payments equal to taxes, our estimate of what that might be.

Just so you understand how some of the systems work, I think Nye County's first estimate for taxes was in over the 100 million range, so we're a little away from reaching an equitable payment for taxes with Nye County.

Quality assurance program. As I said, I can't be more pleased. I'm just really pleased with Don Horton and his approach to quality assurance and how he's worked, providing a sound quality assurance program. Certainly, we want to support ESF. We think we're trying to streamline the program all the time. People like Larry Hayes and some of the scientists worked on what we call a QIG, quality integration group, and they really were able to get together a meeting of the minds, and I think that's helped the program immensely.

I've asked Larry to pick up another duty along those lines, since he did so well in that. We're looking at, do we have to look at all these requirements that are out there, all the environmental requirements, because Larry says, "I need money for drilling, and why do we have to do this and this and this?" So I've asked him to head up a task force to look at that, to make sure we're doing only what's necessary, and not what everybody would like to do, and in that way, he may understand what is necessary, too, because the people out there are saying, "Why do we have to do it?" Well, some of that comes with understanding.

QA, I think, is running very well right now. Information management, we're developing a lot of data. We're, under Barbara Cerney's headquarter's guidance, we're implementing lots of things out here in the field, document control, record center, software development, operate the computer center, operate our VAX cluster, start what we call InfoSTREAMS. That's the implementation that eventually might lead to the licensing support system, and support other record management activities; record inventory and disposition schedule. We're trying to figure out what records do we need to keep and which ones don't we.

Environment, safety and health, Wendy Dixon's program, it's a comprehensive program all required by law. Before we drill anything, we go out and do a pre-activity survey, make sure we don't have any archeological problems, any environmental problems. That's all part of the activity.

Continue our monitoring. We monitor for air, meteorological monitoring, water resources, terrestrial. You'll hear Wendy talk about some of her program tomorrow; do the permitting activities. I just showed major permits. We're continually getting small permits. It's just part of doing business within the state; actually, out of the state, too, for work we do in California.

Continue environmental audits and surveillance, implement this. This is expensive; hazardous materials control.

We've got some new programs. We have a DOE Radiation Control Manual. Right now I'm told that's going to cost me a million dollars a year to implement this manual.

The Secretary has not given any exceptions to that. That requires training for everybody. Why? We don't have any radiation. Well, we're on the Nevada test site where there were test programs on the nuclear rocket before, where there is possibly--some fallout, so our orders, we're going to have to get trained and implement a new program here.

We have been working with the Native Americans, 16 tribes. We do socioeconomic and regional studies as part of the site suitability requirements and, eventually, NEPA; continue compliance with health and safety, maintain our health and safety and establish protocols for functional appraisals as part of the Secretary's self-assessment initiatives.

Institutional. Not much, $3.5 million, but that supports interactions with the State of Nevada, public interest groups, business community, operation of the three info offices, speakers, bureaus, tours, exhibits, educational programs--we're getting lots of requests from the schools right now--Yucca Mountain media relations, and various publications. There's some new publications out there right now you're welcome to.

Planned accomplishments for support services, this just shows you where this money goes. It's rent, it's motor pool, it's telecommunications, it's graphics, it's clerical support, and then, in that category, also includes training.

We do an extensive amount of training required by several regulations.

Before I go on to challenges, I want to point out, what does this mean? What does this mean to Nevada, the new budget? It means we're probably going to have 200 more people working out at the ESF area, 200 construction jobs. People will be out there working, 200 more than are out there drilling now, and trenching, so that's what it means.

Let me just briefly go over the challenges and issues. The first one John alluded to: modify the program as appropriate to be consistent with the new energy legislation. We're going to have to do that. We're not going to do that quite this year because we don't know what that's going to be, but we're going to have to think about that as we do it. We do have adamant state opposition, intense media attention. That's just the way of doing business on this project. It is fairly unpopular in the state. People want to know what's going on.

Complex science and the 10,000-year question, maybe some of that might be addressed by the new legislation, but the people have a hard time understanding 10,000-year questions.

Adequate funding. Right now, simply, we are funding limited, although we have, on the project 60 more million dollars this year to spend, and John did point out, we got 100 million from the Congress, but John last year provided me 20 million--30 million, I guess it was--from carryover, so, really, I spent 180 million last year. I'm going to spend 245 this year. Without the 100 million, my budget was 150 million, so we were looking at bad times until Congress came through with the extra 100 million. We hope we can get out of this constant hassle on funding by some kind of revolving account.

I just keep this up there as a reminder. We've overcome the QA record keeping and procedures. I think there will also be outliers, people who don't quite like it, but we've really overcome that. We've overcome this, I think, now, too. We move from planning to execution. TRW is on board, the M&O team's full bore doing the integration and the major contribution activities that have transitioned to them.

We are going to look at issue resolution. We're going to try to focus the program. You know, it's not only enough to work hard, which I think everybody in the project's doing, but are we working hard on the right things? And this is the convergent activity that tries to determine, are we working hard on the rights things, and are we going to converge and get a site suitability determination, and eventually, the site recommendation, license application, environmental impact statement, all in accordance with the broad suite of laws, using site characterization data and performance assessment to iterate the information for not only regulatory compliance, but design, and redirection of the site characterization program, or establishing additional needs. So it's an iterative process. That's what it's all about. We're trying to describe that now in what we call a convergence plan.

My last slide, I think. We still need help to continue moving on; litigation or legislation to assure permits. Litigation has been successful, but I keep it up there as a reminder, should the state change their position at any time. We need departmental and Congressional support for funding. Permits without funding is not enough. Funding without permits won't get us there, and we have to assure regulatory compliance is reasonable, with a cost effective basis in accordance with the new Energy Bill.

That's some of the things that we're going to need help doing. Without all of the above, this program could become stalled. If this program becomes stalled, we won't be addressing environmental issues, what to do with the 65 locations where there's hazardous spent fuel, and as part of the national energy strategy, many people believe progress towards waste disposal will keep the nuclear option open.

With that, I'll take any questions you might have for as long as you like.

DR. CANTLON: Questions from the Board?

Let me start off with an easy one, Carl. Where you were looking at the Congressional examination of your budget early on, you talked about the scientific test work, and your slide said "have been prioritized," and I notice you changed the language orally to say, "are being."

MR. GERTZ: I guess Jean maybe talked a little bit when I was out of the room earlier, but we have an integrated test prioritization activity going on. Some are being prioritized. We know UZ-14 is the next hole that most of the scientists want to get at. They think that's a priority, so...

DR. CANTLON: Could you give us a kind of a ball park guess at where you think you are on that; half-through, quarter-through, two-thirds?

MR. GERTZ: Jean's been working that. I'll let Jean answer that. I'm interested in the answer myself, too.

DR. YOUNKER: This is Jean Younker from the M&O.

We've provided a draft report to Russ Dyer, Carl's Division Director, who's responsible for getting the priorities sorted out in the testing program, and he used our draft input as the basis for trying to put this together, put the FY93 funding together, working with the managers from USGS and the national labs, so it's already being used. The final report has not yet been provided, but...

MR. GERTZ: But the fact is that the data's being used, and that's what Russ presented to me in his budget presentation.

DR. CANTLON: I take it from that, since the figure was prepared from that, you're suggesting that the work has been done?

DR. YOUNKER: Yeah, this is Jean Younker again.

To the extent that they could, I think, given the kinds of constraints that DOE has to use whenever they make budgetary allocations, I think they've used it to the best of their ability this year to try to put the test priorities in place that we recommended.

MR. GERTZ: We do have a document out. In fact, John was given a presentation by video conference, in an effort to save money on the integrated test prioritization activity.

DR. CANTLON: Other questions from the Board?

DR. DOMENICO: Domenico.

You said UZ-16, is that one of your priorities that you--

MR. GERTZ: That's the one we're drilling on right now. We're 800 feet deep on it. We're going to go to 1600 feet deep with that, then we go to UZ-14.

DR. DOMENICO: I see. I noticed the handout on it. You started in May.

MR. GERTZ: That's correct.

DR. DOMENICO: My calculation says you're getting seven feet a day. Are you having trouble with that rig?

MR. GERTZ: We're not having as much trouble with the rig, in that what we're having trouble with, we're not doing three shifts. We probably should be working 24 hours on it.

DR. DOMENICO: You're not doing three?

MR. GERTZ: No, sir.

DR. DOMENICO: How many days?

MR. GERTZ: One shift, five days a week is all we're doing, and you know the time it takes to get ready, and the time to shut down. We did have a transmission that went out that we had to repair on the drill rig, and we lost some other activity there.

DR. DOMENICO: It's still low. It's still less than ten feet a day.

MR. GERTZ: You can talk to Uel tomorrow. When it's working right and everything, we're getting, I think, 20 to 30 feet a day on one shift.

DR. DOMENICO: Will we see it tomorrow?

MR. GERTZ: Yeah, you'll see it tomorrow and you can talk to the people that are doing the work tomorrow. They'll be out there. That concerns me, too. I'd like to be doing more.

DR. CORDING: Carl, last year, I think the plan was to add more LM-300's, but at present, you're going to stay just with the one; is that correct?

MR. GERTZ: Yeah, that's correct. We had a budget amendment in that didn't get in for an extra 75 million. That would have ordered two more LM-300's. It would have provided for around-the-clock operation which, as a project manager, I think is the most cost-effective way to do it, but that didn't get in and we think that now it's time to--we've listened to you--it's time to get underground with the ESF, and that's what we're really focusing on, and just continuing the ongoing program, but not expanding it.

DR. CORDING: And using that on a one shift per day--

MR. GERTZ: One shift per day, that's what our budget is.

DR. CORDING: --operation at present for the LM-300?

MR. GERTZ: That's correct. I'd like to do it more. As I said--

DR. CORDING: Are you looking into the possibility, then, of changing the scope of the dry drilling program to perhaps do more sampling; take samples, for example, of zones, rather than all the way through?

MR. GERTZ: Yeah. Larry Hayes is eager to jump up right now, but, yes. In my effort to try to reduce costs, I've asked the TPO's to look at things like that. Do we need full core from every one of the holes, or can we get intermittent core? It would save us a lot of time, and Larry's provided some thoughts on that. Larry, if you want to even mention it right now, it'd be fine.

MR. HAYES: Larry Hayes, USGS.

Yeah, we're all worried about what we're seeing as very low drilling rates. I think we're all coming to the realization, with funding limitations to get additional rigs, with funding limitations to put on three crews, drill around the clock, we're not going to get the drilling done that has to be done with this one rig.

We have had some preliminary meetings, the scientists, some of the technical managers, to look at what we can do in our drilling program to get the information we need most in a timely manner. We're looking, at this point in time, of dropping some holes, reducing the amount of core we would take. For example, we may core a few index wells, then we'll run geophysical logs on those index wells, and then we'll core other wells only in selected areas.

One of the big delays in drilling is the dry drilling. We're also looking at, where appropriate, where we can, we're going to go back to conventional drilling methods. We've got a meeting here at the end of this month to finalize these plans, and I think what we're going to end up doing is reducing considerably some of the drilling requirements, not only holes, but how we're doing it, so we can move ahead more quickly with less money, and you'll hear more about that tomorrow from Uel Clanton and myself.

DR. CORDING: Some of that, Larry, you're talking also about what--I assume some of that will be going to the underground program in terms of the type of sampling or testing that's being done, that could have been done in dry drilling, that can now be transferred to the underground; is that also a part of your effort?

MR. HAYES: That's correct. That's a very good point. What we're looking at is, okay, what are we going to learn underground that we can now minimize or eliminate some surface-based drilling requirements.

MR. GERTZ: Ed, you're alluding to a very good point, and I'll make it if you don't, is our drilling program was predicated on an ESF that only had 6,000 feet of exploration, or something like that. Now we have 14 miles of exploration.

DR. CORDING: Of underground.

MR. GERTZ: Can we change--underground exploration. Can we change the drilling program? Are we going to get some data from that?

DR. CORDING: Yeah. I think there's very many opportunities that will be present underground, and I think that's a very valuable and very important thing to consider.

MR. HAYES: Yeah. We're working on that with the Los Alamos people, who are the underground coordinators, and we think we see some really good tradeoffs there.

DR. CANTLON: Other questions from the Board?

DR. LANGMUIR: Carl; Langmuir.

Looking at your budget details table, perhaps you can remember the figures. In essence, I wanted to know, does site mean the surface-based testing?

MR. GERTZ: Essentially, yeah. If you look under the work breakdown structure, site's geology, hydrology, it also means the laboratory testing and geochemistry. Los Alamos work is under site.

DR. LANGMUIR: Okay. So basically, that's the surface-based testing aspect of things and its support?

MR. GERTZ: Yeah. It includes the drilling, too; the work breakdown structure. This would be the one--we go down to much other levels, but in site, we have coordination and planning, geology, hydrology, geochemistry, drilling, climatology, resource potential, deferred site close-out, and special studies. So that's those programs.

DR. CANTLON: Other questions from the Board?

(No audible response.)

DR. CANTLON: I have one, Carl. On the 1.2.4, the repository item, you had a four and a half million dollar there, and one of the sub-items was "initiation of conceptual drawings for waste emplacement equipment." What's the waste emplacement configuration you're thinking about? Are you still looking at vertical boreholes?

MR. GERTZ: Right now, the reference case is vertical borehole, but if we're going to develop indrift emplacement, we need to figure out what kind of equipment we might need to make that retrievable for 50 years, and how would that affect the size of the drifts, and do we assure that we don't oversize or undersize our ESF and make it compatible with future repository designs.

DR. CANTLON: So the conceptual design is looking at the alternative emplacement?

MR. GERTZ: That's correct. We have pretty good designs already in our current conceptual designs if you've seen that for the equipment, for borehole emplacement, but that is six years old.

DR. CORDING: Carl, could you give a little--just briefly, a breakdown on some of the--where the 49 million is going on the exploratory studies facility?

MR. GERTZ: Sure. Maybe.

DR. CORDING: Section 1.2.6.

MR. GERTZ: Now, is Ted here? Who's here to help me; anybody? Bill's here. Bill, do you recall?

We'll get you a detailed one, because we have the cost estimates, but maybe I can--I'm really speaking off the top of my head now, but I think site prep and construction and everything is in the $15-18 million category in the first bullet, but we'll get you the details so I'll just give you a thought process for talking now.

I think this is somewhere in the $5-10 million range. If we're going to award this contract, I think I have to set aside about $10 million here to make a proper award.

DR. CORDING: And that's the contract for the machine, for the TBM?

MR. GERTZ: This is for the TBM itself.

DR. CORDING: And not the contractor, but the TBM?

MR. GERTZ: Not the contractor. That's for the contractor. I don't have to set much aside for that, because that's labor--I mean, that's consulting-type work right now. This is for the machine. This, also, is in the $5 million range, off the top of my head, and that's some of the big--

DR. CORDING: Is that a permanent power supply, or is that generators?

MR. GERTZ: It's not generators. It's upgrading the current NTS so that we can use it for at least the first TBM. Our concept now, of course, is one TBM. When we get to Calico Hills, we'll decide if we're going to use another one at that time or not, and maybe it'll be one--as you all suggested some time ago--one that goes down and makes the U, comes all the way around and comes out, and that may be our very first shot, depending on funding. We'd like to buy one in '94 to do the Calico Hills, but we'll get you that detail, and we have those details. It was given to me in excruciating detail.

DR. CANTLON: Other questions?

DR. LANGMUIR: Carl, realistically, looking at your budget, you show, in effect, a jump of 100 million each in surface-based testing and exploratory studies facilities budgets, more than you have this year, more than the 100 million you just got. I wonder what you plan to do if you don't get it. What's going to happen? What are the contingencies?

MR. GERTZ: If you don't get it, we'll probably continue a one-TBM approach and extend the schedule. I mean, if you've got a limited scope of work, you either need the funds to do it in that time, or extend the time.

DR. LANGMUIR: Are we at the end of saying that we're going to make it by the deadlines?

MR. GERTZ: I am at the end of saying I'm going to make it right now. I think I can still say I'm going to make it with the funding that's provided to me in '93, a little riskier than I could say a year ago. I'd rather have 318 million than 244, but I still think we can make it based on the Mission 2001 study in our talk.

But if we don't come up with the funds in the out years, maybe we can't even spend all that money in the Mission 2001. Maybe we can't spend all that in the out years, but we'll have a commitment and it'll be there, and the momentum will carry on. There is some question of whether we can spend that. Well, you can buy equipment, you can buy two or three TBM's, you can work around the clock in many areas, and you can spend money pretty quick because we have that foundation in place that we use in our foundation chart.

DR. CANTLON: Okay. Perhaps we can take one more question. Any questions from the staff?

(No audible response.)

DR. CANTLON: Any from the audience?

MR. GERTZ: Don, I just wanted to make sure we were on the right track, and you were talking about the expansion in numbers from here to here in ESF and from here to here in site; is that right?

DR. LANGMUIR: Yes.

MR. GERTZ: Okay, and here's the expansion we're looking at. We don't have 321, we have 244. Probably I'll have to spread some of that out. It might not be 685--now I'm just talking off the top of my head as the project manager--but it's in the 600-range that we're going to need to keep on this schedule. If you don't get it, things just go to the right, because we've pushed the critical path. Almost everything's on the critical path. Now surface-based testing is, now ESF testing is, now design of the waste package and repository is, performance assessment, calculations, getting the data to them in time. We've really pushed everything onto a critical path, almost. That's kind of a non sequitur, but...

DR. CANTLON: All right. We'll take one more question. Ed?

DR. CORDING: Just one question, Carl. You had indicated there that you're looking through costs for the remaining, something on the order of $3.6 billion program, and the test programs, and you were also indicating, I believe, that you were looking at costs of management; is that correct?

MR. GERTZ: Oh, yes.

DR. CORDING: All the way up and down through the system?

MR. GERTZ: Up and down through the whole thing, and that's one of Larry's task force that I'm setting up for, because he's saying, "Gee whiz, it costs us a lot to drill these days," and he's right, it does cost a lot to drill because he has to have an archeological survey before he starts, an environmental survey. We have to take care of the grease and the oil and document where we're going with that and who's bringing it on site, and where we're going to store it when we're finished with it, and all that adds to the cost of doing business.

As I said, it's not unique to this country. It's not unique to this program. The oil people I've been talking to, they just put out a report saying they have to shut down about half their exploratory wells because of the cost of doing business and the current environmental regulations. But we're going to look at them. Just because they're there, John makes the good point, we don't want to overkill the requirements. There's a requirement, but do you have to do twice that requirement, or just the requirement? And certainly, there's a tendency of some people who want to be just a little safer or a little surer. But we want to be safe. We want to do all the requirement says, but we don't want to do more because then we're taking away from other things.

DR. CANTLON: Fine. Thank you, Carl.

Well, this brings our day and a half session to a close, and on behalf of the Board, I'd like to thank all of the speakers who presented papers, our consultants who have come in, the audience, who have participated in many helpful ways on a number of these suggestions, and so, with that, declare this session of the Board adjourned.

Thank you very much.

(Whereupon, at 4:05 p.m., the meeting was adjourned.)