UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

FALL 2001 BOARD MEETING

September 12, 2001

Holiday Inn Crowne Plaza
4255 South Paradise Road
Las Vegas, Nevada

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Dr. Paul P. Craig
Dr. Debra S. Knopman
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek
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I N D E X

P R O C E E D I N G S

8:00 a.m.

COHON: I'd ask people to take their seats, and people out in the corridor to join us inside.

We convene now for the final session of this meeting. Let me just remind you, as you may have noted on the agenda, there will be a public comment period at the end of this meeting. We're scheduled to adjourn at approximately noon.

Debra Knopman will Chair today's meeting. Dr. Knopman?

KNOPMAN: Thank you. The main topic for today's session is igneous activity. We will hear presentations from the U.S. Nuclear Regulatory Commission, and then from the DOE. The State of Nevada will also offer some comments. We will have a brief comment from a representative of the Electric Power Research Institute, EPRI, and then we'll have a short roundtable discussion on the subject.

After that, we will finish this half-day session with presentations on the influence of quality assurance, issues on performance assessment, and if a site recommendation were to move forward, on plans for repository development.

The Board had intended to have three consultants here with us. They, because of the events of yesterday, were unable to get here. They included, and I'll just mention their names, William Melson, who is a senior scientist at the Smithsonian Institution, and served as many years as a consultant to the Board on issues related to volcanism, Meghan Morrissey, who's an assistant research professor at Colorado School of Mines. She would have been joining us as a consultant for the first time and would help us in our review of this work we're going to hear about this morning. And Clarence Allen, who many of you already know, Dr. Allen was a member of the Board. He's a professor emeritus in geology and geophysics, and we had also looked forward to his guidance.

Let me also just mention in terms of scheduling, it's our understanding that at 8:45 this morning, there will be a national moment of silence, and we will stop our proceedings and observe that minute of silence with everyone else.

So, we will move on now. Our first speaker is Brittain Hill, who is with the Center for Nuclear Waste Regulatory Analyses. Britt is a geologist. His professional interests are in volcanology, risk assessment, igneous petrology, and numerous other subjects. And we look forward to his presentation.

HILL: Good morning. Brittain Hill. I represent a fairly good team of consultants and staff at the Center for Nuclear Waste Regulatory Analyses, who have been investigating the probability and consequences of igneous activity, and support of the Nuclear Regulatory Commission's review positions.

I'd like to mention some people who are not here. Ono Bokhove from the University of Twint (phonetic), Steve Sparks, Anna Marie Lejeune from University of Bristol, and Andrew Woods from Cambridge University, our consultants who have been conducting a lot of the numerical and analytical models that I'll be talking about today. Also, Chuck Connor, Jim Weldy, and Larry McKague from the Center contributed to the work I'm going to present.

Today, very briefly, I'd like to go over the risk-informed basis for the investigations I'm going to be talking about this morning, give a very brief overview of the models for magma-repository interactions, and, finally, an example of Tephra remobilization that illustrates why we're concerned about the long-term effects of potential remobilization after an igneous event.

I realize you don't have the consultants here today, and that some of the jargon that I inadvertently use may be unfamiliar. I'd encourage anybody with questions to please interrupt me during my talk, and make sure we can clarify what needs to be clarified rather than waiting until the end.

Why we're concerned about potential igneous activity can be simply shown on this figure. Basically, the risk estimates and the proposed 10,000 year compliance period are dominated by igneous activity effects.

I've got a simplified version that puts together a summary of the DOE positions that have evolved from the TSPA-SR, with the probability of the igneous event is from the Probabilistic Volcanic Hazard Assessment at about 1.6 times 10 to the minus 8 per year. You can see how that risk estimate in SR at a probability level that reflects pre-licensing issue resolution, 10 to the minus 7 per year, increases by about an order of magnitude to the current state where what we've seen in the Supplemental Science and Performance Analysis report is about a .1 millirem per year probability weighted risk. And I need to emphasize that that number includes the probability of the event in addition to the consequences of the event.

At a level of probability that both DOE and the NRC agree is sufficient for pre-licensing issue resolution, 10 to the minus 7 per year, that risk is on order of 1 millirem per year, dominantly by volcanic effects.

None of these calculations to date have considered some of the key technical uncertainties I'll be talking about today in terms of number of waste packages damaged and released in extrusive events, in other words, a volcano would form on the surface and potentially release high-level waste onto the atmosphere and deposit it on the ground, or the number of waste packages that are wholly or partially damaged in intrusive events. And, finally, we haven't really addressed the potential long-term effects of ash remobilization following the igneous event.

We can start by thinking of magma-repository interactions in a very simple sense. It's pressurized fluid that flows into a void. We have rising basaltic magma, and by the time that magma is coming up beneath the proposed drifts at about 300 meters below ground surface, it's a mixture of not just molten rock, but gas bubbles. About 50 per cent of the volume of the ascending magma is in terms of a gas bubble.

We have a fluid pressure within that dike before it gets to any proposed drift. That fluid pressure is on order of 1 to 10 megapascals in excess of lithostatic pressure. So, we've got quite a head on that fluid as well.

If the rising magma intersects a drift, we have rapid decompression of that pressurized fluid. So, quite simply, the gasses expand, the pressurized magma flow into the voids, the expansion, the decompression of the gasses also accelerates that mixture to some velocity. Magma ends up filling the voids, depending on the flow rate, will repressurize the system, and then continues to rise to the surface, we think maybe on order of hours for this process to occur.

Now, the processes that we're most concerned about, the ones that have a direct impact on performance, are some of the initial effects of magma flow contacting the waste package. During that initial decompression, what are the physical conditions that we have to worry about? Are there potential damages to drift walls from the shock wave, as this accelerating magma comes down a closed end drift? Potential breakouts from the drifts once magmatic pressure gets re-established. We re-establish that pressurized fluid within a drift, where is it going to break out and continue to rise to the surface?

If pathways to the surface are established, which seems likely, what will be the flow conditions in the drift once that surface pathway goes all the way up to ground level? What's the extent of waste package damage and waste entrainment under these conditions? And, finally, given interactions with the potential drifts, what are the effects on eruption characteristics? It could be that this sort of interaction may fundamentally change the dispersive character of the ensuing eruption.

We've been taking a multi-pronged approach to evaluate the range of potential effects from this process, first by looking at process models from analog volcanoes, primarily the 1975 eruption of Tolbachik volcano in Kamchatka, in addition to several other historically active eruptions around the world.

We've also been developing numerical models for initial magma flow into closed-end drifts. Our starting point has been to take a simplified one dimensional and two dimensional approach to get the computational fluid dynamics set up so that we can modify those models and evaluate a full range of potential effects later on.

But, let me tell you, there's no software I can get off the shelf that's magma drift 101 that we can go and try to quantify a lot of these things in a responsible way.

We're also conducting analog experiments to evaluate these models of initial magma flow. We believe it's very important to look at volatile-free and volatile-bearing systems that scale analogously to a basalt volatile system as well. We're conducting these experiments to verify the limits and strengths of the 1D and 2D models that we've been developing.

In addition, we're going to be taking the 1D, 2D models and expand them for instantaneous steady flow conditions. And I have some of the initial results of that expansion to talk about this morning. Looking at the development of alternative flow pathways to the surface, and also applying a choked flow condition to see what that effect may be on the resulting pressures and flow rates within the magma system.

I want to go very quickly through the model that's presented in Bokhove and Woods, that I believe a copy has been distributed before the meeting. It's also available through the Nuclear Regulatory Commission Public Documents Room. That Bokhove and Woods paper outlines a simplified one dimensional flow-tube model with a closed end. Conceptually, we have a dike that would extend about a kilometer beneath the drift. A drift could be 5 meters in diameter. And the dike would intersect in this model 200 meters from the end of a closed drift.

We're looking at a very instantaneous opening of the dike. We're not trying to capture any rock mechanics process, nor are we trying to evaluate a de-gassing condition within the dike initially. We assumed the magma is at equilibrium at 300 meters, the model volatiles at 300 meters, represents a typical state of decompression. And then we essentially release the pressure as it comes into this 1 meter in diameter opening.

We're evaluating this for a single drift along an 80 meter long dike segment. The reason we're looking at that is constrained by flow rates. Magma, let's look at the high end of ascent rate. Magma may be coming up on order of a meter per second, it may be slower than that, about .1 meters per second more typically, but let's look at the high end, 1 meter per second along a 1 meter wide dike.

So, if we have an 80 meter long segment of dike, we're coming up volumetrically of 80 cubic meters per second of mass rising up in that 80 meter long segment of the dike.

Now, the drifts are 20 meters in diameter. So, clearly, if we have flow into the drift greater than 4 meters per second, we're capturing 80 cubic meters per second of ascent. You will see in the subsequent calculations we're getting well above 4 meters per second flow from expansive decompression of the magma.

So, we believe the current conceptual model is correct, and that all of the ascending mass of the dike can be captured by accelerated flow and flow focusing into the drift. Our model, which is shown here, allows for the cross-sectional area to vary between the 80 square meter dike into the 20 square meter drift through a series of steps.

This is from the Woods et al. paper. Figure 3, again, this is available through the Nuclear Regulatory Commission. In addition, I believe it was also distributed before the meeting. We're looking at the first few seconds of interaction between the dike and drift. I apologize. This is a confusing figure, but the authors of this paper tell me this is the way you're supposed to do it.

We're looking right here at the contact between the dike and the drift, and depth is increasing in this direction. So, here, we're about a kilometer beneath the dike/drift interface. We're coming up to the dike/drift point, and then this is flow distance along the drift. So, we're kind of making that bend in the flow system conceptually.

So, here, we're calculating the pressure that would occur at time step one, two, three, four and five, with depth down the dike, and with distance into the drift. So, we're saying here that in the initial second, I think these are .2 second increments, the initial .2 of intersection, we have this pressure profile along this distance in the drift, stepping up as flow comes in, reflects off the end of the closed end drift, and forms this shock that propagates back into the flowing system. It peaks at about 40 megapascals in our simplified calculation.

The same coordinate system, we're looking at the velocity profiles, starting from an initial high velocity accelerated flow that has a transient up to about 200 meters per second, going down to lower flow rates that are still on order of about 10 to 20 meters per second. It's hard to see in this.

The conclusions that we can reach from these admittedly simplified calculations are that the decompressing magma accelerates in the drift to greater than 4 meters per second. So, we know we're going to be able to volumetrically capture the ascending magma. And that the reflected shock gives large overpressures in the first seconds of this interaction.

We saw yesterday some of the Darcy permeabilities for the drift being on order of 10 to the minus 11th meter squared. So, here, if you look at the fracture permeability, say you're thinking that you can't get that compression without air leaking out, your fractures are on order of 10 to the minus 11th meter squared, per every 20 meters squared of compressed air.

You can see that we're not too worried at the first pass about loss of compressed air through existing fractures. Our primary concern is as we build up to some level of pressure at this drift end, will we be initiating hydrofracture towards the end of that drift? And if we initiate fractures at the closed end of the drift, could that be a preferred pathway for magma ascent and flow?

We also have to look at the pressure and velocity and, of course, temperature conditions to estimate what could be the damage to waste packages contained in the flowing system. And that would be getting ahead of myself, since we have no data and analysis to really evaluate this.

We're also conducting some gum resin acetone experiments. Here, this is a Hele-Shaw cell where we have, in this case, a golden syrup. It doesn't have any volatiles in it. We take this syrup and pressure the system up to some level that scales to the different pressure gradients that we are looking at for the magma drift system, and then open this little gate up at the top instantaneously. And a cross-section, here's the gate. The golden syrup would flow into this glass cylinder that has a series of pressure transducers in it.

We also conduct this with a gum resin and acetone mixture that behave very similar to magma, in that when we decompress the gum resin and acetone, we don't get much of the acetone gas coming out of the system. The only bubbles that expand are the bubbles that exist in the system prior to the decompression. We think the same thing is happening with basalt, that decompression is diffusion limited. We're not getting additional volatiles coming out of solution in those first instances of flow. The only thing that contributes to the flow acceleration are the bubbles that exist under equilibrium decompression as the magma rises in the Yucca Mountain system.

What we're trying to do is develop pressure-density relationships through this to calibrate the experiments with the 1D and 2D numerical models. This is still ongoing work, and I can't report any results for how well the experiments are calibrating to the numerical models.

We're also concerned about the potential pathways to the surface and what flow conditions may be once a pathway is established. We developed three alternative conceptual models, where in the first one, the dike would simply propagate up to the surface once the drift that has been intersected is filled.

The second model is that as magma flows into the drift, the drift fills. The strength of the rock is lower at some point than the point of initial intersection, so breakout can occur at the point of greatest weakness rather than point of initial intersection of the dike, so that we would establish a flow path sometime horizontally through a part of the drift system.

And, finally, an option that doesn't seem likely by design now, because the access drifts appear to be backfilled entirely, but in the absence of backfill or significant obstruction, flowing magma could break out from the intersected drifts and flow to an access drift, or another shaft, and form multiple pathways to the surface.

So, we really need to look at mechanically how the initial intersection point, what the strengths of the rock would be there, how the potential interactions in the drift, both initially and sustained, can affect the build-up of pressure through time, and give us the breakouts.

The blockages in the drift, because we're not dealing with a simple 20 meter in diameter unobstructed drift, there's all sorts of engineered materials in there that create a real significant problem for doing flow calculations. But we've got to start somewhere, and we start with a simplified model.

And, finally, the rock strength variations, and key to this is that there are significant topographic variations under the repository footprint. We can have anywhere from 200 to over 300 meters of overlying rock over the drifts. So, if our intersection point initially is beneath 300 meters of rock, you just intuitively think, all other things and stress being equal, if a magma has to break out over 200 meters of rock, that strength, the force necessary to fracture 200 meters, is probably less than 300 meters. And, again, we're looking at hydrofracture measurements at Yucca Mountain at 300 meters that are on order of 6 to 5 megapascals. And we saw from the initial calculation that we're clearly exceeding in these initial calculations pressures of order of 5 to 6 megapascals.

We also need to evaluate from the sustained flow conditions what would be the waste package response, both in flow paths to the surface remaining in the magma filled drifts, and of course in non-intersected drifts, because we're concerned about potential de-gassing effects as well.

KNOPMAN: Britt, just five minutes.

HILL: Yes. Five minutes? Okay.

This is from the Woods, et al. paper again, Figure 5, when we look at instantaneous conditions for the three flow paths in the preceding figure. We applied a condition of choked flow. In other words, you don't allow this to exceed the speed of sound is a common assumption in modeling volcanic processes.

The thing to take away from this figure is that we get significant backpressures in the system once we instantaneously establish these sort of flow conditions. What we expect this to be is modifying the likely flow paths to respond to that overpressure. So, in other words, we would expect conduit widening, the development of new conduits, et cetera, as we get this backpressure in the system from the instantaneous calculation of flow.

So, why is all this significant? We need to get the temperatures and pressures for waste package response. Basically the voids in intersected drifts are going to likely be filled with magma. We're going to expect dynamic pressure variations during the eruption in response to these flow conditions. And, ultimately, the damage to waste package may be more extensive than currently modeled in the TSPA-SR. In other words, some of the damage in what the DOE calls Zone 2 may correspond more to Zone 1.

Longer flow paths of course may intersect more waste packages than were currently modeled. We can have the surface pathways, as in Model 2, form it away from the plane of initial intersection. It could be exploiting new or existing faults, engineered shafts and ramps, or creating its own surface pathways.

And, again, it's a complex process and I can't, in 20 minutes, go through all the details. But certainly another area of concern is how would waste from potentially disrupted waste packages be entrained into the system, and what are the potential effects on dispersal mechanics. It's a long ways to go before we can truly quantify what the risk impacts of these uncertainties are, but we're getting there.

I think I need to go very quickly through the Tephra-fall remobilization. But, basically, the presence of high-level waste contaminated Tephra affects the probability weighted dose calculations. It's not so much the dose in the year of eruption that contributes to the risk, but how in the longer term does that Tephra deposit exist on the surface and contribute to dose through time that's dominating the remobilization effects.

KNOPMAN: Britt, given the importance of what you're talking about, why don't you take the extra couple minutes, if you'd like, to either dwell on this one or any other points you want to make.

HILL: Okay. Just as a perspective, from analog volcanoes, we would expect the Tephra deposits to be distributed over areas on an order of 100 to 1000 square kilometers from the originating volcano.

We also have a real challenge here in that the 10 to 10,000 year characteristics of Tephra deposits in the Yucca Mountain type environment are very poorly known, even from analog volcanoes. The only datapoint we have is the 80,000 year old Lathrop Wells volcano about 16 kilometers to the south, and we can see that all the Tephra from that eruption has been eroded from bedrock surfaces. But, you know, 80,000 years doesn't tell us a heck of a lot for a 10,000 year performance period.

Analog information says that these deposits erode for hundreds to thousands of years, but it's a fairly complicated process that's controlled in part by rainfall and the permeability of the underlying substrate.

We have also both erosional and depositional processes occurring right in the area of the reasonably maximally exposed individual. We have erosion from slopes and water transport in the Forty Mile Wash drainage system. In addition, we have windblown particles for inhalation dose from not just the area around Yucca Mountain, but for all distances along the Forty Mile Wash drainage.

I don't have any quantifiable models, except some examples from analog volcanoes. This is an example based on the well studied 1975 Tolbachik eruption in Kamchatka. And, again, I want to emphasize this is a speculative model that just shows what the fallout patterns from the volcano would look like if a volcano similar to Tolbachik were to erupt at the proposed repository site. But I would also emphasize that eruptions on the size of the Tolbachik eruption are possible for future igneous activity at the Yucca Mountain site.

We also made a very simplified assumption that here is the erosional basin for Forty Mile Wash. And let's just say the deposits eroded from slopes greater than 5 degrees. By making that assumption and quantifying this in our geographic information system, we would see that if this material that fell within the Forty Mile Wash erosional basin on greater than 5 degree slopes, and came down into the depositional system, and let's just say for simplification sake it was deposited uniformly throughout this roughly 100 square kilometer basin, that mass would be redistributed into the deposit that's about ten times thicker than the initial fall deposit.

We're also seeing very similar effects of scale, on order of a factor of ten increase in deposit thickness from smaller eruptions, things that are several orders of magnitude smaller than Tolbachik.

We're not only concerned about the deposition at the RMEI location, but also bringing down suspendable fines into this general area where they could be entrained in the wind and inhaled by the RMEI.

It's not just, though, the factor of ten that's important. What's really important is the rate that this Tephra is remobilized and accumulates and is potentially diluted through time. We don't have the information yet, but we're working on that, and so is the Department of Energy, to quantify that rate of long-term remobilization, and then quantify the risk significance of that rate in the Performance Assessment.

So, let me conclude. We have some significant uncertainties in the magma flow paths following potential drift interaction, waste package and waste form response to a range of magmatic conditions, and eruption dispersal and long-term remobilization characteristics.

I haven't been able to provide you with any answers today, except to show that there are significant uncertainties that have not been addressed in any of the existing performance assessments. Until we get a solid technical basis to quantify those uncertainties, we only have a speculative basis to say what those impacts on risk could be.

Our models that we currently have have limited capability to evaluate the risk significance of these alternative flow paths, a realistic range of magmatic conditions for EBS response, possible changes in eruptive character, and the long-term flux of contaminated Tephra through the RMEI area. We're going to need to develop new models, and modify extensively the models that we have.

And, finally, the Center and NRC staff are going to continue to develop a technical basis to support the review of DOE progress in these areas. I am very encouraged from the technical exchange last week where the Department has agreed to do significant work in support of these uncertainties.

Thank you.

KNOPMAN: Thank you very much, Britt.

Questions from the Board? Priscilla Nelson.

NELSON: I don't know that I'm the best person to start off, but we'll try. Nelson, Board.

The focus on a Yucca Mountain specific model, because all focus is on that site instead of general processes, what I'm wondering about is the additional information that would be required, say, regarding internal structure of the mountain, or variability of the rock, rock mass properties there, and maybe additional information about the depositional basin that might give you some indication on time or character using the Lathrop Wells as an example. Is the Project going to get additional information? Is there additional information that would feed into your model that is real site specific information that would help to define some of this?

HILL: We haven't gone into the details of the DOE proposed work, but at the level of detail we've talked about it, there is going to be additional information, certainly on the erosive and dispersive processes on the surface at Yucca Mountain. And there's some work that was done in support of the extreme erosion issue back in the early Nineties that's going to be relevant to this.

In terms of the rock mechanics, a lot of that is looking at the range of realistic variability that we have in the system, to try to look more mechanistically at rock response for appropriate Yucca Mountain conditions. We have not integrated rock mechanics into the conceptual model yet, but I think the range of stress distribution, for example, for the undisturbed repository is constrained by existing information.

One of our concerns, though, is during the thermal period of the repository, though we've seen a number of models that show a build-up, an accumulation of stress, those models haven't gone forward and show what would be the appropriate strain response for that stress condition, given the existing structures and rock properties.

So, there's a long ways to go before we really integrate the rock mechanics into a risk number.

NELSON: But from the standpoint of the structure that's present in the mountain, you know, modeling the existing faults and what might be the character of the intrusion, or really looking at the sediment accumulation down in the depositional basin more carefully or more precisely so that you can actually see some of the sediment distribution, get an idea of how fast Lathrop Wells deposits were carried, deposited, whatever, I mean, the real site specific mess of it.

HILL: There's been a fair amount of site characterization. For example, the Lathrop Wells deposit, there are only trace amounts left in a couple of very odd locations. The Department is going to go back and look at the Nye County wells, in addition, do some trenching work to try to look for trace amounts of this deposit. But it's going to be very difficult to find it, and the same thing with the specifics of exact structures.

Of course, we have the ECRB and the ESF data, but trying to look at the range of footprints that have been proposed, and for each drift, a range of potential breakouts, I don't know if we can do that. We're going to have to use a constraining approach.

KNOPMAN: Don Runnells?

RUNNELLS: Runnells, Board.

Just an informational question. I don't know what the melting temperature is of C-22 or stainless versus the best estimate of the temperature of the basalt, so I don't know what mechanism you're talking about. Is the mechanism of release of the radionuclides melting of the canisters and mixing with basalt? Or is it more like logs being pushed in the intrusive picture, logs being pushed ahead of a flood? I mean, can you just describe it for me? The temperatures first, tell me what the temperatures are.

HILL: Okay. The first temperature, I don't know the exact number, but I believe for C-22, it's on order of 1300 degrees C. is the melting temperature. But we're not worried about melting this metal. When we start to get above 800 degree C., we have to worry about ductility under a very high pressure, high velocity, high dynamic pressure flow regime.

We're continuing to evaluate the mechanical response. But the temperature of the magma is on order of 1100 degrees centigrade. So we're not dissolving the waste package. But under a flow condition where you have 2600 kilogram per cubic meter basalt flowing, even at several meters per second, against a waste package that's differentially loaded between the supports or potential collisions, at a sustained flow that may exist for several weeks to a month, you can see why our starting assumption would be that the canister wall is breached and the package contents could be entrained.

We're also concerned for waste or packages that are not directly entrained in the flowing magma, but, say, are sitting out in a more stagnant part. What are the effects on containment? Because it's not just the initial ductile deformation that we have to worry about, but there are data from the Hanes International people that show very significant embrittlement once you get it up above about 850 to 900 degrees C. The impact toughness of this material goes from about 250 foot pounds, down to about 10 foot pounds after exposure on order of ten hours to these temperatures.

So, even if you're not breaching the waste package initially in a stagnant part of the drift, you've got a mass of magma or molten--or excuse me--you've got a mass of cool basalt of density of 2600 kilograms per cubic meter. Probably at least a meter of that, and you can imagine just a little bit of seismic shaking afterwards, is going to give you more than 10 foot pounds of force onto this highly stressed, highly embrittled waste package outer wall.

KNOPMAN: Dick Parizek.

PARIZEK: Parizek, Board.

There's a number of analogs that you bring into play here, which is important in terms of this Tephra plus rates of erosion. Do you have any evidence on the velocity of magma movement? You have that one meter per second and a .1 meter per second. Is this something that's calculated, or is this something that you get from seismic velocity data, or how do you come up with those numbers? Because that's sort of critical to the energy available to disrupt what happens in an emplacement drift.

HILL: Well, the first answer is the 1975 Tolbachik eruption was well instrumented by the Russians, and so we can look at the rise of seismic epicenters through time during the weeks preceding the eruption. That would give you about .1 meters per second as the ascent rate.

There's also a wide range of literature that would look at ascent versus cooling dynamics. Because if you're coming up, we know that dikes are on order of about a meter wide. We can see that from different levels of depth of erosion through time in different geologic settings.

So, if you have this kind of a mass with this kind of a heat capacity at this sort of a temperature, what's your minimum rise time before you conductively just chill this magma, because you're looking at a fairly thin sheet. And that would again give you that order of .1 to 1 meters per second from just thermal loss alone. I think it's a fairly well constrained number.

PARIZEK: You talk about the roof height of 300 meters versus 200 meters, why wouldn't the dike or the breakout head for the low overburden, or thin overburden portion of it. But on that argument, why would dikes want to go to Yucca Mountain at all. Why wouldn't they go to Crater Flat? That's where we have obviously evidence of more cinder cone activity and volcanic activity. So, wouldn't these events then look for low topographic areas with alluvium rather than going for rock, such as in Yucca Mountain? And even if it went for Yucca Mountain rocks, why wouldn't it come up block bounding faults, you know, such as the Canyon Fault and elsewhere? Why pick on some dinky little fault in the middle of the repository footprint?

HILL: That's a lecture in itself, and I don't want to lecture, but there are a couple of very simple concepts. First, the basin that defines the Yucca Mountain magmatic system for the past 10 million years is not bounded by the Solitario Canyon Fault. All the deep geophysical surveys clearly show it is bounded on the west by Bear Mountain, and on the east by something that looks like the gravity fault.

Essentially, when you lose the topography out to Jackass Flat, that's the basin bound. But the eastern boundary is fairly diffuse.

Second, when a magma is rising from an initial point about 30 kilometers below the crust, differences on order of a couple of hundred meters aren't really going to affect the rise. It's fairly insensitive until you get fairly shallow, on order of about a kilometer or so, and that's when you begin to get capture from non-vertical, very slightly in kind structures.

Now, we know that a dike came up on Solitario Canyon about 10 million years ago, 240 meters from the proposed repository footprint. One out of the past 13 events in the past 10 million years came pretty darned close to this proposed setting.

So, once you get fairly shallow, that's when the surface effects become important. But deflecting at a very shallow depth is very hard to do. So, the rise is pretty well controlled by this poorly understood, but likely chaotic process of magma accumulation that has little to do with the small scale structure, but rather reflects the broad basin, which encompasses the proposed repository.

PARIZEK: So that deep determining background sort of sets the stage for it. Then let's try another one. As an engineering solution, my kids always had to drop an egg from two stories up, and cushion it against the shock, and anybody who won that was a hero in the grammar school context.

Can you put in compressible ends to the repository tunnels to take up the shock, rather than just having it be this rigid thing echoing back, you know, the shock wave. Because I think you said the initial effect is less damaging than the echoing effect, or the shock wave that's set up.

But couldn't we have a crushable end or compressible end, you know, cushioning that, if this was, you know, a possibility

HILL: Sure.

PARIZEK: Or is this in the thinking of anybody?

HILL: I don't know the thinking, but it's logical that you could have something that would alleviate that pressure accumulation for the initial points. But anything that would be venting that accumulated pressure, for example, could serve to weaken the rock around it.

And, second, it's not that the initial impacts are the things that give us all the problems. We still would have some measure of flow into that void because we're dealing with a pressurized fluid. That fluid flows into the voids, and it's fairly compressible for the gas, but it still is going to continue to flow into the drifts until it reequilibrates with the pressure from that contiguous magma in the system. And that magma has an overpressure that's greater than the surrounding rock strength. It has to, or how else could it rise?

So, even though we may be attenuating the shock during the initial impacts, we still would have to deal with the over pressures that would develop as magma continued to flow and would reequilibrate with the dike system.

PARIZEK: I was thinking of the number of waste packages that could be destroyed by that initial shock versus, I guess, the heat effect weakening them and eventually destroying the packages. So, it's a timing I guess of release.

HILL: I think it's speculative, but knowledgeably speculative, to say that the damage to waste package from the initial shock impact, from an intact waste package, you just don't have the temperature effects working for you, and these are fairly strong waste packages, and we're looking at a simplified calculation that gave us on order of 40 megapascals as the pressure transient.

I don't think that's sufficient to breach a large number. It's more the long-term exposure to these magmatic temperatures with the dynamic fluid pressure rather than the shock pressure that mechanistically seems to be the process that would lead to more premature failure of the waste package than the initial shocks.

PARIZEK: None of the packages are cemented shut, or cemented in in the lava when finally the thing chills. You're not taking any credit for just saying, well, they're sealed up, they're in the dike, with low permeability after it's cooled?

HILL: And the dike, the igneous system itself, I think we can constrain the physical processes, and we're not talking about the center of the sun. These are temperature, pressure, chemical conditions that don't exceed the realm of engineering capability. It's just engineer for high dynamic stress, high temperature, you're going to be dealing up a lot for the longer term ambient effects.

KNOPMAN: We have a couple more questions, which we will get to. It is 8:45, and I'd like to ask everyone to just rise, and we will observe a minute of silence.

(Whereupon, a minute of silence was taken.)

KNOPMAN: We'll resume with a few more questions here. Again, this is an important presentation for the Board, and I want to make sure we get our questions out on the table.

Leon Reiter?

REITER: Britt, Bill Melson isn't here, and I'd like to raise an issue that he's brought up in the past, namely the use of situations such as Tolbachik as an analog. I guess he sort of felt that this is not quite an appropriate analog. And just could you give us an idea, to the extent that you can, how the 1975 eruption compared to, say, the Lathrop Wells eruption occurring 80,000 years ago, to the extent that you can.

HILL: Okay. First of all, I disagree with Dr. Melson, and based on my 20 years of experience in volcanology, I believe Tolbachik and other analogs we've used are appropriate and do capture fundamentally the processes that we are trying to understand.

The reason I have that position is that the process of magma, magma viscosity, magma temperatures, and the flow dynamics are to the best of our abilities the same as we would expect in a Yucca Mountain region volcano.

The magma rises in Tolbachik from a depth of about 40 kilometers, again, similar to the 30 kilometers rise depth from Yucca Mountain. We believe it has a volatile content of about 2 to 3 weight per cent water, very similar to the range of volatile contents for the Yucca Mountain system.

We look at mass flow rates, although the total volume of the Tolbachik eruption is bigger, the instantaneous mass flow rates appear identical to what we would see in the range of Yucca Mountain, not just Lathrop Wells itself.

Of course, the Lathrop Wells eruption we believe was about an order of magnitude smaller than the total volume of the Tolbachik eruption. But, again, Lathrop Wells is a single cinder cone volcano. The Tolbachik eruption was a series of three cinder cone volcanoes that went on for about two months of duration.

The Tephra columns that we see from Tolbachik raised anywhere from several kilometers high to periods that were about 8 kilometers high, correlates well with the volcanological theories of mass flow and column rise height.

The character of the cone, the character of the lava flow, the cone to flow ratios at Tolbachik are very similar to what we see at volcanoes like Lathrop Wells or the

Sleeping Butte volcanoes, for example, which are the best preserved volcanoes in the Yucca Mountain system.

And, fundamentally, the magma itself has the same sort of viscosity, the same sort of decompression characteristics, the same sort of vesiculation characteristics, the same amount of minerals in it, and would expect to behave the same way as the well documented 1975 eruption.

So, I believe these are very--the Tolbachik is probably the most appropriate analog volcanic eruption.

If we had a historical eruption in the Western Great Basin, we'd use it. But, unfortunately, at least from a volcanologists perspective unfortunately, we haven't had one of those in historical time. So we need to rely upon historically active analogs that present us with the process that's important, that they're in an arc is almost an irrelevancy.

REITER: Just to follow up, would the fact that the Tolbachik you said I think was ten times the volume, would that affect the Tephra distribution that you might expect?

HILL: No. It affects the total thickness. But the process of convective rise and dispersement is exactly the same.

REITER: But the thickness, in other words, the plot you showed of the thicknesses would be different?

HILL: Of course.

REITER: Okay.

HILL: But I'd like to also make sure we're clear that we do not know what the volume of Tephra was from any of the Yucca Mountain volcanoes. We have to look at ratios of here's the amount of cone that's preserved, here's the amount of lava that's preserved. Amongst the analogs we have with that kind of a cone to lava flow ratio, what sort of Tephra volumes do we have? All the Tephra has been eroded away from these 80,000 to million year old volcanoes, but we've got to do something, because this is the source of risk.

I would say, though, that the range of potential future events at Yucca Mountain that we've shown, and that the numbers the Department is using for these kind of Tephra dispersions, clearly encompasses the volume of the Tolbachik volcano.

KNOPMAN: Dave Diodato?

HILL: I hope that answered the question.

REITER: I think Bill would have to look at that.

HILL: Well, I'm always available.

DIODATO: Diodato, Staff.

I just wanted to follow up on the shock wave phenomenon part of the situation to make sure that I've got it clear. This would be mechanistic damage, but you discounted that, if I understand correctly. The mechanistic damage from the shock wave phenomenon would not be a significant player in terms of breaching the canisters?

HILL: I was trying to be very careful in saying that we haven't analyzed that completely. We have not done an engineering analysis, and that my speculative basis, but informed speculative basis, was that that level of shock is probably not sufficient to breach an intact waste package at temperature--excuse me--at ambient temperature.

DIODATO: But it may be in your thinking?

HILL: I think it's maybe. Until--I'm a scientist. Until we've done the calculations using appropriate physical conditions and appropriate engineering conditions, I can't tell you with any certainty. There's a lot that needs to be done here, and we're doing the best we can to constrain these effects.

DIODATO: Well, could you help me to understand just a little bit the physics of it? Is that a resonant phenomena, I mean, does it depend on a resonation, like a critical link? I'm just thinking like a guitar string. When you hit the harmonics, it definitely depends on the length.

HILL: I just have a very simplified understanding of shock tube dynamics. But I believe it's the compression of this flowing relatively incompressible mass against the compressed air, and reflection of that wave more than anything else that leads to that pressure buildup in the transient pressure conditions. But then once you've attenuated that, the mass is continuing to flow into the system, but you don't have that initial compressive impact. So, any subsequent resonance is going to be at a much, much lower value than the initial free air compression. You're compressing the most volume of air during that initial flow condition.

DIODATO: Thank you.

KNOPMAN: Okay. You may want to follow up later. Alberto Sagüés?

SAGÜÉS: We were wondering here, a couple of questions on the mechanical strength issue. This reduction in--that you mentioned from, say, 200 foot pounds to, say, 300 foot pound, or something like that, I assume that that refers to room temperature measurements made in specimens that are being annealed at high temperature, and let cool down and then they test the sharpy, or whatever they're doing?

HILL: It was the sharpy, yes. And returning to room temperature conditions for the impact strength.

SAGÜÉS: Yeah. I don't know at this moment, I seem to think that the high temperature, that that may be able to withstand a little bit more energy for fracture, because of the energy dissipation ability being greater at higher temperatures. This impacts--you're considering just bouncing it--maybe there would be loss while they are still hot; is that the idea?

HILL: I think I may have not made this clear. The sharpy impact argument was for waste packages that would be in the stagnant part of a magma filled drift. The magma would cool, and subsequent to all of this, when the igneous event is over, say 100 years or 200 years later, you'd still have this fractured basalt at essentially ambient temperature sitting on top of a waste package that had been brought up for hundreds of hours to temperatures on order of 1000 degrees C. That's where the impact toughness, say you had an earthquake after that, even in the background, magnitude four earthquake, you'd be rattling this basalt on top of a greatly weakened canister.

SAGÜÉS: Oh, okay.

HILL: I agree with you completely that for the temperature, magmatic temperature analysis, we're not worried about impact toughness at all. We have to look at the appropriate strength characteristics for the duration of exposure, not just an instantaneous evaluation, but a dynamic evaluation.

SAGÜÉS: Okay. And a connected question with this, I assume that are you conservatively ignoring the presence of the inner two inches of stainless, of 316 NG?

HILL: Yes. At this stage, yes. The ductility of 316 is much greater. We're not ignoring it entirely. For example, we're concerned about differential thermal expansion. There's a range of gaps that can be between the proposed 316 inner barrier and C-22 outer barrier. But when we have essentially 30 per cent greater coefficient of thermal expansion for 316, then the overlying C-22, you know you would need to have some sort of a gap as you raise that temperature from ambient condition, up to on the order of 1000 degrees C.

So, we're not ignoring it entirely, but the 1000 degrees C. strength properties of 316, from my engineering colleagues, they're saying that does not appear to be a significant physical barrier at that temperature. It's the C-22 that has the higher temperature--

SAGÜÉS: Some of the negative effects, but not--but you're still being conservative, though?

HILL: We have not done a full scale analysis of this process. We're doing some starting assumptions, and we have to look at this realistically. Under these conditions, would we necessarily expect a waste package to remain intact? And our conclusion is we would not necessarily expect it. We're not concluding it's going to be breached. We have to do the work to do that. But it seems like a reasonable assumption to say under these loads, under these forces, under these temperatures, that waste package is potentially going to fail.

SAGÜÉS: Thank you.

KNOPMAN: Thank you, Britt. We will have other opportunities to ask questions to Britt and the others during a panel discussion that we'll have after the other two speakers.

I'd like to introduce Eric Smistad. Eric was introduced already on Monday when he spoke to us about the Biosphere Panel and the Biosphere Review. He's the DOE Technical Lead for biosphere, igneous activity, unsaturated zone and performance confirmation.

SMISTAD: Good morning.

This morning, I'm going to give more or less a programmatic talk on igneous consequences. We at this point have not analyzed the model that Britt just walked through in detail, so I won't be showing any plots or graphs or any quantitative analysis on this today.

I'm going to walk through our really qualitative impressions of the model that they put on the table here for us recently. I'll talk briefly about our plans to address this, the schedule, the schedule we have in place, how this plays into site suitability, and what we're actually showing now for site suitability in terms of igneous consequences and dose. I'll show a dose table comparing SR doses to SSPA-2 doses, and then I'll summarize.

In June, we had an igneous consequences technical exchange here in Las Vegas. This is where we saw a detailed explanation of actually what Britt just walked through really for the first time. Just recently here in September--I might say there's a step in between here. They put some agreements on the table in this June technical exchange that we did not agree to in terms of this model. We needed time to go back and look at this to see what we thought of the model itself.

We convened just here this month, early this month, and we did come to four agreements regarding the model, and as a result, this particular KTI, the igneous activity, is now closed-pending, as is the TSPA-I KTI is now closed-pending as well. It was held open for this igneous activity issue.

Our view of the model you've just seen. As I said, we haven't analyzed it in a quantitative detail, so I won't have that for you today. We feel that this model is really an idealized conceptual model. We're not at the stag where we're willing to call it an alternate model, because we haven't analyzed it yet. And there really are a whole suite of simplified assumptions that have gone into this model.

As Britt said, it is a smooth wall, closed end, one dimensional flow tube. This is not the repository. This is an idealized conceptualization that they put in place. As I said, it doesn't consider the repository or the major elements of the repository. We've got a geologic repository. There's rock there. It's not a smooth wall system.

We have material in the drifts, packages, drip shields, invert material. We don't have a closed end drift system. We don't have a brick wall at the end of our drift. We have effectively backfill in the mains on either side of the drift. This is not a brick wall system. This is a system that can give.

The preferential diversion of the dike once it hits the drift is a question mark for us. You recall in Britt's pitch, I believe it's Page 9, he had three cases there. He had a case where the dike just intersected the drifts, continued to the surface, and that's effectively what we've got in place now, what we're modeling at this point.

Case B, or Case 2, he had the dike intersecting a series of drifts, and diverting down the drifts, and actually not continuing up to the surface between the drifts. That's my fourth dash here. We don't necessarily believe that's true. There's a lot of energy in that ascent, and we're not convinced that the total flow will be diverted into the drifts.

The explosive decompression and propagation of a supersonic shock wave in the drifts. This is something at the qualitative level we're not quite ready to buy off on. As I mentioned just earlier here, the model they've got in place relies on a closed end tube. We don't have a closed end tube in our system.

There's several other factors involved in a shock wave. There will be friction involved as the wave travels, the initial wave travels, I'll say, not the subsequent wave they're talking about here. There's friction along the walls, and in order to get this sort of a supersonic shock wave and the magnitude he's talking about, you have to have these reflections. We don't believe you'll have reflections at the end. We believe that the wave itself will continue into the structure and the backfill and the main.

So, they're relying on that to ricochet off the end there, and they're relying on the pyroclastic flow to actually serve as a solid mass, too, where you're getting this reflection back and forth, effectively turning up the gain on this wave. We don't think that's going to happen. The pyroclastic flow itself is just that. It's clastic material. There will be dust involved, too. That would absorb some of this wave, we believe, as well.

And we're not sure even if you turned up the gain to the extent that they have in their papers, that you really have, you know, packages moving about in the drifts.

And then the last dash here, somehow the system has to repressurize itself. It has to maintain energy, and it has to find a spot to come up through the drift to the surface at some location other than the original intersection. So, this is something that we have a big question mark on as well.

So, we just don't feel at this point that the model that's been put forth is realistic in terms of the repository as we have it today.

And there hasn't been any, although this is not something they've done or looked at as far as I know, there hasn't been any likelihood applied to this whole model. What is the likelihood of a dog leg occurring? What is the likelihood of a shock wave of that magnitude actually occurring?

And then there's likelihoods and PDFs you could apply to package damage as well. We haven't done that, nor has the NRC.

I wanted to, just back on the other topic just for a minute, I didn't walk through all three cases that Britt put on the table. There was the case we've got now, which is Case A, with a dike intersects and continues to the surface. Case B, I described as all the flow happening in the drifts and not continuing to the surface. Case C is similar to B, except you don't perhaps have the backfilled drifts, and you've got the flow going onto other drifts, or shafts, or what have you.

And I think the important point there is that really Case C for us, although we haven't analyzed it in detail, we think it's pretty much off the table, because we do plan on backfilling these drifts. So, we don't really see that occurring, although we will look and see how this plays out, if it gets to the stage where we actually believe a dog leg will occur.

Okay, the plans to address the new model. We do intend to proceed in a risk-informed defensible manner for licensing. We acknowledge by the mere fact that we did agree to look at this new model and the four agreements we entered into earlier this month. So we will be looking at it. But we're not convinced that it's a model that will play out necessarily in our analysis in the end. We do have a plan. We've put together a plan. We shared that with the NRC at this particular meeting. I'm not going to go through the details of that plan today. If there's questions, I'll try to answer those as best I can.

The main focus of the plan and the four agreements is on the magma drift, magma waste package, magma waste form, and then the Tephra, the remobilization of ash or fall near the cone, and down into the Valley by pluvial or other processes.

Schedule. We have not scheduled this out in detail yet. This is not an analysis that will be in place for SR, obviously. We are talking about LA time frame on this. The analysis that we will do on this, and again there's a question mark as to what degree we will take this to, but we will evaluate it, will be embedded in these four AMRs. I should have put a question mark on the end of this one. This is not yet an AMR. We believe we will have one in the system at the time.

As I mentioned, the completion dates are TBD. We are in the middle of planning dates for LA.

What is our current state regarding igneous activity and consequences for site suitability, and how does the new model that the NRC has put on the table play into this?

We currently have in both the SR documentation and the TSPA-SR and the SSPA-2 documents, we're showing low doses still. These are well below either limits and regulations. We believe from a probabilistic standpoint that the PVHA that we have in place is still a robust analysis. There's nothing that's come up since that particular work that calls into question our probability. That probability in fact was very low. This is a highly unlikely event that we're talking about here.

And we did do some very preliminary scoping calculations on this, and what we did was we took the SSPA-2 model that's in place right now, and we--let's really assume the worst. We assumed that this scenario, or the model, would occur. Let's take the dog leg. Let's have it invade, if you will, the drifts, and let's have it touch every package and throw those packages up to the surface. This is the eruptive dose I'm talking about now, because it is currently the highest dose we've got between the two, the eruptive and the intrusive, or the groundwater dose.

So, we took essentially every package that intersected drifts, and we came up with a dose around 2 to 3 millirems. This is still well below standard in our minds, and I'll just repeat this is an end member analysis. We don't believe that this will play out in the end. It's very conservative to assume all packages in the intersected drifts are going to go up to the surface, and all waste is available to travel down to the critical group.

But it gives you an idea that this model, even taking it to an unrealistic stage, is not beyond any regulatory limit that may be in place.

We do still have conservative assumptions in our igneous consequences model today. This is not an exhaustive list, but these are the ones that we feel really are sort of the bigger ones, and the ones that we can actually take a look at.

We're assuming at this point that all eruptions, future eruptions are violent strombolian. In other words, they're the explosive kind of eruptions. When, in fact there are effusive flows out there in the field, the Yucca Mountain field. So, we're going to look at, if you will, a partitioning. How do we look at this some more in a realistic manner and account for effusive flows, in other words, flows that don't--aren't explosive and carry ash and radionuclides down to the critical group? So, that's something that we will be looking at next FY.

Wind direction. We have assumed in our current analysis that the wind is blowing south all the time towards the critical group, when in fact the wind rows, or the wind data that we have in place really says that, you know, that might occur, the wind may blow south 10 to 15 per cent of the time, something like that. So, it's not blowing down there the majority of the time, and we did a sensitivity in TSPA Rev. 0, ICN 1, and it was a factor of 5X if we actually used a realistic wind row.

Now, one thing that would come into play here is the redistribution issue, and that's something that we plan to look at as well.

And then on the BDCFs, the biosphere dose conversion factors, we're assuming what we're calling transition phase BDCFs through time. This is essentially the dustiest conditions that you can get, when in fact you really will have--this is like a condition that would occur closer to the time of the eruption. Through time, we know that this settles down, the dust will settle down through time, 10, 15, 20 years, whatever the case may be. We haven't taken credit for that. We've used these indefinitely. So, this is something we're going to revisit as well.

The first and the third are certainly not quantified at this point. We did quantify, obviously, the middle one, but we need to look at redistribution and how that plays into this.

Okay, a dose comparison table. What I've done here is I've just pulled doses out of TSPA-SR and SSPA, Volume 2, broken them out into eruptive and intrusive scenarios. I don't have the combined doses on this chart. And this column here is the peak mean probability weighted dose, and this is the conditional dose, and I've done that for both the eruptive and the intrusive.

I guess I'll say one of the points of interest I understand from the Board is we had an increase in dose for the peak mean probability weighted dose from SR here on the eruptive case to SSPA-2. Those differences are detailed in SSPA-2. Essentially, they break down to four areas. We had a change in our eruptive BDCFs where we assumed that an individual would be inhaling larger particles, thus getting a larger dose.

We had a wind speed change. There was some data we looked at that indicated that the wind may be travelling faster, getting more material down to the critical group.

We had a change in our conditional probability of once the dike intersects the repository, or a drift, what's the probability of that actually becoming an eruptive center. And then we had--I think that really accounts for the bulk of it there.

We did have a decrease in dose here. This played out in our groundwater case. Again, this is the intrusive and the peak mean probability weighted dose. And this amounted to a reduction in solubility for a couple of radionuclides, Neptunium and Plutonium. And then we had a partitioning at the drift rock interface between matrix and fracture, and eventually invective and diffusive flow.

There were some other things in here, too, but those were the main swingers for the decrease.

And, also, just as a note, these SSPA-2 conditional doses, these are estimated. We didn't actually calculate those in the documents. So, keep that in mind.

Just a quick summary. You know, we're concerned that the NRC model is simplified, very simplified, and idealized. And if you took it to a consequence standpoint, it would really be overly conservative. And as I said, it doesn't at this point consider the major elements of the repository system.

We propose to strengthen the analysis we've got in place today for LA by looking at the model they put on the table. The analysis we've got in the SR right now we believe is still defensible. We don't think that the model they've got in place calls into question the results that we necessarily have for our documents now.

And we expect our planned studies will continue to show low doses. We don't expect this to be a major concern from a regulatory standpoint.

I think that's all I had. Did I have one more bullet? No more bullets. That's all I had.

KNOPMAN: Thank you, Eric. Questions from the Board? Staff? Jerry Cohon?

COHON: Cohon, Board.

Could we go to Slide--I think we may have a difference in numbers--it's 7 or 8. Let's start with one--no, 8. That last bullet, in your description of it, you said assuming that all of these things happen. If you make that assumption, then why would you use probability of weighted dose as opposed to just dose?

SMISTAD: We could calculate it either way. This is the manner in which we will submit--probability weighted doses that we will submit for the license application.

COHON: No, this is more than a calculation issue. This has to do with a communication issue. If you assume it's going to happen, the probability is one; right? So, what's the logic of using probability weighted dose if you assume it's going to happen? What's the rationale?

SMISTAD: If we assume that their model is going to happen, or just an event period?

COHON: No, if you assume the event is going to happen. Isn't that what you were assuming there?

SMISTAD: Yeah. I've got someone raising their hand in a hurry here. Do you want to take it, Bob?

ANDREWS: This is Bob Andrews with BSC.

I think the distinction is we break the parts into the initiating event, and the probability of the initiating event occurring, and then all the subsequent consequences that may result given that initiating event. And I think what Eric is talking about here is we have not--with these numbers, we have not assumed the initiating event. We still have the probability of the initiating event in those numbers. But then all the subsequent things have been set to probability of one.

So, if you break the initiating event from the consequences, and you're absolutely correct, the associated probability of the consequences, not of the event, but of the consequences, we've set the probability of all those consequences equal to one, not the initiating event.

COHON: Thanks.

SMISTAD: I misunderstood your question, sir.

KNOPMAN: Knopman, Board. I'm going to just jump in here with a quick clarifying question.

You said early in your presentation, Eric, that of course this is all going to be backfilled. Did I understand that correctly?

SMISTAD: The drifts right now we're not planning on backfilling. But the mains on either side of the drift we plan on backfilling.

KNOPMAN: Okay. That was not clear to me.

Paul Craig?

CRAIG: The 2 to 3 MR per year is a really interesting number. You multiply it by the 10 to the eighth and you're up to fractions of a megarem. This is the kind of dose which kills people very quickly if you get it for a period of time.

I said the 2 to 3 millirems is a number which, if you multiple by 10 to the eighth, because of the probability of 10 to the minus eighth, gets you up to 200,000, 300,000 rems, which is really getting into quite a significant dose. Presumably it wouldn't be delivered all at once, so people would have, in principle, time to leave. But nevertheless, that is the kind of dose that kills people in a very short time. These are doses that play out over someone's lifetime.

SMISTAD: Over a long time. They may not be chronic.

CRAIG: That's right. That's not the right way to look at it, but it's getting up into really very, very large doses, and we hope the probability of that is extremely small. But in terms of--

ANDREWS: Let me clarify, Paul, if I can interrupt for one second, with all due respect.

The 10 to the minus 8 is the correct number, or thereabouts. This is Bob Andrews again from BSC. I'm sorry. Annual probability. We must consider these with the integrated, if you will, probability over the time period of interest. And the time period of interest for these analyses is 10,000 years, although we conduct our analyses well beyond that.

So, the appropriate probability you should use here is probably the integrated probability, because each subsequent event has its probability, annual, and its associated consequence, which are then summed in the analysis.

CRAIG: You're absolutely right. And we've gone around the lot on the--problems of displaying this kind of high consequence, low probability information, and we're probably not done with it.

But that wasn't actually the main question I wanted to ask. In terms of your concerns with the NRC analysis, you seem to be focusing on the details of the dynamic analysis. That's where you're concerns are. Is it correct that you have no problem with their observation, which is a new observation for me, that when you heat up the canisters, the embrittlement changes, so they're subject to--the mechanical properties of the C-22 change dramatically when they're heated up, and presumably their corrosion resistance property is also changed, so there will be a whole set of consequences with respect to transport down into the--through the UZ into the SZ into the biosphere? And probably these are not significant from a probabilistic point of view, but at least one should be aware that a whole set of other mechanical and corrosion properties probably do change.

SMISTAD: We haven't looked at this in detail. We're not at this point ready to say the packages turn to dust and are going to head down or head up. I think that's a very conservative assumption to make at this point.

So, we haven't looked at canister damage. And the other thing really is is how long are these packages exposed to these conditions? That's really a key point. Britt made that point as well. And maybe they're not exposed to these conditions long enough to where they change, turn into something else, they get ductile, they turn to dust, whatever, and that's something we still need to look at.

KNOPMAN: Dan Bullen?

BULLEN: Bullen, Board. Just could we see Figure 9 briefly? I just have a point of clarification. Maybe it's not. It's maybe your 10. Keep going. Okay.

I guess I want to know if it's actually 500 millirem, and then 125 rem, or should that be 500 rem for the mean conditional dose on the intrusive scenario? Is that a typo?

SMISTAD: I don't believe so. Are you worried about the--

BULLEN: Well, I just wondered how you scaled, because the mean probability weighted dose was .2 millirem at greater than 10,000 years, and then for SSPA-Volume 2, it was .05. And so I look at the scaling there, but then I've got a 500 millirem, and then it goes to 125 rem?

SMISTAD: Yeah.

BULLEN: Which is it? Is it millirem or rem, I guess is the question.

SMISTAD: That's probably a typo. Do you know, Bob?

BULLEN: You don't have to answer now. I just am curious as to what it might be.

SMISTAD: Should be the same factor of 4.

BULLEN: Okay, thank you.

KNOPMAN: Let me just call on Leon Reiter.

REITER: Again, when you look at the probability weighted dose, there's a conditional dose. As you pointed out, the probability weighted dose takes into account reduction in solubility, the flow focusing factor, and I'm not sure if that translates.

SMISTAD: From here to here, is what you're talking about. We'll check on that, Dan. I believe it should be the same factor.

KNOPMAN: Okay, we will look forward to some clarification on that for confirmation.

Dick Parizek?

PARIZEK: Parizek, Board.

At one point, you stated that the waste packages could be brought to the surface in a weakened condition. If the dike has a 1 meter assumed width, all waste packages are bigger than a meter, so won't they get hung up? Their contents might get to the surface, but could the packages come up a crack that's only a meter wide?

SMISTAD: Could the package itself come up?

PARIZEK: Yeah. That's what I thought you said, that the packages could be brought to surface.

SMISTAD: Well, essentially--well, that's essentially what we're assuming in our current analysis, and that simplified example I was talking about as well. We're not really worried about the package in this particular analysis. We're just assuming everything comes up. In fact, that's probably a very conservative assumption as well. I don't know that all the material, i.e. the fuel or the waste, is going to make it to the surface. Some of it's probably going to be trapped in the drift itself. Not all of it may be entrained through the conduit. There are a lot of conservatisms still embedded in the analysis we have on the table right now.

PARIZEK: That was also a point with your example or case two where you contain it all within the drifts underground. But then the question is what fills the drifts? You ought to have some materials in there that crystallizes. It's not going to be Tephra, is it? What do you assume is going to fill the drifts if this doesn't come to the surface?

SMISTAD: Right, it would eventually crystallize.

PARIZEK: It's a dike that's not Tephra. It's a dike that's--that could encase some of the waste packages?

SMISTAD: It could. I mean, that's conceivable. I mean, there could be some credit taken for that. It's all a function of the duration of the event and the pressures and the temperatures. But it's something that we'll look at as well. It may be lower on our list, but we'll take a look at encapsulation, if you will.

KNOPMAN: Dan Metlay?

METLAY: Dan Metlay, Board Staff.

Eric, could you go to your Slide Number 10? This is more of a kind of philosophical question. I want to call your attention to the first bullet where you say that the NRC model is simplified and, thus, overly conservative. Of course, DOE models are often simplified, and the IAEA/NEA peer review said that one of your models not only is overly conservative, but is incredible.

So, the question is how does the DOE develop concerns on simplification and conservatism? Is there any kind of standard that you would use to say one model is simplified and overly conservative, but another model is appropriately simplified and appropriately conservative?

SMISTAD: I think you've got to look at several things there. I mean, you've got I guess in the classical sense, you look at the data and information. You look at expert judgment, perhaps. And that's what we're doing in this case. That's really the main way we look at this.

But what I'm talking about in this bullet is if you took what are simplified assumptions, and we don't think they're necessarily real in our system, that if you carry that forward to a consequence calculation, that you would indeed be conservative. That's expert judgment in a way.

COHON: This is Cohon. Just to try to get out of this, will you accept a friendly amendment that you delete the word "thus"?

SMISTAD: Yes.

COHON: It's not being simplified that necessarily results in being overly conservative. It's the simplifying assumptions they chose to use.

SMISTAD: Yes.

COHON: Okay. So, it's the "thus" there I think that's bothering our philosopher.

SMISTAD: Okay.

KNOPMAN: Okay. Leon Reiter?

REITER: Eric, could you put the cost comparison slide on, please?

First of all, I want to correct myself. Indeed, it could be a misprint, because the effects of reduced solubility and the drip shadow effect would affect both doses.

SMISTAD: Yes.

REITER: So, it probably is a misprint.

SMISTAD: Yeah.

REITER: But on the other hand, if we can take that apart, we see the very large increase in the TSPA-SR and SSPA, Volume 2, and I went back and started looking at some previous calculations, going back to 1991, through the years looking at the VA, and the eruptive release, and there's no doubt that the numbers have increased tremendously since then. It's been monotonic.

Back, you know, ten years ago, and maybe even the VA, you had eruptive doses that could meet the criteria even without the probability weighting. And now you have doses that are up in the order of perhaps, you know, tens or hundreds of rems. Is there a historian of PA around here to tell us why? I don't know if there's any other example in the PA where you have this monotonic increase throughout the years. Can you or somebody else answer that?

SMISTAD: Go ahead, Bob.

ANDREWS: This is Bob Andrews, BSC.

I'll talk to VA. Going back much beyond that, I'd have to open up the books, I think. But for VA, there were two key assumptions documented in the VA that were driving the fact that the wind dispersal of the eruptive event was not transporting any radionuclides to any receptor at a, at that time, I believe we were using different distances, like 5 and 20 kilometers.

One of those is the grain sized distribution of the eruptive materials themselves, the Tephra sizes. At the time of the VA, and I'll get the numbers wrong here, but they were on the order of millimeters grain sized distribution, to centimeters. But it was in that sort of range of grain size.

When we put it into the analysis model report and used information from Argonne on possible expected grain size distributions, those numbers decreased by about a factor of a hundred, I believe. So, it's down in the less than a millimeter, tenth of a millimeter sort of range. So, they were much more transportable to that distance.

In other words, in the VA, the Tephra would have been, although I don't think we ever plotted it, would have been within kilometers of the event itself, not tens of kilometers within the event itself.

The other aspect I believe had to do with the wind speed, and the wind rows that were used in the VA. We actually used a wind row and a wind speed corresponding to fairly low elevations, low heights, if you will, of eruptive events.

In the SR, both the wind speed and the height of the eruptive event were more directly and appropriately considered. So, again, allowing for greater in this case down wind dispersal of the Tephra materials.

So, those two fundamental differences between the VA and the SR gave the difference between essentially zero dose, I don't know what the numbers actually were, but they were 10 to the minus 6, or 10 to the minus 7 millirems per year, two numbers that you see here that Eric has presented.

KNOPMAN: Thank you, Bob.

I think we'll move on now. Steve Frishman is our next speaker. Steve is a geologist with the State of Nevada, is well known to the Board. He's appeared before us many times, and he's got some comments on consequences of igneous activity.

FRISHMAN: When I put these comments together last week, I didn't realize how sobering it would be to talk about the representation to decision makers about future low probability, high consequence events. And I think maybe it's a little more telling now than it was before.

This will be sort of a continuation of the undercurrent discussion that's been going for a couple days now about how igneous events and their consequences are represented. And I became particularly concerned about it when I looked through the suite of documents that have flowed out over the last few months, and finally at the preliminary site suitability evaluation itself, and as you know, and as you, I have been concerned about this representation for a long time.

So, I wanted to go through a little bit about how it is represented by DOE, why it's represented this way, primarily on direction of the NRC. And then just a few comments that people have made about it, and then try to unwind it so that a normal person might be able to get a feel for what is advertised by DOE and recognized by everyone through the existing performance assessments.

Other than the container failure directly, this is the only failure mode for Yucca Mountain repository, so people should have a clear understanding of what it does mean, regardless of its likelihood. Because it's obvious to anybody that there is some likelihood, if you stand on top of Yucca Mountain and look at volcanic cones that are young, then it is obvious to any decision maker who would recognize a volcanic cone that there is something going on here.

So, if we start out with how it's represented in the preliminary site suitability evaluation, and I think we've all seen these curves, it's a relatively simple representation if you can understand what the curves are, if you can do it without color, if you can get a feel for why the two are different, the two curves are different.

So, this is one of two figures in the preliminary site suitability evaluation that's supposed to tell you about the disruptive volcanic event. And as we all know, these are sort of composite figures. One of the reasons that the curves are different is that the probability of intrusive versus extrusive has been changed between these two cases. The dose conversion factors have been changed between these two cases, and Eric told you about a couple other things that had happened. But it's not clear from the document why they're different, and it's also not clear from the document why they appear to be essentially negligible doses.

This is the other of the two figures in that same preliminary site suitability evaluation, where the nominal case has been combined with the volcanic case, or the igneous case, and again, there are questions about why does it look the way it does, and why did the nominal case not have it in the first place. But this is the second of the two figures presenting the only failure mode other than failure of the waste container under sort of operating conditions.

Now, why is it being done this way? The reason is because the NRC said to do it this way. And here's the NRC's direction from the issue resolution status report that DOE is following literally and feeling that if they do this, then they have properly represented risk of igneous activity in the failure of a repository. And these are the words that are being followed.

"Under the proposed 10 CFR Part 63 rule, the expected annual dose is used to determine compliance with the proposed performance objectives. Expected annual dose is the dose weighted by the probability of event occurrence, i.e. risk, with the maximum annual risk during the post-closure period used to determine compliance."

So, what we have is the invention of a new word, or a new phrase, and that's "expected annual dose," which actually means risk, according to NRC speak. And it's that pair of sentences that is at the root of why we're even having this discussion.

Now, other people have noticed that there is a little bit of a problem in this representation. And the first time it really became clear what the problem was and how big it was was when the International Peer Review Group on Biosphere starting looking at effects of volcanism. And we had a long discussion, I happened to be invited to that meeting, along with some others, and it took about 15 or 20 minutes, with the help of Bob Andrews, to figure out what the curves actually meant. And then all of a sudden, it sort of dawned on everybody that this is what's going on.

But here's ultimately what that Peer Review Group said in their final report, because they stumbled across this problem of how you represent igneous disruption.

"The IRT--that's International Review Team--suggests that even if probability-weighted dose is the main output required by the regulator, it would be desirable to present disaggregated information (doses and probabilities). This information would more clearly illustrate the nature of the potential impact, so as to better inform decision makers and other interested audiences."

Pretty clear from their International Peer Review Group, who accidentally unearthed this problem of representation.

Now, I won't leave this up too long, because you're all very familiar with it. You, almost a year ago, said essentially the same thing in a letter to the Department of Energy.

Since I became concerned about this, I've written two papers for various reasons about this problem that have been published both in proceedings of VALDOR, which is a conference that is held in Sweden every couple years on the theme of the values and decisions on risk, and I talked about it in that paper, plus in another one, Environment Reporter, where essentially I'm saying the same thing. What's happening is there's a manipulation of the meaning of dose and risk, and it all comes from the expected mean annual dose definition that the NRC set up as a means of implementing their risk informed performance based regulation to a point, in this case, to a point of sort of ridiculous implementation.

Now, let's look at the same curve and start seeing if we can understand anything about those two curves that were in the preliminary site suitability evaluation. This first one, we can make it a little bit clearer when you take the colors away, make it a little bit clearer by saying which piece of which curve is which. This is actually combined all the way through. This is the nominal, and you see the connection here. This is igneous, without combining.

So, we see how they come together. This is the combined. This is the uncombined igneous. This is the nominal. We're pretty familiar with it. And, now, over here what we have is mean annual dose, which is, according to NRC speak, a misnomer. It actually should be expected mean annual dose, if you're following the prescription on how to do the calculation. And, in fact, it should be risk, because then people have at least a possibility of understanding what this graphic means.

And now we take the second one, and have to do a lot more writing. Start out, we make sure we understand which curve is which, and as we talked about before, and as Eric did, we have some understanding of why the two of them look different now. If we're going to unrisk inform and actually look at consequences, we have to sort of up the X axis a little bit, because we're dealing with a probability of 10 to the minus 7 per year.

So, if we start looking at what the consequence would be without risk information, you recognize this 13 rem from Eric, but this 13 rem is not even a very clean picture. If you look at the description of where this 13 rem came from, and it comes very deep in another document, and I'll show you where that is in a minute, if you look at where this 13 rem comes from, I agree with that number, with the understanding that that 13 rems represents a dose that does not include an inhalation dose, and it's in the eruptive case, and the inhalation dose that we would have to add to that is on the order of 3 1/2 rems per hour for the number of hours that the eruption is actually dropping ash in the RMEI's corner of the house.

So, even that is not a clean number. But we'll take it for what it is if we can dig through enough thousands of pages to get to what the qualification is on that number.

So, what we're looking at is 13 rems at 100 years, based on unrisk informing what is going on down here.

Now, if we look at this peak right in here out at about 300 years, and unrisk informed again, meaning get the probability out of it, we come up with a conditional dose from this curve that is somewhere between 500 and 1000 rems per year. And we could argue about the specifics of where it is between those two, but it's somewhere between those two having to do with decay, having to do with essentially the decay of the probability in the calculation.

I've been told that other people who have tried to calculate this see it as high as maybe 13 to 1400 rems.

So, now we start cleaning it up. Now, again, we have to make sure we have the proper NRC language here, or maybe the proper language that other people speak. So, this is the information that decision makers and the public should have out of the only alternative failure mode for Yucca Mountain repository, and they're just not getting it.

I told you I'd show you where I learned such things as the 13 rems doesn't include the inhalation dose. And it's in the SSPA, and if you can trace all the way down to 3.3.1.2.4, you see that they have a reasonably complete description of the conditional case. But you can't find it unless you know what you want to look for, and unless you know which document you have to go looking in, and I don't think that the interested public or decision makers are going to do much more than maybe look at the executive summary of the preliminary site suitability evaluation that makes absolutely no reference to this issue at all.

So, what we're looking at is yes, it's absolutely correct and perfectly useless, the way the information is presented.

Also, it's kind of interesting to get an insight into what the Department actually sees is happening when they risk inform this analysis. The peak conditional dose from eruption are significantly higher, but it reverses, and they make it almost sound as if the reversal is a good thing. It turns out that if you start probability weighting it, you get the exact opposite effect of what the peak dose would be. And I think that speaks even more towards the ridiculousness of risk informing when you know that it's wrong, when you know that it gives you conclusions and numbers that are not only misleading, but they're invalid. They're numbers that mean absolutely nothing. And I think it's clear here that the people who are implementing this understand it, and you can interpret for yourself whether it appears that they like it or not.

And, finally, just two weeks ago in Nature, the British journal, they took a pretty hard look at the Yucca Mountain program, did a pretty long article, and also an editorial. In that editorial, they speak in a way that's kind of interesting to interpret, because, you know, Great Britain has had considerable problem with their developing repository program, to the extent that they ditched the program and they're starting over again.

And in the last VALDOR session in Stockholm last June, there were people there who were from Great Britain who were actually almost giddy with the fact that they were getting a chance to start over and try to do it right.

So, in this editorial, you can see the editorial writer at Nature sort of talking about the U.S. program, and applying some of their new thinking about acceptability in waste management and disposal. But I took just one line out of it where they're referring to the recent report from the National Academy of Science Panel on disposition of high-level waste, the one that came out just very recently that a number of us in this room at least attended the long opening session for. And this is what the Nature editorial writer thought was a very important point to make in their editorial, and this is the NRC's report, the one that we're all familiar with, and the two pillars: complete openness with the public, and external scientific peer review.

These are things that this Board has talked about since before it was born, and we have I think maybe the most telling example yet that I've been discussing here. What happened with complete openness to the public? What I've just presented is complete confusion to the public, and a very clear attempt to not be open, a very clear intent to obfuscate what the true consequences really are.

And on top of that, just as sort of a side benefit, we have an International Peer Review Group that told them what they should be doing to be open with the public, and they ignored that, too.

Thank you.

KNOPMAN: Thank you, Steve. Questions from the Board?

Dr. Cohon?

COHON: Cohon, Board.

Steve, I'd like to pursue one detail to make sure I'm not missing something.

As you quote NRC, they call for the use of the expected annual dose. Now, I interpret that to mean the expected value of the annual dose, and DOE has chosen to use the mean as that expected value. So they talk about mean annual dose. You seem to be saying that's not correct or consistent, and you want to use the word "expected" rather than mean dose. I don't see the problem. It seems to me that DOE is being consistent with the NRC rule.

FRISHMAN: I think that the NRC invented a word or a phrase that they define as meaning risk.

COHON: That may be.

FRISHMAN: What they've done is--what I'm talking about is they have called risk expected annual dose. DOE has done the calculation as prescribed here, and they call it mean annual dose, just as they call all of the rest of their curves mean annual dose.

So, what they have done is they have followed the requirement for the type of calculation, but they have not used the NRC prescribed language. What they have done is presented it as a dose, when I think we all understand what we mean when we say dose, and instead of using the NRC's language, expected annual dose that actually means risk, which is what the calculation actually is. So, it's a semantic thing where the NRC has defined risk as expected annual dose, but DOE presents it as dose instead.

COHON: Well, let me just be clear from where I'm coming from. I'm not in any way arguing with your main point about how one handles low probability and high consequence events. I'm talking just about the sort of semantic issue you've raised here. I still think that what--in fact the quote you provided--it seems to me that if you have an issue here, it's with the NRC language, not the way DOE has implemented it.

FRISHMAN: I have two issues. I have one with the NRC language. I have another with the fact that DOE didn't use the NRC language. And if they're going to follow the NRC prescription for calculation, they should label the answer the way the NRC has defined the answer.

COHON: Okay, thanks.

FRISHMAN: And then at least we'd know where to go to try to figure out what mean annual dose means on this particular representation.

KNOPMAN: Any additional questions for Steve? We're going to have more of an opportunity now as we ask the other two speakers, Britt Hill and Eric Smistad, to come up to the front here, if we can just get the table set up so that the three of you may sit in front of us.

(Pause.)

KNOPMAN: If we could get our speakers to get seated, we will take a break after this discussion. I'd like to get this moving right now.

While they're getting themselves settled, I'd like to ask Nick Apted to just say a few words. Nick is with the Electric Power Research Institute, EPRI. And do you need the podium, or do you want to just speak right here? You have a viewgraph? Okay.

APTED: I'm Nick Apted. For those of you who may not know me, I've been active in the area of safety assessments for high-level waste repository internationally for the last 15 years, and particularly relevant to this set of discussions, the last five years, been very active in the Japanese high-level waste repository program, dealing with scenario analysis, particularly in their case, volcanism is one of their key leading scenarios for consideration.

Now, I'm a member of an EPRI team of experts, independent experts, that are brought in to sort of conduct an oversight on the activities centered around Yucca Mountain Repository Program. Two of the additional members of the team were supposed to be here. Of course, because of events in the East Coast, have been unable to arrive. So, I'm going to focus more on my portion of what I planned to say here. I'll touch only briefly on some of the points that they I think were going to make. But I'm afraid that I don't have their backup information with me to present.

The two points we want to really hit upon, one is methodology and one is the science behind this what we all realize is a very significant, very serious what if scenario to be considered. I'm going to be addressing most of my comments to, if you will, sort of the resolution methodology.

For those who do scenario analysis, I think it's all recognized that it's absolutely a bedrock principle in using scenario analysis to resolve these type of what if issues, that we approach it from a top down basis, that we don't get lost in sort of a bottoms up of what are the wonderful issues and sciences, studies that we can do. But if we're going to have an effective R&D program that's going to lead to some sort of reduction in uncertainties and understanding and ability to have a public acceptance of a resolution of some of these very difficult scenarios, we're going to need to do it in a very systematic and a very what we call top down, consider these issues in a top down fashion.

And I'll show you what that means with respect to this particular scenario in a second. But what I want to comment is that from afar, and we've not been involved in the KTI process, it's certainly appropriate that NRC and the DOE have identified this as a key scenario. The State of Nevada has shown exactly the level of concern that they should to this important issue. But we're worried that the approach seems to be a bit unfocused and unsystematic, which is, again, very characteristic of a bottom up, sort of a rush to judgment of all the R&D that might be needed to resolve this, versus a more systematic approach, looking at it from above.

I'm sure--excuse the focus--I may slide this around a little bit. But the point is to take this from an appropriate point of view in terms of international practice, in terms of trying to get to a resolution rather than gravitate toward all of the R&D that might be necessary to look at this, is to look at each of these questions in turn.

For example, this same type of approach has been very successfully applied to the issue of colloids, colloid issues of colloid formation, colloid stability, colloid migration. Let's look at these in a sequential way.

If at any point in this decision tree we can answer one of these questions no, we greatly mitigate, if not indeed eliminate, the impacts, the consequences that are being speculated on in terms of this scenario. I mean, starting at the top, and quite appropriately in the past, there's been this probabilistic hazard for volcanic analysis. One of my co-speakers today was going to be Professor Sheridan, who was a member of that panel, and he had a number of perspectives on exactly what was done in there, how uncertainties were treated, and so on.

But presuming the answer is yes, we move on to these other issues of does the dike intrude the mountain. We heard earlier today from Leon Reiter that, you know, one of the experts of the Board itself has in a sense a different aspect, a different viewpoint, a different conceptual model for some of these questions.

Britt was very firm and very informed on his response to that, but I think we need to look at this now that there are divergents in technical opinions here.

We also heard that, and I was very pleased to hear Britt use the term speculative repeatedly in his talk. I look in vain at the presentations to find the word written down that these are speculations. I think that's a key word to keep in mind. Speculations, good, honest technical experts at this stage of a lack of information, in a speculative state, can disagree. We need to bring those opinions together. They can have an important difference on whether we proceed stepwise down to the bottom of this graph, which is basically full funding of every possible aspect, every consequence that we can see illuminated in the analysis we've seen.

KNOPMAN: Nick, excuse me. We have a limited time for this discussion.

APTED: And that's what I was going to say.

KNOPMAN: Okay, that's fine. Thank you very much. And people can question you later.

I also just at this time, so we get it on the record, asked John Stuckless from USGS to very briefly summarize what the review activity is on this issue that's going on in the USGS, so we have that on the table.

STUCKLESS: Stuckless, USGS.

I can make this extremely brief, because I had a one sentence answer from one of my reviewers. I think that the assumptions invalidate the entire analysis. This is in reference to the two papers we gave them to review. But, in essence, data existing at Yucca Mountain say that the strength there is insufficient to prevent the dike from going all the way to the surface.

And then my other reviewer--these are two reviewers from the CVO, the Cascade Volcanic Observatory, whose job is nothing more than volcanic hazards. The other one points out that the type of eruption being called for has never been documented as the first stage of a basaltic eruption. And the types of deposits that they should make have not been reported at Yucca Mountain, or anywhere else.

And then the last thing I'll address for Leon is one of the reasons Bill Melson and Mike Sheridan do not like these analogs is first of all, they have too much water in them. They also feel that the DOE's modeling is using too much model for these types of basalts.

And the second thing is in order to get those sorts of explosions, you apparently need to have a plugged volcanic edifice. And that obviously does not exist at the early stages of volcanism. There's quite a bit more in here, including some math that I don't follow, but I'll leave this with you.

KNOPMAN: Okay.

SAGÜÉS: May I? I have a problem here, a little bit of a big picture problem. I'm having a hard time following who you're talking about, and in response to what you're talking about, and who was saying what. Can you back up just a little bit for some of us who may not be aware of the intricate history of what is happening?

STUCKLESS: All right. Very simply, the USGS is going to have to comment on the site recommendation. So, the director's office--you know, we had these two papers that have been written by the Center and their contractors.

SAGÜÉS: The Center? Who's the Center? Okay, and now the director, who's director?

STUCKLESS: Ours. Director of the U.S. Geological Survey.

SAGÜÉS: Okay. I'm sorry, I'm just--I'm very confused as to who's what.

STUCKLESS: I'm trying to go too fast here.

In any event, the two reviewers were Larry Maestas and Roger Detlinger (phonetic), both of whom are employed at the Cascade Volcanic Observatory as physical volcanologists. Okay? And this is their review, so I'll leave that for you.

SAGÜÉS: The use of this paper.

STUCKLESS: The use of this paper.

KNOPMAN: This paper means the paper that's been submitted, just for the record, it's a paper that's been submitted to the Journal Science; is that the one you're referring to?

STUCKLESS: They reviewed both of the ones that the Center provided--

KNOPMAN: Right. There is one that's entitled Modeling Magma Drift Interaction at the Proposed High-Level Radioactive Waste Repository at Yucca Mountain. That's by

Woods, Sparks and others. And there's a second paper that--okay, I'm sorry, that's by Woods, called Explosive Magma, Air Interactions by Volatile Rich Basalt, and the Dike Drift Geometry, and that's been submitted to Journal Geophysical Research?

STUCKLESS: That's correct. It's in review.

KNOPMAN: It's in review, okay. So, the two articles that John is referring to that USGS is looking at are these two. Okay. All right, now--

STUCKLESS: I have one other comment from me, since I know something about erosion out there and the Tephra deposits, and that is that the Lathrop Wells eruption left a record in almost all of our fault trenches of nearly a quarter inch of ash.

We also have studies of erosion rates on the mountain, and within the channel. Within the channel, when we get a major rain storm, we have had chains buried ten feet deep that have been picked up and washed.

So, you're talking about whatever lands in the channel being quite small, and then being mixed with up to ten feet of sediment scoured out of the channel. So there's probably quite a dilution effect.

But, in terms of the hill slope erosions, very little comes off the hill slopes except as mass land movement, and that rarely makes it to the channel of Forty

Mile Wash.

KNOPMAN: Thank you, John.

Let's get our interaction going among our three panelists. Perhaps Britt could start off by, if you want to respond to the comments you've heard so far?

HILL: I really don't want to turn this into a technical debate on a number of issues that we're only beginning to hear about. I would just invite any interested participant to come to the next noticed or open technical exchange or Appendix 7 where we could talk about this at a technical level, rather than the approach today.

COHON: I have sort of a procedural question. I'm struck by the sort of veneer of NRC and DOE saying they're in agreement on something. Well, you are, you were pleased that you reached agreement on something, and it sounds like you agreed to disagree.

And the other question is--well, that's sort of a statement. The question is what does closed-pending mean in a context like this where there's such sharp technical disagreements?

HILL: I think what we're agreeing right now is that the DOE will provide information needed to support a license application, should they decide to submit a license application. Before, we had no agreement that the DOE would do additional work to support what we believe are technical concerns. So, we do not agree with the current approach necessarily, nor do we necessarily disagree. Our point is that we need additional information to reach a licensing decision, should a license be submitted. The DOE, as of last week, has agreed to provide us, by the time of the license application, with that additional information.

COHON: And is that exactly what closed-pending means then?

HILL: Yes.

COHON: That agreement to provide additional information that you've specified?

HILL: Yes, it only means an agreement to provide additional information in support of a license application. It does not mean that we necessarily agree with the DOE's approach, or that we would say at licensing, we would have no additional questions or concerns.

KNOPMAN: Thank you.

SMISTAD: Just a quick comment on what we agreed to, and I don't have the exact agreements with me right now, but I briefly went over what they were. We'd agreed to evaluate the model, and I'll call it the new model, along with some other work as well. We haven't agreed to incorporate anything at this point. We've agreed to evaluate it for a bases for the NRC to look at, you know, come license application. So, it is pending. The pending is that analysis or that evaluation come that time frame.

KNOPMAN: Dan Bullen?

BULLEN: Bullen, Board. This is actually a follow-on to the same kind of agreement that you came up with. And maybe it's Eric's answer to what's the impact on cost and schedule? How long is this going to take? How much is it going to cost? Will you have the data readily available by whatever license application time frame is? And what kind of impact is this going to have on the program?

SMISTAD: I think it's yes, no, probably and maybe. I'm not sure. No, I mean, what we're doing now, Dan, is we've agreed to look at this. This is just a recent development here. We've got a plan in place. We're working on costing that plan and scheduling that plan right now, as I mentioned in my presentation. We expect to address what we've agreed to address, and that's evaluate this model. I can't tell you right now the exact schedule of delivery of that or the cost that will be in place for that. There are priorities that are going on right now within the Department in terms of funding, and what not, and schedules.

BULLEN: Bullen, Board, again.

Just then a followup is that the deliverable time is prior to license application, or is there actually an FY 03, FY 04? What did you say in your KTI agreement?

SMISTAD: I think we said prior to LA, and that's when it will be. I'd also like to just continue a little bit in the vain of what John Stuckless was talking about.

We had asked, specifically what John was talking about, we had asked the USGS, the Department asked the USGS to look at these two draft papers, and I think we all understand what the draft papers are now, and distribute those to volcanologists or experts within the USGS. They got permission, as John said, from the Director's office in D.C. to do that, and they contacted a couple individuals at the Volcanology Lab in Vancouver to do that, and we have their comments. And this is something that we'll consider, these comments, they may get more in depth in time, but consider when we go forward with this analysis.

And we are, in fact, still seeking and will get input from volcanologists at LANL that are outside of our program, earth scientists at Berkeley that may be perhaps outside of our program, but other folks, knowledgeable folks, to look at these papers and the information in these papers to see how we might go about addressing the model and the issue.

KNOPMAN: I have two questions here. First, I'd like Eric, if he would, to respond to some of Steve Frishman's comments so that we have an idea of how the Department feels about that.

SMISTAD: I think Steve quite frankly has brought up a semantics issue. In my mind, it's words. We do report unweighted doses, if you will. I showed them today. We show them in our documentation. I think the word risk may be absent there, and that's something we might want to work on, is making this more communicative to people who aren't knowledgeable in the details of these sorts of calculations, risk calculations, that is.

KNOPMAN: Okay. My second question follows up on Dan Bullen's question about timing. This is a site suitability question. The research plan that appears to be agreed to by DOE and the NRC, it's really in a licensing application time frame as opposed to a suitability recommendation time frame. And I think it might be some benefit to talk a little bit more about how the department actually feels that can take place when they can delay the resolution of some of these questions, fairly serious questions I think that have been raised here by the model that Britt has explained.

So, perhaps you could just elaborate a little bit more, Eric, on how the--a question that relates to site suitability is being handled over the next two or three years, when site suitability is coming up as a decision.

SMISTAD: You're really asking the question how does the new model that perhaps may result in higher doses play into our site suitability decision.

KNOPMAN: Right.

SMISTAD: You know, I tried to address that in one of my viewgraphs there. The Department doesn't believe that, again I'll call it the new model that's been put on the table, invalidates or calls into question the suitability of this site. We aren't expecting this, and in fact we've done these scoping calculations, we believe are the absolute worst case, and maybe even beyond an end member in my mind, that shows that we're not calling into question our suitability decision.

KNOPMAN: Okay. Steve?

FRISHMAN: Just for the record, I think I need to say that having been in nearly all of the KTI technical exchanges, and looking at the array of agreements that are now outstanding, it's pretty clear to me in the context of the Waste Policy Act and in the context of just rationality, that the project is not ready for site recommendation, because the intent of Congress is pretty clear that site recommendation is essentially the gateway to a license application to immediately follow. And there's at least from all appearances--I don't know, I'm just thinking through the array of work.

If it can be done in two years, I would be really surprised, and some of it looks as if it would take considerably longer than that.

So, at this point, the Department in its rush towards site recommendation, just to have gotten over that hurdle, is lacking any technical merit or credence as far as we, the State, are concerned, and we will pursue this point however necessary.

KNOPMAN: Eric?

SMISTAD: Let me just stop for a clarification on what I said. When I said the site suitability decision, I meant the decision for the Yucca Mountain Project to proceed with the documentation of the decision by the President or the Secretary of Energy.

KNOPMAN: Okay. Dave Diodato?

DIODATO: Diodato, Staff.

I'm personally laboring to come to an informed opinion about this matter, which the Center has painted a scenario which is, you know, on the face of it kind of alarming. But if we look at Megatta's (phonetic) list and then skip down all the way to the middle where the magma flows into the drifts, but we won't go as far as explosive volcanism, the question would be to Britt Hill then you're an expert, you've had 20 years of experience in volcanism, volcanology, do you have a feeling about the relative likelihood the scenario Dr. Parizek pointed out earlier of breaking the waste packages versus entrainment of the waste versus encapsulation of the waste? There are two scenarios: encapsulation versus entrainment. Do you have any kind of an expert opinion about the relative likelihood of those two phenomenon?

HILL: Well, first, this is Britt Hill from the Center.

First, I'm very reluctant to speculate on a number that has risk significance, or potential risk significance. So, I can't comment on the likelihood, and I don't think it's responsible in my position to speculate on the probability of these alternative scenarios.

Second, I would just fall back to a fundamental approach of we're dealing with pressurized fluid that contains a gas that's under compression. Now, I would never hold up the model that we have, the one dimensional and two dimensional models, to say this is the accurate representation of what is going to happen.

While I don't know the exact values, I know as a volcanologist that when you decompress a pressurized fluid and it has an expandable phase, that phase will accelerate and will flow in the voids.

Now, will that fail the waste package immediately? I don't know. It's a possibility that it's going to need to be analyzed. Will magma flow into the voids? I am very confident it will. So, you're going to be having molten basalt at some temperature flowing into these voids and essentially surrounding waste package and engineered materials in that drift.

Now, if the waste package itself is entrained into a conduit, I've seen conduits. I've been to sites where we look down 300, 500 meters below the surface, and I see these evolve from a dike that is one meter wide to ream out solid rock, the conduits that are on order of 10 to 50 meters in diameter. And all the material in that hole comes up the pipe.

Now, I can't tell you with any certainty what percentage of the waste package would necessarily come up that pipe. What I can tell you is that we would have sufficient information for a licensing decision by saying that all the material in that hole has erupted. And I think as an expert in volcanology, it would be very difficult to say that I could have a defensible position that 90 per cent in that hole would come out, or 80 per cent of that hole would come out. So, there's the hole coming up to the surface, the conduit itself.

And the second part of this process is the packages that remain in the drift. Now, there's a lot, I think, of work that can be done about evaluating the resiliency of these systems, but on the face of it, when you put a waste package to these temperatures for a month, and put this sort of a fluid pressure, because again we're dealing with a contiguous fluid, it's not an isolated little pulse, the roughly 10 to the fourth to 10 to the fifth cubic meters of a drift is completely overshadowed by the 10 to the seventh, 10 to the eighth cubic meter eruption.

DIODATO: Do you have a time estimate in terms of person years for the amount of time that would be required to reduce the uncertainty in the two processes which at present, you say you can't assess the relatively likelihood. But do you have an estimate about the time that would be required to do that?

HILL: We're continuing to work on both the experimental and numerical approaches this year at a sustained level of effort to last year. I don't think we're going to be getting a probability of the different events, and we may have to go forward by treating these as alternative conceptual models rather than stochastically sampled scenarios.

DIODATO: Equally weighted in terms of probability?

HILL: No. You would evaluate the alternatives. Here is scenario one, here is scenario two. What is the probability weighted expected annual dose difference, and then do we have a basis to say which of these models presents a licensing decision that will not under estimate risk to public health and safety.

DIODATO: Thank you.

KNOPMAN: Okay. Do you have a closing comment, Eric?

SMISTAD: Just a quick followup to what Britt and Dave were talking about here. You know, when Britt talks about perhaps a 30 day event, and, you know, this is possible, but I think the point to be made here is that for a full 30 days, we're not going to see--if it is a pulsing system, we're not going to see a constant pressure and a constant temperature. There are variables here. I just wanted to make that point clear. It's not, you know, the max pressure, and perhaps the max pressure--or max temperature through time.

KNOPMAN: Okay. We're going to try to wrap things up. Britt, did you have another clarifying comment there?

HILL: Right, and that's in an engineering analysis, you're looking at dynamic pressure variations as opposed to a static pressure variation, which gives you additional stresses. I agree with you completely, Eric, this is a very dynamic system, and in order to ensure public health and safety, we're going to have to evaluate the dynamics, not just the simplifications.

KNOPMAN: Okay. Steve, did you have--

FRISHMAN: I just wanted to add that duration of the event is very important in this system, and the Department uses a pretty short duration that I have not been able to discover the origin for. But it factors heavily into the model results. So just it might be just a point that somehow the duration of that mean event needs to be validated.

KNOPMAN: Okay. Lake, did you also want to speak?

BARRETT: Yes, very shortly. Lake Barrett, DOE.

Let me try to summarize this up and put this in context. This is a very, you know, interesting model, and there are going to be infinite number of these that will always come up. And it's healthy. We do it twice. We do it for ourselves, the NRC will do it, the Board does it, as we should be doing this. But they're always going to come up at all times.

Now, on this one with the NRC, we have reached an agreement on what work should be done to resolve this issue, and we've mutually agreed to that. And we will do this before the LA. We will do it as soon as we can. We would like to have done it yesterday, but we only can do what is--we can balance this with the hot and cold and all the other issues as well.

But as these new conceptual models come up, new ideas come up, new challenges come up, we need to deal with those. Where it basically stands at this point, we did not stop our process of this. We didn't feel it was necessary. We did debate it, and we had a discussion about it. We feel we can accommodate it within what we have now. And we do this whenever a new issue comes up. But new issues will always come up, and there's always uncertainties, and there's always questions and always challenges.

So, we will look at it and continue on and evaluate this question, and other questions as we go forward. And if it's significant enough in our judgment to stop the process at that point, we will do so. We don't think this one is, in our judgment. And that's kind of where we are. So, we will deal with this in the context of all the others as we go forward.

Thank you.

KNOPMAN: Thank you. In the interest of time, I'd like to take a break now. Instead of 15 minutes, I'd like to just do this for 10 minutes. We'll reconvene at 10:35, and we'll be trying to finish up around noon.

(Whereupon, a brief recess was taken.)

KNOPMAN: Okay, our next speak is Bob Andrews, and right up there with Mark Peters, one of the longest running shows in front of the Nuclear Waste Technical Review Board. Bob will be speaking about some of these quality assurance issues. He is with Bechtel SAIC. He's the science and analysis project manager. Okay, Bob.

ANDREWS: Thank you, Debra.

The Board asked for a discussion of some quality assurance issues, and where we are in the status of resolving those, and I want to walk through three of them.

First was a letter from NRC that then had some precipitating events following that letter. The letter was sent to DOE on May 17th. Prior to that, there was a phone call from NRC on May 4th regarding some apparent errors and inconsistencies in the TSPA-SR technical documents, of which there are two. There's the TSPA-SR technical report itself, and the TSPA-SR model document that supports that.

Now, I want to walk through some of those and what were the follow-on activities resulting from those. The other two are respect to some quality assurance corrective actions that are in progress as we speak, and I want to talk to you about the status of those. One regards model validation, and how models have been documented and the validation of the models they used in support of TSPA-SR, and the second one is with respect to software and how software is managed, and the configuration management of that software used in development of the analyses that in turn were used in the development and supporting the TSPA-SR. And I want to conclude with a path forward.

There are some other not really quality assurance issues, but questions that the NRC has asked to support their sufficiency review of the documentation that the Department of Energy has prepared. That relates to having used unqualified data, data not qualified per the applicable procedures. It might be literature data, or pre-existing data, early data, and its use in DOE products.

And then the same question for software, software that has not gone through the full QA qualification status. It's at some stage in that qualification status, but it hasn't been completed yet.

Those issues were addressed in a letter from the Department to NRC on August 31st, and the impacts of having used unqualified data and unqualified software, but I won't address those in this particular talk, due to time constraints.

So, starting first with the NRC identified concerns, and in the attachment to this briefing, the last 12 or 13 pages, goes essentially one by one through the ten issues that were raised, the exact page where the issue was identified, and a summary of the response to that issue as it was given to NRC in a QA meeting in June of this year.

The actual formal letter response to NRC on those ten issues was submitted to NRC in July sometime. So, point by point, issue by issue, question by question was addressed in letter form in July. It was in viewgraph form in June, and I've simply cut and pasted those exact viewgraphs into the last of this talk.

But I think it's worthwhile to talk a little bit about what kinds of discrepancies was NRC identifying. I think it's probably useful for all of us. We are all human. We all make mistakes in our documentation, and we can learn from those mistakes in our documentation. But it's useful to understand what's the nature of these potential errors, because if taken out of context, you know, it might imply that there's some real errors somewhere in here, not that these aren't, but I think it's important for us all to evaluate the significance of these things in the context of the decision making that's going on as we speak right now.

So, I have picked a few of the examples, and as I say, the details are in the backup. First was in a few tables in the model document, there was inconsistent values. So, sometimes we said it was long time and it was really a short time, where long and short are, of course, relative when you're talking about performance assessment. Long is greater, 100,000 years, and short is 10,000 years, or so. And there were inconsistencies in those tables, and those tables were inconsistent in a few cases with the actual text that was describing those tables.

And that's true, there was an error in one of those tables. It was a remnant, in fact, of earlier versions. This was a document produced over a six, seven month period, went through about seven or eight versions before it was final and approved. And there was one remnant of an earlier version of a table representing an earlier realization that was being documented in that table.

In addition, there was another table that had some slight differences in the fourth significant figure of the values presented in the table. I think it was pH values or carbonate concentration values.

In both cases, the analyses we did and documented in the letter showed there was no impact on the TSPA-SR results, or the model itself that was used to generate the dose calculations that had been looked at.

Another case had one exponent in a functional relationship. There's a lot of functional relationships in the TSPA where you're correlating one parameter to another parameter. In this particular case, it was the in package chemistry that related to the carbonate concentration used inside the package, which then in turn affects the degradation rate of the waste form, which in turn affects or could affect the release rate from the waste form, so it was a kind of a correlation of chemistry to the impact on waste form degradation.

And there was one input relationship that had the wrong exponent. It should have been a minus 10, and it was a minus 8.3, or vice versa, if you will, a typo on the input file used to generate the results.

We looked at this also, and that particular exponent made, first off, it made no difference to dose at all, made no difference to release rates at all, and that difference that it did make was, you know, less than a 1 per cent difference in the actual dose, and it was even on the conservative side, that particular error.

Another one was that models from the supporting AMR that supports the TSPA, the supporting AMRs say this model is applicable within X and Y range, can be parameter range, can be conceptual range, but it generally says it's applicable within this range.

One model, based on incoming chemistry, was in fact run outside of that range, outside of the range, it was a pH relationship on solubility, or a chemistry relationship on solubility, and we ran outside of the range. This was discussed with the originator of the AMR, but the AMR was not changed prior to the TSPA being changed. It was still applicable. It's just that the AMR needed to be updated. So, the model is still applicable outside that range, at least within the range that we stressed it. Therefore, again, no impact on the TSPA results, or the model itself.

Another issue associated with--this is a process--many of these are process issues, and we'll come to the conclusion here in just a little bit about these. But in NRC's review of the model document and in the technical report, they identified areas where they needed additional information. Not surprisingly, you don't put every piece of information in a technical document. So, they wanted additional pieces of information.

We provided additional pieces of information informally, what we thought was addressing the question at hand. Unfortunately, what was informally transmitted wasn't exactly what was in the document. But, again, it was a chemistry issue, and the chemistry that was provided to them represented slightly different conditions from the chemistry that was used in the actual model itself.

So, it was an issue of informally transmitting them some tables that the tables were not checked or reviewed prior to sending in this particular case, and so we tightened up the process of sending informal information to NRC on these quick turnaround responses that they had in their review.

Another one is potentially more important, and that is the fact that when you run the model in its expected case, you run the piece of software, the user of the piece of software is given a number of error messages, essentially warning messages, warning you are running outside of the bound, warning, you may not have convergence, numerical convergence, as you run this model.

So, the issue was, well, did we evaluate these warning messages? Did we evaluate the potential significance of these warning messages, and how did we know that sufficient numerical convergence, numerical accuracy was being attained from the runs?

And, in fact, these warning messages were evaluated. They only print out when we run certain cases, not for the full probabilistic cases. But for those cases, the numerical results were evaluated, the inputs and outputs, mass in and mass out across different boundaries was compared, was determined to be sufficiently accurate. In other words, we tightened the time constraints, the time step constraints, and the spatial constraints, such that they were sufficiently accurate. And, again, no impact on the TSPA-SR results.

We since have run this one with even tighter constraints, and gotten exactly the same results. So, again, no impact.

Finally, there was a typo, a minus 3, it should have been a minus 4, in the TSPA-SR report. That was a typo. It didn't impact the input, didn't impact the results, didn't impact any of the curves. But it was a typo in the actual document itself. And, again, in your backup you have the point by point.

But we were concerned when we got this, first the phone call on May 4th, and subsequently the letter that followed up detailing it in words, we were sufficiently concerned that we initiated a number of actions. Remember, this is in the time frame when the preliminary site suitability evaluation report was in draft form. It was in the time frame when the SSPA, the supplemental science and performance analysis report, was being prepared. We were in the middle of preparing that particular document.

So, we wanted to initiate a number of activities on those products, and on the TSPA-SR itself, because these ten indications are just that, they're indications of something that's probably a little more deeply seated issue that we and the Department felt needed to be addressed quickly.

So, the first thing we did logically was look at those ten things, and we did those immediately and determined, as I said, that we had no impact. In fact, we already had a phone call with them after May 4th about our assessment of those impacts of those ten things.

We then did two things. We conducted an internal assessment to the TSPA-SR itself, that is, internal to the TSPA, looking at were there any other known discrepancies or known issues following the release of the document. Both of these documents were checked, reviewed and approved in December of last year, and everybody, even if you approved the document, you may have identified, self-identified issues, you know, you might have made a typo that wasn't caught in any of the check review process. So, we wanted to make an internal assessment of those particular documents, and the model document and the technical report.

We did do that. There were several tens, it was probably on the order of 30-something, other small discrepancies or inconsistencies in the model file, all of which were determined to be insignificant, and all of which were, in fact, had already been corrected or modified in the SSPA model.

The second thing we did was use an external review. We used people not directly involved with the production of that product, and we conducted what we call a vertical review, essentially going from the TSPA-SR report, into the TSPA-SR model document, and then into the supporting AMRs, and how did that information flow and were there any inconsistencies between how information was documented and the AMR, using the TSPA, et cetera.

We've completed this review and I'll go into its results here in the next couple slides.

And, finally, in order to develop a path forward and understand what really was causing it, a formal root cause evaluation was conducted. And I'll go into that result here in a second.

The next slide, the review that was completed identified a number of other issues, incomplete referencing, a statement was made without a reference to support the statement; editorial issues identified, some typos, some style issues associated with that; some discrepancy, how something was phrased in one document and how something was phrased in the TSPA document were just ever so slightly different, unclear, of course, which way is the right way, but they were slightly different.

There's issues of transparency, statements made without the bases, or at least in the reviewer's mind, bases adequate in the TSPA document itself. You had to go into the supporting document or other documents or quiz people for that additional description. Some conceptual model issues were raised in the review. Some small discrepancies in the model document itself, and then some traceability of software identifiers and data identifiers within the document.

All of these were looked at by the TSPA team, and they were binned in terms of level of potential significance. None of them were found to be significant. None of them changed the results at all.

In six cases, we, in order to provide additional objecting evidence that it had no impact, we actually reran the model with the change. In all six of those cases, there was zero impact on dose. In other words, you couldn't even see the needle move on dose, and that's looking at the whole time sequence of dose, whether it's the early time, the time when packages are failing and drip shields are failing, or if you look at the peak dose, so, looking at all those time sequences.

In one case, there was a description of how water seeps through the drip shield. When the drip shields are failing, is that area dependent or length dependent? It was implemented as--I don't want to get them backwards--but it was implemented one way, and we said it was implemented another way. So, that had a small impact, where small means less than a factor of 2 on those doses when the drip shields are failing. So, the time period between 40 and 100,000 years when the drip shields are failing, there was a small impact on dose, not an impact prior to that time, and not an impact after that time. But during that time, there was an impact.

So, this review is completed. The documentation of all of the objective evidence for these individual cases is being reviewed. It's been prepared, but it's being reviewed right now. It should be completed, I think we've said, by the middle of next month, end of this month, middle of these month.

The root cause that was conducted on the TSPA identified four primary root causes. There were some generic causes also for the errors getting through the system. Remember, this is a checked, reviewed and approved document.

Two of them--well, the first three are very interrelated. They deal with the sequencing of events and the sequencing of the documentation and the sequencing of the check and review process in the production cycle of the document itself, that being that the model document and technical report were being done more or less in parallel, and the checking and review process, although should have been somewhat more sequential, was planned to be more sequential, ended up being also somewhat in parallel, and using the same resources.

So, these things have to do with project management, better use of an integrated schedule, and with the check and review process itself.

And, finally, another root cause was the issue management system. When you identify an issue and identify an error, how do you track and resolve that in the quality assurance program that we have? So, there's a process issue here and a procedure change resulting from that.

I'm going to address these recommendations from the root cause when I get to a concluding slide. I'm going to bring all three of these quality issues together.

Skipping onto the model validation corrective action. Model validation and the issue of model validation has been an issue in front of this Board before. I know we've had some lively discussions of it. Back East, I can remember one very lively one. And it is a QA requirement for model validation, and appropriate documentation of model validation in the analyses and model reports that have models in them.

In various reviews, internal reviews, quality assurance reviews and self-assessment reviews, it was determined that the actual documentation of model validation in the AMRs appeared not to be sufficient, in accordance with the existing procedural requirements for the documentation and model validation, model validation being confidence building in that model.

So, first, we had some deficiencies, and then they elevated themselves to a corrective action requirement. And various actions have been taken. One is to revise the QA procedure. That's still in progress. This was also a topic, I should point out, in the August TSPA Key Technical Issue technical exchange within NRC. There were some suggestions that NRC raised in that, and we're looking at incorporating some of those as appropriate into the final procedural requirements. That's in review as we speak right now.

In parallel to that, there's development of a guidelines manual associated with best practices for implementing model validation to assist the analysis model report authors.

And then we conducted a survey that I'm going to go into here in a second, and then we also did a formal root cause evaluation on this QA concern.

This model validation review used, again, 30 independent off-project personnel, a number of them from National Labs, they were from other Bechtel sites, but they were totally independent of the work produced.

After some initial briefings, they were reviewing all 125 AMRs used in support of TSPA-SR Rev. 0. You generally think of the number 122. There were three additional ones that were in process throughout. So the total was 125. Of these, there are 128 models. So, some models--some AMRs don't have models, some AMRs have several models in them.

And then they binned them. Those that met the procedural requirements, those that did not meet the procedural requirements in that product itself, but there was enough other pieces of information in other products, related products, where it did meet it, to demonstrate adequate confidence, and then a Bin 3 where in the assessment of the reviewers, additional work was required to fully validate the models to add sufficient confidence for a licensing kind of setting.

Many of these Bin 3s, and we're still working on the documentation of this, many of these Bin 3s are correlated to KTI agreement items. So, in our agreement items, we identified the need for more work to provide additional confidence in the licensing setting, and many of these Bin 3s fall into that category.

Impact assessments for all of the Bin 3s, all 34 of these--well, it was actually 32 unique and two that are duplicates--have been completed. There has been no impact on the TSPA-SR results, the TSPA-SR conclusions, but the documentation of those impacted assessments is still in review and expected to be complete by the middle of next month.

The root cause evaluation for this one identified a lot of issues, which you can read here, the expectations for model validation, how do you--okay, how do you flow that down. The roles and responsibility, the training, the interpretation of the procedure being somewhat ad hoc, a number of process and project management related issues that have to be addressed. And, again, we're going to come to the recommendations here in a second.

And software, let me speed up here a little bit, there's a number of issues associated with that corrective action. The procedure is being revised. There was a stand-down initiated as soon as that corrective action was in place on any continued use or development of unqualified software on the project. And there is unqualified software, software that hasn't gone through all the steps to be fully qualified in accordance with NQA-1 requirements. And then the formal root cause on that, as well.

The root causes here are very similar to the root causes for model validation, the roles and responsibilities, communication, and training. So, three of them are overlapping.

All of these root causes from the TSPA root cause, from the two root causes, plus other pieces of information, including various reviews we've done of in process work, and quality issues that have been associated with that, various self-assessments that have been performed where the Department and BSC staff and personnel review in process work, and the self-assessments of that to identify procedural issues.

Lessons learned from previous corrective actions that may not have been fully implemented. There is a QA management assessment completed each year, provided to Lake. I think Lake gets that in the next week or so, and the impacts of that review will also be considered in our path forward.

But the objective in this path forward is to provide, you know, a joint comprehensive plan to address all of the above.

The plan, if you want more details of the plan, I think Nancy can address that in the question and answer period. This plan is being modelled after proven performance and proven plans that have been applied at various NRC, quote unquote, watch list plans. So, we're bringing in a lot of Bechtel experience and experience from elsewhere that have been working on various power plant issues around the nation.

The plan will include a lot of metrics to evaluate how effective is the implementation of that plan. When the plan is actually implemented, how do you know month by month how you're doing on the implementation, kind of prevent recurrence. The idea is to develop this plan in the next few months, and during that development, have interim status reviews with NRC staff on the development of the plan to correct these quality assurance issues.

So, in summary, these implementation concerns of the QA program, whether it be procedure or process steps, or the actual QA requirements document itself, have been identified by both ourselves and NRC, both software, models and technical document in the case of the TSPA-SR.

In all cases, they've had no impact on the TSPA-SR itself in terms of the results that are presented, the curves that are presented, if you will, and the conclusions that are derived from those analyses.

It does, however, indicate the need for significant process improvements. To get to this nuclear culture or licensing kind of culture, clearly, a number of improvements are required on implementation, project management, project control, and how we implement our baseline schedule. And as I say, there's a transition plan being developed to address all those concerns, and we expect to complete that by December.

That's the finish of my prepared remarks. The logical question is, well, how do you know it's still okay?

How do you know, you know, that document you put on the street last December and is now used in PSSE that had your signature buried on it, or it's not buried, it's actually on the front page, how do you know it's still okay? You know, because I can sleep at night, just like everybody else does. And there's a lot of reasons. One is this isn't the first one we've done of these.

As was pointed out earlier, we've been doing them since '91. There have been successive improvements as we've gone along. The models have changed as we've gone along. The processes have improved and the control of the processes has improved as we've gone along. And the information that comes in from site programs and testing programs, et cetera, has changed and improved as we've gone along.

But the process has been more or less the same. The process of doing the TSPA, of creating a TSPA, of testing the TSPA, of implementing the TSPA has been fairly constant.

And probably more important than that is the people. We've talked about kind of process and programs and project management, but, you know, the most important "P" is probably the people. And the people who have been doing it, whether you're the lab tester at Argonne, or the UZ tester at Berkeley, or the performance assessment person at Sandia, those people have been pretty constant, and those people have been doing high quality work and testing their work and evaluating their work as they have been going along. Sometimes not all those tests are documented. You know, the production of a TSPA is a fairly complicated process, as you probably can realize, and there are a number of tests that go on in the actual development of that TSPA.

And we test it against other people's TSPAs. We test it against EPRI's TSPAs. When NRC was publishing their results, we tested it against their results. We understand their results in the context of our models. Can we explain our result in the context of their models? So, we flip it around both ways. And the answer always was yes, not that we don't have still conceptual understanding and conceptual issues and uncertainties still existing, but I believe there's enough testing of the model, enough testing of the inputs to that model to give very high confidence in the results of the model.

And I think the fact that even though we've identified discrepancies as we've gone in the last six months since that report was published, none of those have impacted the results.

So, with that, I'll stop and answer any questions.

KNOPMAN: Okay. Questions from the Board?

Paul Craig?

CRAIG: I suppose I'm feeling unduly grumpy today. But I just found this presentation that you gave so discouraging, and particularly discouraging because you were one of the people in the program who gives some of our finest stuff and we interact so well with you. John, give us Number 29 in the backup. It just illustrates, now, what have we come to when the Board's time is taken up on issues which are like this one?

Something is running amuck, and we've heard over the years about problems with QA, and it seems to me there's something similar going on. There's a preoccupation with minutia when there are conceptual issues of the deepest sort which are hanging out there, and it just seems to be sad that you in particular have to spend time making up viewgraphs like this. I don't know where the problem is, but it just seems indicative of a deep, deep problem with the Program. You shouldn't be doing this?

ANDREWS: Well, with all due respect, let me disagree. Somebody identifies an error. In this particular case, it's an error in this fifth significant figure. I understand. And there's actually an explanation why it's different in this fifth significant figure. It happens to be that at the fourth significant figure, some of the inputs were slightly different. But it's a discrepancy, and if you see something like this, how do you know as a reviewer, as a respected reviewer, how do you know there's not something else?

CRAIG: Bob, my point isn't that these things shouldn't be picked up and looked at. The problem is that this kind of trivial issue has been brought to the level of the Board.

ANDREWS: That's why I put it in the backup.

CRAIG: Yeah, except that because you had the same kind of thing in the front, I was just turning through the pages, and this is a particularly egregious one, but you had similar things about typos in the front of the whole thing. And, basically, every point that you brought up fell more or less into this category, perfectly understandable human errors, shouldn't come to the Board level. Something is wrong that causes this to happen.

ANDREWS: I agree, it shouldn't come to the Board.

CRAIG: Something is wrong that causes you to have to spend a significant amount of time worrying about this kind of thing.

ANDREWS: Well, I worry about errors and discrepancies. I think all of us do. And we worry that small errors may be big errors somewhere else, and how do we know until we do the kind of reviews and rechecks and retesting and reevaluation, which I think we prudently did. And I think that's appropriate on something of this nature to assure the quality of the product.

KNOPMAN: Priscilla?

NELSON: I'm not sure exactly what I'm going to ask. But let me see if I can work to one. Nelson, Board.

When I was looking at this title, I was trying to think about what kinds of questions, what I'd expect to hear. And I was struck by a couple of things. I think first of all, I wasn't really sure, the interaction with NRC on QA, is this--it seems to be predominantly procedural process oriented in terms of do we have a regular process. Are we following it? That sense. Whereas, there's really also a question of correctness of the model, and verification of that correctness, and maybe even some question about is this the right conceptual model.

How is that handled from a quality standpoint, and is there a role of NRC in that sense of acceptance of quality?

ANDREWS: I mean, let me try one way of attacking it. That's a very good question.

The conceptual issues, the conceptual understanding that's used and embodied in the TSPA, and in fact the conceptualization of the TSPA, were reviewed not so much from a quality assurance process procedure point of view, but more from a technical defensibility and documentation of that technical defense, scientific technical defense point of view. And it was reviewed in the context generally of the AMRs and of the TSPA itself with NRC staff in those key technical issue meetings that were conducted over the last year plus time period.

NELSON: Just let me ask a question then. Many of the KTI agreements that come out are really focused towards acceptance of, not giving the stamp of approval, but more or less saying that acceptance that information will be there to allow some eventual determination that NRC has to make.

So, that doesn't really say anything about the models being correct or validated. It seems like it's more process driven instead of demonstrating the validation or accepting the validation.

ANDREWS: You could twist--the question as posed in those technical exchanges wasn't validation per se. But, clearly, the agreements that came out relate to additional confidence and documentation of that in models as you go forward. So, although not stated, they are very intimately related to the issue of confidence building in general and model validation requirements in particular.

NELSON: So, if NRC closes something, that generally means that model and everything in it, the model is acceptable?

ANDREWS: If it was actually closed, yeah.

NELSON: The model is acceptable to--and considered validated?

ANDREWS: Yes, because when they do the close, I mean, granted, they were going key technical issue by key technical issue, not model by model, but the conclusion would be if it's closed, not closed-pending, but closed, then the model is sufficiently valid for licensing purposes.

NELSON: Can you give me an example of some models that have not been accepted on that basis yet, or that are still going through a validation?

ANDREWS: Yes, I think the best examples are probably in the package area, passive film model was raised, localized corrosion model, given stability of passive film.

NELSON: Okay. So those are conceptual models. What about like software that's being used for the analysis, some of the off-the-shelf stuff, is there anything that's not validated?

ANDREWS: On the software side itself, I don't think there is any--and somebody can correct me if I'm wrong--I don't think there's any agreement items that the software per se, although it has to go through the full qualification and documentation of qualification, but I don't think there was a question on the technical adequacy of any of the software.

Rob, do you--

HOWARD: This is Rob Howard, BSC. I think we do have some agreement items to provide the NRC with the actual software that was used so that they can go do their own evaluation and use of the software to understand how we developed our numerical implementation. So, in a sense, I mean as far as confidence building, they get the software, they use it, they understand it. That helps they understand the underlying conceptual framework.

KNOPMAN: Jeff Wong? I'd just remind our--my fellow Board members we're a little bit behind. So just keep your questions to the point here, and we can get back to the schedule. Go ahead. It's never a problem with Jeff.

WONG: I have my own 19,000 rule imposed upon me.

But we've had a number of instances, or points brought up about risk communication, and I understand that this is a complex model, there's probably, I want to say elastic in the system. But this presentation about the various sort of editorial errors, in essence, you're saying to the public we put in the wrong data, we can run the model wrong and we'll still get the same answer. Maybe I'm looking at that too simplistically, but that might be a problem for you.

The second thing is about model validation, and that actually ties to the first point. In model validation, you can put in that data, you still get the same answer, but--and help me understand, because I looked at the viewgraphs from the Center from your August 6th through 10th meeting of the KTI exchange, and they actually give some fairly strong language that many of your models are not validated, or poorly validated. And that, to me, would indicate or would tell the public, and tells me because I wasn't there, that I shouldn't have good confidence, and actually, you know, there's greater uncertainty because there are a number of models which aren't validated. And you just talked about one of them was a corrosion model. Well, much of the performance of the system relies upon the engineered barriers, and so help me to understand that I should have confidence.

ANDREWS: Well, I think, you know, of those models that we in fact say the validation is insufficient for licensing purposes, of which that one falls into that category, it's probably true. You know, for licensing purposes, there are a number of other issues that need to be addressed, and development of--we have a conceptual model. We documented that conceptual model in the SSPA. We also documented in the SSPA a wider range of possible uncertainty associated with various aspects of overall system performance.

So, I think between the base knowledge and the AMRs that support the Rev. 0, and the SSPA, additional tests, I guess if you want to look at them that way, what if studies, what about this, what about that, that there is adequate basis right now. But for licensing purposes, which is the purpose of that KTI meeting, the purpose of all the KTI meetings really, and the purpose of our path forward here, is focused on licensing type documentation, not on--

WONG: But again, I mean, you're talking about in terms of license application, and I'm kind of thinking in terms of site recommendation. Are you going to again provide the decision maker with, well, we have 35 of our models which are declared by the NRC to not be adequately validated, is that piece of information going to be put forward?

ANDREWS: Yes, because this assessment of the model validation and the impact assessments that are associated with that will be done in the next, like I say, the next month, the middle of October. So, that will be provided to the decision makers, yes.

WONG: Thank you.

KNOPMAN: Don Runnells?

RUNNELLS: Runnells, Board.

I just want to agree with my friend and colleague, Paul Craig, that he's unusually grumpy this morning. And I want to disagree with you, Bob, that this should not be brought to the level of the Board.

You in the past--the Project in the past, I think, have recognized criticism from the Board that everything you present to us is such a high level, that the mechanisms and the details are hidden, and we complain about how much work it is for our staff or for ourselves to dig out these details.

I'm pleased to see this sort of mind numbing presentation of details to show what you have to go through to answer some of the criticisms that you receive. I would not want to see this level of detail at every presentation by everyone at every meeting, but I think it's important for us to see it now. And the word now means that when I read the NRC letter with its critique of the model, I said to myself good Lord, look at all of the things that are wrong. And am I going to have to dig through this and verify this for myself, or ask one of the staff members to do it? That's almost overwhelming.

Instead, what has happened is you've presented to us the detailed response to what on the face of it from the NRC was a very I'll use the word devastating letter, and as it turns out okay, fine, the issues were not very important. But I did not know that before your presentation.

So, again, I don't want to see this constantly at every meeting, but I'm very, very pleased that you responded in this way to that NRC critique, and I think it's important.

So, I wanted to agree with Paul, and disagree with you.

KNOPMAN: Thanks. Just as a point of clarification, I think when the Board, when we ask for a discussion or some presentation on this topic, we were looking for a higher level discussion about impacts of some of these QA issues on model uncertainty, and other--and TSPA uncertainty in general.

So, you know, what we got, according to your title, is a status report on QA issues as opposed to maybe the kind of analysis we were looking at for impacts on model uncertainty. So, it's still at least something that we will want to follow up with you on.

Anyhow, thank you very much, Bob.

Our next speaker is Jeff Williams, who will be talking about repository plans. Jeff is the Director of the Systems Engineering and International Division of the Yucca Mountain Program--actually, I'm sorry--with OCRWM. He's been with the Federal Government for 21 years, and with the Department of Energy, Office of Civilian Radioactive Waste Management for 16 years.

WILLIAMS: Thank you. I think this presentation is a little bit different than most of the other ones you've heard the rest of this meeting, and I hope this is something that should be brought to the level of the Board.

Rick Craun thought that it was important that you receive it. The title may not be exactly description, repository development plans. Really, what I'm going to talk about would be primarily the cost work that we've done, the total system life cycle cost report that Russ referred to the first day, the fee adequacy report, and then some other cost work that we've done recently on flexible operating modes. And then lastly, I want to get to the modular approach that

Lake referred to also on the first day.

The total system life cycle cost is basically, this is a report that we do periodically for several different reasons. It provides input with respect to, you know, the cost of the program. It provides input into our fee adequacy report. Basically, what we base this on is we look at all the waste for the whole system, which in the last TSLCC, was 97,000 tons, which consisted of 83,000 tons of commercial waste and 13,600 tons of defense waste. This is calculated based on 104 reactors operating through their lifetimes.

From a cost standpoint, it's important to realize that we need to allocate cost to either the civilian share or the defense share. Right now, it's about three-quarters goes to the civilian share, which is paid for by the nuclear utilities, and about a quarter that's paid for out of the General Appropriations Fund. Again, this report was published May 4, 2001 with the S&ER Report. It's consistent with the project description document Rev. 2.

What goes in the TSLCC is the costs for doing the repository. It also includes costs for doing the waste acceptance, storage and transportation part of the program. Right now, we don't have a storage part of the program, however, in the past, we have spent money on doing designs for storage concepts. But it's not part of the program right now. It also includes costs for Nevada Transportation, and then other associated costs, which include NRC, the Nuclear Waste Technical Review Board, the Office of the Negotiator in the past, and several other costs. One important cost that's not considered in here are utility costs.

Okay, in the May 4th report, we had a Section 8 which talked qualitatively about potential costs associated with lower temperature operating modes. It didn't give any specific costs at all, but it provided some qualitative costs, and I'm going to talk a little bit more about that.

This right here is just what the costs are based on. It's what we call the reference system design characteristics. I think you're probably all familiar with this.

I thought I'd sneak in another slide that I just used earlier this week with the National Academy of Sciences. It includes some other more important things other than just the design characteristics. That's not the only thing that drives cost. It's also how much waste we receive, it's the operational costs, how long it takes to receive it. So, these are all the things that go into basically the repository reference design.

This is what was published in the May report. Basically, we total to a $57 billion program, which starts from 1982 through the closure of the repository. The way this is costed out is that some important dates are you begin acceptance of waste in 2010. You emplace waste in the repository to 2042. You monitor the repository until 2010. And then it goes through a closure period to 2119. And all these costs are included in detailed spread sheets, and obviously there's probably some uncertainty associated with this cost, as well as some of the other things you've been talking about in this meeting.

We have historical costs in here. The historical costs, in this report, we inflate them to $2,000 so that we can compare different scenarios from one year to the next, and the historical costs in this program have been about $48 billion.

This slide just shows the cash flow, and there's two things I wanted to--or a couple things I wanted to point out, is we break it down into the licensing phase, which comes from now until about 2005, then the construction phase where we have this peak of costs. Then we have the emplacement phase, which goes to 2042, followed by the monitoring phase, which goes all the way until 2010, approximately. There's some ramp-up costs as you go to what we call the closure phase.

And what's important to point out here is these peaks. One is this peak to build the repository, and the other is this peak in the end to close the repository. One of the major cost drivers in here is this is when the

Titanium drip shields get emplaced into the repository, and this cost flow is important for determining whether the revenue that we receive is adequate.

KNOPMAN: Jeff, I hate to do this to you. We are pretty far behind in our schedule.

WILLIAMS: Okay.

KNOPMAN: I wondering, do you think in like 15 minutes, you could probably get through most of the points?

WILLIAMS: Sure. Okay, yes.

All right, why don't we just quickly skip through this, go to the next one. This is the fee adequacy determination. Go to the next one. Basically, what this shows is our reference case, and various economic assumptions, and how much margin we have based on our income and our costs for various economic assumptions. What this shows is that everything, if economic assumptions fell into this range, we would be fee inadequate if they fell into this range--I mean, we would be fee adequate. If we fell into this range, we would not be adequate.

Historical economic assumption shows that we're right here. This basically margin is about $10 billion at the end of emplacement.

Why don't I--okay, the next slide, basically, the important part of this is the fee adequacy report shows that there was a positive balance at the end of emplacement in

2042.

The next part of this is basically this is work in progress. We don't have any report out for this, but this is costs for looking at different operating modes. What we had in the 2001 report was the cost for the reference system, which was the EBA-2 modified that I think you're familiar with, hot around the drifts, below boiling between the drifts. That's what was costed out in that $57 billion TSLCC.

The flexible operating modes, as I said, this is work in progress. We don't have detailed costs. They're parametrics. We don't have detailed designs. And some of the things that we cost out really aren't designs, but it's a parametric study just to look at variations from--and there is high uncertainty in some of these costs.

These are the things that are buried in this study, basically waste package spacing, and this was talked about in the 2001 report. Basically, as you space it out, you have to build more drifts, and there's a cost associated with that at about $60 million a kilometer. Ventilation, as you ventilate, if it's forced ventilation, it costs more than natural ventilation. There's a cost associated with that. Aging of spent fuel prior to emplacement, if you build storage above ground, commercial utilities are spending about $100,000 a year. It's possible that we could do it for less than that. There's also increased handling associated with storage above ground.

Drift spacing causes the repository to increase size. Waste package sizes, if you build one that's half the size as a big one, you don't get it for quite half the price. Basically, all of these things lead to higher costs.

All right, why don't I go to the next slide. Okay, this one I put up here basically follows these two here, and it's just to show you the seven different scenarios and how we varied the seven scenarios, and this is sort of a graphical representation of what's in the charts there. You can study these a little bit later, but you can see how basically the reference design, here's the key parameters, and the things that we varied. Waste package spacing was varied in this scenario, this one, this one, this one, and that actually should also be yellow. That's a mistake in the graph.

Surface aging, two scenarios had surface aging. Emplacement period, these two had a longer emplacement period. Waste package sizes, one scenario had small waste package sizes. And then we also varied the drift spacing, as well as the amount of time for ventilation. And that's, as I said, you can study that in detail, and these two slides, you can skip to the next one.

And then this one is really the results. Okay?

And basically, again, this is work in progress. We don't have a report out on this yet. We did some work. I think I provided a draft report to the Board from a February study that had several scenarios. However, most of those scenarios were just below boiling, and not 85 degree scenarios like this. So, that draft report we did in February, we never finalized it because we've now moved to the 85 degree cases.

But, basically, what we reported in here is undiscounted costs, as well as looking at various discounting factors. And what is shows is for the various different scenarios, how the cost changed from the referenced design. And then what this column shows is how--basically undiscounted costs through 2119, which is the same year as the reference design, the reference system closes.

And then costs that go beyond that in an undiscounted fashion, beyond 2119, and then we've discounted these additional costs at various discount rates to show how that affects it.

SAGÜÉS: Which one is the coolest of those designs?

WILLIAMS: Which one is the coolest? They're all 85 degrees.

SAGÜÉS: Oh, okay.

WILLIAMS: All 85 degree waste package cases. There's just different ways to do them. And if you go back and you compare this in detail, I know we don't have a lot of time to do this, but if you compare that to the previous chart, you can figure out what drives these, you know, whether it's surface storage, whether it's underground, additional drifting, whether it's ventilation, whether it's forced or whether it's natural, and again, once we do--if we were to do specific designs on these things, we may have some cost adders, we may have some cost downers. So, this is just sort of a rough order of magnitude of the factors that are involved. So, don't take them to the bank.

Okay, the next one we can just skip through. Okay, I think I've already said all this. Basically, the things that cause the addition in costs, and then some discussion about net present value. The net present value actually brings the cost down. And then a fee adequacy, this is very preliminary. Basically, our real preliminary analysis on the fee adequacy of doing these scenarios, it really needs to be looked at in some more detail, using--I didn't get a chance to explain the difference in our economic assumptions. But using the economic assumptions that were projected in the year 2000, every one of those scenarios ended up to be fee adequate at the end of emplacement.

Using the historical economic assumptions, there were some that were borderline. However, we believe that, you know, were that the case, it's something you could probably manage.

The fee adequacy is sensitive to economic assumptions, especially to interest rates, that's the biggest one, inflation, possible future settlements, Lake referred to this the very first day about the lawsuits, and so forth. In our fee adequacy determination, we have--there's been one settlement with one of the utilities, and we have that factored into the income stream. Basically, our income stream is reduced to reflect that settlement.

Now, there's many outstanding lawsuits that are under litigation right now, and we don't know how those will turn out. So, we haven't included those.

Okay, the last thing I really wanted to get to quickly, again, was some other additional work. We started off on modular construction and design. Now, Lake also mentioned this the first day, and talked about our NAS staging study. Well, we started this back in 1998. The real reason we started it was really to address ways to reduce the peak construction cost. That's what I showed you in our cash flow analysis, how the peak cost during the construction phase were real high. And so we started looking at how can we piece this out to potentially reduce those should we not get the funding to build the facility as planned. So, anyway, we've looked at various ways to change the system around.

So, we looked at the underground. Right now, the reference design, you build the whole perimeter drift. First, you build all the ventilation shafts. And so we started looking at ways of maybe we can piecemeal it out to reduce those costs. The surface facilities, the current plans are to build a large facility. Maybe we could piecemeal that out.

Nevada transportation mode, the current plans are to build a railroad. Maybe we could have heavy haul, we could start with legal weight truck. We could also adjust the receipt rate, storage and emplacement rates. And if we separate the receipt rates from the emplacement rates, we start to get into ways to develop cooler repositories.

Now, we've put two of these studies on the web. You can go look at them. Anyway, the May modular study considered two basic approaches, one modifying the waste handling building, and then modifying the subsurface construction.

Various design operations were investigated, including constrained funding. What we're talking about there is if we don't receive, like I said before, the amount of funding that we would need to build the facility as planned.

Early receipt. Maybe there's ways if we build it in little teeny pieces, we can bring our receipts up, or maybe at least enhance the schedule, or enhance the confidence in meeting the schedule that we have planned.

And then also we started, as we moved further into these studies, we started looking at lower temperature designs.

Why don't I just put up this one slide that isn't in your chart. Again, this is one I used earlier this week. This is just to show you, and I can get you copies of this, just to show you what this represents over here is our acceptance rate as planned, 300, 600, 900 tons per year, 1200, 2000, 3000. And then what this shows is how much waste we would emplace in this scenario. This is scenario 58. We've actually looked at 63 different cases.

Again, the primary thing was to look at funding. This line here represents our funding level for the reference case. This is the total system life cycle cost, and this line here shows the funding of this case. So, what we're trying to do is to bring down these peak construction costs. Everything in red is the difference, it highlights the differences in this case from the reference case, and everything on the right shows you the difference in cost and also different items. For example, this one has 40,000 tons of surface storage, because we're receiving more waste than we're emplacing.

I'm just about done here. When do you want me to be done? Right now?

KNOPMAN: Key conclusions?

WILLIAMS: Basically, the modular design and implementation approach will address key programmatic and technical uncertainties by providing a significant reduction in peak costs to build and construct the repository. It will enhance flexibility for blending and thermal management. It will enhance flexibility for accommodating various thermal strategies, warm versus cold. It will accommodate different fuel utility selections. We don't really have control exactly how we bring the fuel into the system, so this modular approach with surface storage or different things like this could help us to accommodate things that they send in. Also, different fuel characteristics, high burnup, high enrichment, things like that.

Schedule opportunities. If we build it in little pieces, we may be able to get started quicker. We may be able to look at early performance this way. We could build a little up front piece, possibly underground, and look at that for a while. That's some of the scenarios we looked at. And significantly reducing sensitivity to other program uncertainties.

Okay, so, basically I think what I've told you about is that we've done the TSLCC in the fall of 2000. We showed that the fee was adequate. We've done some additional studies of cooler designs. Basically, what we found out from there, there's some additional costs associated with the different ways that we've looked at it. We haven't done the detailed work on that. The fee probably would be adequate.

And then the modular study that we've done shows that there could be significant benefits from developing the repository in a modular fashion.

Okay.

KNOPMAN: Thank you, Jeff. And I'm sorry we had to speed you up there. Let the record show Jeff compressed a 30 minute talk into 20 minutes.

Let's take a few questions now. We do have some other things, a few other small matters to clear up, and then we need to move into a public comment period.

Dan Bullen?

BULLEN: Bullen, Board.

Just a quick question since you did deal with uncertainties. How certain are your numbers with respect to total system life cycle cost at $57 billion? Is it plus or minus a couple hundred million, or plus or minus a billion?

WILLIAMS: No, no. Well, you know, at this stage in design, while we're in pre-conceptual design, I think in accordance with the DOE guidelines and principles, it's about 30 per cent, or so. However, we also have contingency added in there, and we have contingency added in in different ways.

In other words, some things that we have a more detailed design on, we may have a smaller contingency. Things that we have, you know, less of an idea of, we have a bigger contingency on. And I don't have all the details, but we do have lots of contingency built in there, and if we don't build it exactly the way it's planned, then there's a whole other thing comes into play.

BULLEN: Bullen, Board.

Because if you look at the backup slides, and I always take a look at the backup slides, you're basically looking at about plus or minus 20 per cent for all the scenario analysis. And, so, if you build in 30 per cent contingency, the cost could essentially be the same?

WILLIAMS: Well, I think what this study shows is the relative difference. Okay? That's the way I would look at it.

BULLEN: Okay. Fine, thank you.

KNOPMAN: Carl DiBella?

DIBELLA: Thank you very much.

Jeff, I think your comparison of the high and low temperature operating costs is really unfair and misleading. And let me explain why I feel the way I do, and see if you can tell me why I'm wrong. So, my question is why am I wrong?

What you've done is show seven different scenarios for developing a low temperature design. And people are going to come away from that saying, gee, they ran the entire universe, seven scenarios of low temperature designs, and every single one of those was more expensive than the base case design.

There's certainly one scenario that you're not running that's an obvious one, which is to take the existing design, the waste package spacing, the drift spacing, and just ventilate that until it gets to the point where you don't need any more ventilation, and then put in your--you don't have that case.

WILLIAMS: Well, I think we have one very similar to it, I believe. Number 7.

DIBELLA: You have one that has everything the same except for wider drift spacing, I think.

WILLIAMS: Okay, this one has the same waste package spacing, the same emplacement time, and it has a bigger drift to drift, and it has 300 years of forced ventilation. Okay?

DIBELLA: Right, and it somehow uses more drift space of the characterized area; right?

WILLIAMS: Yes, it does. It does.

DIBELLA: Okay. So, you don't have the case that I mentioned, and I think it's an obvious case that you need to add to it. And I suspect if you do, you will find that there is very little difference between that case and the base case.

WILLIAMS: Yeah. I don't know if Jim Blink is still here, but one of our assumptions going in was that we weren't going to go over 300 years. And I don't know whether you could get actually to the 85 degree temperature using your scenario without going beyond 300 years.

BARRETT: Lake Barrett, DOE. We did not do a study in definite ventilation. If you ventilate it forever, or for post-300 years, you will keep the temperatures down, and it could be cheaper. I believe under the existing statutes and the licensing regime, that could not be licensed under what we presently have. I don't know how we'd ever demonstrate certainty if the ventilation paths would stay open for those periods of time. So, we did not spend time on that. We know we could do it, but we did not. That was not what we would consider a credible scenario for the study. We did ventilate for 300 years, though, to try to capture that aspect, which is beyond what is normally ever done in a licensing case, which is close to 100 years.

KNOPMAN: Okay, thank you. I think there will be some followup from the Board to get a little bit more information on some of these scenarios.

At this time, I understand there is someone from the Nuclear Regulatory Commission who would like to make an announcement on the NRC/DOE technical exchange meeting that was to occur this Thursday and Friday. If you'll just identify yourself?

LESLIE: Brett Leslie from the U.S. Nuclear Regulatory Commission.

As April Gil indicated earlier in this meeting that there was some uncertainty on whether the technical exchange between the DOE and NRC would proceed on Thursday and Friday. Due to some logistical things, we are going to hold that meeting on Tuesday and Wednesday, the 18th and 19th, next week. We are setting it up so that it would be a video con., given the uncertainties associated with travel, et cetera.

It is our intention to try to have people here as well to--from the NRC for that meeting. The meeting would be held currently in Las Vegas in Building 9 in the BSC complex in Room 916. We will be updating our website, which gives the schedule of interactions with additional details as soon as we can.

KNOPMAN: Thank you, Brett. Brett, just go back, if you could let everyone know where the meeting will be held so members of the public--

LESLIE: The meeting would be held up in Summerlin in Building Number 9, and you're going to need to get someone from DOE to give the exact directions, et cetera.

Rob, could you handle that for me?

KNOPMAN: If anyone in the audience has a question about directions or access, please see Rob Howard immediately after the meeting.

One point of clarification that I'm told will take a minute, I'd like to bring Jim Blink just to come up. There was a question yesterday in our discussion about uncertainties of the high temperature and low temperature repositories and repository designs, and there was a paper written by some National Lab scientist that suggested there were higher uncertainties with a low temperature design, and I'd like to get a response on that.

BLINK: Jim Blink from Livermore.

The paper was Wilder, et al., published in this year's International High-Level Radioactive Waste Management Meetings Proceedings. It compared the uncertainties in the THC and TH regimes for a hot and cool design. But the hot design that was the basis of the paper was a design that was similar to the VA design that had total boiling and dryout of a very large volume of rock. And most of the uncertainty reduction advantages of the hot design were predicated on a very large dryout region that persisted beyond the period of cooling.

The SR design is not such a design, and most of the advantages that were in that paper would not pertain. The one that does still pertain is the higher temperature might lead you to be able to apply models that didn't have so much kinetics in them, because equilibrium was established faster.

But in the THC region, the results indicate that the impact on performance is quite low, so that the uncertainty bar around that level probably doesn't matter whether it's a little larger or smaller.

KNOPMAN: Thank you, Jim.

COHON: Thank you, Debra, for your efforts as Chair of this morning's session.

We turn now to the public comment period. Four people have signed up. I would ask you to each aim for five minutes as your time limit. I'll call you in the order in which you signed up. Dr. Paz?

PAZ: I'd like to cite a very recent EPA publication, which was in July, and anyone who wants it can have it downloaded. Particularly I'm concerned about the lower temperatures. If you cited the EPA, you're going to see a wide variety of micro-organisms can reduce and oxidize and increase in corrosion. Specifically, it can be two mechanisms. One is a physical, which breaks down the passive film, and then it will follow by growths of micro-organisms, which are prevalent in Yucca Mountain. You have the substrate of sulfate. You have nitrate, and the presence of iron, manganese. And particularly I'm concerned is the oxidation, the reduction which can occur by sulfur material from sulfate to sulfide, and this is a major corrosion.

The material, just as I mentioned, will not be growing at high temperatures. It will be killed. But you have the chance of the--later on to interact.

Also, I'd like to interject two corrections. The last time on Monday when I talked, I spoke about hermosis. After the Committee and other papers which questioned whether at low level, there can't be hermosis, it can be synergism.

And as a scientist, I don't take any position favoring any other points. If you want to be guided by ethics, such as evaluation control, and try to find the solution, and specifically, I went out yesterday to the University of California at Irvine to collaborate on the potential research on the complex nature, which is a major issue. I again called for Yucca Mountain to be more open, not to be so much resistance, and we have to be guided by scientific principles, and I got a little bit more encouraging news from Dr. Abe Van Luik who said I will take it under advisement.

On the other hand, I was surprised that the--interested in my paper. I'm not a lawyer, I'm just interpreting what is the scientific implication, and just showing what is the uncertainty, because uncertainties have a major impact on the suitability and study of Yucca Mountain.

Thank you.

COHON: Thank you, Dr. Paz. Russ Dyer has an announcement he would like to make before we turn to the next comment.

DYER: In the light of the tragedy yesterday, I announced that we were postponing the public meetings for this week, and we're still working on the schedule for that. We're trying to, in line with the President's desire that the government go on as near as normal as possible, we're trying to get them rescheduled as soon as we can.

What we're tentatively looking at is the same locations next week. The logistics are still being worked out. We will, whenever we get confirmation about the availability of the locations and times, we will announce that in as comprehensive a media coverage as we can here.

In the meantime, I know that perhaps the word didn't get out to everybody, so we will have a hearing officer and also a court reporter in Amargosa Valley and Pahrump this week, although the real meetings we think will probably be next week.

Please bear with us. These are uncertain times.

COHON: Thank you, Russ. Sally Devlin?

DEVLIN: Sally Devlin, the public, from Pahrump, Nye County, Nevada. And of course I have to say thank you all for staying. I hope some of you eventually get out of town. I really do mean it. I do want to announce that we have a lovely new art museum, library that's magnificent, and it's at 9400 West Sahara, and after this meeting, I'm going to get my beauty. They have a Rodin sculpture exhibit there, and other things. So everybody is invited. It costs $3, and it is lovely.

So, we'll start again. Again, thank you to the Board, who I dearly love and are so communicative, and to all the things that have gone on at this, as usual, very informative meeting.

Now, I would be terribly remiss if I didn't tell Bob Andrews to go back and tell his boss, Mr. Ness, that I still want the 100 million for the cancer research. So, you may relay my message to him. I'm sorry he isn't here.

The other thing is that the biggest problem that I see, and of course I hope everybody will get the literature. I read literature by the pound. This is the one I told you about on the transportation series of the National Conference of State Legislatures. And you must see what the different charges are on transportation for these states.

The last speaker, Jeff, he talked about the money, and here it is. This tells you. These are two reports, and if you want to see the numbers, I'll be glad to show you. But when I came up with $58 billion, I wasn't too far off, and I always give an extra billion to my friends. I think that's only fair.

And, of course, the third book is the one that I showed you that tells how the Draft Environmental Impact

Statement for a Geologic Repository, Volume 2, and so on, that gives all of the details on the DOD stuff, as well as the Yucca Mountain stuff, and the deaths that they're going to cause of the workers, and what have you. Therefore, we need a cancer research center right below it.

But, mainly, I want the Board to enjoy the book I gave them on the 50 year history of the test site, and I don't give tests, I don't care, but I want you to read it because it will tell you the mess at the test site that is eventually going to get into Yucca Mountain. And I found it fascinating and very easy reading, lovely photographs, and so on. And I'm a member of the UPTA (phonetic) Group on occasion and we just were out at DOE. I took a tour of the test site. But prior to that, they gave us a learning lesson, which was very exciting, and we saw their computers, and everything on their computers is classified, but they did let us see how things work. And they actually have on these computers from the first day when it was Paramount, and of course I have several girl friends that worked for Paramount 50 years ago. So, I really got a lot of dirt, but it was beautifully documented, and amazing to me that some of those things were handwritten on some of the shots and things that happened, and what have you.

So, you read it, but mostly what you'll read about is the word that I love--stop talking, Lake, I'm talking to you--uncertainty, which you invented. And I love it because what you'll see in the book from the test site is not only the uncertainties, but wherever they wanted to dump anything, they put it in a crater and covered it up. And so there is so much undocumented, including portions of Yucca Mountain, which is on the Tonapah Test Range. And a few years ago when they went up and picked up the uranium bullets off of it and did something with them and put them in poly packs, and so on, and sent them to Utah, these were just dumped in, because in the early days in my youth, you wanted to have a tank trial, so you went out and you shot the uranium bullets in the middle of nowhere.

It is still the middle of nowhere, and as I said, 87 per cent of this state is federal. But, even so, you never know, they just found an airplane that was hot. They found--I mean, an airplane engine that was hot buried somewhere. So, this stuff comes out because I have many friends that work there, and this is the real uncertainty. What is at the test site, and what is going to go in?

And the problem I see, and of course I got into this nine years ago, was transportation. And Jeff very casually said transportation and storage in the buffer zone, and so on, and I gave to my friend from the EPA the insulting report that was done to the Peer Review, as well as to this body, and it was totally insulting because they used 1990 numbers for Amargosa. They called them for the third time in writing, strange people of strange habits. I've got a lot of friends over there, and they're certainly strange, but I don't ask their habits, and I really don't think it's DOE's thing.

But, without transportation, and the sort of laconic passing over the entire subject, which is the most important thing, there are no railroads here. The roads, as I have for nine years, are 9 and 7 hazards. There is only one intrastate highway. We have no emergency facilities except out of Fallon if there is any big booms or anything. We have nothing in the rurals where you're going to bring this stuff.

And, again, I'm going to really close, because I think we're all tired and I want to go see Rodin, but I want to welcome you to the meeting in Pahrump, and I promise I won't poison you with the cookies.

And the other thing is that until--if this suitability is accepted, it's total fraud on the public, because this site is not suitable, and you can't prove it to me for the licensing. And I will really fight this, because I am furious with the dirty politics that go on. It's not science, because the science is all uncertain. It's political science. And if this comes from the Executive, which we're very much afraid of, and bypasses the Congress, bypasses the Board and bypasses everybody, we the people are you know what.

So, in closing, I do want to say this now that Lake just said they're going to have ventilation for 300 years. Who's going to be there to ventilate? Where is your stewardship? I don't ever hear any of these terms, and they're big questions, and I want Jeff to know it, and I want Bob to know it, because we're talking, and since Abe and I have to be together for 200 to 225 years, and everybody this time realizes there are two repositories, that we want everybody to sign on the sign-in sheet with Linda, and we would really like scientists that have talents, like the physicians, and so on. So, if you have talent or you can dance or you can be an artist or a poet, that's who we want with us for the 200 years.

But I want you to take that back to the people, and I say it in the French sense, because we are the people, and we're the ones who are going to suffer. And until you show me something on transportation and something on the buffer zone, and more on the 7500 cows that are in Amargosa and that you've tested, and all this kind of stuff, you're really proving the uncertainty of this project.

So, that's all I can say. We're going to welcome you in January. I expect Lake and I expect Russ to keep me informed as to the progress in all this, because it's been an enjoyable journey scientifically. I think it's wonderful, and I hope the modeling, my favorite word, continues for at least another ten years, until transmutation or a pill the size of an aspirin tablet, or something like they bring the natural gas in from Australia and you condense it 14 million times, or whatever it is, and then it blows up again, I love these things and I hope that's what the science will come to, and not killing 43 states and all their people with the radiation.

So, that's it. Have a good time until we see you in January, but keep me informed on this, because we are very concerned in Pahrump.

Thank you.

COHON: Thank you. Tom McGowan?

(Mr. McGowan's complete written comments are attached hereto as an appendix.)

MCGOWAN: Tom McGowan, Las Vegas resident.

In conclusion--I--get a standing ovation at this point. Don't get up. And I--3 minutes and 48 seconds, or thereabouts.

In retrospect, the cost comparison presentation very ably conducted by a fellow by the name of Jeff somebody was interesting inasmuch as it made no reference whatever to the in situ generation of thermal energy induced power adequate to offset in substantial segment or entirety the cost of operation at a net profit in terms of both tangible and intangible--benefits over an enduring term. --have deep pockets. --making money? You earn it. Let's assume somehow we'll have the capability to figure out why not, because you didn't have to, that's why.

There is one bit of additional information you may find variably rewarding or alternately disconcerting. The whisper has it that Dr. Lake Barrett may be verging on voluntary retirement. I said voluntary. Certainly those shoes and that fast pathway will be difficult to fill, consistent with the caveat good help is hard to find, you get what you pay for, and if you want it done right, you almost have to do it yourself, don't you. Which is why in an unguarded moment, I jumped into the breach and submitted my unsolicited application to succeed Dr. Barrett as the next Acting Director of DOE/OCRWM/YMPO, which if and when accepted and approved will cause great consternation and anguished gnashing of teeth far and wide, offset by a spontaneous victory celebration involving ecstatic Native Americans and other interested members of the public. But fear not, I do have other pressing priorities, and someone, conceivably far less worthy than me, undoubtedly will rush to fill that gaping void.

But, seriously, Ladies and Gentlemen, I would be remiss were I not to express my sincere appreciation, admiration and respect for the exhaustive work product and dedication to purpose exhibited by Dr. Barrett, as well as each and all of you, without exception, in tireless pursuit of an elusive issue whose daunting complexity is the more so confounding because it challenges our introspective understanding of human nature itself.

(Pause.)

Do you have a moment? --because it challenges our introspective understanding of human nature itself, and literally dares us to strive toward attainment of a seemingly impossible goal in terms of a higher idealized standard of human quality in the spirit of genuine community.

I firmly believe, beyond the perception of circumstantially adversarial roles, we are essentially one people, one species, in sight of a Supreme Being, and I submit to you, in my personally elective role, I am neither your enemy nor your friend, but only the seemingly presumptuous reminder of your private and personal conscience, and of the unalterable fact that, within each reasoning being, creator-endowed with intellect, free will and conscience, there is a heart that beats for all mankind, and that essentially, in the words of Buckminster Fuller, "Unity is Plural."

The issue, then, cannot else but ultimately serve to bring us together enroute toward a brilliant horizon of extraordinarily human achievement, which lies just up ahead.

Phillip Wylie's book, "The Answer," told of an angel who fell to earth, and carried a golden book with but a single page, on which were inscribed three words, "Love each other."

And, perhaps surprisingly, but irrefutably, that, and nothing else is the key to a closer understanding of ourselves, each other, and the subject topic issue surrounding the storage and disposal of high-level nuclear waste, as well as a broad range of priority imperative issues which challenge reasoning humanity.

And I remind you--of yesterday. It undoubtedly won't be repeated.

Assistive to a closer understanding, it's been said that "Music is a universal language." Let me hear the music of a nation. I have no need to know their laws. And, today, increasingly, it seems good music may be the only sweet thing left.

And so, in closing, I wish to share a factual story about a dishevelled hermit who lived in a tree, high above the Laurel Canyon wilderness area of Griffith Park in Los Angeles. One day, he descended from his leafy perch, and, garbed in his threadbare robe and open sandals, and carrying a manuscript, he made his way downhill to the then world capital of recorded music, at Sunset and Vine, in Hollywood, where he managed to have published the only song he had ever written, which was promptly recorded by Nat King Cole and became an instant hit. The song was biographical, and although the hermit composer, whose name was Eden Abez, later died and was forgotten, the song, entitled Nature Boy, deservedly took its place in the annals of immortal music, because of the insightful human wisdom expressed in its unforgettable closing line, which read, "The greatest thing we'll ever learn, is just to love, and be loved in return."

At this juncture, in the spirit of fellowship, I'll thank you for this opportunity to provide my final public comment to the eminent and prestigious Chairman and members of the Nuclear Waste Technical Review Board of the United States, secure in the cognizance that, bolstered by that closer understanding of human nature and the intrinsic value of all current and ensuing human existence, you will indeed do the right thing in sight of Almighty God.

Thank you and Goodbye.

COHON: Thank you. And we note that you wrote those out, and you're going to leave it with us. Good. Thank you very much, Tom.

Jim Williams?

WILLIAMS: I'm Jim Williams. I work for Nye County in the State of Nevada, and at some time past, for Clark County. I hate to follow the comment by Tom, but Jeff's presentation this morning prompts me to raise a few concerns that I've carried about this process that regard the dominating focus of this very high-level process that we see here on the repository design, the Total System Performance Assessment, and repository licensing, which has the hazard of systematic diversion from other dimensions, arguably equally important in a successful waste management system for managing the nation's high-level waste.

My quick list of those concerns include the cost and the national commitment to meet those costs, the organizations for 100 to 300 years of implementation, the transportation, rail is in Jeff's Total System Performance Assessment, but there's a wide opinion that Congress will never appropriate monies for rail construction, and equity, not just geographic, but other dimensions of equity.

Regarding the models for the Total System Life Cycle Cost, I think it might be useful for this group to compare with the very intense and long-term review that's been focused on the Total System Performance Assessment, and its underlying models and its underlying data, compared to the Total System Performance. I would argue that there's been very little attention, public, independent, very high-level, and external to the factors incorporated in those models, into the models themselves and their validity, their application to look at alternative program approaches, such as the alternative that most of our existing plants could be extended in their license terms, and increase the amount of spent fuel beyond what could be stashed in Yucca Mountain into major contingencies in the program, and the program response to those contingencies, and to the cost to others, including industry, which Jeff mentioned, but also corridor communities and others.

So, my confidence in the process would be increased if there was attention to this cost, and some of these other dimensions comparable to that focused on Total System Performance Assessment.

COHON: Thank you for those comments. Thanks to all of our commenters, and thanks to all who participated in this meeting.

As I said at the outset, which now seems like about three weeks ago, not just two and a half days ago, this was an important meeting, important both for its content and for its timing. If DOE stays the schedule, we may see a Secretarial recommendation on the site early next year, or sometime in that time frame. This, therefore, is likely our last meeting before that key milestone in the life of this project.

Therefore, the information we received over these last two and a half days, both from DOE presentations, from other agencies, and from the public, are especially timely and valuable, and I thank you all.

I want to thank Dan Fehringer, who is the lead technical staff members, who arranged this meeting. Thank you, Dan. You did a superb job.

To our administrative staff, the two Lindas and their colleagues who put together the meeting, to our technical support staff over here who allow us to be audible, allow us to see things, and even to make a record of it all.

I want to convey a special thanks to the speakers, especially yesterday and today, for your willingness and ability to stay on task during a very difficult time. You did a superb job, and we thank you for that.

We are adjourned. Thank you all.

(Whereupon, the meeting was adjourned.)