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NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON PERFORMANCE ASSESSMENT
TSPA-VA

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I N D E X

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P R O C E E D I N G S

(8:00 a.m.)

BULLEN: Welcome to the continuation of the Performance Assessment Panel Meeting of the Nuclear Waste Technical Review Board.

We're going to start this morning with public comment. As I understand it from Linda Hiatt in the back of the room, there's only one person who had signed up. That person is Dr. David Shoesmith from the Mine Geologic Disposal Program of ACL. So, I would like to recognize Dr. Shoesmith and welcome his comments.

SHOESMITH: Please, take your seats and put them in the chairs provided. Good morning. I had a few questions and a few comments to ask about the waste package modeling and about the cladding model.

First of all, a question. Is there now a performance function for the carbon steel within this model or is it irrelevant in determining radioactive release rates?

ANDREWS: It delays, of course, the initiation of corrosion of the C-22, but as you saw on those curves up there, that delay is a few thousands of years, you know, from 2,000 years to 8,000 or 9,000 years, and then the C-22 starts degrading. So, the performance benefit is only that. Only; 2,000 to 9,000 is still some significant performance benefit.

ANDREWS: Yeah, it doesn't seem to me that that's any benefit, whatsoever, unless you can show that it affects the early radionuclide release rates, things like technetium and iodine. It didn't strike me that there was any sensitivity in the model to carbon steel.

I had a couple of reservations about the weights in the model, as well. You're still using the pitting factor and I think you're getting relatively short lifetimes to carbon steel because of that. That will give you, effectively, a linear dependence for pit growth rate and that's not what's normally observed. Since you haven't yet got any influence of microbial corrosion in there, if you are forced to put that in, then the carbon steel is not going to work like a barrier, at all. So, just a caution on the use of that pitting factor.

I had a question on the C-22. The presentation showed all the variability in the general corrosion rates, and yet the thing that's going to kill the C-22 is whether under seepage conditions you get a localized corrosion failure. There was nothing presented yesterday that told me how important the localized corrosion failures are for the C-22. I assume that there is a model in there, a pit or crevice growth model, but there was nothing there that told me how important it was. Do you have any sense of how many containers or waste packages suffer from localized corrosion and how they affect the early release rates? Is that a sensitivity analysis on how it's being done?

ANDREWS: Yeah, in fact, the initial failures of the package--failure being defined by any penetration that goes all the way through the package--those initial ones are through pitting, through crevice corrosion.

SHOESMITH: Yeah, but I wasn't clear from the presentation yesterday that that was the case.

ANDREWS: Well, you have to look on the one plot that--you can't see it on a linear scale; you have to look at the log scale, the log scale I presented. And, the first ones are occurring at, I don't know what it was, 8,000 or 9,000 years from pitting and no general corrosion is going all the way through until some 15,000 or 20,000 years from the model. So then, general corrosion, in fact, is greater, at least from the results that we have and from the stochastic modeling that we've done and for the weights that were applied on the different chemistries that Jerry talked about, the general corrosion, in fact, dominates the pitting corrosion.

SHOESMITH: I can see that it dominates in terms of the number of packages that fail that way, but those few that fail by localized corrosion must dominate the technetium and iodine releases at some times.

ANDREWS: Yeah, at early times; that's correct.

SHOESMITH: So, it's critical to know how many and how sensitive that is because any expert elicitation, there's a very large distribution in rates that were provided by the people on that Panel. What's concerning me is are those rates in there and is that the measure of uncertainty because within those differences, there are actually conceptual model differences? There's still a large amount of uncertainty not only about the rates that should be put in, but how the process is going that is dictating those rates. So, I think, but I'm not certain, that convoluted into your uncertainty is a conceptual uncertainty in the model. I think that's a dangerous thing to have in this prediction. I'm not quite sure how to get it out. It's just an indecisive conclusion that came from the expert process.

ANDREWS: No, I agree with you. I mean, when we elicit some things, some things are parametric and you can go measure, and some of them, as you point out correctly, are conceptual. In this particular case, probably those uncertainties are being combined and it's difficult to parse them back out again. As you know, we elicited this and had a continual elicitation on this to try to parse that out, and it was even difficult for the experts to parse out. So, in fact, they were combined as one global distribution of uncertainty; some of it conceptual, some of it parametric, and even some of it environment because the environment is uncertain. So, all three of those uncertainties are buried in that CDF or PDF of degradation rate; or corrosion rate, essentially. And, you're right, you can't parse them out and always say, okay, that process and those rates have a significant impact on performance; which, I think, both Mike showed and Jerry showed, whether you're talking about total system performance and therefore it points the finger at additional work as required to (a) breakout that difference in uncertainty, and (b) collect additional information, analog information, laboratory information, to help, you know, narrow that uncertainly band, or maybe it still exists.

SHOESMITH: Well, it does--it will exist until somebody does some experiments.

ANDREWS: Yeah.

SHOESMITH: But, just to reiterate my point about the localized corrosion failures, I don't think they should be lost in general uncertainty. I think you should look at them and determine how important they are for those early releases because there's still a lot of leeway in experimentation and opinion to change the rate base that dictates those. 8,000 or 9,000 years is a pretty short lifetime for C-22.

If I could have just a last comment on the cladding model. I think a cladding model is way too optimistic. I would like to ask you where you got the basis for assuming that the corrosion for the zircaloy would be 10 to 1,000 times less than the corrosion for C-22? Where did that number come from?

ANDREWS: I can't answer that directly, but it came from some literature values that were a comparison under similar environments of C-22 testing and zircaloy testing. They were from the literature.

SHOESMITH: I'd just like to put a little perspective on what--if that is a comparison to the general corrosion rate for C-22, then to assume the rate is 100 times less for zircaloy is to say that one atomic layer of material is oxidized about once very 10 to 20 years. I think, there's a credibility gap in making that kind of claim. I don't think actually that in the model you can have compared C-22 to zircaloy corrosion under the same conditions because the C-22 is analyzed on the conditions of acidity, high chloride concentration, and oxidizing conditions provided by the carbon steel. Those conditions could be transferred to the inside of the container. Carl de Bella mentioned that yesterday. I don't think you've made that comparison. If you look at zircaloy under those conditions, it's much more susceptible to pitting and crevice corrosion than C-22. So, if that is not included, I would suggest that you could loose two orders of magnitude in your cladding failure lifetimes if you compare them under the same basis.

The second point I'd like to make is this is already a hydride loaded material. All you have between you and more hydrogen going between the environment and the material soaking up more hydrogen is this oxide layer which cannot retain its properties as a nonpermeable membrane for hydrogen forever. So, a second likely failure process if that you put in a little bit more hydrogen, reach the embrittlement limit way before you would ever grow the oxide through this layer, and it will fail by an embrittlement process. My point is that I don't think it's viable to have a reasonable cladding model and I think yours is overly optimistic. Again, I think a cladding model could be developed, but it's something that's just off the end in terms of experiment at the moment. Just my opinion.

ANDREWS: Okay, thank you.

BULLEN: Thank you, Dr. Shoesmith.

Are there any other members of the audience or the public who would like to comment at this time? John Kessler?

KESSLER: John Kessler, EPRI. I guess, I'd like to begin by complimenting Bob Andrews and Abe Van Luik. I thought their presentations were excellent. Bob, you're very right in acknowledging the graphics department because I think that a lot of the graphics that were shown yesterday help a lot in terms of telling the story and explaining more what you're doing. I think that's going to help when you try to put together a description of what is the Viability

Assessment here in terms of pictures. So, I thought that was very good. I was left almost reeling with how many changes I've seen happen so quickly in terms of the modeling approaches, especially in the saturated-zone. I think I understand why that was.

Another comment would be I like the sensitivity studies that were shown. There needs to be a whole lot more of them. As I heard the comments yesterday, that certainly again helps understand more what's in the models. Another impression I had was the same as Dave's; the cladding model did seem marginal in its justification. Another one would be the net infiltration numbers again, seemed like they've swung to the other end of the pendulum. Another thing that seems to have changed is that there's no more matrix interaction and it seems that the project is taking care--credit for when we see sensitivities that suggest when you turn matrix interaction on or off--you know, base case versus absolutely none--there's almost no difference which suggests that there is almost no matrix interaction. That seems on the conservative side.

BULLEN: Thank you, John.

Would anyone else like to comment? Yes, please, identify yourself?

WIKJORD: My name is Alf Wikjord. I'm with AEC. I have a question of clarification regarding the radionuclide inventories that are going into these analyses. I presume that you're handling end member decay chains for actinides and a suite of fission products. I'm not clear as to what went into the front end of the TSPA analysis and what screening procedure you took to limit that number.

ANDREWS: Yeah, well, let me try to answer that. In previous TSPAs, there have been a wide range of nuclides that we've looked at. In TSPA-95, for example, we looked at 39 radionuclides and tracked those through. In TSPA-93, I think, it was a similar sort of number; it may have been 36 or 39. In those previous TSPAs, you know, the ones that came out as important were always these 11 or 9, whatever it was, that we've chosen to look at here. In order to verify that those are the correct 39 or--sorry, the correct 9, we're doing additional analyses with all of the 39 in there. In fact, there's more than 39, but with the other 30 in there to assure ourselves that, in fact, they are retarded which is generally why they didn't come out or they have very low solubilities or they have sufficiently low half-lives so that they aren't long-term dose contributors. So, those analyses will be documented in the final VA, but we didn't present them here. Yeah, or if they have high dose conversion factors.

The first part of your question is the--all of the parent's ingrowth is already incorporated into the source-term. So, for example, neptunium does have parents; you know, half of neptunium is there initially and the other half of it essentially is ingrown and so we have that ingrown neptunium already in the source-term when we go and do the transport and release calculations.

WIKJORD: I think there would be also ingrowth and decay calculations in the other component--compartments of the system.

ANDREWS: Yeah, there are.

WIKJORD: I have a corollary question. Chlorine-36 is noticeably absent and I'm wondering what the basis for that screening out chlorine-36 was?

ANDREWS: It was never a big dose contributor. Much of its dose conversion factor was small or whether its concentrations were small in previous analyses, but it has never been a significant dose contributor.

WIKJORD: It's one radionuclide that has sort of cropped up in other international programs in recent years.

BULLEN: Bob, this is Dan Bullen. The dose conversion factor for chlorine-36 is pretty darn big. So, even though the inventory might be low and it's got a pretty long half-life, it's an issue that actually has been raised in the low-level waste disposal realm and we've had to struggle with it. So, it might be something that's a very good suggestion to take a look at because you'd hate to be blindsided by something that may not be a problem, but you should at least have the analyses done.

ANDREWS: Like I say, in those 30 nuclides, chlorine-36 is one of them. So, in those analyses, it still didn't come out to some major dose contributor.

BULLEN: Right. Well, it might be where it is. Since it's an activation product, it may be as an impurity in the metals and it could be tied up and never come out in the time frames that you're interested in. As compared to a fission product which would be released, you know, when you reach clad. But, that's something that should be considered and probably addressed.

Very good comments, by the way. Thank you very much.

Any other comments or questions from the public?

(No response.)

BULLEN: Well, thank you very much.

Now, I'm going to announce a little chairman's prerogative here. We have some people who have to leave and would like to see the Rally Barnard talk. So, we're going to move the Rally Barnard talk first if Cliff doesn't mind. I didn't mention this to you, Cliff. So, can we do a quick rearrangement of schedules here and just flip the first two talks? So, we're going to hear the evaluation of disruptive scenarios presentation by Rally Barnard so that certain members of the audience might hear this before they have to catch their planes. I don't normally do this; so, I'm not a soft touch. But, in this case, we'll make a special exception for Rally.

So, this is Rally Barnard from Sandia National Laboratories.

BARNARD: Well, thanks, Dan, you'll get your $5.00 at the end of the meeting.

Here's our obligatory picture of what all the components of the analysis are. Off to the side here from the main path of analysis, you find some of the disturbed events. So, that's what I will be talking about.

Before I launch into disturbed events, per se, I will give a little bit of philosophy about what we're trying to do with disturbed events. Specifically, what we're talking about are disturbances and they are defined as having a probability of occurrence of less than 1. The nominal case has a probability of occurrence of 1. It is going to happen. The nominal case in all its glory is going to happen for sure. So, what we're talking about are events which are not necessarily going to happen. Generally, it's easy if you can identify your disturbances as having very low probability; for example, 10^{-7} or 10^{-8} per year, and these numbers are important, as we'll discuss a little later on. The other thing which makes it easy to define a disturbance is if you have some kind of event going on like an earthquake or a volcano or a significant identifiable change in conditions, such as would happen with criticality. You notice rockfall isn't up there because it's kind of a different animal, but for the time being, I'm including it in with disturbed events. We do say that the disturbed scenarios do not incorporate expected changes, such as climate changes and so forth. So, for that reason, that is not being considered in my analysis.

Okay. For TSPA-VA, there are four disturbed analyses conditions/scenarios that are done; igneous activity, seismic activity, nuclear criticality, and human intrusion. Of these four, I am going to talk about the first two here today, but if there's interest in other ones, I'll go extemporaneous and talk about them, too.

Igneous activity, there are three subcategories of igneous activity that we're considering. Direct releases at the surface, this is the igneous activity that people all envision a volcano popping up and lobbing waste packages onto the Mirage volcano in Las Vegas or something. An increased source-term for groundwater transport that results from an intrusion occurring inside the repository and shake-and-bake, if you will, on the waste packages. The third one is alteration of saturated-zone transport that might occur from an intrusion that happens outside the repository block, but someplace downstream in the flow path that the normal groundwater is transporting the waste and alters that in some fashion which has a deleterious PA effect.

Today, I'm going to talk primarily about direct volcanic releases and the emphasis I'm going to place on the analysis that I'll talk to you about is calculating what the radionuclide source-term looks like for this. The problem consists of having a source-term and then having some kind of dispersal mechanism, which when the radionuclides reach the surface are dispersed, in this case the dispersal model that we're using is the Center for Nuclear Waste Regulatory Analysis. That's a mouthful. They have developed a code called ASHPLUME which provides an analysis of dispersal by an ash plume, believe it or not, of contaminants. So, they have an analysis which has struck the DOE as being a very good model for dispersal and the part that we are providing for TSPA is to calculate a source-term that is going to be used by this dispersal model. The source-term, as you will see as I'm talking along, is attempting to incorporate the real physical processes that are going on in mobilizing the waste into an eruptive stream. The performance measure for what we're doing is the good old boy or girl, 20km, downwind in this case, from the eruptive vent, from the eruptive activity. This person is considered to be in the Amargosa Valley. It's your friendly Amargosa farmer. So, that's the point at which the doses will be reported.

Okay. Here's some pretty cool pictures of exactly what the steps are in order to have an igneous activity scenario result in releases at the accessible environment. The first thing you have to do is you have to determine whether you have an igneous intrusion occurring in the Yucca Mountain area, at all. If it does, you have to ask whether a dike intrudes the repository. It doesn't necessarily have to do that. Does the dike intrusion directly contact the waste? Maybe; maybe not. Can you breach the waste packages if it happens? If you breach the waste packages, can you remove the waste from the waste package? If you remove the waste from the waste package, can you entrain it into the ascending ash? If that happens, is the waste dispersed in the ash plume? So, that's the sequence--that's the hoops that we're going to try to jump through here to see whether we're going to have releases.

I'll start at the beginning now with intrusion characteristics. The DOE has provided the probabilistic volcanic hazard analysis from which I got the probabilities of various dike lengths and orientations in the Yucca Mountain region and whether the probabilities that those would intrude the repository and the length of the dike in the repository and its orientation. All that's important because the orientation and length of a dike in a repository determines how many drifts are crossed by the dike, and therefore, the potential for having interactions between this intrusive body and the waste packages.

There were a number of other plumbing parameters, I'll call them, that were necessary in order to be able to do the analysis which were not part of the PVHA, but which were elicited from the experts in volcanism; a couple of the boys at Los Alamos, Greg Valentine and Frank Perry. Among them are the dike width, the number of vents--the number of vents, not events--vents that occur in the repository; the fragmentation depth, the depth at which the liquid magma containing dissolved gas turns to gas with magma, lava, ash shards in it--that turns out to be extremely important for the analysis; then, the eruption duration, volume, and properties such as density of the magma.

Here's an illustration of dikes shown in red randomly cast upon the Yucca Mountain region showing how they might intersect with the repository. In this case, it shows just one dike intersecting, but in the course of the analysis, numerous dikes were found to do so.

Now, we're talking about trying to have the intrusion interact with the waste packages. The ground rules for this analysis were that the intrusion had to directly intersect the waste package. If it's a near-miss, that's only good in horseshoes or hand grenades, but it doesn't count for direct releases of radionuclides. The near-misses are reserved for the second part of the problem which is the enhanced source-term caused by the heat and the aggressive environment causing rapid degradation of the waste packages and conversion of the spent fuel or other waste inside the waste packages into a form that can be transported by groundwater. The interaction can either be between liquid magma or ash, as I mentioned, depending on the location of the fragmentation depths.

Here's a visualization of how the dike intersects the drift. I'm talking about a plan view here showing the dike come through. Here's the drift and here's a couple of stubby little waste packages. Here is the dike. And effective dike width, you can see, is dependent upon the angle, the orientation angle. It's dependent upon the dike width, and so a little bit of trigonometry leads to an effective dike width. You can see that if you had dikes that are more towards 90 degrees, you get more packages hit. And, if they are more zero degrees pointed north, you have fewer packages hit.

BULLEN: Rally, just a quick question on that picture?

BARNARD: Yes?

BULLEN: Does it assume that there's no magma flow down the drift after you've intersected it?

BARNARD: Glad you asked that. When we're talking about the enhanced source-term, that is the main way that you can interact a lot of packages by waste flowing down the drift. But, the rules of the game are for this one that you have a direct intersection of an ascending magmatic body with a waste package because it looks like it's a real stretch to have event up here where this is vertical and waste and magma to somehow flow back down the drift and up the conduit.

BULLEN: Actually, a parallel is that is there another vent that it could go down and then up again? Is that part of the model?

BARNARD: Oh, never mind. Yeah, the ground rules for that one are if the fragmentation depth is above the repository so the interaction is with liquid magma, then it's assumed that if you do have a vent occurring inside the repository, it might be possible to have some sloshing around and liquid transported. In the case where the fragmentation depth is below--so, you're talking about an ash blast coming up which is the picture I'm showing here--each vent, only those packages in that conduit are affected and there may be flow to the sides and stuff like that, but that's being ignored.

Okay. Here's a conduit and dike widths based on the work that the Los Alamos people and others have provided. Dike widths are generally about 5 meters at the outside, smaller than that. That's the typical in their opinion of dikes at the Yucca Mountain area. In contrast, conduits can be considerably bigger. There are going to be a lot of erosion of the conduit rising up and so forth. So, we considered conduit sizes, radii going up to perhaps 60 meters, 120 meters across. That's a big one. Drifts are 28 or so meters across. So, it's possible, you can see, to have a conduit that comes up that could intersect up to three dikes. So, a little bit of trigonometry done here will give you the number of waste packages that are actually within the radius of the ascending conduit and those are the ones that contribute to the source-term in that case. To anticipate what things are going to look like, the number of waste packages hit range from 1 to 20 or 30. So, in some cases, we can have a pretty big contribution of radionuclides to an eruptive event.

Okay. We've gone through down to here and so the next thing we have to do is find out whether we can breach the waste packages. What are we talking about? We're talking about the magmatic intrusion is really a pretty nasty environment. Temperatures are above your average poached egg temperature and there's some pretty nasty stuff present, and it's felt that in this environment the corrosion allowance material on the waste package does not survive. We are kissing that off right away. You're talking about a lot of iron. Iron is not a very good thing to have in a sulfurous environment. So, just as a conservative treatment for this, no matter what time the event occurs, we just say we're going to forget about the corrosion allowance material.

In contrast, the corrosion resistant material, C-22, really looks like it's going to be fairly resistant in this environment. We're talking a super alloy similar to what's the exhaust of a jet engine. So, these temperatures are not out of its range. It is designed to be quite corrosion resistant in the presence of some of these nasties. Given the fact that the eruption duration for the Yucca Mountain volcanos is in this range, only five to 40 days when you're having the active stuff come up which is the requirement for being able to actually directly remove the waste package, this time is insufficient to corrode full thickness, 2 centimeters, of corrosion resistant material.

BULLEN: Have you done erosion corrosion analyses? I mean, this is going to be hot ash blasting by. It's not exactly like a jet engine plume when it's got these particles in it. That's another scenario where you might want to consider the package going away in a lot less than 5 days.

BARNARD: Well--

BULLEN: Oh, am I straight person for you? I'm sorry.

BARNARD: Well, right, you're up to 10 bucks today. No, I have not considered oblation and corrosion, but I have considered but there's more. I mean, this is a real Ginsu knife performance here you're about to see. But, wait, there's more in a just a minute. Okay?

Essentially, as I'll show on the next slide, you can be assured of having the waste package breach if it is previously corroded by the steps which were discussed yesterday to roughly 50 percent of its thickness. Okay. So, the implication there is that you've got to wait a while before the volcanos that come up are going to have an effect, are going to be able to reach the waste packages. The failure mode is both corrosion and high temperature deformation; the stuff sags on you.

Before I get to that, I'm going to put up this picture. Okay. The corrosion rate, the red diamonds down here are the values which Jerry showed yesterday. Talk about a stretch; here's an extrapolation from the temperatures that they've worked with to the temperatures where we have to be for a volcanic eruption. In previous work in TSPA-93, we've looked at this once before and some work by Douglass and Healey back a few years before that made the general statement about an increase of 10^4 was seen in the corrosion rate for nickel based alloys in the presence of a simulated volcanic gas melting tests with basalt and simulated volcanic gases. And, so this gray blob here is 10^4 higher than this extrapolation.

Wang and Douglass did an analysis where they took nickel 10 percent chrome, nickel 20 percent chrome, nickel 30 percent chrome and exposed it at 800C, which was thoughtful of them, to simulated volcanic gases, sulfur, oxygen, hydrogen, carbon dioxide. And, lo and behold, the value that I calculate interpreting their weight gain results as a corrosion rate is this X here which is kind of a rewarding location for that to be. So, it looks like the value that we're using increasing an extrapolation of this by 10^4 is not out of the ball park for the corrosion rate of C-22.

I will mention that the analysis they did was on a simple nickel chromium alloy, a chromium mixture. C-22 contains roughly 20 percent chrome, but they threw in 13 percent moly and some tungsten and, undoubtedly, it's for enhancing the corrosion resistance. So, this point very well may be a conservative measure of what the corrosion rate is.

BULLEN: Again, Rally, one quick question. In your mechanical analysis, did you also look at pressure buildup in the can and the creep rupture equivalent of frag rupture? I mean, if you're at these temperatures of 1200C for five days with PV=RT, or at least I hope it still does, what kind of pressures do you develop inside the can and is that a creep rupture problem?

BARNARD: That is summarized in this viewgraph here. This is work by the waste package design guys. I just don't know how to thank you, Dan. This is work which summarizes that of the waste package design people who have looked at over a lower range of temperatures, the gray bar there, all those effects. They are looking at simple temperature. They are looking at internal pressure. Essentially, what they find is that for more than 50--here's 50 percent degradation of the corrosion resistant material. For more than that, the critical stresses are such that the yield stress of the CRM will cause failure. So, 70 and 75 percent are a bad area to be in.

What I've done is another extrapolation based on the information I've gotten from C-22. Its liquidous temperature is 1350, at which point I said its tinsel strength is pretty much zero and so here's an extrapolation. I suppose I could have gone like that, but essentially it isn't a big difference. To be conservative, if you have 50 percent corrosion reduction, again good old 50 percent, in this environment, you have to consider that the waste package will deform of its own weight and so that defines the fact that, yes, indeed, we have breach. So, the conditions are if the waste package, the CRM, is half gone due to normal corrosion in some fashion, then if a volcanic event occurs, you consider the waste package to be breached.

Okay. You aren't done yet because if you have an open waste package, you still have not ejected these cute little radionuclides out of the waste package into the ash stream. And, this becomes a fairly tough problem which lent itself to being analyzed by freshman physics. You look at particle kinematics, you look at momentum transfer, and you can come up with some arguments about what size particles can be ejected from a waste package based on the size distribution of ash or liquid magma particles that are coming by. The first thing you have to realize is the waste package you're talking about have the density of 11 grams per square centimeter. In contrast, the impinging material is very much lighter. So, momentum transfer is going to tell you it's going to take a lot of momentum to get these folks moving here.

We have a range of values for ascent velocities for both ash and for liquid magma and we have a distribution for particle sizes. And so, combining those, in essence, what we come up with is, depending on whether you want to talk about an elastic billiard ball ejection of a waste particle into the stream or an inelastic one where the waste agglomerates onto the moving ash, you're talking about roughly a ratio between the size of the waste particle and the size of the ash particle of either being the same or the ash particle has to be twice as big.

WHIPPLE: Excuse me, Rally, can I ask a quick question?

BARNARD: Sure.

WHIPPLE: Do you assume that the spent fuel turns into fine particles when this process begins?

BARNARD: Yes. That is part of the distribution of waste particle sizes that are used. You start out with a 1 centimeter cylinder of centered uranium dioxide, and by the time you get down to radiating it and dropping it a few times in transport and then hammering it with a volcanic eruption, we consider the range of sizes from .01 centimeter or it may be millimeters--I don't remember right now. But, pretty darn small to reasonably small, okay? So, there's a great supply of these small particles to be ejected and entrained.

BULLEN: Rally, quick question. Did you also convert to U308? Have you done the--

BARNARD: No. No.

BULLEN: So, you didn't do any chemistry, but you just did size distribution?

BARNARD: Chemistry, what's that? I'm talking freshman physics here.

BULLEN: Okay.

BARNARD: I mean, we've got to stick to freshman physics, okay? One thing at a time.

Okay. The next thing that can happen is--and this is a pretty standard calculation for the settling of xenoliths--I mean, your average volcanologist will do this calculation, the settling of xenoliths in an ascending ash or liquid magma stream. So, I stole that work in this case using particles of either 11 grams per square centimeter for the elastically ejected waste or about 6 grams per square centimeter for an agglomerated particle to see what the conditions were for settling. Essentially, what it amounts to is if you have an ash flow where the ascent velocity is above 10 meters per second, anything will get carried up. Once it's kicked out of the waste package, it can be carried up.

Okay. So, we finally struggled through all these hoops here and--incidentally, I would like to inform people that the big thuds that you hear are not from me. It's from somebody at this table pounding because all these microphones are on and so that's why you get the noise. So, I'm innocent.

Okay. The waste is entrained to the surface in the ascending ash. That can happen. Then, the last step is the dispersal. Well, here's what the source-term analysis looks like. I had to do 300 realizations of this, Bob, because 100 wasn't enough. As a result of that, 17 of them, less than 6 percent, resulted in, having jumped through all these hoops, having a release of radionuclides at the surface which could be dispersed by the ASHPLUME model. Some of the realizations were eliminated because the dikes didn't intersect the repository. There was a small probability that the orientation of the dike, when we're talking about it starting at the western edge of the boundary, was actually pointed to the northeast so the dike was going off in the wrong direction. Some of them had zero length in there. Another thing is that the repository, as you saw the map of the Yucca Mountain region, the repository represents a pretty small area in the whole region. So, in many cases, although there may be up to five vents which occur along any given dike, none of them happen to be inside the repository. So, that hurt when you were trying to get waste lobbed down on the Mirage Hotel, also.

In almost every case, the ascent velocity of the liquid magma was so low that it either couldn't eject the particles or they would settle faster than the ascent velocity. So, they weren't released. So, what we're looking at is those cases where the fragmentation depth was below the repository and you had this ash blast going by. It turns out that one of the biggest hoops to jump through was the fact that it takes about 400,000 years before you can count on the corrosion resistant material being reduced to 50 percent of its thickness in order to be able to breach the waste package. In my analysis, I used only the 100 percent dripping cases, rather than the no dripping cases, because when I used that, you find that your range is in the 400 to a million year range. If you take the case where there's no dripping, it starts at about 800,000 or 900,000 and you don't get any releases, at all, essentially. It's no fun. So, that's why I didn't do that.

SAGÜÉS: Excuse me, Sagüés.

BARNARD: Yes?

SAGÜÉS: Yesterday, we heard about, what, 30 percent of the packages were supposed to be in a dripping--would that cap down your 6 percent then to a 2 percent?

BARNARD: Yes. Yes. I'm going to have to give him five bucks because--let me just mention that the volcanic has been done before, but not in this detail. We have developed some fairly sophisticated models now for waste package degradation and this model is coming along in a sophistication, but it was definitely developed independently of the model for waste package degradation and, as you'll see with seismic, it's ever more important there. The next step will be if we decide to pursue events like this, disruptive events, is to integrate these models so that exactly the type of point that has been made about the number of each type that is occurring will be reflected in the source term for volcanic, and even more importantly, as I'll show, the source-term for seismic.

BULLEN: Rally, one of the other things we learned yesterday was that when it does fail, it fails by pinholes which probably wouldn't have any impact on any of the entrained waste because I doubt that you'd get a submicron sized particle in and out at the velocities necessary to raise it up--the plume up.

BARNARD: I just don't know what to do without you, Dan, because it's one thing that I meant to mention. The ground rules for the groundwater flow are that all you need is one pinhole. I think you can see that to mechanically remove waste from the waste package requires a much bigger hole--in fact, you really have to rip the waste package open--that's beyond the bounds of what their analysis is. I attempted to incorporate that a little bit, but it's strictly qualitative. So, essentially, all the numbers that we have for that first patch failure probably can be multiplied by a considerable amount to a longer time in order to reflect what it takes to really rip open the waste package.

Let me discuss the ASHPLUME analyses. The ASHPLUME code can be run in both the stochastic and the deterministic fashion. In the stochastic fashion, it rolls the dice a number of times and comes up with different realizations for eruption parameters and waste particle sizes and so forth like that. Essentially, all that stochastic information in source-term was done previous to this. So, we ran it in the deterministic fashion. We used exactly one set of parameters to see where the ash would be dispersed to. The wind direction and speed which are very important for ash dispersal were stochastically selected for the 17 runs. Nine of the cases had the wind blowing to the north. When the dose receptor point is due south--I guess, this is a metaphysical question that the DOE is going to have to answer whether anybody is in trouble to the south if the wind is blowing to the north from this volcanic eruption. We counted those also, but I don't know how to score them in the terms of this poor person in the Amargosa Valley.

This is kind of a neat picture. So, I'll put it up. It's the Wind Rose. This is taken from the Center's analysis of wind velocity and direction and frequency over a number of years at the Yucca Mountain site. So, this is what we can use. As you can see, the blue here is the fraction of the time that the wind is blowing in a direction and, as you can see, to the south is a high frequency time period.

PARIZEK: That wind direction, is that--

BARNARD: Yes. That is something that I just have not considered, at all, but it would be a good point to consider to see if there is any significant difference.

Okay. After all this work, here's what the results look like. This is the base case peak dose expressed in mrem. And, here's the volcanic results, peak does again. In order to get this curve, you remember 300 realizations were run. So, I made the assumption that 300 times a volcano came up in the Yucca Mountain region, and of those numbers, 17 resulted in direct releases. The last step in the process is to multiply by the probability that a volcano is going to occur there, at all. That is the primary output of the PVHA and that work came up with a maximum frequency of occurrence of about 10^{-7} per year and a minimum of 10^{-10} or something with a mean of about 10^{-8}. The NRC feels that the 10^{-7} number is the one that they like a little better. So, in deference to those guys, I multiplied my conditional CCDF by 10^{-7} times over a million years which is 10^{-1}. And so, this is the number that I come up with for the peak dose. This is on a logarithmic, a log-log plot, and it doesn't take a mathematical genius to see that this is going to contribute absolutely nothing to the base case releases.

This is the point where if I had a summary slide, it would be in there and this would be the point where everybody would jump on me and start asking questions from the Board. So, pretend there's a summary slide up there in case you guys want to ask questions because, otherwise, I'll move on to seismic.

BAHR: Rally, you'll probably like this scenario even better. Freshman fluid mechanics would suggest that if you have a magma that reaches a repository horizon which is already a zone of higher permeability that the magma would flow laterally through the repository horizon rather than continuing up through the mountain.

BARNARD: Yes. We are aware of that. That is taken into account in the enhanced source-term model and I haven't finished those analyses, but there are going to be many, many, many more instances of the 300 that I have; not merely 17 will qualify for that. Many more will qualify for that and so that source-term will look considerably different.

The one thing that I need to point out that we're well beyond freshman fluid mechanics, and this one is crack propagation. The method by which a dike intrudes and actually ascends to the surface is by crack propagation. And, any of you machinists in the crowd know that if you have a crack propagating through a piece of metal, the way you stop the crack is you drill a hole right at the tip. So, I think you can easily envision that the combination of a crack rising up reaching this hole in the tip, which is what the drift is, very well could stop the further ascent of a conduit, a dike. So, this is one thing we have not yet analyzed, but it may turn out to make this whole thing an extreme conservatism as to whether there will be eruptive releases at Yucca Mountain. It's something that we will be looking into once the dust settles on VA. It was a chance to have kind of an excursion to talk about that, but--

BAHR: Okay. Anyway, I think that may be the more realistic scenario, in which case your magma is going to intersect most of your waste packages or a large number of them because it's going to be--

BARNARD: Yes, but it's unlikely that there will be any surface releases from them.

BAHR: --but you do have an enhanced source-term. I have one question about your flow chart and maybe some of your text suggests that this is not correct. But, in a few cases, you say that--for example, as I'm going down here, you've got magmatic dike intrudes the repository, no, no performance assessment consequence. Dike intrudes directly--directly contacts waste package, no, no performance--it would seem like in both of those cases that there might be a thermal effect that would affect the source-term. And, in a couple of other places, you do say model is an enhanced source-term scenario. It would seem like you should be modeling an enhanced source-term scenario even in the cases when it doesn't necessarily intrude directly into the repository, but near enough to affect the thermal regime in the mountain or when it intrudes into the repository, but doesn't contact the waste. Again you'd have a thermal effect. And, that's not explicitly on your flow chart there. Maybe that's just an oversight.

BARNARD: No, the shorthand on this reflects what we did in TSPA-93 which was precisely what you've talked about where we did look at the thermal and, as I mentioned previously, the gas, but not immersing the waste packages in magma. Okay? In that case from TSPA-93 results, we found that there was essentially no measurable effect. So, there would be no

PA consequences of having that occur.

BAHR: And, I don't think you had the same thermal models for the mountain in '93 that you have now.

BARNARD: Well, that's true.

BAHR: It would seem like you'd need to update--

BARNARD: It would not be a major change to include that and actually the analyses we're doing may include some of that, but primarily it's going to be immersion of waste packages in liquid magma and in a pyroclastic ash flow in the drift.

BULLEN: Other questions from the Board or the Panel?

(No response.)

BULLEN: Questions from the staff? Leon?

REITER: Rally, there are many countries around the world, industrialized countries, that have volcanic activity; Japan, Iceland, Italy. Do we have any experience or analysis of these kinds of events in those countries; namely, not the dispersal of ash, that's done routinely in lots of places. I'm talking about the intersection of some sort of facility with either eruptive columns coming up or with dikes or some unique analysis? Maybe somebody else in the audience knows the answer to that. I don't know.

BARNARD: Being that I am not a volcanologist, this is not an area I have pursued, but in talking with Greg and Frank and a few other people, I relied on their knowledge to have included that if that information was available. So, that may be not a good answer. The answer is, no, I haven't considered it, and it has not been brought to my attention by the experts. So, I guess, the complete answer is no. But, I will certainly check with them and ask them whether they have considered that in what they've provided to us.

BULLEN: Other questions?

(No response.)

BULLEN: Do you want to continue Rally?

BARNARD: Okay. Moving right along, seismic. The primary disruption that we're going to be talking about is expected to be from rockfall. Other possibilities are water-table rise, seismic pumping, and refocusing of the unsaturated-zone flow. This one was actually talked about yesterday. It might be possible for us to include that, but based on prior analyses, it looks like these are either short-term or low PA impact events. So, the focus of TSPA-VA was to look at rockfall. Rockfall can occur from either thermo-mechanical or seismic effects. The seismic is certainly a lower probability, we think, than the thermo-mechanical. I say we think because the work on thermo-mechanical is developing and it may or may not provide a large indication of rockfall.

So, here's the scenario. You have seismic events or thermo-mechanical events causing rocks to fall from the ceiling. The rocks hit the waste package or they don't. The rock ruptures the waste package. If it ruptures the waste package, then we have an immediate breach which can occur possibly at an earlier time than would have occurred from normal corrosion activities. What we look at then is an enhanced source-term of groundwater radionuclide transport and so the performance assessment consequence of this is the same one as is for the base case. We look at the dose at the receptor site.

Maybe the rock doesn't actually rupture the waste package, but just damages the waste package. It could be hard enough to ding it, but not to break through it. If it doesn't do that, then we consider the waste package has not been affected by rockfall and the base case strictly applies there. If it is, then we consider that the waste package corrosion increases at the site of the damage. This is an area which we're developing in the WAPDEG, waste package damage code to look at means of modeling an increase in localized corrosion that occurs as a result of damage like this. If we don't have localized corrosion, we will just model this as the base case, and if we do, we look at the increase in waste package failure time that will result in a changed source-term.

Rockfall that is initiated by a seismic event, in the model that we're using is determined by the peak ground velocity and that will determine the extent of the rockfall. Rock ranges in its competency from weak rock, very weak rock which essentially is gravel that might just barely be stuck in the roof of the drift to very competent rocks with very few fractures in them and it looks like it's going to stay up there forever. Now, if you had a seismic event, the weak stuff is very likely to fall, but it's all going to be small rocks. If you finally get a seismic event big enough to bring down a big rock, that may be a very rare event, but when it falls, it can be a doozer. So, the much more competent rock is the one that has the biggest potential for actually causing waste package damage as a result of falling. The damage caused by rockfall depends on impact, the distance that the rock falls from the ceiling of the drift down to the waste package. Throughout this entire analysis, we magically have a drift which has remained open and contains no rubble for 1,000,000 years. It's quite a simplification and I think that we may want to reconsider that, but you don't have rockfall damage occurring when you have a drift that's already half full of rubble because you mitigate the impact so much by the rocks that have already fallen down to the bottom.

What we have from the analysis by the waste package design people is the minimum sized rock that can breach a waste package, split it open like a coconut, or the minimum sized rock that will just barely dent it; anything bigger will make a bigger dent in the waste package. It's these latter ones which are used for calculating the enhanced, the increased waste package corrosion, localized corrosion. As the waste package gets thinner, as the corrosion allowance material and then the corrosion resistant material corrode away, it's going to take a small rock to cause damage. For example, when you have a virgin waste package down there, it requires a 38 metric ton rock to fall on the waste package to split it open. That's a big one. When you finally get down to having corroded away all the corrosion allowance material and you only have the CR in there, it only takes a 350 kilogram rock falling on the waste package from that same height to split it open.

PARIZEK: --here about juvenile failures that we've been hearing about. These are babies that just arrive in the repository. Would that also be true with the rockfalls on juvenile failure? Are those prone to juvenile failures?

BARNARD: Yes. In order to help provide a justification, if you will, for juvenile failure, I looked at rockfall which occurred in the first 10,000 years and tried to see if those could be contributors towards juvenile failure. The finding was that because the waste package is so robust and the sizes of rock available are the sizes that look like they're there, until about 8,000 or 9,000 years it's very unlikely that you'll get any waste package failures due to rockfall that you could attribute as juvenile failures. Let's see, have I covered everything there? Yeah.

Okay. Here's a curve of peak ground velocity. This was developed by the people working on the probabilistic seismic hazard analysis at Risk Engineering. It is an interpretation of the PSHA information provided on the peak ground acceleration. It's expressed as an annual probability of exceedence. So, at the 10^{-3} level, you can see that you have a peak ground velocity of around 10 centimeters per second. The interpretation of the annual probability of exceedence is that its inverse, 10^{-3}, means that every thousand years, 10^{3} years, you would have a peak ground velocity of 10 cent--an earthquake resulting in a peak ground velocity of 10 centimeters. You can see that by the time you get down to 10^{-6} or million years, you're out in the hundred centimeter for the median, and the 85th percentile, the one--is up 300 or so centimeters per second.

BULLEN: A quick question on that one, Rally. Is this peak ground velocity at the repository horizon or peak ground velocity at the surface of the earth?

BARNARD: There are four points at which the PSHA analyses were done. This is actually at what's called Point A which is a rock outcropping at the elevation of the repository, but out in free air. It does not include the 300 meters of overburden which is Point B. And, actually, I'll just ask Robin to tell me what's going to happen. At Point B, it's going to be less or is it going to be more at PGV?

MCGUIRE: Point B would be at the actual repository elevation which has the top 300 meters of tuff added back on. The velocities there would be lower than at Point A which is a hypothetical rock outcrop with that top 300 meters of tuff stripped off. So, it's a hypothetical outcrop for Point A. Point B is at the actual repository horizon with the 300 meters added on.

BARNARD: Oh, and the other thing I did is these are actually horizontal PGVs. Gabe Toro of Risk Engineering said that the standard procedure is to take two-thirds of that for vertical. So, that is another factor that I included.

But, what I wanted to show down here is some empirical work that was done by Kaiser, at al., in the mining district in Ontario where they developed a measure of, what they call, damage levels, these DLs, as a function of peak ground velocity. And, peak ground velocity can depend on the type of rock that's--or, excuse me, the damage level that you measure can depend on the type of rock that you're talking about falling. And so, I am considering here this is for medium quality to high quality rock. DL3 would imply moderate damage and that would mean that you would have sufficient shifts along joints that you would have the probability of falls of loose rock here. You could have more serious falls here. And, finally, up here, you can have complete tunnel collapse is their measure. I want to emphasize that damage in this case does not mean waste package damage. This is their term for the amount of rock that falls and they're calling that damage. So, there could be a little bit of confusion and I'll try to point out that that's not what we're talking about with these damage levels. But, remember damage levels because they turn out to be kind of important.

NELSON: Did Kaiser develop this in the context of a size effect in terms of the tunnel diameter when these damage levels kick in?

BARNARD: John, do you know the answer to that one? I have a whole cadre of experts since this is not exactly my field. This is John Kemeney from the University of Arizona who has worked on this.

KEMENEY: Peter Nelson took into account lots--the four different effects, the quality in the rock, whether or not there was falling occurring at the time of the seismic event, and the stiffness of the--which sort of affected the room that maybe you're thinking of if you have a number of openings together, it would be more squishy--and also the effect of any kind of support. But, size specifically wasn't taken into account, but it's sort of taken into account in the stiffness effect that we had. So, there's four parameters and that was used as part of the analysis.

BARNARD: This is another presentation of some of the information you saw yesterday. Here, we have the corrosion--the fractural wall thickness is a function of time for the corrosion allowance material and the corrosion resistant material. As I mentioned, I'm only looking at the dripping case here because for the corrosion resistant material in the dry case, it's beyond the million years for a lot of it. So, what you can see is that our magic 50 percent point on CRM is coming way out here in the 7 or 100,000 or so range, but in contrast, the corrosion allowance material can be 50 percent or more gone in 10,000 to 20,000 years.

Combining this information we have on the thinning rate of waste packages with the information we have on the critical rock mass that can damage a waste package gives us the plot down here. So, now, we see the mass to just bonk the waste package enough to start localized corrosion ranges from 8,000 kilograms in 8 ton rock at virgin waste package, an undamaged waste package, or out to about 1,000 years falling to 2500 and so forth like that. And, out beyond about 10,000 years, you can pretty much forget about the corrosion allowance material. It's just gone for being robust enough to prevent much in the way of waste package damage. Here, notice the break in the scale up here, 38,000 kilograms for a package that contains--still consists of 100 percent of its corrosion allowance and 100 percent of its corrosion resistant material down to, as I said, about 350 kilograms. And, when you only have 50 percent of the corrosion resistant material left, that's all that's left, and it takes only a 50 kilogram rock to slit it open.

BULLEN: If you put that one back up for just a second, it sure looks like if you didn't have the corrosion allowance material going away, you wouldn't have much problem. So, any consideration to lobbying the designers and saying let's flip the barriers and let corrosion resistant material be there forever, the corrosion allowance isn't going to corrode on the inside, and so you're out to a million years and we don't have to worry about rocks falling? Did you talk to the designers about that or is that not a purview of your analyses?

BARNARD: Oh, I wouldn't dream of talking to the designers about that. They make enough trouble for us already with changes.

BULLEN: Well, it might be one of those things you want to discuss because it sure looks like the problem goes away if you want to take credit for it.

BARNARD: Yeah, it also could be that just increasing the thickness of the CRM might help also.

BULLEN: Yeah, but that one looked like it would cost you some money as opposed to flipping the barriers might not be a big cost differential.

SAGÜÉS: Excuse me. Those assumptions also are all considering uniform corrosion of the corrosion resistant material, right?

BARNARD: Yes. Uniform corrosion and the question about what happens if you have a fully corroded patch as is the model and the rock happens to fall on that is one that I have not considered. So, I am looking at uniform corrosion until the rock falls and then we look at localized corrosion to see--and then, we're looking for a patch opening up in more rapid fashion as a result of the damage.

SAGÜÉS: I would assume that the high aspect ratio of pitting would have a negligible effect on the mechanical strength?

BARNARD: I would think so, although that's nothing that I have yet considered in the model.

WHIPPLE: Jean and I have been doing high school arithmetic over here.

BARNARD: Yeah, well, we have freshman physics; let's have some high school arithmetic.

WHIPPLE: I'm just curious about by working simple geometric units for a spherical rock, your 35 metric ton rock is 2 meters larger in diameter--actually, in radius than your drift is in distance.

BARNARD: We don't have to worry about that one, okay?

WHIPPLE: I was just wondering how you got it to fit?

BARNARD: Well, I'm going to owe you five bucks, too.

Here is a plot of the distribution of block sizes that have been inferred in the survey in the ESF of joint spacing. This was done by John Pye of the M&O and interpreted by him and provided to me. What I've done is I have been to this histogram according to the sizes of the critical rock masses. If you look really closely, you'll see that there's no 38 metric ton rocks to be found in there. There's no 24 metric ton rocks to be found in there. This is for the TsW zone only and they didn't even find any 8 metric ton rocks. You manage to get one at 3500, but most of them you see are down here in a considerably smaller size. So, what we first established is the size of rock which is required to fall on the waste package to do the damage. Now, we look and see whether any of those rocks are available for falling. And so, that's what this shows.

When you combine that information--

NELSON: Let me just ask one question. In obtaining that plot, were there also sections of tunnel that, in fact, did not have blocks available for falling because of the joint-frequency study? Do you know what I mean? Cases where the jointing was not intense so that there really were no blocks that would fall, in which case how was that observation counted in the size distribution?

BARNARD: I am sorry. I do not know the answer to that part of the analysis.

KEMENEY: I do know that biggest block is from the biggest joint spacing that they found along the tunnel. So, there was one case where there was a joint spacing of a couple meters thick and that represents that one biggest block. That's where it came from. So, the block sizes come from the--derived from the joint spacing.

BARNARD: And, the way they arrived at 38,000 kilograms was by doing a finite element analysis on a model waste package. This is the size block which caused the--what do you call it--the margin of safety to be sufficiently low that for engineering purposes they considered that it would be a failure. So, that's where that number came from.

Okay. I'm going to wrap this up in just a second by talking about exactly what the analysis looks like. What you do is you start by randomly picking a time of occurrence which will lead to getting a peak ground velocity which will give you the rockfall characteristics. The greater the PGV, the larger the rocks that fall. Knowing the time of occurrence, you can figure out the extent of waste package degradation. So, you know the minimum size rock that can do damage. When you sample from the rock size distribution, you'll determine the nature of the damage that's done and then you pass this off to the WAPDEG calculation that is going to give the source-term to RIP.

The results of what is passed off to WAPDEG is shown here. The analysis were done and stratified by hazard level because both the waste package wall thickness and the PGV are essentially correlated because they're both driven by time. So, what we see is that in 1,000 years, hazard levels less than 10^{-3}, in other words in the first 1,000 years, no packages are breached and no packages are damaged. That's because the wall thickness is too big and the predicted earthquake is too small. You have a PGV of less than 10 centimeters per second. In the 1,000 to 10,000 range, we still aren't able to reach the waste packages and we get a tiny number which are damaged. Finally, in the 10,000 to 100,000 range, roughly 17 percent of the waste packages are breached and 1 percent or so of them are damaged.

WHIPPLE: No, those numbers have to be reversed. No?

BARNARD: No. I don't have time to explain it, but you're looking for a narrow window because what it takes to split the thing open and it's too small to do any damage. There's a narrow window dependent upon the analysis they did for--you remember those two curves of critical rock mass versus time--it gets really thin down at the end. So, that's why there's a small number there.

The other thing to consider is although Bob's and other people's visualization of the drift for the waste packages in them made the thing look like an LA freeway from cheek to jowl all jammed together, there's really roughly only 40 percent of the linear distance along a drift occupied by a waste package. Almost two-thirds of it or 60 percent is empty space. So, when a rock falls, it has a 60 percent chance of falling into empty space and not falling on the waste package. This is taken into account in these numbers. And so, you can see that finally when you get out to the 100,000 to million year range, now about 30 percent of the waste packages will be breached by rocks falling and actually hitting them and still a very small number are damaged. Overall, over a million years, about 12 percent of the waste packages are damaged.

At this point, I do not have the WAPDEG results to show you. So, we'll pretend I have a summary slide up here and I'll try to entertain questions.

BULLEN: Just a quick question, Rally. Your data for the ESF is joint frequency for that, but one would assume that, as you got out into the repository horizon and you got data from the ECRB, you would have a different joint-frequency distribution and so you could get a different probability of rockfall?

BARNARD: Yes. What I showed you in that histogram was actually from the TsW portion. There is a different histogram for the entire joint-frequency that I didn't show. I am aware of the differences and, as we get new data, those will be incorporated.

BULLEN: Your analyses also have--should have some effect with respect to tunnel size, right? I'm thinking of the Jim Blink diagrams now where the rocks seemed to fall and it looks pretty nasty--

BARNARD: Yeah, this, I am not taking into account. But, as those data become available--or, I mean, as we decide to model those, we can do it.

BULLEN: I was just wondering about the reverse analyses that says maybe if we make smaller tunnels, we won't have to worry about this after a million years. Have those kind of thought processes been involved in the analyses?

BARNARD: No, they have not. I should mention that seismic rockfall, this is the very first time this analysis has been done and it was done in isolation from the base case waste package degradation. I think it's abundantly clear to people that what needs to be done next is to incorporate rockfall into the standard base case analysis because then we'll have the specific failure of different groups of packages and different locations and you can apply the PGV, and thus, the potential for rockfall to those packages and integrate it much better. So, next time around, this is the way this will be presented.

BULLEN: You just answered my next question because I wanted to know what future work you were going to have.

BARNARD: Now, you know.

BULLEN: So, that's the summary.

Questions from the Board? Nelson?

NELSON: Not so much a question as a comment. I've been looking for this kind of an analysis, early though it is, ever since I got on the board and I'm very delighted to see this and hope that it will continue to an analysis of the non-seismic related rockfall situation.

BARNARD: Yes, I'm sorry, I didn't have time to go into that.

NELSON: Of course. And, please, when results are available and as the model evolves, the Board is very interested in hearing about it.

PARIZEK: Of the four scenarios for disruptive events, you've mentioned two.

BARNARD: Yes.

PARIZEK: There's two more; the human intrusion and, I guess, criticality. That's somewhere else or will that be also in a TSPA--

BARNARD: Do I have time to say anything about that?

BULLEN: Two minutes.

BARNARD: Two minutes, okay.

PARIZEK: I'm sorry I brought it up.

NELSON: You've got to pay him now.

BARNARD: Criticality is essentially a function of the following. You have to have the waste package fail really in a special fashion; namely, by having the top fail rather than the bottom because that will give you a bathtub to hold the water which is the moderator for commercial spent nuclear fuel. The next step is to remove the neutron absorbers which are placed in there specifically to keep criticality from occurring. Generally, it's boron and boron is soluble. So, it is possible to do.

There are three locations for criticality that you can have; in-package, near-field, and far-field. Of the three, the one that looks the most likely is in-package because you have the fissile material there. You don't have to reconcentrate it by some geological process at a later time. In the case of commercial spent nuclear fuel, it's largely in a physical configuration which lends itself to criticality because that's what it was supposed to do when it was in the reactor. So, adding water and removing the neutron absorber and a few other things of that nature, making sure you have enough moderator, will according to the analysis done by the waste package people, produce a criticality.

The probability of that occurring, there's at least as many hoops for that as were required for the direct volcanic release. What they find is that it is not out of the bounds of reality and could occur, but the likely increase in fissile fission products and other fissile products is about 25 percent for a 10,000 year criticality starting about 15,000 years out. Okay? It turns out that the waste packages most susceptible to criticality are those with low burnup, but having an initial enrichment of the 3 to 5 percent, but happen to have low burnup. They also are the ones that release fission products. So, they contributed originally the least number of radionuclides to the inventory. So, it almost washes in that those which originally contributed very little in the way of radionuclide inventory are contributing the most in the case of a criticality. If you have 55 gigawatt day burnup fuel, there probably isn't enough U235 there available to sustain an criticality. It is possible to have a near-field criticality if somehow you have the bathtub, you dissolve everything very nicely, and then the bottom fails like a dump truck and it dumps out in the drift and somehow manages to form a critical configuration. Fairly unlikely. So, for the analysis for TSPA-VA, we're going to do a single example calculation, essentially what I described.

Human intrusion--the last thing, this will just take one minute. This is one which is a stepchild of the analysis in that we are not supposed to include the probability of occurrence because if you want to tell me the probability of future human technology and behavior, please do and I'll use it. Okay? So, what we're doing is we're looking at a waste package that magically is drilled through by some driller. He drills onto the saturated-zone, magically a bunch of waste falls down through the hole into the saturated-zone where it theoretically can be directly transported by saturated-zone advection to the dose receptor point. It turns out the spent fuel dissolution model which was discussed yesterday is a function of oxygen and carbonate and temperature and so forth like that and saturated-zone conditions are much less conducive to dissolution than are UZ. So, if we use UZ saturation rates, we will bound the problem with a much higher dissolution rate than we expect. What we will do is to use RIP here, and instead of having a UZ transport model bringing us a distribution down to here, we will have essentially a spike release based on having bare CSNF, commercial spent nuclear fuel, in the saturated-zone which can be transported by RIP and convolved against the saturated-zone breakthrough curve.

Thank you.

PARIZEK: I had one other question regarding the diagram you had on your left which showed the groundwater impact part of it. Now, part of it from the--analog you just used, that would be one example. But, in terms of the dike emplacement, is it possible that we had, say, canister failures, we have the unsaturated-zone with some radionuclides, groundwater has some, the dike approaches that environment to reflux that in the form of steam to bring that back through the atmosphere because you have all of the gases that you mentioned as your nasties list and some of that's obviously going to be brought back to surface. Is that going to be in the analysis or the thought process?

BARNARD: It is not yet. We can look at that and see if it looks credible and possibly include it.

BULLEN: A quick question on criticality followup. Is your analysis going to include DOE spent nuclear fuels, specifically aluminum clad degradation, or--

BARNARD: Yes. Yes, it is. You only gave me two minutes. So, I couldn't talk about that. There's also plutonium and--

BULLEN: Right. That's a yes/no question. Yes is fine. That's sufficient.

BARNARD: DOE fuel, it will be to cover it the best we can in our example calculations.

BULLEN: Don't go away yet, Rally, because I've got to ask everybody else. Any other Board questions?

(No response.)

BULLEN: Panel? Chris Whipple?

WHIPPLE: Rally, from the seismic hazard analysis you presented, you've got frequencies of ground motions. You can relate ground motions to the rock size and probability of falling. It strikes me that with that analytical framework, you could calculate the curves on the distribution with time of the buildup of fallen rock in the drifts and come up with probability distributions for that.

BARNARD: Uh-huh.

WHIPPLE: Has that been done and, if so, how long do you find that it takes for earthquakes to backfill the drifts for you?

BARNARD: I have not done that calculation. It may be just due to time pressures, we will not be able to do it, but it looks like a very useful calculation to make for future analyses because, as you heard, there's a fairly big stretch for me to consider the drift to be open for a million years for these analyses. If we knew this better, it could be used for the thermohydrologic calculations and a number of other base case calculations.

WHIPPLE: Well, it probably affects your corrosion calculations.

BARNARD: Oh, yes.

BULLEN: Other questions from the Panel?

BAHR: Yeah, I guess, just along the same lines, if you have a lot of rockfall and you've backfilled your drift and you've changed the geometry of the upper surface of the drift, that should seem to have some feedback into your dripping model. I understand that that hasn't been incorporated, at all.

BARNARD: We recognize its existence, but have not included it in TSPA-VA.

BULLEN: Any other questions from the Panel?

(No response.)

BULLEN: Questions from the staff?

(No response.)

BULLEN: If not, thank you very much, Rally, and I appreciate you switching places with Cliff here.

Our next presentation is going to the traceability analysis example and the presentation will be made by Cliff Ho also of Sandia National Laboratories. Cliff, it's all yours.

HO: As Dan just introduced, my name is Cliff Ho and I work at Sandia National Laboratories with the M&O. I'm primarily responsible for helping to integrate many of the unsaturated-zone hydrology activities. However, today, I'll be speaking on a somewhat different, although related, topic and that is traceability for TSPA-VA.

Now, yesterday, we heard a number of technical presentations and this morning on the analyses and the results and the bases of our VA calculations. But, in addition to providing you with the technical content and defensibility of our work, we are also required to provide you with traceability of those calculations. As you saw yesterday, there are a number of calculations and there are a number of different components and those calculations have information transfer from all those different components and we have to insure traceability for each piece all the way to the end product. So, that will be the emphasis of my talk this morning by providing a traceability example for TSPA-VA.

First, I'd like to acknowledge many of the people who have really enlightened me in this traceability area at Sandia. Eloise James is our technical data coordinator; Susan Howard who is helping to try to make traceability a reality for VA. At the technical database, I've been working closely with Ray Keeler to get a lot of this VA information into the technical database and Steve Bodnar. At the DOE, Claudia Newbury has recently done some nice work in tracking down some of the data tracking numbers.

First, I'd like to define what traceability is. There's a lot of formal and informal definitions for traceability. They all seem to have the salient meaning. From the Board, the Panel, the NRC, there's a lot of different definitions, but they're captured here by the definition that I pulled out of the dictionary. This is, by far, the most concise. So, I decided to use this one. If something is traceable, you're able to ascertain the successive stages in the development or progress of that system or process.

Now, for Viability Assessment, we have two main facets of traceability. The first is in the decisions and the assumptions that go along with these models that we're using. I hope that we present that through our presentations and ultimately through our VA document. It's the second facet that I'm going to be concentrating on and that is the traceability of our information transfer; the data files, the model inputs, the model outputs, the raw data. How is all that information transferred from one piece to the next? This is behind the scenes. This is something you're not going to see in these presentations or perhaps not even directly in the VA document, but that is an important part of VA, as well. So, my objective is to demonstrate the framework to produce traceable performance assessment calculations with regard to data, model input and output files, such that they are traceable and retrievable. The primary reason for being traceable is that we would like these analyses, if so desired, for an independent PA group to reproduce them. So, reproduce-ability can be achieved if we can provide traceability for all these different data, input and output files, and all the information exchanged that goes all the way to the end product.

So, this next slide shows an illustration of again all these different pieces and components to VA and I invite you to look at your handouts for a clearer view of each of these individual components. The somewhat cluttered nature of the slide is quite effective in actually showing my main point and that is there are all these handoffs for information transfer that have to go from one model of a component to the next before it is then used by another component and then another until we finally get to the end product which is the final performance measure of dose.

And, this example that I'll be providing this morning deals with the unsaturated-zone flow component and the feeds to it and the output to the unsaturated-zone transport. So, that's the example I'm going to emphasize. I'll admit that the reason I'm choosing this example is because it's an area where I feel that we have good traceability. In some of these other areas where perhaps the traceability is not quite as good, we are working to get the traceability that is required for the VA.

So, on that branch of the VA components, I've sort of expanded that here. For unsaturated-zone flow and transport, again the output for this example is going to be UZ transport calculated by the RIP/FEHM coupling that was explained yesterday. Okay. So, the output here is the unsaturated-zone transport, radionuclide transport that spoke to the saturated-zone, and to get to this point, though, we needed some flow fields to do the particle tracking of those radionuclides. We needed a post-processor to convert those flow fields into an FEHM readable file from the TOUGH2 flow fields where they were actually calculated. Well, those TOUGH2 flow fields, in turn, needed a lot of information to develop those flow fields and we're getting down to the basic properties and the input parameters for those UZ flow fields. So, here's an example where we have a lot of information transfer to get to our end product for this example and that is UZ transport. So, what I'd like to in the next several slides is just show for each successive pairing how is the information controlled and transferred so that we can insure traceability each step of the way.

So, for that first pairing on the top, this is another good point that I'd like to make. As scientists, we often like to think from the bottom up. We like to start with the theory, the fundamental theory or the basic properties, and work our way, present it in a journal article or whatever, to the end product. But, for traceability, it's often easier to start with the end product that you can identify with and then trace your way back from that point to the basic properties. And, that's the way I'm going to present this.

Starting from the top, again our output is this RIP/FEHM calculation of UZ transport. The output is the radionuclide transport in the unsaturated-zone. What we need to make that calculation are the flow fields that can be read in by this FEHM particle tracker. That was done with this T2FEHM2 post-processor. Now, instead of just handing off that information to this component to control that information and insure traceability, we go through this side process where we submit that data and information via Technical Data Information Form, this TDIF, to the technical database. Now, if you look at the fourth to last slide in your handouts, I've got an example of Technical Data Information Form. So, when we submit this information to the technical database, we submit it along with this form and it has some very basic information. It's very simple to fill out. It has the identification of the data, the title, the method that was used, and importantly on the bottom, it also has the source data or the input information that went into that component so that you can then trace it back from that point. So, this accompanies the data that is then sent to the technical database.

The technical database is a storage, as indicated by this file cabinet, of all this data information that is being brought in, but is indicated by this happy computer here that's actively taking information and handing it off. It is also very computer accessible. If you look at the third to the last slide in your handouts, I have an example of the--or the home page for that technical data management system. By being computer accessible, one of the best ways of common technology is to have it on the Web. So, when information is sent to the technical database, they then work to put it on the Web and this is the home page from that web site and if you are a participant of the Yucca Mountain Project, if your domain name is acceptable or valid, you can access this site and that's shown up on the right hand corner there.

BULLEN: I see you've got 30 day posted data changes. Is that the frequency of refreshing? I mean, is that as fresh as the data gets or--and, I guess, the corollary question is how hard is it to get this data in and how readily accessible is it after you get it in to people who want to get to it?

HO: Yeah, and you've got several good points in there. Let me see. The first one is this 30 day posted data change. Steve Bodnar had once mentioned that to me. I don't know if Claudia--do you have an answer to that? I'm not quite sure what this button means. I've never actually pressed it, as you can see. I only go here and here.

NEWBURY: The 30 day posted data change is any changes that occur to the data in the last 30 days because sometimes datasets are superseded, sometimes they're changed for another reason, sometimes new data comes in. So, we want a real quick show of the last 30 days worth of changes to the databases.

BULLEN: Before you leave, Claudia, if somebody is using the data, is there a way that once data has been changed that that person is notified that what they did may no longer be valid?

NEWBURY: That's something we're working on. What we had done in the past is said it was up to the person using the data; go back and check periodically. And, of course, that doesn't work too well. So, what we're doing is we're--as this has gone online, we can now keep track of who has accessed the data, and if it changes, we can notify them. It's something we'll have soon.

HO: But, getting back to your other points--thanks, Claudia. Getting back to your other points of how easy is it to submit and retrieve? I will concede that in the past and perhaps to some degree right now, it is not as transparent to some of the principal investigators as to how to use this system. That's what we're trying to work with is work with the principal investigators and say, hey, it's on the Web. It can't too much easier than this. But, as far as putting it in the Web, we submit the technical data information form. We do require an independent technical review of that data when it is submitted. But, outside of that, submittal is very easy. As far as retrieval, once it is on the Web--and it can take from a day to maybe a week or two depending on what the priority is of that data. If you have a contact at the technical database. I have Ray Keeler. I say, hey, Ray, I've got something that I really need up on the Web for this TRB meeting, can you put it up there, and he'll do it within a day.

But, once it's up, you come to this home page. For the performance assessment calculations, I click here on system performance assessment and there is a list of all the relevant data, the inputs and outputs, for the VA calculations that you can click on and download via the Web. What I envision--it's not quite there yet, but what I envision is that when you click on this, you might get a nice page where you have each of those nice little icons that we're using for each of the areas. So, when you click on each of the icons, you then go into the area for that icon and you see all the different inputs or outputs for that component. We're not quite there yet. The advent of using the Web for the technical data management is relatively recent. Again, we're working towards this.

BULLEN: A quick followup question. You did mention that it was reviewed by a technical person to make sure that it was valid. I assume that also means it's got a QA pedigree associated with it and so if you're going to use it for design data, you've got to have QA level whatever and--

HO: Yeah, yeah.

BULLEN: --is that also a check and balance that you have or can somebody sneak in a little bogus data to pose a problem for you?

HO: No, I think that the control is here. When you fill out the TDIF, Technical Data Information Form, if you look on here there is a place where you specify whether or not the data is qualified. So, you do indicate if that data is qualified and there are certain requirements to be qualified.

BULLEN: Okay. Does anybody check the fact that it is qualified, I guess would be the follow-on question? You might want to consider that one.

HO: Yeah. No, I think that's up to the technical data coordinator, and on our end, Eloise James makes sure that we do follow, say, the rules. In this case, if you look at the example, she said, no, this isn't qualified for certain reasons. I think that it depends on the institution. You know, one of the things about technical data management and traceability for the Yucca Mountain Project, as opposed to, say, the WIPP Project, is we have a number of different participants, not just Sandia controlling WIPP. So, it's up to different institutions to maintain the integrity of, say, their submittal to the technical data base.

Okay. So, we've submitted it to the technical database. The analyst who is running RIP or FEHM that does the UZ transport, then retrieves that information. If you look on the top of that Technical Data Information Form, that TDIF, there is a unique Data Tracking Number or DTN associated with each of these post-process flow fields. Okay? So, when you use this in RIF/FEHM for UZ transport, you are assured of using a controlled version of that file and also of showing consistency that if somebody else wants to use this process flow field that with this unique identifier that they keep using a consistent dataset.

Stepping through the stages of this example, that was the handoff to the top level UZ transport, but the T2FEHM2 post-processor, of course, needed the flow fields from TOUGH2. So, that's the next stage. A very similar viewgraph except this is just the input to the post-processor where we needed information from the TOUGH2 calibrated UZ flow fields. And, again, this information, each of these flow fields is submitted to the technical database with the Technical Data Information Form which then identifies it uniquely with this Data Tracking Number, and then it is used by the post-processor.

Then, finally, the flow fields that were calculated, of course, need a host of information; hydrologic properties and various input parameters. And, the input to that TOUGH2 flow field is shown here where we're getting now down to the raw data and supporting information. For this, I have a couple of example icons. The borehole data, infiltration maps, all of these have unique DTNs or Data Tracking Numbers once they've been submitted to the technical database through this Technical Data Information Form. And, here are some examples of some of those DTNs; porosity measurements, air permeability, infiltration rates.

If you look at the last two pages of your handout, I include a table of input data for the LBNL site-scale UZ flow model and it ranges over information for the geologic framework model, thermal properties, matrix and fracture properties, air permeabilities, temperature, geochemical, a host of information that went into the model. Again, the DTNs associated with each piece of information uniquely identifies that piece of information.

SAGÜÉS: Excuse me, I want to proceed now coming from the bottom up, right?

HO: Yeah.

SAGÜÉS: So, we have now data from the field and from the technical literature evidently judging by the example that you gave down there. And then, that's taken to something called calibration and unsaturated-zone flow. What is done in that particular state?

HO: This up here has been performed by LBNL. It was, more or less, lumped. In other words, the DTN that came out of that actually includes the calibration of those flow fields using the raw properties and--

SAGÜÉS: Okay. But, what do you mean that you calibrate the flow field?

HO: Okay. The flow fields--the properties are then calibrated using this raw data. They run inversion calculations. So, they take a series of, say, 1-D columns, okay, and they have observed data--matrix saturations, matrix moisture potential--and they put in the raw data with some standard deviation associated with it. They constrain those parameters that they can then fit those parameters, the parameters that are deemed uncertain. Fracture properties are very uncertain. So, we usually let those be fitting parameters in a lot of our calculations. But, you let those vary using the inversion algorithm to match the data. Okay? So, that's the calibration is you're trying to match the matrix saturations, moisture potential, other observed data, using that flow model and then optimizing, if you will, the uncertain properties.

SAGÜÉS: Okay. So, in other words, you affix some kind of a theoretical model to the data?

HO: In this case, a numerical model, TOUGH2, right.

SAGÜÉS: I see. Now, the input--as important as the data are the assumptions used to massage the data to get the abstractions that you're going to put upwards. What is that particularly; namely, the ideas and the theories that go into the model? That's a fixed thing in that particular scheme, I presume?

HO: Yeah, and we have different ones. And, again, that's the first facet that I talked about traceability. What are the different models, the decisions, and the assumptions that go into the theoretical development of the models that we use? Now, there are calibrations not just for our base case, but we have sensitivity type models that have alternative conceptual models of, say, fracture flow. So, that is documented.

SAGÜÉS: Okay, I'm sorry. What I mean is there is an arrow coming from the left there into TOUGH2 calibration and UZ flow which is coming from the left side. It's not shown there in this screen. It's the dark part. And, that these theories, hypothesis, and all that, right, that is sort of like a static thing. That's been done once and that's not done anymore. Is that correct? Am I looking at that correctly?

HO: Yeah, I believe so. What I'm interpreting is you're again addressing a different facet of the traceability. The theory and assumptions that go into this is another facet. It's important, but it's not part of this framework of the data information transfer between models. I think it's more appropriate. This has come up several times and I believe it's come up several times because in the definitions, the more formal definitions of traceability, it is not just data and information transfer, but it is the model and assumptions. I think those models and assumptions can better be explained in a document when you describe what you did. I think, it's more transparent. This is a framework for insuring the control of--

SAGÜÉS: The word "calibration" is what sort of throws me there. It's fitting it to a model, isn't it? I mean, you're making a numerical fit to your data. You're assuming --you have some assumption and then you're finding the model parameters that feed the datas. That's where that is--

HOXIE: This is Dwight Hoxie with USGS. Let me just elaborate a little bit. According to our quality assurance program, the way that this should happen in principle is that most of the scientists have something called a scientific notebook which is a very much controlled, reviewed document in which they can make daily, hourly, minute entries. So, the assumptions and all of the processes of the calibration, all the iterations that they might go through should be explained and documented in those scientific notebooks. Then, those can be translated into our formal reports that are qualified reports that then can be distributed and documented for your consumption.

SAGÜÉS: No, what I mean is over there in the calibration--suppose that one of the things that is needed for the LA eventual model is, say, porosity as a function of XYZ. Every point in space needs to have a porosity, for example. Is that where--TOUGH2 calibration and UZ flow where that number is obtained or is it farther up?

HO: I think I can probably take this one. For porosity itself, that is not a calibrated parameter, but rather it is used based on raw information. So, that would, more or less, come from the raw data submittal. That is then included in the input files that they use for the property fields for their UZ flow fields and then that can be used from--the porosity can be used from those DTNs. Now, let's take a calibrated parameter--

SAGÜÉS: But, someone has to interpolate those values, for example? Where is that interpolation made? Interpolated, extrapolated, etcetera. You know, you get porosity measured here and porosity measured, say, half a mile away, and then you need porosities in between. Where is that in between, the definition?

HO: Well, I think when you research interpolation of property fields, we're talking about a geologic framework model and that is also inherent in the UZ flow model and that was included in that table that I had for describing how is that property distribution used in the TOUGH2 flow modeling? But, that is in there. A lot of that information is all down here.

SAGÜÉS: So, what is then going into TOUGH2? That's what I cannot understand.

HO: Okay. I think I can answer that. Simply, once you have all the information to create an input file, you're running a numerical model. You need an input file that describes all the properties for all your different elements, the structure of your grid. Okay. That input file creates, if you run TOUGH2, an output file, a UZ flow field. That has to be controlled and that's what I'm describing by the handoff from here is when you create those flow fields from all that supporting input information that that output--just simply those output files which contain information on mass flow, spatial mass flow, saturations, that is submitted to the technical database and it has a unique Data Tracking Number. Now, also what's submitted with that output file is the input files themselves. So, those input files for the TOUGH2 runs, for someone who is familiar with TOUGH2, they can pull out and say, okay, I know what the porosity is of each of these elements, I know what the different values of the different properties are.

There's still maybe some confusion and I'd be happy to--

SAGÜÉS: Well, it looks like you have one or two blocks too many there. That's the problem. So, we can--let's keep going.

HO: Well, actually, the only thing left is the summary. I also had to give it a lot of thought in terms of how you break out these blocks. Even though there is only four shown here, it can be confusing.

So, in summary, I believe there is a framework that currently exists to provide traceability of our TSPA-VA calculations. I've tried to do that with this example centered around the unsaturated-zone flow. Although we're not all quite there yet, we are striving towards the traceability for all the TSPA-VA components.

BULLEN: Thank you, Cliff. Questions from the Board?

(No response.)

BULLEN: Actually, I'll jump in here. When will this be available to non-project participants like the TRB, but also maybe the general public because at some point there's going to be interest in finding out, you know, to the smallest bit of minutia how you guys came up with the results that you did. Is there a plan that--

HO: I'm going to turn that over to Claudia.

BULLEN: Okay.

NEWBURY: Coincidentally, I got a e-mail last night answering your question. Can you put up the home page? We signed off to put out on our Internet Web site the ATDT, the tracking system that has a listing of all the data that's been collected; the GI, geographic information database which is all the spatial data, the maps, the--actually, there's some satellite photos in there, too; it's kind of nice--the SEP, the site and engineering properties database; and the requirements traceability network over on the left. The datasets in the middle are in-process. This is where we're storing things as we develop those particular documents. They'll go out later as the documents are released. And, the other ones, I don't think we've decided on yet.

BULLEN: And, Claudia, who can access these data?

NEWBURY: As of, I hope, mid-July, they will be open to the public, those that I mentioned. We're working with your staff to make our whole Intranet available specifically to the Board.

BULLEN: Okay, thank you. Other questions from the Board?

(No response.)

BULLEN: Panel?

BAHR: Cliff, if you put your flow chart back up, maybe just to try to address Alberto's questions, I think what you probably need is a double headed arrow between the hydrologic properties input parameters in your TOUGH2 calibration because it's really an iteration between model runs and that interpolation process of the properties where those property values get updated. I think by making it look like it's a one-way street there, that may be what's causing some of the confusion.

HO: Thank you.

BAHR: Is that an accurate assessment of the process?

HO: I think there is some iteration, but I'm not sure if we go back and dip into the raw database so much. Once the properties are--

BAHR: You have a raw database of properties. But, what Alberto was talking about is you don't have those properties continuously over the whole system and you have a certain amount of uncertainty in those in the calibration process. What you try to do is match outputs that have been observed at particular places and the best way to do that is to update your interpolations or to vary within your range of uncertainty the values of your parameters like permeability.

HO: Okay. But, I don't want to confuse the calibration effort with what defines the framework of our model; what I'm calling the geologic framework. There is studies--perhaps, if it's down here or another box--that uses these discrete raw data and interpolates and interpolates onto a stratigraphy for that information. Once that's established, then that is used here for the calibration.

BAHR: I guess, if that's established and that's never changed, then that does beg the question what are you calibrating or are you calibrating or are you simply running

TOUGH2?

HO: The calibrations are to optimize the results such as matrix saturation potential, perched water--

BAHR: And, what parameters do you vary in the process of the calibration?

HO: Right. And, I didn't really elaborate on that, but we vary or estimate parameters that we deem to be uncertain. For the base case, for example, those parameters that were highly uncertain for the dual permeability model are the fracture matrix multiplier term. This is a conductance multiplier that modifies that fracture matrix conductance because of heterogeneities that cause fingering or channeling or fracture coatings. And, I didn't, at all, get into this and this probably wasn't touched on yesterday much. But, that is an uncertain parameter. What is the reduction? Is it a tenth, a hundredth, 10^5? That is an uncertain parameter that we use as a fitting parameter in the calibration to optimize the match between predicted and observed measurements of, say, saturation, perched water, moisture potential, air permeability.

BULLEN: I think we went beyond the scope of traceability here when we're talking about calibration and datasets. So, maybe that could be reserved for an offline conversation.

Any other questions from the Board? I think, Jeff

Wong had a--

WONG: I just have a quick one. Cliff, are you going to have a database that basically catalogs or maintains a record of all your runs, historical runs? You know, you're going to have some runs of your models which you reject for whatever reason and then you're going to have those that you keep and put in your report.

HO: This is an area that perhaps I'm not that familiar with. Maybe Claudia can speak to it. But, in terms of the different pieces, I think for the actual RIP calculations that that may be used in this framework. Any and all runs that are used are submitted; if they're rejected, they're tagged as such. As far as the individual components, say, this UZ flow, if I have a run and I end up not using it, the cataloging or the archiving of that flow field from my little world of the entire VA calculation, I'm not sure about how we're going to do that. I do end up submitting all flow fields that are used, but for those that are not used for whatever reason, I believe we would document things that we reject, but I'm not sure if we actually archive those actual files.

BULLEN: Claudia, do you have anything you want to--

NEWBURY: Yeah. It's a difficult question because of the volumes of data that you're going to generate. What we've tried to do is say we have a controlled software package. We know exactly what TOUGH 2 is and the volume diversion that you've run. If you document your various runs, that they were, and we have all the inputs, then we can regenerate any output including one of the ones that you rejected. So, we don't have to necessarily keep them, but we can always regenerate them.

WONG: Yeah, I make that comment because it's pretty complicated and you have historical records as to runs that you rejected and why you rejected them. Then, others who come along after you won't repeat those runs.

HO: Yeah. I mean, that's a good point, too. I say that we are actually required to describe in our VA document, I believe, why we chose what we did and why we chose not to include what we didn't. But, it may at this stage just be a simple description. We considered this type of a conceptual model, but we didn't include it. Now, what I was trying to address is, well, for those that we considered, do I actually take those reams of files and actually store it somewhere on the technical database, as well, or is it sufficient to say here's the conceptual models that we considered, and because of these reasons, we rejected it.

BULLEN: Other questions from the Board?

(No response.)

BULLEN: From the Panel?

(No response.)

BULLEN: From the staff? Victor?

PALCIAUSKAS: Just one brief question, but it's probably not dealing with the passage of data there. But, I think it's more perhaps higher level or appears elsewhere. Where will the information appear, for example, that explains how the decision was made to--or were these, for example, curves that came out of the UZ travel times. For example, what were the key decisions and what were the key data that basically forced you to come to these travel times?

HO: That facet is the first thing I talked about and that is how do you know what we did and why? Okay. It's also a little bit redundant with transparency. What we did and why that justified that curve of travel times.

PALCIAUSKAS: Where will that information appear?

HO: As I mentioned before, I think that that's more appropriate in the actual VA document in those chapters for either the technical basis document that is associated with the VA document that helps describe and elaborate that information that is on the graph. As far as this process here, this is more for the--you understand it's for the transfer information. But, it's a very good point and again that's why I from the onset illustrated what I perceive as two main facets. The one you just described is, I think, that first facet of the decisions and the assumptions associated with the models. And, I hope that that can be described in a transparent manner in the VA document.

REITER: I thought when Claudia--maybe I misinterpreted that those kind of information would be available in hypertext version. In other words, you could scan the document and, say, you come upon something and you can trace back to what the assumption--the kind of thing that Victor wants. Is that correct?

NEWBURY: That's correct if it is included in the document. We've given direction to the authors that they will include that information. So, when the VA document is complete and they say, well, it's in my calculations in this report, X, and this is where I've described my process, you'll hypertext link to that other report and you can read it and judge whether or not they--

REITER: When will that be available?

NEWBURY: Well, the hypertext version of the VA is supposed to come out three months after the VA itself.

REITER: Three months after TSPA-VA? In other words, if they're coming out in June, you're talking about September?

NEWBURY: No, no, no. The official VA documents comes out when the Secretary signs it sometime after September 30. On September 30--

REITER: In other words, we won't have access to the hypertext version until--

NEWBURY: Until after the VA is officially released. It will be on the Internet. We can talk off-line.

BULLEN: Other questions from the staff?

(No response.)

BULLEN: Thank you, Cliff.

I'd like to stay ahead of schedule. So, we're going to take a break until 10:30, and we will reconvene then.

(Whereupon, a brief recess was taken.)

BULLEN: Before we start, I wanted to ask Abe Van Luik to make a couple of comments about chlorine-36 in response to the question that was raised in the public comment session this morning. So, Abe, could you enlighten us a little bit about the project issues related to chlorine-36?

VAN LUIK: What was it someone said this morning? Take your seats and put them in a chair. This morning very early, I thought, there was a loud ringing noise in my room and it took me a long time to figure out what that meant. When Alf Wikjord was talking about the chlorine-36 problem in some international programs, a bell went off, but it took me a long time to figure out what that meant, too.

What it meant was that three years ago, we had an official inquiry from UK/NIREX saying why is that in our PAs we have chlorine-36 as a dominant dose source and in yours you don't even see it? At that time, we did kind of a comparison side by side of where the chlorine-36 came from, etcetera. What it turns out in their calculations and I'm not sure if it's that way for everyone, but chlorine-36 is part of the spent fuel, but it's not part of the high-level waste to a greater extent than it is in ours. Where they find it is in their intermediate level waste which is the holes and end caps and the hardware.

The thing that we decided is that they create their cladding under an argon atmosphere and it incorporates argon. Chlorine-36 is an activation product of argon. And, as far as we could tell, U.S. vendors do not create their cladding in the same way and that's the reason that we don't see chlorine-36 to the same extent that at least UK/NIREX did. We did that about three years ago.

BULLEN: Just a quick side question. Do you happen to know the inventory of chlorine-36 in the characteristics database for 70,000 metric tons? Is it on the order of a few curies or tenths of curies; off the top of your head? You've probably got that one memorized, right?

VAN LUIK: No, I don't have that one memorized. I can look it up, but I--

BULLEN: Well, actually, I can, too, when I get home. I just thought I'd ask.

VAN LUIK: I'm sure it's a few curies. It's not just tenths of curies.

BULLEN: Okay. Okay, thank you.

We're going to allow some time for a summary of TSPA-VA and that presentation will be made by Holly Dockery and it will be followed by comments from the NRC. But, since we're 15 minutes ahead of schedule, I'd like to stay that way. I'm going to introduce Holly who is from Sandia National Laboratories to give us her perspective in summary of VA.

DOCKERY: I'm going to give just a really brief summary of the TSPA-VA results just so everyone can go home remembering what it was that you've been hearing for the last day and a half. I'm also going to have a little bit of a discussion and some of it may try to address something that Dr. Parizek asked me early this morning which is where are we going to specifically be going for the LA in terms of gathering new information. That wasn't something that I originally thought about talking about, but I'll try to address that briefly and punt any questions that you have in detail on site or design to Dwight Hoxie for site and Vic Dulock and other folks here from design.

Just to refresh your memory on what the expected case results were in terms of dose. And, again, as Abe has said earlier, this is something we might be concerned about if this was the license application, but what we're using this for right now in the VA is directories where we need to go and what information we need to gather. So, 10,000 years, if you looked at--if you recall back to Bob Andrews' talk where we were looking at the dose to the average individual, then the expected value for the peak dose there is .04 mrem/yr. The 5 percent case, the--again if you take it off the CCDF from Mike Wilson's talk, it showed the CCDFs for probability of the various dose rates, 5 percent was about 0 mrem/yr or exactly O mrem/yr. The 95th percentile was .85. That wasn't plotted on this. At the 100,000 years, again this is from Bob Andrews' talk, the expected value is 5.3 mrem/yr which magically very closely replicates what Bob did in his little hand calculation. Again, the 5 percent is about O and the 95 is about 210 mrem/yr. And, finally, for the 1,000,000 year dose, we're seeing some large values. The expected value is 300 mrem/yr, the 5 percent is .071, and the 95 is on the order of 1000 mrem/yr. So, that's where we are in terms of the dose calculations.

Perhaps, more important, is what we came up with from the uncertainty analyses because the VA is a springboard for what do we need to do to get to the LA. Well, the five most sensitive parameters in all the regression analyses, I'll refresh you from Mike's talk, were the seepage fraction or the number of waste containers that are contacted by seeping water, the corrosion rate of the corrosion resistant material, the number of juvenile failures, saturated zone dilution, and the percolation flux. And, again, I'll remind you, as Mike said, this includes the uncertainty, as well as the impact on the final dose values. So, these tell us not necessarily what we'll find to be the most important parameters in the long-term, but they give us a lot of direction on what we need to be addressing our studies over the next few years because here, if we can reduce the uncertainty, we'll have the greatest impact on our final results.

I was asked by Claudia to say what is a juvenile failure exactly. Exactly, it's sort of exactly anything that--the containers that fail early for things other than corrosion. It may be mechanical defects, it may be that somebody dropped that container really hard when they were placing it on the pedestal. There was some reason that the container got a breach in the container not due to corrosion. And so, this happens--is forced to happen at about 1,000 years for that juvenile failure. Does that explain it any better? Okay.

SAGÜÉS: Excuse me, when you identified the five most sensitive parameters, indeed, you indicated the importance was a combination of the increasing importance of the parameter and the uncertainty of the parameter. The cladding corrosion is not in that list. Do I understand correctly that it is not in that list because they assume uncertainty in that parameter was relatively small? Am I interpreting that correctly?

DOCKERY: That's a good question for Mike, I believe.

WILSON: Yes.

DOCKERY: Yes. Mike Wilson says absolutely.

SAGÜÉS: Okay. And, the sensitivity, just to refresh my memory here, is done by looking at the upper 95th percentile, say, the 5th percentile of the 95th percentile of that particular parameter, for example, and then seeing how the curves come out? Is that--

DOCKERY: In this particular case--

SAGÜÉS: That's how that sensitivity was evaluated?

DOCKERY: Yes.

SAGÜÉS: So, you have a parameter that has very little uncertainty. Then, of course, the result is independent.

DOCKERY: Exactly. And, that's why we're not relying exclusively on the uncertainty analyses to help us determine what are the most important things to study. This is one of our guidelines, but as Mike also talked about, we were looking at the "what if" scenarios and we're looking at one off studies and there are other methods that we're also using to look at how we would go about testing for the next set of the VA. This is one method for which we will come by this. There's also reason for us to be looking at these because we believe that--in particular, in some of these areas like the saturated zone dilution and the CRM corrosion rate, as we get more information, we think we may be handling those relatively conservatively. So, they may be areas where we would get better performance if we had more constrained information.

Just in case you wondered if we were all going to go home and sleep for the next three weeks in the PA team, I would like to assure you that that is not the case. What you've seen in the last day and a half is basically what will be in the VA. There are a lot of things that we would like to do, a lot of ongoing information and collection activities that will be incorporated into the license application, additional "what if" studies that we would like to perform. But, basically, what you've seen is what you'll get in the Viability Assessment. We're in the documentation phase right now. We had to complete our VA documentation for review and that basically is in a freeze date of May 15. So, again, the basic math says we'd better be writing two months ago. So, that's what we'll be doing is looking at these results and trying to interpret them and we may have some time to do a few additional studies, but mostly we'll be writing. We have internal review comments. One of the real important things that we're going have to do in the next three or four weeks is make sure that whatever recommendations PA makes to design that we have that incorporated into the LA plan section of the VA document.

For those of you who are not familiar with what the VA document is going to have in it, this is just the total system Viability Assessment document. There is a first volume that we've heard talked about that has the site description and introduction and other information. Volume 2 will be the design information. Volume 3 is where the total system performance assessment will sit. And then, very important, the Volume 4 is the license application plan and costs. That's what the program has decided needs to be done for LA will be captured in a gross sense. Then, Volume 5 is the cost to construct and operate the repository.

Then, the next stage of what we're going to be doing in PA is writing what we call the technical basis report, the technical basis for PA. Whereas the VA document is going to be the 300 page summary document of what you've heard, we also want to capture every nuance, every data point, every conceptual model, every sensitivity analyses. So, whereas this is the 300 page volume, this is probably the 3,000 page volume. That's due over the course of the summer. So, that information will be coming out and we'll be going through formal review processes for that, as well.

We're also scheduled to initiate review with the PA peer review panel. That's going to be a very important part of synthesizing the information that we have and seeing what

Chris Whipple and his crew of merry men believe are most important goals for the total system for the license application. So, as soon as we have our TSPA-VA and technical basis report through our review process, we will start to work with them on what we need to do for that.

We have a plan to address QA issues and I have another viewgraph on that to talk about that in a little more detail. We will work on modifying the TSPA-VA document for public forum which is addressing one of the questions that Bob brought up in his talk and Abe brought up in his talk about trying to take this information and make it more readily accessible to the public. The DOE is actively working on methods to do that and we expect it will be supporting them in some fashion, although we know that PA analysts are not perhaps the best people to draw the final diagrams for that forum. Then, we're going to start the activities for the abstraction/testing plans. We expect to go through another round of workshops and start this whole process for the LA.

The TSPA-VA volume--I'm putting up an outline that we have that's on the story board right now. It will show you somewhat of the contents that are going to be in the VA volume. This is the first part of the outline. I'll put this one over here so you can see the whole outline. It will be a very high-level overview of, first of all, what is PA in general; second, what did we do specifically for the total system performance assessment for Yucca Mountain; what we the results; what did we do for each one of these components or the type of presentation that Jack Gauthier and Mike Wilson and David Sevougian and Jerry McNeish made for the various components; and then, we'll have a summary and discussion that talks specifically about what we in PA believe we need for the license application.

Just for your knowledge and because it actually came up because--and Paul Craig is gone, but when we presented to the PA Panel just a few months ago, we tried to do it in this order, but we presented the results first and then we presented the components. That failed abysmally because everybody wanted to know the details of why did you make that assumption before you got to the results. And so, we are going to request that the DOE think about switching it so they can talk about the component models first and then talk about the results second.

Now, the details of the technical basis report, this is where the majority of the information on the specifics of the components is going to occur. We will have just a brief introduction to what we found out in the TSPA-VA, but then we'll have the very detailed information on the components. We'll talk about what other models have been done before, what is our current approach, what kind of data quality traceability did we have for these components? Then, we'll go through and talk in detail about the characterization of the component, how do we analyze this component in the TSPA, what was the base case, what were the parameters, the results, the interpretations, the sensitivity studies, and then the summary of the methods and results of what that means for repository performance for that component and what guidance does that component give to the license application.

Then, we're going to have a synthesis of the models that were in the TSPA. So, this will be the details of RIP and the details of how we incorporated that into the TSPA model. Then, what information do we think is needed for the LA? Whereas in this section here, we're going to be talking about all of the details of what each component thinks they need, that still doesn't give you a prioritization. That's doesn't give you a relative ranking of what do you--you don't need everything in geochemistry to the same level as you need everything perhaps in thermohydrology. So, how do we prioritize that information based on the TSPA results?

Okay. To kind of go back to some of the things that Cliff Ho was talking about in terms of the QA program, there's a very active effort that's underway right now and Rob Howard and Joe Shelling are working hard on helping us bring the performance assessment group into a quality assurance program. I know that there were some specific questions that the Board had on what are we doing for quality assurance and data traceability. There is a plan in place to develop a phased approach for getting QA into PA that is underway right now. The documentation of the requirements that are going to govern PA has been completed and is in a formal--has completed a formal review. One of the interesting things that has come out of that is there have been two vertical slice reviews where contractors have gone in and looked from data to TSPA models. Actually, it was the other way. It was from TSPA models down to data to find out if they could pull a string on one end--as people would say, does it pull up all the way down to the data. Well, we found there were perhaps a few places. It wasn't as traceful as we might have liked. What we did out of that is we had a "lessons learned" meeting here in Albuquerque just a couple of weeks ago and the results of those findings were presented to the PA group and we're developing a checklist and we're developing a way to handle some of the problems that we found immediately. We also are going to springboard off of that, and by the time we have a list of procedures in place to govern PA, it will incorporate some of those checklists in the procedures. So, that will be formalized beyond just the good will approach that we're talking right now. Software qualification and configuration has been initiated. We do have several PA codes into configuration management. There are several more to go and that's ongoing.

The implementation effort that we are going to undertake will be documented in the summer of '98. So, all of the issues that were developed from the vertical slice, all the issues that are outstanding for code configuration, all of the issues that are outstanding in terms of documenting analysis and conceptual models will be documented and a plan to address these issues will then be developed. So, we will know basically what all of our issues are based on this document that will come out in the summer.

I stole this from Bob again, too, so I could back through them. The QA issues are an important thing that we're going to address, but the other thing is what are we going to do in terms of model and information development for the LA? Where do we see the information coming and what kinds of things do we expect to incorporate in the LA? So, climate and infiltration, we don't see a lot planned right now in the site program and there are some things that we think that PA would really like to have. Those are things like information on whether the three climate states we're using are appropriate and the way we handle those climate states. We mentioned do we need to have a different form other than a step-function or is there something better that we could use for that? We need a better definition of the

Super Pluvial. So, that's kind of our worst state scenario. We'd like to know if we're handling that correctly. And, we would like to see appropriate analogs for the vegetation and temperature and other vegetation states than we might have right now. For infiltration, we would like to have an improved model that also takes into account some of the variability that occurs at the site like the vegetation, the soil cover, the slopes, and the associated uncertainty with that.

For the seepage, there are more niche tests in the plans and right now that's one of the areas where we feel fairly good because the modeling and the data that came out of the niche test to date were reasonably closely applicable. Now, the question of whether we'll get the type of information we need for the seepage test is something that we need to address ad we'd also like to have maybe some analog studies at Rainier Mesa and Apache Leap so we can look at seepage in those places.

The thermohydrology, there is a drift-scale heater test planned and we expect to get information. Basically, are thermohydrologic models correct and are we handling matrix fracture coupling correctly? So, we're looking forward to the information that's coming out of that. We think that will help us with our thermohydrologic drift-scale models.

For the waste package, there's a lot of tests planned. There's a lot of tests ongoing. The C-22 test for both the generalized corrosion and for crevice corrosion is ongoing. I believe both of those are at Lawrence Livermore. So, we will get more data on C-22 specifically, although we'd like to have an analog unless Eric Von Damm (phonetic) can come up with something from an alien invasion. We don't think the ancient Romans used that much. So, we don't have a good analog for that particular material.

The waste form, the cladding, we'd like to incorporate the information that is currently also underway. There are several tests that are going on for cladding degradation, waste form degradation. We would like to see a little bit more information on the secondary phase development for the waste form because we think that might be an important conservatism in our model right now that we don't allow secondary phases to form and decrease the early rates of dissolution.

To kind of jump to the saturated-zone, there are a number of tests planned in the saturated-zone. They will be putting in additional C-well tracer tests. There are going to be more drillholes putting in with geohydrology--or hydro-geochemistry coming out of those holes. One of the things that hasn't been decided, according to Dwight, is where exactly will some of these tracer complexes in the future be located? We would like to argue for something further to the south in the 10 to 20 kilometer range to the south so we can get a better understanding of what's going on in the saturated-zone. We'd like a little bit more geochemical information on the alluvial aquifers that we'll be going through so we can handle the uptake issues there.

So, those are just a real brief hit on where we think we're getting information. The detail of information will be coming out in that LA plan. So, that's really the place that the Board should be looking for the information on what the project has decided.

If we go back to the first question that you asked, we'd kind of put the question to you on how well did we do on traceability and transparency? I wanted to refresh your memory on sort of what the questions were. Hopefully, through all the different talks you've heard, we've identified the relevant processes for the key components and talked about how they can address long-term performance. We tried to identify the models that corresponded to the key components and how these models were interconnected. Most of that happened in Bob's talk. We need to identify the data in the model that forms the basis for the model and the traceability talk that Cliff gave showed how we're trying to trace back to the information that was given for those models. How the information flows from one component to the next in generating total system behavior? Again Bob was trying to address the connections in his first talk. The results of the component and the total system in physical terms and then producing a simple calculation that elucidates the key aspects. And, Bob showed you his little hand calculation and was trying to get at here's a very simplistic representation of our model and here's how we would address that in a basic one-page mathematical formulation.

So, I have the question open for the Board now. How well did we address these questions because we're in the middle of writing this up and we need some--we would appreciate additional help. Paul gave us some help yesterday on how we could enhance our graphics. But, is the TSPA, as we presented it to you, an effective tool for assessing the safety of the potential repository? Would this TSPA generate confidence? How well did we do in our presentations for the assumptions, the information, and the results? And then, finally, do you have specific suggestions on how we can improve the process and the presentation?

And, you certainly have generated some of this information already during the course of the talks, and we're taking all those things under consideration. But, as you sit there and have seen the whole fleet of talks, we'd like to ask you to give us any feedback on what very specifically can we do better.

BULLEN: Thank you, Holly. Do you want us to take these on one at a time right now or are we going to be able to think and cogitate about this for a while and get back to you? Before we do that, I'll ask the Board if there are any questions of Holly?

NELSON: Hi, Holly. On the outline of the TSPA volume, there's some nomenclature that I'm not sure that I consistently understand. One of them comes to the point in the sensitivity analysis part which is currently under results. It deals with alternative models and design options.

DOCKERY: Okay.

NELSON: We were presented a couple of weeks ago with the terminology that a design option might be something that in the past we had considered to be an enhancement, more like a plug-in model, a plug-in element. Like when you're buying a car, if you want AM-FM radio or something, you can have it.

DOCKERY: That's an option these days?

NELSON: Evidently. Well, all right, CD, I don't even know. I don't have a car. So, I don't know these things anymore.

But, the sense of what is an alternative model, does this represent a conceptual model of the repository or is this a model which represents a component of the analysis?

DOCKERY: In terms of the alternative models, the way we're thinking about this is we presented the base case. This is how we anticipate the repository will behave. The base case that Bob presented you had the reference design and the natural components within a certain range of expected values that we sampled off of. That's our base case. The sensitivity analysis are basically taking exclusions on the base case. In the case of the alternate models, we generally think of that more as taking the ranges of values or the "what if" scenarios for the reference design and the reference natural system, if you will, and expanding the range, changing it, doing the sensitivity analyses, as you were saying, in the subsequent talks. The disruptive features of events and processes are kind of like an--those are like the add-ons, but we take it and we change some major--or we add on some major aspect to the base case. The design options are the things like adding the backfill, putting in the drip shield, adding ceramics, other options that, as you said, might have been considered enhancements in the past. And so, we're taking those and adding those into the base case and trying to find out what kind of performance we might expect to get if we did those add-ons.

NELSON: So, I guess my problem probably comes with the word "models" because I think the way you've explained it, it's more of an input parameter characterization trying to include the uncertainty associated with that as opposed to a model uncertainty which is also a viable uncertainty. Will model uncertainties be considered even in terms of--I guess, one thing that came out here in the discussions was the distinctions between the different kinds of models that have been used for the hydrogeology in terms of equivalent continuum, dual porosity, WEEPS, and when are they plugged in and when are they not plugged in. And, that's a potential source of confusion, I think, for people trying to rapidly grasp what's been going on. But, in some cases, they are, indeed, alternative models.

DOCKERY: Yes, they are models. So, it could be alternate models and/or parameter distribution.

NELSON: And, I suggest that if you're going to do both that you separate them because they're pretty significantly different.

DOCKERY: Thank you.

BULLEN: I want to follow up on our semantics here. We also learned that alternative designs would be new design paradigms entirely different from the base case; lower areal mass loading, ventilation, self-shielding, those kinds of issues. Will that be post-VA where those would be seen?

DOCKERY: Yes.

BULLEN: So, would they be in the--how do we get to licensing document then, as you looked at your table of contents here? Where would we look to find those, I guess is my question.

ANDREWS: Let me try to answer that. There will be a chapter in the design volume that addresses alternative--now, these are major alternatives, as you alluded to, not the plug-in to your CD radio.

BULLEN: Not the enhancement. We've got to get to semantics again.

NELSON: Now, wait, wait, wait. Options?

ANDREWS: Options.

NELSON: And, alternatives.

ANDREWS: Options. Options will be quantitatively assessed. Alternatives will be described in Volume 2 and they will be described in Volume 4 and there will probably be some qualitative description of the potential performance benefits of those alternatives, but there won't be quantitative analyses of alternatives within the VA.

BULLEN: And, I didn't expect that, but the other thing that I might like to see is what challenges or changes or surprises in the repository would you expect to see that would trigger your use of these options? What would make you want to use these options as opposed to the base case? I mean, that's just a thought process that's another qualitative description. But, I mean, you can go ahead and say, well, we've got, you know, 19 other enhanced alternative designs, but why would you pick one? What are the things that--you're carrying these designs along because you might be surprised because there's something that you find in the characterization of the mountain that says, well, what we've got probably isn't the right design, but we've thought about that and this is what we would do. That's the kind of description that you'd like to see as alternative design paradigms--I guess, is the best way to call it--would be considered in the how do we get to the licensing from here--

ANDREWS: Let me try to answer that and then maybe somebody from DOE should probably follow up.

BULLEN: I mean, that's probably beyond what you should do.

ANDREWS: Because, you know, what design is actually used in the licensing and design alternatives that are, in fact, incorporated in licensing now; not Viability Assessment, but in licensing. Because even in the licensing arena, I think it asks for alternatives to be evaluated and assessed. The EIS asks for alternatives to be evaluated and it said reasonable alternatives, I think, to be evaluated and assessed. This is VA time frame. So, we want to look at options and we also want to look at alternatives. You know, and options, we looked at quantitatively; the alternatives addressed more qualitatively.

DOE and, in fact, everybody else who is a decision maker in this process because costs enter into this process, as well as performance related--you know, there's a cost benefit relationship that has to be evaluated. So, hopefully, there's enough information in the VA to look at costs of options, the performance benefits of options, so that decision makers--you know, DOE, Congress, you know, the President, etcetera--I guess, Congress can make decisions and how to move forward.

BULLEN: And, I agree with that. I guess, the question I would ask is that, you know, when the option would be applied, what criteria would you use that say, well, this is probably the best option if? It's another issue that may be beyond the scope of what you're expecting to do, but it's something that--I mean, sort of when you come up with an unlimited list of options, you look at them and say why would anybody want to do this, you know, kind of question. So, when you do a succinct list, the succinct list would include we would use this option if, kind of description.

ANDREWS: I think you're kind of getting into sort of a licensing strategy or--

BULLEN: Yes, I am and I apologize for that.

ANDREWS: --defense in-depth strategy and perhaps, you know, DOE would want to address that in their more general comments.

BULLEN: That may be beyond VA. Okay. Thank you.

NELSON: I'm not a PA person. So, this might be a little bit naive. I think I got the quote right from Abe yesterday that said that PA is not a quantitative, predictive tool. I'm wondering what you're going to present PA as?

DOCKERY: We've discussed the use of the word "prediction" and the wide-ranging belief that we can't absolutely predict the future. Probably, what it more closely is is a forecasting tool. Here's the range of probable futures and here's the most probable future that we think will exist. But, we're not saying we are going to predict the exact state of the repository system at any point in time.

NELSON: But, you do think of it as a quantitative rather than a qualitative or a relativistic prediction?

DOCKERY: Yes, absolutely. There's a lot of sensitivity to the word "prediction".

WONG: Holly, I have a few questions about the QA overhead. I'm trying to understand what you mean by the work underway to implement a QA program. Does this mean that you're trying to improve something that already exists or are you trying to put in place something that doesn't exist now?

DOCKERY: Yes and yes, I think are the answers. There is a QA program in place. The program that has been in place hasn't been as effective for the performance assessment end of things. Many of the procedures are written for data collection and both kinds of analyses and we found that the procedures don't well-capture the exact process that PA goes through. So, one of the things we're trying to do is capture the way in which PA does business and try to write the procedure so that it reflects that, but also provides a process that can be followed from beginning to end.

The software configuration management is something that had been underway, but for a long time we were still determining which codes we were going to use and now we are coming to the point it's time to get those codes that we now know for sure we're going to be using in the license application and make sure we have those in configuration management.

There are just a number of issues that, as we come closer to licensing and we start to actually look at how hard it is to make sure all the handoffs are maintained from data collector to process modeler to PA analysts and the same along the design, we're finding that there's some weaknesses in the program and we're just trying to make it a very robust QA program that works for the licensing arena.

WONG: So, this QA program is designed to benefit LA, not PA?

DOCKERY: That is correct. We are trying to implement some of the processes so that the VA will be a better document, but these recommendations--and some are obviously things that are direct pointers to what we need to do for the license application.

HO: I'd like to add something. In addition to what Holly said about a lot of the QA that exists that was not specifically designed for performance assessment is very true. In addition, on this project, there are again a number of institutions that individually may have their own quality assurance programs. For example, Sandia has a very robust software quality assurance program. The question is do we have a centralized QA program for the entire project? And, I think that's part of the things that we're working towards is how do we handle QA that handles all of the VA components and not just for individual institutions? So, the question about does QA exist, yes, it does exist in many of the institutions currently, and I think that we're striving to improve upon it for performance assessment calculations, for the entire Viability Assessment, or the licensing as a whole.

WONG: Well, the lack of this comprehensive QA program, do you think that compromises in any way the integrity of VA?

ANDREWS: No, I don't think so. I mean, part of the VA--and, I think, Cliff alluded to it--is to identify the Q related exceptions of the VA process, the whole process of generating the VA from design all the way through to performance assessment. Those will be documented, all of the Q related exceptions. And, I think that helps the project, you know, say, okay, these are the processes that need to be tightened up, you know, between VA and LA. So, I think, it, in fact, helps.

DOCKERY: I'd have to agree. I think it helps us fine tune the areas that we can put the most confidence because we are very sure of how all the information flows and it also helps us identify where we might be a little bit less confident. So, I think, it will give us a better basis for VA than we would have had before.

WONG: Okay, thanks.

BULLEN: Further questions from the Board?

(No response.)

BULLEN: Okay. I'm going to step on the Panel's toes here to keep us on time since my Board members got us right back on schedule. You're going to talk for about 10 minutes each in about a half hour. So, I'd like to move on to the next presentation.

We have a few comments from the NRC graciously provided by Keith McConnell, and he'll give us NRC's views on DOE's preliminary work in the TSPA-VA. Keith?

MCCONNELL: We appreciate the opportunity to come before the Board and present our views, and in true regulatory fashion, I've asked that the temperature in the room be raised by 10 degrees just so you're uncomfortable through the entire thing.

At NRC, I'm the section chief for what's called the performance assessment and integration section within NRC's Division of Waste Management. There are three activities that are ongoing within that section that have a direct bearing on our review of the TSPA-VA. And, that is the continuing development and maintaining our own independent review capability in the form of developing a total system performance assessment code that we call TPA code and also the conduct of our own independent sensitivity studies that have been ongoing and has served as a basis of some of our comments to date at the technical exchanges and here today. Second, we're developing acceptance criteria that we would use to judge any particular TSPA, whether it be VA related, LA related, or any TSPA and those are in progress right now at the staff. Finally, we're developing a site-specific rule for high-level waste disposal at Yucca Mountain and that is now ongoing and we hope to have a draft to that rule completed by the fall of this year ready for Commission review.

With that as some context for our review, I also think it's important to give you a framework for our review. So, what I'm going to do in the viewgraphs is, first, define our role, then tell you how we intend to implement that role, and then finally we'll get around to the comments. But, I think my briefing will go fairly quickly.

Basically, the Viability Assessment is a DOE management tool. It's going to guide the development of the course of future repository investigations and licensing. So, obviously, we have a high-level of interest in what's done in the VA. However, the Energy and Water Appropriations Act did not define a particular role for NRC in the Viability Assessment. Therefore, there's no statutory requirement that we review the TSPA-VA. However, we do expect that we'll be asked probably by Congressional sources to comment on the VA. And so, that is the basis for all of our work over the past year or so and our interactions with DOE. Our view of the VA, however, will be viewed in the contest of what is needed for licensing. We're not going to comment on the viability of the Viability Assessment. It's what's needed for licensing as we move towards that in 2002. So, that's basically NRC's role in the VA.

How do we intend to implement that role? Well, again, it relates back to the activities that have bearing on our review. We intend to provide the timely regulatory guidance as a good regulator should. We're developing the site-specific high-level waste rule now. Again, we expect to have a draft available sometime in the fall of this year for Commission review. Assuming they accept what we propose, that it would go out for public comment and we're shooting for a July of 1999 date for a final rule. Of course, that's a very short period of time for rulemaking, particularly a rulemaking that might be done in either the absence of an EPA standard or with an EPA standard coming out somewhere in the middle of our process. We're developing acceptance criteria. Again, acceptance criteria for how we would review a TSPA. That's going to take the form of the TSPA Methodology Issue Resolution Status Report or IRSR and we expect that to be out this month, at least the Rev. O of that. What we want to do is insure there's continuity between what we put in the IRSR, what would eventually be in a review plan that would probably evolve from the IRSR, and what would be in our proposed rule.

We're going to conduct all of our reviews, the VA, the pre-VA, the LA, and pre-LA reviews from a common basis. We're going to use our TPA code as it evolves as the principal post-closure review tool.

We're going to facilitate sound and timely decision making. The staff has taken on the Issue Resolution Status Report as a way to document the closure of issues. We're going to document the technical differences in uncertainties in knowledge. And, I think any of you who have attended any of our technical exchanges know that at the end of the exchange, we basically sit down with DOE and document the various technical differences. Those will be coming out in tabular form in the TSPA Methodology IRSR.

We're going to focus in our review of the VA on the total system performance. We're going to use a single overall performance measure. We're not going to look at subsystem performance objectives that now exist in the existing 10 CFR 60. We're going to use our TPA code to evaluate our own internal issues and sub-issues to make sure that we're focused on the right issues that relate to overall system performance. And, we're going to assess the contribution of individual barriers as they relate to the multiple barrier concept. Again, as many of you know, one of the underlying philosophies in NRC's approach to licensing is the use of multiple barriers and the individual component of particular barriers to provide defense in-depth. And, probably, most importantly, we're going to use the results to evaluate the need for additional site characterization or design information that should be included in a license application to make it robust.

Finally, we will use the review to assess DOE's progress on this path towards licensing. So, that's basically the framework of our review and again it's been derived largely through the interactions that we've had with DOE over the last year. And, I guess, I have to compliment the Board that they've been able to digest in a day and a half what it's taken us close to a year and three technical exchanges, basically, to hear from DOE.

But, one caveat I have on our comments is that these comments were derived from the technical exchanges. I think, as you've heard over the past two days, there have been a lot of changes in approach that have some impact on what we would comment on. We do believe that DOE has made significant progress in completing a very imposing task and they've been very flexible in responding to our comments that result from the technical exchanges. Again, our independent analysis has identified a number of positive attributes and a number of questions and I'll go through those now.

Some of the positive attributes, we believe that DOE is effectively using its TSPA to focus its site characterization activities on those aspects of site and design that are most important to performance. We give as an example the niche tests that are looking at the seepage flux into the repository.

We also believe that they are using their sensitivity studies in a way that's looking at the licensing vulnerabilities. That's something that we do and we believe it's beneficial to the program that DOE also do that.

Our approach to the consideration of the fractures and matrix in unsaturated flow are similar, and we believe it's consistent with the existing data.

We believe that substantial progress has been made in addressing the near-field. I think, as maybe Jerry McNeish pointed out yesterday, in TSPA-95, there was very little information on the near-field. We believe they've come a long way since then in their TSPA-VA. In our view, it's a fairly critical aspect particularly when you start talking about corrosion potentials.

We think that DOE recognizes again the corrosion potential of C-22 and it seems to be coming even more important with their approach to disruptive scenarios, seismicity, and volcanism. We think that it's going to be a key factor in building confidence in the overall analysis for licensing.

And then, DOE's approach to identifying the reference biosphere and receptor groups is consistent with ours, and we think that's a very positive aspect.

Okay. Some of the questions--and, again, this not an all-encompassing list. This is just basically some of what we felt were the more important issues that have been coming out of the technical exchange and we divided them into three categories; the natural barrier, the engineered barrier, and then integration and transparency.

Although we had an Appendix 7, I guess, a couple of weeks ago on matrix diffusion, I think there's still some lack of understanding about matrix diffusion, particularly in the saturated-zone and how much credit is going to be taken for matrix diffusion in the saturated-zone. We believe, based on our own independent sensitivity studies that this could be a key factor in the analysis.

We also question whether there's sufficient technical basis to support DOE's saturated flow and transport. Of course, as you heard yesterday, there's been substantial modification to that. It's now much more similar to what we're going. Again, in NRC's model, we assume basically a one-dimensional flow regime with no vertical or transverse dispersivity. It's only longitudinal dispersivity.

We questioned what the technical basis is for the alluvial K_ds used in the analysis. In our model, basically, we take no credit for matrix diffusion in either the saturated-zone or in the unsaturated-zone or the tuff aquifer in the saturated-zone. Therefore, the alluvium and it's porous flow characteristics become very critical to our analysis, and we believe perhaps DOE's analysis.

With respect to the engineered barrier, the use of C-22 as a corrosion resistant model, we believe brings some importance into how rockfall from repeated seismic events and juvenile failures--or we would call them initial failures--are considered. As Rally presented this morning, I think there are at least a lot of new information that we'd have to review, but at least we now understand that there is an approach to considering that. I guess, that would also play out to the approach used in volcanism.

We question whether there's sufficient technical basis for estimating the performance of C-22. Specifically, some of the waste package folks at the center question whether stress corrosion cracking has been adequately considered and also question the corrosion potential values that were assigned to C-22 from the expert elicitation.

I guess, one other thing I would add to this list that didn't make it is the question of neptunium solubilities. I think there's a lot of discomfort at NRC about the two order of magnitude reduction in neptunium solubilities--I think, Jerry McNeish may have suggested there's a scenario, to look at that, after the VA. So, that may be addressed.

In the category of integration and transparency, I think we've heard a lot of discussion for both of these during the meeting. We question DOE's approach to considering alternative conceptual models. I think, David Shoesmith mentioned this morning the issue of how, I guess, it was localized corrosion is being incorporated in the analysis and the conceptual model uncertainly, how it's being convolved in the overall analysis. I think we share that concern, and it extends probably broader to alternative conceptual models for other aspects of the analysis.

Then, finally, I think there's been a long-standing concern on NRC's part about how expert elicitations are used. Again, we've taken the position that we don't think they should substitute for readily available data collection. And, like I said, consistent with the peer review panel, read their report last night. I think they commented on that. I think the Technical Review Board has also commented on expert elicitation, in general. But, we would also question whether elicitation results are actually being updated when new information is being generated and also whether they're being properly incorporated into the analysis.

In the interest of time, I'll skip the last slide. It just is a summary. Thank you.

BULLEN: Thank you, Keith.

First, we want to take questions from the Board?

PARIZEK: How clear a signal do you give the DOE as to what's adequate and when is enough enough? Obviously, you have these issue resolution discussions and I guess it's clear from that process that, I guess, we understand and we come to the conclusion that we agree. But, in terms of the models and database and so on, is it going to be clear to them that they have enough information in the adequate quantities and quality before license application? Do they get a clear signal from you? In the nuclear power plant business, there's enough plants around that you can sort of say, well, what I'll at least do is do as I did on the last plan and I ought to be home free. But, as far as repositories, I guess the only thing we have to work with is

WIPP as an analogue, right, to this whole process?

MCCONNELL: That's probably the closest analogue, yes. I think that our goal is to when we get to the actual receipt of a license application that we know quite clearly where we are and DOE knows quite clearly where they are in the license application process in terms of before. That's the whole idea of developing these issue resolution, particularly in the TSPA Methodology Issue Resolution Status Reports, because that addresses abstractions and how conceptual models are being implemented.

PARIZEK: And, we've heard juvenile failures mentioned. You know, I dropped the package, I'm sorry, but that's maybe why it's going to fail. Will you folks indicate that someone has to inspect the waste package before it's placed and if it's got a dent, it's rejected? Is that something NRC specifies or is that a DOE requirement because I'd like to think that you didn't drop a package, and therefore, that's the reason for the juvenile failure. Maybe it's a manufacturing problem which is harder to control, perhaps.

MCCONNELL: I guess, I would beg off on that question. There are regulations for pre-closure and they are being developed as we develop the post-closure regulations for 10 CFR 63 which is the high-level waste standard. So, I assume there will be something in there, but again I'm not the person to respond to that question. We focus almost totally on post-closure performance.

BULLEN: Any other comments from the Board?

(No response.)

BULLEN: If not, Steve Hanauer wanted to make a disclaimer here and I'll allow that as my chairman's prerogative. Steve?

HANAUER: This is Steve Hanauer, DOE. I have to object to the statement on your viewgraph 2 that this is a DOE decision document. It is not such thing and the distinction is important. Some people may base decisions on the technical information in the document. The most obvious example that comes to mind is that Congress will or won't fund the project in fiscal year 2000. Nevertheless, there is no decision, no set of options set forth in the Viability Assessment. The decision documents are the site recommendation scheduled for 2001 and the environmental impact statement that goes with it scheduled for the year 2000. The distinction is very important because a DOE decision has a lot of baggage that a company says, whereas an assessment of viability--which is what the VA is and that's why we picked those words--doesn't carry and doesn't need to carry that baggage.

MCCONNELL: Well, I guess, I'd just respond I think we tried to qualify what the decision making aspect was and that was assuming you get approval from Congress and the President or whomever is going to review your TSPA decisions, basically, you're going to proceed in some fashion. Those decisions are the ones that we were referring to, not the over-arching decisions about whether--the Congressional decisions of whether you'll go forward or not.

HANAUER: Well, they make this decision every year. The VA was invented at a time when our budget was drastically slashed as a way to focus the program at a time when it looked like the site recommendation and the license application were many years in the future. It has been enormously useful to do this.

BULLEN: Thank you, Steve.

Any other questions from the Board?

(No response.)

BULLEN: Okay. Well, I'm going to go ahead and jump ahead and let Keith sit down because we're going to ask our Panel to provide comments and nobody looks like they're jumping right up. Could I start with you, Steve? Do you mind and we'll work our way toward me?

FRISHMAN: That's fine.

BULLEN: I'll do Steve, Chris, and then Jean. Is that all right? Do you want to keep it to like 10 minutes total so we can do this in a half hour? Is that all right?

FRISHMAN: I'll keep it to less than that.

BULLEN: Okay, thank you, Steve.

FRISHMAN: I mainly wanted to return to the question that I asked Bob yesterday about uncertainties in the climate forecast in the climate model. I think, it's a key one because you can find examples throughout the presentations from the last day and a half where what are essentially small differences in the climate model end up making pretty large differences in the overall dose predictions. I guess, as the beginning of it, you cite as the basis for your perspective climate model the past record which is of approximately the same length of time that you're trying to forecast. So, what you're doing is essentially taking a mirror image of the past record. I'm not sure that there's a really good basis for taking that essentially literally without understanding that there's probably a fairly large variability that needs to be accounted for, especially since the Seabed O-18 record doesn't necessarily reconcile with some local record.

Let me give you just one example of how what is essentially a small change makes a fairly large change. That's if you look--and, I'm not saying you have to; I'm just going to throw some numbers here--at the UZ transport presentation that includes dilution. If you look at the graph on Page 28 that's the 5th and 95th percentile dilution factors, if you look at the Super Pluvial that's out at about, oh, 350,000, that comes just shortly after what you're estimating as a peak dose throughout. If you move your projection of a Super Pluvial on the order of 50,000 years and look at the 95th percentile, you're going from a projected dose of about 1100 mrem/yr up to 1500 or 1600 mrem/yr and I think that's significant. It's one where at this stage of the game, people are going to be looking primarily at your ability to project, what your assumptions are in your projections, and ultimately what the predicted doses are.

Just as the question was asked yesterday, aren't you worried about 200 mrem, well, yeah, I'm worried about 200 mrem. I'm really worried about over a rem in a climate model where your own experts are telling you that there's--or appear to be telling you that they sense a pretty high degree of variability in what that projection ought to be. So, I don't know how you might handle that, but I think it's one where just as you have already said you can't predict future human activity, well, with climate, I don't think you can predict it anywhere as closely or as glibly as you appear to be doing it. From my observation here anyway, it has some real dose consequences that are up on the high end and I think will be particularly disturbing to other people.

Another point that I wanted to raise is when you're talking about options and alternatives, you seem to have settled in very deeply now on your interim standard. Is 20 km viewed in the VA as an option or an alternative or neither?

ANDREWS: The VA, 20 km is the expected distance. So, it's neither; it's fixed.

FRISHMAN: All right. So, does that mean that you're not going to do alternative quantitative presentations at like 5 km?

ANDREWS: We will do, in part. The EIS is doing some analyses at 5 km, 30 km, 50 km. I think some of those analyses will be brought into the VA, and based on comments we've heard yesterday and today, I think probably will be brought into the VA.

FRISHMAN: Because, first of all, I've heard at different times what the basis for the 20 km might be, but at the same time, there's absolutely no precedent for it other than that's what the Department decided they wanted to do. And, I think, from a standpoint of at least looking back at what was a result of at least public review of a previous rule--meaning 40 CFR 191--5 km, as the boundary of the accessible environment, was something that at least has precedent for having been reviewed by the public and whether everybody liked it or not, it went through a public process of having been accepted as a regulation. 20 km has no such precedent in anything that I know of.

Also, just in thinking, I know the standard is instructed to be site-specific. But, if you were dealing with a location, for instance, in New York or Pennsylvania, and you said we're not going to count people and exposures to people until we're 20 km away from the source, and if you said that in a room similar to this, I imagine the roof wouldn't hold on this building. So, just to put that in perspective.

Another thing that I find kind of interesting and I was looking at and I didn't try to calculate it through, but I think it has some fairly large consequences is if you look at the--or think about the question that was asked yesterday, what if you have all juvenile failures? I think that's an important question not because I think it's likely that you will, but because I think it's significant that just one juvenile failure that you have used as an assumption appears to make a real difference in the early portion of the dose curves. So, the question may be more appropriate; what if you had 5 percent juvenile failures? Five percent juvenile failures would make all of these curves look very, very different and would make anybody's consideration of the site probably very, very different just from a perception of, you know, what doses they may think is acceptable and may not.

I don't think it's out of the question to use some real percentage failure numbers because you're dealing with a lot of containers. Just for spent fuel, you're dealing with, what, 10,000 to 12,000 containers for commercial spent fuel.

If you look at such things as the quality of pipeline that is produced, yeah, the failure rate is way, way down there because industry isn't going to allow it to be anything worse than these very, very low failure rates. But, you're also dealing with things that are a little bit more complex than pipeline, things that require a lot of internal engineering to have everything right, and you also have to remember right now that we're sort of in the early ages of making dry cast for storage either at reactor or centralized facilities. The track record is not that good on the few that have been fabricated. There are, at least, a couple of them out there out of, what, less than 25 or 30 that can be considered juvenile failures. So, I think, you need to use a realistic number for juvenile failures, especially since you have such a large projected number of containers and we know that there are going to be failures and fabrication quality, I don't think in this business, you can assume is going to be near perfect. I think you have to make the alternative assumption until there's a track record that may let the assumption be altered at some later date.

I guess, the last point is I'm really concerned about the seep fraction. I'm not sure that I really understand the basis that you have for it. The whole performance assessment is extremely sensitive to it, and I'm not sure that from your projections from really what little data you have are meaningful at this point. If you want to look, for instance, at what must have been seeps, like I mentioned yesterday, the presence of chlorine-36 on fractures in the ESF, you see that they're largely concentrated to the northern end. Whether you were surprised or not, you really didn't see them in the southern end. More than half of the emplacement area is going to be in the northern end which extends north of where the current ramp is. So, in the space of about a mile and a half, you saw what obviously were seeps over the last 50 years and a number on the order of tens in only about a mile and a half and you're going to have a repository with maybe 100 miles. Just extrapolating out, I think, is an interesting exercise, but I don't think that it's the end of the exercise. I think, your assumptions about low seeps--for instance, in your layout where you flatten the draft of 45 meters, what do you have, three effective seeps in 45 meters? In some places, that may be realistic, but I doubt that's the rule for the volume of rock that you're looking at and I know you don't have data to support in the volume of rock that might be the repository. You have practically no data in that volume of rock other than one line down the east side of it.

So, that's some of the things that I observed over the last couple of days.

BULLEN: Thanks, Steve. And, this does not require a response. This is just comments from our Panel.

And, I'll move on to Chris who can have an equivalent 12 minutes if you want to get ample time. I'll give everybody 12 minutes.

FRISHMAN: I thought I was counting right.

BULLEN: No, you were two minutes long, Steve.

WHIPPLE: Like Steve, i'll try to hold it to less than 10 minutes with the best of intentions.

I've got four points I want to make. First, I'll try to go from kind of general to specific and programmatic to technical. First, I really want to compliment the program for the progress over just the last couple of months. I've heard the presentation on TSPA a number of times and not only is the presentation getting better as they have some experience and a number of people have commented on the usefulness of the graphics and so forth, but the technical improvements of parts of the analyses in a short period of time, I find really astonishing in a program this large and something that one would expect to be more inertial. They have not thrown an anchor to halt and do VA and to worry about new science and analysis next year. It moves ahead. I know from the reviews in trying to keep up with the program that our peer review panel has done, this gets to be a hard problem because the work is well out in front of its documentation, but that's our problem.

In particular, I think the colloids work which was a low point six months ago is much, much improved. Presentations I heard at this meeting removes a complaint from my next report on the absence of juvenile waste package failures. I'm glad to see those are in. The presentation that lets me see what happens for different climates states and doesn't Monte Carlo the whole gamish together, I find terribly useful. So, all and all, a real congratulations for nice work that's been done.

Now, you always follow up the compliments with criticisms. Two. As I mentioned yesterday, I'm not convinced that the uncertainty analysis is capable of really identifying the key uncertainties. The model is too complex a mixture of bounding and realistic analyses. In some cases, the true uncertainties are not included and instead a point estimate is put in. We heard several times the question of why cladding doesn't show up as a more sensitive parameter, I think is an illustration. I went back last night and looked at one of the overheads that was shown several times yesterday, the one having to do with neptunium solubility. That curve shows 10 data points for dissolution measurements of neptunium solubility. Only one of those data points falls within the range used in the analysis. The other nine are below that range. I suspect that somewhere an analyst is trying to be conservative. I know in the report that came out, Dave Sevougian talks about different chemical forms of neptunium and perhaps these studies have to deal with different forms--excuse me, it was Dave Sassani--had to do with different forms that we might expect to find in the mountain. But, those differences, I would think, would tend to drive uncertainties broader, the lower limit would be lower. The arrest code predicts lower numbers. So, I'm picking that to argue that the neptunium analysis is right or wrong; only that the starting distributions that give rise to the sensitivity analysis are in some cases probably over extended for some parameters, too narrow for others, and I think that in the end common sense has to be the guide to performance sensitivity more than what the mathematics tell you.

The third point on that. One of the parameters that did show up, I think, in second place in most of the sensitivity analyses that was clearly a high item in Keith's talk, NRC's list, is how C-22 performs. If anything is obvious from the presentation yesterday, that is where most of the isolation occurs. Therefore, I think that the work to assure that that part of the analysis is robust is far more important that some of the other aspects of the analysis.

I'm not picking on any of that work specifically, but one of the take home lessons for me, as I see estimates of large numbers of waste cans persisting out into the hundreds of thousands of years, it struck me in Rally's talk which is, I think, by 100,000 years or so either by intention or by seismic event, those drifts are going to be backfilled. If the corrosion model is really based on corrosion in an air filled drift and the processes and rates and events are far different in a backfill drift, that needs to be addressed. I know if was on the list to do as a sensitivity analysis, but it needs to be followed up the chain. Are the experiments being done at Livermore meaningful for the backfilled case if that's, in fact, the likely future situation either by design or by seismic event?

Okay. Fourth point and this has to do with kind of a real time comment on the SZ flow work we heard yesterday and that some of our Panel heard Wednesday afternoon. I do want to comment that we understand this work is very new. The ink isn't dry. I sure wouldn't want to show my first drafts to this room and they showed it to us. So, this is just kind of an online comment of technical thoughts that occurred to me in looking at that work.

The general notion of a dilution factor that has been adopted for the revised SZ analysis and in large part because of the critical comments from Al Freeze and Gelhar, may be an appropriate way to do it, but those dilution factors are clearly scale dependent. The amount of lateral mixing one would expect to get in a plume that's over 1,000 meters wide is clearly less than in a plume that's a meter wide. So, the choice of six streamtubes in the analysis, you have to adjust that scale for the dilution factors. I suspect that when you get through doing those dilution factors, doing the super position and addition of different concentrations back on top of themselves, what started out as individual dilution factors of 10 in each of those streamtubes probably works out to a much lower dilution factor for the whole plume which is what I think Gelhar had in mind when he talked about a dilution factor of 2.

In addition, I think the SZ treatment of a single canister failure, whether it's a juvenile failure or whether it's the stylized human intrusion calculation, is probably not suitable for the existing models. To take a single injection at a point into the SZ and to use a box model many hundreds of meters wide is questionable. Clearly, the dilution factor for a single injection will be much greater than for 1,000 meter wide plume, but I'm not sure the same tools are very much applicable other than the work to assess the general direction and locations downgradient that one wants to do the assessment at.

Those are my comments.

BULLEN: Thank you, Chris.

So, this means Jean gets 12-1/2 minutes.

WHIPPLE: Did I overrun?

BULLEN: No, you're fine.

Jean?

BAHR: The problem with going last is that a lot of the things that you wanted to say have been said by other people, but I'll repeat some of them anyway.

I haven't had the opportunity to follow this process as closely as the other two panelists, but I did see some of the TSPA-93 work back when I was serving on a National Research Council committee. And, I certainly appreciate the increased transparency and explanation that's provided in this version. There are many fewer figures that were presented where I had to say, well, you know, what were the assumptions in this, what do you mean by your base case. There's still a little bit of that in some of the figures that we saw in several of the presentations where some of the assumptions that were inherent in the simulation results weren't necessarily specified. So, I think, that's something that you've gone a long way with, but just to keep in mind for future presentations that it's important to specify exactly what is your base case scenario, and when you're doing sensitivity analyses, exactly what parameters you're varying and what parameters you're keeping fixed. But, it's certainly a lot easier to tease out interesting questions when you know what the assumptions are there.

I think that the sensitivity analyses that we were shown, several other people have commented on this that while they pointed to five most sensitive parameters, if you look more carefully at the base case scenarios, it's apparent that there are some things that are important in there that didn't fall out as the most sensitive parameters. That may be because there was very little uncertainty associated with those. But, just a few of the factors that are of interest to me that I'll mention. The first is the relationship between the dose peaks and the climate change that a number of people have commented on suggests that we need a lot more sensitivity work on the climate scenarios and, in particular, on the timing of the climate change, on the shapes and duration, how does climate change from one mode to another? Is it instantaneous or is it more gradual? Is a Super Pluvial always followed by a dry event or dry period? Is that realistic? What's the consequence of that assumption?

The second issue is that the volume of infiltrated water that actually contacts the waste is very important for the release of solubility limited nuclides like neptunium and it's not--it wasn't clear from the presentations what percentage of the total flux going through the mountain that represented. Is it half a percent, is it 1 percent, is it 5 percent? I think that those are important things to show to people. And, associated with that is what are the conceptual uncertainties in the models of infiltration? In particular, how would focusing a diversion away from the drifts in response to thermal effects perturb your assumptions of how much water is coming into contact with the waste? We've also heard that drift collapse needs a lot more attention. One of the things that that can affect is this focusing or diversion of flow and that's not built into the models. There's also a lot of question about the drift collapse timing; not only its effect on the structural integrity of the canister, but on the permeability distribution around the drifts. So, that's something else that's been pointed out.

I think the saturated-zone model that was presented with streamtubes is certainly more conservative than the version that I saw, just some overheads that was apparently presented about a month ago. But, there needs to be some better linkage between the revised zonation and the concentrations at the water table directly beneath the repository. There's some questions about how you calculate equivalent concentrations at the 20 km distance, and I understand that this is more work and that some of those issues are still being ironed out.

So, I think that's the sum of my comments.

BULLEN: Thank you, Jean.

BAHR: I think I took less than 10 minutes.

BULLEN: Oh, does Bob get to talk? I guess Bob does get to talk. Bob? I should have looked at the list. Bob, would you, please, take--well, let's see, you get nine and a half, I guess. We'll average Jean because she was short. No, you can 10; go ahead?

ANDREWS: I don't know whether to go point by point. I think all the comments we've received throughout the last day and a half including the ones just summarized by Steve, Chris, and Jean are all excellent comments. And, you know, this is a work-in-progress and the production of the VA itself and the documentation of the VA itself, as Holly pointed out, is also work-in-progress. All of these comments, I think, will be addressed. We've tried to be, you know, as flexible as we can. Sometimes, a comment isn't addressed quantitatively; it's addressed with additional analyses or additional figures to illustrate what's going on a little better. As time goes on, we're trying to improve all of our graphical outputs and communication tools of what's going on. Does that mean that all uncertainties that have been identified in the last day and a half that perhaps have not been addressed will be addressed quantitatively? Probably not. Some of them will still remain at the end of VA, and we'll acknowledge that. There are still some uncertainties that we have not had a chance to address or evaluate the potential consequences of in the VA. We will discuss them more probably qualitatively and then point to that this is an issue that still needs to be addressed between VA and LA; so, please, see Volume 4.

I think Chris' comment, there was a part of us who think we should have thrown out the anchor a while ago, especially as we go through the documentation of this thing. We've been a little bit too flexible to accommodate new information and some of that information changing rapidly with time. You know, the VA is a snapshot, the TSPA is a snapshot, and today is a snapshot. We may decide between now and the actual publishing of the VA that, you know, the priority of a particular issue warrants additional analyses, whether those have come up from the Board or from NRC or from others. So, I think we have to always take that into consideration. We do still have a lot of work to do. We welcome all of the comments. I think that will help the authors, most of whom were presenters, and there's other authors, as well. Right, they better document in a more complete document and a more thorough document and a more integrated document. So, I think, we welcome them.

I won't go point by point through each of the comments that we received just now. I do have rebuttals to them, but maybe I'll just save those for the documentation of the VA when we start writing it and complete the writing of it. That's probably the best place to put all of our responses. I think each one of them warrants and everything we've heard in the last day and a half, as well as comments we received from NRC a month ago, comments we continually receive from our own peer review panel, comments we receive from the repository consulting board, comments even internal. You know, we've gone through a long, some might say, arduous process of getting all the project people together for each of these components, conducting workshops on each of the components for some of the key components, holding open and expert elicitations involving outside people to enhance the uncertainty range for some of the key components, all of those provided a lot of, I think, very useful external, as well as internal, feedback into the TSPA organization and, hopefully, will create a better VA document.

So, we welcome this. The timing was lousy, of course, but the timing was great. Had we had this a month from now, we would have been a little further in writing, but it might have been a little less flexible in what we actually wrote. So, we thank the Board for this opportunity to lay out things in front of them and take the arrows now rather than three months from now. We'll probably take some then, too, but let's get some of them out in the open now.

BULLEN: Thank you, Bob. And, we do reserve the right to sling arrows at any time.

ANDREWS: Yeah, I know.

BULLEN: Well, as the chair of the PA Panel, I want to issue some closing remarks. First of all, I want to reiterate and to also compliment you and express our appreciation for your timing. I realize how bad the timing was to present this, but I want to compliment you on both the presentations, the graphics, the improved quality that we've seen over the course of the past two or three months in all aspects of what you've been doing. It's not to say that it's perfect and that you're there yet, but the clarify has significantly improved, the technical content has improved, and we also appreciate the timing of this meeting and how difficult and arduous it was for you to do this at this time. We do want to give you some timely response so that maybe we can make the process a little bit later on downstream somewhat easier.

Now, I also have to issue the disclaimer and this disclaimer probably should have been issued with my first speech, but I'll issue it now. The comments made by individual Board members are just that. During the course of this Panel meeting, our comments are the comments of individual members and they do not necessarily represent Board policy, but they do give you an indication and in some cases maybe a very strong indication of the direction of thinking that we have as a Board on these issues.

In light of the fact that you'd like some timely response, we will probably provide an informal response, comments, feedback, within a few weeks. I've asked the members of the Panel to provide me with such comment and it will probably be sort of an informal kind of comment. There will be formal response. The formal response, however, will probably not come until after our June Board meeting because it's a Board policy comment and not just the Panel comment. So, we'll have to go through the full Board before we get the letter that would come out. That letter most likely would be to the OCRWM office with copies to you. But, we will get you feedback which is the purpose of having this meeting before June so that you can get some feedback. I just wanted to reiterate that. That's the goal.

Now, another comment on the use of TSPA-VA and we heard that it was a--and I'm going to get this wrong--a quantitative predictive tool or a qualitative? Quantitative predictive tool? Not quantitative predictive tool, okay. Well, if it's not--don't get the predictive tool anyway. I'll take that out of context.

I guess I'd like to comment on a couple of things because I think it's very important that the TSPA-VA be used to fine tune, refine, redefine the design of the repository, the design of the waste package, specifically for license application. So, don't short sell the PA that's been done for VA. The reason I say that is because the analysis that you've done in this can go a long way to helping you make design decisions that would be appropriate and help ease the licensing process because you've got the basis for the decision already laid out. It also indicates the importance, as we saw today, of getting very good data on the seepage flux because that's one of the issues that constantly arose as being important.

We, as the Board, would like you to get that data in the enhanced characterization of the repository block that's currently under construction. We encourage the niche tests, but I understand that the Board when it originally wanted you to do that just really wanted to know what was the ability to reduce the hydrogeologic uncertainty associated with crossing the block and telling us what you're going to see in the drifts. Obviously, you know, my druthers would be to close the drift off for a year and walk in and take a look, but they may not be the case. But, using the VA as the ability to drive the science and drive the design may be a very important factor.

Now, again, I want to express my thanks from the Board and specifically from the Panel on Performance Assessment. With that, I want to declare this meeting adjourned.

(Whereupon, at 12:07 p.m., the meeting was adjourned.)