UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SUMMER MEETING

REPOSITORY DESIGN AND THE SCIENTIFIC PROGRAM

Beatty Community Center
200 A Avenue South
Beatty, Nevada 89003

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The Scientific Program

<u>NWTRB BOARD MEMBERS PRESENT</u>

Mr. John W. Arendt
Dr. Daniel B. Bullen
Dr. Norman L. Christensen
Dr. Jared L. Cohon, Chair, NWTRB
Dr. Paul P. Craig
Dr. Debra S. Knopman
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek, Co-chair,
Session on Geochemistry and Hydrology
Dr. Donald Runnells, Co-chair,
Session on Geochemistry and Hydrology
Dr. Alberto A. Sagüés, Chair,
Session on Materials and Performance Modeling
Dr. Jeffrey J. Wong

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I N D E X

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P R O C E E D I N G S

8:00 a.m.

KNOPMAN: I'd like to welcome everyone here again this morning for the second day of the Board's meeting?

Our plan for today is to focus on the progress in the Yucca Mountain Science Program, hear about the Nye County Drilling Program, and the State of Nevada's Hydrologic Studies.

The Board is most interested in hearing of new results in these various programs, their interpretation, and their implications for performance and reduction of key uncertainties.

My colleague, Alberto Sagüés, will chair the first session on Materials and Performance Modeling. When we reconvene at 10 o'clock following a short break, Dick Parizek and Don Runnells will co-chair the next session on Geochemistry and Hydrology.

We're going to be breaking for lunch at noon and resuming promptly at 1:10, and I just also want to remind everyone that we will again be taking comments from the public at 2:30, and we plan to adjourn as close to 3 o'clock as we can.

For those in the public who would like to ask some questions during the sessions themselves, we do have cards in the back, on the back table there, and you may write out your question, give it to Linda Hiatt of our staff, and she'll pass it on to us.

I'd just also like to again offer our Chairman's disclaimer from yesterday, so everyone is clear on the conduct of our meetings and what you're hearing and its significance. When a member of the Board speaks, and this includes the Chairman, that member is speaking for himself or herself. We are not stating Board positions, unless we indicate otherwise. When we speak, we're speaking as individuals.

So I'm now very pleased to turn the gavel over to Dr. Sagüés, and look forward to a good session today.

SAGÜÉS: Very good. Thank you, Debra. I'm Alberto Sagüés, a Board member, and we're going to have a couple of presentations in this session. The first one, of course, is the all important issue of the performance of the waste package materials. And this first presentation is entitled Waste Package Corrosion Testing and Model Development. The presentation was prepared by Joe Farmer, John Massari and Venkat Pasupathi of the M&O Waste Package Operations. And we have seen Joe Farmer present them in the past to the Board. I don't think that he needs much further introduction, and we're ready for him, and there he is.

FARMER: Well, the title of this presentation is Waste Package Corrosion Testing and Model Development, and if we could go to the introduction? We, of course, all realize that we have a very difficult task. We have to maintain long-term containment of the various high-level waste components. We have to do this for an extremely long period of time, 10,000 years. This means, of course, that we have to measure very, very small penetration rates, or corrosion rates, and we have to do this with a very high degree of accuracy. So a lot of what we've been doing over the past year has been directed in this way, and I think you'll see that as we go through some of the viewgraphs.

Of course, we also realize that site recommendation and license application requires a number of credible predictive models based upon sound scientific understanding. We've also been endeavoring to do this. We have developed a number of models to satisfy this end. These include models to address general and localized corrosion, stress corrosion cracking, juvenile failure and phase stability.

And, finally, we've already been able to draw I think some fairly solid preliminary conclusions based upon this initial modeling effort. First of all, with Alloy 22, and this is of course one of the reasons we picked it, we don't anticipate any significant localized corrosion. It has a very high repassivation potential, so this turns out to be an excellent material for the application that we're looking at.

We also believe from looking at data from the long-term corrosion test facility and other sources of data, that the life of the waste packages will not be limited by general corrosion. There was a concern early on with the phase stability of material. As you'll see in some of the viewgraphs today, we've been conducting in depth phase stability studies of this material, looking at the precipitation of Mu and other potentially undesirable phases, and determining at what time and temperature you have to have to encounter these undesirable phases. And basically, we've also concluded that this does not appear to be a significant problem for the material under repository conditions.

At the present time, we're focusing most of our attention on the final closure weld. This final closure weld is unannealed. We can't relieve the stress very easily and, consequently, it's a potential place where stress corrosion cracking might occur. So we're putting a lot of effort right now on stress corrosion cracking.

This schematic is actually an overall, a roadmap of how we're marrying or integrating these various models together. Up at the top of the page, you can see one module that says, "Waste Package Surface Environment." It turns out that the environment that the actual waste package interface sees is not going to be exactly the same environment that you have in what is referred to as the near field environment. It's going to bee exacerbated because we'll have evaporation and refluxing of the groundwater on the waste package surface. This will tend to concentrate ions that could bring about more rapid corrosion. So we have actually been conducting a lot of studies, and have now more or less experimentally determined what this waste package surface environment will be.

We also have a phase stability model at this particular point in time. This phase stability model includes TTT diagrams, time, temperature, transformation diagrams, based upon transmission electron microscopy data, as well as precipitation kinetics that tell us how fast it takes for these various undesirable phases to precipitate.

You'll remember that a year or two ago, we were calculating environments in crevices, things such as dissolved metal and crevice pH. We've now set up experiments and we've gone in and determined these pH quantities in situ, so the data you'll see today are not calculations, but experimental measurements, and these measures have tended to validate the computational models that we've developed.

We have experimentally determined corrosion and threshold potentials. This is a very important parameter in determining whether or not the waste package will undergo general corrosion or localized corrosion. And, in fact, it's these measurements that we use as a basis for justifying the use of general corrosion rates for the waste package.

Once we go down through a number of switches, based upon thresholds of potential and relative humidity, we finally get to various penetration rate models. We have our rate models for dry oxidation, humid air corrosion, localized corrosion, general corrosion, and also stress corrosion cracking.

As you'll see later in the talk when we go to deal with stress corrosion cracking, we actually look at two competing methods for dealing with that particular degradation mode; one method based on a threshold stress intensity factor, and another based upon a finite rate of crack propagation that's dependent upon the stress intensity of the crack tip and environmental parameter.

As I mentioned before, in the past, we've calculated the types of conditions that we have in crevices that might bring about premature failure of the waste package. At this point in time, we've actually gone in and experimentally determined these. The top lines in the graph that are horizontal and centered between pH 8 and 9 represent the type of pH that you would see in a crevice of either 316L or Alloy 22 in the presence of the various buffer ions that exist in the J13 groundwater.

And, in essence, what you see with a lot of buffer capacity in the water, even though we form crevices, and even though we apply a very high potential to the mouth of that crevice, the pH does not suppress to any great degree.

Now, in contrast, if we remove those buffer ions, as might be the case at high temperature, you can get almost spontaneous acidification with an alloy such as 316L stainless steel. And, of course, this is one of the reasons that we're using a much superior material, Alloy 22, as the waste package wall.

If you look at the curved line that is labelled Alloy 22 in 4M or sodium chloride, this shows that even with essentially a saturated chloride environment, no buffer ion present, and a very high applied potential, let's say 400 millivolts, which is probably the highest that one could ever imagine, the pH suppression is only 6.

So, frankly, even with the crevices, with a material like Alloy 22, which remains passive, it seems that the crevice environment will in itself be fairly benign. This, of course, is good for us.

Another criticism that we've received in the past, and I think a criticism that we've now addressed, is the fact that we have not been working in the most saturated possible electrolytes. We've now determined what those worst case or most saturated electrolyte compositions are. One of those we now term SSW for simulated saturated water, and this particular electrolyte has a boiling point of around 120 degrees Centigrade. So we go in and run a cyclic polarization curve such as this, and you can see that between the corrosion potential and threshold potential 1, we maintain good passivity throughout this entire region.

And for those of you who are familiar with the cyclic polarization curves for 316L, some of which are shown in the back of your package, this is very different. If this were 316L, you'd see a very large excursion and current near the corrosion potential due to the spontaneous pitting of the 316L. And, of course, we don't observe this with the Alloy 22. It remains passive over quite a large range of potential.

As I mentioned before, we're having to go in and measure extremely low corrosion rates. In the past, the only tool that we used to address the issue of low general corrosion were weight loss measurements. We have determined through air analysis that as basic as these weight loss measurements are, they're still quite sensitive. We can measure penetration rates down to the order of 16 nanometers per year with the weight loss measurements. So we can do reasonably well with that.

But to add credibility to these general corrosion rates, we're now using atomic force microscopy. This basically shows two surfaces exposed in simulated acid water, which is an acidified concentrated J13 water in the long-term corrosion test facility for one year. And, frankly, and most of these coupons, you see virtually no attack of the substrate. Any morphology that you generally see is due to the formation of a silicate deposit on the surface, and I have a number of those images I'd be happy to share with some of you at the break afterwards.

We're focusing most of our attention on stress corrosion cracking. We don't believe that, as I said before, general corrosion, or even the localized corrosion, is going to be the most important mode of failure of this particular waste package design.

Looking at the waste container in an unperturbed state, there are three sources of stress that could bring about stress corrosion cracking, and this is the unperturbed waste package sitting underneath a nice intact drip shield.

First of all, there's the weight stress. That's due to the mass loading of the container between two pedestal supports. There's then a contact stress, which would be due to some process, for example shrink fitting. And finally, there's a residual weld stress. Most of you who are familiar with welding realize that after you weld a material and it cools, you have a very high stress left in that weld region unless you take steps to mitigate that stress.

Now, as we look at the Method A stress corrosion cracking model, and again we're approaching this in two different ways, and I'm just going to show you the Method A today, we base the propensity of stress corrosion cracking on whether or not the stress intensity factor at the base of a preexisting flaw exceeds a stress intensity threshold, K_{1scc}.

So in making this determination, we need two things. First of all, we need a distribution of flaw sizes. We need to know how a flaw is distributed, and this data we're getting from John Massari. Secondly, we need to know exactly what the stress is in this weld region.

So we've gone in at this particular point, and we had used the ring core method to actually go in and quantify the stress in this weld region. And, of course, you can see that in an unannealed weld, we can be up close to the yield stress, around 55 ksi.

Now, one could just take this weld stress as the way it is and say we're going to have to live with it. And I guess to respond to some of--I've heard some critics that maybe we need to exercise somewhat more creativity, and I think this is a creative solution that we're looking at for complete mitigation of these weld stresses.

We have new laser peening technologies that we're using on turbine blades, and we've now actually laser peened some weld samples for Alloy 22. And the beauty of laser peening, or even shot peening, for that matter, is that you can introduce compressive stress over the weld, and that compressive stress can completely mitigate the residual tensile stress introduced during welding. And, of course, if you don't have that tensile stress, there's no way to have stress corrosion cracking. So we are in fact looking at some new techniques like this to completely mitigate some of these anticipated mode failures like stress corrosion cracking.

John Massari, who's in the audience, has done a great deal of work in the past year to look at juvenile failures in the waste package. He has conducted a very broad based literature search, and he has found a number of generic flaws that might be anticipated in the waste package.

For example, you would have welds and base metal flaws. You might also have out-of-spec material in the weld or base metal, improper heat treatment, surface contamination, handling damage, and also other types of administrative errors that could bring about problems. And he also lists four flaws there that are not actually anticipated in the waste package.

He, in looking at the literature, he looked at a broad base of information. For example, he looked at data having to do with boilers and pressure vessels, nuclear fuel rods, radioactive Cesium capsules, dry storage casks for spent nuclear fuel, and food storage cans.

And the interesting thing to me that came out of all these studies that John did is basically, the failure rates for a container that is welded shut would be expected to be somewhere between 10^{-4} and 10^{-6} per container. And I think that these failure rates are within the realm of acceptability for the waste package performance.

John has also furnished us with a very nice set of flaw size distributions that we're directly inputting into the stress corrosion cracking model. Remember, this particular mode failure model requires two inputs; one, the weld stress. We've now experimentally measured that. And a distribution of flaw sizes. And from the study that John has done, we now fortunately have the distribution of flaw sizes that we're using in the model.

As I mentioned to you before, one of our biggest concerns when we first started looking at these extremely corrosion resistant materials like Alloy 22, and frankly, one of the deterrents from using these materials in the first place were possible problems having to do with phase stability.

Frankly, it's not that we thought that there would be a phase stability problem, but it's known that many alloys, such as 316, are in fact metastable over extremely long periods of time. So when you want to use those in an engineering application such as the one we have here, you have to do fundamental studies to go in and assure that you aren't going to precipitate out an undesirable amount of phases that can cause invertilement of the material.

We've now aged a large number of samples. We've done transmission electron microscopy, and in a nutshell, we've concluded that we could hold this material for 300,000 years under isothermal conditions before we would have the onset of any of these precipitates to form on a grain boundary. So we think that this is a very positive statement for the waste package material. So at this particular time, we don't believe that phase stability will be an important life-limiting problem with Alloy 22.

This is a time, temperature, transformation diagram. I think many of you who are in the engineering field recognize this. This is something we frequently rely on in terms of deciding what ranges of temperature we can work with a given material. And basically, by looking at this diagram, you can conclude that, again, at 300 degrees Centigrade, we should not have appreciable long-range ordering or precipitation of undesirable phases, such as P or Mu.

And I know Jim Blink explained this yesterday, but the reason of course we don't want these undesirable phases is if we completely decorate the grain boundaries with these, the material becomes mechanically embrittled. If these materials precipitate in the bulk, they tend to be rich in tungsten and molybdenum, which are the two alloying elements that are responsible for the tremendous passivation of this material.

So if you form these precipitates that are rich in tungsten and molybdenum, you deplete those alloying elements adjacent to those particles, and you might potentially open up a pathway for a localized attack. So fortunately in our case, it would take, you know, many times longer than the life of the repository to achieve those conditions, 300,000 years.

We are putting all of these various waste package models into reports that have gone under various names in the past six months. But I think at the present time, they're called ARs, if I'm not mistaken. I think those are analysis reports. They were analysis model reports at one point. But at any rate, these reports summarize the models that we're using to assess waste package performance. And I won't go to the trouble to read all of these for you, but you can see the large number of these analysis reports that we're preparing right now. Many of them are nearing completion, and we are pulling them all into one collective report called a process model report that will be used as one of the basis documents for site recommendation.

Let me see, there are a number of other important issues that we have to deal with. First of all, I think we have to realize that in the last year, we've come up with actually a very good design, but it's a new design and it has some new materials in it. And for this reason, I think we have to realize that we don't have as much data with some of these new materials that have recently been introduced as some of the materials, such as Alloy 22, materials that have been in the program historically.

So, frankly, we're probably going to need to acquire some more data to give a more solid foundation to the models that we're developing.

There's some uncertainty because of the fabrication processes, and the TSPA/VA design, when we did the shrink fit, the shrink fitting operation actually introduced compressive stress into the Alloy 22. If we do the same shrink fit operation in this particular design, the stress that's introduced into the Alloy 22 is actually tensile, and might conceivably contribute to something like stress corrosion cracking.

So I know that many of the engineers in the program are looking at exactly how we pursue these various fabrication processes to make sure that we don't inadvertently introduce problems into the design as we seek to improve it.

And, of course as I've shown you, we have other techniques available to us now, such as laser peening, where we can mitigate weld stress.

There are other issues having to do with the modeling itself. For example, in one particular area of most concern to us now, stress corrosion cracking, we have two competing models, so one is faced with an academic argument of which model is better. Frankly, rather than try to answer that question, we've decided that we will probably pursue both of these models in parallel, and do an assessment with both models more or less as a type of sensitivity analysis. So hopefully in the future, you will see an assessment of the container based upon these two competing methodologies for assessing stress corrosion cracking.

We at the present time have given much consideration and done a lot of experimental work on microbial induced corrosion. We know that sulfate reducing bacteria can produce sulfide, and sulfide might exacerbate stress corrosion cracking. We have not yet quantified this model, but are going to strive to do that in the future.

Other effects that I think in the past have been assumed fairly minimal, and I think, in all fairness, are probably still not very significant, are probably the effects of gamma radiolysis. There have been various versions of the newer designs where the gamma field was quite high, and in those cases, we would have had to account for the anodic shifts in the corrosion potential due to the formation of hydrogen peroxide. But I think in the EDA II design in its current incarnation, I don't perceive that as a problem, but we are undertaking a strategy to deal with issues such as gamma radiolysis and the impact on the corrosion potential.

And in summary, we of course have picked materials that have very, very long lifetimes. In order to assure public safety in dealing with these materials, we have to make very small penetration rate measurements, and we have to do these with a high degree of accuracy. In order to make these measurements, we're using a number of cutting edge tools, as I've shown you, such as atomic force microscopy.

We're also trying to develop models that have a sound scientific basis for site recommendation and license application. A number of these models have been developed and are going to be used as a basis for performance assessment. These include general and localized corrosion, stress corrosion cracking, juvenile failure and phase stability.

And, finally, to reiterate our preliminary conclusions, we don't believe that there will be any significant localized corrosion. As you see, the conditions in the crevices, will probably be quite benign. The life should not be limited by general corrosion. And we've also seen at this point that it looks like phase stability is not going to be a life limiting issue either. We're at the present time focusing our attention on the stress corrosion cracking, and hopefully by the next time that the Board meets, we'll have some good things to say about that.

SAGÜÉS: Thank you very much, Joe. Let's see if we have some questions from Board members. Dr. Bullen?

BULLEN: Bullen, Board. Joe, actually I've got three questions. The first one actually comes back to your last point on gamma radiolysis where you make a point that you're going to repeat the gamma pit studies for Alloy 22 and Grade 7 titanium. Are those gamma pit studies open system or closed system?

FARMER: When you talk about open system or closed system, you're talking about the electrochemical cell?

BULLEN: Well, actually, I'm not interested in the potential changes. I'm actually interested in the production of the products, and in an open system--

FARMER: Nitric acid.

BULLEN: Well, in that case, nitrites, and then the potential for the formation of nitric acid in the right environment. And I guess the question that I have is in an open system, and to be honest, I'm not really concerned about radiolysis effects on the waste package. I'm interested in radiolysis effects on the structural components in the near-field environment. Is there any effort to take a look at that?

FARMER: Frankly, I think the general assumption has been that the gamma radiolysis is insufficient to cause any significant defects in the material. If it was neutron radiation, it would be a different issue. But with the gamma radiation, I think that most of the impact we'll see will be on the environment. The gamma field will couple with the environment, and then that will couple with the material itself.

BULLEN: Right. And the concern that I have is in an open system, if you take a look at an unlimited amount of water vapor and an unlimited amount of nitrogen in the air, then what I've got is the potential over the 300 years or so that the Cesium and Strontium are giving us the big gamma doses, to produce radiolysis products that probably won't affect the waste package, because even with the potentials that you're going to get, and the acidification of the near surface, it's not a problem. But if it moves to other locations and actually condenses in cool spots in the repository, that's where you end up with the problem.

FARMER: I think the experimental limitations are that the studies will be just--we did these studies in the mid Eighties, as you remember.

BULLEN: Right. These are the Bob Glass studies?

FARMER: Right, the Bob Glass experiments.

BULLEN: Okay.

FARMER: And, of course, Kim Jin Young at General Electric, he's doing those types of studies at the current time, same type of apparatus. And just because of the practical limitations of the experiment, you wind up doing these in a closed electrochemical cell.

BULLEN: Right. You might want to consider in the far term, or at least addressing the issue of an open system and the potential production, particularly in light of the fact that the Shoesmith studies results are mostly closed system. They're looking at brines, they're looking at anoxic environments, and it's not what we have. And so the concern that I have is that you're really going to have something like the Climax Mine effect, where in the heater holes, it didn't rust, and in the waste holes, it rusted.

FARMER: Yeah.

BULLEN: And I think something of that magnitude might be important, particularly replacing the concrete liner with steel sets and rock bolts, is going to give us a potential for accelerated degradation and a very high radiation field environment. Of course, you could make that radiation field environment go away if you made your waste package a little thicker, but that's not anything that I'd want to suggest.

FARMER: Yeah. I think in terms of modeling, you know, the gamma radiolysis, one thing we'd have to do for an open system is you'd probably run a closed system experiment where you would measure the accumulation of hydrogen peroxide and corrosion potential, and then you would probably construct something like, you know, the standard chemical engineering stir tank reactor model.

BULLEN: Yes.

FARMER: Where you basically have in-flow/out-flow and you've got a simple first order ordinary differential equation you can solve to calculate the build-up of those species.

BULLEN: That would be a great model to set up, because then you could tell me in a quantitative sense over 300 years, what's the cumulative amount of bad actors you're going to make.

FARMER: Okay.

BULLEN: And then that would be something that's, you know, readily doable and you could say, well, we've got a whole lot or we don't have very much, and maybe work from there as to address the issue.

The other thing is you could do that effect with varying dose rates. I mean, the dose rates of a few hundred r/hour, to depending on the waste packages that you talked about, if you get to 10 to the 3rd r/hour, you get into some really exciting areas of radiation production.

Can I change gears here and ask another question?

Could you put Number 16 back up, please?

FARMER: Okay.

BULLEN: It's the TTT diagram. You've got a not yet sampled box on sort of the 350 degree C. range there where you're looking at the purple curve on the far right. Are those data points going to be filled in any time soon?

FARMER: Actually, this is an old viewgraph. Those data points are filled in, and I think the TTT diagram has not changed very much.

BULLEN: Okay. So you actually have the boundary there for the Mu phase formation?

FARMER: Yeah. You know, this is a fluid and dynamic program and sometimes the viewgraphs come off the printer before the data gets filled in.

BULLEN: Ah, before the data are available. Okay. So you're pretty confident that at the 350 degree C. range, you've kind of turned the corner and you're not exponentially going down any more?

I guess the question that I have, and you always ask this on a TTT diagram, is if I wait long enough, can I still get it? And if I have a couple hundred years at 200 degree C., am I going to be in the range where I've got some Mu phase formation at the grain boundaries that might give us some problems?

FARMER: We've done two calculations. For example, it's very easy when you look at the micrographs, this actually is Tammy Summers' data, as I know most of you are aware, but as Tammy looks at the micrographs, it's very easy to see the point at which the grain boundaries are fully decorated with these precipitates. You know, it's like Christmas decorations. It's actually very pretty. It's a little frightening, but pretty.

And then the harder thing to spot is the onset of that precipitation process. Of course, from a design performance point of view, we're not so interested as to when that ultimate precipitation occurs. We're more interested in the onset. So it turns out that through a mixture of techniques, both transmission electron microscopy and interestingly enough, just electrochemical measurements, you can actually see at what point that precipitation process begins. It's just like using an EPR or some of the electrochemical techniques to detect M23 C6 carbides and sensitized stainless steels.

So, in essence, I think Tammy is using this particular technique. She's using electrochemistry coupled with both the transmission electron microscopy, electron beam defraction, and the SEM. And she's marrying those together to pinpoint the onset of that grain boundary precipitation.

BULLEN: Well, as with any kinetic process, you get a lot more confident if the temperature goes lower, that it's probably not going to happen. And so if the waste package temperature never got above 150 or 200 degrees C., you're really confident that you'll never get the Mu phase?

FARMER: Well, I think you're getting into model and the issues of model uncertainty, and I'd say that the biggest thing that we're concerned about now with the phase stability modeling is reducing uncertainty, and I think that probably is true across the board. But I would say based upon Tammy's preliminary calculations with the model she has in place at this point, the indication is that, you know, and this is holding the waste package isothermal, that means that it doesn't go through a pulse, but it stays there for a long, long time, 300,000 years, to get the onset of precipitation.

BULLEN: At what temperature?

FARMER: 300 degrees Centigrade.

BULLEN: Okay.

FARMER: So 300,000 years at 300 degrees Centigrade. And you'll notice yesterday when Jim Blink gave the presentation, I think that some of the temperatures selected in the LADS process were not entirely coincidental. I think the folks who went through that process, I think they obviously had the cladding temperature limit of 350 degrees Centigrade in mind, but they were also trying to keep the temperature on the waste package materials down as well, because, you know, a low temperature benefits you in many,

many ways.

BULLEN: And actually I would agree with that wholeheartedly, and the only concern that I have is that if you've nucleated this during the weld process, that you may actually want to stay cooler so that you can buy yourself some more time. And what level of coolness you ascribe to, whether it's the Board's, let's not boil it, or if it's something a little higher than that, I think the lower the temperature, the better off you are.

Now, one last quick question and then I'll be done. I promise.

COHON: Dan? Dan, wait a minute. Could I just jump in for a minute while you're talking about this?

BULLEN: Yes, Jerry, any time.

COHON: This is a question from the real world as opposed to you experts on this stuff. I'm trying desperately to understand this because it sounds like it's very important and goes directly to the life and performance of the waste package.

How many orders of magnitude, looking at this diagram, are you going out to the right from data that I'm presuming those dots represent?

FARMER: Well, obviously quite a few orders of magnitude. And, frankly, this data, this is more or less a map of the data points that we have. So at this particular point, you know, we have samples that have been aged up to 40,000, 50,000, 60,000 hours, five years, and those are about the longest data points that we have.

What we do is we take a whole array of those samples, and interestingly enough, in terms of reducing model uncertainty, I didn't believe it until it was proven to me, but in terms of increasing the certainty in this TTT diagram, some of the data points at high temperature and low time actually are heavily weighted in terms of reducing model uncertainty, you know, the point where you can actually determine the nose of those curves in the TTT diagram.

But they basically take these samples that have been aged five years, and some that have been aged for shorter periods of time, and they build Arrhenius rate expressions. And by looking at those samples and determining the volume fraction of Mu or P phase, or the combination of all of those phases, are precipitated on the grain boundaries. You can actually develop kinetic rate expressions. You can determine the uncertainty in those model parameters, and you can extrapolate those out to a long time, and that's been done.

I would have to say that I believe that, for example, if we say that the life of the waste package based on phase stability is 300,000 years, we're probably saying that it's probably somewhere between 30,000 and 3 million.

But the point is at 300,000 years, you have quite a good margin.

Now, if we were telling you that the waste package lifetime was 10,500 years, you know, it would be somewhat more squeamish.

COHON: 30,000 to 3 million based on order of ten years of data?

FARMER: Correct.

COHON: Okay.

FARMER: And unfortunately, in our finite lifetimes, that's probably about as well as we can do as human beings.

SAGÜÉS: Since I'm the Chair, I'm going to take advantage of that. Of course in any of these things whenever you're getting to an Arrenhius extrapolation, you're doing that in conjunction with some kind of a nucleation and growth model for the precipitates.

FARMER: Correct.

SAGÜÉS: And that model has thermal dynamic assumptions, and it has assumptions as to in which regimes the phases can grow up and they cannot grow as well; right?

FARMER: Right.

SAGÜÉS: So that is more than just simply--I mean, the Arrhenius extrapolation is, by itself, an assumption.

FARMER: That's correct. You can have changes from one mechanism to another. You know, you would get a discontinuity in the extrapolation line.

I would tell you, for those of you who are familiar with modeling of metallurgy, we have two codes that we're now using. We're calculated the phase diagram with a code called THERMOCALC. That was prepared by Dr. Larry Coffman at MIT. He's working in collaboration with Patrice Turchi at Livermore. I think these two folks are probably worldwide, probably just about two of the best experts in terms of doing these types of solid state thermodynamic calculations. So they're working on the modeling of a phase diagram.

We're also using another code called DICTRA, which can in fact account for some of these more subtle changes in mechanism. And aside from that, I probably can't give you a better answer at this particular point.

SAGÜÉS: Very good. Are you finished?

BULLEN: One last quick question. Bullen, Board.

Back to the laser peening, I'm just curious as to how, when you do laser peening of the closure wall, and you say you can do multiple passes and actually get the compressive strength?

FARMER: Correct.

BULLEN: How deeply can you go, and have you considered the fact that ultimately, you've got this oxidation or corrosion that's going to take place, and so can you completely mitigate the yield strength, or the yield stresses that are in the weld, or do you only have a surface effect, and how deep is this?

FARMER: I don't think you can completely mitigate the weld stress. I think most importantly, you can probably completely mitigate the weld stress at the surface. And if you'll look back at Slide Number 11, these are some laser peening data collected at Livermore for 4340 steel, and I have to apologize, we did this with a different material. Now we have data for Alloy 22. We don't have it plotted, but hopefully at some point in the future, we'll be able to share that with you. But what you see from this diagram is that you can do multiple pass laser peening. In essence, they set the, if it's a circumferential weld, they set it on like a turntable and rotate it, and you actually have--it's a very interesting process. You take a Q-switch laser and you zap the surface of this weld, and every pulse they hit the waste package with, you have to have black electrical tape, you know--

BULLEN: Uh-huh.

FARMER: Of course these metals are reflective surfaces and you don't get much light absorption. So what they do as a tamper, they use black electrical tape wrapped around the weld, and then they spray water on that black electrical tape to cool the waste package. So when the laser beam hits the black electrical tape, it turns the electrical tape into a plasma, if you will. That plasma couples with the water. The water is keeping the base temperature of the metal cool, but it's also helping probably form a steam bubble. And that sends an acoustic pulse into the material that dries it into plasticity.

And, in fact, I think we took one sample back to DOE and showed some folks there, but you could actually see-- you see the little pock marks all in the Alloy 22 surface around the whole weld. So it's amazing that, you know, light beam could do such a thing.

BULLEN: In the process of your modeling of stress corrosion cracking, can you use the corrosion rate, so that after I've basically corroded away the surface that I've affected, I switch on to SCC model, and so if it takes me a thousand years or 10,000 years to get rid of 50 mils of the outer surface, then I actually have the underlying residual stresses that would be available to switch on an SCC model then?

FARMER: Exactly. That's exactly what we're trying to do. I know that June has set up, or he's in the process of getting his model set up to do that exact thing. So as he iterates through the cycle, he'll probably, every iteration, he'll do an inspection to see if stress corrosion cracking is a problem. And if we introduce laser peening, we'll know that we've mitigated the stress down to a certain depth, and then after we've penetrated beyond that mitigated depth, then you turn on stress corrosion cracking.

BULLEN: Thanks, Dr. Farmer.

SAGÜÉS: Very good. The next question is from Don Runnells.

RUNNELLS: Don Runnells, Board. Just as an aside, I'm interested to hear you're using black electrical tape. I think when you reach the level of using duct tape, the program will have really arrived.

FARMER: Well, I can tell you in my lab, we're there.

RUNNELLS: Okay. I'm not surprised.

I have a couple questions about Diagram Number 5. Let me ask first about the pH, the crevice pH. You made the point that the potential at the crevice mouth of about 400 millivolts versus silver/silver chloride produces a pH of about 6 or so, 6 1/2.

FARMER: That's the worst case probably.

RUNNELLS: Right; in Alloy 22. Even with high electrolyte, even with sodium chloride?

FARMER: Right.

RUNNELLS: Now, you don't have to pull up the diagram, but in the back of your packet, Diagram 34, you show a pH for the inside of the crevice--

FARMER: Well, actually let me--you may be misinterpreting. When I say crevice pH here, the pHs plotted in Number 5 are in fact the pH inside the crevice.

RUNNELLS: But on Diagram 34, maybe we will have to pull it up then, at 400 millivolts, I guess I'm seeing a pH of 4 or so.

FARMER: In that one, let me see, I look--

RUNNELLS: So I'm trying to understand the--

FARMER: No, actually I look at that one--maybe I could point it out.

RUNNELLS: Or even 3 1/2.

FARMER: At any rate, we have 400 millivolts at this particular point. So in this experiment, the reason I put this in the packages, in a lot of the experiments we would set at a fixed potential and let the crevice sit there for a week, two weeks, some very long period of time. I like this experiment because we basically did steps of potentials so you can see sort of how the pH steps down as we increase the potential at the crevice mouth. And here at 400 millivolts, you can see that the pH inside the crevice is around 6, and I think that's more or less consistent.

RUNNELLS: Okay. I need to use the left-hand side.

FARMER: The blue one is right outside the mouth.

RUNNELLS: Thank you. So that is consistent.

Let me ask you a question, though, about the interpretation in Figure 5 again, if we could go back to that. And it concerns the chemistry of the water and the chemistry of J13 water. You made the point that with the upper two lines in Figure 5, Alloy 22 was in saturated chloride water; is that correct?

FARMER: Correct.

RUNNELLS: And in 4M, NaCl, with buffer ions, even under those severe conditions, if you have the buffer ions in the water, your pH is maintained at a fairly high value.

FARMER: Right. That's what we observed.

RUNNELLS: Right. Now, on the right-hand side of the diagram, you say the buffer ions precipitate at elevated temperature.

FARMER: That's correct.

RUNNELLS: Now, can you explain to me what happens--what connection is there between the precipitation of the buffer ions and that pH that requires the buffer ions dissolve in water? In other words, if the buffer ions precipitate at elevated temperature, does that pH that we see there as being very favorable, a pH of 8, does it change at elevated temperature when the buffer ions precipitate?

FARMER: Well, it can. I mean, the first thing to point out here is we are making an assumption about what happens at--we'll not we're making an assumption, but we know that the buffer ions precipitate out at elevated temperature.

RUNNELLS: Right.

FARMER: All these data are for ambient temperature. we now have some data and are collecting more at high temperature. Frankly, making these in situ crevice pH measurements is sort of a new thing in its own right, so we did the easiest measurements first, which were at ambient temperature, and now we're doing a high temperature. But the reason that we gave this curve with saturated chloride and this curve with buffer is the realization that reality will fall somewhere between those two limits. You can think of it as two bounding curves, if you will.

And, in fact, this particular point here, we took and mixed 4 molar sodium chloride, so we basically have used SCW with 4 molar, or enough sodium chloride to make 4 molar solution, dumped into it. And basically what you can see is that as you move somewhere between the simulated concentrated well water and the 4 molar chloride, the pH moves between these two limits. So that's actually what you see with this particular single point.

RUNNELLS: Okay. So I think what you're saying then is the extrapolation of elevated temperature, you'd expect the pH, the crevice pHs, to be somewhere between those two sets of lines.

FARMER: That's what I would expect, and we've done measurements up around 85 degrees C., and that is true.

RUNNELLS: Okay. One last question then on this diagram. What role, if any, do the minor components that may be present in the water, the interstitial water, if you like, in the unsaturated zone play in the corrosion pH, the corrosion potential? I'm thinking particularly of things like fluoride and nitrate that may be present in the water in very small amounts, but may be important for corrosion. Have you investigated that at all?

FARMER: We have. We have a whole ensemble of test media that we have right now. There's the simulated dilute water, a simulated acid water, simulated concentrated water, simulated saturated water, and some other variants on those, plus these concentrated electrolytes--or these saturated electrolytes we use. So from that, we can look at those and infer at least first order to what impact ionic strength affects corrosion, and for example, the presence or absence of things like fluoride.

So the quickest answer to that question is just to tell you that in a long-term corrosion test facility, after exposing these things for two years, there's no evidence of crevice attack, there's no evidence of stress corrosion cracking, and there is virtually undetectable general corrosion, though we see some silicate deposit on the surface. And I can show you some of the x-ray defraction results.

RUNNELLS: But that's favorable.

FARMER: That's very favorable.

RUNNELLS: So just in a nutshell then, you're saying even in the long-term tests that you've run in the presence or with trace amounts of fluoride and nitrate, you're still seeing no unexpected or accelerated corrosion; is that correct?

FARMER: That would be my conclusion.

RUNNELLS: Okay, thank you.

SAGÜÉS: Okay, very good. Quickly, on this figure, what happened--your crevices there, the tightest ones are about .11 millimeters.

FARMER: Yeah, I think 110 microns; right, .11 millimeters.

SAGÜÉS: What would happen if you would make the crevice either tighter or deeper? Wouldn't then that tend to bend down to separate curves?

FARMER: Theoretically, you would expect somewhat more pH suppression in a tighter crevice. I think the biggest effect we see in these particular experiments is whether or not you passivate the alloy being used to form the crevice.

For example, we could get some fairly modest suppression of pH, and in fact, if you'd like to see them, I have some photographs where we can show you how the surface actually breaks down.

SAGÜÉS: We can look at those later. It was just a quick comment because we have a couple of other questions.

FARMER: Okay. Tighter crevices theoretically give you lower pH. Frankly, here between 540 microns and 110 microns, you don't see much effect. We could probably cut this crevice size by another factor of two for kind of a more typical crevice dimension. It's probably not going to suppress the pH a lot more. And, of course, this is based upon what we observed, not theoretical, because you get a larger potential, you know, as you constrict the current path, the potential drop becomes greater, the electric field becomes greater, and you pump more chloride in and the pH goes lower.

SAGÜÉS: Thank you. We have a question from Debra Knopman.

KNOPMAN: Knopman, Board. I'm not an expert in this area by any stretch. Let me ask a very general kind of question. If you had to rank the three primary materials that are going to be used in the waste package and the engineered barrier system, A 22, titanium, stainless steel, rank them in terms of your confidence in your ability to make long-term predictions based on current knowledge of materials behavior at the 10,000 year time frame and then 100,000 year time frame, how would you--

FARMER: Well, I would, at this particular point, I'd rank C-22 first. We've had it in the program the longest. You know, we're pretty far along with the phase stability studies. I think, you know, people know a lot about 316L stainless steel, but only on time frames of 10 to 20 years, maybe 30, 40 years. But, you know, once you get into these really long periods of time, you have the same phase stability issues with the 300 series stainless steel that you have with Alloy 22.

So I'd tell you right now, Alloy 22 would be first. I would probably, as long as we don't galvanically couple the titanium drip shield to something like carbon steel, and I think the designers went to great lengths to make sure that this was not done, I think titanium would probably be the second because, you know, its only failure mode is general corrosion. It's a very easy thing to understand and model, relatively speaking.

And, finally, in terms of predictability, I would make the 316L, it's a great structural material, and that's why we picked it, and we're not claiming any performance based on corrosion resistance, and that's not saying that it isn't going to perform well, but it has a very--its pitting potential and corrosion potential are very close together.

If you could go over to Slide Number--it's beyond Number 20, but I'll show you just an example. This is a cyclic polarization curve for the 316L in the simulated saturated water, but here only at 100 degrees Centigrade. And, of course, the reason that we aren't using 316L as the outer barrier of the waste package is notice here, you get this great current excursion. This is due to very aggressive pitting of the 316L.

And if you recall from the curve for the Alloy 22, this was just a flat passive region. So there's no evidence at all of pitting of the Alloy 22, but in the case of the 316, you know, the place where you have pitting occur potential-wise is quite close to the corrosion potential, and that introduces a lot of uncertainty and would make it probably somewhat difficult to predict the long-term performance of this material.

The beauty of the 22 is that, you know, the repassivation potential or the threshold potentials are out close to oxygen evolution, and well removed from the corrosion potential, which is where the system sits at equilibrium.

SAGÜÉS: Okay, I'm going--yes, if we can have a quick question from Paul Craig, we have a couple of additional questions from the audience.

CRAIG: Craig, Board. First of all, Joe, I'm just amazed at how much progress you've made in the last year with your course microscope and the crevice work is very, very impressive.

FARMER: Thank you.

CRAIG: I want to jump a little outside the presentation, and we're now going to two metals, titanium, mailboxes, drip shields, and the C-22, and what I'd like to ask you to do is to help us to think about the problem of common mode failures between those two. Are there concerns, and if not, why not?

FARMER: I guess in my mind, I think that you have, by having a--if you had, let's say, an Alloy 22 drip shield and an Alloy 22 waste package, I would have been concerned in terms of arguing defense in depth error, because if you develop some long-term environment in the repository that, let's say, brought about stress corrosion cracking of the drip shield, it would also bring about stress corrosion cracking of the waste package.

The fact that we have now picked titanium and Alloy 22, something that we might worry about with Alloy 22 is--I don't think this is going to occur, but if I wanted to start playing what if, I would say, well, what if we have a lot of sulfate reducing bacteria, we form sulfides. Well, that is fairly aggressive to C-22, or can be under the right conditions, but not to the titanium.

So I think the fact that we have these two different materials placed apart, as they are, I think that does, to some extent, give you defense in depth. Things that I would expect to bring about a mode of failure in one material do not necessarily bring about a similar mode of failure in the other material.

In terms of--it depends on what you also define as common mode failure. I think there was some interest in trying to model the corrosion of the drip shield initially so that we would be opening up patches or holes in the drip shield, and this would form areas where water would sort of, you know, drip like a leaky roof onto the waste package. I think in reality the corrosion rates of the titanium are so slow, I personally would doubt that you're going to get any patches like that to open up on the waste package over the repository lifetimes.

What you might have happen, though, is you have these drip shields butt to butt, you know, end to end, so if you had ground movement and you somehow displaced those drip shields so that you open up one of those junction points so that you could get water coming through, I think that, if you said that that was a failure in the drip shield, the water dripping on the waste package underneath, you might view that as some type of a common mode failure, because the failure in one has somehow influenced the failure of what's directly underneath it.

SAGÜÉS: Joe, if I may, if it's all right with you, Paul, I would like to address a couple of questions that were given to me from the audience. We have been able to address quite a bit in this issue. But very quickly, if you could give a very brief answer to both, one of the questions is have you or are you planning to look at the effects of radiation induced segregation on phase stability?

FARMER: I honestly have to say we have not proposed to do that. For the most part, I think we felt that the neutron dose for the Alloy 22 is so low that it's not necessary. It's an excellent question you ask, and I think we need to come up with a good technical basis for either doing that or not doing it, and we haven't done that. That's a good suggestion.

SAGÜÉS: Thank you. And the second question from the audience is a more general question. What effect will the heat from the high-level waste have on corrosion of the waste package? And are there any tests planned? This is from Sally Devlin.

FARMER: Yeah, the temperature of course will or could impact the corrosion rate of the waste package. For those of you who have had the opportunity to visit the project's long-term corrosion test facility, you'll remember of course, you know, we test at two different temperature levels, 60 and 90 degrees Centigrade. With the advent of these new test media like the SSW that boils at 120 degrees Centigrade, we're probably in the future going to be bringing along tests at higher temperature. So it's a good question. Temperature is important.

Frankly, though, the general corrosion rates of these materials are so low that they're almost below the detection limit. So I would say even though there is a theoretical temperature dependence there, it's probably not going to push us into an area that we would be overly concerned about.

SAGÜÉS: Thank you very much, Joe. We're exactly on schedule.

FARMER: Thank you.

SAGÜÉS: So we're going to go straight to the next presentation.

The next presentation is an overview of future Yucca Mountain project total systems performance assessment modeling plans, and this presentation is by Mark Tynan from the Yucca Mountain Project Office of Project Execution, U. S. Department of Energy. Mr. Tynan?

TYNAN: Good morning. The title of the presentation is an overview of the performance assessment modeling plans. But before I launch into that, just a brief I'd like to acknowledge Holly Dockery for helping put this talk together, and Bob Andrews and the PA team, I'd like to compliment them for the demonstration of excellence during the past year and perseverance and leadership in the development of workshops that are trying to assure integration in the program.

There's a quote from Huxley that says "The great tragedy of science is the slaying of a beautiful hypothesis by an ugly fact." And the PA people have attempted very hard this year to eliminate those ugly facts by assuring integration with design throughout the year and in the work that they're doing, and I'll lay out how they will be brought into the program, and leading parts of the efforts to assure integration, and to make sure we don't run into ugly facts.

The overview for the presentation is a look at the major drivers for TSPA LA and SR, key findings of the Peer Review Panel Report, and some of the NRC comments. NWTRB, since you're here, you know what your comments are, but you asked for us to look at those.

Philosophy and scope of the TSPA SR/LA iterations, and the PMRs and the AMRs, and we'll introduce what those are, if it hasn't been done already for you, with process model reports and the analysis model reports; the implications of the design changes to the TSPA program, and the schedules, finally, for production of our products.

Programmatic and regulatory drivers for TSPA SR/LA will be that the work to be performed in compliance with the governing procedures and requirements, responsive to review comments on the VA, and implementation of the proposed EPA standards, NRC regulatory requirements, and the DOE guidelines. We will look at the NRC issue resolution status reports acceptance criteria that will be rewritten into the

NRC Yucca Mountain Review Plan. So the NRC is evolving their strategies for us to demonstrate compliance in the final products.

Major technical drivers for TSPA LA is the interpretation of the TSPA VA results, and addressing the comments from the various groups, changes in the repository and waste package designs that we've looked at in the past day, and also the changes that will be made to the process models, updating those that were used in the VA. And then we'll attempt to focus on key information to complete the postclosure safety case.

Key findings of the PA Peer Review Panel on TSPA VA would be the first major area you asked us to address, and the panel said that they had different objectives for VA and SR, but they're exactly the same, but different.

The intent of Congress for the VA was that show that the site performance would meet existing standards. And the objectives for SR and LA is that we can show with reasonable assurance that the repository complies with regulatory requirements.

The use of simplified bounding analyses may be necessary to achieve the desired level of confidence was one of the main points of the panel, and will demonstrate that we're making a shift in some areas to use bounding analyses because they are necessary.

"For cases in which it is feasible to improve either the component models or their underlying data, the Panel recommends that efforts be made to implement such improvements wherever such changes would affect the overall assessment." And we're doing a comprehensive look at that internally at the process models and their updates, the inputs that we have to make changes to those process models, and to examine very carefully what does have a significant impact on the overall performance, and then we can focus our work in those areas.

"Where conservative bounding analyses do not result in unduly pessimistic estimates of the total system performance, the Panel recognizes that it may not be cost-effective to spend additional time and effort refining those assessments and making them more realistic." So I'm pleased to see that they recognize there are diminishing returns on some of the investments we've been asked to make in the past.

"For those issues for which, by virtue of their complexity, it is not feasible to produce more realistic models supported by data, the Panel recommends that a combination of bounding analyses and design changes be applied." And I think the program is demonstrating responsiveness to those comments, with the design changes that you'll see and the approach that we'll take in PA for the assessment.

Comments on the TSPA VA from the NRC staff were in some regards very similar to those that came from the Peer Review Panel. There's a general agreement, we say here, between the DOE and the NRC approaches, with five major areas where significant differences do exist.

NRC states that it's unclear whether sufficient data on waste package corrosion, under conditions applicable to the potential repository, can be acquired to demonstrate compliance with the NRC requirements. And I think we've seen in the last half hour that we've made significant advances in those areas, and we're making progress and should make significant progress to meet our objectives.

Data and models of the quantity and chemistry of dripping water are inadequate to describe the process of dripping under ambient and thermally altered conditions, and we'll focus some of our additional testing over the next couple of years to take a look at some of those features, and also during the past year.

The saturated zone has not been sufficiently characterized to the proposed 20 kilometer receptor location to adequately address its contribution to the performance. And the Nye County program that's being developed during the past year, highly successful program that you'll hear about, and we'll integrate that into our models for SR/LA, and Nye County will continue to do work with us in cooperation with us to try to add and augment our saturated zone model and update that as we move along through the various iterations leading towards the potential license application.

Volcanic disruption analyses and supporting documents not necessarily representative. And that's been a common complaint from the NRC staff for a number of years, but during the past year, we've had numerous interactions, and I think we're moving closer together on resolving those issues.

And then a key concern to the NRC, and I know to some of you, is the implementation of the QA program has raised the issue of whether data products will be acceptable and appropriately qualified. We've had a major programmatic effort this year leading towards assuring that the necessary procedures are in place, that we fund the systems that are necessary to implement those procedures, and assure that the people follow the procedures. And we're having training programs all the time now. It's not like we didn't have a quality assurance program in the past, but we're going to have a standardized approach across the program that will include NEPOL, the science side, the design side and PA, and compliance will be mandatory, so to speak.

Philosophy of future TSPA iterations, it's somewhat changed as we move toward the viability assessment. During the initial phase of TSPA development, they were in large part scoping exercises. So the '91 to '95 iterations of TSPA were non-Q, and not very well controlled. Traceability wasn't what you'd expect to be for a licensing case.

TSPA VA made considerable evolutionary process for its compliance in that area, and all future TSPA documents will have needed controls placed on all data, models and software analyses and documentation. This is to enhance reader review of the documentation by improving traceability. Any changes will be controlled under the change control process, which includes conducting impact analyses. And that will be very key to us in conduct impact analyses to see if new information has significant impact on the models.

TSPA for SR Rev. 00 forms the fundamental controlled basis to which the incremental changes will be made. So within a year from now, we'll have the documentation in large part for the basis for our postclosure safety phase.

Now, this one I apologize, because I'm not a draftsperson, but they tried to help me, and I don't know if they did. The science and engineering activities that provide the data and inputs for the analysis model reports, which are the AMRs, and there's almost--I think there's 150 to 200 analysis model reports that will be feeding into the PMRs, which are the process model reports.

The process model reports, along with the TSPA, will form the postclosure safety compliance arguments, providing the technical basis for that argument. So the documentation for all the TSPA work that will be used referencing back to the PMRs and to the AMRs. The AMRs, some of the data can feed the TSPA directly, or in large part, it will be synthesized within the process model reports.

The process model reports are the equivalent of our synthesis reports in the past, and the process model reports are comparable in part to the technical basis documents that were produced to go along with the VA last year. Each process model report will contain a section that's an abstraction of the complex process model by PA, to be utilized in the TSPA model.

In the PMRs, also the sub-models and model documentation will be available, along with the abstraction. The data uncertainties will be discussed. It will state the assumptions that were made to do the modeling. Model results are output, the code verification, it will contain opposing views, and a discussion and support information for the regulatory evaluation relative to the key technical issues of the NRC.

The linkage of the major programmatic SR/LA milestones are shown in the next illustration, which is difficult for me to see here, but the left-hand column is a listing of the PMRs that will be developed, the process model reports. The integrate site model, that is going to contain what we were shown in the back of the room yesterday. The geologic framework model, the rock properties model, the basic porosity information, the rock properties data, and then the mineralogic and petrologic model developed by LANL.

The second PMR is the UZ flow and transport, the biosphere model, the waste package degradation model, the waste form degradation model, engineered barrier systems, and near-field models, the saturated zone flow and transport, and then for a good measure, we threw in the tectonics report, which is really a consequence analysis that still needs to be done as a complemental document to the prior tectonic reports that are already available.

So those PMRs through time should be updated and impact analyses done to see what major changes may have to be made to the models, feeding Rev. 1 of the PMRs and Rev. 2 of PMRs through time, and the same case for the TSPA documentation for the SR, SR Rev. 1, and then finally, if we move on to a license application.

The TSPA SR overall scope will be to develop the process models, abstraction models and TSPA models, incorporate those features most significant to performance, and include the uncertainty in the conceptual models and the parameters, identify and screen relevant features, events and processes, the FEPs database, and I've given an overview in the supplementary materials at the back of your presentation on what that will consist of and what its schedule will be.

Features and events and processes database is an important one as a guide for the NRC. They've requested that that be developed, and it's also a bright thing to do so that we can use it as a source of information and as a place to point back to the PMRs and the AMRs for the technical basis for each one of the features, events or processes that we know we have to include or exclude in the total system performance assessment evaluation.

In addition, the scope would be conduct analyses using the process and abstraction and total system models most important in accordance with applicable QA controls for data, models and the software. And the PVAR process was the process validation and reengineering process you've heard about from past presentations, and then formal presentations. We will be implementing over 20 new procedures this summer.

Document analyses and technical basis in TSPA SR Rev 00 and the process model reports, and provide the basis for suitability evaluation for the site recommendation.

For future TSPA iterations, again, screen the FEPs using the regulatory criteria, use controlled models and analyses, evaluate the total system performance incorporating the uncertainty and using probabilistic case runs. There's some developmental or evolutionary steps that will be followed in this process. Conduct a stylized human intrusion scenario analysis in anticipation of what the requirements will be from the NRC, and perform limited subsystems performance evaluations.

Again, for future, the SR Rev. 1, which would come after the consideration hearings, they would respond to the comments on Rev. 00 of the TSPA document. We'd revised those analyses with applicable changes in models or data, as we see appropriate document why or why not, conduct a subsystem performance evaluations and conduct specific multiple barrier analyses. We'd document those results and the interpretation in accordance with regulatory acceptance criteria.

For LA, again, it would be very similar. We'd revise and incorporate their comments, make a better document, integrate the new information from site characterization, do impact analysis to see if it would have a significant impact on the total systems performance assessment to see what models had to be updated, and review the Rev. 1 if we were to move on for LA.

As you've gone through the LADS exercise yesterday, and some of the corrosion testing results this morning, you've seen that we've anticipated the changes in the design for several months, simply because it would be a better design.

The changes that were anticipated in the engineering system and components and the representation of coupled processes, we've now got different waste package and design and materials and we've altered the in-drift chemistry, as a consequence, compared to the VA based case.

System changes as a result of the changed design features that we have considered or have incorporated are the backfill, the invert, drip shield, et cetera. There's a smaller zone in the host rock that will undergo changes due to thermal effects for this lower temperature design. It isn't necessarily a low temperature design, but decidedly lower. With 81 meter drift spacing, it will make our modeling much easier, we hope.

In general, the effects on the natural systems models are expected to be minimal compared to the VA base case models, and there's quite a bit of work in progress right now, as you've seen from the PMR schedule presentation.

Now, another point that you wanted covered was how was TSPA used in the LADS exercise, and I think that Jim Blink and some of the other speakers yesterday did show you ways that it was incorporated in the LADS process for estimation of performance relative to the various design options and alternatives that were being considered.

TSPA was used in the LADS exercise to develop and refine insight about the potential for each proposed feature or alternative. The analyses were expected to estimate the change in timing and magnitude of dose rate for each design option.

The level of detail for the PA analyses for the LADS were consistent with the level of detail provided in the design concepts themselves. They were scoping in nature. They were intended to support conceptual model development for the various designs.

The PA analyses for LADS were not expected to provide the detail required for the safety case, and that will come. We will do it. And as we get more information from the various fields on corrosion and other process models to integrate, we will make the necessary changes to the TSPA.

Additional data collection and analyses will be necessary to develop a defensible representation of selected options for use in the TSPAs.

And an important point from the EDA is that all five exhibits markedly better long-term performance than the VA base case from the analyses that were conducted.

Schedules I won't cover in any great detail. We've already gone over some of those. What I'd like you to see is that it's relatively tight. We've got PMRs coming in in a flood within the next year, TSPA production by next year in July, so I guess this is almost the month of July, twelve months, a heck of a lot of work.

A good part of the analysis input to the TSPA for next year will be concluded this year, because you've got to cut off someplace, but we will continue to bring in data and do impact analyses to see what we may or may not have to change for that Rev 00 throughout the year.

And, again, the next couple of pages are the schedules. Rev 00, analysis and model reports during that period of time, and then the preliminary suitability evaluation in November of 2000. Analysis and models for Rev. 1 in 7 of 2000, final suitability evaluation in 3 of 2001.

In summary, I'll go to Page 24, TSPA SR will require that all data, models, analyses and software are under baseline control, that we assure traceability and transparency for our arguments in the development of such.

TSPA SR will have adequate, necessary and sufficient information to provide the technical basis for compliance evaluation. And I believe we've made, again, a lot of progress during the past year towards this, and my confidence level is growing decidedly.

In a timely manner, TSPA will integrate updated material and incorporate model and analysis modifications required to reflect the selected new design. And, again, that's in progress, as you've already seen from some of the work as we've directed it and prioritized it in the near term.

As recommended by the PA Peer Review Panel, the

TSPA SR will include conservative bounding analyses as appropriate. The combinations of bounding analyses and design changes for complex issues where it's not feasible to produce a more realistic model will be an approach that we use. Limited improvement in component models where such changes significant affect the overall TSPA will also be incorporated.

And I guess one way of putting it is we try to avoid unduly pessimistic bounds and assumptions, as the panel reported, by enhancements to our process model and enhancements to the TSPA models themselves.

Implementing plans to prioritize work continues with the analysis of principal factors, and work required to serve as an adequate basis for SR.

This is an important time for policy making decisions and for us in the technical areas to make technical decisions and prioritizations of our work, and I hope that this has been beneficial for you. You've learned a little bit about where we've been and where we're going, and it's addressed your questions.

If you have any questions, I'd be happy to take them.

SAGÜÉS: Thank you very much. Do we have some questions from the Board? Dr. Wong?

WONG: Jeff Wong of the Board. I have a multi-headed question. Probably Lake will answer some, and you can answer the other.

On your Slide Number 10, you talk about some of the QA issues.

TYNAN: Yes, sir.

WONG: And I'd like for you to sort of expand upon what have been some of your successes or some of your problems in addressing the QA problems and what impact that will have upon this very tight schedule. And I guess for Lake, I'd like to know what impact he believes the $50 million deficit, I guess, in your proposed budget for '99-'00 will have impact on this tight time frame, and also what impact do you suspect that rebidding of the M&O contract will have on this time frame?

TYNAN: Gee, thanks, Lake. I have a triple-headed answer.

The program and all component parts are trying to figure out exactly what our quality assurance program issues are, and identify those clearly. During the past year, a series of TIGER teams has been set up in each one of the areas, each one of the technical areas of the program, design and in the science programs, and in PA where necessary. A series of audits has been conducted. A series of informal vertical slices have been conducted by the M&O to see where issues are still open.

It was in part a question of following the procedures, and they ranged in degree of difficulty from things that were fairly serious, where traceability of our documentation was weak or absent, or whatever, but those were hopefully relatively minor, to items that were comparable to the PI bought the material on his credit card. So we have a range of simple things that deal with non-procurement issues, to fairly significant programmatic issues.

We will approach the qualification problem systematically, and have been I hope. In order to assure, there was a plethora of procedures--I won't say that twice--but several hundred. I mean it was awful. Each organization had its own set of procedures. There were governing procedures in the Department. The implementation became very cumbersome for everybody, and the analysis model reports is a group of reports that will be developed in response to a program effort to consolidate procedures in those areas where analyses and models are done, and they have one procedure govern for all areas of the program, the conduct of that work, to make the auditing easier, to standardize the approach to documentation, and to assure traceability.

I've been extremely pleased with PA because they've been in the forefront of this in the workshops trying to assure that that is put into the planning phase up front. So since last November when the analysis procedure was implemented, they've been planning the plan in accordance with anticipated procedure, and implementing the work, either in accordance with the newly implemented procedures, or with anticipated coming procedures.

The impact of the budget, I think I'll leave to Lake, but some of the implementation of the program could be impacted by a severe cut, if we experience one. The TIGER teams are coming close to the conclusion of their work. There's some additional work that they'll have to do for the PMRs themselves, what data was actually used in those analyses, what have we really got to qualify for the licensing case.

A lot of items have been collected that can be used to support, or we can use different ways to integrate that in as support material, but it may not necessarily have to be formally qualified.

So everybody has I think for the past three months been in an uproar trying to figure out what it is that they absolutely have to have, and what direction they're going to take, and we'll have those plans in place, and we will have implemented a good part of the initiation of the qualification process in several of the PMRs during the next four months.

WONG: So when do you expect to have closure on all those issues?

TYNAN: I would put it to you from my perspective for the SR Rev 00 for TSPA, there are a couple of process models that could have a majority of the material qualified for that time. By the time we get to the final SR revisions, it would be DOE's hope that the vast majority of our datasets are qualified, and by the time we go to licensing, there's no question that that will be done. And it's a very high priority, even compared to the initiation of new work in some areas.

It's more important to fix what we've got, so that we can validate the models and datasets and analyses that we have before we move forward and mess ourselves up more.

We're in fairly decent shape at this time. Now, Lake, I'm going to make one more clarifying point.

BARRETT: You're doing fine.

TYNAN: The database, as has been heard by almost everybody, almost everything in our technical basis documents produced for the TSPA were labeled TBV, and they've gone through that--that's to be verified--they've gone through that. It was a policy glitch. We just said we're not certain about some of it, so let's make it all this way. And as we go through the validation exercise with the PVAR validation process, we'll begin to switch the switches back to QA. We want to make sure we had all our I's dotted and T's crossed before we do that, and that's a wise thing, a prudent thing before we enter the licensing arena. I really want to see that done, and I think everybody in DOE does, and I think you do. Go ahead.

BARRETT: I think Mark gave a good example of where we are. We have several number one priorities that we're working simultaneously. We know that we must have world class science done extensively as we can. We also know for license application, we must have documented processes and documentation that we followed that, and it all must be verified. And we have to sort out, you know, minor problems on procurement versus major data uncertainties, so that has to all be cleared up for the LA.

As we are struggling under a constrained budget, we have to balance between energies on process, on documentation of the processes, on starting new scientific work, and confirming old scientific work and balance all of these together, and that's what Mark and Steve and thee whole team are doing.

We don't know yet with a $50 million cut, how this is all going to come out. We don't know what slips, what doesn't slip. We know some things are going to slip. We have to look to see what's necessary and sufficient for that stage. For example, in the draft SR, you could have more TBVs, not TBDs, "to be determineds", but "to be verifieds," and we know we must clear the to be verifieds before a license application. And that's why I expect the license application would probably slip before the other, but I think we'd like to get a national decision, do we or don't we have a suitable site, as soon as we can, but not before we have adequate science to sustain that as it relates to the matter of degree of uncertainty, which the Board is very much attuned to.

So, I mean, that's kind of the forces that are going on, and there is no answer today what it is, and we're going through that this afternoon in some detail, and continuing on.

SAGÜÉS: Dr. Cohon?

COHON: I'm especially interested in what results will be generated with TSPA, and how those results will be used to support the SR. On Page 5 of your presentation, you're talking about the Peer Review Panel. They mention, and I guess you agree, that the focus for TSPA SR is expected performance and reasonable assurance. We all know, and you know better than any of us, that there is a great range in performance here. The error bands will be large, uncertainty will be important. How do you plan on quantifying uncertainty and how do you plan on presenting that to policy makers and DOE and to people outside of DOE?

TYNAN: I think what I'm going to do on that one is let Bob Andrews address it. But before he does, I'd say that I think he could probably handle that best. In the VA base case presentation documentation, and in the support material, in most cases we discussed the associated uncertainties, and how uncertain are we about the uncertainties, I'll not go beyond with that, but I would think that the SR document would be comparable in treatment to what we saw in the VA. And the more robust sensitivities and other things will be added in.

BARRETT: Lake Barrett, DOE. Let me add a little on that. In the development of the EPA standard, and the NRS Part 63, there is this issue about the historical EPA of reasonable expectation, the NRC historical reasonable assurance, and how do those translate into our TSPA base and our projecting into the future.

The standards will have numerical criteria, say at 10,000 years. The EPA may have other numbers, you know. And now how does that fit in, and how do you turn the probabilistic analyses in TSPA which have uncertainties, how do those turn into a go/no go criteria? In the SR, we would compare the performance of Yucca Mountain as our TSPA tells us what the performance is, against the EPA and NRC criteria plus, and how is that interpreted.

There was some discussion Monday. The National Academy of Science's Board had a meeting Monday and this was discussed with EPA and NRC, and I was there for DOE, and this was discussed. And there were discussions about is it the mean, and how do you handle the uncertainties. Bob Budnitz was there on the phone, and Chris Whipple, who were both on the Peer Review Panel, John Ahern, and there was a lively discussion, Roger Casperson (phonetic), about what does this mean.

So they were wrestling with exactly that, and my non-statistical view of that, and I'm terrible at statistics, was that the reasonable expectation and reasonable assurance were starting to sound like the mean values that would be used, but there was no conclusion. And there was a comment kind of made to the NRC and also to the EPA, that whatever the regs are ought to be fairly explicit for DOE, that everyone would know kind of what was meant by reasonable assurance and reasonable expectation in TSPA space, so that there isn't any societal misunderstandings when we go into site recommendation decisions and licensing decisions in the future. I don't know if that clouds it up or helps, but--

COHON: No, it doesn't cloud it up. I think it helps in what is a cloudy issue.

This might be, Lake, exactly what--or one important distinction between SR and LA. One could discuss and question the wisdom of having a standard for LA which is only expected value. Let's put that aside. But SR is not LA. As you've observed in your presentations before, SR is inherently a political process that will include anybody who has anything to say about this site.

In that kind of process, being very clear about the range of values, the uncertainty, quantifying it, not just characterizing or discussing it, as was the case in VA, but quantifying it and figuring out how to convey that to non-technical people, policy makers and public, I think will be very important. And I don't think it will be enough to focus just on mean performance. I think that will mask a set of issues that are fundamental to suitability, to SR, which may or may not carry forward to LA.

I have another question, if I may, Alberto. On Page 20, I think, the last bullet there talks about additional data collection and/or analyses necessary to develop defensible representation of selected options. Two questions about--or two requests on this. One, if you could expand a bit on data collection, what are we talking about? What kinds of data collection, about what? And discuss what we mean by defensible; how do you measure defensibility or how would we know that we have a defensible representation?

So what kind of data collection and how do you define defensibility?

TYNAN: Well, we're in the process right now of trying to make a decision on what design we'll use, and it's been strongly suggested EDA II would be the way to go. Several aspects of the EDA II design probably require us to look at them in considerable detail. So not only for the design-related aspects, but also the natural systems, what do we require for the process models, such that it is defensible, and I'll define it for you. If it's not a good story and we haven't got data to back it up and we haven't used reasonable bounds and we can't demonstrate why those are reasonable bounds, then that's not a very defensible argument. I take it from the opposite direction rather than a definition.

For the year 2000 and beyond, that testing is still being finalized and negotiated with the M&O, so at this point, I can tell you some of the ongoing tests related to the Richard's Barrier would be utilized, I would hope, for analysis of backfill, inclusion of backfill, exclusion of backfill, the role that the backfill could play in insulation of the waste package, and its long-term performance effects, and ongoing tests with approximately 18,000 coupons--that's my favorite number for the program--is that a lot of metallurgical type tests ongoing in the program, that Mr. Farmer has talked about, and the additional data that would be collected from the Nye County wells to augment out SZ program.

We expect also in the future to do an alluvial complex testing program to add to the saturated zone understanding, and to help defend our model, or make a better model.

For TSPA itself, it's impacted by everything else in the program. So where a piece of data that's an ugly fact or a beautiful fact comes up, we can bring that into the TSPA process and change those models. And defensibility, again, I won't go back to define it, but in the TSPA realm, the technical basis for the TSPA analysis had better be defensible in the technical sense and in the quality assurance sense.

SAGÜÉS: Okay, thank you. We're running short of time. We have a few more questions. Dan Bullen, Leon Reiter and Dick Parizek.

BULLEN: Bullen, Board. Mark, you'll probably regret putting asterisks on viewgraphs, but I was going to ask you about this one. You noted that all EDAs exhibited better performance than the VA base case, but all EDAs had titanium drip shields. If you put a titanium drip shield on the VA, how does it compare?

TYNAN: That would be wonderful, too.

BULLEN: I know, but you're going to defer. So the comparison isn't a fair one, and actually the comparison that you make in your backup slides isn't a fair one. If you wanted to take a look at that kind of performance, you should do an apples to apples comparison, as opposed to an apples to orange comparison.

TYNAN: I agree. I agree.
BULLEN: That was my comment on this one.
TYNAN: But I think if we threw out the drip shield, they would still be better.
BULLEN: You might be hard pressed.
TYNAN: Some of them, EDA II especially.
BULLEN: Well, that's true. The next viewgraph is actually the one I have a quick question on, and you can provide a little bit more explanation. The integrated site model PMR, can you tell us--I mean, we heard a little bit about that it's an all encompassing PMR and all the other PMRs kind of feed into it. Could you tell us a little bit more about what the ISM/PMR is and how it works?
TYNAN: Let's make sure I didn't make that mistake. The ISM is an important model because everybody below it has to use it, and the integrated site model is a trash basket name because people like the word "integrated."
BULLEN: I guess the question I had was it includes both the natural system and engineered layout design, et cetera?
TYNAN: No, it will not. No, what this doesn't show is the feeds to the design side of the house, the direct feed. And the design group maintains that the ISM covers an area from roughly the Prow down to Busted Butte. The design, detailed design stratigraphic model that's used and transported all around the design organization for imposing

the repository on the rock is a much smaller geographic area. But in order to assure consistency, as the geologic model is produced for utilization in the UZ, and then incorporation in the SZ flow and transport, and other site area related features, the design group has translated that into the volcanic program, and then builds their detailed model, which we're trying to assure consistency from model to model to model.

And then also, the ISM in terms of the rock properties, that those rock properties that are produced there, are consistently used either in some sort of an abstraction form throughout model to model to model, and that when you go and check from our ISM to the other flow and transport areas, are designed that you can go back to ISM and make sure that there is agreement, technical agreement, technical inputs are similar, the documentation is there, et cetera.

BULLEN: Okay, thank you.

SAGÜÉS: Very good. Leon Reiter?

REITER: Yes, this is a question directed towards Abe, and Mark raised the issue of the criticism review of the TSPA LA and also of the use in the LADS. If I remember correctly, both the Peer Review Panel and to some extent the Board found both conservative and non-conservative--or potentially non-conservative elements in TSPA VA/LA, and felt uneasy in classifying the TSPA VA as being conservative or non-conservative. And I think, Abe, at the January meeting, we asked you specifically what are the things that TSPA VA is good for and not good for, and one of the things I think you said was it's not good for judging compliance.

Now, yesterday, we heard all kinds of comments about how the TSPA VA shows this wonderful, superb designs. What has happened since that time to allow us to make these kinds of judgments, or maybe the judgments should be more considered?

VAN LUIK: For a person of my age who doesn't remember what he says from one meeting to another, this is very difficult. This is Abe Van Luik, DOE.

What I meant when I said that this was not to be used to judge compliance is, first of all, we don't have a compliance line to judge ourselves by, an official one. And, secondly, we knew that the QA and the technical defensibility were not quite there to make a licensing type case. That's where I was coming from.

The VA shows a very low dose mean value for 10,000 years. That was encouraging. The TSPA VA basis was used to judge the relative merits of gross portions of the different design options that were being considered in LADS. And I think to the extent that it was just a pointer or an indicator of relative merits, that it was fine to use it that way. To go any further in detail in judging the details of designs would have been improper, because as the Board has pointed out, and as we're very well aware ourselves, some of the coupled models that would have to be invoked to look at the nuances of differences between these designs were just not in the TSPA model.

So I think the VA tells a very nice story, a defensible story. You know, conservatism is in the eye of the beholder also. We believe that we were either realistic or conservative, whereas the judgment of others reading the document was that in some places, we were non-conservative. And I think that we readily admit that in some areas, in retrospect, it turns out that it may not have been as conservative as we thought it was. But this is part of the growth process, and this is part of the learning process for doing the SR in a more defensible and more transparent way.

And we've also gotten the message from the State of Nevada, Steve Frishman, that just because you're transparent, doesn't mean you're defensible. You have to have a basis. And so the basis is very important also. So we do listen and learn, but I think at the same time, even though historically we're stepping away from the VA, in my opinion, TSPA VA is a very nice piece of work that for the first time integrates every aspect of this program and, you know, is a giant step forward to SR and LA.

SAGÜÉS: I'm going to have to limit at that. We're going to have just one very brief question before the break.

PARIZEK: Parizek, Board, and it has to do with Figure 21. It's the time schedule between the PMRs and then the blue and green boxes. If you take Nye County Drilling, which really the first round of drilling has just been completed, the testing is not all done on those wells yet, assuming funding is there for the second and third round, it's a three year program. To get to the saturated zone, the six month 00, then to go on into the, you know, the time schedule, it seems to me you're not going to have all of that saturated zone material in there. The inter-agency regional groundwater flow model is a five year effort; I guess it's the second year of five years. Again, that might not be up to speed.

So how does the saturated zone box in there fit in there if it's still incomplete? There must be other examples like this. Maybe it's not necessary to answer that now. We'll learn more about the time schedule this afternoon, I guess, on the Nye County work. But it's troublesome to me about the time schedule here, of getting the work done to have a credible saturated zone model, to get the benefit of what you're going to get out of the saturated zone for TSPA, and then site recommendation.

SAGÜÉS: I guess that was more of a comment than a question, Richard.

TYNAN: I'm very grateful. Thank you.

SAGÜÉS: Thank you very much. We will adjourn and return now at 10:00 a.m. punctually. Thank you.

(Whereupon, a break was taken.)

KNOPMAN: I'll turn the gavel over to my colleague, Don Runnells, who will be co-chairing this next and last session with Dick Parizek on Geochemistry and Hydrology.

RUNNELLS: Thank you, Debra. I'm Don Runnells. Dick Parizek and I will share the chairing of this session, and I'll take the first section, and then after lunch, Dick will pick up the second half.

We're going to shift gears here a bit, and start to look again at the natural system. The last day, day and a quarter, have been devoted pretty much to the repository design and the waste package, but there's still a great deal of interest in the natural system, in particular, the movement of moisture, the age dating issues, the composition of water, and in that context, how the composition of groundwater relates to the regional hydrology.

We'll also hear about the Nye County drilling program, its status and relationship both to geochemistry and to regional hydrology. So we have quite a lot to cover, and I'd like to go ahead and get started.

Our first speak, Mark Peters from Los Alamos, will give us an overview of the scientific program itself, and as you can see from the bullet lists there on the agenda, a number of items will be covered that are of considerable interest to the Board and to other people in the community.

So if Mark is here, we can go ahead and get started.

PETERS: It's nice to be here again. Today, I'm going to go through a whirlwind tour of the status of a lot of our testing activities. There's going to be the conspicuous absence of thermal testing, which you heard about yesterday from Debby Barr, and Chlorine 36 and Busted Butte, because Paul Dixon will talk about that later. I have a lot of material to cover. I'm going to hit things that are pretty high level. There's quite a bit of time for questions, so I'll probably--I'm sure you all will have more detailed questions as I go through the whirlwind tour.

I'm going to try to hit a lot of the items that were discussed prior to the meeting that the Board was interested in hearing about, and I also added some things that I thought the Board might be interested in. I'll talk some about moisture monitoring and the ongoing work in the ESF and the cross drift, Alcove 1 and 7 in the cross drift program, the ESF niche studies which are done in the Topopah Spring, the middle non-lithophysal unit, an overview of the status on the Chlorine 36 validation study. I know there's some interest in the status of that. Another short status on the cooperative work on fluid inclusions that's being done with DOE and the State and UNLV, then get into some of the status on the cross drift, the mapping and the alcove studies and where we're at there.

I'll talk about the steep hydraulic gradient and some of the results from WT-24, give a brief status of SD-6. We've actually done some things at SD-6 since we talked in January. Give a brief overview of what we're doing with Nye County, without stealing Nye County's thunder in the next presentation, we'll give a very detailed presentation of what Nye County has found to date, as well as their plans for next year. So I'm not going to go into those details.

And then following on from January, I gave you a couple slides on the EBS pilot-scale testing program that's going on over at Atlas, and so I went ahead and added the status on that, and that will tie back to a lot of the discussions on LADS that have been going on the last day or so.

At the end of the talk, I will bring in how we're in the process of prioritizing our testing program, talk briefly about the process model reports. I won't go into any detail; that was really discussed in the previous talk. And then talk a little bit about our plans right now in terms of long-term testing and performance confirmation.

So first, the testing update. I'd remind everybody this is just a schematic of the ESF. You have the ESF main loop. Here's the potential repository block, and then you have the cross drift, which has now completed excavation across the block, across the block but above the potential repository. A reminder: all of these alcoves and niches that you see in the ESF are constructed, and there's either ongoing testing or testing is complete.

Right now, in the cross drift, all we have is the cross drift. We're in the process of starting the construction phase for a lot of the alcoves and niches. I'll talk today mainly about Alcove 1, Alcove 7, as well as results from Niche 2, and some preliminary results from Niche 3.

First, Alcove 1. This is more of an update. You all heard about this in January. Again, the purpose is to evaluate infiltration and percolation through the UZ above Alcove 1. Remember, we've got an infiltration plot, and I have a diagram later. We have an infiltration plot almost 30 meters above the crown of Alcove 1. We're introducing a known amount of water, and we're looking for how much water seeps into the opening and how that water is flowing through the fractured rock of the Tiva Canyon. We're also evaluating the climatic effects associated with increased precipitation, and that's by varying the flux and really over driving the system.

A reminder on the Phase 1, which was really done primarily last fiscal year. We introduced over 60,000 gallons of water at the surface. It was all traced with lithium bromide. So we were able to, we saw first water entering the opening in Alcove 1 after about eight and a half weeks after we'd applied over 30,000 gallons of water, and overall, approximately 10 per cent of that water that we applied at the surface actually entered the opening.

Let me back up. We actually did a series of predictions, blind predictions, LBL did a series of blind predictions in conjunction with the USGS PIs on when we thought we would see first arrival. And the initial models actually predicted we would see first arrival much faster than we actually did. But the adjustable parameter there is the fracture porosity. So as you change fracture porosity, you were actually able to get much closer to within the range of when we saw first arrival.

Similar for Phase 2. For Phase 2, we've just started water application in February. Those numbers are as of mid May. We've continued to infiltrate, so we're well above 30,000 gallons of water applied. And you can see that we're definitely applying a tremendous amount of water, seven years of average annual precipitation. This time, we saw seepage first time in three weeks, so there was what I'll call a hysteresis effect. The fractures were still wet from the first phase, so you saw initiation of fracture flow much faster in the second phase. But, again, we're still getting that magic 10 per cent of the applied water seems to be entering the opening.

Again, we were using lithium bromide traced water. We're also varying the infiltration rate now, so we're looking for sensitivities in terms of infiltration rate, but we're also starting to add more lithium bromide to start looking at whether we see sluds of higher concentrations entering the opening. We haven't seen anything yet. And we're also going to start introducing more tracers to further evaluate the flow and transport phenomena, and there is also a suite of predictions for that that LBL has done. Once we do that, we'll then compare that to the predictions.

This is just to remind you this is a plan view of the north ramp. This is where you walk in. Alcove 1 is that first alcove. At about almost 30 meters above, there's an infiltration plot at the surface, and for those who have been out there, that's that--it depends on when you're there, but right now, it's a gray tarp. Underneath that is the sprinkler system that we're introducing that water, and it's bigger than the plan view of the alcove, and here's just an idea of the scale here. The hill goes up above. This is the drainage ditch of the portal.

There's also some data plots in your backup, two data plots in your backup from Alcove 1. But this shows, for the second phase here, again, it started in February, on this axis plotted in blue is the cumulative amount of water applied. I apologize for the non SI units. And also in the red is the cumulative seepage, how much water we've collected within the alcove. And, again, here's that magic approximately 10 per cent.

Alcove 7. As you know, we've had a bulkhead-- Alcove 7 is where we excavated across the Ghost Dance Fault, we've done that at Alcove 6 and Alcove 7. Alcove 7, we actually constructed two bulkheads to isolate really more than the back half of the alcove, and just watched it, had it instrumented and watched it return to ambient conditions. This was initiated about the same time as the Alcove 1 test, during the 1998 El Nino year.

What we've seen so far, I don't really have any data, the rock returned to ambient conditions, and what I mean by ambient is greater than 99 per cent relative humidity, within a month. We've also got instrumentation in there to try to visualize, or within the rock at least see drips, and we've seen no evidence of any dripping from the rock into the opening. That test is ongoing. We periodically go in, check the instrumentation, check the drip cloths to see if we see any evidence.

NELSON: How long has that been going?

PETERS: How long has that been going? About a year and a half now, if I've got my times right, around that. Over a year. And right now, we are continuing that.

The cross drift; these observations in the cross drift are based on moisture monitoring holes in the cross drift, so we've got hydrologic instrumentation in the cross drift, and we're looking for effects of evaporation and looking at water balance. You've heard a lot of this before. This is just to remind you of what you heard in January.

Construction water use. In terms of the different response of the different units within the cross drift, remember in the cross drift, we're exposing parts of the Topopah that we've seen in the ESF, but also parts of the Topopah that we did not. So we're seeing the upper lith, the middle non-lith, the lower lith, the lower non-lith. And remember, the repository is primarily in the lower lith.

The observations are we see construction water was observed more than 30 meters within the middle non-lith, and less than 2 meters in the upper lith. That's really driven by the fracture density, the through-going fracture density. The middle non-lith has many more larger fractures in terms of fracture length.

When you do the water balance, about half the construction water was actually lost to the fracture network, but overall, we have a net drying of the cross drift due to ventilation--due to excavation and ventilation.

In terms of additional observations, we're still seeing the drying front migrate away from the excavation due to ventilation. In the ESF, it's more than 2 meters. I think that was discussed a little bit yesterday. In the cross drift, it's actually approaching 2 meters right now. That will continue to migrate further into the rock, so we're having a net removal of water from the cross drift, and we talked some about the implications that might have yesterday.

And this bullet here about the response varying in terms of drying, I'll get to that in one of the data plots. You'll see how there's a differential drying, depending upon which unit you're in in the Topopah.

I'll also get to this bullet where the water potential measurements that we're doing using heat dissipation probes in the cross drift are relatively uniform across all the sub-units, and are higher, the water potentials tend to be higher than observed previously. But, again, that's using a set of instruments that haven't been used to do some of the other measurements. I'll talk about some of those implications for that. But we are in the process of doing some additional investigations to evaluate the importance of those higher water potentials in the cross drift, including doing some work on using two different sets of instruments that measure the same thing to see if we get the same answer. I'll put it simply.

Some examples of some data. This is from a nest of heat dissipation probe holes. This is in the ECRB at construction station 23+50. That is in the lower non-lithophysal unit, almost out to the Solitario Canyon Fault.

What you've got is you've got four boreholes with a heat dissipation probe at the bottom of the holes. So you have a 30 centimeter hole, et cetera, on up to a one and a half meter hole. The data plotted here is a function of time, water potential in bars, so as you get wetter, you get closer to zero. You can see the effects of the drying. The deeper hole, it takes a while to incorporate, once it incorporates, it's relatively flat, and here's that nearly between minus 1 and minus a half a bar. These are minus bars.

So you can see the most shallow hole is showing this really steady drying. You never even see any ambient conditions in this hole. By the time it incorporates, the drying front has already passed. And similarly, you're seeing progressive drying sort of in the close to meter range. The drying front is past there. And there's a hint of drying here. Most of the heat dissipation probe boreholes in the cross drift are 2 meters depth. They were drilled at that depth to try to get at the ambient conditions prior to the drying front passing.

This gets back to the differential response. What this is is, again, heat dissipation probe data, water potential and minus bars as a function of time along the ECRB. I've got the nomenclature here for the Topopah sub-units, upper lith, middle non-lith, lower lith, and lower non-lith. This is as a function of time.

This gets at what I was talking about about the fairly uniform ambient water potentials, but you can see that as we've progressed in time, we've got preferential drying, particularly in the middle non-lith where it's highly fractured with long fracture lengths. These are all 2 meter depth boreholes. You can see some heterogeneity, but you see preferential drying. You can see that the upper lith and the lower lith tend to still be close to what we would consider ambient water potentials based on the heat dissipation probe again.

But, again, we are going about, we've installed some thermal couple psychrometers, which is an alternative way of measuring water potential, in some holes right next to some of these heat dissipation probes to confirm that the water potentials that we see ambient in the cross drift are, in fact, what we're really seeing. So those have been instrumented behind the bulkheads, and I'll get to the bulkheads in a minute.

The ESF niche studies. Again, the ESF niche studies, we've constructed four niches. Lawrence Berkeley has done a series of niche seepage tests in those niches. These are all in the middle non-lithophysal unit.

Just to reiterate, the purpose is to evaluate drift-scale seepage processes and seepage threshold. We've measured seepage threshold fluxes at Niche 2. Niche 2, I pointed it out in the earlier diagram, but it's down by the Sundance Fault, down towards Alcove 6.

We saw a capillary barrier forming and we saw what I call fracture wetting history effects, meaning when we first started liquid release tests, we didn't initiate fracture flow immediately, but you saw hysteresis, so when you did the second test, you initiated fracture flow much faster because you'd wetted up the fractures.

Also, we do air permeability, both before and after excavation at the niches, and we saw an air permeability increase in the near-field after excavation at both niches.

Some of you have heard Rob Trautz of LBL, or maybe Bo, talk about some of this work. But before we go in and do the excavation, we actually inject dye, and as we're excavating, we take samples to see where the water has travelled. In the fracture system at Niche 3, and let me back up, Niche 3 is up closer to Alcove 5 and sort of more what I'll call run-of-the-mill middle non-lith. Down by

Niche 2, you're much more closer to the Sundance Fault zone. But at Niche 3, we saw dye travel about 1.2 meters below the release point, whereas, at Niche 2, it travelled about twice as far.

And we're in the process, Niche 2 testing is really winding down, we're in the process of doing liquid release tests at Niche 3 right now, and are focusing again on determining seepage threshold to compare the results from Niche 2. And there's some results for Niche 2 in the next slide.

This is actual data, saturated hydraulic conductivity on the Y, versus seepage threshold flux. What I mean by that is, and first let me describe what I mean by the two colored symbols. This is actual field measures from liquid release tests. Fracture network is where you have a combination of high and low angle fractures, and then high angle fractures is just what it says. So we have a series of boreholes and we're releases at different intervals, and we've characterized the fractures and then grouped them into those two broad areas.

In general, what's done is you basically start at a very high infiltration rate, and you basically do a series of tests, marching down until you reach a so-called threshold where you see drips into the opening. So what I'm plotting here, as you can see, the flux, at least at Niche 2, the results are much greater than 100 millimeters per year. You have to get to fluxes--let me back up. Any flux up to this point, you wouldn't expect to see any drips into the opening. Okay? So this is real important to performance. If you can demonstrate the threshold flux is very high, and you could have significant flux through the repository horizon and still not get any drips in the opening, so that's a very important part of the natural system performance. And we're in the process of trying to define that better.

A brief update on the Chlorine 36 validation study. You're aware of the work that's been done by June Fabryka-Martin and co-workers at Los Alamos over the years in the ESF and now in the ECRB. There's an ongoing independent validation effort going on where we're attempting to validate the occurrence of bomb-pulse Chlorine 36 at two locations that we saw in the ESF, namely we chose the Sundance and the Drill Hole Wash Fault zones.

This is a joint effort involving USGS, and you can see the list down there, Livermore, Los Alamos is involved, AECL, and then the Accelerator Facility at Purdue is still involved as well. We're drilling 50 boreholes, mostly 6 meters deep. There's two that are 10 meters deep. And we're collecting core, they're dry drilled, 40 at the Sundance, 10 at Drill Hole Wash. We'll take those samples and conduct the suite of analyses that you listed, chloride, Chlorine 36, Tritium, U series, as well a Technetium 99. And also in talking to Zel earlier, we're exploring possible Iodine 129 as well.

In terms of the status, we're drilling these as we speak. As of early June, a couple weeks ago, we had finished 20 boreholes at the Sundance and there had been a core party at the SMF where they had taken some initial samples. As I said, we are drilling, and that's supposed to be finished by August, and the analyses are ongoing. I don't have any data to report today, but the initial Chlorine 36 and U series work should be available by mid July.

Cooperative work on fluid inclusions. This is related to some of the issues of, you know, alternative interpretations of the fluid inclusion occurrences that you see in some of the fracture minerals in the ESF and otherwise. There's a cooperative study ongoing, UNLV, the Department and State of Nevada. Right now, all we've really done to date is we spent a tremendous amount of time taking samples in the ESF and the cross drift, not only within the tunnels themselves, but in the alcoves.

The way it's working is there's quarterly meetings where all the technical people get together and discuss what they're seeing in the rocks, actually sit there with the microscope and look at the fluid inclusions. They had a preliminary kickoff meeting in April, and they just had another meeting a week ago. I think it's probably premature to--I'd say next meeting, it would be good to have an update on that. But really, it's too early to really say a whole lot about the results from that, but we are sampling. We finished sampling and we've taken more than 150 samples at this point.

Now, the cross drift. Just to remind you, you've heard about the work that's gone on in the ESF, Zel Peterman and co-workers looking at fracture minerals and trying to get an integrated picture of the long-term percolation flux through the repository horizon. That work is ongoing in the ECRB and the cross drift. This is just a smattering of what they've done to date.

They've done some line surveys like they did in the ESF to determine the spatial distribution and abundance of the deposits. They've done a significant amount of sampling, including some feature sampling in the Solitario Canyon Fault. And then the bottom three bullets tell you what they plan to do, akin to what they did in the ESF. Ongoing analysis right now, I can't really say much about the analysis, but the one thing we can say is the occurrences tend to be very similar to what you see in the ESF, regardless of what unit you're in in the Topopah.

Ongoing mapping work in the cross drift. As you know, the Bureau has mapped the cross drift as we were excavating, and they're in the process of finalizing the report for those mapping results. But there's also an additional activity going on right now, which we call the small-scale fracture study. As we were driving through the cross drift, we noticed--remember when the Bureau maps the first pass, they use a fracture length cutoff of a meter. Anything greater than a meter, they map. As we were going particularly through the lower lith, we noticed a lot of smaller fractures that we weren't mapping. So they've gone back in and done six traverses, horizontal traverses about 6 meters long, with some vertical traverses associated with it to characterize the fractures that are down to 4 centimeters.

This is the location of the traverses. The construction station on the left, that's the meters, so the first one is, for example, 1115 meters from the start of the cross drift, with the lithostratigraphic unit, and then the middle non-lith. So we have two traverses in the middle non-lith, three in the lower lith, and one in the lower non-lith.

This just shows you the same thing in plan view. This is the cross drift coming across. The black lines show where the contacts are between the different units as exposed in the tunnel, and then the locations of the six traverses.

This is actually some data. What we've got here is, again, we've got as a function of construction stations, so here's the four units, here's the Solitario Canyon Fault, and again as you cross over the Solitario Canyon Fault, you go back up in the section, all the way up to the upper lith. This is fractures per 10 meters, so it's fracture frequency plotted along the Y axis.

The actual observations from their initial mapping where they had a fracture length cutoff of a meter are shown in red. So you can see the lower lith and the upper lith fracture densities are very low, close to zero. Whereas, in the middle non, you get upwards of ten fractures per 10 meters greater than a meter length.

What's shown in the purple is an actual predicted frequency that Chris Rautman of Sandia did based on the raw quality data that the Bureau collected in the cross drifts. You can see that that would predict that you would get a much different distribution of fracture frequency as you walk through the cross drift.

This small-scale fracturing study is addressing that issue, and on the bottom here is a preliminary observation based on what the Bureau is finding. They're seeing that regardless of unit, they get around 150 to 305 per 10 meters, regardless of unit, when you go down to 4 centimeter cutoff, which is, as you can see, much more in line with what you would predict from the RQD.

I know there's a lot of interest in what's going on in the alcoves and niches. The next slide will have a--maybe go to the next slide, and then we'll go back. This gives you the detail of the cross drift. You've seen this before. Some of the alcoves and niches planned have move around a construction station. Cross drift running out here. We stopped the TBM right around in here after we had crossed the main splay of the Solitario Canyon Fault.

There's ongoing work in there right now. We discussed at the January meeting the prospect of actually bulkheading off part of the cross drift. That was raised by the Board. It's also been discussed by the NRC. We, in fact, have gone and done that, and as of last Wednesday, the bulkheads were closed.

So what we've done is we put a bulkhead right here at about 1750 meters from the opening, just before you get underneath the crest, the high infiltration area at the crest, and also an additional bulkhead right before the main splay of the Solitario Canyon Fault, about 2500 meters out. Those have been closed. We're not ventilating behind there, and we've got the instrumentation run to fiberoptic, and we're collecting data ongoing to see how the system returns to ambient, and get a feel for the difference in hydrologic response as we move across that part of the cross drift.

Also, what's planned in the immediate future, is we're preparing to start excavating the cross-over alcove. That starts its way in the upper lith. Remember, that goes out over top of ESF Niche 3, and there we're doing a flow and transport test. We're introducing a known amount of water in the bottom of the cross-over alcove, and then we're going to see how it flows through the upper lith, and also address seepage issues underlying Niche 3. Remember that it starts at the upper lith, but because of the different units, you actually enter the middle non about 5, 8 meters below the cross-over alcove. So we're actually look at two stratigraphic units there.

So, again, that drill and blast excavation is scheduled to start here in the next--probably in the next three or four weeks. Testing would continue in 00, according to current plan.

Following that, the current plan would call for moving to Niche 5. That is a seepage test in the lower lithophysal unit. Again, remember the lower lithophysal, you pick up the lower lithophysal unit right about here as you're walking down the cross drift. So this would be a seepage test akin to the ESF test, but again in that lower lith unit, which makes up the majority of the potential repository.

And following that, the plan would be to move to the cross drift thermal alcove, and do a smaller thermal test in the lower lith to complement what's going on in the drift scale test. But, again, these are all according to the current plan. I'll get to some of the caveats on that, and you've heard some of that already.

I've already said most of this. This gives you a detailed status on the things that are in the immediate work scope out there at the ECRB, the bulkheads, the cross-over alcove and Niche 5. I won't go back through that.

This is a schematic of the cross-over alcove. Again, off the left rib of the cross drift, and then below here is the ESF Niche 3, we'll have a series of vertical boreholes coming down, and also up from Niche 3, for observing the wetting front, and also we'll put an infiltration plot in the floor of Alcove 8 and introduce a known amount of water.

COHON: What's that distance again?

PETERS: It's about 15, 20 meters. 15 meters. Actually, closer to 18.

This is a schematic of what a niche will look like in the cross drift. Remember, in the ESF, some of you have seen the niches, they're just short 10 meters drives off of the main there in the ESF. Here, in the ECRB, we're actually going to excavate an access drift off of that, and then the actual testing niche will be at the back end. So we'll still do these pre-niche excavation boreholes, as well as some radial boreholes for long-term testing. Same concept, liquid release from these upper holes, look for seepage in the niche itself.

Continuing in sort of random walk through the testing activities, I'll move to the surface, the surface based testing program in the saturated zone. The steep hydraulic gradient. As you heard in January, we deferred any further drilling at WT-24 unless we deemed it necessary to meet PA needs in support of SR and LA. But we can say--and that's been done, but we can say from the results from 24 and earlier testing, there are some important constraints that we can make on the steep hydraulic gradient.

We did encounter the regional potentiometric surface close to the bottom of 24, and we did see above that, perched water above the regional water table in 24, as we've seen in some of the other holes up in that area. And we know that's perched. That's based on hydrochemistry and some other constraints.

So what can we say to date? Right now, the favored hypothesis is that the gradient does exist north of the potential repository, but it's probably not as steep as we once thought, and the condition that causes that gradient may actually tend to divert some of the saturated zone flow eastward around the potential repository, and down Fortymile Wash or Midway Valley, along some major structural features.

This is probably out of place, but just to remind you with WT-24, it looks like a lot of colors, the main point is that we TDd WT-24 in the Calico Hills. It was a relatively tight portion of the Calico Hills, and we were unable to get a reasonable pump test.

SD-6; in January, I told you we had steel stuck in the bottom of the hole. We weren't sure what we were going to do. We had never TDd that hole. We were stopped at about 2500 feet. We had just hit the water table. We have since decided to go ahead and use a whipstock technique. We diverted around that stuck steel. Instead of going in and trying to fish it out, we went around it, and we TDd the hole about a month ago, and we've just finished the pump tests there at SD-6.

We did a series of short pump tests, then we did a two week pump test. We were able to pump about 16 gallons a minute for a couple weeks. We TDd that down to the Bull Frog. We actually were in the Bull Frog . That C-well, that actually is the producer. Up here, it was still a relatively low producer, but we were able to sustain a pump test.

I can't say a lot about the pump test right now. They're still analyzing it. I'd say the next update, you'll probably hear more about what we think we know about SD-6, but they are looking for not only recovery data within SD-6, but also looking at some of the local wells to see if they see any draw-down locally from a more regional perspective.

More on the saturated zone. You heard the last meeting about the cooperative work we're doing with Nye

County, and I won't dwell on this because you're going to hear a lot more about Nye County next talk.

The field work is mostly completed. We're in the process of analyzing cuttings and also analyzing water samples, and providing that information in a cooperative fashion with Nye County. The data that we're collecting is being incorporated into the project SZ flow and transport model that is being iterated in preparation for SR.

The last bullet is actually old news. Nye County next talk will tell you what they're planning on doing in terms of details for 00. The main point is is that we are involved in working with them on that planning, integrating that. The parentheses are actually in error. Things have evolved in a couple weeks, and they're doing a different number of shallow and deep.

Now, to switch gears completely from the natural system and go over to the pilot-scale testing, as you know, over at the Atlas facility in North Las Vegas, they're doing a series of engineered barrier system tests to address EBS performance. I told you about Test Canister 1 that they initiated in mid-December, where they were looking at a Richard's Barrier, medium sand over coarse sand, very high drip rates. That test is continuing, and what they have found is Richard's Barrier continues to effectively divert the water. They've been able to collect greater than 98 per cent of the water, and it hasn't actually compromised the Richard's Barrier itself.

When I talked to you in January, they were just in the process of starting the second canister. That was initiated in mid-January. That was a coarse sand backfill. The sand was the same coarse sand that they used as the bottom part of the Richard's Barrier in Canister 1. That actually--fail may be a strong word--but the mock canister actually got wet, saw water within 24 hours, and the backfill was fully saturated I believe within like a week. So that only ran for a month. They turned that off. That was, again, at very high infiltration rates.

Now, the new development that's coming out of the LADS effort, as you heard about, drip shields have become a big part of that effort. So just really two weeks ago, they initiated a third test, and I'll show you a schematic of that in the next diagram. But the concept is a drip shield, they have a crushed tuff invert with a steel mock canister, and they're going to emplace a 2 centimeter thick stainless steel drip shield over top of that, no backfill over the top of the drip shield.

Phase 1 is ongoing. What they're doing is they're heating it up. They've got a single element heater within the canister. Let's go to the next slide. This is a schematic. Here's the scale. Again, I apologize for the non

SI units. This is about four foot diameter. They have a large test canister. Inside that, they have a metal canister with a single element heater in it. This is the drip shield. Then they have guard heaters around the external part of the canister.

The goal is to maintain the canister surface at 80 degrees C., the surface of the test canister at 60 degrees C. They're in the process of doing that, and then they're going to come back in and emplace the drip shield and then start the dripping process, and they've got this instrumented. They also have the crushed tuff invert instrumented. But they're looking for phenomena like if they get, for example, condensation up under the drip shield that might drip onto the can, et cetera, those kind of phenomena that are really important to drip shield performance.

So that was quick, not a lot of detail on a lot of the things that are going on in the science program.

What about SR, and if we continue on past SR to LA, and now we're integrating with the LADS process, and you've heard a lot about we have constraints on the program, budget constraints, et cetera. We are in the process, during the planning process, to prioritize the testing program. We're linking it to the evolving safety strategy and LADS, so this is an ongoing process. I can't sit here and tell you exactly what's going to happen and what's not. We are prioritizing that list, and then it's almost a matter of where you draw the line, depending upon things that the budget cycle will do that Lake discussed yesterday.

To date, and I'll emphasize to date, this is the priorities, because again the safety case, safety strategy is evolving as we speak, but to date, these would be some of the priorities for the natural system. UZ flow and transport, finishing up some of the ESF testing, starting up some cross drift testing, and continuing Busted Butte. This is really I'm speaking for 00 here.

Seepage is going to likely be a very high priority, again, finishing up some of the ESF testing and hopefully getting in there and doing Niche 5, in particular the lower lith niche. Near-field coupled processes, and then of course SZ flow and transport, and there, the cooperative work with Nye County comes in.

A short slide on the PMRs. Mark talked a lot about the PMRs, so I won't dwell on this. An important point probably is that all the testing data that we're collecting that goes into the subsystem models and the abstractions is being documented in these analysis and model reports, which are sort of the basis for the PMRs.

And Mark I think also mentioned this. It's important to note that there's going to be testing that will continue through the revs of the PMRs, and there will be periodic feeds to those. Just because we have a feed in August doesn't mean we're going to stop work, for example, at Busted Butte. Busted Butte will continue and will feed the next rev of the AMRs and PMRs.

What about long-term testing? This kind of relates back to the prioritization and some of the things that I said were priority. Current plan, and I emphasize current plan, for long-term testing would include the drift scale test, work in the cross drift, and SZ work in cooperation with Nye County.

This is some long-term testing that actually would go past, in the current plan, would go past the SR and LA milestones. So it's all in how you define performance confirmation, but we're still looking at processes there, but we're also looking at prioritizing the performance confirmation program. In longer term, that will be linked to the TSPA sensitivity analyses and the regulatory requirements.

We're in the process of reving that plan, right now, looking like it's going to be completed in 00, and that's linked back to these same principal factors of the evolving safety case and the LADS process. So we're in the process of refining that.

So it was fast, but I hope it gave you all a feel for what we're doing.

RUNNELLS: Thank you, Mark. That was an excellent presentation, and a lot of material in a short time.

We do have time for questions from the Board. Debra?

KNOPMAN: Knopman, Board. Mark, would you elaborate a little bit more on the priority setting process for the scientific work in the near term, next six months, as well as the following year? We heard a little bit about this just in an informal discussion, but it would help to hear how the science folks are interacting with the TSPA team in identifying what needs to be done and when.

PETERS: Okay. You remember the discussion at the January meeting about Table 2.2; right, Abe? It was the sort of principal factors of the safety case as was contained in the VA. That table is being looked at, and based on the new information that we've got, and the results of the TSPA, we're updating that table to reflect our current understanding. A draft of that table was provided to the contractor by DOE, and the M&O is now in the process of looking at that and identifying the key factors that affect performance, and that will drive the prioritization. The science organizations are involved in those discussions. I'm involved in those discussions.

KNOPMAN: Is that being done quantitatively through TSPA sensitivity analysis or is there also--I was just going to ask if that's the only way in which the priorities are being set, or can you in a sense have an override from the scientific teams because of a strong opinion that that TSPA analysis is not producing a reasonable result?

PETERS: We're pushing back on it, and there's a lot of back and forth on that. There's people in the audience who can address it a lot better than I can, but they are using the TSPA VA expected value, and they're doing it, they're looking at neutralizing, they're neutralizing different barriers, and then it's a neutralization analysis, and then they're seeing that it's allowing them to identify the principal factors for performance.

Some of the things that are coming out; seepage is very important. Drip shield performance is very important. We're in there pushing back, and there's a lot of back and forth on it. It will be the key to driving the prioritization of the program.

RUNNELLS: Dan Bullen?

BULLEN: Bullen, Board. You showed us a lot of very good data inputs that will be coming available along the lines of the next few years or so. What's the absolute last chance for new data to get into the AMRs or the PMRs prior to the TSPA for site recommendation?

PETERS: First round of data, and Bob, you can correct me if I step on myself, but August, the first round for Rev.

00 comes in around August. I believe there would be an update sometime in the winter of 00.

ANDREWS: Bob Andrews of the M&O. Let me try, Dan.

As you know, the AMRs, the analysis model reports, feed on the data that Mark was just describing here, as well as software and other pieces of information, literature values, et cetera, and those then get rolled up into PMRs, get rolled up into TSPA.

We have, as Mark Tynan said, a very controlled process now to incorporate new information and evaluate the impacts of new information. But there are kind of freeze dates on data for different revisions of the TSPA. The freeze dates for Rev. 00, as Mark Tynan pointed out to you, are essentially, you know, this summer or early fall, late summer or early fall of this year. It depends on the model. Some of them come in a little bit later. And for Rev. 01, it's essentially eight and nine months after that, so next spring, next summer sort of time frame.

BULLEN: Bullen, Board. And so at that time then, the data that we've collected, for example, in the next ten months is going to be what's going to be available for TSPA SR, and post that, there will also be--I mean, if the siting recommendation continues on, there will be more inputs that will be Rev. 02 of the PMRs that you'll provide input to?

ANDREWS: Yes, there is, as Mark showed on his slide, there's a Rev. 02 of the PMRs, which essentially goes into the license application.

BULLEN: Okay.

ANDREWS: So between now and then, you can think of three revisions of data, analyses, process model reports and TSPA.

BULLEN: Okay.

ANDREWS: And, of course, if any new piece of information comes in late, you know, the system will allow some impact analysis either at the analysis level or at the process level or at the TSPA level, depending on the severity of that new information, either positive or negative.

BULLEN: Now, as a follow-on to that, I'll come back to Mark and say are there any pieces of data that are key that won't hit those deadlines? Is there something that you think that you really would like to have had, or you're looking for, that won't be available in time for the example, August of this year, or the eight month later time frame? Or are you pretty comfortable with the way the schedule looks right now?

PETERS: Well, let me maybe not answer it directly, but let me jump around the question a little bit. There are things in the schedule that aren't going to be available by August, namely the SZ stuff won't be totally mature, and the cross drift is not going to be totally mature. But we're going to have to go with what we've got, make the proper assumptions, be it conservative or bounding, whatever we need to do, and then as we get information, update. But, yeah, the reality is that certain things are not going to get done for August.

BULLEN: Thank you.

RUNNELLS: A question from Dick Parizek?

PARIZEK: Parizek, Board. You discussed the Richard's Barrier and the results seem very favorable based on the Atlas experiments. I have not visited that facility, but would be interested in see that. EPRI also did some calculations and demonstrations that show that works. That's natural material and the physics of that won't change in 10,000 years. On the other hand, maybe the properties will change if you cement it or harden it, maybe fracture it, various people have pointed that out. Are you doing experiments at the Atlas facility that deal with cementation possibilities, and the chemistry of reflux waters?

PETERS: A lot of the materials testing on those kind of, a lot of the chemical stuff that might go on, some of that's being done at Livermore on a batch or column type experiment. They're not actually doing that at the pilot-scale test at Atlas, but those issues are being addressed at Livermore as part of the same program.

PARIZEK: Because yesterday, we saw an example of a drip shield with about two feel of backfill on top of it. It was not a Richard's Barrier arrangement; at least the cartoon didn't suggest that. It was said to be informal, so we allow for that. But you can get so much out of Richard's Barrier, why wouldn't you use it along with another drip shield, since drip shields are metals and you already have super metals in the canister with the stellar performance results we heard about. So, you know, we're adding redundancy here, but drip shield is good. You've got to construct it, but Richard's Barrier on top of it would also buy you a lot from what your data is suggesting.

PETERS: It seems to be very effective in the test, and there's other examples, and you see a lot of examples out there where they are very effective performance-wise.

PARIZEK: Right.

PETERS: Is there a question in there, or do you--

PARIZEK: Well, it's a design factor. That may be put in the design. That is a question. When do you put it in? When would the drip shield go in? I guess that's late before you close the door, and then this question of roof stability?

PETERS: Right.

PARIZEK: Is remotely placed, and so there's some operational things, and I'm sure those are things the program is thinking about.

PETERS: Yes. If you want to get at the sort of operations aspect, I see Jim standing up already because I'm going to ask him to comment on that.

BLINK: Jim Blink from the M&O. We did consider a Richard's Barrier as the backfill. It is an option that is not precluded, because we don't have to finally design that backfill and emplace it until sometime into the future. The reason we didn't put it into the EDA II directly was because of the construction difficulty of emplacing it remotely in a thermally and radioactively warm environment. We just decided not to take that on unless we had to. But we haven't precluded it either, and we're very interested in following the results, both of the Livermore tests and the Losee Road tests.

PARIZEK: One other question about priorities. You didn't happen to mention the drip shield heater experiments. I assume that will run just like Busted Butte. You didn't state that.

PETERS: Right now, yeah, that's considered.

RUNNELLS: A question from Priscilla Nelson?

NELSON: Thanks. Nelson, Board. Two question; one, when will air permeability or other direct evaluation of permeability of lithophysal units data become available?

PETERS: We've drilled three boreholes at the Niche 5 location that we're going to use to do pre-excavation air K. Those have been drilled. We will hopefully have those measurements by later this summer. That's in the lower lith.

NELSON: So those will make into Rev. 00?

PETERS: Be right on the harry edge, but the information will be available hopefully to incorporate into the UZ process model.

NELSON: Okay. Let me ask you a question about that apparently free knowledge about the drilling construction water that got down and went an order of magnitude further in the non-lith than it did in the lith; is that correct?

PETERS: Right.

NELSON: How was that observed? And were there observations made about how much of that water might have entered matrix porosity?

PETERS: It was observed by slanted down deep boreholes, they cored them after the TBM passed, and they analyzed the chloride.

NELSON: And these were air cores, air drill?

PETERS: Yes.

NELSON: So there isn't any separation of whether--where the water was, how much might have entered fractures?

PETERS: There was a bullet in there. The only part of the detail that I can give you is the water balance suggested that about 50 per cent of it was lost to the fractures. But how much of it entered the matrix, I'm not sure how well we could address that actually.

NELSON: Is there a report on these observations?

PETERS: There is an informal report on that that I could talk to Claudia and we could try to get you a copy.

NELSON: Okay, thanks.

RUNNELLS: There's time for one last question. Anything from the staff?

(No response.)

RUNNELLS: Okay, let me ask a quick question then on Alcove 1. I've always been kind of worried about the 30 meters as not being representative of the thicker rock that will overlie the repository. Has there been any characterization of that 30 meter thickness above Alcove 1 to demonstrate that the hydrologic and chemical properties are, what should I say, representative or at least near enough to the repository cover as a whole that it will be followed?

PETERS: It's not in the right unit. It's in the Tiva.

RUNNELLS: I'm thinking more of the fracturing, the mineral coatings on the fracture surfaces, those kinds of things, the number of fractures.

PETERS: Well, the fracture coatings are different because you're so close to the surface, and you get a lot of calcrete type, you know, evaporation produced type deposits there. So fracture mineralogy is probably different in terms of abundance, and maybe even type. The fracture density, it's different in the Tiva. It's a fractured unit, but I can't really make a direct, totally direct comparison to what you see in the Topopah.

RUNNELLS: So is the primary purpose of it to, let's say, calibrate a model, to test the model, as opposed to simulate the Topopah?

PETERS: Yes, and to look at the effect of varying the infiltration rate on flow through the Tiva. That is the cap rock that controls a lot of the infiltration into the mountain. It was originally formulated as part of the El Nino test.

RUNNELLS: Right. Okay, thank you very much, Mark.

PETERS: Okay.

RUNNELLS: I think we're going to have to terminate the questions at this point.

Our next presentation will be by Tom Buqo from the Nye County Hydrology Program, and I have two things I want to mention before Tom gets started. Number one, the Board had the opportunity, thanks to the Nye County people, to visit three of the wells that are involved in the Nye County program when we came out here the day before yesterday. And I want to thank very much the Nye County people, Nick Stellavato, Tom Buqo, Parvis Montazer, and half a dozen other folks who were our hosts and hostesses. It was a very nice trip. We also visited the Oasis Valley study site, and we had there hosts from the USGS. So I want to thank those people.

The second announcement with regard to Nye County is in the back room back here, they have set up a demonstration of the West Bay sampling devices, and anybody who is interested, is welcome to go back there and see how these West Bay sampling devices are working in the Nye County wells.

So with that rather lengthy introduction, we'll wait for the microphone to be attached, and I'd invite him to proceed.

BUQO: A couple of quick announcements. In addition to the West Bay out there, again, we'd like to thank you for coming to Nye County. It's very significant that you're out here and we appreciate that effort. There's some road work going on out there. For those folks of you that are trying to push to get to an airplane, figure in another 20 minutes. Those flight people out there live here in Beatty and we want them all back tonight. Okay?

Some acknowledgements. We have to acknowledge we're working in a very cooperative environment and we want to acknowledge that that environment exists. We wouldn't be doing this work without the Department of Energy. Of course they're funding it. But more importantly than that, is that we have a true spirit of cooperation going on I think between all the participants in this program, and that's assisting us in doing this work, and hopefully it will assist us in collecting the data that people need collected.

And, finally, a little bit of our own disclaimer. I can't speak on behalf of Nye County, and I'm here to basically summarize the information. We have limited resources. We've got a team of 10 to 15 people that we can bring to bear on this, and we're not experts in every single discipline out there. We just do not have that capacity. So when I present things, I'll be talking about what our speculation is. In some cases, we are not in a position to be able to state definitively here's what these data mean. We're taking our best guess at it, and we're doing the best that we can, and we're going to try not to overstate things.

I want to give you a little interim status report. We're breaking up our evaluation for the data into some functional areas: hydrostratigraphy, aquifer testing, water chemistry. We covered those quite a bit on the field trip on Monday. We're going to do a little bit of rehash on that. Today primarily we're going to be talking about the hot water and the Phase 2 plans.

I'll give you a little background on the program. September, October, November were very busy times for us. We had to get our plans and procedures into place. We had to get our BLM permits in. We had to get our funding all lined up. We sat down at a meeting in mid October and Nick said, "Realistically, when are we going to start drilling," and I looked at him and said, "November 30th." Well, we got our funding November the 27th. We got our BLM permit on the 29th, and we were able to actually start drilling on November the 30th.

December, January, February, we were drilling. Okay? We generated a great deal of data. We continued our well completions into March. In March, we made an effort to get this out. This is how I spent my March, putting together the data package. We distributed this, what Nick, to about 60 different people.

As we generate new information, we will be sending out updates to this. We've gotten some feedback from some people, found a couple of glitches. When we send that out, we'll be sending out an errata and explaining things that people have had comments on. I think it's good, because that shows us that people are actually using this data.

Under hydrostratigraphy, we started out, this was our conceptual model over here, based on Felderhoff 25-1. What we found in reality is confirmed some of these things. The valley-fill deposits in Amargosa Valley are indeed quite variable. It's not a single layer system of alluvium, as it appears in some of the models. We have suspected that there are preferential pathways for groundwater flow and, hence, contaminant transport. We went out and did some testing and found out yes, indeed, there are preferential pathways.

We suspected that these volcaniclastic sediments may have a pronounced impact on flow. Again, the drilling confirmed it. In areas out there, they do have a pronounced impact on flow, especially with respect to this situation at the Pavits Spring formation, being juxtaposed up against a block of volcanics, resulting in shallow groundwater and the formation of some paleospring deposits out there.

Based on that, we think some of the geophysical interpretations that we used in our work in siting our wells may need to be revised.

We've done a lot of work with the USGS. They've been helping us a lot. They gave us geophysical interpretations, so we have Felderhoff well sitting down here where the carbonates are known to be 2200 feet. Based on that, we follow this contour around, and we thought over here at 1-D, it should be about 2500 feet, too. Well, we drilled it 2500 feet, and although we think we were getting close, we didn't hit any carbonates.

Well, we're concerned that the geophysical models maybe use the properties of the volcanics in other areas to come up with these interpretations, and now maybe we have to go back and rethink that and reprocess the data with new information that reflects the properties of the Pavits Springs.

And finally, in hydrostratigraphy, we're finding that the compartmentalization of the aquifers complicates the definition of hydraulic gradients. And one thing I'll be stressing to day over and over is don't try to make too much of this. Right now, we're in the first stage of an exploration program, and we've got a lot of work to do and a lot of thinking to do.

Here's a cartoon that we put together that shows our basic hydrostratigraphy, what we think is going on, a big thick layer of these tertiary sediments sitting down here, and depending on where you are in the system, it can have a pretty pronounced effect on things.

Again, the geophysical interpretations may need to be adjusted, and we know that within this conceptual model, we've got a bunch of permeable pathways for flow.

We're doing more than just drilling. Every chance we get, we go out and do some aquifer testing. For our wells that we've completed, before we put in those West Bay completions, we go out and we do a constant discharge aquifer test that gives us a composite water chemistry sample, and it gives us some composite hydraulic characteristics on it. So we've sampled three shallow West Bay wells. We came over last month--or actually, earlier this month, and did a 48 hour constant discharge test at one of the wells at Lathrop Wells.

This turns out to be a real important well. It saved us a lot of money. We were going to go in and put in a well at 4-D, and I'll show you on this map, 4-D is sitting right about here approximately. It was going to cost us half a million dollars to put in this well because it was going to have to be large diameter. We wanted to get several thousand gallons a minute out of it so that we could use our other wells as monitoring wells.

Well, the lady up at Jackass Aeropark was kind enough to allow us to shut off her water for five days, pull her pump out, put in a higher capacity pump, and do a quick and dirty 48 hour test around there while we monitored. Based on the result of that, and her cooperation, we're not going to need to drill that half a million dollar well. We can spend that money elsewhere in the program.

So later on this calendar year, but not fiscal year, we plan to go back in, put in a larger test, larger volume pump, and do a longer test with better instrumentation.

July the 6th, in cooperation with the Park Service, we're going to be going in and testing a well completing in the Stirling quartzite over here. That will give us better definition of what leakage we may be getting across the Funeral Mountains.

I'll talk a little bit more about the Lathrop Wells test. It's really important to us, and the reason why, the Lathrop Wells you see now is not what you saw five years ago, or even two years ago. It's quite different as you're driving up through here, and Nye County believes that it's going to be quite different ten years from now, and even more different 20 years from now.

I'd like to point out BLM has designated land for disposal to Nye County for a business park at Gate 510. We're moving forward with the concept of a science and technology corridor along Highway 95. We plan on putting a big time science museum up there if we can get the funding from the federal government, and we hope that we can. So we see big changes happening up there.

One of our concerns has always been as Nye County grows, and we're using more and more water, what is the impact on the water table going to be, and is that impact going to extend to Yucca Mountain, and do we have to take those things into consideration.

Pahrump is booming. We project that by the year 2050, there will be a minimum of 150,000 people living in Pahrump Valley, with an associated water demand of 80,000 acre feet a year. Right now, they're pumping 30,000 acre feet a year. We don't know what the impact of that is going to be, but we do feel that we'd better take this sort of thing into consideration.

So getting back up to Lathrop Wells, we've got different types of wells up there. We've got a big thick clay layer that we think is somehow associated with the Pavits Spring that thickens over our Washburn Well that we put it. It was only seven feet thick. As you move eastward, it gets thicker and thicker and thicker. By the time you get over here to 5-S, it was 450 feet thick.

Well, in the Lathrop Wells area, there's different types of wells. If it's an individual living in a trailer up there that only needs five gallons a minute 20 minutes a day, he can get by with a 500 foot deep well that just penetrates the alluvium above the clay. If it's a commercial operation, they need more water, they have to bite the bullet and they have to drill a well, in the case of Fort Amargosa, the new development up there, it's a 1280 foot deep well. So they've gone through and they penetrate the alluvium above the clay, some sands and gravels within the clay, and then they get beneath it and produce out of there, too.

So our pumping well is over here diagrammatically shown, that as we pumped it, we wanted to determine what the response was in each one of these zones. Amazingly, we saw a response in our own observation wells in both Washburn and 5-S. Before we started, we had to figure out where we're going to spend our resources, where we're going to put our transducers and data loggers, and so on. So Dr. Montazer did some analytical modeling, and based on that, we thought, well, we'll have this much draw-down after this much time this far out. We got in there and started pumping it and, well, we found out that first of all, we could get more water than we thought, 1300 gallons a minute with 125 feet of draw-down, and that those impacts went much further than we thought that they would. And it wasn't higher transmissivity. It looks like some real complications in the storativity out there.

We generated a lot of data when they're processed. Analyzing it now, and it's going to take some time to do that. Again, the purpose of doing this test is simply to design the long-term test.

Moving on to water chemistry, all we've got right now are preliminary results. They're starting to filter in. We've got completed results for the first water. During drilling when we hit water, we immediately stopped drilling operations and called the geochemist to come out and sample. That included scientists from the National Labs, it included the USGS scientists, it included our own Dr. Don Shettl that would come out and collect samples.

Then we did, after our wells were completed, more extensive sampling in May where we had our individual zones, we went in and sampled those. We don't have those results yet. Therefore, I bring your attention to the statement at the bottom, "Don't try to make too much out of this until we have those results in."

But in a nutshell, there's our preliminary results plotted on a Piper diagram. Now, what I did here is I plotted our results and the USGS results for the same wells. Good news. We got the same results. We're up here at site one, the USGS data plot is right up with ours. Same thing at 3-D, and so on. So we feel very comfortable that we're generating similar results, and that means that down the road we can kind of divvy up who's doing what, and not have to do so much replication.

Then I plotted some data from some of the published literature, primarily Benson and McKinley, where they had looked at water chemistry in the repository area at H-1, H-3, P-1 and then some in Crater Flat. Well, look where P-1 plots up. P-1, if you'll remember, is a well that penetrates all the volcanics and gets down into the carbonate aquifer. Chemically, it is almost identical to what we're seeing at 1-D. We think that's kind of interesting.

We've got an outlyer up here, which is 5-S, it's geographically the furthest located, and it's complicated because it was the old sample and it may not be representative of the groundwater. We have to take a look at that and see. We've got some additional work to do to develop that well before we feel that it's really going to be giving us a representative sample.

3-D, we show some separation between the USGS result here and our sample, and what that separation is is the difference between a pumped sample down here coming out of the entire borehole, versus our first water sample here of dirty, crummy water that you bail out of the open borehole.

The water chemistry for J-12 and Jackass Flats is just like 2-D and 9-S, and again, the P-1 to us is quite significant because that tells us we've got water coming up from the carbonates.

Okay, let's talk a little bit about this hot water. First, there's some limitations, and we have to take these limitations into consideration, so we don't try to make too much out of it just yet. Yes, we do see steep temperature profiles. Those were done with geophysical logging, temperature logging in an open borehole. So direct comparison with what you see in the published literature is difficult because those were done in cased boreholes.

But we can make some basic observations. First of all, we're seeing significantly higher thermal gradients in the vicinity of 1-D and 3-D. When we look with the strontium data, it suggests that these thermal signatures may not be reflecting a single source. Strontium, the highest strontium values in the region were found at 1-D. The second highest strontium values in the region were found at P-1. We think that's a good piece of evidence there.

Over at 1-D, we think we have a really significant contribution from the carbonate aquifer. We've got high temperature and high strontium. Over at 3-D, we're seeing strontium and high temperature, but it's a steeper gradient. It's lower temperature overall, and it's lower strontium. So we're either getting less of a contribution, or it's being buffered because we've got to get that water through a couple thousand more feet of sediments before we can get it up to where we're sampling. And then finally the gamma spike at 3-D may be providing additional clues.

So here's apples to oranges. I took the work out of some of the old Yucca Mountain studies and I plotted up the bounding values for those, and it turns out that H-4, P-1 generally bound the range of temperature profiles that were found and were reported in the literature. I simply plotted on top of this the results of our geophysical logs. So bear in mind this log here, while the profile is probably pretty good, it might translate back that way when we go back and do it in a cased hole. But as you can see, we have much steeper gradients than we've ever seen anywhere out there, and we've got some real breaks in things.

Where we get a big change in gradient like there, there and there, we can correlate those with our caliper logs and say, well, this is where the fractures are. And that's why we went in and put West Bay completion specifically at those zones, so we'll be able to come back now and get chemistry for those and be able to monitor pressure and temperature over a longer period of time.

And I said the gamma spike was kind of interesting, and we spent some time working with that because we like to know. The top figure shows what it looks like on the geophysical log, just this huge spike in the gamma log. It fried out the neutron log and every nuclear log, we got a similar response. So we decided we'd better take some time and look at this, so the first thing we did was rerun the logs to make sure that it was real. Well, it was real. It showed up on all of them.

We had our petrographer, Dr. Morgenstein at Berkeley, do some detailed evaluations of it, and then we're looking at it in terms of the water chemistry, too. Based on our preliminary results, here's the observations that we can make.

It occurs over a very tight horizon. Down here below, we've blown it up. And you can see the spike comes in at a very short interval here from 495 to 507 feet. It's near the base of a thick volcanic unit, which is probably Bull Frog Tuff, that's what the M&O stratigrapher said it probably is, and it's a the black ash flow tuff. It's kind of significant because we were logging a lot of holes out there. This is the only place where we recorded the color black, innate on the Munsel scale.

The petrography work says that there's a magnetic-- they went through and they looked at the magnetic susceptibility over a much larger interval, and they found that it coincides, the highest magnetic susceptibility coincides with that gamma spike and is probably related to hematite.

Well, that's interesting and we'll get to that. The peak uranium activity also coincides with the gamma spike. There is no potassium or thorium spike, so that says we're looking at uranium.

We got pyrite present through the entire interval of this Pavits Spring, and that tells me that it was deposited in a reducing environment. So our petrographer comes back and says, well, yeah, you've got pyrite present for the entire interval, but you also have abundant iron hydroxides and oxides, geothite, limonite and locally magnetite. Well, then says it's obvious you have pyrite and magnetite, that you have a situation with thermodynamic disequilibrium.

Well, I'm the guy at Arizona that used to run screaming down the hall when they pulled out the phase diagrams in petrology, but Dr. Morgenstein, so I believe him. We don't know what the uranium mineralogy is yet, but he's going to be taking a detailed look at that, because we really want to know.

And another interesting observation is although this well has an elevated temperature profile, the gamma spike coincides with the lowest temperature in the borehole. So based on that, we scratched our heads, we did a little speculation, and we said, well, how can we get that gamma spike up there in that uranium, and we came up with a very simple four step process.

We know that we've got a sender coming out there, so we're saying injection of magic magma, it's coming up a dike feeding the Lathrop Wells. That's step one. Step two is you have a pulse of uranium enriched water that's moving upward through the Pavits Spring to the base of the overlying volcanics.

Now, we don't know, we don't think that the uranium came out of the carbonates, because it wouldn't be a source. It would have to be the volcanics. So hot water coming out of the carbonates probably leached the uranium out of the lower portions of the volcanic sequence, and then they brought it up until they hit this one reactive bed. So you've got the groundwater that's oxidizing the iron in the volcanics. That's what gives us our coexistence of the pyrites and the iron oxides in this reactive bed, and then the uranium is deposited in the lower volcanics as a front at the chemical boundary.

Other observations are that the temperature profile that we see now may be a remnant of what was a much steeper profile associated with the volcano. The lower strontium values suggest that upwelling of hydrothermal water from the carbonate aquifer is not as likely, or maybe not in the same order of magnitude.

We're going to be doing additional work with that, additional petrographic studies to determine exactly what uranium minerals we're looking at. We're going to be collecting water samples from the two zones--well, we already did it in 3-S. The results are pending, and one of those zones is located right there.

So what's next? On the issue of hot water, is we're going to evaluate the results from the May 1999 sampling effort when they come in. We'll be taking a look at sodium potassium, silica geothermometers to see what sense we can make out of it. But what we really want to see is the strontium profiles on it. We want to see that vertical segregation to see, at P-1 where we had previously the highest strontium concentrations, they increased a lot with depth and we want to see are we going to have that same sort of increase with depth, because that tells us we've got water coming up from the carbonates.

We're going to go in and we're going to log the casing strings at 1-D. So we'll use our existing West Bay temperature probe, go in and log that, and that will allow us to make an apples to apples comparison between the logs that we've got and the published literatures.

And any suggestions, I mean if somebody thinks there's something we should do, if there's something you want to do, we've got a borehole, you're welcome to come play in it.

Okay, Phase 2 plans. We've done a lot of thinking. We're looking. We had a postmortem on our drilling earlier this month to look at ways that we could do things better, to get better data, and to make sure that where we're drilling is at the best locations.

We've got a 2500 foot deep borehole sitting out there at 3-D, and we still haven't hit the carbonate aquifers. This hot water threw us for a loop for two basic reasons. The West Bay equipment that we'd already purchased doesn't function over 40 degrees C. They will not certify it. So, therefore, once we got over 40 degrees, we can't use the equipment we've got. We've got to go with stainless steel, and it's a lot more money and we didn't have it.

So we said, okay, we'll leave this. We're going to have a deeper--oh, the other thing was the drilling limitations. When we got down to 2500 feet, the O-rings started heating up and they started blowing O-rings and seals, so we had equipment failure, and the driller was ready to--you know, one day I said, well, can't we go to 2700, and the driller is wanting to pack his suitcase and get out. So we're going to get a deeper capacity rig out there with the capability of going 6000 feet. We're going to take 3-D down deeper.

We are hoping that we will hit the carbonate aquifer. We can't guarantee it because we don't know how deep it is. We know that we do need that point so the geophysicist can go in and we'll have two points in the carbonates. We want to give them a third so they can triangulate and reprocess all their data, and give us a really good top of paleozoic surface.

Okay, we want to obtain a deeper temperature profile to see what happens at depth in that borehole, and collect as many vertically distributed samples as we possibly can for chemical analysis.

We're going to do a longer term, higher discharge aquifer test at the Jackass Aeropark well. We've spoken with the lady and she'll allow us to come in after the growing season, and we can go in and pump it as long as we want. Our intent here, we're going to go in and analyze the data we got from the preliminary test. We got two different sides. Our reservoir engineer says, well, you might not really have to pump it any more, you might just have to pump it longer.

And I'm from the old school of hydrology, more is always better, so we'll sit down and we'll plug in real values in our analytical models, and it will tell us if we need to pump it for five days, seven days, ten days, 30 days, whatever it is, because we have to get our permits from the State and our discharge permit, and so on, and our waiver from DWR. It's all doable, but we just have to do some more analysis.

We're going to change the test well that was planned at 4-D at half a million bucks to a couple of peizometers, one set above the clay and one set below the clay. So for $40,000, we've just gotten the same information as having to spend half a million before.

We're going to change 12-S from a monitoring well to a test well. 12-S is in the vicinity of the old Rosie's well, just on the other side of Highway 95 in the vicinity of 1-D, 9-S and 3-D. And based on our results, we want to know if we go on the Valley side of the Carrara Fault system and start pumping the water, are we going to get leakage across that fault. So we plan to do a test there.

We're going to investigate the spring deposits, aquifers and water levels in Crater Flat. At 7-S, we see an analogous situation there where we also have paleospring deposits. We were really surprised at 1-D. When we did our work, we thought we'd hit water at 330 feet below land surface, because that's what all the regional data said. We got out there and hit water at 50 feet, so we knew immediately something is going on here, and we got the feeling that the hydrostratigraphy was such that the water is flowing through the system, it hits this big thick plug of Pavits Springs, it can't go through it as fast, so it stacks up behind it.

We think we may be seeing the same thing at 7-S, so we want to go in there and see just exactly how deep is the water there. So we wanted to find the depth water in the paleodischarge area, and we wanted to find gradients in part of Crater Flat. Now, the reason I say part of Crater Flat, it again gets into this thing of how much do you need to drill to start defining things. When we drilled at 1-D, we got surprised. The water table is a lot higher than we expected it to be.

Well, if we go and drill at 7-S, we may find something different, but we can't correlate between 7-S and 1-D. We can't just simply take the heads and say this is the gradient, because they may not be related. We've got a big structure, a big linament between 7-S and 1-D, so we may have totally separate hydraulic gradients representing different compartments within the aquifer.

And we're going to do additional deep and intermediate drilling. Right now, we're planning on doing four additional deep wells, 6-D, 12-D, 15-D and 20-D. Those are all distributed in the lower end to give us more information down towards our receptors. And intermediate drilling depths at 7-S, and we're going to start working our way up Fortymile Wash.

So that's a real quick overview. Oh, one other thing. Some of the stuff I showed today is not available in the handouts. I hope all of the Board members got copies of the handouts from the field trip. If so, you've got everything. We've got a limited number and we have to prioritize them for the state, the effective units of local government, and the public. Come see me afterwards, and we'll get you sets. Anybody else, talk to Nick and we'll get you copies of them. Thanks.

RUNNELLS: Thank you, Tom. You covered a huge amount of material in a very short time, and I'm going to steal the first question.

Your comment on the fact that the water temperatures show a much steeper gradient than anywhere else around here, I think you said, when you look at your chart, it's about 20 degrees Centigrade, it's about 20 degrees per kilometer of gradient. What is normal for around here? Why does that amount surprise you relative to other locations?

BUQO: Well, you know, the more I read, the more I wonder myself, and the reason I said higher is because I'm looking at the results for Yucca Mountain. But then Bill Dudley in Colorado was nice enough to send me this work he'd been doing, and then I see, well, actually we're sitting there in this one geothermal low in the area, and now maybe this may not be as anomalous, and it may be a situation where it's anomalous with respect to Yucca Mountain, but we're outside that low now, and it's not anomalous for the part of the geothermal regime we're in there.

RUNNELLS: Yeah, because it doesn't strike me as an outrageous geothermal gradient by any means for this part of the world. Thank you.

Other questions from the Board? Debra Knopman?

KNOPMAN: Knopman, Board. If money were no object, and you had the ability to drill another 10 to 20 wells, where would you put them and why?

BUQO: Well, I would start putting wells in the vicinity of the wells that we've already got to define gradients, because, you know, in an exploration program of this nature, you dig up more questions than you answer. So we found the depth of water over here was 50 feet, and the potentiometric elevation is 786 meters. Well, our contour says it should be 700 meters. I want more definition right in here so that I know what the gradient is from this point into Amargosa Valley, because I want to calculate the flux of groundwater that's going through this part of the system.

If I come back up here to 7-S and punch in a hole, which we plan to do, I can't use that reliably to get my gradient. I'd have to have additional wells around 7-S. So if money were no object, I'd want more definition of the gradients right off the bat. Deeper wells would be the other thing, and more testing.

Now, we have logistical problems that if money were no problem, in talking with the USGS, these wells that we've got going up Fortymile Wash, they would prefer to see those on the west side of Fortymile Wash. And their point is very well taken, and we're going to take a look at that, but there's the realities of things. There's no roads out in this part of the test site, so that means we're going to have to get in there and bulldoze a road to get to some of these sites. We have to get a lot more environmental clearances and that sort of thing, and the cost of doing business on the test site is a little more than it is doing it off of the test site.

So we certainly want to put more wells in the vicinity of Fortymile Wash because we still believe this is our number one pathway to get anything from the repository to our receptor populations down here in Amargosa Valley. The fact that we found high water in this area suggests to me that that is acting as a hydraulic barrier, that we're not going to be getting any contribution. That was one of our concerns, all these faults sitting in here going like this and this, that we could be getting some communication through that area. But if the water table here is higher than here, we're not going to be getting that communication. That forces us here and says we should be spending more time over here.

RUNNELLS: One last quick question from Priscilla Nelson.

NELSON: Nelson, Board. I've got two. One is real quick. Does it make any sense or can you or do you have plans to do anything with age dating of the water? Given the difficulty in completing the wells and everything else, is there any--

BUQO: No and no. We're not going to be doing age dating, but the USGS is, I believe. Zel, are you guys going to be age dating this water?

PETERMAN: Yes.

BUQO: Yes.

NELSON: Okay. And just the end of that, you've got a lot of people doing a lot of different things on the water coming out of here.

BUQO: Yes.

NELSON: Who's in charge of integrating all of that?

BUQO: From the Nye County point of view, Nick and I.

NELSON: Right. But you're producing data, USGS is producing data, other people are producing data. Who's in charge of pulling that all together in your mind?

BUQO: Well, in my mind, each organization has their own principal investigator, and Zel and I talk and I assume he's taking the lead with respect to the USGS, and their capability of doing geochemical analysis is superior to anybody. So they're going to look at it from their perspective, and they'll do their interpretations. We look at it from a different perspective, and we may come up with something different. Well, then we can sit down with the same data and talk back and forth and see who's right, who's wrong. In some cases, both of us may be right, and in other cases, it may just point to the need for additional data collection, that we can't resolve it, and in other cases, it may be that we're clearly both wrong.

DIXON: Paul Dixon. Zel Peterman and June Fabryka-Martin are doing a joint AMR on the geochemistry issues in the UZ and SZ, and that AMR will incorporate this information in the revs as they go up. So as the information comes in collected, at least within the M&O, affected organizations and the interpretations of the data collected from Nye County that the M&O uses, that will be incorporated in those AMRs and used in the PMRs in the future. So the integration will come through the AMRs.

RUNNELLS: I'm afraid we're going to have to terminate this. I'm sorry, sir.

SHELL: Just one quick comment.

BUQO: He's our geochemist.

RUNNELLS: Okay.

SHELL: Don Shell from Nye County. We are running Chlorine 36 and radiocarbon analyses on our samples. So we will be getting into some age dating.

RUNNELLS: Thank you very much. I'm sure there are many, many questions. Please grab Tom separately and chat with him, because we do have to move along.

Our next presentation is by Paul Dixon, who just had a short comment here a moment ago. And he's going to give us an overview and a summary of the work being done on Chlorine 36, Chloride data and other issues related to hydrologic flow and hydrologic pathways. Paul?

DIXON: I guess where I'll start today with everybody is that the work that I'm presenting here today, I'm representing the work being done by June Fabryka-Martin and her team of colleagues working on Chlorine 36 and Chloride mass balance.

This is the first slide here. One of the purposes and kind of just a general overview, and I'll skip over a few slides and just move along, since I have a short time period here, one of the things I want to look at here is the whole purpose of the Chlorine 36 study and the chloride mass balance study as they stand now is to try to understand flow rates and pathways. And the reason for wanting to understanding some of this in the mountain overall is that these pathways may become potential seeps in the future under different climatic conditions, so we're trying to understand how water moves through the mountain.

The objectives here, the project has conceptual models and is trying to help constrain some of those conceptual models on UZ flow and transport, based on measurements and simulations using Chlorine 36 and chloride. You guys can read through the specific objectives here at your own leisure, but there's a list of things that she's trying to address here.

The approach here is to try to develop a pretty extensive dataset of Chlorine 36 and Chloride porewater concentrations for the UZ, from ESF, cross drift, as well as surface based boreholes. Then having gotten this information, use the detailed structural and petrologic characterization at each sampling site to try to put this into some sort of a geologic context. And that's why on the cover slide, you see there's a team of people that work on this, people from USGS, modelers, and the infiltration people.

And that's really the key to what I think is making this study important, is that it integrates a lot of the different disciplines and gives you an answer that is not just looking at the Chlorine 36, but it's integrating it with a lot of other parts of the program.

And then to try to test the models through simulations of Chlorine 36 transport, using our most current infiltration geologic models and hydrologic parameter sets. And then, finally, try to test once you have these conceptual models, looking at some of the predictions we've done in the cross drift.

This is just kind of a background slide for some of the new members on the Board and other people, just to kind of give you a background on where we are.

And this, again, is just another one of those for people for background, and I don't need to spend time with them right now.

To date, right now, sampling in the ESF and cross drift excavated beneath Yucca Mountain, we have almost 250 samples that have been analyzed for Chlorine 36. 13 per cent of those have an elevated Chlorine 36/Chloride ratio, and we'll talk about that in a few minutes. And 40 samples to the end of this last month have been measured for chloride porewater.

Just a reminder, the bomb-pulse Chlorine 36 is a fortuitous tracer and it's one of the simplest ways that we have right now of identifying pathways where water has travelled in the last 50 years, at least to the depth of either the cross drift or the ESF.

For some of you, this has old data, it's got a little bit of new data on here because June has put some of her porewater data on here. This is a review of all the data along the ESF for Chlorine 36. And in general, you see most of the data falls within the blue band, or slightly below it. There are elevated values above what we consider to be an estimated meteoric signal over the last 50,000 years.

What June is trying to do and has done, I'll show you in the next several slides, has done a statistical analysis of this dataset now that it's this large, to try to break it into what are the components and can you define in a mixing model to give you an idea of how these components break out. Note that the porewater shown in the little solid boxes there that are larger, tend to be in fairly good agreement with what we've been measuring out of the rocks to this point in time.

This is the statistical analysis that June has one of the statisticians at Los Alamos working on, basically looking at the distribution of the Chlorine 36. Here, this is the roughly 250 samples. You can see that you have a long right tail that's well above background here, and values here. You seem to have a bimodal central distribution, and then you have a small fraction of values at the lower end here. So the object was was to try to see how you could break this up statistically.

If you look at what's been done to date, we know that the middle part of this curve is a combination of Holocene and Pleistocene signals. Mitch Plummer, who did pack rat middens down at Sequoro, has actually supplied what some of that curve would look like over the past 40,000 years, 50,000 years. We know we have a decayed signal where you have decayed Chlorine 36 and a bomb-pulse data. You can actually put together a mixing model where you can break up these different components and it actually fits the data extremely well.

And on the next slide here, this is just to kind of give you, looking down the ESF at this point in time, to kind of give you an idea of how that different components break out as you go down the tunnel. And what you'll notice is that as you get down further in the tunnel, as you come into the south ramp, you'll notice you see a lot more decayed values. And this is evidence that you have very slow groundwater travel times in this area, and as I go through the further parts of this talk, this is backed up by some of the chloride mass balance data, and it's also backed up by the more current infiltration models. And by and large, this is related to alluvial cover in the south ramp where you're not getting water into the rocks, even though you have a thinner PTn at that region.

There's been in the past a lot of criticism about whether the elevated signals are actually truly related to bomb-pulse and things. We do have the validation study going on with the USGS right now. Lawrence Livermore Laboratory will be looking at, as Mark said, Technetium 99. Purdue will be redoing some of the Chlorine 36, and they're also going to try to look at Iodine 129 in these.

In one area where we do have coeval data that's been measured in the past, I just want to show an example here of, is the fact that in places where we do see elevated tritium, we also see elevated Chlorine 36 in the tunnel, which is a very, from my perspective, a very good indication that we are looking at true bomb-pulse getting down to this level of the horizon, and it's not some other process.

To date, this is the preliminary data that June has to date on the cross drift. I don't know if you guys have seen this yet or not. Bottom line is that she has more samples extracted and ready to analyze, but at this point in time, funding has been stuck towards getting AMRs and other things right now rather than spending it on analysis of samples in the short-term here as we try to get these reports and data qualifications done.

But what's interesting here is that as you go along the cross drift, in general, you don't see much of an effect of things like the Sundance and stuff at this point in time, but where you get out here where the TSw is exposed and you're out by the Solitario Canyon, you notice that you start seeing extremely high values of Chlorine 36, meaning that the PTn does play a major role in this, and where you don't have the PTn covering, you have exposed Topopah Springs. Water gets down to this level very, very quickly in the system.

You guys have all probably gone through this once, but I'll go back through it again. The conceptual model right now for getting water down to the tunnel is based on the fact that you have a continuous structural pathway from the surface to the depth of the tunnel, be it the ESF or the cross drift.

The magnitude of surface infiltration must be sufficiently high to initiate and sustain at least a small component of fracture flow. And the tests from Alcove 1 kind of give you some indication that you've got to have fairly high rates along certain saturated pathways to get water to come down the system, or even get it to drip. So that's kind of--you start to get some idea that you've got to have wetting pathways and you've got to have a high infiltration rate that's concentrated for some period of time.

And the last thing is that the alluvial cover has to be thin enough so that the residence time in that is probably less than 50 years. Otherwise, you're not going to see this. So as the alluvial cover becomes thicker, and in general this is probably less than a meter of alluvial cover, and you also have to, if you have thin alluvial cover, you also have to have elevated infiltration on top of that. So it's a combination of these factors, not one or the other, that makes this happen, but it's the combination of all three factors in series.

Andy Wolfsberg, you guys have seen this in the past, people have always wanted to know can you get a handle on what the infiltration might be based on the Chlorine 36. Well, one of the ways you can do that is you can take your conceptual model and you can say at different infiltration rates, when could I, with this conceptual model, get bomb-pulse Chlorine 36 down to the level of the tunnel. And what you notice is that at low infiltration rates of .1 or so, even with a fault in the system, continuous fault all the way in the system, you can't get Chlorine 36 down to the level of a place like the ESF.

But on the other hand, at higher infiltration rates, where the minimum case here is 5, and above five it becomes easier, but at a minimum case of 5 millimeters per year, it isn't until you put a fault into the system, or a continuous pathway, even if you have high infiltration and thin alluvial cover, that you can get Chlorine 36 down to the level of the tunnel.

So you can start to bound with Chlorine 36 what the infiltration might be. You can say definitively, but you can start to put bounds on where you might be with that, so you can get a bounding information.

What I'd like to do now is talk about the chloride mass balance method, and I will apologize that this equal sign is supposed to be a times here. That was a typo I didn't catch before you guys got it, so everybody who's out in the audience, please make that a times. That's my fault.

What the chloride mass balance method basically says is that infiltration, concentration of salts in the infiltrating water is equal to precipitation times the concentration of salt in the precipitation, divided by the concentration of salt you measure in the porewater. So if you want infiltration to equal precipitation, all you say is that concentration of the precipitation is what you measure in the salt. But as you get evaporation and you lose water into the system, concentration of the salt here increases and, therefore, the infiltration, the amount of infiltration decreases as you head into that.

So as you get higher and higher salt concentrations, the amount of infiltration becomes less and less. So by using this method, you can get a direct indication of what the infiltration rate is. Now, there are lots of caveats and other things that go into this method, but by and large as a first cut, this is a pretty straightforward way of looking at infiltration, and it's probably one of the best methods that we have at this time to try to address it directly from what we're measuring in the field.

In the cross drift right now, we have measured values of chlorides from the porewaters, and then we have calculated from the surface infiltration model, what is the chloride concentrations you would predict from that.

By and large, you had fairly good agreement here. And as a first cut through this, there are places where you don't have agreement, and then we're looking into those things right now as they interpret the data to what we see.

The estimated infiltration rates above the cross drift, if we look at what the calculated infiltration rate is baseed on chloride concentrations, that's what's shown here in squares, and then the predicted. So in other words, when we went in before we did the cross drift, we did a series of predictions of what we thought it would be, and then we went back in and measured chloride concentrations, and calculated an infiltration rate. And you can see that there's fairly good agreement, again, with the exception of those several samples in that one area there, which are under interpretation right now, and not fully understood.

So overall, the model that's been generated right now for infiltration and stuff appears to be fairly representative at this point in time to what we're able to measure in the field from chloride concentrations.

Chloride porewater concentrations from the ESF are shown here versus chloride concentrations predicted using the site scale transport model. What you see is there's fairly good agreement along here, until you get down to the south ramp. And as you remember before when we looked at the Chlorine 36 in the south ramp, it showed decayed values. And what we're seeing here is that our current models here for predicting this are not doing it correctly, because we probably have the infiltration wrong in this system and the current models as they stand, and we have to adjust the amount of infiltration we have in that system, because we're seeing much, much elevated chloride concentrations. We're seeing decayed Chlorine 36, all indicative that we have very, very slow travel times from the surface down to the level of the ESF in the south ramp of this region. Both of these pieces of information combined say the same thing.

If we go back in and we do some tweaking on the infiltration model based on what we know, and we look at what we would predict for infiltration versus what we calculate from the chlorides, this is what we actually get here, and you actually get a much better fit when you start to change the amount of infiltration you get in the south ramp, based on the values that we have.

And, finally, summary and conclusions. I think for me it was kind of exciting to see that you can actually fit the data for the Chlorine 36 very well with the four component mixing model, based on Halocene, Pleistocene, bomb-pulse and then decayed signal, and it's amazing how well that fit in the four component mixing model.

The bomb-pulse observations are correlated with faults in the northern ESF, and that's basically based on the distribution of ratios in the ESF and cross drift, and is supported by elevated pH levels in several deep boreholes and in two ESF alcoves, as well as we have limited Technetium 99 measurements that I did when I was at Los Alamos for June, which are also supported with this, although at this point in time, June is holding those out until we get the values out of Livermore before we combine everything together.

The average flux at Yucca Mountain is probably higher than 1 millimeter a year, but most likely in the range of 1 to 10, based on the data that we see here today, and that's really based a lot on the chloride porewater concentrations.

Model simulations of faults with increase PTn fracture permeability yield local fast pathways from the surface to the ESF, in combination with high infiltration and low alluvial cover thickness, or thin alluvial cover thickness.

And there appears to be a large dampening in the PTn of spatial and temporal variations by and large. So the PTn acts kind of as something that has a hysteresis effect on the signals we're seeing. If you take that into account, you can see the signal come out the other end.

And the discrepancy between model and observed Chlorine 36/chloride ratios that we saw on some of the things here may be due to the fact, as I pointed out, we have variations--you know, we have an incorrect infiltration rate, and these appear to be supported by the chloride mass balance method, as I pointed out, and there may be more lateral flow in the system than we're able to account for, because the chloride mass balance method assumes a vertical flow through the system. And if you have more lateral time, more lateral transport in the system right now, you're going to see longer travel times.

And I'll stop there and entertain questions.

RUNNELLS: Thank you, Paul. I appreciate you putting the preparation in to finish that much material in almost exactly 15 minutes. Priscilla Nelson

NELSON: Nelson, Board. I'm not sure I understand the difference between the data referred to first of all in Figure 8, which refers to rock samples and porewater sample. Can you explain the different kinds of samples and what your expectation was in having two separate kinds of samples?

DIXON: Initially, all the samples that were collected in the ESF were rock samples that were taken back, crushed in size, and then leached for Chlorine 36. June has gone back into those same areas, taken samples of rock, put them in the centrifuge, the ultra-centrifuge, UFA, and has spun out water, taken that water out without doing the leaching, but taken the water concentrate out, and then measured that for Chlorine 36 to see if there was a difference, to see if there was something in the process that she was doing in the leaching process that may have been causing a problem, or may have led to erroneous Chlorine 36 results.

So the difference between the porewater and the rock samples is one is just a leach on the bulk rock; the other one is actually water extracted from the rock, porewater extracted from the rock, where all you do is do a concentration on that, and then measure the Chlorine 36 just to look at commonalities between the two to make sure there's not a problem with the methodology that she was used to date, because there had been some questions of whether the methodology that was being used could lead to erroneous results, or spurious results.

NELSON: So one of these is not going to be more likely to show matrix content as opposed to fracture flow path, fracture wall content?

DIXON: In general, if you think about what you're spinning out of the porewater, is the matrix water at that point. So you're looking at a matrix water from the porewater extractions. Where you take the bulk samples, you're looking at more of a surface coating, plus the matrix. You get a combination of the two, Priscilla.

And so, in general, it's harder to pull out I would say a true fracture component from the porewater unless you have a sample that's along a fracture surface, and some of that salt gets leached from the water as it comes out.

NELSON: So you would expect, if anything, that the rock samples would show more peaks on Chlorine 36 than would the porewater?

DIXON: Yes, I would, and considering the amount of matrix permeability you have in the Topopah and the amount of fracture matrix interaction you have, that would be strongly indicated to me.

RUNNELLS: Question from Alberto Sagüés?

SAGÜÉS: Yes, I'm sure that I'm not the first one that asks this kind of question, but one of the conclusions that you indicated is that the bomb pulse observations are correlated with faults in the northern ESF, and I presume that that goes through the data in Figure 8.

DIXON: Yes.

SAGÜÉS: But, you know, if one looks at that figure without really being very much involved with this issue, I would say, gee, you know, there seems to be quite a bit of data over the estimated range of signal for the Sundance Fault, and there seems to be about five data points for the Drill Hole Wash Fault indication, and then most of the other areas I see probably as many instances of high signals away from faults than closer to faults. So really, it seems to boil down almost like just two cases for which there seems to be what appears to be, you know, to the casual observer, a somewhat convincing--

DIXON: I'll start there with not all faults are pathways. Some faults are barriers. So the presence of a fault in and of itself does not necessitate a fast pathway. And, in fact, you see that. As you start down here, you have the Bow Ridge Fault. That shows you have an indication of a large fault, and you see Chlorine 36 in it. You have Drill Hole, Diabolus, Sundance, and within the Ghost Dance, as you notice, the background dropping down here, you do start to see values elevating out in this region. And if you look at the analysis, these points in here actually do, when you do a statistical analysis, pop out as being anomalous from the background, because you do have a shifting background in the Chlorine 36 rates as you go through the ESF, and that really has to do with the infiltration and the residence time of surface water heading into the system.

Now, there are places where you have elevated signals outside of that. Understand that once you get through the PTn, there's such a fracture matrix network within the Topopah, you would expect it to likely come down with faults throughout the PTn, but that does not necessitate that it would have to be in that region. You can have a spreading. I mean, you could have a fracture network, a low angle fault in the Topopah that's in unit fault that could drive things. Something could come down the Sundance and be driven over here, or Ghost Dance and be driven that way, or other faults.

So the fracture network within the Topopah could drive fluids a lot of different directions, Alberto. But I agree by and large there appears to be a structural control on a lot of this. In some places, you don't have the direct linkage of the structural control, and that just has to do with our lack of knowledge of how actually water flows through the Topopah. And I think when you saw the niche studies, you saw that there was the close matrix flow in the thing, and then you saw that there was a high angle fault that moved the water in different direction when they looked at the seepage studies into the niches.

I assume by and large that on larger structures and larger things, you're going to see that sort of behavior, where you have a nesting of faults, the water will come kind of straight down through, but if you have high angle faults, you can move water a long ways laterally or semi-laterally through the Topopah, because it has such a low matrix permeability.

SAGÜÉS: But still am I wrong in interpreting this as saying that that correlation is, say, looking at the data, statistically would be supported clearly and undisputable for about two, or at the most, three locations?

DIXON: I mean, I see it clearly at four locations, and I see it also--I mean, again, you're looking at faults that were identified at the ESF or if you have faulting at the surface which projected down, and a lot of areas, we see the faults projecting down. I mean, I really see two areas, one area being here and one area being here, two areas that are anomalous rather than exceptions, you know, rather than being the exception, where I see being anomalous. But then this is in the eye of the beholder. Claudia?

NEWBURY: Claudia Newbury, DOE. I just wanted to point out that those are feature based rock samples. In other words, there was something there that caused someone to take the sample. It's not part of the systematic sampling. So there was a fracture at that point, or there was some other reason for June to be taking that particular sample. So it wasn't just faults, but there are other features, other fractures that are where those samples are.

DIXON: I mean, I think the example would be in the cross drift, where they went down to the 4 centimeter scale mapping. You saw the distribution difference between things that were a meter, versus things that go down to 4 centimeters.

SAGÜÉS: Thank you.

DIXON: I hope that answers it, Alberto.

RUNNELLS: Time for one more question. Anybody on the Board?

(No response.)

RUNNELLS: Anyone on the staff? Yes, Leon?

REITER: Leon Reiter. Paul, it's a little lost on me. It may be a question; why is it that the four component mixture model gives you such comfort? Just sort of looking at it, it looks like you have sort of something with four knobs that you turn and you can get a fit. I mean, maybe I don't understand something.

DIXON: What I'm saying there is is that that model has an object you can turn, but they have tried to make that model based on realistic distributions of what we know the different input values are for the Pleistocene, Holocene, what the bomb pulse would be. You can actually break into those components. Yes, you can tweak the knobs to make it fit pretty well, but in my viewpoint, when you look at the data and how it's distributed, the only way you can get a true fit to that data is if you take the bounding distributions for each one of those.

And that's where this calculation first started; what is the bounding range within each one of those four components, and how well does that fit. And this calculation was done with bounding values for what the ranges in the Pleistocene, what the ranges in the Holocene.

Now, if you took and you wanted to make it a perfect fit, you could add more Holocene and you could bring that peak up so you could make it fit even better. But the bottom line is is there appears to be a fairly good agreement when you do that sort of analysis statistically on this, and I'm not a statistician, there may be people out here who are much better statisticians than myself, but it does give you a feeling for, as you go down the tunnel, what you're seeing and why you're seeing it. The exact fit to that, as you point out, is semi-arbitrary.

RUNNELLS: Okay, that will have to conclude our question and answer session.

Any announcements either from Debra or Jerry before lunch?

(No response.)

RUNNELLS: Okay, folks, we are going to start at 1:10.00, not 1:11, so be back here at 1:10 and not a second later.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

PARIZEK: We want to start on schedule, because we have both these presentations, there's room for a public comment period, and then we also have an adjournment deadline to meet.

The first talk represents the State of Nevada, it will be on regional hydrology studies in the State of Nevada by Linda Lehman, and it's both the saturated zone review and unsaturated zone review that she wants to present. We'll need all of the time available for that presentation.

That will be followed by the unsaturated zone transport tests at the Busted Butte facility, which is also imported to the performance of the Yucca Mountain project.

So, Linda, we'll start right up here. So everyone who's not sitting down, please join us. We are starting on schedule.

LEHMAN: Thank you, Dr. Parizek. Can everyone hear me?

Thank you for the opportunity to present the State of Nevada funded research to the Board and to the audience.

Today, I'm going to try to cover a lot of ground, first starting with the conceptual model for the saturated zone flow and transport. And in this model, I'm going to summarize briefly now some of the important elements.

Basically, what we are proposing is a structurally controlled flow field as opposed to matrix flow. In this talk, I'm not going to try to develop or define the structures at Yucca Mountain in terms of the structural geology, but rather to take what we know about the structures and tectonics and apply it to how does it influence the flow.

Some of the features that are of interest and significant to our flow model are the north-south trending faults, Ghost Dance, Solitario, and the Bow Ridge; the northwest trending shear zones, the Drill Hole Wash, the Sundance and the Abandoned Wash Fault; and the northeast-southwest trending faults, which exist at the southern part of Yucca Mountain.

These faults, we believe, are open conduits in some cases, and the ability to transmit water seems to agree with some work recently done by the Center for Regulatory Analysis, and they agree that the northwest trending faults could carry water based on a slip tendency analysis, and that the northeast-southwest faults carry a lot of water and are transmissive based on dilational tendency analysis.

Also, I want to say that we used temperature as an indicator of flow paths and recharge. And at the end, I will present some of our latest 3-D non-isothermal modeling results.

In terms of the unsaturated zone and infiltration, basically our model consists of several elements. One is focused flow, where flow is not infiltrated directly over a large area, but rather channelled into certain areas where it can infiltrate. We believe that lateral flow does exist in several of the units, most notably the PTn. Also, the concept of non-equilibrium flow, which means that flow can move in the fractures without the matrix around it being at 100 per cent saturation, and the concept of hysteresis, which was brought up several times earlier, basically the difference between the wetting curves and the drying curves of the rock mass.

Also, I'm going to briefly mention the role of structure in the unsaturated zone as well as the saturated zone, and if we have time in the end, I'm going to talk about how representative the current total system performance assessments and the NRC's TPA are to this structurally controlled flow.

The database that we have in the saturated zone basically consists of temperature, hydraulic head data, some information on the structural geology, and some hydrogeologic parameters which were measured from well tests, and also some hydrochemistry data.

We believe that any model proposed, or any flow paths, have to be consistent with all of the data and should not use selective datasets.

This is the temperature database that we are using, and it's derived from SASS 1988 from the USGS, and the important things to notice here, and I don't know if you can see this red marker very well, but we basically have three cold fingers, one of which is a 30 degree--this is the 30 degree contour line, and we have a cold plume of water essentially coming down the Fortymile Wash area, one that's coincident exactly with the trace of the Ghost Dance Fault in the Yucca Mountain area, and you can see right here maybe this little dotted line is the repository footprint, and then another cold plume coming down in Crater Flat.

Over here on the west, we have a hot area, which is up to about 38.8 degrees C. at WT-10. We interpret this to be upwelling hot water along the structure of the Solitario Canyon Fault.

Another thing I'd like to point out is the shape of this cold finger here along the mountain, and you can see it takes sort of a zig zag shape. This could indicate that recharge is occurring either from infiltration from the top, or from across either the steep hydraulic gradient which exists to the north. Other things could cause a cold temperature plume, elevation effects on heat flow, for example, and evaporation in the unsaturated zone.

Right now, we're looking at two concepts, one is the infiltration only through the mountain, and infiltration combined with recharge coming from above on the steep gradient, because the water up here is colder, about 29 degrees C.

KNOPMAN: Excuse me, Linda. What depth are you talking about here?

LEHMAN: This is at the surface of the water table. This shape, this zig zag shape, I'm going to come back when we talk about structures in a moment.

I guess before I--I should go to this one. Basically, the flow path that you would assume from this type of a setup are like the arrows. It would be moving down gradient according to these arrows that are shown here. And that is a different picture than what you get from looking at the potentiometric surface.

Now, this is the potentiometric surface from the USGS. It's the revised water table map, the elevations of the water table, and the GS in '93, '94 time period re-levelled all of the wells. They resurveyed them and they corrected the head measurements based on temperature and density. Then they replotted the potentiometric surface.

They have smoothed the surface, as you can see, and as a hydrologist looking at this, I would assume that the flow would go exactly at right angles to this, or to the southeast. However, the numbers shown in red are the corrected levels that the GS also came up with, but did not use in this contouring.

They didn't use them because they felt these numbers were too low, and they could not explain the physical significance of those numbers. However, we took and replotted the data and used all of the data, and came up with a different picture of how water would move through the saturated zone. These hydraulic lows, or embayments as I like to call them, in the 730 meter contour line seem to coincide exactly with the positions of the Drill Hole Wash Fault, the Sundance Fault and the Abandoned Wash Fault.

This gives you a more complex picture of how flow can move through Yucca Mountain. Moving at right angles to this, we would have these faults basically acting as drains. Some of them that are very deep and almost touch the Solitario Canyon could be ways in which we have interbasin flow occurring, and this is quite a common occurrence in this part of the world, as we have suspected interbasin flow for quite some time, but really have not worked out the mechanisms, and this may be one way in which flow is moving across some of these steep hydraulic gradients.

So with this concept, we have flow, as the temperature shows, moving down the Ghost Dance Fault, and then draining into the northwest trending shears.

When I saw the zig zag shape of the temperature plume, it was reminiscent of transform faulting structures to me, because you do have that same zig zag shape. One thing I'd like to point out here is that these shapes are not scale dependent. They can be seen at many scales, and we can see them at the mountain in different scales. These shapes here range from thousands of kilometers, down to 1 centimeter. So this is a tendency of fractal geometry or fractal mathematics that could control this flow system, and the USGS actually did a fractal analysis of the fractures in Yucca Mountain, and it did have a fractal dimension. I believe it was something like 2.3.

On the larger scale, here's Yucca Mountain, Death Valley region, and this dotted line is the northern extent of the Walker Lane Belt, a large tectonic structure that runs all the way from Las Vegas, way to the north of Yucca Mountain. And what I want to point out here is, again, the zig zag shape. As we can see up here from the north, we have sort of a zig zag shape coming down. So this shape in the structures, we see it quite regularly when we look for it at the mountain.

Now, to the south of the mountain, we have some aeromagnetic data that were provided by the Center for Regulatory Analysis, by John Stimaticose (phonetic). You see here the position of this Route 95 Fault which runs through Lathrop Wells, and where all of Nick Stellavato's wells are near, also the Carrara Fault. And you again see this zig zag shape in the Carrara and in the Route 95 and in the trough itself which extends south down to Ash Meadows.

Again, a slide from the center showing more of their interpretation of what this trough looks like, and as Tom Buqo pointed out to you, this trough is filled with a lot of valley fill sediments, gravel, sands, and in some cases very fine sediments. But this, again, would represent sort of a different part or different piece of the flow field, and early on, we had noted there were differences in the water table response to--when we measured the water table response over time, different on the east and different on the west. So we recognized early on that this system was compartmentalized, and it's probably only loosely connected hydrologically, and this may be also another different part or different compartment in the flow field. But, again, you can see the zig zag shape.

Zel Peterman just gave me this slide. This is, again, Yucca Mountain right here, and what I want to point out in this in terms of structure is this area right here. South of this line, we have an apparent rotation of the East Crest, Middle Crest and West Crest of Yucca Mountain, and it's apparently rotated at about 30 degrees, and you can't see it in this slide, but the Fortymile Wash also takes a jog along that same trend.

The USGS and Oneal, et al., they interpret those structures as rigid bodies that have been rotated in a clockwise fashion, and separated by left lateral slip faults. These faults are also at the same angle as the northeast-southwest open fractures that the Center has defined. We believe that those fractures could be fast flow paths.

Now, a more recent interpretation of the structure at Yucca Mountain comes from the Center for Regulatory Analysis, and their interpretation here is that it's a series of en echelon normal faults. And as these faults propagate with increasing displacement, then they become connected by these ramp, relay ramps they call them. And as you can see, this zig zag structure still is apparent, even in this interpretation.

Also, we have these things called pull apart grobbins or basins where the extension is essentially taken up in these regions. This is also from the Center, and this is in the southern part of Crater Flat, and they have identified such a basin here. There are several of them along the Windy Wash Fault as well.

What was interesting is that this is coincident with the Black Butte lava flow, and it could be that these grobbins actually provide a conduit from the deep carbonates or deeper mantel for magma, but they also may provide some channels for hot water as well.

The main structures that we're dealing with in our flow model, here's the repository footprint, as I said before, the Solitario Canyon Fault, the Ghost Dance and the Bow Ridge. In my model, we use the Fortymile Wash as a boundary condition, as a no-flow boundary. But here, the big question is if we believe our temperature, cold water temperature plume, then what happens at the end of the Ghost Dance Fault?

Now, this is shown to go all the way through here, but really it's a big question mark. We don't really know what happens, whether the Ghost Dance is through going.

The USGS has come up with some interpretations of that, and they've come up with several alternatives, but basically it's the Abandoned Wash Fault, it could connect to that. It could go down the west side of East Crest or the west side of Middle Crest. And I've drawn that out graphically here so that you can see. The flow could go down any one or all of these pathways if it stays channelized in the fault structures.

Now, I want to compare that to some of the TSPA results, and this is not the most recent one. The most recent ones use six flow tubes, but they still follow this same pathway. And as you can see, they still are using an eastern flow path from the repository to Fortymile Wash, and down into the Amargosa Valley. This type of a flow field would have slower velocities, more dispersion, more dilution than you would see in a version where you would have a more confined channelized pathway. You would also have higher velocities in this type of a situation than you would with a plume moving through alluvium.

About a year ago, Zel Peterman presented some information on chloride data. I don't know how well you can see this from back there, but this is a chloride plume coming from Yucca Mountain, and it basically is moving north-south. I feel this is more supportive of the north-south flow path than the easterly moving flow path.

He has also recently produced some more data. He has some more sampling points down in the Amargosa area. But basically, we still see the same trend in the blue and green dots coming more this way. They're not on here, the Nye County well data are not on here, but Zel says that it confirms that this is basically a north-south trending flow path. And we can see this same sort of thing if you'll look at the green dots from Yucca Mountain moving southward.

Now, we decided to construct a model, and this is our three dimensional model setup. Basically, you can think of it as three layers. The first layer is the volcanic aquifer. The middle layer is the volcanic aquiclude, and the third layer being the carbonates. Carbonates are not simulated by a specific layer, but are simulated through boundary conditions. And the boundary conditions that we used are those that are at P-1, since that was the only data point that we had in the carbonates. So the pressure head throughout the whole model is set at 750 meters above sea level.

The temperature in the carbonates, we have done a little messing with that, it ranges from 53 degrees on the east, over to about 57 degrees on the west, and we did add one point for Nick Stellavato's hotter wells. But I want to say that the end of this model is just at the southern repository tip, and it does not go down into Nick's area.

In this model, we started with the tuff porosities and permeabilities that were in the literature of 10^{11} meters squared, and then we added fault zones. We have the Bow Ridge, the Ghost Dance and the Solitario. This is the Drill Hole Wash, the Sundance and the Abandoned Wash Fault. The darker area here, these are less permeable, being barriers, and the fault zones are more permeable. And up here, we have a boundary condition of 1000 meters and 29 degrees C.

To get at the permeabilities of these fault zones, they were arbitrarily adjusted from the 10^{11} meter squared in order to match the potentiometric surface. So it's totally arbitrary.

Also, I want to say that we did add infiltration where you see these blue lines along the Drill Hole Wash and Ghost Dance and the northern part of the Bow Ridge, and we put infiltration at the rate of 10 millimeters per year and assumed that it was 15 degrees C.

We were able to get a very good match we think in terms of the pressure surface. We were able to get the embayments in the 730 contour line, or close to it, and the steep gradient on the west and on the north.

And we feel like we did a pretty good job on matching the temperature plume. This is the Ghost Dance. So we have gotten the cold finger down the Ghost Dance, we've got the cold fingering down Fortymile Wash area. The reason we don't have these is I realized after we did this last run that I had set the temperature boundary condition here to 36 degrees, so we don't see the cold plume coming down here.

From previous runs, we found that we could not match this pressure head over here unless we did add some boundary conditions, because the highest heads that we have in the model are 750, and as you heard Tom Buqo say, there are heads of 775 meters, approximately, over there. And we added a boundary condition to counteract that, but I think that Tom is exactly right, that there could be some barrier to flow in the southern part of that area which is just damming it up and not allowing the flow to come out on that side, and in future runs, we will try to simulate that.

Now, as good as those matches were, we looked at the velocity fields and we were quick shocked with the numbers. We calculated groundwater travel times on anywhere from a range of 18 days to the 20 kilometer boundary, to 96 years. And the way we got those is not through a transport analysis, but basically we looked at the inter-block transmissivities or velocities block to block, and we said, okay, if this was the velocity for the whole thing, if it went at this velocity for 20 kilometers, this is the travel time we would get. It's not an analysis going through different fracture zones.

So in order to try to lower this, we decided we needed to see what we could do to lower these velocities. So what we did was we just across the board multiplied all of these permeabilities by 10^{-5}. No other changes. Infiltration and everything is the same. And we did a pretty good job of matching the pressure surface. Once again, you can see the embayments. And we were able to lower the velocities quite significantly. Now our travel times range from a low of about 5,000 years to about 8 million years.

However, we didn't do such a good job on our temperature. As you can see here, our temperatures are way too hot. They're about 13 degrees C. too hot in the Ghost Dance, and very, very hot over here in the Fortymile Wash area, and just along the Solitario canyon. So this told us that perhaps that the permeability fields are really controlling a lot of this temperature distribution, because we can't push enough water through here fast enough to cool it down.

Now, of course, the $64,000 question is what is the lower temperature boundary condition. We really don't know if that temperature field that we have in the bottom layer is correct. We have only one data point. We don't know if the pressure field is correct. We have only one data point. So these are the kind of things that we have to know in order to see whether or not a model like this is representative.

PARIZEK: Linda, we're going to be tight on time here, so if we could maybe hit the high point here and then--

LEHMAN: That's it for saturated. I'm going to move right now to unsaturated.

With regard to the unsaturated zone, this is a model we proposed in 1991, which was the idea of having focused flow on the west side of Yucca Mountain, especially in areas where the PTn unit was absent. And I'm really glad that the speaker before me came up and said a lot of this same thing.

One thing that we had noted was that there could be lateral flow in the PTn, but that you really needed to look in areas where the PTn was not present to look for higher flow paths.

The idea here is that rainfall can hit the western side of this mountain, and then actually run off under the alluvium, and then go into the open channels directly in the Topopah Springs.

The concept that's being used in the TSPA is that elevation and the depth of alluvium controls the infiltration, and we generally agree with that. The only thing is I don't agree with it on the western side where the slopes are very steep. And as the data shows, we are getting some infiltration, or at least some Chlorine 36 hits there.

The other thing that was mentioned is the role of hysteresis. We believe this should be accounted for and fast pathways for fracture flow.

In terms of structure, since we talked about the extensional nature here and most of these large open faults are running north-south, there's reason to believe that there are smaller scale fractures also oriented in this direction. And a lot of the estimates for the TSPAs were done based on the tunnel, and the tunnel, as you know, runs parallel to a lot of these structures, and now in the cross drift, we're actually going at right angles to a lot of these faults. So like a well, you're intersecting more fractures, you should get more infiltration or more hits in terms of water movement. So we believe that the structure there is also important.

And that's it for the modeling. If I have a minute--I don't know how much time I have left.

PARIZEK: Just take two minutes so we allow time for questions, and you can perhaps elaborate on some of the other work.

LEHMAN: All right. Basically, the handouts that say analysis of representativeness, I'm not going to go through that because you have it there and you can read it. But basically we feel that the flow paths are incorrect, that it's not necessarily consistent with the chemistry that's coming from the program, and they have not used any temperature in their analysis of flow in the saturated zone.

I've asked several times why don't you use temperature as a flow path indicator, and they've told me that basically, we want to develop a good flow model first, and then we'll add temperature. But I maintain that you don't know if you have a good flow model until you use temperature, or some other parameter to calibrate against besides the hydraulic head.

The whole issue of hydraulic apertures and effective porosities needs to be examined. But more importantly, also the idea of dilution, especially the NRC's model, and there's a typo on here, it should be 10 to the 6 gallons per day, not 10 to the 8th. Basically, they dilute the repository releases by a well bore dilution of 10 million gallons a day. If you do have segmented systems, especially in the lower part of the system, if we have isolated blocks separated by faults, then we probably don't get that huge kind of production rate that would be needed to create those dilutions. So that needs to be looked at.

In terms of the unsaturated zone, I think Bo has done a good job of putting temperature and a lot of structure into the model. What we would like to see now is maybe on some of the transient mode analysis, to incorporate hysteresis and also a distribution of infiltration that does incorporate slightly wetter conditions on the west.

PARIZEK: Can we take questions now, Linda?

LEHMAN: Yes, that's good.

PARIZEK: Okay. Board, questions anyone? Debra Knopman?

KNOPMAN: Knopman, Board. I'm wondering if we could ask Zel to come up and perhaps respond or elaborate a little bit more on the chemical data and its consistency with your hypotheses about the flow paths?

PETERMAN: Zel Peterman, USGS. I guess the one with the chloride would be the useful one, Linda. This is from our integrated hydrochemical isotope database that we continue to work on and unfortunately does not include the new data from the Nye County wells yet, although those are in the database. We just haven't had time to redo these maps.

It does include our two new sampling campaigns down in the Amargosa for which we have complete dissolved ion and isotopic data. So what it's done down there is tighten up what appears to be a zone or plume of water consistent with the low chloride water at Yucca Mountain. That's exemplified by the blue and green dots.

Now, these contours are developed using a program that grids the data, and you can see that the distribution of data points isn't really optimal for gridding, but it does basically do a pretty good job. It does a craging approach, and there is, if you notice that the contour that encloses the blue and green dots in the Amargosa, there's a northerly closure to that. I think if anybody were hand drawing that, you'd just continue those on up and connect them with Yucca Mountain. So I'm pretty confident that we are looking at, you know, a general flow zone, or whatever you wish to call it.

I think that the sulfate on the right shows the same thing. Now, what we're seeing there is an increase in sulfate as we move southward, and of course what's happening is we're transmitting from the water table in the volcanic rocks where you would expect low sulfate, to the water table in the alluvium, and the alluvium is composed of a variety of rock types, including material derived from the Paleozoic limestones, which probably has little bits and pieces of gypsum in it at various places. We're also looking at Precambrian detritus coming off the central Funeral

Mountains. We're looking at these early tertiary spring deposits. So the valley fill there at the Amargosa is lithologically pretty much of a complex assemblage of rock types.

So at the moment, I feel confident that we are, that the hydrochemistry--and this is only a sample. We have stable isotopes and strontium and uranium isotopes--are all pretty much supporting this north-south, and as you come south, if you're facing south there, you can see the contours do sort of take a little bit of a job left, and that's consistent with the geology that we saw that Linda had, and earlier, the maps that Tom Buqo showed on the regional structural fabric. So I think everything is kind of coming together there.

KNOPMAN: Thank you.

RUNNELLS: Other Board members? Priscilla?

NELSON: Nelson, Board. In this figure, you show Solitario Canyon Fault as more or less of a damming, low permeability zone?

LEHMAN: Yes, for the saturated zone. But for the unsaturated zone, I believe that while it's a dam to horizontal movement, it can always be a conduit to vertical infiltration, because a lot of these faults have quite large damage zones associated with them.

So I think what happens in the unsaturated zone is we have runoff going under the alluvium there at the fault contact, and just kind of dropping down into the disturbed zone just east of the fault there.

PARIZEK: Other Board questions? Staff?

(No response.)

PARIZEK: Parizek, Board. I have a question about choice of drill site locations. Tom Buqo said where he would go with the next cluster of drill holes. If you had that money that you were offered this morning, where would you put drill holes to test your hypothesis of flow and this fast path question that you raised as part of a conceptual difference?

LEHMAN: I would first of all try to get a deep hole somewhere close to the mountain so that we could see what the temperature and pressure boundary conditions would be from underneath, because I think that, at least in our concept of the model, controls.

The other thing I'd like to do is move up in this area where the bottom of the--where the end of the Ghost Dance Fault is and perhaps do some tracers up in here to see if it in fact does follow this central ridge, central crest east of Lathrop Wells cone right in through here. Or if like the others think, maybe, you know, it is going more over to Fortymile Wash, to try to sort that piece out.

PARIZEK: The consequence is quite important in whether it's fracture flow or whether it gets into alluvium a little earlier. You point that out as your TSPA type comments; right?

LEHMAN: Right. Yes, I think it's going to make a big difference. And also in the designs that are being considered, the alternative conceptual designs for the repository, right now are only analyzing the eastward flow path, and I'd like to see someone analyze a more north-south flow path and maybe fracture flow controlled scenario. It might make a big difference in what design options you choose.

PARIZEK: Parizek again. The question about USGS dropping out some control points which are red dots on your map, I didn't get all those written down. Is anybody in the Survey here prepared to comment as to why those holes were dropped out of the database for contouring the water table configuration? You more or less briefly told us they were dropped out. But do you know why?

LEHMAN: They said in the report why, and they said that they did not understand why they would have hydraulic lows at those locations, so they just took them out.

PARIZEK: Priscilla?

NELSON: Nelson, Board. On this figure here where you first showed the north flow, at about what point do you think that this flow exits the rock?

LEHMAN: That's the $64,000 question, as I said before.

NELSON: Any guesses?

LEHMAN: But I don't know how well you can see from this one that's up here, it could follow this line here and come out by Busted Butte. It could come out over here. All this is the volcanics, and where actually the line is where you get the alluvium or valley fill, I've been assuming that it's been along this Route 95 fault. It may not be. It may be in other places up here. So I don't know.

NELSON: Nelson, Board. That's why I sort of was thinking that that would be a good place to at least get some more definition of where the top of rock is and the thickness of the alluvium varies.

LEHMAN: I agree definitely.

PARIZEK: Debra Knopman?

KNOPMAN: Knopman, Board. One very quick question. Based on what you know now and the data available in your modeling, what in your mind is a conservative range on groundwater travel time from the repository to the 20 kilometers down.

LEHMAN: Well, it depends if it's fracture flow. If it's all fracture flow, I think we're looking at probably in hundreds of years range. Certainly, we could have faster paths faster than that if we had the gradient, if we had the head gradient. I don't know that we really truly have the head gradient. But I'd say probably hundreds of years rather than thousands of years.

PARIZEK: I think we have to cut it down for the next and last presentation. Thank you. There's a lot of conceptual ideas and you'll be in the drilling program. I'm sure that it's being inquired as part of it.

The next speaker is Paul Dixon who will talk about the unsaturated zone transport tests at the Busted Butte facility, and always an update there is very informative.

DIXON: Good afternoon. I hope everybody is semi-awake after lunch after having sat through one talk. So I'll try to keep you entertained here.

Again, I'd like to start this talk with this talk is being presented for Gilles Bussod, who is the Los Alamos Laboratory lead, who's been in charge of this test and he regrets that he's not here today to give this. So I'm presenting for him, so if I get outside of my bounds here in some area, please be kind.

This is just to kind of give you an idea that standing on the crest looking from the northwest towards Busted Butte, Busted Butte really is proximally close to Yucca Mountain. And in studying Busted Butte, one of the reasons why we looked at the Calico Hills and Busted Butte is it is very proximal to Yucca Mountain, and it appears both chemically and stratigraphically and mineralogically just to be a distal portion of the Calico Hills where it starts to thin. And where you have maybe 70 meters of Calico exposed to Busted Butte, you have somewhere like 70 to over 100 meters exposed to different places underneath the repository. So this is just to kind of give you a warm and fuzzy that we're not looking and trying to make correlations to a unit very, very distal from Yucca Mountain.

Most of you have probably seen this, but we'll review back through it. The test objectives at Busted Butte are to try to evaluate what the influence of heterogeneities are on flow and transport in the unsaturated zone; to evaluate other aspects of the site, including fracture/matrix interactions and permeability contrasts. And I'll show you some of this information as we go through with the examples from some of the mine backs we've had there.

Look at colloid migration in the unsaturated zone, what sort of effect did we have on infiltration. Use laboratory sorption data at the field scale. And one of the things that I'll point out about this test is this test not only has the field aspect of it, which I'll present here today, but there's a whole second set of information, which is using the analog tracers and real radionuclides in the lab so that we have a direct correlation of real radionuclides, of how they would interact in this system. And that work is ongoing right now, and there was no preliminary data that I was allowed to present today.

And then what we're trying to do right now is give enough information that we have some calibration and validation things for Bo Bodvarsson and the UZ flow and transport team to use in that to build our confidence in the use of that model and some of the things we're doing.

And the other thing is is that this test is really the intermediate scale test that we have right now between what we have at the lab bench and what we've been trying to model at the site scale. It's kind of something in between that gives us a real solid data point that's at a fairly large size.

The test plan for Busted Butte that was developed three years ago, the test was designed to go in three phases. I'll state right up front now the test will probably end after Phase 2, based on where we're going into site recommendation, and we may either continue in the future after site recommendation and license application to a Phase 3 at the Busted Butte complex, or we may in fact move to a different site to study more parts of the Calico Hills and how it interacts.

This is just for your background, just for everybody's knowledge of what tracers did we use in the Phase 1 and Phase 2 tests as I talk about them, and as I go through this.

In the test, a series of samples were sent up to the USGS. What's been interesting about Busted Butte is for the first time, we actually have been able to make physical measurements on the type of rocks that have this high porosity permeability of the Calico Hill. Almost all the drill hole information we have from the Calico tends to be from the more zeolitic units that we made more intact. When we became more vitric in the southern part of the repository, middle to southern part of the repository, this information was just not available, so we took a distribution of what we thought it would be.

So for the first time, we're able to actually go in and physically measure and validate some of the values that we've been using in our modeling and cut off the distribution tails and really get to a mean value that's real.

You can see here the porosities are extremely high. I mean, this is really like a very loosely held together sandstone. I mean, it's very, very high porosities.

This is some data from Larry Flint of the U. S. Geological Survey, just looking at moisture retention curves and conductivity data, fitted with the van Genuchten model, and this is just to show you that by and large, that model, the data very much follow that representation of that model. So we're not seeing any strange behavior in these rocks. In fact, it's a pretty amazing dataset.

To date on June 30th, we interchanged pads in the collection boreholes, and to date right now, we've collected over 7000 pads from Phase 1-B and the Phase 2 test. And of those 7000 pads, over 1000 have been extracted for everything, except for the metals and microspheres. And we have over 10,000 separate analyses right now on the fluorinated benzoic acids, bromide and the fluorescein, rhodamine dyes that we've used in this test.

The Busted Butte test layout where we're getting the pads from, just to kind of come back and review it with you guys a little bit, is this is the Phase 2 test, which is a large block test. It's roughly 10 by 15 meters. We have a series of injection boreholes here in white, and those injection boreholes are at two levels, and they have--we have anywhere between 1 and 50 milliliters per hour injection up here, and down here we have roughly 10 milliliters per hour injection rates in the thing.

This unit here is the TPV-1 and the actual test--the actual injection boreholes will set up right at the base of the TPV-2, which is the bottom part of the Topopah. The actual true Calico is in the unit down here where we have the second set of collection boreholes. There tends to be more fracturing and things in the TPV units because of the clumber joining coming down out of the basal vitrosphere. You get some of that clumber joining extending down into the TPV 2.

In fact, the Phase 1-B test, which I'll show you results from here next, which is set up here, which is Boreholes 5 through 8, that is set up in the fractured TPV-2 clumber jointed fractured TPV-2 to specifically try to address fracture flow in kind of a vitric, slightly altered rock.

This here shows the Phase 1-B test I just pointed out. Phase 1-B, we had two high injection boreholes and two low. The distance between the injection and the collection borehole was 30 centimeters. In the collection borehole, we had a series of pads for collection spaced out, and in each one of the injection boreholes, there was at least one fracture, and this is the six set of boreholes.

Pre-test predictions on this test here where we have a single injection point over top of the fracture suggests that you'd be hours to a minimum of a couple days before you'd have breakthrough along this fracture of fluids.

If you look at the chart here, you'll see that the injections started on 5-12, and it wasn't somewhere until about 6-23, over a month later, over 30 days later we got breakthrough. Everybody thought for a while that we had problems with the injection. We knew we were putting tracer in, but we just weren't sure what was happening. You can see that once it breaks through along the fracture there, that's the main flow that heads along that. You see there's a substantial amount of matrix flow that heads out into this.

And as I start to talk about some of the Phase 1-A tests, you'll actually see, I didn't have a slide, I didn't get one before I came here, what it looked like. We actually over cored these holes in the process of analyzing it.

We also injected in this hole here the microspheres, and we're looking at the transport of those microspheres and their filtering along fractures and out into the matrix.

What I'm going to do now is just show some pictures of the Phase 1-A mine back. I don't know how many of you people got to visit when Gilles was mining back the Phase 1-A, but it was pretty exciting.

This here is actually the 100 centimeter depth, just to kind of point out that this is the second mine back layer, and I'll actually show all four progressive mine back layers. There's Dr. Bussod standing there.

Note that in these two boreholes here, the injection rate was 10 milliliters per hour, and in this borehole and this borehole, the injection rate was 1 milliliter per hour. A single point injection, and the injection point was at 3 o'clock in each one of the holes.

This is to kind of give you an idea of what it looked like as we mined back. We started at the surface. You didn't really see any effect of the surface, but at 20 centimeter mineback, you can see the injection point here was at roughly about 100 centimeters into the hole here. It's about a meter in was the injection point. You can see that you've had migration back this direction. What you notice is the strong capillary flow.

So what you get is you get not only--you have the fluids injected at 3 o'clock and you get the fluid diffusing out all the way around this due to capillarity effects. And you'll notice that there's a lithologic contact here that I'll come back to later, but anyway, you get a kind of a picture that this is--the capillarities are very, very strong in this unit, which is a confirmation of some of the things that we used in the VA model where we used very large capillarities for this.

And the other thing to point out as we go through this here is that we saw it in the Phase 1-B test, but we see it in the Phase 1-A test, where you have fractures, fracture flow appears to be pretty much insignificant, and that lithologic contacts are much greater fluid boundaries or fluid retentions than you would ever get from migration along the faults.

Just looking at the 10 milliliter per hour injection borehole here, at 50 centimeters, here's the lithologic contact. You see the halo of capillarity around there. When you get to 90 centimeters, there is a fracture or fault heading through this zone here that's actually got opal along it, so you know you've had fluid travel along that fracture at points in the past.

And you notice that you get a ponding at the contact, and within the Calico Hills, these contacts tend to become slightly more silicious at the contacts due to the ash falls and alterations at the surface. You see a large effect of ponding here, and you see a very minor effect of fluid moving down the fault, as opposed to the ponding. And, again, the red dot is the injection point.

What was done with the Phase 1-A test is we did a series of three-dimensional pre-test predictions of what we thought we would see. And I'll show you in the next slide an actual example of the field test versus a model prediction. And two-dimensional heterogeneity simulations were used for pre-test predictions, unfractured, Phase 1-A, and fracture, Phase 1-B. And stochastic models were used for pre-test predictions and to understand the uncertainty in what we were doing.

For the high injection rate borehole, this is what we saw in the field. You guys have seen this here. This is basically what we modeled. We modeled that you would see a basically diffuse halo. What you notice is that this is--we put a fair amount of heterogeneity into this based on the test and some things we saw. What you notice is that with the exception of a lithologic contact, that's the major heterogeneity that's causing retention of flow here. Other than that, capillarity seems to be a pretty equant system in here.

In the lower injection rate borehole, I found this pretty interesting, is that when you're injecting here at the 3 o'clock, the fluid is being injected at a low rate and it will slowly wrap around the borehole, and what you notice in the model is you see this kind of bell shape here, and you see that very similar bell shape in the field, which I thought was pretty exciting, that you're actually able to model that phenomena of how the fluid slowly wraps around at low injection rates.

So at this point in time, from the Phase 1-A and 1-B tests, the tentative conclusions are that we're able to confirm that you have long travel times in the Calico Hills. The capillarity is really showing you that you're really going to be able to slow fluids heading through there. The migration of water from fractures into the rock matrix appears to be very strong. Where you have fluid flow down fractures, it tends to get imbibed in the matrix due to capillarity. And by doing so, you're increasing your sorption sites and you're increasing interaction with the rock and the ability to retain things.

The other short-term thing here right now is the preliminary information about where the colloids went, at least in the Phase 1-B test from visuals, because they are fluorescent, is that visually, it doesn't look like they moved more than about a half a centimeter away from the injection point. They really just get in there and clog things up. They're really being filtered extremely strongly as they're being imbibed into the matrix. And those results hopefully will be coming out here in the next month or so as Gilles puts in some information for them to use in the UZ flow and transport PMR.

And another thing I'd point out is that sorptive retardation in the Calico Hills is going to be important, especially in the vitric units. We've already talked in the past about how the zeolitic units, if they have the right zeolitic content, somewhere less than 15 per cent, you'd have a high enough porosity permeability to allow interactions. What this is showing is that even in non-zeolitic Calico Hills where you have alteration with clays and calcite, you get a fair amount of sorption going on.

And I'll just point out here that the data and analysis from this test are being used for the site recommendation and will ultimately be used in the license application.

Just a slide Gilles put together just to point out that in the VA model, when we looked at one of the analysis where you had fracture flow from the repository horizon down to the water table, this is the sort of breakthrough curves you got for non-sorbing to sorbing tracers. In fact, you got fairly quick travel times for non-sorbing tracers.

In VA, we had a range distribution of what the Calico Hills might give us as far as hysteresis effects or due to matrix capillarity, and what you see here is that if you use the information we currently have from Busted Butte, you see that you can basically gain and verify three orders of magnitude hysteresis in the flow time by in fact using the data that we've been able to verify at Busted Butte in the field test.

So we've been able to take that distribution from the VA model and very much restrict it in what we believe is really out there in nature.

What I'd like to do now is Gilles has done a series of pre-test predictions for the Phase 2, and I'd kind of like to run through a couple of these and just, you know, just show you where he's headed with some of this, and give you an idea that we've put together, as we start analyzing and putting back into the model the data, we actually had something to go back against for the Phase 2. And, again, remember the Phase 2 block is here where you have nine injection holes, and I think 12 or 13 collection holes over here.

Also remember that these little holes, or the stars of them here, they're doing electrical resistivity tomography in, and also every time we'd pull out liners here to collect pads, we'd pull them out in pairs, and they run a series of ground penetrating radar two dimensional plane analyses on those holes. So we have neutron logs for the initial saturation. We run a neutron log, and then we run GPR or ERT on these. So we're trying to get an idea of the real time saturation front movement in this block, as well as what's coming out on the pads as far as tracers and dyes.

And this slide here, this is basically from the VA, this is just to give you an idea of the validation of where we're more highly zeolitized in the northeastern part of the repository, you tend to get much, much shorter travel times, because the flow tends to be more fracture dominated. What we're learning now from the mineralogic model as we add more detail, and which Bo is adding into this thing, is that this may in fact actually be very, very conservative, and the reason for that is that where you're zeolitized in the northern part of the--northeast, you're also interlayed with vitric units. And from Busted Butte, you're able to show that you get more of this kind of a behavior in travel times with the vitric units, and so the interlayering, you may have fast flow like you have through the Tiva Canyon into the Paintbrush Tuff, and then it slows down like in Chlorine 36, and it picks back up when it gets back out again.

Well, what this is showing is that some of these diagrams will definitely change as we head into the new representations with the UZ flow and transport model that Bo Bodvarsson is working on.

This is just to show that from a model prediction where you have the high injection boreholes up high, and the lower injection boreholes down low, this is the sort of saturations we would expect at one year. So when we start-- the mineback for Phase 2 is supposed to start right after Christmas in 00. We'll start mining this back, and that way, hopefully when we start the mineback, we will be through with the mineback in a couple of months, the analysis by mid-summer, and wrap up the writing of the report on Phase 2, with all the analysis, by the end of FY 00, is the goal right now.

And you'll see that the tracer front here has moved significantly less. So what you're looking at here, you'll see fluorescein and some of the other dyes, the more mobile components, on the pads. The reason why we need to mine back the Phase 2 like we did the Phase 1-A test is that the tracers by and large we predict are not going to move very far, and the only way we're really going to know their distribution and how they went in the system is to do a limited mineback at Phase 2.

And I think I'll stop there and entertain questions.

PARIZEK: Thank you, Paul. It was a very informative update. Any questions from the Board? Debra Knopman?

KNOPMAN: Knopman, Board. Help us, Paul, to understand how you can fairly extrapolate from the results of Calico Hills to the rest of the Calico Hills throughout the repository horizon in terms of properties. You made some fairly bold statements about what you can say now about travel times through Calico Hills, and I'm just wondering how you get from here to there.

DIXON: Okay. What I'll start with is that the latest three-dimensional mineralogic model for the mountain that's been developed at Los Alamos and is being used in the ISM PMR right now gives--they've been able to, from all the drill holes we have to date, and using analysis techniques, basically put together a three dimensional picture of what the Calico Hills looks like in both horizontal and vertical depth. Based on that, and knowing the connections from the mineralogy and petrology of the Calico underneath the repository, looking at the mineralogy and petrology of Busted Butte, even though it's a thin section, you know that you're looking at the same--basically, you're looking at kind of a pie shell thinning out to the edge.

So you're looking at the same unit, and what we're saying is that from the drill hole information we have, again, it's very limited, we tend to in these vitric units get dust at the Calico Hills and Busted Butte, we know why we get dust, because it's even hard to hand auger it and get intact samples for analysis, that mineralogy matches. And where we have missing chunks of the Calico Hills underneath the repository, we infer that that is there, based on basically dust samples that have come back up.

KNOPMAN: So the density, just thinking in terms of a slice, a plane through the repository block, you have approximately how many boreholes, direct borehole mineralogical samples that you are working from to do your craging, or whatever, your interpolation?

DIXON: Right now, there's 21 or 22 boreholes around the repository that they used to do this. Again, SD-6 is the only borehole that actually penetrates the actual repository where we have coring. And I agree with you, most of our data occur east of the Ghost Dance, and north of the north ramp. I mean, we have a large hole in there of where we are and we're projecting, but you have things like the old H-walls, the hydrologic walls, and other places where you can get cuttings and chippings that give you an idea of what's happening.

So some of this right now, we're in the process of trying to determine which of this data you can use, and has to be qualified for site recommendation moving on, but in general, as you get more holes, and again, you don't want to punch the repository full of holes to do things, but there's fairly good indications from everything that's been drilled in the south, even as you go down east of the Ghost Dance, that as you go south, the vitric content of the Calico Hill increases dramatically, and as you go north, the zeolitic content, and that has to do with the tip of the beds and the amount of time they spent underneath the water table at some point in their past history.

PARIZEK: Paul Craig?

CRAIG: Craig, Board. I'd like to explore with you your confidence--I'm really following on Debra's remark--but back on Number 20, it looks like using your calculations, that for the significant portion of the repository, the delay times are in the order of a thousand years for 50 per cent. And you know the Board is very much interested in defense in depth, and so these numbers are important in thinking about that.

I'd like you to, if you can, speculate on the level of confidence that you have in these numbers, and what kind of findings might cause those hold-up times to move up, so that the 50 per cent time for the bulk of the repository wouldn't be on the order of a thousand years, but it might be in the order of 10,000 years.

DIXON: That's a good question. I mean, I'm not sure--I mean, I have confidence right now based on just the hysteresis effect you get due to capillary as you move through there. I mean, you slow the water front. I mean, this is strictly just a slowing. I'm not even looking at retardation, because the data aren't fully in on how you're retarding things.

The fact of the matter is is that you can get potentially in the zones where you're more vitric and you have a lot of porosity permeability, you may be able to gain an extra order of magnitude if you have retardation there. And the retardation on clays of the key radionuclides that Inez has looked at, and on calcites, there is a fair amount of retention of those key radionuclides to those different elements, and that's one of the alteration products in the more vitric units. You might have 3 to 5 per cent of those sort of constituents, and what has been shown in laboratory experiments, it's not if you have 40 per cent zeolite or something like that, or 40 per cent clay, it's trace amounts of clay, but it's having the interaction and having the capillarity effect where you suck stuff out into the matrix and you get a lot of interaction of that fluid with the matrix allows you to get very, very high sorption coefficients, because you interact with a lot more of the rock.

The confidence level right now that we understand--again, these are from the VA, but we understand--we believe we understand what's happening in the longer travel time zones there based on Busted Butte. We believe we have a lot more confidence there. Where we have less confidence in my viewpoint, and where we're doing modeling, is what really happens in the zeolitic zones? Because in some of the zeolitic zones, when you get over 15 to 20 per cent, the porosity and permeability drop off enough that you tend to be dominated by fracture flow. The question is is that how continuous is that zeolite horizon? Based on the boreholes we have, we know that it's interlayered with vitric.

So if you have fractures in one part of the Calico, do they extend through the vitric and allow you a fast path, or do you hit, and it's going to be modeled, or is trying to be modeled right now at Berkeley, when you come out of the fracture in the zeolitic and you hit the vitric, what is that spread halo before you head down to the next part, and what is that hysteresis and time of that fluid flow, and then the interaction and sorption.

I don't know if I fully answered your question, but I tried there.

PARIZEK: Other Board questions? Debra Knopman?

KNOPMAN: Knopman, Board. Let me just follow up on this picture, because the more I look at it, the more puzzled I am, I guess, or trying to reconcile this with what I thought I understood about flow through the UZ close to Solitario Canyon Fault, which I thought was more rapid there, but you have more rapid infiltration rates on that side. But also you're showing the longest travel times there.

DIXON: I might just ask Bo to step up, because I believe he probably can represent this figure better than I can at this point, since he is the flow and transport guru here, and I'll ask for his assistance, if you don't mind.

BODVARSSON: What was the question?

KNOPMAN: The question was just trying to understand why--I thought your most rapid infiltration rates are along the west side, along the crest in the Solitario Canyon Fault. But these plots are showing the longest travel times to water table, so--

BODVARSSON: Right. What these plots show, and these plots were done by Bruce Robinson of Los Alamos, based on the UZ flow model for the viability assessment, basically what it does is it takes the repository horizon there that you see--see these particles there on the left-hand side, you have the red particles and you have the blue particles, that is just the repository level. And then he follows these particles down without any sorption going into--he's interested in knowing where they go, and interested in the travel times.

So the Solitario Canyon high infiltration rates are much further to the west than what you see in this picture there. The very slow times that you see on the left-hand side and the bottom side are due to the vitric part of the Calico Hills that has a porosity of 40 per cent and permeability of 300 millidarcies. The blue thing is the zeolitic rocks that are almost impermeable, as we understand them now, so flow is pretty much through the fracture, so it's very fast.

KNOPMAN: So this is just a partial area of the--

BODVARSSON: Of the UZ flow and transport model.

KNOPMAN: So it's not covering the whole repository?

BODVARSSON: No, this is covering where the repository, if you put particles at the repository, where they go, but not the entire UZ flow and transport model that includes Solitario Canyon and further west. That would indicate exactly like you predicted, much faster travel times over there.

PARIZEK: Thank you. Parizek, Board. A question on why you might not go to Phase 3. Is that the success of what you've been able to learn from Phase 1 and 2 might be the reason?

DIXON: Success of what we learned in Phase 1 and 2, and I would say also the fact that in talking with people, the next phase you might want to try something different than the vitric. You might want to try something where you have a mixed zeolitic and vitric unit, or you might want to try something underneath the repository, or something different related to flow underneath the potential repository.

PARIZEK: You have access to rocks like that somewhere in the area?

DIXON: There's potential access in different areas. I mean, there was potential to come in off the Solitario Canyon side and get to some things on that side there.

PARIZEK: And one other question about the organic dye. I guess it's a fluorescein?

DIXON: There's fluorescein and rhodamine used in this.

PARIZEK: Which both have maybe some organic tendencies to hang up, different than just working in a pure capillary sense?

DIXON: Yes.

PARIZEK: Do other tracers show similar behavior, or is it a little premature to answer that yet? I mean, is this a good tracer?

DIXON: At this point in time, we've run--before we started some of these tests, there was a series of laboratory tests run to try to get an idea running with what we thought were very conservative tracers versus dyes. The amount of retention of the dyes in these rocks due to sorption or other things, you know, was minimal. But, again, these were short-term tests. We do have other things like lithium bromide here, and other tracers that are conservative, where we can try to look at, and that's what some of these chemical analyses are showing, and there's not a big difference between those two at this point in time.

PARIZEK: Thank you. Staff? Leon?

REITER: Leon Reiter. We heard about some measurements of Chlorine 36 at Busted Butte. Could you summarize this and tell us what's going on there?

DIXON: I'm not prepared at this time to give you that information. I'm sorry. I know that June has some of those numbers, but I was not privy to that information before I came. What I can promise you is that I will get those numbers together from June, get them to Claudia, and she will get them to you here soon.

PARIZEK: I have one question from the public, and it has to do with boreholes that are being planned for monitoring purposes and testing and instrumentation. When will these be sealed, and if not, what impact will they have on repository performance? So that's a question that's not identified who gave it.

DIXON: I mean, are we talking in the ESF, or are we talking at Busted Butte here with that question?

PARIZEK: No, I think this must be in the repository, in the vicinity of any drill holes that might be put there for instrumentation, performance assessment.

DIXON: I would think that Mark Peters is probably, or Claudia probably are good people to answer that.

NEWBURY: Claudia Newbury, DOE. The sealing of the boreholes would occur at closure. We'd have to have a program in place and the NRC would have to agree that the sealing of the boreholes was adequate before we could actually declare the site closed.

PARIZEK: And it might also apply about deep drill holes that are near or that penetrate through the repository horizon. That would also be true for any deep drill holes?

NEWBURY: Deep drill holes off the repository block?

PARIZEK: Well, there's one. Isn't there just--you said there was one?

DIXON: Well, SD-6 is--

NEWBURY: SD-6 is on the block?

DIXON: It's on the block. But what other holes are on the block--

NEWBURY: Anything that was on the block would have to be sealed. Anything off the block would be related more to environmental reclamation than to actually sealing as in sealing for the repository.

PARIZEK: If the person who asked that question didn't think they had a complete enough answer, please put up your hand or come back to us and we'll try to get more detail on that. Thank you. Now, Jared Cohon?

COHON: Thank you very much. Before we enter the public comment period, we've asked Steve Frishman from the State program to make whatever observations he would like to make about the meeting or about the program, or both. Steve?

FRISHMAN: Thank you. I'll try to keep it in line with the types of observations that I have made in the past to this Board, and that's maybe not tell you anything that you don't know or haven't already heard, but maybe present it from a little different perspective to help you think about it, where your responsibilities lie and what the Congress has directed this Board to be doing.

First, from Abe's comment this morning, I think it's really interesting that I had to present a paper that was published 6000 miles away for somebody from the Yucca Mountain project to finally read it and notice it. The title of that paper was Transparent Doesn't Always Make it Right. And Abe apparently understands that now.

I think since the program is careening towards a site recommendation, that maybe we need to think a little bit about where the Board's duties lie under the Nuclear Waste Policy Act right now. It seems to me that first of all, the most important decision that the Secretary of Energy makes in this entire program coming, oh, more than 15 years after it officially started, is the decision to recommend the site to the President.

It's also pretty clear to me that the intent in the Nuclear Waste Policy Act amendment that established the Board was to have the Board in a position to, among other things, inform both the Secretary and the Congress about that decision, and according to the language of the Act, the technical validity of the work that went into that, or at least a portion of that decision.

So at this point, I think you're very close to being put on the line, at least in terms of the public's expectation of what you as a statutory body are doing.

The Nuclear Regulatory Commission is essentially immaterial in your duties. At the time of site recommendation, you don't have a specially designated duty, other than your normal reporting duty. The Nuclear Regulatory Commission has a duty relative to its regulatory jurisdiction, and that's that at the time of site recommendation, the Commission has to provide a report on whether the information from site characterization seems sufficient for license application.

I see that as quite different from your charge to look at the technical validity of site characterization. And the Board, after site recommendation, if the site is recommended, the Board still has some duties, but those continue to be to the Secretary and the Congress in terms of reporting on technical validity, not reporting on your views of what NRC ought to be doing, and your duties end essentially when the Secretary can't do any more than either accept the fact that a license was denied, or is off on--has told the Congress that it's time to start thinking about something else.

So the Board's responsibility is to do for the public what the NRC is doing in a regulatory world, I think, and that's does the site meet the expectations of safety, but from a slightly different perspective. In your case, from essentially a standard of scientific completion, comprehensiveness, excellence, and so on. The Commission, on the other hand, has previously established rules maybe, and their judgment is whether the information presented to them demonstrates some reasonable certainty of compliance.

So now we're fast getting to a position where the expectation from the Secretary, the Congress, and especially the public, is that you will have some very definitive things to say at this most important public decision.

I think what we've heard over the last couple days makes me pretty uneasy about the Department's view of the seriousness of the site recommendation in terms of a demonstration of the degree to which a Yucca Mountain repository might be safe or not, and safe by no standard that we know of today. And I've heard maybe some uneasiness in some areas, but let me go to I guess the way that I and people that I work with and people that I speak to in the public who when they can learn to understand DOE's language in this program, begin to expect things to be shown to them.

First of all, I take the Yucca Mountain safety strategy as sort of a basis for a site suitability determination, and it's invented by the Department, but it says how they think the site ought to work, to create a site that meets expectations of safe disposal. The elements of that are just in shorthand, low seepage, long-life waste package--this is nothing new that people don't know--slow release in transport, and concentration reduction.

Well, if we look at the things that we've seen over the last couple days, low seepage is pretty wide open, and I think some of the information that was presented from the drift scale heater test talks about how wide open that might really be, because there were things that I think were maybe unanticipated in the sense that we have to sort of rethink conceptually how water might move around based on both the heat-up and the cool-down.

Now, in general, I think maybe we're all sort of thinking the same thing. But in the specifics of it, I see some sort of interesting things about how water may move, especially during and after a cool-down, once we see that the water doesn't violate gravity, which I hope we didn't expect it would, but also that water is likely to flow back through on the cool-down. But also, those tests aren't going to be completed, and especially the very important aspects of cool-down aren't going to be completed until well after the site recommendation.

And this is one of the key elements, regardless of what the temperature is, as long as you have it high enough to where you have a vaporization condensation process going on, and it looks to me as if even though the impending decision by the Department for its latest design, and I remind you only latest design, is a couple weeks away, and I'll talk about that a little more in a minute, it looks to me as if the decision has been made that we are going to have a repository design that includes some amount of rock at boiling temperature.

So that long-term heater test is not going to be available to tell us anything really reliable about seepage until way after the recommendation is made.

The long-life waste container, well, we've heard that it's even better than anybody ever thought in the last few days. I have a hard time conceiving of something that is essentially indestructible in nature for over 100,000 years, when we don't really have a lot of experience, and some of you may think that I'm ignorant, but at the same time, I'm not convinced, and you may convince a licensing board at some point, but you're out in an area where it's very difficult to convince people that from your engineering judgment, and a few years of research, that we can have essentially near 100 per cent reliance on a repository, when in fact what would be made under these circumstances is not really a site recommendation, it's a continual recommendation, because that's where the reliance really is.

We look at slow release in transport, we have some places to go. I think the question about this latest Calico Hills work is important, and that's how do you know it means anything in the repository system overall. We also know that the UZ model still relies on things like that, and probably isn't going to get very much, if any, better for the site recommendation. We also know that there are some conceptual issues having to do with the UZ model, and back in I think it was '91 and '92, there was a small flurry of concern about alternative conceptual models, and yesterday, Marty Mifflin brought up the point that a lot of what is being discussed was first mentioned back in that, and even before that time period.

So we still have a what I believe to be a controversy over conceptual model, and I think there's enough information out there, and also if you look at even the most lenient requirement that might show up in NRC's Part 63, the Department still is going to have to look at alternative models and explain something to some extent why they objected to models that they're not using.

If you look at the concentration reduction, once again, I think the saturated zone model is under as great or greater, probably greater fire than the unsaturated zone model. And it likely will get better through time, not because it was planned all along to understand the saturated zone, but because work finally is being done now. Once again, the results of that work and any definitive basis are not going to be available to improve people's confidence in a site recommendation, when that site recommendation is the one that is really a go/no go as far as the judgment of an official who can only really be challenged on whether that decision is arbitrary and capricious or not.

When that site recommendation decision is made, there are essentially no criteria that the Secretary is bound to hard and fast, and what few there are, the Department is trying to get rid of. So it's a decision, a policy decision where there is essentially no hard and fast standard. So the only thing that we, the public, can do is try to hold the standard as high as possible.

Now, I think for M&O's recommended design, you're really being sort of unfairly put on the spot, where you were told yesterday that it's sort of expected that you'll give your views on that EDA II before the Department makes its final decision in a couple weeks.

I'm sensitive to at least one of the comments that was made yesterday about there's an awful lot of hidden policy in that recommendation that is not explicit, and also should be out there for other people to be involved in. And I guess the only way that I can see how to do that, and I know this once again violates Lake's schedule and all the rest, but at the same time, I think this recommendation is important enough to where Lake can put his schedule aside if enough people think the job is not being done well enough and there's not enough certainty, and there has not been enough involvement in the types of policy decisions that are very deeply embedded in this latest design, and also in other parts of the whole repository evaluation.

The simple thing to do, and the fact that we're dealing with not only the need to rely on essentially all future for containment of the waste, we now know that we have 10,000 years free time on a container, I think maybe we could take a few extra months, or even a little bit longer than that, why not get the real repository proposal in the EIS and then we'll all review it, rather than putting the Board on the spot to piecemeal something that I know you're uncomfortable doing, and I think it's absolutely unfair for you to be put on the spot for what is only the latest greatest design.

I've been in this program since day one. I've seen a lot of latest greatest designs, and I also am getting more and more sensitive to presentations such as we had almost all day yesterday where we, by the end of the presentation, we're supposed to be thoroughly convinced that the engineers thought of everything. And current members of the Board have been through this a few times. So why not be in a position where instead of putting the Board on the spot for something that in the long-term probably is not really very meaningful, because what they'd love for you to do is say yeah, yeah, that's great, and then they'll change it again in about a week anyway. So you've bought into a little piece of it, and now you're going to have to buy into more, and that's going to keep going on, because that's the history of this program.

So why not just say okay, DOE, it's time for you to tell us what it is you think you can do, and tell us in very definitive terms, and maybe just stop for a little while and let the Secretary decide whether to recommend this site, do an EIS like all other big projects have to do, let the people decide, let Congress decide whether from a policy basis it's a good thing to do, rather than as is very obvious, and other people are using the terminology now, and that's "there's a train coming and get the hell out of the way." The alternative I think at some point, we're going to have to end up, and it's just a matter of how many more hundreds of millions of dollars get spent before that happens.

I guess only one other point that I want to make just for your thinking, and I think it goes once again directly to the idea of how important the site recommendation is, and that's the idea of essentially deferring a closure decision, which means deferring a disposal decision. That's not what the original Act was all about.

Also, if you go back, and I never believed, really believed the good intentions at the time when in the late Seventies, people started out spouting this "we created this problem, we can't leave its solution to a future generation." Well, now what we're suggesting, or what is being suggested, if you defer closure, which is deferring a disposal decision, what we're really saying is we are deferring the problem to a future generation.

And what is implicit in the idea of pushing it out there, and I know I heard Lake and others talk about how wonderful it is to let them make this decision, when the idea is sort of bounded in the assumption well, maybe there's a resource there and we can make them rich or something.

Well, what's really underlying it from this program's point of view is passing on the presumption that it will be absolutely safe, so they can make decisions about all other wonderful things if they want to close it, or be absolutely safe if they want the resource, fine. But the presumption and the idea of deferring is we're deferring something safe.

If you look at what is behind the recommendation, that presumption is not supportable, and in fact what we're doing is we're deferring more risk to future generations than we are--than we would if we didn't do anything.

So just a point to think about in terms of the importance of the site recommendation, and sort of the planning that's behind it, and where the Board needs to weigh in because, and I told myself I wouldn't do very much of this, but if you look at all of the oversight, which is the most overseen program in the world, we're about--you're about all we've got left, because the regulator sure isn't going to do it for us, and we know that.

Thank you for your time.

COHON: Six people have signed up to make public comment. Let me just go over some ground rules very briefly for how we'll conduct this session.

First of all, please recall or be aware, if you're not aware of this before, your remarks are for the record. They're being recorded, which is one of the values of the public comment period in our meetings, I think. Consequently, we'd like you to identify yourselves with your name, your affiliation, if any, and if you care to give it, it's not necessary, and please talk into a microphone, either the one right there or the one up here, whichever is your preference.

Because of the number of people who have signed up, and the lateness of the hour, I'd ask each of you to try to restrict your comments to five minutes. I will motion or otherwise make myself annoying, I guess, at five minutes so you know time is up.

We'll start with Englebrecht Tiesenhausen from Clark County. Is he still here? There he is.

VON TIESENHAUSEN: Thank you, Mr. Chairman and Board. I'd like to thank you all for giving me the opportunity to listen to your questions and to listen to all the presentations that were made by the program people. Many significant issues were discussed, and obviously I'm not going to go through those again, and I promise not to incur the wrath of the Chairman by going over my time limit either.

In my estimation, the Board's next report will be one of the most important ones as far as the program is concerned. I, therefore, urge the Board to evaluate as comprehensively as possible the information they have then and will be given.

I also strongly urge the Board to present their findings and conclusions in a very direct manner. This would make the report more understandable to the public, and clearly identify the areas that need further study, or those where more transparent presentation or further elucidation of past studies or decision processes is needed.

Thank you.

COHON: Thank you. Sally Devlin.

DEVLIN: Again, thank you so much for coming to Beatty. It's always a pleasure to see you here in Nye County, and I hope you'll do it again, and I hope we can have you in Pahrump. But of course I have to leave you laughing, and I have some very good news for you. The bad news is first, I'm all out of geriatric tables. So I'm sorry about that. But the good news is we got some money at the University, and we are going to test the water starting in November, which we will present to you all for radiation, as well as fluorides and nitrates. So that's going to be fun, and you will hear from me on that.

Now, I see Lake Barrett isn't here, so I can't yell at him. Is someone here from his office? Abe? Oh, good. Because I had a comment for you. Abe was talking and he used in one sentence six acronyms, and I said if you're going to write this book and the public is going to read it, you had better put a large glossary in the front so that we know English. We're so used to talking these acronyms, you don't even realize that the public doesn't understand a word you're saying. So I really do, I would like his address so that I can have the Board write to him to start on our $50 million project.

The other one is that I need the Board's address so that I can ask you, or the hospital board can ask you for a recommendation for our big project, because it's got to start now. This is not something that you can go with, and I understand the attitude that if we don't get Yucca Mountain, well, who cares. My feeling has always been about the test site, that Yucca Mountain is at the test site, and the test site is totally contaminated, and even though you're on part of the Tonopah Test Range, that you're just as contaminated. And if it doesn't go, you're still going to have to clean up the mess. So you need something medical for your needs.

I think it is important for this Board to care about the workers and the doses that they're getting, and DOE is having all the people that have worked at the test site, Hanford, SRS, and so on, go in for physicals. At Hanford, they had 8,000 people show up. They had money for 2,500. So you see we're doing this at the test site. These people are dying of cancer, and I know them, and that's not nice. So we have to have all kinds of medical, and this goes towards transportation, too.

I've talked to OSHA. They're not going to do a darned thing for ten years. But unless you have something that not only the dangerous test site, Tonopah Test Range, Nellis, and so on, and you have some medical, and then you have the people, and you heard Tom talk about 150,000 in

Pahrump, and then you have our one interstate highway, which is a nine hazard, U. S. 95 is as high a hazardous road as they have, we have got to have something medical and it's got to start now. Because it will take five years, it will take someone with imagination and guts, and so on, to put the PUB, the DOE, everybody together, because this is a major project that may lead to the whole state going this way, that may lead to the whole nation getting modernized. But until Nevada grows up and realizes this has got to be done, somebody has got to take up the cudgel.

So the only other thing I have, and of course I was rather concerned, and that is I have been talking about the Nelson limits, and when the man was talking about the canisters and I asked my question, what I really wanted was them to invent a little tiny canister that is in proportion to the one that will be used, and that they would put the four or five tons of the high level waste in. But I want them to reduce it, and I want them to reduce the waste, and then I want them to put it in a little iron room and fill it with water and all the rest of the stuff that's going to happen in Yucca Mountain, and then if the Nelson limits prove true, then it will explode, and that's what I'm talking about with the Nelson limits.

So this is my suggestion for a test to see if this catastrophic explosion will occur. I didn't mean for everybody to put four or five tons in a real grown up canister. So please understand that this would be a little bitty test, so only a little bitty would blow up.

The only other thing I have to add, and of course I have to leave you laughing, and that is about costs. I have been talking about, and it's on Page 45 of Volume 2 of the Viability Assessment, about two repositories. I brought with me Senator Domenici's remarks about two repositories. I have Russ Dyer's quote about oh, gosh, we found 109 metric tons of high-level waste. And I've been saying for years to this Board we've got 126,000 metric tons. So I'm very concerned about the cost, because in Volume D in the Appendix on Page D-1, it says 1,500 canisters will cost $3,000,000,681. And the reports I heard today, they will be 10 billion to 20 billion.

Now, if we have two repositories and time goes on, and so on, and I know we have inflation and I know all this stuff is $98 or $97, we're talking just for canisters, 40 billion. Well, the Congressional report back in '94 says 25 billion for the first repository, and 35 billion for the second, and I'm so sorry, but being an old housewife, you know, on a budget, I don't think that's nice to double and triple the money. And will the public accept it?

And so with that, I hope you're laughing, because if it gets up to 60 billion, I'm leaving.

COHON: Thank you, Mrs. Devlin.

Mrs. Devlin, just one clarification. I won't call it a factual clarification, but a data clarification. In the designs that DOE presented yesterday, their cost estimates are in the range of $20 to $24 billion, and that includes--well, good or not, that's a number and that's their latest estimate. So that's the number that they've been using.

DEVLIN: That's a very nice number. Thank you very much.

COHON: Thank you, Mrs. Devlin. Jerry Szymanski. You might want to repeat your name and if you care, your affiliation just so we get it right on the record.

SZYMANSKI: Jerry Szymanski, that's S-z-y-m-a-n-s-k-i. I do consult part-time for the attorney general, State of Nevada.

My comments pertain basically to Nye County results. First of all, I would like to welcome the Board to Beatty, where it's hot. I found that the title Early Warning is particularly appropriate for the program, and I do not mean early in terms of the future releases from the repository. I mean in terms of the conceptual understanding of the hydrologic system with the old geological system, which is--to the decision to be taken very late next year, which is site recommendation.

Now, what we have found already, based on the results, is there is a hydraulic mound. We do know that this is not perched water. We also know that the head is abnormal, it is higher than it is upstream in the flow paths. So we are clearly dealing with a hydraulic mound.

Now, what is causing it? And there are two general possibilities. One, the process which might be being explored there is what we call a forced convection, that is, there's something which are broken the floor and the water is flowing upward. There's another possibility, and that possibility is that we are looking at the terminally unstable system, which is hosted in a mechanically unstable system. What do I mean by that, terminally unstable? It is basically that the number exceeds its stability limits, and the water is convecting. Mechanically unstable, I mean that the temperatures are changing. In other words conductivity of the host is changing.

By putting two together, you might begin to understand the relevance of the second possibility. Now, I do understand that the first one, this forced convection, is a natural way that hydrologists think about things like that. However, there's no basis for it. Short of observing the system for a very long time, and I mean like tens of hundreds of years, we cannot tell the two apart. We just don't know.

Now, it would inappropriate if we were to call one of the Delphi systems to pass the judgment on what is it.

Now, fortunately at Yucca Mountain we got lucky. The--hosts the whole bunch of minerals which we all agree were deposited by water, and mostly agree lately that some of them were deposited by hot water. What we do not know is the age of these minerals. Now, what is important is that we will not know this age when the site recommendation report is submitted to the President. So, in other words, the most fundamental question won't be answered.

Well, there's another opportunity, which is offered by Nye County results. It is quite easy to obtain samples of the water, obtain isotopic signatures of the water, and I mean strontium, I mean uranium, lead, carbon and oxygen, and try to match the signatures with the signatures of the deposits which exist at Yucca Mountain.

If you recall, there is the USGS work whereby they had found that the minerals which we're talking about were not deposited by water immediately from below the water table. That's true. Somehow, USGS is getting to the conclusion, therefore, they must have been deposited by rainwater. Well, I wouldn't go that far. Logic tells me that the conclusion that some other water was involved is the only one which is justified.

In other words, we can perform the testing, is it a forced convection or is it an unstable system, with a very small investment of money and time. It can be done within a few months, and would cost tens of thousands of dollars, no more.

Now, at the end of my speech, thinking about responsibilities of the Board that Steve Frishman outlined, I would seek the assistance of the Board to assure that the question, is it A system or is it B system, is answered before--I underline before--the recommendation is given to the President. Otherwise, we will be looking at a very, very messy process in the future.

Thank you very much.

COHON: Thank you, Dr. Szymanski. Earl McGee?

MC GEE: My name is Earl McGee. I'm from Amargosa Valley. I'm a United States citizen; that's what I represent, nothing else. And I want to repeat what I tried to get across to Lake Barrett and the other group in 1995. I asked a question what is your alternative if Yucca Mountain is found to be unsuitable. There was no answer. I told him it would appear like man has created a monster and now they're admitting that they cannot control that monster. Burying any of that in the earth is the most insane thing I've ever seen, and that goes back to 1943, when three months after I turned 16 years old, I was 6,500 miles away in the South Pacific, and I saw a lot of things there that I didn't agree with, and I certainly don't agree with this.

You people have worked hard, every one of you.

There's no doubt about it. It's the very beginning of that work that I differ with. If we can't find some way of processing or controlling that, and like I told Lake Barrett and the other group, build the vaults on the surface of the earth, build them strong enough to where in case of an earthquake, any seismic activity, they would rock like this, and you could line that with space age technology. Like one gentleman said, well, concrete won't last 10,000 years. Hell, humanity as we know it is not going to last 10,000 years. That's obvious. They get their mind out of their backside and start thinking positive. I'm a member of the human fraternity. I don't deny any other segment of the human fraternity for their views. I may differ with them, but I don't deny them, and haven't.

And like the meeting on Fernald when I asked the quest of what are you going to do with that when you get it on the test site, well, we're going to bury it in a shallow grave, which is ridiculous. We've got to take all waste and do something with it, process it in some manner. And like I said more than once, this so-called Superfund cleanup is a joke, and it's nothing but a joke. We can do better, but it has to be at the very foundation.

The gentleman who just spoke gave a lot of reasons for not having that out here. Hell, it shouldn't be anywhere, and you shouldn't transport it across the country.

I said that at another meeting. It's stupid.

I notice that Mr. Frank Kane, who's president of the Building and Trades in Las Vegas, wrote a letter and I think it was, oh, the Senator from Alaska who read that on the Senate floor. Frank worked for me down there when I was with Raymond International, one division of it. I left him on the project to cover it. He's an iron worker. When I heard about this letter that was read there, I got ahold of Frank and told him, "What the hell is the matter with you?" He said, "Well, Earl, we've got to put it somewhere." It's a joke.

And I want to thank you very much, and I commend you people for your endeavors. You've worked hard. The only thing is the work is not in the right direction.

Thank you.

COHON: Thank you, Mr. McGee. With apologies for mispronouncing, I won't even try so I don't mess it up, Ms. Hazlett. Maybe you could restate your full name, because I didn't get it.

HAZLETT: My name is Glen Hazlett. I am a Blackfeet woman that lives in Tocopah, California, and I work with Corbin Harney, who's a Western Shoshone elder who can't be here today, and asked me to say a few words, and some people that came with us.

You know, I have to say some things that he wanted to get across, and for the record, this Yucca Mountain is a sacred site to the native people. It would be the same thing as if the government wanted to store this waste in your churches, because we cannot have our ceremonies without our land. We don't use buildings. We have these sacred places, and the whole test site belongs to the Western Shoshone people, and they are not allowed to use it. Of course, it's a little unsafe right now.

And to even consider using Yucca Mountain as a repository with all of the earthquakes going on making it unstable, with the sub-critical tests that are being conducted that further make the ground unstable, as I said to someone last night, it's like trying to store water in a shaky sponge, and the safety of all the people is at risk. Everybody is at risk. It's not just people 25 miles down the road in Amargosa Valley, like I heard somebody else say at another meeting. You know, it's all of us. It can't be stored there. It's not--the scientists know this, the geology reports, it's unstable. And transporting it across the country is very dangerous.

I know from personal experience, because I'm an anti-nuclear activist also, and some of our road blocks that we've had, we have found the trucks hauling these things into the test site to be leaking very badly. So it's just--really, I don't know what the solution is, but if you stop producing it, if you shut down the nuclear power plants, contain it there and then put all your energies into finding a solution for this, solar power is good, wind power is good. In fact, considering the talk about this Y2K problem, you're going to have to use solar panels on these nuclear power plants or they're going to blow.

Thank you.

COHON: Thank you, Ms. Hazlett. Ms. Gillmore? And maybe you could state your full name, since I didn't get it.

GILLMORE: My name is Maria Gillmore. I'm speaking as your sister. I'd like to address the overall problem of the nuclear waste site and to ask, if not demand, the cessation of any further production. Healthy and harmless alternatives should be fully utilized from this moment onwards, regardless of the cost. And we are like lost children right now with a problem far bigger than our young minds can consciously deal with, and who do the children turn to for advice?

Not so many of the native elders are left, but they are due to meet soon and I feel compelled to inform you of the forthcoming gathering of the traditional elders from all over the world, which will take place between the 12th to the 17th of October this year at Angels Gate Cultural Center in San Pedro, California in the hopes that a group of your members may attend and join in talks to find a solution to our present predicament, because it's all of our problem.

Thank you.

COHON: Thank you, Ms. Gillmore.

Are there any other members of the public who would care to make a comment or ask a question?

(No response.)

COHON: Seeing none, let me just make a few closing remarks.

This has been a very good meeting I think, and a very significant meeting for many of the reasons that Steve Frishman offered in his observations. This is a key moment in the history of this program, as DOE nears a decision about a design that they will carry forward.

In a lot of ways, it's the beginning of the stretch run, if you will, towards a site recommendation to the President. That's a very short time away, 2001 is very close. There's a great deal to be done, not just by DOE and its contractors, but by this Board, including, as we've heard, commenting on the recommended design that DOE is considering.

We heard about the scientific program, a status report on that, which was very useful. It was particularly encouraging to hear from the participants of Nye County in the science program, especially their drilling program, and it was good for us to see it on our way up here on Monday.

I do want to emphasize that we, the Board, both understand the role that we play, not very different in fact, Steve, from what you suggested, and we're very aware and sensitive to the key decisions and milestones that are before this program between now and 2001. We will be up to the task, I hope.

I want to thank several people for this excellent meeting. Let me start with Carl DiBella, a member of our senior staff who was responsible for putting together the technical program. Carl, raise your hand, please. We appreciate your efforts.

Mike Carroll, our Deputy Executive Director, who also handled all the coordination for this meeting. Mike is sitting at the back. Thank you, Mike.

Linda Hiatt and Linda Country, both of whom already left for Las Vegas to advance our next meeting, our business meeting which will start tomorrow, they handled all of the logistics, as they always do for our meetings. I imagine it's always a challenge, but doing it in a place like Beatty, a good two or three hours from Las Vegas, is a particular challenge, and it went off very well, and I want to thank them.

Our support people who make the record possible and make it possible for all of us to be heard, I want to thank them, Scott Ford, our recorder who's always with us. Scott, that's S-c-o-t-t. And John Stout, our audio guy.

I want particularly to single out one person here in Beatty who was just magnificent. If she's not in the room, I hope we can find her. There she is. Please come out, Mary. This is Mary Ball. Mary, I just want you to know with all the talking and all the stuff that's gone on the last few days, you're the only person who got a real ovation. Now, what does that tell you.

BALL: Thank you. But what did I get that for?

COHON: For everything you did. We appreciate not only the coffee and the drinks, but everything was on time, everything was great, it was flawless, and we thank you very much.

BALL: Thank you. I appreciate that. But let's also give a hand for Dorothy Foresithe. I don't think she's here, but she did help me out.

COHON: Let's hear it for Dorothy. Thank you.

The folks at the Senior Citizens Center, who could not have been nicer in feeding us and caring for us, and the Town of Beatty generally for welcoming us in a variety of ways, it's my first time here, the second time for many of the staff, but the first for most of the members, and I'm looking forward to coming back.

I do want to say that having our meetings in places like Beatty is not only important so the Board and DOE and others can hear from the public, but it gives the Board members a sense for the place where the possible site will go. You cannot get that being in Washington or Pittsburgh where I am, or where any of the other members are located. You have to be here.

Now, although we were only here for a couple of days, we have been elsewhere, not in Beatty, but Pahrump, Amargosa Valley--Valley, as Mrs. Devlin said, and we'll be back. So thank you for the experience, not just for the hospitality, but for giving us the chance to get to know Beatty and this place a bit more.

Thank you all for your participation and comments. We'll see you at our next meeting. I don't remember when that is, but watch our website and watch your mail.

Thank you very much. We're adjourned.

(Whereupon, at 3:10 p.m., the meeting was adjourned.)

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